UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FALL 2001 BOARD MEETING

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

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Dr. Norman Christensen
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I N D E X

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P R O C E E D I N G S

(8:30 a.m.)

COHON: Welcome to the meeting of the Nuclear Waste Technical Review Board which we're holding under a pall, to say the least. As you all know, the country has suffered a national tragedy this morning, one that is still unfolding, and it's not too much to say it's a national crisis. Lake and I conferred this morning to decide whether we should go ahead with the meeting, and obviously, we decided to do so. The Federal Government is basically closed and that's, I think, for security reasons to keep people out of buildings in Washington. But, here we are and we can't go anywhere because all the airports are closed. We decided to go ahead with the meeting, anyhow.

We'll all be distracted, there's no question about that. It will be perfectly understandable if some people decide that during various times during the day they'd prefer not to be in here and prefer to be elsewhere. In addition, I for one have to keep my cell phone on because I also run a research university in Pittsburgh. So, I have to be in touch. So, interruptions by telephone will be perfectly understandable today.

May I ask you all for a moment of silence in respect to those who have died already and with our prayers to those who are trying to handle this crisis. Please rise?

(Whereupon, a moment of silence.)

COHON: Thank you, very much.

This morning's session will be chaired by Jeff Wong, Board Member. Jeff?

WONG: Good morning, everyone. Today is our second day and we will begin the session with a presentation by the U.S. EPA on their environmental protection standard for the Yucca Mountain Repository. After that, we'll turn to preparations by the DOE on possible site recommendation. We'll hear a presentation regarding the response to key technical issues, a summary of the preliminary site suitability evaluation, and a presentation on the supplemental science and performance analysis. Among those after that, we'll talk about or we'll hear a regular update to the science program and then updates on Nye County and on the Nevada funded studies talking about --well, actually, look into the flow in the saturated zone near Yucca Mountain.

This morning, Dr. Craig will be helping me with keeping us on schedule, as he did so great yesterday, and I don't want to ruin his reputation. So, with that, I'd like to start.

Today, our first presentation will be by Ken Czyscinski of the Radiation Protection Division of the U.S. EPA and he'll provide us again with a presentation on the EPA standard.

CZYSCINSKI: On behalf of the agency, I'd like to thank the Board for the opportunity to come and talk about a rule that's been a long time in the gestation. First of all, I'd point to some introduction that you're well aware of. On June 5th, we finalized the standard and the Nuclear Regulatory Commission will implement our standard through their implementing Regulation Part 63 which has just recently been approved.

A little bit of history. In 1992, the WIPP Land Withdrawal Act exempted Yucca Mountain from the EPA generic standards, Part 191, that applied to geologic repositories. Quickly thereafter, the Energy Policy Act directed us to set standards specific to Yucca Mountain and no where else. Establish a limit on individual dose as part of these standards. In the process, we were to contract with the National Academy of Sciences for them to provide technical input, insight to us for use in developing these standards, and directing us to make our standards as consistent with the NAS findings and recommendations. In 1995, EPA received the NAS report.

We, of course, followed the usual rulemaking process. We had a public comment period, public hearings in four different places. We received about 800 comments, 28 people testified, we had about 69 sets of written comments that had most of those 800 comments within them.

The fist part of my talk, I simply want to run through the standard, itself. Then, I'd like to talk about some specific aspects of the standard and give you a little bit of the reasoning and rationales behind why we did what we did. The standard is divided into two parts, the storage standard, Subpart A, and Subpart B, the disposal standard which has three substandards; the individual-protection standard, the human-intrusion standard, and the groundwater protection standard, and a few other miscellaneous provisions that are necessary in order to understand what the rest of the standards are supposed to mean.

The storage standards. 15 millirem maximum dose to any member of the public in the accessible environment, which is outside of the Nevada Test Site, the Yucca Mountain site, the Nellis Air Force Range. An important aspect of our regulatory development was what happened when Yucca Mountain was taken out from the purview of 191. The directions to us were to develop standards that were unique to Yucca Mountain. And, as our lawyers say, we have to develop those standards de novo, as they say. In other words, we start from scratch. So, we have to look at all the concepts for radiation protection, radioactive waste disposal, and say, okay, do these things make sense still for Yucca Mountain? Do they fit? So, we look at the storage standards and we say, okay, is there any reason why the public should get a different dose from any releases through storage than they might get through disposal? The answer is no. So, the standard is 15 millirems for storage.

Moving on to the disposal standards for the individual-protection standard, again when we go back and look at the fundamental concept of should there be an individual-protection standard, there's no argument about that. Congress told us to make one. The question arose as to just what should that standard be? If we got back to Part 191, we have a 15 millirem standard there. Again, that was a generic regulation. It applies to any and all repositories. We asked ourselves is there some reason why the Yucca Mountain Site should be allowed to give people a larger dose than what we had in the generic standard? Again, the answer comes back no. We look at our precedents, we apply Part 191 to WIPP. The standard there was 15 millirems. Is there any reason why Yucca Mountain should have a higher individual release standard than what was allowed in WIPP? The answer comes back no. So, this was actually probably one of the simplest decisions, almost clear cut in developing the rule.

The individual-protection standard is 15 millirems. The receptor to that is what we define as a reasonably maximally exposed individual, the RMEI, who lives in the accessible environment above the highest concentration in the plume of contamination. We require a 10,000 year dose projection. In other words, this 15 millirem standard applies for 10,000 years within the regulatory legal context of the rule. The standard also is an all pathway standard. Again, these are things that are very much consistent with Part 191 and previous precedent in applying Part 191. When we ask ourselves do these things make sense for Yucca Mountain, the answer comes back, yes, they do. So, these provisions are very much similar to what was in 191.

To talk about the RMEI in a little more detail, the RMEI is a hypothetically representative person. We looked at the demographics in the area downgradient from the repository and decided that our RMEI should be representative of the majority of people in that area. So, we called this a rural residential lifestyle. There's about 1,000 people or so in the Amargosa Valley area. Only about 100 of them from the information we have actually call themselves full time farmers. The rest of the people do other things. In fact, when you look at people out there, most of them are doing two jobs and sometimes three or more. So, we cast our RMEI as a rural rigid residential person. The characteristics of the RMEI should be characteristic of the people who live downgradient from the repository now in the town of Amargosa Valley. We've used the term reasonably maximally exposed. So, what we do here in this rule is we don't assume all the characteristics of that RMEI are always to the worst extent in terms of potential exposure. This is not a maximally exposed hypothetical individual. It's a reasonably maximally exposed. So, what we're doing is setting a couple of parameters at the high end. The location, we're putting them smack on the border of the controlled area which is something I'll talk about a little later and we say that the RMEI drinks two liters a day of water. Again, this would push the likely exposure or distribution toward the high end, but not excessively toward the high end.

Okay. What's the accessible environment? This is a term that's been around a good while and again was in 191. It's an area outside of the fire area, outside of the controlled area. Now, the controlled area, which I'll talk about in a little more detail, for the Yucca Mountain site should be no more than 300 square kilometers and should exceed no further south in the direction of groundwater transport than the southern border of the Nevada Test Site which is a distance of about 18 kilometers from the repository and should extend no further than five kilometers in any other direction. I'll talk about this in a little more detail later. Again, the whole concept of a controlled area was something that came from 191. So, we had to look at that and say in terms of the Yucca Mountain site, does it still make sense to have a controlled area? And, the answer comes back yes.

The human-intrusion standard, we agree very much with the assessment that the NAS made in terms of the usefulness, the importance, the intent of the human-intrusion standard. We recognize that resources are rather sparse in that area. There hasn't been much found except groundwater which is the predominant resource. So, in comparison to, say, a sole repository, human-intrusion is not a particularly likely pathway where individuals would get exposure, but it is a potential pathway. The NAS recommended that you look-- we look at human-intrusion in terms of resilience of the disposal system. They're effectively saying, okay, if you had some sort of intrusion, would it cause severe degradation of the disposal system? They recommended that the exposure limits for a human-intrusion scenario, a stylized test of resilience like this, should be no higher than the exposure level for the anticipated case. We agree with that. So, the exposure standard for human-intrusion is 15 millirem. They recommended that this exposure should take place when the canisters have begun to fail, and at this point where they have failed, such that an intrusion may be possible without the drillers actually being immediately aware that something has happened. So, we've included this recommendation also in the rule. These last two bullets effectively address the time when this human-intrusion event occurs. Again, that's something the applicant has to determine for the licensing process. We're giving them some direction here. The time of this possible penetration happens when the cans are beginning to be degraded.

Okay. What do you do with the dose? If this time of intrusion is within 10,000 years, the dose, when it gets to your receptor down at the end of the control barrier, if that is within 10,000 years, that's reported in the license application. It's judged against this 15 millirem standard. If the intrusion happens or the dose gets to the receptor after 10,000 years, that dose then is not compared in a legal licensing context against this 15 millirem standard because of the compliance, the regulatory period that we've denoted of 10,000 years. This is a line in the sand effectively from a legal standpoint. What happens before 10,000 years is critical to the license acceptance. What happens after 10,000 years is additional information and understanding of the disposal system that's made available during the licensing process. But, again, the legal line in the sand is the 10,000 years.

Okay. Circumstances, some more detail on the intrusion. A single intrusion from water exploration, again the only type of resource exploration that seems reasonable for the top of Yucca Mountain. As the NAS recommended, the borehole would penetrate the waste package and proceed directly to the aquifer. The borehole is not carefully sealed. Only releases through the borehole are analyzed since this is a test of resilience of the disposal system. No releases caused by unlikely natural events or processes are considered.

Okay. Moving on to the third standard, groundwater protection, we've applied the EPA MCL limits to what we call the representative volume which I'll talk about in a little more detail. Representative volume is an annual groundwater withdrawal representing current and planned groundwater uses in the town of Amargosa Valley. The size of the representative volume is 3,000 acre feet. It's in the accessible environment and it's to contain the plume's highest concentration. So, the representative volume is centered on the plume. There's been quite a bit of controversy as to whether or not groundwater standards should apply, its EPA policy, its national policy to protect groundwater. Groundwater protection requirements are put on every sort of disposal facility down to municipal landfills. We feel that when you're putting, oh, more than 5 billion curies of high-level radioactive material in a system that's sitting directly over a fresh water aquifer that is the sole source for people downgradient of it, it just makes no sense to stand up and say we're going to throw away groundwater protection requirements.

Some of these other provisions, I'll talk about this one in a little more detail, but it's a very fundamental point in terms of evaluating long-term performance of a geologic repository. How do you predict over a 10,000 year time frame what people will be doing, what the world will look like, what the whole disposal system will really look like in those kinds of time frames. If you look in detail at what's involved in projecting performance, there's virtually nothing that is actually verifiable in real time. Everything is an extrapolation. So, this question of what you--just how confident can you be in the myriad of extrapolations that you do is a very critical one. This is something that's been looked at ever since the geologic disposal concept was born in the 50s. It's been examined by expert panels and blue ribbon panels over the years. About every 10 years, the whole issue recirculates again and the same conclusions come up. So, you assume that society, biosphere, human biology, technology, and knowledge are essentially as today because any other assumption leads you to speculative scenarios none of which are any more defensible or usable than any others. So, the only thing you can really do is simply default and say, okay, things are the way they are today, the only thing we can deal with.

The question of where do you cut off the events, processes, and so on that are involved in this process also pops up. Do we have to look at highly improbable things like meteoriting packs, again a question that's been around for really generations? The same kind of probability cutoff that existed in Part 191, we simply carried forward. It makes just as much sense in Yucca Mountain as it does generically. So, you do not analyze events that have a probability of less than one in 10,000 occurring within 10,000 years. Again, as I said, the 10,000 year compliance period is a line in the sand from a legal standpoint. That does not mean that the assessments of disposal system performance are absolutely infallible one year before 10,000 years and meaningless one year after. It would be irresponsible for us to simply make any kind of statements or implications like that. Our rule does require that the long-term performance, the post 10,000 year projections, be examined by the applicant and that they be put in the environmental impact assessment which is part and parcel of the whole package of materials that goes into a licensing process.

Okay. I'm beginning the second part of my little spiel today. I want to talk to you about a few areas in more detail as to what the thinking was that went into some of these things. Some preliminary background information, again the Energy Policy Act mandated that we do a site-specific standard, not apply 191 to Yucca Mountain as was done with WIPP. We had to essentially rethink everything in terms of whether or not it made sense for Yucca Mountain or not. The

NAS recommended we use a cautious, but reasonable approach to the standard development. And, again, we agree with this approach. In terms of looking at every component and determining whether or not it makes sense for Yucca Mountain, an important example of this is there is no containment standard in the Yucca Mountain standard as there is in Part 191, as there was in WIPP. The containment standard as described in 191 was to address the situation where you could have a poorly performing repository which made its releases into lakes, streams, oceans, large bodies of water where you would get a massive dilution and that dilution would then spread this high release over a large population. If you look at the physical situation of Yucca Mountain, that just doesn't exist. There is no purpose, there is no need for a separate containment standard. Between the three standards that were applied at Yucca Mountain, the release paths are covered. So, this is a prominent example of what happens when you have to redevelop the standard de novo, as the lawyers say.

Okay. Again, considerations. Consistency with site-specific information doesn't make sense for Yucca Mountain. The approach to public health and environment protection should be cautious, but reasonable and non-extreme. In other words, are we taking a prudent, cautious approach to public health protection in the options we look at and in the final decision? The third point is an important one. Whatever we pick here, we want to reduce regulatory uncertainty. In other words, we want to pick approaches and requirements that don't generate a wide spectrum of possible scenarios, any one of which is as equally defensible as any other, but whose performance may be dramatically different. The licensing process is really a-- it's not just simply an academic meeting. It's a consensus development process that involves the regulated, it involves the applicant and any other interested parties. Reasonable folks have to be able to sit down and look at this assessment for licensing and be able to come to a consensus that this is okay, this is good. If you put in provisions that simply introduce a wide spectrum of divergent views in your licensing process, you're setting yourself up for a very difficult, maybe impossible time.

I've alluded to this early, a persistent question in framing the details of the standards. Of course, we're looking at long path life waste, waste that will be around for a very long time. So, we have this concern of long-term protection based on these wastes, a generic concern for geologic disposal. We have to project his performance. So, what do we use, current conditions or do we use projections of what we think things might be? The answer to this is something that's been around a good while. We call it the

Future States assumption. It was explicitly put out in the WIPP regulation for these performance assessments. You assume that human activities, technology, knowledge, etcetera, are as they are today. The thing you vary is geologic and climatic variations because we can have some sort of handle on at least a way to vary these things. If you look at the history of predicting human events, it's just about hopeless. No one 200 years ago really predicted what the world would look like today. Predictions we make today about what the world is going to look like 300 years from now are essentially the wildest of speculation.

So, the Future States assumption again, you cannot rely on what we predict is human actions and activities, assume the current situation. That does not mean that we assume that everyone at Yucca Mountain is rooted in the ground like a tree at some point in time. When the NAS put their report out and the license application is submitted, we think it's reasonable to look at relatively short term projections of changes in the local area, 10 or 20 years, rather than assuming some fixed date in time. And, again, climate and geologic conditions are required to be varied in the performance assessments.

Things I want to talk about in a little more detail, the RMEI versus critical group, compliance point location and controlled area size--these are closely related-

-the regulatory time frame, the representative volume, and a concept that we've always used in our regulations is the term "reasonable expectation.

One of the major concerns has been who should be the receptor? Should it be this RMEI or should it be a critical group? The RMEI, as I said, was a hypothetical individual; a critical group is a more diverse group usually spread out. The site-specific situation, in other words what makes sense at Yucca Mountain? We have a small, but widely dispersed population. The characteristics of the site is we have a fracture dominated hydrology, contamination plumes, and this type of hydrology will be relatively narrow. The closest population downgradient from the repository--again this is rural residential group.

The exact path of contamination plume will remain uncertain. For small exposed populations, dose to the RMEI or critical group member is expected to be very close. So, we don't see that there should be a big difference in the actual dose assessments to the critical group as is usually defined compared to this RMEI hypothetical person who actually will have a lot of characteristics of a critical group if you follow the normal way critical groups are defined. In fact, if you look at international texts on critical group development, you'll see that one of the ways a critical group is actually implemented is to define a hypothetical representative person. I believe the German program uses this approach. There will be less decision-making uncertainty with an RMEI in the path of the contamination plume as opposed to a critical group.

To kind of illustrate that, this is a rather busy slide, but it's from one of our technical reports. What we have here is a particle track of the contamination plume from Yucca Mountain. The critical group--the groups that have been looked at in the past have largely been collections of farms. So, to look at this, we effectively took a 255 acre alfalfa farm which is the largest water consumer in the area and as an average size for the farms there and then we just arranged them in a couple of different geometric patterns. Here's a pattern of 25 alfalfa farms in a square, here's 25 in a curves outline, 15 in a vertical line. The important point you see here is that the particle part path cuts through only a relatively small number of these farms. If you were to use a critical group, you're faced with basically an arbitrary choice of how you were going to arrange these farms to get a representative dose or a truly protective dose. You could put all these farms in some sort of arbitrary line in the direct path of the plume itself. They'd all get very much the same kind of concentrations. There's all sorts of essentially regulatory uncertainty and arbitrary decisions that have to be made to use this kind of farm critical group approach. We believe that it's more protective to take a bit more conservative approach and simply put your RMEI up here in the path of the plume, the characteristics of which again are defined to be representative of the people in this entire area down here where the actual farming takes place. It's simply much more straightforward than a critical group approach, it reduces the uncertainty, and is unquestionably a conservative public health protection stance.

The compliance point location, this one of the fundamental concerns. The location should reflect again the cautious, but reasonable approach. It should be consistent with site-specific information.

Let's look at the site-specific information. The predominant release path is through the groundwater. The groundwater moves down and to the east in the unsaturated zone. When it hits the saturated zone, it moves generally south. The location of potential receptors now, the closest people are a handful or residents down at what's called the Lathrop Wells area about 20 kilometers distance from the repository. Again, looking at the relatively short-term projections of what's going on in that area, we see some impressive development plans, industrial park, a science museum, projections of thousands of people coming through this science museum. So, this area between 20 kilometers and the Test Site which is a boundary that's very much fixed--I've been told it's legally been determined the Test Site will be a restricted area essentially indefinitely. So, the northward extension of people can only really go up to the Test Site boundary. So, the Test Site boundary appears to us to be the cautious, reasonable, sensible southern limit for the compliance point. That's as close to the repository as people are likely to be keeping in mind our Future States assumption.

The disposal system controlled area again, a very closely related concept, something that's been around for quite some time. The controlled area has two major regulatory functions. It's a compliance measure. The standards apply at the border of the controlled area. The controlled area essentially defines the extent of the natural barrier which is part of the disposal system. So, the standards apply at the boundary and beyond. They do not apply inside the boundary. It also has an institutional control function in that you want to--by institutional controls, you want to keep people out of this area maybe potentially contaminated. So, the controlled area is of a minimum size, exclusionary to prevent inadvertent exposures.

Again, we look at the concept, is the concept of a controlled area valid for Yucca Mountain? The answer comes back, yes, it makes sense. A site-specific assessment is based on the receptor locations, again you can't be any closer to the repository than the Test Site boundary, the projected repository performance--and here's an important point for determining the size in terms of projecting repository performance--we considered both the anticipated, as well as the unanticipated releases to come up with a maximum size of 300 square kilometers.

Here's a plat we've put together. Again, it's from one of our technical reports in the docket. It shows here the layout of potential repository locations that have been published, projections of the contamination plume from the repository, and again you can see the eastward movement and then the southern movement. Based on the institutional requirement of the controlled area is to keep people out from inadvertent exposure, you want to essentially put your controlled area around the entire projected release path from the repository. If we look at the performance projections within 10,000 years, we see that very little gets out of the repository under the normal operating conditions, the normally anticipated slow degradation of the engineered barrier. However, we've had a long-running kind of evolution in the off-normal performance. In other words, what's been called in DOE documents premature releases or early waste package failures, etcetera. These kind of things can't be precluded. We can argue about just how many premature failures there are likely to be, but they can't be unequivocally eliminated. We know from the fracture flow hydrology that the plumes from these kind of releases could be relatively narrow. Those kind of releases could happen anywhere in the repository. So, it's possible that premature release plumes could be coming anywhere within this envelope. Again, for an institutional control measure, that envelope of potential releases should be contained within the controlled area. We simply draw a little box around that down to the Test Site boundary, we come up with at least about a kilometer or so buffer from the actual operation facilities and the ends of the projected plume. We come up with an area, 14 by 25 kilometers. That's more than 300 square kilometers. However, if we assume that the controlled area should be tailored to what the actual plume projection is, we cut off--taking these areas out of the box, we come up with a size of about 300 square kilometers which we feel is a reasonably conservative size for the controlled area, itself, which will still fulfill the two major functions that I talked about initially.

Okay. The regulatory time frame, our objective here is to provide a reasonable expectation of long-term safety. We've got very long-lived wastes here. We want to give the public protection for a long time frame. The decision of just what that time frame should be must balance the long time frame and the uncertainties inherent in the projection of repository performance and, again, coming back to this point about regulatory consensus. In Part 191 and in previous regulations, the 10,000 year time frame was selected. If you look at international regulations, you'll see the 10,000 year number show up in just about all of them in one way or another. The entire waste management community has apparently made some sort of decision here, the consensus feeling, that 10,000 years is long enough to have to make long-term projections of this performance in terms of a regulatory decision. And, the rationale has always been that as time stretches into the tens of thousands, into the hundreds of thousands of years, the uncertainties in these projections begin to get so wide that you can make almost any number of scenarios for performance, any one of which may be as equally defensible as any other. So, in terms of making a yes/no decision on a regulatory time frame, if you're doing it on a widespread performance scenarios, none of which you can justify dramatically one versus the other, you've set yourself up with a very, very difficult thing to develop this regulatory consensus on.

So, looking at whether or not 10,000 years make sense for Yucca Mountain, again we look at some of DOE's performance assessments and you say, okay, when does the peak dose come out? If you look at the viability assessment, the peak dose comes out at about 300,000 years. If you look at the site recommendation TSPA, the peak dose comes out at around 300,000 years. If you look at the difference between these two assessments, you see the waste package has been fairly dramatically changed, there's no drip shield in the VA. The corrosion resistant material is put on the inside and it turns out putting it on the inside makes it very susceptible to a crevice corrosion process which is at that point in time identified as 25 percent more corrosive than the general corrosion. And, there was a rather unrealistic and extreme model of waste package performance and release. If you look at the site recommendation assessments, again the drip shield shows up, the corrosion resistant material has been put on the outside to increase the lifetime of the package. There's a much more realistic model of waste package behavior and release. But, yet, the peak dose still comes out at about 300,000 years.

So, this isn't giving us a whole lot of help looking at the time of peak dose. It appears that the peak dose based on the performance assessments is relatively insensitive to the waste package design and the modeling assumptions. However, if you burrow down into the modeling assumptions, you'll see that even with the SR, there are some very conservative extreme assumptions taken on the waste package performance. One could say, as Abe alluded to yesterday for the migration of radionuclides along a continuous water film, these things are incredible. The assumptions in the TSPA analysis are again difficult to defend on a realistic basis. So, even though the waste package design and the modeling become much more highly engineered, the analyses don't help us in terms of realistic assessment of just when the peak dose time is. We've got trouble there if we want to set a standard based on a peak dose time of arrival. We just don't really know realistically what that would be.

Again, falling back on the more generic rationale, i.e. that the very long dose assessments contain uncertainties, if you look at the long-term assessments the DOE has done, you have considerable uncertainty in the projection of site characteristics and the simplest source term model for long-term release has been used. DOE simply takes the waste and dissolves it up in the percolating groundwater at the solubility limit. The uncertainties in very long-term performance really confound our decision-making in terms of looking at these long-term performance scenarios and what the projected releases are.

We have to ask ourselves will the hydrologic regime remain unchanged over periods of hundreds of thousands of years? This is a seismically very active area. If you look at the displacements on the active faults over the periods of hundreds of thousands of years, some of the geography here can be displaced kilometers. If you have a fractured flow regime, the water movement here is controlled by the fracture characteristics. Would 300,000 years of additional seismic shaking in this area loosen up the fracture network? Assuming the same gradient if we loosen up the fracture network, the groundwater travel times could go down. You actually have better performance. That's assuming the gradient remains the same. And, why should we assume that? Will the gradient for the system stay or alter in such a way that the performance is in terms of groundwater travel time a little bit better, worse, or pretty much the same? How can we really determine that? How reliably can we assume that?

How reliable are climate projections? We can make estimates of what some of this rainfall can be, but can we really have any certainty about how much it will be or when it will be within that long time period? Again, groundwater is what takes the waste out. If we have large uncertainties in how much groundwater over hundreds of thousands of years will actually move through this system, again we have a hard time here justifying any particular performance scenario.

How will the heat pulse alter the near-field? There's been a lot of talk about this over the years. There have been actually very, very few assessments that have been made to try and quantify these results. People kind of wave their arms around and say, oh, well, this is a complicated business. We just don't know. We have these large uncertainties of what the heat pulse will be. We tend to assume that it will always be negative, but yet there are lots of examples in the geologic world where minerals precipitate in fractures. We expect a lot of action effectively right around these emplacement drifts in the situation. Will this heat pulse in the near-field result in a situation where more water gets into the emplacement drifts or actually less? Will it channel this water kind of more uniformly over the entire emplacement drifts or will you have just a few places where we might have more conductive fractures dripping into this environment? How do we know? Again, this confounds the number of different scenarios that can be conjured up and would have to be evaluated in a long-term licensing process.

How will groundwater react with and transport wastes after the waste package is substantially gone? Again, the assessments that DOE uses, so far, uses the simplest approach possible. You simply dissolve the waste into the groundwater as a function of its solubility limit and again this considerable uncertainty over the years over just what the solubility limits are for some of these poorly soluble radionuclides, that uncertainly remains. Is it reasonable to assume that in the hundreds of thousands of years time period that the system will behave simply like stirring the sugar into your coffee; it will dissolve it at the solubility limit? If the waste package is largely gone and water drips on the waste, it drips off. Perhaps, leaking experiment numbers are just as legitimate in the long-term as solubility numbers are. Again, that's a fundamental change in the source term for those assessments. Which one is right and how much is right?

So, with the combination of all of these uncertainties in predicting what the system will look like, what the performance will be in these very, very long time frames, you say for the purpose of regulatory decision-making, this just presents us with a situation where reasonable people just probably will not come to much consensus.

So, we settled on the regulatory time frame of 10,000 years. It's consistent with the existing precedents. It avoids the speculative performance scenarios and the very long performance scenarios are required. DOE still has to make this long-term prediction. It's simply just not the subject of a licensing decision.

Representative volume. Okay. It's a volume of groundwater for resource protection. It represents a spectrum of resource uses in the downgrading area; again, agriculture uses, residential, municipal, industrial uses. A calculational approach, as we've put in the rule, for the representative volume for the groundwater compliance are essentially based on the plume itself. We have the slice of the plume method where the representative volume would be that piece of the plume which annually goes by the compliance location. We also have a pumping well approach where you'd simply put wells around the plume and pump out the representative volume and again compare that groundwater concentration to the MCL limits.

I'm going to go back to my busy slide. How did we come up with 3,000 acre feet? Again, look at this particle track here and look at about--here's the size of one average alfalfa farm. To put the agricultural use across the plume where we need at least two farms in there for the plume to go through them to get kind of an even reasonable approximation of what the plume composition would be. We've also added in the residential and industrial uses and come up with a number that's very close to 3,000 acre feet. We've taken a conservative approach because, after all, the farming area is down here. The amount of water tapped by that farming area is in the tens of thousands of acre feet. The actual estimates of the discharge from Basin 227-A that has the releases in it is about 8,000 feet. So, 3,000 feet, by taking the representative volume and putting it up here, protecting a smaller volume higher up at the compliance point, we're protecting a larger volume for all the users downstream.

This is the last thing I wanted to talk about. This is our concept of reasonable expectation. It's a word that has appeared in Part 191. It's a word that's kind of in our lexicon. We believe that absolute proof of compliance with the standards cannot be gotten in a conventional sense. As I said earlier, everything in this business involves a long-term time projection. So, under a reasonable expectation approach, I've got three bullets here that sound very much like motherhood statements, but really aren't when you come right down to actually trying to implement them.

Recognize and evaluate all the uncertainties. We've heard a lot of talks yesterday about the uncertainties here. The fact that performance assessors begin to look at a couple of different conceptual models, but they usually select one to do analyses on and then all the mathematical uncertainty studies are usually done on that conceptual model, that set of data. There are other conceptual models every bit as defensible that should be looked at in order to provide the context, the total picture, of what the possible performance could be. Because what happens when you publish this one conceptual model and begin talking about it and label it as your base case or your nominal case is that everyone looks at it and thinks that is the expected performance. It may not be, at all. It may, in fact, be a very conservative, perhaps even extreme, performance scenario set up for the purpose of doing some calculations. Often performance assessors make very conservative assumptions simply because it's too difficult, too controversial to try and quantify some of the processes. So, they take the extreme end. They pick the extreme data, the extreme process, for the sake of conservancy. These things build up and build up and build up. So, what you're actually looking at in some of these assessments are scenarios that have very, very low probabilities and you need to keep this in mind from a regulatory context. You don't want to be regulating on scenarios that are extraordinarily improbable. Your assessment should be as realistic and practical as ever. Again, this comes right back to looking at alternate conceptual models. And, avoid extreme assumptions to simplify your calculations.

Yesterday, we heard a lot of discussion about what these uncertainties were and a lot of the points brought up here hit directly at those things. If you looked at, say, the VA assessment, you had an extraordinary conservative performance scenario. So, what you were actually looking at there was a low end, not a base case. To give you a little bit of anecdotal information, when the VA assessments came out, we had our technical contractor burrow down into the assessments to try and find out what the assumptions were and what the conservatism in them might have been. The answer came back that the VA assessments could be as much as eight orders of magnitude higher than what you would actually anticipate. So, again, if you were to look at these things in a more realistic way, you would look at also scenarios that touch on things more conservative than what you've actually done.

And, that's it.

WONG: Thank you. Questions from the Board? Ladies before gentlemen, Priscilla first?

NELSON: Right. Motherhood and fatherhood, both important. Nelson, Board. Can you tell me about your thinking about placing this discussion in the context of Yucca Mountain specifically with its proximity to the Nevada Test Site and the existing character of the groundwater and the site in terms of maybe existing millirems that are already there or could be expected from other sources?

CZYSCINSKI: That question comes up a lot. Does this standard consider the exposures that may happen in the long-term from the other activities of the test site? We've kind of addressed that in two ways. One is the simple way that we were directed by Congress to set a standard for the releases at Yucca Mountain. We can't take that mandate and expand it to exposures from the entire Test Site. That's not what

Congress told us to do. So, the simple answer is, well, we can't do that because Congress told us not to. The more detailed answer is in the radiation protection community 100 millirems has kind of been identified as the maximum level of exposure that the public should have from what's called practices and that the releases from an individual activity should be a number smaller than that. So, the fact that we have set the individual protection standard at 15 millirems is consistent with that kind of approach. And, we also believe from looking at the data that a lot of the releases from activities at the Test Site are going in different directions. They're not all heading down Forty Mile Wash toward Amargosa Valley. The combination of Congress telling us this is your job, Yucca Mountain Repository, period, setting the standard at 15 millirems, not 100 millirems, and the fact that not everything at the Test Site comes roaring down toward Amargosa Valley, kind of addresses this, we hope.

RUNNELLS: Runnells, Board. Ken, I think you almost answered the first question I have and that is--or came close to Priscilla's question--natural background radiation not related to the Test Site, but just natural background radiation. Does it play any role in the setting of this 15 millirem number?

CZYSCINSKI: The 15 millirems is for releases from Yucca Mountain.

RUNNELLS: So, it's independent of whatever natural background may be at Site A, B, or C?

CZYSCINSKI: Right.

RUNNELLS: Okay.

CZYSCINSKI: Natural background does play into some of the MCL limits because they were set in a very different context, but when you look at the releases from Yucca Mountain, what actually gets to your receptor is really beta photons and natural background on an iodine 129 and tech 99 is virtually nothing.

RUNNELLS: Okay, good. Thank you. Another quick question, I think. The model that you use in the maps that you showed us of the plume, is that an independent groundwater model that you have developed or are you using DOE's model?

CZYSCINSKI: No. This is a particle track trace taken out of the VA document.

RUNNELLS: Okay.

CZYSCINSKI: And, if you look at the SR documents, the same general shape shows up.

RUNNELLS: Gotcha.

CZYSCINSKI: Now, we looked at this diagram to kind of give us an idea of what the whole plume would look like. We did a little estimation of how the plume would increase, how it would get bigger as a function of dispersion. That's the first graph I showed you. It was only a particle track and this one has some dispersion built into it.

RUNNELLS: We heard unofficially from Abe yesterday about the unofficial conclusions of the International Peer Panel that the groundwater model may need some work. That would ramify to your work, as well, I presume. You follow what the project is doing in terms of groundwater?

CZYSCINSKI: We are unfortunately kind of the poor sister in the business. We don't have a lot of money to create a completely independent performance assessment capability. So, we look at what DOE does, look at it, and critically what can we rely on, what might be a little fuzzy.

RUNNELLS: I understand. If I don't look at Dr. Wong, I can ask another question, okay? I'll avoid eye contact. This 10,000 year thing because these are long-lived radionuclides, metals like arsenic and chromium and lead and mercury are longer lived. They never go away. How does the 10,000 years come into something that is long-lived, but goes away, radionuclides, in terms of regulations the EPA uses for other metals at other sites that never go away? I mean, what's the rationale for a time frame for something that decays and--do you have a time frame for things like the heavy metals that never go away?

CZYSCINSKI: Well, you're talking about two very separate regulatory worlds, the radioactive waste world and the RCRA world, toxics and everything else. In the RCRA system, the way you dispose of something is to put it in engineered barriers within engineered facilities that are projected to last relatively long periods of time. But, there is no long-term assessment of will a RCRA site remain intact for 10,000 years, 100,000 years. The regulatory philosophy is essentially to treat the waste, to put it into a form that's extremely--that's not mobile, or in the case of characteristic waste, to do something to the waste that removes that characteristic. So, you treat wastes and then you put them in these engineered disposal facilities in the RCRA world. In the radioactive world, we're putting spent fuels, a waste form. We're not treating that. We're putting it into an engineered disposal facility that we're requiring these long-term projections for and putting in a low dose limit.

RUNNELLS: I hear what you're saying, but I would argue that we are, in fact, treating the waste form here at the site and I see it as an internal inconsistency in regulations because the heavy metals last a lot longer. Enough said, though. Thank you for the extra time, Dr. Wong. I'll look at you now.

BULLEN: Bullen, Board. Thank you, Dr. Wong. Actually, I have a couple of quick questions and I'm going to show you my ignorance in not having read the Yucca Mountain standard report from the National Academy in the last week or so. The reasonably maximally exposed individual is different than the individual that was cited in the minority report of Tim's study.

CZYSCINSKI: Of the subsistence farmer?

BULLEN: Actually, could you just elucidate? I vaguely remember the subsistence farmer, but can you tell me the differences, please?

CZYSCINSKI: Well, the subsistence farmer essentially stays in one place, gets all of his food, water, etcetera, from that farm location. We looked at that idea. We said, okay, does it make sense for Yucca Mountain? Well, it doesn't. There are no subsistence farmers there. There's no real evidence that there ever were any. It seems unlikely that there would be any. From Future States assumption, we look at what's there now and what's there now is this rural residential RMEI that we've identified.

BULLEN: Bullen, Board. That sounds very reasonable. I guess, did you also do the calculation to determine what would be the maximum to that individual or was that something that you didn't carry though?

CZYSCINSKI: I don't think we actually looked in any detail on that.

BULLEN: Okay. I was just curious as to what the magnitude of the difference is.

CZYSCINSKI: We eliminated it as an extreme approach. It's not consistent with the rules of the game, as it were, addressing what's there at Yucca Mountain.

BULLEN: Okay. Moving on to the human-intusion scenario, you mentioned that you have to evaluate this when the containers begin to fail. I suppose in the strictest sense of the term, the containers begin to fail the day you put them in the ground because the oxide layer starts to grow. Can you quantify it a little bit more succinctly than that or--I mean, I know significant degradation, half the waste package gone or--

CZYSCINSKI: Well, when that happens is essentially an implementation detail we've left to the DOE. DOE has to prove when that intrusion could happen. It's up to them to say, okay, we think under the expected conditions, the cans will deteriorate to the point where they could be penetrated without obvious awareness of the drillers at some point in time.

BULLEN: Okay. Well, Bullen, Board, again--

CZYSCINSKI: We can't really set that because it is a function of the waste package and we don't control the waste package. So, we have to punt on that and say, okay, it's DOE's responsibility to identify that time. It's NRC's responsibility to determine if it's adequate or not. We really can't do that.

BULLEN: Bullen, Board. Just one more quick one here. I guess I was intrigued by the 300 square kilometer site determination and then I was very intrigued when you started changing dimensions on it when it was 350 kilometers that said, you know, you can sort of lop off this lobe and that lobe. I also understand the logic of using the Nevada Test Site as a reasonable boundary, but what's to stop the Government from deciding that the Nevada Test Site actually is a lot longer and narrower and extends down to Death Valley and basically buy all the land and make it the Test Site. Does that have any impact on the type of standard that you set up or is the 18 kilometers what you've decided and that's--

CZYSCINSKI: Oh, well, we've set this as a boundary as it exists today.

BULLEN: Okay. One final quick question and that deals with sort of your Slide #35. It's right here. Cautious, but reasonable expectations. You mentioned a couple of things with respect to taking a look at the long-term dose and the uncertainty being confounding to the decision makers. So, as you look at this reasonable expectation, did you come up with some sort of quantification of it? Is that sort of 2 sigma on the mean or is it a 6 sigma determination or--

CZYSCINSKI: What you're asking for is what we consider an implementation detail on the performance assessments that we have to pass this off to NRC.

BULLEN: Okay. I guess, it follows on to we were quoted back from our letters to the DOE by Bill Boyle yesterday talking about the acceptance of certain levels of uncertainty with decision makers and I just wondered if you'd quantified that. I mean, if you had, that would save us a lot of time.

CZYSCINSKI: No, we really couldn't because that's not our purview. We identify that or consider that to be an implementation detail.

BULLEN: Okay, thank you.

CZYSCINSKI: If you look at our WIPP regulation, some of those things are in there. That was an implementing regulation.

KNOPMAN: Knopman, Board. Ken, would you clarify EPA's role now in compliance? Is there a role, at all, or is everything now in the hands of the NRC in terms of pre--if there is to be some development at the site, then in the preclosure phase, as well as post-closure, does EPA have any role in monitoring and what precisely is it?

CZYSCINSKI: We will certainly look and monitor what's going on. We will remain an interested party. We're commenting on DOE's documents, but the responsibility for issuing a license is NRC's; it is not ours. They implement our standard.

KNOPMAN: I know. I understand--

CZYSCINSKI: So, we believe we don't have a role in that sense.

KNOPMAN: Well, I understand the licensing role of NRC. The question is in the compliance or the groundwater protection standard. Are you involved in discussions with NRC and or DOE on compliance monitoring of any kind or the confirmation testing or anything that might give you indicators of whether the standard is, in fact, being met, not just obviously--not the distant 18 kilometer compliance point, but within the controlled area?

CZYSCINSKI: We do not have the legal authority to do that. We do speak with DOE. We do monitor what they do. We try to understand what they do and the reasons behind it. But, our role stops at writing the generally applicable standards in the environment. Inside the Test Site boundary, inside the repository itself, we don't have the legal authority to step in there and make things happen.

KNOPMAN: Even on the groundwater standard?

CZYSCINSKI: Yeah, groundwater standard will be implemented through the NRC regulation.

WONG: Okay. I have two time devices up here in front of me and they have two different times. Decision-making under uncertainty. I'm going to allow questioning to continue, Dr. Cohon. Okay. Dr. Parizek?

PARIZEK: Parizek, Board. On the figure that's the colored one, the second viewgraph you showed, it looks like that's a plume from the TSPA-VA '98 or is that sort of a broader one in terms of possible pathways that--

CZYSCINSKI: Actually, the figure we took out of our presentation to the TRB and I don't remember exactly where it was. I think it was in between the VA and ESR.

PARIZEK: Well, another thing. In terms of the compliance boundary box, could that shift or is that fixed? Would there be four stakes in the ground and that will not move?

CZYSCINSKI: Well, it's really up to DOE and NRC to define the actual shape of the controlled area. The only thing we're saying is that its size cannot be greater than 300 square kilometers. If the DOE wanted to put the controlled area one kilometer on each side of the repository footprint, they could do that.

PARIZEK: What I'm allowing for is if the flowpaths on the basis of Nye County drilling and new updated information happens to be different than shown here by the particle tracking diagram, then they could still fix the position is what you're saying?

CZYSCINSKI: Yeah. Well--

PARIZEK: It's up to them to fix the position.

CZYSCINSKI: The only thing that's really fixed is the Nevada Test Site boundary. These other dimensions are up to

DOE to define.

PARIZEK: The particle tracking figure looks like the wells that would support the farms aren't pumping. This looks like a steady-state flow particle tracking.

CZYSCINSKI: Yeah.

PARIZEK: Now, if, in fact, you turn on pumps to give you the 3,000 acre feet to sustain the farm, it would be diversion of flow. So, any one of the farms could actually get the plume, divert it, or some portion of the plume diverted. How does that factor in here? You're saying there's only really a couple of hits by that configuration, but I'm saying many farms could get hit if they have to be in any position. You just turn on the wells and pump.

CZYSCINSKI: Again, that sets you up for all sorts of scenarios you have to create and evaluate and make some judgment on, none of which are--they're all speculative, none which are any more defensible than any other. So, for the sake of a cautious, but reasonable approach to public health protection and trying to get some sort of regulatory consensus, that's why we put the RMEI up there at the test site. That's why we use an RMEI rather than a critical group farm scenario because of all the assumptions you would have to make to actually try to apply the farm critical group and all the variations that you could have, I think, equally defensible about that. You'd essentially be arguing about it within a licensing context maybe forever.

PARIZEK: That sort of relates to the 3,000 acre feet withdrawal amount. I'm not sure that would be the number that Nye County farmers or Amargosa farmers would restrict themselves to if they were allowed to take more water out of the system. Again, that's a local debate.

CZYSCINSKI: Well, they do. I mean, the water would draw down here in the farming areas in the tens of thousands. So, our attempt to be protective is we're saying, okay, you protect 3,000 acre feet up here by doing that when making sure that that location meets the MCLs. You should be meeting the MCLs down here in the wider area. You know, considering the outflow of the entire basin is estimated about 8,000, you're not sacrificing the entire basin to contamination; you're focusing on where you should be focusing on for protective approaches, the plume itself.

COHON: Since the acting chair is distracted, I'll take over the chair and call on myself. This is Cohon, Board. I want to pursue the 10,000 year time limit a bit more. I don't want you to rehearse again all the arguments you used for using it, but as we know, the National Academy of Sciences recommended using the time at which peak dose occurred. What's your argument against doing that? I mean, why did they recommend that and how did you reject those arguments?

CZYSCINSKI: Well, it's hard to predict what was in their head. You can only go from what was on their papers. They seem to say that you could believe that the geologic conditions at the Yucca Mountain site would remain relatively stable for a period of about 1,000,000,000 years allowing the performance assessment projections to be bounded. They said since you can do that, you can sit down and you can run your models, put numbers in there, there's no specific scientific reason to stop at 10,000 years. When we look at that, we say, well, licensing is more than just running performance assessment models. It's this consideration of all these complex uncertainties and making a decision how much is enough. And, you really defend one particular scenario versus another. We look at these long-term uncertainties and say we really can't say that there is the only--there is only once scenario, there's only one performance assessment that we need to look at. While the conditions could be bounded, the probability--they didn't touch the probability of that, at all, which is the big question. When we put up one of these performance scenarios, what is its actual probability?

COHON: Yeah, but the logical inconsistency of that is that one infers from that statement that somehow you're confident about estimates at 10,000 years. I don't get it.

CZYSCINSKI: If you accept it, you're not confident about the probability of 10,000 years which is certainly plenty of uncertainty. As you go out even further, those uncertainties get higher and your confidence goes--should go lower and lower.

COHON: Yeah, but you're on quicksand. I didn't ever hear you quantify the confidence or provide the standard for confidence.

CZYSCINSKI: We don't think you could. Those long-term assessments, we don't know what they are.

COHON: You did implicitly, that's my point.

CZYSCINSKI: Excuse me?

COHON: You must have done it implicitly to write it 10,000 years.

CZYSCINSKI: Well, we've done it--the 10,000 year again is something that's been ensconced in existing regulations, as well as international--

COHON: Well, that's arguing that you--

CZYSCINSKI: There's a consensus there that 10,000 is long enough to look at this.

COHON: You did it because everybody else is doing it. That's the argument.

CZYSCINSKI: We have a consensus that exists in the rad waste community for years that 10,000 is long enough to do these assessments, the point of making a regulatory licensing decision.

WONG: Thank you, Ken. Thank you very much.

We have two questions and I apologize to Sally. So, she'll have to either ask the questions in public comment or pass them to Ken and he can answer them for you.

We have Russ Dyer who wants to do an announcement.

DYER: Thank you. I just got off the phone with Lake who has been in communications with the Secretary's Office. Because of all the things that are going on and our attentions are focused elsewhere, we're postponing the public meetings in Pahrump and Amargosa Valley, it looks like, for two weeks. We're still working on the logistics. We've notified the press. There should be an announcement coming out this afternoon, perhaps this morning, in some of the news media. We'll keep you informed as to what the logistics are for the meetings.

For the Federal Employees here, I've declared administrative leave for all Federal Employees today. We're talking to Ken Hess. I'm not sure what the ESC team will do, but if you're expecting some logistic support from the project, plan on working with what you've got. Okay? Thank you.

WONG: All right. We will continue our next presentation. It will be from April Gil. She's the Team Leader for Regulatory Interactions and Yucca Mountain Project. And she will be providing a presentation on plans for addressing key technical issues.

GIL: Thank you, Dr. Wong, Members of the Board. I'd like to thank you for the opportunity to speak today about DOE's plans to address NRC's key technical issues.

This is an area that we have had considerable progress and considerable effort applied over the course of the last year, and there's people here in the audience, I think I've seen a lot of you at these key technical issue meetings that we've had with the NRC over the course of the last year. I know that many members of the board and your staff have attended these meetings. And, I'd like to recognize Carol Hanlon and Tim Gunter, who have both been so instrumental in making these meetings successful.

My presentation today is divided into three parts. It's process-oriented. I'm going to talk about the process that the NRC has set up for the key technical issues, or KTIs. Then let you know what the status of our agreements is and talk about our future plans to address the KTIs.

The NRC is responsible for sufficiency review as outlined in the Nuclear Waste Policy Act, which requires any recommendation that the Secretary makes to the President to include the preliminary comments from the Nuclear Regulatory Commission. We have formally requested the sufficiency comments from the NRC by November 1st, and they have indicated that they will be able to provide those comments on schedule.

NRC has also made very clear to us in a series of interactions that their sufficiency review is going to be based on Proposed Part 63, the available issue resolution status reports that they have prepared for each key technical issue, and acceptance criteria for each KTI.

Some years ago NRC reorganized their High Level Waste Program to look at a series of topics that were important for post-closure repository performance and they characterized these as key technical issues. And, I believe they've made substantial progress toward evaluating each of these KTIs that progress is documented in the Issue Resolution Status Reports for each KTI.

There are nine KTIs that are listed on the rest of this slide and the next one: Igneous activity, structural deformation and seismicity, evolution of the near-field environment, container life and source term, thermal effects on flow, repository design and thermal-mechanical effects, unsaturated and saturated flow under isothermal conditions, radionuclide transport, and total system performance assessment and integration.

The general approach to address each of these is laid out in public meetings, technical exchanges/management meetings and these are defined in the DOE NRC procedural agreement, which is the bilateral protocol that states that

DOE can make commitments in the management meetings. The NRC has split each of the KTIs into sub-issues. The status is determined by the NRC. They make this very clear at the beginning of each meeting. Jim Anderson, who is their project manager for this area, reads out a statement at the introduction and objectives for each meeting notes that the goal is to reach resolution on all issues such that sufficient information is available on any issue to enable the NRC to docket a proposed license application. And this is in accordance with the requirements of the Nuclear Waste Policy Act.

In general, there are three categories for KTI status. Closed is when the NRC believes that the DOE approach will acceptably address the NRC concerns. Closed Pending is when the DOE proposed approach, together with additional information that DOE commits to provide, will acceptably address NRC questions. And Open. And that is DOE has not yet acceptably addressed NRC questions or agreed to provide additional information identified by the NRC.

And I'm pleased to let you know that, as of last week, all of the issues are either closed or closed pending as a result of the igneous activity technical exchange which took place a week ago tomorrow.

Now, everyone, I believe, has a clear understanding of exactly what closed, closed pending, and open means. And that means resolution at the NRC staff level at this time. However, it does not preclude the NRC from re-raising an issue during their licensing review.

So the KTI process I think has been very effective. It has allowed all parties who are involved with these open meetings to determine whether sufficient information is currently available for DOE to prepare a potential license application. The NRC has identified where additional information is necessary and we're able to assess progress against our plans and agreements and put in our planning to provide the additional information that the NRC has identified that they will need. So that's the overall process. Let me just bring you up to date on the status of KTI resolution.

We have reached agreements, as I stated earlier, with NRC for a path forward for closure of all 37 of the KTI sub-issues. There are 292 KTI agreements, and I have a list here. If anyone is interested I'll be happy to share that with you. These agreements identify almost 250 documents that DOE has committed to provide, including additional work before submittal of a possible license application. So far we have sent 67 documents to the NRC. This is a feedback process. At the meeting DOE and NRC agreed to what DOE needs to provide. We do so under formal cover letter and then the NRC evaluates the information that DOE has provided and provides us the feedback on whether or not they agree that the issues have been resolved. So far we have gotten four letters back from the NRC, so it's early in the feedback loop process.

We have formal correspondence that keeps the NRC apprised of the status. In addition, on a quarterly basis, we meet for management meetings, QA meetings, and we also have a KTI status breakout session--we just had one of those on Thursday of last week--where we provide the status of all the issues. And, we expect to provide an adequate response to all the NRC issues by the time of a possible license application.

There are different categories of KTI agreements and what I've attempted to do here is just kind of put them into general categories, the way we look at these for our planning processes. Probably the simplest ones are those where we have the technical information available but we need to provide it, the documentation or clarification on work that we've already done to the NRC. There are areas where we have the data available, analysis needs to be done and documented. And then there are a few areas where additional testing and analysis is needed on the part of the department to address the KTI. Each agreement is tied to a specific document with a specific date. And the KTI agreements are an important part of the department efforts with Bechtel SAIC to do the planning for FY 2002 and the out years. So we are making sure that each agreement is tied to a product, is tied to a part of our plan.

There are two areas I wanted to focus on, give you some examples of what the KTI agreements look like, and we have chosen two that are potential mechanisms that result in a calculated dose during the 10,000-year compliance period. And these are two areas that we thought might be of particular interest to the Board.

The first one is early waste package failures due to defects in a nominal scenario. Second is igneous effects in a disruptive scenario. And so what we have here is just some examples of the additional work that DOE has agreed to complete to support closure of these issues.

Under container life and source term we're going to do additional work on the effects of corrosion processes, and the effects of phase instability and initial defects on mechanical failure. Under igneous activity we're going to look in more detail at the consequences of igneous activity.

With more specificity here on slide number 12, you see for container life and source term the sub-issue on effects of corrosion processes. On container lifetime we will be doing some additional testing and analysis. In addition, this second bullet just shows you some more detail on the scope of this key technical issue and DOE's agreement.

On slide 13, a little more detail for you on the igneous activity sub-issue. As I mentioned, we were able to achieve a closed-pending status in agreement with the NRC staff and management last week, and these--the igneous activity going to closed-pending also made the TSPA sub-issues closed-pending as well.

So consistent with proposed Part 63 which is final if not nearing finalization--I see Bill Reamer in the audience--consequences of igneous activity must be evaluated since the probability of an igneous event is greater then 10 to the minus eight per year.

Focus of interactions with NRC are on consequence analysis for a low probability event. And I believe members of the Board staff attended the KTI meeting on igneous activity, if I'm not mistaken.

Page 14, a little more detail on igneous activity. We're going to be looking at soil suspension effects, doing some more technical work to establish and then defend our position with respect to this KTI. We're looking at effects of repository and contents on magma flow, response of waste packages to magmatic conditions, and the potential for incorporation of high level waste in magma.

So that's just to provide you kind of a flavor for the areas that DOE has committed to do additional work.

In order to support the NRC sufficiency review, DOE has had to demonstrate adequate progress towards meeting the KTI agreements to provide confidence that we will meet the commitments that we have made, and also adequate plans and progress for resolution of quality assurance implementation issues, which I know most of you are very familiar with.

The intent of the KTI agreements is to insure a complete application. We believe it will also facilitate NRC staff acceptance and review of that application. So in summary, I think the KTI process, although extremely labor-intensive for NRC and their contractor at the center, and for DOE and our staff as well, it has provided a very useful framework for pre-licensing interactions with the NRC. It is also I believe the first time in the history of this program where DOE and NRC have a clear understanding of the regulator's expectations for information needed to support a potential license application. So over the course of the last 12 months we've had, I believe, 17 of these interactions with the NRC, but I think we've made substantial progress. And DOE is committed to address all the agreements prior to submittal of a potential license application.

That's the end of my formal remarks. I'll be happy to take any questions that you might have.

WONG: Thank you, April. Questions from the Board? Dan Bullen?

BULLEN: Bullen, Board.

First I'd like to complement you on the interactions because I've been to a couple of these KTI exchanges and they are very intense and they come up--they take a lot of time and a lot of effort.

I have a couple of questions and you alluded to one because you mentioned that the igneous activity going to closed-pending also took some of the closest forms of assessment issues to close-pending also, and I guess could you illuminate a little bit more on that because I guess I was there four weeks ago when they were still open issues and I was wondering which issues got closed and why. And how? Could you give us a little more information on that?

GILL: Sure. It was the TSPAI, igneous consequences, I believe. Can anybody help me with this? And we held it open during the TSPA meeting with specifics in the meeting summary that if and when the igneous activity items came to closed-pending that they would automatically close out the TSPAI. So we had a forward reference to the igneous activity meeting with the positive expectation on the part of DOE that we would be able to reach closed-pending on that issue. And I can get you more details on that.

BULLEN: Maybe we could talk about that off-line because I'd like a little more detail.

GILL: Okay.

BULLEN: I have a couple of more, sort of more fundamental questions. Each of these deliverables that are identified in the resolution of these--of the status resolution reports identifies a time frame for delivery. And sometimes that time frame delivery is a year or two years. What's the range of delivery and is that delivery contingent upon the ability of DOE to do the work. I mean you'd have to have the money and the opportunities to do that. Could you speculate or maybe give us a little bit of information on what the current range of the agreements are and how firm those dates are?

GILL: Sure. The range goes from fiscal year 2002, which we're about to enter, all the way to submittal of a license application. And wherever possible where we have in the near term specific products planned with completion dates, we give those in the agreement.

Now, at the beginning, last August, when these KTI technical exchanges first started, we had tied a number of agreements to TSPA SR, or to analysis model reports and process model reports that we believed at that time would be completed last spring. However, since that time our program has had some major changes and the NRC has been very accommodating in reevaluating these agreements and tying them to specific product in the future. So it's with the--we go in with the understanding that these are DOE's good faith best estimates on our present funding level and the status of our program that we will complete these agreements.

BULLEN: Bullen, Board. You were still a little bit nebulous because you said up to the time of submittal of license application. Were there firm dates identified for them or did you leave it as open as that--that if license applications time frame slips from 90 days after the set recommendation which the law says, to whenever, could you identify when the "whenever" was, or you just left it at license application?

GILL: I think the latest date we have in there is FY 04. But a lot of them are tied to just before submittal of the license application without a specific date.

BULLEN: Thank you. One last little quick question with respect to the QA issues, with respect to trying to get all this done. If it is all going to be in support of license application then all the challenges that have been identified with respect to the QA program has to be addressed and also rectified? Is that correct, or--

GILL: Yes, and we have committed to do that, to have 100 percent data qualification at the time of license application. The NRC was understandably very concerned about the status of data qualification, software qualification and model validation because of exactly what you have alluded to, Dr. Bullen, and that's the 90-day link between sight recommendation and license application. And also the way the

Waste Policy Act states specifically the link. So they were very concerned about it, but it's for license application. I hope that answers your question.

BULLEN: Yes. Bullen Board, just one last issue. You raised all the issues that I talked about. You mentioned data qualification but also model validation verification and essentially all interfaces necessary to docket a license. Basically, all the QA requirements are going to be met then?

GILL: Yes, sir, that's correct.

BULLEN: Thank you.

WONG: Priscilla Nelson.

NELSON: Nelson, Board.

The question I want to ask has to do with which of these KTIs, or tell me how the KTIs and your response to them have captured flexible design, including low temperature operating mode. And have there been additional questions raised associated with the information to evaluate the low temperature operating mode?

GILL: Well, as a matter of fact, our last KTI meeting was scheduled for the end of this week and it was to address the range of operating temperatures. And the plan, Dr. Nelson, was to look at the KTI agreements that we had made which were operating under the hot regime and look at what effect introducing a range of operating temperatures to the design could have on those KTI agreements. And as of yesterday we had fully expected to have that meeting at the end of this week where we would go through specifically each KTI agreement and look at the potential effect having a repository design on the cooler end of the range would have on the KTI agreement. So that's kind of to be determined, but it was in our planning to do that meeting the end of this week. I don't know if the--what has happened today will have an effect on that.

NELSON: So there has been no discussion about low temperature operating mode before now, this coming meeting?

GILL: Well, I didn't mean to mislead you. We also had an interaction that was not part of the formal KTI process on the supplemental science and performance analysis report. It was about three weeks ago, where we walked through the SSPA with the NRC staff. And we talked at some length about the cooler operating mode at that time. So it was kind of an introduction to them so that this meeting at the end of this week on the comparison of the two would be more productive.

NELSON: Thank you.

WONG: Don Runnels.

RUNNELS: Runnels, Board.

April, just a question about communication. DOE has come under a lot of criticism for its mode of communication with the public. These are public meetings. How does the public know ahead of time that these are going to happen? And secondly, based upon the 17 that you've had so far, do you have public--non-technical public, non-DOE public, board public, etcetera, participate?

GILL: There are a number of ways that the public is made aware that these interactions are going to take place. And you are right, they are very public. We've had a number of them at casinos, in conference rooms. So some of the criticisms that the department has had recently about having the SR hearings at a DOE facility would not apply. Some of the other meetings have been at our Bechtel SAIC offices.

One of the ways that the public can find out about the meetings is the NRC announces these formally. There is letters that go out. They are also noticed on their web page. And at each meeting we usually look ahead at when the next meeting is going to be so those people in attendance at a previous meeting could tell about a future one.

With respect to the second part of your question on whether or not members of the public actually come to these meetings, I would say, unfortunately, no. We usually get these same representatives from the State of Nevada and Nuclear Waste--Nevada Nuclear Waste Task Force. Very infrequently, I would say, do we get just interested members of the public. It's usually people who have a specific interest or responsibility, such as the press.

WONG: Dan Bullen promises that his question will only last nanoseconds.

BULLEN: Bullen, Board. Thank you, Chairman Wong.

Actually, Dr. Nelson illuminated a question that I want to follow up on because you mentioned that taking a look at the lower temperature operating modes was one of the things you did with the NRC staff as you walked through the SSPA about three weeks ago. And I guess I'd like a little reconnaissance report on the NRC's response.

At our joint panel meeting in the summer, the NRC expressed some concerns that there be a design chosen for the license application and maybe even for the sufficiency requirement for the site recommendation. And, has there been--what kind of comments did you get as you introduced the low temperature operating mode, and was there consternation by the NRC in their abilities to identify the changes in the KTIs and to resolve the issues necessary prior to the sufficiency requirement, or were they not concerned?

GILL: I hesitate to speak for the NRC. I can tell you what my personal point of view was. I did not attend this meeting personally, but I was detailed into it.

BULLEN: Well, if Ed could give his personal opinion yesterday, feel free today, April. That would be great. I'm asking for a little bit of espionage here, so if you could just sort of clue me in because I couldn't make it to the meeting.

GILL: I would say it has been a challenge for us primarily because the only document where the range of operating temperatures is really fully explored is in the supplemental science and performance analysis report, which, as you well know, was prepared under our project quality assurance program; however it used data and software that has not been fully qualified. So the NRC is very concerned about the cue status of that document and the fact that if DOE is going to be basing some of its decisions on that document we would need to, of course, have everything qualified. So I think that's one of the--my primary recollections and area of concern from that meeting. We've got people here in the audience who were actually technical presenters at that, if you would like some more details, but I would say in general, the NRC has been very accommodating.

We've had a number of telecons to get ready for the technical exchange that was scheduled for the rest of this week. They had well over 100 questions on exactly how the potential to change the operating temperatures would impact the KTIs. So I would say they are very engaged and concerned about this. I'm not familiar with the specific technical issues that they are raising, but if you need more information I see Rob Howard is standing up to assist me.

BULLEN: Bullen, Board.

I can get this information off line. I just wanted to try and get a feel for the impact of this kind of change on the KTI resolutions, which I think is what you're trying to address. But, thank you very much.

NELSON: Now, I know what a nanosecond is and that was no nanosecond.

BULLEN: Dr. Nelson, you forget I'm a nuclear engineer. I can play with special relativity as much as I like, so I could define the time frame, however. Thank you.

WONG: You guys used up all Dick Parizek's time. Dr. Parizek.

PARIZEK: Parizek, Board.

In terms of sufficiency reviews I assume that the whole KTI process for closed-pending review would be similar to what's been done to date by NRC, you know, the things that are still to be delivered. You'll get a review and comment on sufficiency of new deliverables?

GILL: Well, that was the purpose of the KTI process.

PARIZEK: Yeah, and then therefore the license application we would assume would not get submitted until such time as all are closed?

GILL: Correct.

BULLEN: Go right ahead, Sherman.

WONG: Okay, sorry. Dr. Diodato?

DIODATO: I'll defer in the interests of time.

WONG: Okay. Dr. Metlay?

METLAY: Dan Metlay, Board staff.

April, just a real quick question. I'm not sure you answered this question when you responded to Dr. Nelson. How does the possibility of a lower temperature operating mode affect NRC's sufficiency comments?

GILL: Well, I would say that that's the purpose of the last key technical issue, technical exchange, that we're going to have on the range of operating temperatures because the NRC is obviously very interested in the potential impact that a possible DOE design change could have on agreements that were made based upon a hotter design.

METLAY: So, if I can just follow up. It's at least imaginable that an issue that was closed or closed-pending based on a higher operating temperature may become open given the possibility of a lower temperature operating mode?

GILL: Well, I'm not an engineer, sir. I'm a geologist, but--

METLAY: Concerned scientist.

GILL: Yeah. In my humble opinion, it seems intuitive, and I know there's danger in using intuition, that a potential lower operating mode would simplify things and not make them more complicated. I really can't speak to what the NRC is intending on doing, but we should know within a couple of weeks exactly what the answer to your question is. Unless the NRC would care to answer. There's representatives from the NRC here today.

WONG: Thank you, April. I'm not going to let Dan Bullen torture you.

GILL: Thank you so much.

WONG: Thank you. We are not trying to be time bandits here, but we are trying to keep a schedule. But there is an announcement now to be made by Rob Howard.

HOWARD: This is for all of the Bechtel SAIC staff. We are not to return to our office facilities today after this meeting, so once you are finished here today, go home. Don't go back to the office today. That's all the information I have. I don't have any information on what our actions are for tomorrow.

WONG: Okay. With that, we are scheduled to have a break. It is now 10:20 by my watch here. And we're scheduled for a 15-minute break, so since we have a gift of time, I'd like to see everybody back here in 15 minutes.

(Whereupon a recess was taken.)

WONG: I think the only person here that can read time is Scott Ford and Lake Barrett.

All right, so we're going to move on to the next presentation, which is going to be the Summary of Preliminary Site Suitability Evaluation. Mr. Sullivan is not here, so the presentation today will be provided by Carol Hanlon, and she is the Manager of the Site Recommendation Program, and again, she is with the Yucca Mountain Project. Carol?

HANLON: Thank you, Dr. Wong.

My name is Carol Hanlon and I'm here with you today to discuss some considerations in the development of a preliminary site suitability evaluation. The evaluation was released August 21st of this year. So in doing so I will discuss with you just briefly the basis for the site suitability evaluation, the preliminary site suitability evaluation summary, evaluation results and, finally, conclusions.

In developing the preliminary site suitability evaluation we not only were cognizant of our own departmental regulations and proposed siting guidelines, we were also cognizant, of course, of Nuclear Regulatory Commission's proposed regulations as well as Environmental Protection Agency's standard. That is required by the Nuclear Waste Policy Act. It requires, as Dr. Czyscinski discussed this morning, the Environmental Protection Agency to promulgate standards for protection of the environment in accordance with the Nuclear Waste Policy Act and the Energy Policy Act of 1992. The Environmental Protection Agency has issued final 40 CFR Part 197.

As Ken discussed this morning that establishes environmental radiation protection standards, including preclosure public protection standards, post-closure individual protection standards, human intrusion scenario discussion, and groundwater protection standards.

Nuclear Waste Policy Act also requires the Nuclear Regulatory Commission to establish requirements and criteria relating to receipt of high level radioactive waste or spent fuel. Waste Policy Act requires Nuclear Regulatory Commission to adopt and implement the EPA standards. These requirements and criteria apply to applications for authorization to construct a repository, applications for licenses to receive and possess spent fuel, high-level radioactive waste, applications for authorization and closure--for closure and decommissioning.

Proposed Nuclear Regulatory Commission's, or as I understand those which are about to become final in the very near future, the Nuclear Regulatory Commission proposed technical and licensing criteria for the Yucca Mountain Site to be codified at 10 CFR, Part 63, and they include radiation protection requirements for preclosure operations. Those are included in proposed 10 CFR 63.111, an integrated site--excuse me--an integrated safety analysis to demonstrate compliance with the NRC requirements in the Geologic Repository Operations Area through permanent closure period. And that is in proposed 10 CFR 63.112.

Also includes performance objectives, performance assessment requirements to demonstrate compliance with radiation protection standards after permanent closure, contained in 10 CFR 63.113 and 10 CFR 63-114. Includes additional requirements for licensing, such as retrieval of performance confirmation and so forth.

DOE proposed siting guidelines to be codified at 10 CFR 963 were proposed November 30th, 1999. Final rule is contingent on Nuclear Regulatory Commission concurrence. The proposed rule is based on technical requirements in Nuclear Regulatory Commission's proposed licensing rule. Proposed rule would also include preclosure and post-closure criteria reflecting processes and models that are important to repository system performance at Yucca Mountain. In addition, site suitability would be based on applicable radiation protection standards established by the EPA at 10 CFR, Part 197, as implemented by the Nuclear Regulatory Commission.

DOE's proposed preclosure suitability guidelines include a safety evaluation method that is consistent with the preclosure integrated safety analysis required in 10 CFR 63.112. DOE's regulations also emphasize performance requirements, analytical bases, and technical justifications and evaluations to assess the adequacy of design and safety functions. And we addressed applicable preclosure radiation standards contained in proposed 10 CFR 63-111 and 40 CFR 197.

DOE's proposed post-closure suitability guidelines include a method for conducting a total system performance assessment that is consistent with the method required in 10 CFR 60.114 (a) through (j). Requires the acquisition of field data, accounting for uncertainties, consideration of alternative models, and a structured method for evaluating features, events, and processes that might affect performance.

DOE's proposed post-closure suitability guidelines state that DOE will consider performance of the system in terms of the likely compliance with the applicable radiation standards. The standards include individual protection, groundwater protection, and human intrusion scenario.

As I said, in August of this year DOE issued Preliminary Site Suitability Evaluation to evaluate public review and comment on a possible site recommendation. It considers the--this document, Preliminary Site Suitability Evaluation, considers scientific investigations and preliminary design descriptions in the body of technical work completed to date, as summarized in the Yucca Mountain Science and Engineering Report, as well as the Supplemental Science and Performance Analyses document. It will be discussed later, by Rob Howard, this afternoon.

Suitability evaluation also provides preliminary evaluations of the compliance with DOE's proposed siting guidelines, and it addresses the EPA final radiation protection standard.

Preliminary site suitability evaluation has considered the Supplemental Science and Performance Analyses Report in terms of the evaluation of the significance of uncertainty and the degree of conservatism or optimism that was not quantified in TSPA-SR Rev 00 ICN 01, just for completeness. The evaluation--it also, the PSSE also addresses the evaluation of significant new information available since completion of that TSPA. Additionally, it also includes additional analysis of thermal dependencies to more fully evaluate effects of coupled processes and the thermal operating mode on system performance. That includes a comparative TSPA analysis using supplemental TSPA model over a range of possible thermal operating modes.

As I said, Rob will discuss the Supplementary Science and Performance Analyses in more detail. It has two volumes, Volume 1 focusing on technical work within each process model area, encompassing uncertainty quantification, updated scientific bases, and analysis of range of operating modes.

And, just for your information those subjects are organized in a manner similar to that found in the Yucca Mountain Science and Engineering Report.

Volume 2 documents analyses that provide insight into the effects on total system performance assessment, and the information in Volume 1.

Moving on to the suitability evaluation itself, there's four sections. Section 1 is an introduction, Section 2 contains the preliminary preclosure suitability evaluation, Section 3 is preliminary post-closure suitability evaluation, and Section 4, summary of the results.

In terms of conducting a preliminary preclosure suitability evaluation, we proceeded from the bottom box on the left-hand corner in evaluating structure systems, equipment, operator actions, looking at design basis, limits on operations, adequacy of facilities to perform their functions, hazards, event sequences, consequences and site characteristics, surface and underground facilities. That information was documented in a number of areas, including the preliminary preclosure safety assessment, the design documents and system description documents.

In addition, we looked at our ability to preserve the option to retrieve waste during preclosure period and that evaluation was documented in the retrieval equipment and strategy documents as well as system description documents. Those fed into our evaluation process to evaluate whether the site is likely to meet applicable radiation protections and standards and to consider the performance of systems in terms of the criteria. That was documented in the Preliminary Site Suitability Evaluation, as I said, Chapter 2.

And, the summary of the Preliminary Preclosure Suitability Evaluation looks at dose to repository workers during the preclosure period. It would fall below the limits specified both in EPA radiation protection standards and the formerly proposed NRC requirements. So it was below both. Dose to individual members of the public for normal operations and category one. Design basis events would fall below limits specified in EPA radiation protection standards and proposed Nuclear Regulatory requirements.

The next slide is a slide that shows the standards, the limits, and the preliminary results. I'll let you look at that in more detail at your leisure, but you can note by looking at the right-hand column that the preliminary results fall far below the limits in the standards.

Moving on to the structure of the preliminary post-closure site suitability evaluation beginning again on the lower left-hand corner, the process was developing process models and empirical observations which were documented in the Process Model Reports, as well as in the Analysis and Model Reports. Those were used both to provide the technical basis for the total system performance assessment models as well as to provide technical bases for the features, events and processes evaluations, and to identify and use data related to criteria.

That next box, the blue box, was documented in features, events and processes documentation screening, as well as model abstraction. And both of those fed up into the total system performance assessment SR where we conducted the total system performance assessment accounted at that point for certain uncertainty and variability in conducting sensitivity analyses.

Finally, moved to preliminary site suitability evaluation itself and evaluated whether we believed that, based on that information, the site was likely to meet applicable radiation protection standards, identified natural and design features which were important to isolating waste, evaluated post-closure suitability considering suitability criteria.

Just a bit of a schematic that indicates our process for looking both at the nominal waste scenario as well as human intrusion. In number 1, the TSPA, we looked at the TSPA without human intrusion, with the nominal scenario and disruptive scenario. We evaluated both of those against the TSPA projection and compared them with applicable standards.

In terms of the TSPA for human intrusion, we included in human intrusion we got, compared it with the TSPA projections for annual dose over time, evaluated against TSPA projections for annual dose over time and compared it with the applicable standards.

So moving on to the curves, you can see in this first curve that in comparison with the TSPA-SR information that we obtained from the Supplemental Science and Performance Assessment indicated that the releases began earlier, but were considerably lower than the projections in TSPA, and that is because of the new information we have. The refinement of the uncertainty discussion, and the earlier release of course comes from the fact that we have chosen to incorporate early failures.

Next slide shows the results of evaluating the mean concentration of gross alpha activities and total radiation, radium, excuse me, groundwater. And, of course, that comes from an evaluation against the EPA standards. This particular slide is for high temperature operating mode.

Next, temperature in the next slide is for lower temperature operating mode.

Moving on to the next slide, we have the projected annual doses for igneous activity. The bottom slide in black, the bottom curve in black, was that, from the TSPA-SR, the higher slide, the higher curve in blue, this blue over red initially showed the supplemental science and performance assessment modeling, shows that earlier because of changes to does conversion factors, evaluation of changes in wind speed, initial probability of eruption, increase in conditional probability of eruption and increase in total number of erupted scenarios, that is higher than was established in the TSPA-SR.

Moving on to the next slide, we have compared total mean dose histories for human intrusion scenarios with both human intrusions occurring at 100 years and at 10,000 years, and you can see that at the time varied--the time may vary, but the doses released are approximately very, very close and there are orders of magnitude, approximately three orders of magnitude below the EPA standard.

So, in summary of our results, summary of preliminary post-closure suitability for individual protection, the dose estimates from combined nominal scenario and disruptive scenarios both fall below the limits specified in the Environmental Protection Agency radiation protection standards of 15 millirem per year, as well as the NRC proposed post-closure performance objective of 25 millirem. Of course, that's what we had to work with at the time we were finalizing the Preliminary Site Suitability Evaluation that will now be conformed to EPA's. Groundwater concentrations calculated would fall below the EPA groundwater protection standard, and the human intrusion related release calculated would also fall below EPA radiation protection standards. And you can see those again in the next slide, Summary of Post-closure Dose Limits and Preclosure--excuse me, and Preliminary Results. And again it's broken out into standards, the limits, and the annual dosage, and you can see that that right column again is far below the limits in the standard.

So, in summary, Preliminary Site Suitability Evaluation documents a preliminary evaluation of the Yucca Mountain standard against criteria proposed at 10 CFR Part 963; reflects consideration of analytical requirements are consistent with the technical approach embodied in the Nuclear Regulatory Commission's proposed regulation, 10 CFR Part 63. It presents the results of preliminary preclosure and post-closure evaluations of suitability over a range of thermal operating modes, and it shows that the calculated doses fall below EPA's radiation standards and the proposed NRC performance objective.

That concludes my presentation and I'd be happy to take any questions or comments your might have.

WONG: Thank you, Carol. Questions from Board? Deborah Knopman?

KNOPMAN: Knopman, Board.

Can you explain why in the presentation of the Preliminary Site Suitability Evaluation Executive Summary there is no description of ranges of estimated performance, only point mean estimates after all conversation about uncertainty and presentation of uncertainty?

HANLON: I think I'm going to turn to Candy for that question, too, but also the executive summary itself was to hit the high spots, and explanations of the high spots. And, there are other places in the Preliminary Site Suitability Evaluation where those discussions are--is Candy here? Candy, did you hear that question? Or Rob?

HOWARD: This discussion--oh, this is Rob Howard, Bechtel SAIC.

These discussion's in the summary section weren't meant to discount all discussions we've had with this Board and other review bodies with respect to uncertainties, but the criteria was against the mean, not the range of uncertainty, so I think that was part of the point.

KNOPMAN: Part of the point is presentation of information to decision makers and the public. All right? And this is the key--key document that is communicating information about this site. I'm just bewildered. I'm sure there is an explanation. I know it's simpler, but--

COHON: This is Cohon, Board. I'd like to follow up on this and make it as forceful as possible. Could you go to Slide 24?

You are communicating to the Board--it said the dose estimates would fall below the limits. Doesn't say mean dose. I couldn't concur more strongly with Deborah. I mean it just--it is mind-boggling that you would make a presentation like this that does not acknowledge in any way the uncertainty associated with these estimates.

WONG: Further questions from the Board or its staff?

(No response.)

WONG: I have a question. It's related to the KTI documents. How will the KTI and all the information being reaffirmed or affirmed or generated be integrated into this set of documents?

HANLON: Basically, they are on a separate track. The key technical issues and the technical exchanges that are being conducted with the Nuclear Regulatory Commission are to address specific concerns that they have on performance points that they are very concerned, and they may have identified those as--in issue resolution, IRSR, Issue Resolution Status Reports. And so, based on those Issue Resolution Status Reports and the key technical issues that they are interested in, we've conducted the set of meetings over the last many months of preliminary site suitability evaluation. The next suitability evaluations are for the purposes of addressing the department's own proposed, and later final, guidelines. So they have two different purposes and they are basically on separate tracks. The information that's used for the KTI evaluations was and will be included in the suitability evaluations as they were addressed in the Science and Engineering Report, and considered additionally in the SSPA. But they are basically on two different tracks.

WONG: Okay, thank you. Dr. Bullen?

BULLEN: Bullen, Board.

I guess along the lines of a follow-up to this question then, will there be a reanalysis based on the new standards that's out from the EPA? I noticed that your groundwater standard is essentially for 1285 acre feed at 20 kilometers, and we just heard from the EPA it was going to be a 3000 acre feed at 18 kilometers, and so will you rectify the differences between--that's just an example, but all the other differences between the dose standards from the EPA and the NRC?

HANLON: I'm not sure what you're reading from, Dr. Bullen. But in fact, the Preliminary Site Suitability Evaluation was released after the EPA was final, so we were aware of that and we tried, to the extent possible within that time frame, to rectify it, and I believe we did conclude the 3,000, and we did an evaluation against the 18 or the NTS southern boundary. So, to the extent possible, we did at that time evaluate the differences and correct.

And Rob has something else he'd like to say.

HOWARD: In the analyses that were presented in the PSSE and in the SSPA, as you recall those analyses were being done at the time the standard was released, so the PSSE has sensitivity analysis at the process level. We did not do any calculation for 18 kilometers.

There was also additional sensitivity analysis for the different critical groups. We are doing additional analyses now to evaluate what the implications are of those standards, but they are not documented in the--the TSPA calculations are not documented in the PSSE or the supporting documents for those standards.

BULLEN: Bullen, Board.

As a follow up to that, will they be done before the SR, and in support of it? I mean this is a document as Jerry mentioned is going to be the basis for decision makers to look at. Will those--I mean you want to get everything self-consistent if you can hand the package to the secretary, right?

BROCOUM: Russ Dyer yesterday told you the (inaudible) basis for the SR was his presentation, but there's a few extra things that will be coming in as work goes on. The first is a letter report that's being completed right now which does the TSPA against the 18 kilometer exactly. What they did to the PSSE, they--when the new standard came out, they made--I want to use the word extrapolation. Would that be an accurate--they made extrapolation to 18 kilometers from the 20. But the actual TSPA calculations is being done to 18 kilometers, and that report will be out this month. It's a letter report.

The second thing, we're--in November we'll be issuing another letter report that looks through all the information we've collected since the SSPA and the SR issue and see if that has any impact on the clues we have reached, and that will be coming out in November. So those two reports will be coming out this Fall to supplement information done so far. Of course, the PSSE will be updated to incorporate the latest standards, including a final standard as soon as--when it becomes final.

WONG: Would you identify yourself for the record, please?

BROCOUM: Steve Brocoum, DOE.

WONG: Lake, do you have any comments?

BARRETT: Lake Barrett, DOE.

Let me try to explain a little bit about PSSE summary as it was in the front, and the frustrations that you have, okay? And we had some of the same, as we went through generation of that document. That document is very much a legal document. This is very much a legal document that has went through the channels, through the Court, etcetera. So, as we put that document together we had the lawyers very much involved, as well as ourselves. We are well aware of the dialog that went on in one of the previous meetings and explained to the decision-makers, etcetera. This is not that document. This document was primarily a legal document. We are working to find a improved way to communicate with the general public and certainly to decision-makers. So we recognize that this is not the end-all document. It is the Preliminary Site Suitability Evaluation. One that I support, I stand behind. It was not my first choice going into the lawyers regarding what we had.

Now, I believe there is going to be a way that we can address the uncertainties, address the range, and some of the frustrations I sense on the Board, in subsequent documents yet to come out. And, I would ask the Board's indulgence to try to wait a little bit. I know it's probably very frustrating as to how long this will take to do some of these things in many different areas. But that's really kind of what happened with this and that's why you do not see it in that document. But we are going to bring, commit to bring that across in a method that I just don't know what it is yet.

KNOPMAN: Jeff, may I follow up? Knopman, Board.

I went through the whole document and it reminded me of the volume 2 of SRCR, just looked like a compliance document, is the way it reads. It's not a narrative about how this thing is going to work and what we know, and--well, to some extent it's what we know. But maybe you could explain what the legal argument is, what the lawyers told you about why a range of estimates could not be included.

BARRETT: What--on any issue, not yet the non-issue. On anything that you've got to meet a standard, you know the lawyers would advise you and you know it's going to be challenged. Just say what you need to meet the standard and don't say anything else.

Now, in our view, we believe we meet the EPA legally-designated standard in the NRC 63 by many orders of magnitude. Yes, their own certainties, etcetera, as you all know very, very well. So as we engaged with the lawyers and tried to construct something here, we were not successful with it in the Preliminary Site Suitability Evaluation. That's all I can say, but they start off by saying, "Don't say anything extra because whatever you say will be used against you when you put in additional, quote, helpful information." And there are many things the lawyers will say on that issue, but when you start putting in helpful information, you've jeopardized a lot of things, so in the legal--in legal defense. But then again a public--public information aspect, these are competing goods, we say. And so that's what we're trying to wrestle with.

I recognize, and we all recognize that the PSSE, you know, does not do some things that we want it to do, but I believe it fulfills all legal requirements. I don't believe it fulfills what is your frustration and some of our frustration, but we're not done yet. This is a Preliminary Site Summary Evaluation. We do believe firmly that we do meet the standards and we should start the process, but it is not a good communication document. It is not what is our commitment, let me say, to decision-makers as we have made in many meetings before about communicating to ranges to the best of our ability. And that is not in the executive summary.

KNOPMAN: Lawyers were aware that this was the document. This was a key document that was going to be used for the basis of public hearings.

BARRETT: Yes. Yes, they are, and they looked at it from a legal defensibility point of view, and we needed that, okay? But nonetheless, the balance of this is, one, I do support the document. We support it, you know, but it was--it has some--we want to do more in certain areas and certainly you are right on one, that we need to do more on it.

COHON: Cohon, Board.

I'm sympathetic, believe it or not, to that response, having to deal with my own lawyers. The Board's job of course though is we don't have to worry about that, thankfully. So our job is to push on what we think are key technical issues, including uncertainty. One could debate whether the communication about uncertainty is a technical issue or not. I happen to feel strongly that it is a technical issue, at least related to technical practice. And therefore, this issue of uncertainty in communication to decision-makers and the public, I believe is something that the Board can comment on. And I, frankly, I think the lawyers, in focusing so strongly on EPA standards, are not being sensitive to this suitability hurdle that you have to get over; not well-defined, unfortunately, but that doesn't help us very much. But one thing the Board has communicated strongly to the program is that we believe uncertainty quantification and communication related to your performance estimates is key. And so, part of this may be related to the long-standing confusion that pertains to suitability versus licenseability. And the EPA standard really, and the EPA I don't think ever thought about suitability. And that--that wasn't their job. And so, I think that being so focused, and constrained by your lawyers to focus on the EPA standard as stated, really, it only addresses, in part, the suitability change.

BARRETT: When we issued the proposed revision to 10 CFR 963 back in the mid-90s we made a fundamental policy decision within (inaudible) to move--if this site was--met the duly, legally-promulgated environmental protection standards and health safety standards, you know, that based on the way the law was, that this would be a suitable site, so that policy became basically one. But we also recognized immediately that in the communication, and there was a report of public opinion type thing, that was necessary, but insufficient. And that we needed to also address the issues that you have driven on as well, so I think with these gentlemen, with what you're saying, but we tried to split this, the program, into technical sustainability, legal sustainability, and sustainability in accord with public fairness. And we're having difficulty struggling with the last one, with the first two. But we have some plans that we are working internally on to address the issues, and I guess--this is the preliminary evaluation. We are not done yet. And, you know, please await to see what we can do to try to rectify, not only this situation, but other issues as far as uncertainties and design work and things like that. So we are struggling. We really are struggling to try to balance the sometimes competing goods--and they are all good and they are all right, and we're trying to down-balance and progress them all fairly. So it is--I would just ask your indulgence to wait to see if we can do some more. We are doing more. I mean, now if it would be good enough you will have to judge when we bring that forward to you.

WONG: Alberto?

SAGÜÉS: Thank you. Can we look at the number 19, please? Also get through the schedule for the rest of the meeting, the agenda and I--we may not be seeing this particular curve too many times, maybe perhaps in connection with Bob Andrews' presentation tomorrow, but in a different context. So this may be a good time to bring this up, although it's a little bit peripheral to your overall presentation. But let's concentrate for a moment on the SSPA projections. And the red curve represents--they have the case, the blue color represents the cooler model. And what would happen if, because of, say a person's scientific ignorance, if that's all there is, indeed the localized corrosion mode that develops and there is great likelihood for it to develop on the high temperatures, and that of course is not contemplated in the provision because I understand that what we happen to call models at this moment, they do not consider localized corrosion development. And now there is localized corrosion development and there is widespread pitting that develops and it tells us that that pitting does penetrate through the two centimeters of C-22 in a period of time which is relatively short. A couple hundred years, something on that order. We have a big fat surprise because of not enough development in present science, and I, for one, think that that surprise would be more likely to have in high temperatures than low temperatures, the way localized corrosion tends to develop. How--what would that do to the projections if there are any--is there any likelihood that we will then be shooting up, all the way up to the (inaudible) one level, which I think would create the problem from expectations, the normal expectations standpoint, right? If that--how far would that be--

HANLON: Dr. Sagüés, I think Rob is going to take a cut at this right now, but I would suggest to you that in the detailed presentation he gives on supplementary science and performance assessment this afternoon, that might be a better place for him to take it up in detail. But is there anything briefly that you might want to say, Rob, as a prelude to this afternoon?

HOWARD: Yeah. The question is, well, I think it's how much waste package failure can the system tolerate? Is that a reasonable summary of the question? What happens to these curves?

SAGÜÉS: I guess you could say that. Suppose that you end up with widespread pitting developing, say some time during the hotter part of the period.

HOWARD: Right. Well, we have not done any calculations particularly with pitting and the characterization of what that failure looks like as far as how radionuclides would be either advected or diffused out of the system. The igneous intrusion analyses where you have on your, you know, 40 or 50 waste packages failing within a realization catastrophically would give you some indication of those curves shifting up several orders of magnitude. But again, the pitting--suffice it to say that as the waste packages fail the dose rates are going to go up, but to characterize that as far as EBS releases I think would be speculative on my part, just that it would go up--I couldn't give you a quantitative answer right now.

SAGÜÉS: So if--I guess I'm just trying to think about the curve in terms--maybe what is being said in a way is the probability of that happening is so small, I mean if you were to put it quantitatively to--probably we are wrong, you know, with theories of corrosion, say. If the probability were very high then that would result in a--that red curve would climb up.

HOWARD: Yes.

SAGÜÉS: Way up, and maybe even not get it in right compliance.

HOWARD: Right. I see.

SAGÜÉS: And in a way we could imagine at this moment there is a multiplier of zero with that probability in that particular model. And now, could it be possible perhaps to do a little bit of quantification of uncertainty by saying, okay, what is the chances that our corrosion scientists are wrong now?

HOWARD: Yeah, I--

SAGÜÉS: What is that kind of number to that?

HOWARD: What are the chances of our corrosion scientists being wrong? Well, I--put a probability to that?

SAGÜÉS: Right. There's more of the chances of zero because, you know, saying that now we're sure that local corrosion isn't going to happen, but that is a non-event.

HOWARD: Yeah. I don't think I can put a probability on the chances of our corrosion scientists being wrong, would be, I mean when I don't know what it is that I don't know I usually put a uniform distribution on it. Another way to get at the problem, I think that we're going to have to quantitatively, as these so-called barrier (inaudible) analyses that the--I have issues with that. I mean it's another way to slice the problem to different a thought experiment to get at what it means if they were wrong, so it gives you a consequence. But to put a probability on it, I have to scratch my head. Maybe I can give you an answer this afternoon.

SAGÜÉS: I see. I see.

HOWARD: (Inaudible). Maybe this afternoon then, you think because I really would like, you know, to--

HOWARD: I'll give it a shot.

WONG: Paul Craig.

CRAIG: Alberto, you've asked the right question as always. But I think there is a number. They have made implicitly the clear statement that the probability of any corrosion mode, be it localized or general, is below one and 10 to the 8th per year. That is to say below the level of regulatory concern. It's clearly implied by the way the analysis has been handled and presented.

SAGÜÉS: And suppose there is the one percent chance that the scientists are wrong? Wouldn't that--

CRAIG: Well, see what I mean, Alberto, it's how much they are wrong, right?

SAGÜÉS: Right.

CRAIG: So it's the probability that they are outside some range, and that goes to the (inaudible), the substance issue. It's not just a matter of being right or wrong.

SAGÜÉS: Yes, but we can say--

CRAIG: But Rob's point also is it's interesting to consider the consequences of being wrong. That's what you're suggesting?

SAGÜÉS: Yes.

WONG: Any further questions from the Board? Board staff? Thank you, Carol. Thank you very much.

With that that brings us to public comments, and I turn the meeting back over to Dr. Cohon.

COHON: Thank you, Jeff. We have two people signed up for public comments. Sally Devlin.

DEVLIN: Thank you. My name is Sally Devlin, and I'm the public from, well, Nye County, Nevada.

And I just loved all the presentations because it brought to the fore something that we, the public, are very conscious of. And when my friend, Mr. Jared Cohon, said I don't do anything with (inaudible) unless I talk to a lawyer, and this is what Lake was referring to. And I will iterate what happened at our NRC meeting in Pahrump some months ago. And they came down and they talked about the licensing. And they were effusive, offensive and obnoxious because they talked down to us. And when the question came up is how does one protest, and Larry told them how you do it. He said, "Will it cost $1,000,000, and he said yes.

And so it was not only offensive, but it really was discouraging because I know and I say it in my heart that the assumed uncertainties are so grand that this will be rejected because it will kill the people. And that's what I'm going to talk about today, about the cancers.

And the first thing I'm going to bring to you is, again, talking about EPA's standards and what have you. And I am no longer Sally Devlin, ignoramus and the public, but I am Loren Moy from Berkeley, Ph.D and so on, with the tooth fairy program. And unfortunately her car broke down so that we talked extensively and she tried to give me information. So just call me Loren Moy for a minute. I'm wearing a different hat.

But this is what she taught me. She said the EPA has standards for all the elements, and there are 117 isotopes of uranium and all this enormous volumes. EPA sent me a book with 2,000 of these things. Lovely. I'm delighted and I give it to friends who want to know what the 2,000 are. But for me it really doesn't mean anything because I am not a scientist and so this is what she gave me.

One was the gases, and I hope I'm saying this right, and I expect everybody to correct me when I'm wrong, is krypton. And, krypton breaks down into yttrium and strontium 90. And, what happens is, and these are in immeasurable quantities, whatever that means, and I'm assuming they are seen microscopically for a minute like A Argonne gas and stuff.

But anyway, what happens, and this is one of the theories on the cancers of Fallon, is something she taught me which is called pyrophorics. You shoot a bullet and the fire that comes off in the blast is in a colloidal state which never settles to the ground. And that remains in the air, and the colloids from these uranium bullets and other things of that nature--which, of course, I am not too familiar with, but you are--stays in the air and it gets in your orifices. And as she explained to me it only takes one cell or whatever they are--I'm not talking about my bugs till this afternoon--it is one cell to get in your eyes, your nose, your mouth, your ears, metastasizes--right word? Thank you. And you've got cancer. And this is something the board will get and I'll get it to Russ and I'll get it to Lake if everybody will cooperate. This is a report from Marion, Ohio, and it's from "Family Circle" 87-01, and this town has had 23 or so luekemias, cancers, and Fallon have been 14. And they built the high school on an Air Force chemical dump. And of course, it rains in Marion, Ohio. Is there anybody here from Ohio? Ohio, are you there? No? Okay, guys.

Anyway, this is the paper you're going to get. And it's terrifying. And so when I looked at all the stuff you shown, and I've watched for nine years, and we've all grown old together, I keep saying something about the future. And April did a brilliant program, and she mentioned the NRC staff. Well, I just read an R & D article on the NRC staff, and it's just like being married to Abe for the next 200, 225 years till they close that thing. And that is who is going to be here on the NRC staff that is continuous. They said in the article, "You're going to lose 40 percent of your staff."

Now, NRC I do not love because they are so snotty towards us in Pahrump, but--what's the most important thing is they are the inspectors. Who is going to be trained to inspect and oversee--maybe that's the proper word--on this stuff when there is no stewardship? What happens after the 225 years when Abe and I are gone? Where is the continuity and who is going to do it? Can you do it by computer when we talk about the robotics and we talk about the health of the workers? I gave it to you from the book yesterday. And it said seven, eight and 12 per thousand deaths.

Now, we get in on all this stuff and I'm--I don't mean to be equivocal about it, but again, I have to say that what's the most important thing to me is the health and welfare of this nation. And I have to give you a history lesson. And that is Nevada is the third largest state in the nation. 87 percent, give or take, is owned by the federal government. My friend has a map from 1930 on lambskin, and there were 30,000 people in the whole state. Now you've got almost 2,000,000 in Las Vegas. We're going to have 120 to 150 in Pahrump, and Magosa (phonetic) has a few, and so on. But we don't count because the government does this.

And, I showed you the article about the capability to make the germs. Anyway, it's really terrifying. And again, we get back into the water. When I look at the tests I do not see it any--the way you see it. For all these years I've said how can you? And that's why I brought that book on the 50th anniversary. You're going to see what went on in the test site. But what you're not going to see is that there are 20,000 airplane flights over the Air Force bases from Fallon to Vegas, to Nellis, that flies over the test site. You've got fuel dropping, you've got plane crashes, you do not see the Tonopah Test Range with all the uranium bullets and so on. That is next door and you are part of that 25 miles in the Tonopah Test Range. You do not see what comes out of Payute Mesa and Frenchman Flat and so on because they are above Yucca Mountain. They are not that far away. The 1370 square miles is just a small portion of what the feds own. Nye County owns the roads.

And, I just did a report to the PUC and to Pahrump and so on on the wind machines that they anticipate putting in. And I can assure everybody here I was coerced into doing it, and I was the least prepared, but as always I was the only one available, so of course I went into 20 minutes of testimony. Now, they are going to put 541 wind machines up there. Huge things that generate a million--1-1/2 megawatts. That's enormous. And they are going to get so big they can generate three megawatts.

Now, having been on the NRP Committee all these years and I see the water that we're measuring in Lake Meade that's loaded with PU and U-237, 238 and 242, and that's not nice. I don't know about the lead. But this is the stuff that is going on, and I see this project as it's whole. I do not see it as 25 miles of Yucca Mountain and you have separated everything and everybody from the test site. All the cores are out of the test site. All of this, all of that. And it's hidden from the public.

And then Lake, my dear friend, he's not going to be with Abe and me. You talked attorneys, who is the bottom of the barrel. And I mean that with all my heart. I have judges and attorneys in my family, that I know intimately. And they set the laws. We, the people, don't. And it's scary because you can look at law upside down, inside out, and backwards and interpret it as you choose. And this is not, in my opinion, for the benefit of the people. I regret the cancers which will be occurring. Remember your law, and that's what I want, the boundary map for Pahrump. You cannot be closer than 800 meters with a vehicle or rail car. You can abide yourself without the 800 meters. So that is my shot--yes, Gerry.

COHON: Time.

DEVLIN: Oh, okay. I thought you were correcting me. I'm so used to being evaluated.

Anyway, thank you, but understand my feeling about this. It is extremely negative and it really scares me more, and I'll just close with this in light of what goes--going on in New York and the Pentagon. Because I mentioned bioterrorism yesterday. We don't know, it is insanity, but it's there. And we have no one and nothing in Pahrump to handle it, or at the test site, or in the entire state of Nevada.

And where is Mr. Morgan Moskowitz? Is he here? Morey Moskowitz, is he here from the state? He can corroborate this. Thank you.

COHON: Now we will hear from Bill Vasconi.

VASCONI: My name is Bill Vasconi. I've been a resident of Nevada since '64. I notice quite a bit of the audience left, but that's all right because you are the folks I want to talk to. Maybe even ask a question or two.

You know, there is a good bit of Nevadans that don't believe they are part of this nation's nuclear waste concerns. But there's just a good many Nevadans that believe they may be the solution for this nation's nuclear issues and concerns for many generations to come.

Now, I'm a construction worker, and 17 years of that was at the Nevada Test Site. I came to realize that we have the technological and scientific expertise developed for over 50 years. Then we start attending our meetings on the EIS, you folks, NRC, EPA. We got those for and those against in the State of Nevada. Some of us have convictions. I'm one of them. I'm not college educated. But I depend on what I hear here at your scientific and technological meetings, because I need assurances that the way my heart and brain feels is right. Now, I know the antis that don't want to talk to me because of who I am. I've got several world organizations that don't want to talk to me because of association. I'm not paid to stand at this mike and address you people. I do take my time and read the articles. I read the articles that's written by other organizations, but I also know what you don't say. You don't say the site is illogical. You don't say the site won't work. You do have credibility. National Academy of Sciences nominees, Nuclear Waste Technical Review Board. This Nevadan is paying attention to what you do. This Nevadan is not humiliated or put down by crowds of demonstrators. This Nevadan realizes this is a national issue, not all a state issue, who this morning would like to see us dock our 70 atomic submarines because they produce spent fluorides. Our atomic aircraft carriers because they produce spent fluorides. (Inaudible) Snaring wants to close the reactors at so many universities and medical facilities because they produce spent fluorides. I (inaudible), I worked in radiation. I was a radiological technician monitor for a few years. My God, your levels are low enough. I hope everybody just don't jump on the band wagon. We could start cutting out mammograms, chest x-rays, no more (inaudible). Think about it. I think about occupational safety because I'm a construction worker. I hear a lot of estimates, a lot of guesses, a lot of limits. What's our occupational safety limits? Right now the way things stand that guy can drive that truck. Who talks about the guy driving the truck? He can drive that truck for three months. He don't have to eat, sleep or go to the bathroom before he reaches his occupational limits.

Yeah, we got county commissioners that don't realize when radiation passes you as a light in the flashlight it's not no longer there.

Paul Perkins, keep this in mind, and I don't believe in surveys, because you can write the damn things any way you want to. I don't believe in surveys, but you asked through a survey of UNLV what do you believe in most, your scientific community or your politician. Your scientists have won out by 96 percent. Politicians get credit for two. But they are hard to talk to. I stand with Nevadans that do believe, do believe, this is a national issue. But I also stand with Nevadans that are concerned. They are concerned about world welfare. They are concerned about issues like impact, mitigation. They are concerned about emergency response from their communities. I'm concerned about the economic development in Nevada and people in Nevada. We have businessmen who want to put railroad ties in a concrete rebar, a million of them for a north/south railroad system, to be utilized for economic development after the nuclear waste assault.

We got state senators who want to see transition of federal lands. We're 86 percent federal, because they want to build a sustained tax base for future generations to come. I see communities, rural communities, that want economic development. But keep in mind those rural communities, the railroad, the road, is going right through their town. The people of Nevada--Clark County is an example. Let's use

Clark County, 1.4 million people. They are not concerned with Yucca Mountain. If they were, they would have been at that fiasco they called a demonstration at the DOE facility. They're not the Nevadans. I was ashamed. Nevada can do better than that and so can a congressional delegation. They attacked the process, not the substance, and that's where you folks are at. You're the substance.

Yeah, there's a lot of Nevadans out there who want our university system vested in. They want research centers. But their wants are for ourselves and the concerns of the nation.

Thank you very much, for in the past I've talked to you folks and we said we don't want it closed. We want it monitored for water, for temperature, for radiation. We want the capabilities of extracting it if there is something wrong. Some of you listened. Some technical review board listened. I appreciate that.

Again, I'm not paid to speak. I'm not paid to stand here. I'm standing here because I want you to know there's Nevadans like me that exist. Put down your concerns are crime, water, waste, jobs, schools, and the amount of (inaudible). About number 14 is Yucca Mountain. Your concerned the fact that in Las Vegas, Nevada, there's a murder every other day, a rape every nine hours, a car stolen every 40 minutes.

When I go out in the world I just want 960 miles around the center of Nevada after Thursday's last meeting. I talked to a couple county commissioners, I talked to a few residents. I had to reaffirm my convictions that this is doable. It's a viable solution to this nation's nuclear waste concerns. But I need you folks beyond the EPA, NRC, beyond DOE, because DOE is a fly in your eye. They don't give a compliment from anybody trying to do their job right. Here is what is wrong with DOE. I'm not afraid to compliment. I don't want to find no faults with them. Because if I find faults I'd probably be on the other side of the mike saying we don't want it here. We don't want it here, but we know you're coming. There's equity issues, benefits that this state is entitled to. You keep doing what you're going. You make sense out of it. Don't forget to write it in layman's language. Some of your (inaudible) mouths will be just as clear as that (inaudible). But do give me assurances. Don't be like DOE, don't be reactive, be proactive. (Inaudible) what's going on, and I'll carry the message.

I want to thank you. If there's any questions you want to ask, now is your chance to do it. I don't care what it's about--transition of federal land, how many nuclear devices was detonated on that Nevada Test Site. Or one, I'm going to shut up. (Inaudible) again. Thank you very much.

I hope we meet again, and my heart and soul goes out to those that--what's happened this morning. I hope we meet on a better day. Thank you much.

COHON: Thank you, Mr. Vasconi.

We now stand adjourned until 1:00 o'clock. Thank you.

(Whereupon, a recess was taken.)

WONG: --provide an update from the project on science, and the supplemental science and performance analysis. Our first speaker is Dr. Mark Peters from the BSC, Los Alamos National Labs. Dr. Peters?

PETERS: Can you all hear me okay out there?

SPEAKER: Yeah.

PETERS: With the events of today I'd kind of like to laugh to take my mind off the events, and I noticed that I have till 2:30 in the morning to give this talk, from the agenda. So bare with me. Rob, you're going to have to wait for a while, if that's okay.

HOWARD: Mark Peters is pretty good.

PETERS: You're good at questions (inaudible), even better.

What I'm going to do is today, thanks again for having me back, give you all an update. I think what you've gotten used to seeing over the past several meetings on where we're at with the Scientific and Engineering Data Collection

Program, the testing program--if you hear me talk about testing and also saying data collection, that's maybe semantics, but it gets back to a comment that Dr. Sagüés made at the last meeting about, "Hey, everything isn't a test. In some cases we're collecting data for parameters, in other cases we're using it for validation." So I'm going to try to sprinkle in here more specifics on why we're doing particular testing or data collection and also what it's telling us about our models. If I don't catch all the ones that you all are interested in, please ask me in the questions. I'm going to try to sprinkle that in. But it's structured in a very similar way when I've done previous presentations.

I'll start with the unsaturated zone, talk about the drift scale tests, spend some time on chlorine-36 validation, which I know is of much interest to the Board. A brief update on fluid inclusions work. You heard a lot about fluid inclusions at the meeting in Arlington in May. This will be very brief. Then move into the cross drifts, still focusing on the unsaturated zone, the crossover alcove, seepage tests in Niche 5, as well as borehole based seepage tests. And then another item that I know is of much interest to the Board, the bulkhead investigations in the ECRB. An update on where we're at with Busted Butte. The field work at that test is now complete and we're in--pretty much finishing up analysis and modeling of the test results through this year and into next.

I'll move into the unsatura--or the saturated zone, excuse me. Talk about some of our cooperative work with the Nye County drilling program. I will not steal Dale's thunder. You're going to hear quite a bit about the NYE County program I believe later this afternoon. Move into the alluvial testing complex. All of this you've pretty much heard about before. This will really be updates on previous information.

Moving into the engineer barrier system, some of the testing that we're doing at the Alice facility in North Las Vegas, the ventilation tests, as well as a brief mention of the construction phase of the natural convection test that's going on over there. Talk some about thermal conductivity measurements. I know there's interest in that. These are--I'll focus on the field based measurements that we've started in the ECRB, and then about three or four slides on waste form. I'm--you'll notice waste package is missing. I'm assuming that was covered in great detail yesterday by Gerry Gordon so that we're not going to go over that at all at this presentation.

A diagram that you've seen before. Again, I'm going to start with the ESF here, is the exploratory study facility with the cross drift. North is in this direction. This is the primary potential repository block. I'm going to talk first about results from the drift scale test here in Alcove 5. Move into talking about chlorine-36 validation. There we're looking at samples from both the drill hole wash area, fault area, as well as the Sundance Fault area down here by Alcove 6. Fluid inclusion work, of course, covers samples from throughout the ESF as well as the cross drift. I'll talk about the cross drift. I have a more detailed map later before we get into the cross drift section.

First, the drift scale test. You've seen this diagram before. Just to remind you how the test is laid out, the observation drift, the connecting drift, the heated drift with the wing heaters, 25 on each side, and remember we also have the nine large waste canisters inside the drift with electrical heaters.

The primary purpose of the drift scale test is to evaluate the thermocouple processes. Here we're after competence building in our models--validation, if you like that word.

It's--in terms of boreholes, again, we have the boreholes that come up, the observation hearth both above and below the drift, heated drift, and then of course a lot of temperature mechanical measurements within the heated drift itself.

An update on where we're at. As we've been heating since December of '97, I'll talk about the heating phase of how we're going to handle the end of the heating phase later, a couple slides down the road. This is showing the power. Remember, we started at about power here on this Y-axis. This is a function of time. At close to about 190 kilowatts. This slide shows we've turned the power down four times, we've since last week turned it down a fifth time to maintain the 200, approximately 200 degree C at the brick wall. So this is just to update you on where we're at. You can see it was starting to climb here so we have since again turned it back, power back one more five percent increment.

Some temperature plots. These are along horizontal boreholes about half-way down the drift that run along the plane of the wing heaters, or just above the plane of the wing heaters, so that's why you see the humped profile because remember the wing heaters are segmented, they have an inner element and an outer element. Just to give you an idea of the peak temperatures that we're seeing out in the rock near the wing heaters are upwards of 250 degrees celsius.

In terms of the measurements, temperature--we've compared temperature measurements to our predictions. We've talked in previous meetings about predictions of--where the water is going to hydrologic predictions, and also chemistry. I'm going to focus a little bit today on the temperature and the hydrology.

In terms of temperature, we've done a lot of statistical analysis of our measurements. First is the predictions--pretest predictions, and find in the mean error that almost all of our sensors is within a few degrees C. You do see some local effects, hydrologic effects in terms of temperature signal in some of the temperature sensors, and that local heater in 80 are primarily drains and fractures is what we're interpreting to produce some of those systematics.

Hydrology, in general, we do, as you know, geophysics using different techniques--logging, radar, resistivity techniques, as well as air permeability to look at changes in fracture saturation, and in general they corroborate well with the redistribution of the moisture. We've done some statistical analysis as well in a more quantitative sense. But I don't really have any plots to discuss that in any great detail, but in general, the statistical analysis corroborates that we're doing a nice job of predicting where the water is going.

This is just one example, again from borehole 160. One of these horizontal boreholes, about half-way down the heated drift just above the plane of the wing heaters on the west is temperature versus time for measurements and on the right is the simulations. I didn't want to put them on the same plot because it muddies it up, but this is a function of distance down the borehole. If I overlaid these you'd see that they are well within--they are within a few degrees of the predictions and what we actually see in terms of the measurements.

What about the cooling phase? We started the heating phase, again in December of '97. We had always planned on a four-year heating phase. We've recently evaluated primarily at the thermal test workshop that we had here in June, we remember that a lot of the big drivers for the four-year heating phase had to do with the chemistry. We wanted to have enough time to bore enough water away from the dryout zone, maintain it in the condensation zone, and get enough time for kinetics to take place so that we could see real changes in water chemistry and potential mineralology infractures. We discussed whether there was any value in extending the heating phase to continue to meet those objectives.

The determination of the scientists was we had met the objectives that were necessary so right now the plan is to begin cooling at the end of the four years. So as of January of next calendar year we will start the cooling phase. We haven't talked in detail. In all likelihood that will probably be switching the power off and watching it cool naturally.

In terms of predictions, the same borehole that I showed before. The horizontal borehole again just above the plane of the wing heaters. This is just a series of sensors.

Sensor 3 starts at the collar, moving towards the back of the borehole, just to give you a feel for the cooling phase if we just flip the switch. The end of three years, all the rock temperatures in that borehole are below boiling. Right now the schedule would have us cooling for four years. We will evaluate the cooling phase as we go and determine when the cooling phase will actually end. At that time there will then be post-test characterization. As of right now the drift bulkhead will remain closed during the cooling phase.

Chlorine-36 validation. Probably don't have to go over the purpose. Remember we've done a lot of chlorine 36. The chloride analysis in the ESF and the data sets that were collected by the project showed evidence of apparent bomb pulse at five to six locations in the ESF. Two of those were two of the faults in the ESF, the Sundance near Alcove 6, and the Drill Hole Wash Fault Zone is exposed just towards the portal from the ECRB breakout.

Because of the importance of those analyses for the conceptual model for UZ flow, we've gone in and attempted to validate the occurrence of bomb-pulse Chlorine-36 at these two structures. You are aware of the fact that Livermore and Los Alamos have both been involved in previous meetings you've seen some detailed presentations from them that show some pretty significant differences on the validation samples between the two laboratories. So we went through a long, arduous look at a set of reference samples to try to understand what was causing the discrepancies. We honed in on how we process the samples, meaning how we crush them. And also how we leach the samples in distilled water. The approaches were distinct.

In Livermore's case they were what we call active where they were fusing--they were shaking them and grinding them as they were leaching; whereas, in the case of Los Alamos they were putting them in the beaker and letting them sit.

As we went through a detailed analysis of the reference sample, we've arrived at what we think is the right technique to look at the additional validation samples, and that is to crush them in a common crusher, one party, and then simply do passive leaching, meaning put it in a beaker with the ionized water for one hour. This is what is now being used for the additional analysis for validation samples. That's ongoing. The USGS is leaching approximately two kilograms of crushed core per one meter of additional core. And we're getting about two liters of leachate per sample. That's being split, provided to Livermore and Los Alamos. Those analyses are ongoing. Livermore has scheduled to do the chlorine 36 to chloride and I'd say scheduled to do the Chlorine-36 to chloride measurement in the accelerator there in September--later this month. Los Alamos is likely not to happen till thereafter, but we do hope to have preliminary results here real soon on those additional validation samples, again using this common technique.

We intend, USGS intends to develop a letter report on the results in early calendar year 02. There will be additional--there will be additional analysis, some additional trillium (phonetic) analysis as well, and that'll be included in the final report.

But as we discussed the other day, the USGS will provide a report that will interpret the Chlorine-36 results specific because we understand that's really the hard spot in this whole thing. So we understand the priority and we're moving forward as swiftly as we can.

Fluid inclusions. The USGS fluid inclusion work, the isotopic work and the geochronology, a lot of what you heard about from Joe Whalen and others at the last board meeting, is nearly complete. They continue to do some microscale work in the Cal Site, particularly looking at isotope variations on the grade scale, etcetera. The results of the USGS studies have been reported at several meetings, GSA high level waste and you all saw quite a bit at the Arlington meeting in the Spring, in May. The USGS is very close to having completely submitted all their data into the Technical Data Management System.

You also heard from UNLV at the last meeting and you remember that they were writing up their results in peer review journal articles. That effort continues. I believe they are real close, but I'm not willing to speak for UNLV. But the intent will still be, once the DOE has received all the documentation you will still see the DOE position on this particular issue once they have all the documentation. That's what Bill Boyle referred to in the previous meeting.

What about the thermal modeling? There was a lot of discussion at that last meeting about how long can the system remain hot to explain the fluid inclusion systematics. The USGS continues to do some thermal modeling. A lot of this is Bryan Marshall's work, who I believe is still sitting in the audience. His simulations continue to show that the modern thermal gradients weren't reached until about three to six million years ago. So the point is, as we were elevated thermal gradients that can explain the fluid inclusion geochronology studies for quite a long time. And then again this work continues to try to really nail this down.

Moving into the cross drift, I'll talk about an update on where we're at with the crossover alcove, which is the drift to drift test between Alcove 8 in the ECRB and Niche 3 in the ESF below. Talk about a brief update on where we're at with seepage studies in the lower lithophysal in Niche 5, and then some discussion of systematic seepage measurements in the lower lith.

Just to jump ahead a little bit, the thermal conductivity measurements that we'll talk about briefly are in the EDS section of the (inaudible), but the rays that I'll be discussing are in the Lower Lith. One is located right about here and one is located down here towards the bulkhead. The bulkhead studies, remember we have three bulkheads in the ECRB. We're not ventilating beyond this first bulkhead here. When I say first bulkhead I mean the first one here at 17+63. Second bulkhead here just before the Solitario Canyon Fault. Then the third bulkhead just behind the back of the tunnel boring machine.

I've also shown on here the italics in blue is tests that are in the current plan but are not yet constructed. And I also have the contacts for the different parts of the Topopah Spring, again, the middle model is Middle Non-Lith here, the lower left--over this extended tunnel, lowering on all the way up to the Solitario Canyon.

Alcove 8, Niche 3. Remember here we're starting in the Upper Lithophysal. It transitions into the Middle Non-lithophysal. There's about 18 meters between Alcove 8 and ECRB in Niche 3 below. Here we're after flow and seepage processes. This is truly a confidence building exercise. We do series of predictions to validate the UZ flow and seepage models.

Just a schematic, a lot of what I've already told you. The infiltration plots are at the floor of Alcove 8. Niche 3 underneath, again, this is about 18 meters. We have down-looking and up-looking boreholes that are instrumented and also used to look for progression of the wetting front during the infiltration.

This is a map of the floor of the Alcove towards the back of Alcove 8. Remember two meetings ago probably I told you about some preliminary infiltration in the very small plot here along the fault at the back of Alcove 8. We weren't getting a lot of water uptake by the fault, so what we did is we went in and we did a trench along the exposure of the fault as exposed to the floor. I told you about that the last meeting. We've now got updated information. We have seen drips. I believe that was available when I was here in May. And there's more information on how much seepage we're getting in the niche in the distribution of the infiltration.

Some bullets on where we're at. That fault is broken up into four different sections so we have hydrodyscol infiltration permeameters that are controlling the head in each of those four sections along the fault. We began this phase of the test in March, saw first seepage in Niche 3 underneath about a month later. Right now it's taking up a steady, over 200 meters a day and we're seeing about seven percent of what is applied as seepage in the niche underneath.

Collection trays in the roof. Just like we've done in a lot of other seepage tests that quantify the amount of seepage. We're mapping the seepage area in Niche 3. We're recording it by remote video and we're also keeping track on a weekly basis of how that wetting front niche progresses and how that ties with the geology.

We continue to collect the water. We're analyzing it, chemical analysis. The observations suggest that we're quasi-steady state. We were pretty quick, within two months after the initial releases. Right now the tracers are just lithium bromide. We're starting in on a program to add additional tracers that down the road is planned to include colloids to look at unsaturated transport of colloids and also reactive tracers. That's to get at helping us build confidence in our models for matrix diffusion in the unsaturated zone.

Just some pictures--here's the trench, the fault within the trench. These are the permeameters that control the head in each of the sections of the fault. A plot of infiltration in liters versus time and then seepage and liters versus time. The orange is simply the cubital of infiltration in Alcove 8/4 along the fault, and then the pink showed the seepage. Again, about seven percent of the water that we're infiltrating is being collected in the trays underneath.

I should say that the--we're of course over-driving the system very significantly here. We're putting in a lot more water in order to be able to see seepage.

This is--I don't expect you to study this in detail. This just gives you an idea. These are maps, the full periphery maps of the tunnel. The best way to look at them is the crown of the drift. Think about the drift and then just flatten it. So this is right spring line and left spring line would be just below. The point is we've taken the USVR maps and we're mapping very carefully where we're seeing seepage in both the ESF. The Niche would break out in this direction. And both the ESF as well as associated with the fault, the fault is right here, and the blue areas are showing where we're seeing seepage within the Niche. We'll continue to map the progression of this front. It's still concentrated along the fault, but we're going to map how that is associated with the fault over time.

In particular when we go to the next phase of the test we're going to go to a larger infiltration plot that isn't just associated with the fault. And there it will be real interesting to see how the seepage interacts with the rest of the Niche, and how that ties with the geology.

Some pictures to show the seepage. If you're facing into Niche 3, the right rib, this is the right rib here, you can see some wetting, wetting along so that you can see--pick up almost a spider web look where you're wetting along the fractures. And then here's wetness in the ceiling just inside the bulkhead above the Niche, right where the fault cuts through the Niche.

I should say that in general our predictions for that test were good. We predicted the breakthrough about right and we expect the fault to be controlling fully early on here and that's expected. The predictions for the next phase of the test aren't yet complete, but they'll be complete prior to us starting infiltration.

Niche 5. I talked some about seepage in this previous test, but here we're looking at calibrating and validating the seepage model. This test is in the lower lithophysal. Remember, a lot of ESF studies were in the middle nonlithophysal. Here we're in the ECRB in the lower lithophysal.

A reminder of what that test looks like. This is the actual test area. We have an access drip here. It's excavated. We drilled these boreholes prior to excavation of the test niche, do some air permeability to look at permeability prior to excavation and then also after. We then excavate this niche and set up seepage in these boreholes above and quantify it through using collection trays--very similar technology as to what we're using in the other test.

The first phase of that, of the seepage for this test, I talked about this before, we didn't see any seepage into the drift at all. Lithophysal porosity was like replaying a role in that in terms of storage. We've since went in and excavated and I had a diagram in the last meeting, what we called bat wing. It's a slot on the rib, the left rib, because when you think about this you can put a lot of water in it here saying there's a capillary effect, a lot of it is flowing around. Where is it going? So we have a mass balance question that always is there. And so we excavated this to try to improve our mass balance. We've excavated that. Once we excavated we had to go back in and do additional air permeability because of the possible changes. We've done that work. We went in, did some geophysics to look for the water from that previous liquid release test, and we're setting up right now to do some additional seepage threshold tests at varying liquid release rates. Those should start within the month.

Systematic, the Niche 5 is at one location in lower lith. We're also doing tests, borehole based tests, where we're drilling boreholes in the crown of the drift and doing borehole based liquid release, also doing systematic air permeability and gas tracer tests. This is work that's being done by Berkeley, Lawrence Berkeley, providing very similar information that you get from the niche data except here you're getting it variability, along the lower lith.

I've already said a lot of this. This work continues ongoing and I have some bullets in the next couple of slides to talk about, some of our observations.

There's a lot of small fractures in the lower lith. We've talked about that before as well. When you go to a cutoff length of say 30 centimeters--let me back up.

If you have a cut-off length of a meter and you map the tunnel, it looks like the lower lith is less fractured than the non-lithophysal. But if you go to a shorter cutoff like 30 centimeters, the Bureau did that, you find that actually the fracture density is comparable. The nature of the fracturing is different, but it's comparable in terms of density.

The air permeability measurements suggest that these fractures are well connected. They tend to terminate lithophysal cavities, has been my observation. But you get (inaudible) level type permeabilities from the air permeability level, measurements.

One of the boreholes where released water along almost a two-meter section. It tends the flow down. No surprise. Pour the drift. Not uniform. There's some heterogeneity but it's along preferential pathways. Because of this heterogeneity some of the water is just going to miss the drift without ever getting to the capillary effect, is the way I would look at it. Whereas, but a lot of the water is diverted around the drift due to the capillary effect.

The lithophysal porosity in this particular bullet it says it's small, but at Niche 5 we still have some things to work through here because at Niche 5 we think the lithophysal porosity might be playing a role in why we didn't see seepage right away. This is an area we need--we continue to work on.

It's real important to quantify evaporation in these experiments. These tests are in the ventilated drift so we're working real hard on making sure we can quantify the evaporation rate. And finally, there is uncertainties and there's evaporation that we have to account for, but the conclusions of the Berkeley scientists is that the seepage threshold does exist, does in fact exist.

Bulkheads. Remember the three bulkheads in the ECRB? Here we're really making observations. This test was constructed to--we're underneath the high infiltration area under the crest, if you look at the surface infiltration maps. If we're going to see drifts, we have--here is where we're going to see it. So we set up the test, the mappers, along those lines, isolated ventilation. We all know, I think you all remember the history. We have a TBM that's being powered in the back of that drift. It's hot relative to the other parts of the drift, and we're seeing some condensation that's likely masking our ability to observe seepage.

I talked in early May about the January bulkhead entry. Remember also we put that third bulkhead up behind the TBM to try to isolate that heat source as much as possible. Work totally successful, as you've heard previous talks. Again, I talked about the January entry. And we're seeing the same phenomena that you heard about before. A lot of condensation primarily towards the back end.

Later in May after we talked last, we actually entered, but this time we did it unventilated. Because in the previous times for safety reasons we've always ventilated the drift. This time we went in with full PPE, personnel protective equipment, for those who don't understand that, without ventilation to see what we could see because the reason we had to do that is because we had lost power back there. So the bad thing was is that we were about to lose power to the data collection system so we went back to fix that. And the nice thing is is that PBM has been off since April. So that provides an interesting--in my opinion, that provides an interesting comparison.

Some pictures. Won't probably do a whole lot for you, but there is still evidence from the May entry of water. Let's not--I don't want to call them drips. It could be condensation on surfaces and then dripping. These drip clothes were installed in January so there was still similar kind of evidence of what we had seen in previous entries despite the fact we were unventilated and the PBM had been off only since early April at this point.

We have continued to analyze water from the previous entries, and we still feel we're getting more and more certain that the observed moisture is attributed to condensation, and it's related to the temperature gradient. I won't sit here and tell you that's the final, final answer, but that continues to be our hypothesis. We've seen no reason to doubt that.

Again, the TBM, the power of the TBM has been off since early April. So measurable temperature gradient that we saw has diminished. And, I have a plot, the next figure, that'll show the temperature at three stations. Overall, this is a qualitative observation. David Hudson, from the USGS, is the PI for this test. He has been in all the entries and he observed less moisture during the May entry than he had seen in the January entry, and I would say in previous entries as well.

Temperature versus time for three temperature sets, there's a different locations within the drift. Here's--when we ventilate everything goes to equal temperature in the January entry. You can see the temperature behind the third bulkhead back there by the TBM gets pretty significant--pretty high. The temperature behind the first bulkhead and behind the second bulkhead is roughly equal, some gradient, but a pretty large gradient between the TBM and the other parts of the bulkheaded area. Once we lost power, that, of course, cooled off pretty dramatically.

This represents just opening the doors and not even ventilating. All we did was open the doors. But you can see the data here in the August time frame shows that the gradients basically disappear.

We haven't been in since May. We're going in in about three weeks. And our plan at that point is again to do the same thing--go in the first day unventilated with just a couple scientists. Not a large entourage, just a couple of scientists are going to go in and have a look and take very careful notes.

Path forward. I've said some of this a little bit already, but I need to talk a little bit about this first bullet. The next bulkhead entry will be in early October. We also intend at that point to move the first bulkhead. Right now it's about half-way down the cross drift. We have a lot of other testing that's currently proposed to DOE that they are evaluating right now, next year to address some other issues related to thermal-mechanical properties. And while we're looking at some other testing, it really requires us to have access to more of the lower lith than we currently have access to. That, logistics speaking, plus the fact that the test we feel most of what we're learning in this test is happening at the back end. So we're going to shorten up the test bed, so to speak, move that first bulkhead well down towards the second bulkhead and work with about, along on the order of--it ends up being a little less than 300 meters a drift, isolated from ventilation at that point.

We're going to also improve our monitoring conditions by remote video behind the bulkheads to try to still get our-- here we're seeing seepage or condensation, improve our measurements of some of the atmospheric conditions.

There's some things going on in terms of injurious processes that we're going to try to improve our measurements within the drift to try to better model those phenomena. We're going to improve our collection system for moisture, not just have drip cloths, but try to quantify the moisture a little bit and also continue--collect samples in a cleaner fashion in some cases to get better chemistry.

And again, the analysis and modeling is ongoing and not only do we look at the seepage, we've done predictions for the seepage, but more importantly the analysis in modeling is cranking up to look at what's going on inside the drift.

Busted Butte, here we've moved out of the Topopah Spring stratographically down--well, actually to the very bottom of the Topopah Spring and the top of the Calico Hills formation. Remember, Busted Butte is located southeast of the ESF and the cross-drift where we were just talking about all of the testing, data collection.

Here we're looking at--this is really, I don't want to call it an analog, but this is a validation experiment. We're building confidence in our transport, flow and transport models for bedded Calico Hills, vitric Calico Hills. As it's below the repository horizon, we're not trying to say this is totally applicable, but it certainly is a good test for validation of the models. So we do a series of predictions and then validate our observations.

Some objectives. I won't dwell on these. You're heard these before, looking at a variety of different processes, fracture matrix interaction, colloid migration, how we can--how the sorption data from the field scale match up with extensive laboratory sorption database that we have already on Yucca Mountain. And of course, get a scaling.

This will really be a snapshot of where we're at. This is still a work in progress. What you're looking at here is--go back real quick, John.

The test block that I'm going to discuss is the

Phase 2, the large test block. Remember, we have injection holes to root out this face, two planes, one up here in the Topopah Spring and one down here in the Calico Hills. We had inject--collection holes coming off of this face when we were collecting tracer periodically on pads. We've now turned that tracer system off and we are doing post-test characterization by coring in mine back. Collecting samples for lab analysis. So what you're looking at here is the face, that injection face. You had the two planes of injection holes. And what we did is we went in and we did a series of overcores of those injections holes, and here we were driving at trying to get a handle on how far the reactive tracers had gone. Because again the transport distance should be relatively small here. You're going to see some preliminary data from results from these two overcores here which were for borehole 20, a high injection rate borehole. This happened to be sitting up in the bottom of the Topopah Spring which is a fracture welded vitrophyre.

Some results, what you're looking at here is again the injection hole, the two overcores. What they did is they sliced it into three. Then the did analyses as a function of distance from the borehole for all three slices. So what you're looking at here is simply results for the fluoerbenzoic acid, which is the conservative tracer that we use. It's tagged so that we know which borehole it came from. And also results for nickel which is a sorbing metal in this system.

Don't have a lot to say other than this is the kind of data that we're collecting that's going to be used to analyze the tests in great detail and, see, the concern is basically equal as you would expect, whereas the nickel tends to climb as you move away from the injection borehole. Behavior in general that we would expect from a reactive tracer in this system.

There's some things going on here with humps and things. We're continuing to do some analyses of other splits of these same slices to see if some of that is real. Or what it's telling us, is a better way of putting it.

Here is a good example of that. We've talked about colloids before in this test and we weren't having a lot of success. There were some things going on, we think, in the effects of the chemistry on the microsphere tracers that they were probably--and coming out of solution before they ever left the borehole. These are difficult things to find and measure. We've done a series of lab measurements and now we've also improved our techniques, and we are now hopeful and we think that we can actually get some real, I'll call semi-quantitative information on colloids from the actual test block as opposed to just relying on lab experiments.

What this is is another one of those slices from the injection bore hole going down. This is a--not a very quantitative scale. It's a relative count of microspheres. What they do is they image a sample and they simply count the number of fluorescent microspheres that they see. It's not calibrated totally, but it gives us a relative idea of how far the colloids are transported. They know the size. There's some that saw in those splits this interesting rise, and talking to the scientists we don't yet have a clear explanation for that. It could be a filtration phenomena at this location. They are looking at the core in great detail to try to figure that out. I guess the main point here is we are getting some useful colloid information out of the test block. Before I told you that wasn't looking real good.

I talked about the overcore. Now, what about the mineback? We have since--this is again that Test Alcove. These dotted lines here are the injection holes. Number two plane, and the collections holes come off of this face here. We have gone in and excavated a mineback. When we mineback into the test bed, ran along, crossed several of the injection holes, and our ultimate goal was to get back here to this fault where it crosses an injector. We're taking a series of samples. If you remember the previous phase, phase one, minebacks where we had the pretty fluorescein pictures where you could image where the dye had gone. Similar kind of thing here. Taking a series of samples--I have a couple pictures. No real results yet. This just finished last month so the analysis is ongoing. So I'd say next meeting I would hope to have some preliminary data to show you on the lab analysis.

These are kind of hard to see so bear with me. But this is simply a picture of that fault at the very back of the mineback.

Go back a second, John.

The pictures that I'm going to show you, one is going to be taken from here looking into this face, and the other picture is going to be looking as if I'm standing here and looking over at this face at these injection boreholes.

Okay. This is simply a picture of the fault. You can see the offset, not significant offset. This here--this total exposure here is about five meters. But these are where--these are collection holes that were drilled in. We crossed an injection borehole just as it crossed the fault. This is prior to the sampling. If you look at it now it's like swiss cheese. They've taken a whole series of hand auger samples all around these holes, go along the fault to quantify the tracer movement.

This is hard to see also. This is again looking down at the lower injection array. If you squint, and maybe you need to, believe me, there's a little bit of red just below this borehole. That's rhodamine dye. That's a dye that actually sorbs, so you can see that it hasn't trailed too far from the borehole. The yellow here, some of which was scraped away, is fluorescein stain. So--and it has been scraped away because we were continuing to excavate and the dirt was piling up in this area. But I hate to say, believe me, but you can map the fluorescein distribution tells you a lot qualitatively about the flow system local to the test, and then compare that to the tracer, tracer results.

Remember that ACL in Canada is also doing some large block experiments--large blocks may be the wrong way of putting it--some block experiments from, taken from Busted Butte in the Calico Hills. They are doing two blocks. One is an unsaturated transport experiment. The other is the saturated transport experiment. They are using real radionuclides in this particular case. They are in the laboratory. And this has been very useful information to compare to what we're seeing in the real test with the analog tracers.

Some preliminary observations: In the unsaturated block we're seeing technetium in the under-oxidizing conditions is traveling as fast or faster than transport solution. You're seeing some anti-exclusion effects and likelihood that they are causing it to go faster. But it's acting conservatably. No surprise.

In the saturated block, the technetium is actually being sorbed, slightly sorbed. That's likely due to what is reducing conditions in the block. There's some discussion up there that they may have some microbes growing in there that are causing reducing conditions and they are still looking into that. But if in fact there are reducing conditions you would expect technetium to go to an oxidation state such that it could be sorbed in these rocks, to a weak extent.

Neptunium weakly sorbing in our system. That's, we assume, our models, and it is in fact being borne out by the experiments. And the bottom line is we're agreeing well with the experimental-determined coefficients from batch measurements, which are of course crushed tuff inside of the beaker. Here we're at least dealing with a scale. We're scaling up to the meter scale and comparing that to the analog tracers that we're using in Busted Butte.

Saturated zone. This is somewhat an out-of-date map. There's more updated maps that Dale has put in the--in back showing layout of the Nye County boreholes for the Early Warning Drilling Program. Dale again will talk a lot more about this. This is US-95, Lathrop Wells, Yucca Mountain, up here to the north. I will not steal his thunder on that, but we are working cooperative with Nye County to collect data under our QA program. It's being used on support of our saturated zone model, models. This is a list of the sorts of things that we're doing in cooperation with the Nye County samples, and also in the boreholes.

I'll touch on an update on where we're at with the lithostratigraphy for the frame work model and also touch on where are we? Some updates on the results of the alluvial testing, at the Alluvium Testing Complex.

Lithostratigraphy. Rick Spangler, from the U. S. Geological Survey, is the PI for this work. The focus up till now has been to take the results from the Phase II drilling, and he is developing cross-sections, then integrate all the data collected up through Phase II. He is also looking into geophysical data, the aeromag data and some of the other data, and using--and that was used to update the hydrogeologic framework for the saturated zone model.

A lot of these products are near completion and are now in technical review within the USGS. We will continue to work with Nye County in Phase III to collect additional cuttings and further refine the hydrogeologic framework based on Rick's work. And it's--these cross sections are starting to become a very useful tool for helping, working with Nye County as they decide where they want to drill in future phases.

This is a hard-to-read diagram. It's lifted the same, the same area as that diagram in the back of the room, the previous one I showed. The point here is it shows the cross sections that Rick is working on. The black lines are faults that have been either mapped or inferred from gravity in aeromag data. Rick's interpretation. And then also shown are the borehole control. The yellow are YMP boreholes and the blue are existing or planned Nye County boreholes.

The cross sections that Rick is currently working on is 40-Mile Wash north, roughly north to south cross section. And east-went here going from the east side of 40-Mile Wash over to the southern part of Yucca Mountain, and then one right along US 95.

Moving into the Alluvial Testing Complex. Here we're again after collecting data that provides parameters to confirm our basis for the saturated zone pull and transporting alluvium and also doing a series of predictions for model validation.

This is just one potential flow pathway coming out of the repository, coming from the saturated zone model. You're going to hear a lot more about saturated zone flow, I believe, a little later this afternoon from somebody who knows a lot more about it than me. But this is one potential flow pathway coming out of Yucca Mountain.

Here is 19-D, which is the cornerstone of the Alluvial Testing Complex. I've told you before and I'll bring you up to speed on where we're at. We've done three sets of single hole tests where we inject tracer and then pump it back. The drilling is being finished up in the field to do the multi--to set up for the multi-well test as well. And again I'll let--Dale will likely discuss that later today.

Just a stratographic section of 19-D water table sits about right here, a little over 300 feet. The alluvial aquifer is in this area. You have the tertiary tuffs and then the tertiary sediments all below the water table. Shows where we set up screens to possibly do interval testing, both hydraulic and tracer testing within different intervals. We concentrated on the four intervals within the alluvium for the testing for the single hole test.

Some of these are reiterations of what you heard at the last meeting. We've again done a three-plan single-wall test. We inject tracer and then we did three different tests. One case we pumped back immediately and the other case we shut it up for two days and pumped back and let it drift. In another case we shut it up for 30 days and let it drift and then pumped back. The results indicate insignificant diffusion from the foreign ground water into the stagnant water, and advection-dominated system. This is consistent with a single porosity continuum transport model that we're using for alluvium in the PA. I already mentioned the remaining alluvial testing complex injection monitoring wells. Our plan is for installation this calendar year. Fill work is ongoing. And we will then start the crosshole test that will give us information--confirm our understanding of several parameters, including conductivity, porosity, looking at KDs and also colloidal transport.

Very busy diagram, but what I want to show is some preliminary results of analysis of the single-hole tests. What you're looking at is--the best way to look at this is probably the first three and then the second three. What you've got plotted is for the three different tests, remember I said zero days of shut-in here on the left, two days of shut-in in the middle and 30 days of shut-in on the right. You're looking at red, analytical solutions for the 1-D invection diffusion disperson equation and the blue is real data. This is absolute concentrations versus time. The bottom three are simply normalized concentrations, normalized to the peak. They're again analytical fits.

From that--this is work done by M. J. Marhi (phonetic) of the U. S. Geological Survey. By varying and holding certain parameters constant you can back out, and these are simply different runs using different assumptions for how he handles the parameters and the equations where you can back out dispersivity, effective porosity, as well as specific discharge or flux. These are some of the preliminary results from those fits. Dispersivity, longitudinal dispersivity is the dispersivity along the flow path. Effective porosity on the order of 10 percent into 15 percent and the flux is on the order of one and a half to three meters per year. All these are consistent with what we were assuming in our basis for the saturated zone flow.

Switching gears to the EBS real quick, I'll move a little smarter here so we get through it. We've heard about the ventilation test at Atlas. We have a large simulated emplacement drift. We've got simulated waste packages inside of the quarter scale test. We've got a crushed tuff invert, and we're doing a series of measurements, again to support validation of the preclosure ventilation model.

Some pictures from the field just showing the installation of that test. Remember that phase one of that test was where we were flowing ambient air through. We're now in phase 2 where we're recirculating air, so we're recirculating what I'll call conditioned air in phase 2 of this test. And I'll talk about some results in the next slide.

Again, quarter scale test. In general the phase 2 test results are in good agreement with our pre-ducted surface temperatures. We have heaters in the test, and again we can vary the heat load within the drift. We can also vary the flow rate, and we can also vary the temperature of the incoming air. So we were doing experiments controlling the air, 25-C, 35-C and 45 celsius. And from that we can see how well we're predicting surface temperatures, and also get an idea for efficiency of removal of heat. You can see the efficiencies that have been calculated for four of the phases of these tests on the order of 70 to 80 percent. Incidently, similar to what--very similar numbers to what we assumed in the SSPA and the PI calculations.

Natural Convection. This is a test that construction is ongoing. Here we're doing two separate tests. There has been a lot of discussions about scaling and how well you handle scaling. In the ventilation test we've had to do a lot of modeling and analysis to address the scaling issue. Here we're going with two tests, the two different scales to try to better nail down those issues. Again, the construction is ongoing. We've got a 44 percent scale test and a 25 percent scale test. Here we're looking at national convection within a heated drift. So it's building confidence in the in-drift TH models. It's very--this is an important test in relation to the analysis of the ECRB bulkhead experiment as well.

What about thermal properties? There was discussion yesterday about thermal conductivity. We are in the process of starting up a program to further bolster our database on thermal conductivity. It is focused on both a field and a laboratory program. I'll focus on the field program. I'll say about the lab program, we are starting a series of analyses of thermal conductivity and other thermal properties for the matrix. But as was pointed out yesterday, when you talk about lithophysal unit in particular, what does matrix thermal conductivity mean? So the fuel program is put in place to try to help us address some of those issues.

The first test, this one is ongoing, is again in the ECRB where in the lower left we have two holes, one with a heater and one with a string of thermocouples. And we're simply running this heater at low power and we're running this below boiling. The maximum temperature right now is 50-C, and we're backing out thermal conductivity and other thermal properties using Carl's Law and Yeager type equations. So analytical solutions to Carl's Law and Yeager type equations, we're backing up thermal conductivity.

The first phase has been run and I'll show you some preliminary results. The second phase we're going to crank up the heater up to about 3kW, create a dryout zone and see what happens and see how that affects thermal conductivity in terms of its function of saturation.

The second test, which is the holes have been drilled, we've installed the instruments and we're wiring them up now. Would be a larger test. Three meters and three instrumentation boreholes. I believe one of the boreholes is above the plane of the heaters and two are below to look at any up-down effects. But here we're looking at perturbing more rock, creating a larger dryout zone, again still in the lower lift, different section within the lower lift.

What about back to the first test. What you're looking at here is, if you remember back, it's an X, so zero here is the crossover point where that X fits together, and then we're moving in meters away from that crossover point as a function of time. And here's temperature. You can see phase l. The highest point was at about 50-C. We take these temperature profiles and we can then--let's go to the next one--as a function of time back out thermal conductivity and thermal diffusivity. I primarily want to focus on the thermal conductivity numbers. You can see this is in watts per meter K, on the order of 1.6 and 1.7 watts per meter K. Yesterday Jim talked about thermal conductivity in the lower left and we assume one point in the SSPA calculations, we assumed 1.87 wet and 1.27 dry. It's within the range. This is a positive result in my opinion, that we're seeing some reasonable numbers compared to what we're assuming in the SSPA. But again, this work is ongoing. We'll have additional results and the second test will start up. And we're going to look at a couple different locations within the lower lift to get at the effects of lithophysal porosity, which are, as you heard yesterday, will affect these results.

Skipping over waste package because you've heard about, that and going into waste form. There continues to be work on--in the waste form area, primarily developing or building further confidence in the parameters that we use for waste form degradation, both for spent fuel as well as glass.

First let's talk a little bit about commercial fuel--mainly have some pictures. These are two separate fuels, different burnups. This is data that's being collected at Argonne National Laboratory in support of the Project. Two different sample holders. There's chunks of fuel inside there. These have been--these particular samples have been subjected to dripping, not batch or flow-through experiments, but dripping of water at elevated temperatures below boiling. I believe like 60 to 70 degree celsius. You can see there is underlying fuel here that's black, but we're seeing the fuel fragments being covered and submitted by a layer of uranyl silicates, consistent with the basis that we used for the waste form degradation in the model. These test continue. Again, these have been going on for eight years. They will continue into next year.

Same two samples. Here looking at neptunium relative to uranium release from the fuel in the drip experiments. What you've got plotted here is time. Again these have been eight-year experiments. First is the ration of neptunium to uranium. What they are looking for here, is there any systematics in how neptunium is released versus uranium from the waste form. And the conclusion of the scientists is that as time goes on, they tend to level off at one. This is hypothesized to be consistent with the fact the same alteration phase, in this case dehydrated schoepite. Schoepite is a uranium oxide hydroxide mineral, which is one of the primary alteration products of the fuel. And it seems--that phase seems to be controlling the release of both neptunium and uranium, and actually taking up quite a bit of the neptunium and uranium and not allowing it to be dissolved into solution. This is again consistent with our assumptions about solubility, etcetera, that we've used in the models.

What about glass? There is a series of drip tests going on with glass wasteform as well. This is a, I think a pretty picture of an actinide-doped--waste glass. It has been exposed to dripping for 16 years. No surprises. This is basically a rhyolite glass, a hycylical glass. So when you expose it to dripping at elevated temperatures it's going to alter the clay. To build up a layer of clay that tends to spall and you build up an additional layer of clay on that, that kind of process where you get dissolution controlled hydrolysis of the glass is consistent with our basis. This clay layer tends not to--we do not take credit for this clay layer in terms of sorbing, but it is consistent with our conceptual model for how the glass breaks down over time.

So to conclude, I hope I've given you a feel for where we are with a lot of the ongoing data collection, analysis testing program in the underground at Atlas and in the laboratories. These results continue to confirm our technical basis. We're still focused on reducing uncertainties in the key areas and also providing additional confidence in our models.

So that was all I had.

WONG: Thank you, Mark. Questions from the Board? Dr. Parizek?

PARIZEK: Yeah, Parizek, Board.

Mark, is there anything new on analog work, such as Pena Blanca or elsewhere? You didn't have that on the list, but just didn't know whether you had some--

PETERS: Yeah, they're still working through, specific to Pena Blanca, we still intend to do some drilling down there. But we're still working through some logistics issues with drilling in Mexico, which is--provides some difficulties, let's put it that way. So we're working through that, but there's still full intent of going and doing that drilling. Yellowstone--the work, you know, we continue to work towards--Ardyth would be better to speak to that, but we continue to work towards synthesizing a lot of the natural analog work later this calendar year, I believe. You know, Yellowstone, there's stuff, looking at INEEL and some of the NTS stuff. All that continues.

PARIZEK: There's a--Figure 23 you showed the wetting that was induced as a result of the addition of water above.

PETERS: Right.

PARIZEK: If you take rock fragments out of the wall, do you see wetting inside the rock fragments as well, or just movement of moisture down along joints or cracks, or is there some evidence of water effusing inside the solid piece of rock?

PETERS: I haven't looked myself, Dick, but I would guess--the way it has been described to me and the way that looks, it probably hasn't imbibed a whole lot into the matrix, so to speak. It's probably concentrated along joints and fractures. But I can--Dave probably didn't show up given the events of the day. I think a lot of people went home, but I can find out.

PARIZEK: It would be interesting to see what is happening there.

PETERS: You bet.

PARIZEK: As far as how the shutting down the boring, the invectious drip rather, with the bulkheads, you indicated that the drips or at least the moisture was a little less noticeable this last--

PETERS: Right.

PARIZEK: --than previous. Is that maybe season of the year type to say hematic responses during the dry hot summer days versus winter period, or do you thinks that's really cooling of the PBM, finally, as a result of loss of power, or can't say why you seen less moisture the second visit?

PETERS: Well, it was interesting that--I guess I--we still, we still think that it's the condensation. I can't totally rule out other effects like you alluded to in terms of dry season, etcetera, hot. I'd say a lot--entering next couple weeks will tell us a lot more.

PARIZEK: Yeah. Any other visits like that will begin to shed light on whether it's--

PETERS: Right.

PARIZEK: --or whether it's seasonal or both.

Then as far as the moisture, I just asked the question about the third water type, the J-13 is the corewater chemistry?

PETERS: Right.

PARIZEK: Two distinct chemistries. But then the condensation of water, if you have any chemical tests on any of that, or preliminary results, that relates to really quite a bit of moisture that might be involved in working on waste packages. And that chemistry is a better water, I guess, it's more dilute water than anything--you have about, two were used in the corrosion experiments. On the other hand, is that the kind of water the people from Nevada that April talked about yesterday as an example that showed all of the evidence of pitting and so on.

PETERS: Yeah, I--this is my--that water would basically be condensate, dilute. It would interact with possibly the dust and you'd get into concentrated brines that Greg and others are already accounting for. So I can't imagine that process producing water composition that we haven't already thought about. That's personal opinion. That would be my take.

PARIZEK: Thank you.

WONG: Dr. Craig?

CRAIG: Paul Craig. Couple or three questions. What is the project's current position on the reality of the bomb pulse clarity?

PETERS: We have--we continue to have a conceptual model in the UZ that are consistent with the presence of bomb pulse. Okay?

CRAIG: Yeah.

PETERS: So both the conceptual model and the model fully account for the occurrence of bomb pulse core in 36, along structural pathways. There's no plans for us to not account for that in the model until we resolve this issue. I guess I would also say, and this is now me talking. If Livermore was right and the numbers are more like 210 to the minus 15, that tells us that we're--I don't want to sound like I don't want to find out the answer, but we're still conserv--we're conservative because if Livermore is right the pore water is 400,000 years old. So it goes in the right direction. That doesn't mean that we don't need to follow this through to the end to understand why we're seeing those differences.

CRAIG: Okay, the second question is actually Don Runnels', but he didn't have his hand up so I'll ask it for him. It's what he asked a while back. What are the criteria you're going to use to decide whether the (inaudible) is bomb pulse or not pulse or not--

SPEAKER: Paul, you're--

CRAIG: Oh, I'm sorry. Don Runnels' question which he asked last time. What are the criteria you will use to decide once you get the two laboratories working together with a common methodology, whether or not the quarry is or is not bomb pulse quarry. Since the results seem to be enormously sampled perforation-dependent you need some kind of criteria to decide what the origin is?

PETERS: Well, I'm not sure I'm going to answer your question, Paul, but when you say criteria, the criteria for bomb pulse I don't think are what's at question here. We had--June's work, June Fabryka-Martin's work, had gone through and established, looking at, you know, the change in production rate over time, etcetera, and what you'd expect in terms of background. 1200 to 1500 to the minus 15 is the threshold where you think you either have apparent bomb pulse or you do not. I don't sense, in talking to the scientists involved in this study, that they question that. But I don't think I'm answering your question.

CRAIG: But the way in which you prepare your samples--

PETERS: Yes.

CRAIG: --affects the amount of material which goes into the measurement. And consequently the volume of material which is dissolved from which you do your leaching affects the results intimately.

PETERS: That's correct.

CRAIG: You have to come up with a criterion that takes into account the preparation method, and that's the criterion I'm looking for.

PETERS: Okay. I'm probably not going to be able to answer your question to your satisfaction, but they looked very carefully at the time. When I talked to you in May they were talking about seven hours. They've continued to evaluate the data on the reference sample and they are down to an hour. So--in terms of leaching time. So they are trying to--I think maximize is the wrong word, but I'll use it anyway--trying to maximize the possibility of finding that component in the salts.

CRAIG: The last question is in a completely different area, and that has to do with--it may not even be when you--it's your area. Has to do with the mock-up experiments on canisters. You mocked up some C-22 canisters, done some welding on it, I understand. And I don't think the Board has heard anything about that. What is the status of that--

PETERS: There's been--Tom Doering still here? There has been--we haven't-- you mean mock-ups like small weld samples.

CRAIG: No full.

PETERS: Full scale. I don't believe there's been a full scale done yet.

CRAIG: Well, there's a response--

PETERS: Yeah, there's a--go ahead, Gerry. Yeah, you're taking me out of my area of--

CRAIG: I was afraid of that.

PETERS: Yeah.

CRAIG: Well, we can do it later on.

PETERS: That's okay. Go ahead, Gerry.

GORDON: I'm not sure exactly what your question is, but there have been some full diameter, quarter length mock-ups made which have been characterized in terms of ultrasonics and diameter and other nondestructive evaluations. To my knowledge there have been no defects.

CRAIG: Okay, it would be interesting to hear about that work at some point because that's--the question is to whether you can actually make canisters the way you claim to be able to make them is important.

GORDON: Right.

PETERS: One thing on that, Paul. We are also doing--this is--and this isn't what I'll call a constructability question. But we--in the corrosion test facility that you call, I think, the dunk tanks, we are looking at welded samples versus face metal samples to look at the performance of welds in that space.

WONG: Dr. Nelson?

NELSON: Nelson, Board. Hi, Mark. This is just a lot of information.

PETERS: If you want to give it next time.

NELSON: Let me ask you one thing right at the top. Is there any evidence of rock deterioration in the ECRB?

PETERS: Yeah, there's--there's, I'd say, things caught in the mesh. That's, you know, kind of just a small--

NELSON: Do you plan on doing anything with that, trying to understand the character of that deterioration product?

PETERS: In terms of observationally going down and quanti--or mapping kind of what we're seeing in terms of deterioration? I've talked to the guys who do the ground support walk-downs and asked them to start taking note of what they see in different places, but in terms of formally, we don't have a program right now to go in and systematically evaluate that.

NELSON: Okay. Let me switch a little bit to the thermal conductivity questions. In the tests that you're doing and I really happened to hear about doing some (inaudible) tests. That's good, a good start. But your approach, the approach with hydraulic conductivity in bulk properties is to figure out somehow how to control the water, knowledge of water content and porosity--

PETERS: Right.

NELSON: --in terms of understanding the result of any measurement. When you work through a mass of rock that's being tested, which is hidden from you, necessarily, because you're working in cross boreholes--

PETERS: Right.

NELSON: --how--what's your strategy to know something about the water content and the porosity with the cases that in particular that you're not taking it up to dryout? You know--

PETERS: Right.

NELSON: --where you've got a water content that's responding and an unknown porosity in terms of lithophyses.

PETERS: The collecting core, and the intent is, we characterize that core for things like moisture content that will give you at least some idea of it along the borehole. Lithophysal porosity is a real bugger. If you're working off a flat face and you're going back four meters into the rock, that--we're doing two things. We're mapping the face in more detail than we did during the first pass through the tunnel.

We also have a proposed viewing next year that we look at the borehole video as well and try to put together as best we can a picture of the lithophysal fracture distribution within the general area. And we're exploring if there's something geophysically that we can do that can tell us something--probe the rock and tell us something about lithophysal porosity. That's a challenge.

NELSON: Yeah, and I think the importance that was shown in the figures that we saw yesterday about knowledge of thermal conductivity and water content in terms of its impact on peak temperatures and what's happening--

PETERS: Right.

NELSON: --we met a relationship between conductivity and water content or porosity in the waves that you're trying to cope with this and develop a way to calculate a--

PETERS: Right.

NELSON: --bulk conductivity--

PETERS: Right.

NELSON: --really requires an awful lot of calibration before it's going to be believed. And, it's going to be really hard to calibrate it, isn't it? I mean that you have some methods that you pulled out, some--

PETERS: Yeah, I agree with you that it's a difficult problem, but I guess I--I look at the preliminary data anyway, and the fact that the calculations you're referring to, Jim talked about yesterday. John Case does calculations where he calculates thermal conductivity from the matrix values and uses a lithophysal porosity term and calculates. I'm--I'm encouraged by the results so far. I mean what if it came back a 2.2? Then I'd be up here and you'd be really running me up a flagpole.

NELSON: These are expensive things to validate.

PETERS: I understand.

NELSON: You know, each one of these tests being a one point measurement effectively.

PETERS: I understand.

NELSON: Let me just ask you one connecting question to this, which is, to understand what's important about hydraulic conductivity--I mean not hydraulic, thermal conductivity, both from a heterogeneity as well as the range in properties requires a context like an analytical code, something that's predicting what is going to happen with the temperatures and the fluid flow. What--how plugged in are you to developing those analyses so that you might now, for example, say, well, if we don't get this much of a variation in hydraulic--in thermal conductivity, we're just not going to be able to drive any unanticipated response of the repository. Do you know what I mean?

PETERS: Yeah. Well,--

NELSON: It may be that the range of hydraulic--of thermal conductivity that you have reason to expect--

PETERS: Right.

NELSON: --and you continue your site investigation isn't enough or is enough to actually cause some maybe concerns or other kinds of behavior for the overall repository when you put it into the analytical method.

PETERS: Okay.

NELSON: So I mean it's playing somewhat with the analytical code to see exactly how far away from what you might--what you've expected in the past do these values have to be before they start generating a behavior that's not currently--

PETERS: Maybe a couple comments. I'm not sure if I'm going to hit what you're after. As I said, what you saw here was they're backing out the parameter using stuff out of Carl's Law and Yeager, a technical--an analytical type--they will look at these as well with more sophisticated, like NUFT type codes. We're also, as an aside, looking at possibly trying to look at things like the drift scale test and NUFT codes are two type codes to back out thermal properties as well. So I guess what I'm saying is--

NELSON: Let me just hit it one more time and see if I can get it. We saw some plots that showed temperature.

PETERS: Yes.

NELSON: And presumably moisture distribution that would also go along with that overall repository footprint.

PETERS: Right.

NELSON: My question is how far different do the real parameters or the ones that you're making measurements of now have to be from what was assumed before you start getting significant differences in the prediction of the performance.

PETERS: Oh, that's--I'm probably the wrong guy to answer that, that question. But I think Jim kind of touched on it showing the sensitivities yesterday, didn't he?

NELSON: Yeah, but I was just asking you to see how you're connecting between the analytical code and the experimental--

PETERS: Okay. Well, I'm--I'm--Jim Blink, who was up here yesterday talking to you about a lot of those issues is intimately involved in helping me plan the tests, is probably one way I'd answer it. Jim just stood up so maybe Jim can help me, but I'm certainly connected in with the people who are analyzing the data. I can't--I'm not the right guy to speak to sensitivities.

NELSON: There he is.

BLINK: Jim Blink from Livermore. We are working together both for the model of conductivity based on core results plus mapped lithophysal porosity results so that we can properly interpret the laboratory and field measurements. And then parallel to that we're looking at the sensitivity of overall temperatures and variation in temperature to, not only the level of conductivity in each stratographic unit, but also the variability, the spacial variability and the scale length of that. We've done the first part of that in the SSPA. The second part remains to be done. It's in our plans for next year.

NELSON: And you're using the Hadley correlations that would do that, that you're using now to make a bulk property?

BLINK: We have--in John Case's calc report we have, I think, five different approaches, including the Hadley method. They range from a series to parallel as the end numbers, and we are trying various combinations of those. Probably the best one is the Zimmerman method which assumes the steroidal cavities.

WONG: Dr. Knopman.

KNOPMAN: Mark, I have a question about the drift scale test, but while we're on the subject that Priscilla was asking, I just want to clarify, Carl's Law and Yeager's textbook on thermodynamics, I'm trying to remember--

PETERS: No, I'm probably thinking of the wrong one. I'm sorry. I'm--it has been--

KNOPMAN: All right, because whenever it was, I'm sure it didn't deal with this material

PETERS: No, it's deduction of heat and solids.

KNOPMAN: It's a heat transfer text.

PETERS: Heat transfer, yeah.
DR. KNOPMAN: Okay.
MR. PETERS: Excuse me.
DR. KNOPMAN: But still there were assumptions about--
MR. PETERS: Yes.
DR. KNOPMAN: --in the material. That's an old, old book. Now, I mean the laws--
MR. PETERS: My point being that I guess I--or I was trying to get across there, although I probably stepped on myself, was we aren't just using sophisticated, complicated codes to do this.
DR. KNOPMAN: Okay.
MR. PETERS: We are doing analytical solutions with simple 1-D approaches, and I've heard that from some of the board members before.
DR. KNOPMAN: Okay. Yeah, I was just getting the impression you're using numbers coming out of what might be--
MR. PETERS: No, I'm sorry.
DR. KNOPMAN: --a formula that's--
MR. PETERS: That's my fault.
DR. KNOPMAN: --designed for homogeneous materials.
MR. PETERS: That's my fault.
DR. KNOPMAN: I don't know how sophisticated they got on that. Could we look at Slide 9 on the drift scale test?
MR. PETERS: Uh-huh.

DR. KNOPMAN: I'm just trying to get a sense of when you say "good agreement" what you mean. If you look at the two graphs there below, I just tried to match up the different color curves. And you take that purple line that sects from the bottom, for example, reasonably good agreement after a year, it's about 85 degrees on both the measured and simulated. By the time you get a little past three years it's a 10 degree difference. If you move up to the next blue line on the measured plot there, it's about 150 degrees, maybe a little lower than that, and about the same after one year for measured and simulated. When you get out to three years, it's a 10 degree difference.

In the case of the green line that's close to the top of that first chart, you actually get a 10-degree difference after one year and it actually then comes a little closer by the end. So, you know, for most of the sensors there is a growing disparity between measured and simulated after three years, which if you multiply the same trend, for example, by 1,000 years, you're really far off what you're saying.

So tell me again sort of what your criteria might be for goodness of fit here?

MR. PETERS: I didn't show the statistical analysis, and I'm probably not going to be able to reproduce it, but they've gone through a very rigorous statistical analysis of some predictions by grid block versus what we see in the sensors. And the mean error is a couple degrees Celsius. I'm probably not going to give you a real satisfactory answer because I don't have all the information off the top of my head, but let's see, the temperature measure is probably good to plus or minus a degree.

DR. KNOPMAN: A degree?

MR. PETERS: Yeah. So it's outside--

DR. KNOPMAN: Is that based on these couple of years?

MR. PETERS: Yeah. Certainly the differences are outside the error of the temperature measure, assuming that the thermocouple is still good. There's no reason to believe it's not. What criteria, I'd have to rely on the statistics guys for looking at it in more detail to tell you what the criteria are in detail. If I'm within 5 to 7 degrees to what is a very complex test and a very large set of measurements and the courses of the grid blocks, etc.--I'm using course grid blocks to predict this--I'm within 5 to 7 degrees, I call that excellent.

DR. KNOPMAN: But you're using this to predict out thousands of years.

MR. PETERS: Right.

DR. KNOPMAN: So you might have a different way you'd want to look at what is acceptable--

MR. PETERS: That's fair.

DR. KNOPMAN: --tolerance of error here.

MR. PETERS: That's fair.

DR. KNOPMAN: That's the point.

MR. BOYLE: William Boyle, Department of Energy. If we look at those temperatures, let's not forget that's what we would have seen in hundreds of years for a hot repository. We ran this test greatly accelerated, so it's not a legitimate--we can't extrapolate this out for hundreds of years of repository to perform this because then we'd get up into thousands of degrees, which we're not going to. So this is as hot as we would ever get in the hottest repository. So that may be the maximum amount of error we would see in the order of the number of degrees that Mark mentioned.

MR. PETERS: But your point is well taken. I think it hadn't ever been put quite that way, and it's clearly something we should go back and think about.

DR. WONG: Dr. Sagüés.

DR. SAGÜÉS: Thank you. We'll keep on applying heat here. If we'd go to, please, No. 32 I believe is the one we want to see, 32, please. How about, then, 33, I guess, 33, please. There it is. Thank you.

Of course you have spoken about this before, but that picture brings home how reductively small of a temperature difference from one point to the other along a drift. It can make a relatively big difference in observe accumulation of moisture and distribution of water and so on. And we had this before, but in a natural repository situation where you will have packages with different amounts of heat generation from one to the other, what will be the graininess of that temperature along the drift? And second, will that differential of bumpiness in the heat generation, natural heat temperature differences, would that generate significant movement of water from one package to the next or family of packages to the next group? How does that work out?

MR. PETERS: I'm not going to be able to answer your first question because I won't know the exact, say, temperatures at maximum temperature and how they vary between, say, defense packages, which tend to be cooler than the commercial packages.

DR. SAGÜÉS: No, I mean the same kind, for example.

MR. PETERS: Yeah. There will be--once you stop ventilating--of course during ventilation everything is pretty much the same, but once you stop ventilating, there will likely be gradients. The answer is we're certainly aware of that and you'll hear Tom Buscheck of Livermore call it "the cold trap effect".

DR. SAGÜÉS: Right.

MR. PETERS: In fact, there is an extensive program to look at that in much more detail using both testing data from this test and the convection test and, you know, improving our model in that area.

DR. SAGÜÉS: Are any effects of this being considered in the present performance analysis?

MR. PETERS: I don't know the answer to that myself. Can anybody out there who's a PA person address that?

MR. BLINK: Jim Blink from Livermore. The graininess is of the order of 5 to 10 degrees C from the warmest to the coolest packages at the time of the highest temperatures, and I showed that on one of my temperature graphs yesterday for you. In the SFDA we also a very detailed table that goes to various points within the cross-section of the drift, within several cross-sections of the drift, looking at the relative temperatures at different points on the drip shield, the drift wall, the invert, and of course what we see is the waste package is the warmest point in any cross-section and the cooler points are usually at the drift wall. So we think that most condensation would occur near the drift wall or in the near field rock.

In the Ventilation Test No. 1--or actually in the earlier tests, the canister tests, we also tried to mock this up at a quarter scale and we did not see condensation on the inside of the drip shield but rather we saw condensation down near the bottom of the drip shield at the invert. So we're very interested in the subject. In the SSPA, Chapter 8, we also did an alternative model to take a look at that and the model did not show any condensation on the bottom of the drip shield. We were prepared to carry that forward if we did see it, but the model didn't prove to show the condensation. We're doing more work in that area because all of the models of condensation so far are fairly coarse models.

DR. SAGÜÉS: I see. So far the most that you have done don't show any important humidity effect and whether the humidity matters due to a short-term or short distance temperature differences in packages; am I saying that right?

MR. BLINK: Yeah, we haven't seen any firm results that look like it's a problem, but we're not ready to write the issue off. In fact, we're doing more sensitive calculations using fluent--

DR. SAGÜÉS: Okay.

MR. BLINK: --code to try to get at it.

DR. SAGÜÉS: But it appears this needs to be a strong humidity effect due to the fact that there's a relatively small temperature difference. So one would say how come you don't see it in your models?

MR. BLINK: In this situation you have an axial temperature gradient, but in the region of condensation you don't have any radial temperature gradient. You don't have any heat source in the region of the drift that's getting wet in the ACRB. So it's a different situation. What it would imply is in a repository situation you're going to see condensation in the perimeter grips rather than in the emplacement grips.

DR. SAGÜÉS: How about when the temperature begins to come down, and wouldn't then some packages begin to develop--you know, you get the whole deliquescence issues and so on--wouldn't then some packages be getting wetter on their surface a lot sooner than other packages and maybe even getting wetter at the expense of the others because of the others being warmer?

MR. BLINK: Our PA models assume that there's a dust on all of the engineered surfaces, drip shield and waste package, and it's controlled by a particular salt. And when the humidity, the local humidity, comes back up to the level that you would have deliquescence, we turn on the corrosion switch. So we have a conservative approach to that already.

DR. SAGÜÉS: I see, I see. I have another question that maybe I should have asked from Gerry Gordon yesterday. But in the science studies, when it comes to analogues for materials, the issue as to whether there is any kind of a long-term example of passive behavior, specifically I think you are going to be looking at things like Josephinite. Has anything new been done on that?

MR. PETERS: I know the work's ongoing. Tammy Summers can speak to it.

DR. SAGÜÉS: Okay.

MS. SUMMERS: Summers, Livermore. We have looked a little bit at the Josephinite since the last meeting. Specifically we looked at the sample Gerry showed, which had a metallic appearance. We looked in XBS. We sputtered down to 120 nanometers, and we did see metallic iron and nickel mixed with oxides as little as 2 nanometers. So we do know that there is some metal on the surface. We don't know the morphology yet. It's likely that it's a mixture of oxide and metal, probably very small grains. We're looking into that further.

DR. SAGÜÉS: So the question as to whether we have anything resembling an active or passive layer, that's still open, then, or do I understand correctly we don't even know if it is a metallic sample yet?

MS. SUMMERS: We know that there is metal near the surface, at or near the surface. We don't know the size of the grains, we don't know how much metal.

DR. SAGÜÉS: But what I mean is, if you look at it, it's a shiny piece of metal looking thing, like nickel, or--

MS. SUMMERS: This particular sample is, but I believe, and I'm not sure, that some oxides can have a metallic looking appearance, so we're attempting to sort that out now.

DR. SAGÜÉS: Okay. All right. I just want to say quite explicitly that I, for one, feel that it would be very reassuring to find an example of a metal that has stayed passive over a geological time frame. Needless to say, that would I think answer a question that has been asked already for quite a long time. Thank you.

MS. SUMMERS: I think we agree with you.

DR. SAGÜÉS: Yes. Okay. Thank you.

DR. WONG: Unfortunately, my colleagues have decided to ask 19,000 questions within the time allotted. I have four people who still want to speak, but unfortunately we have to move on, so I apologize.

Thank you, Mark.

MR. PETERS: So what you're telling me is I should run right now so they can't catch me afterwards?

DR. WONG: Right. We do have till 2 a.m., but--

UNIDENTIFIED SPEAKER: Yeah, we've got 12 more hours.

DR. WONG: Our next speaker will be Rob Howard. He's the integration manager in Science and Analysis Organization for BSC. He was up here earlier this morning answering questions. So, Rob, please continue with your beating.

MR. HOWARD: Okay, well, for the beatings to matter I have to feel them, so there is some good news. We spent two pretty good days with a subpanel on the board on this particular document in June, and I'm not going to go through all of the details we went through in those two days if that's okay with you. I do have people here, not as many as

I thought because of the travel situation, who can help answer questions that you may have on details, and I know at least some of the staff have been digging into the document pretty hard because I've gotten some pretty good and insightful questions from them over the summer. So what I'm going to go over is just kind of what the scope and contents are, I'm going to try to correlate it a little bit to what the NWTRB priority areas are, and wrap up with some conclusions.

Next slide, please.

The scope of the SSPA, we had three general types of information that we were going after and trying to capture in this document. The unquantified uncertainties analyses, those sometimes called conservatism. We'll show in at least one case they weren't conservatisms, they were in the TSPA-SR. We've tried to more explicitly quantify including different parameter ranges, looking at different conceptual models and alternative assumptions. And where we had biased inputs in one direction we were looking for more unbiased information out of the principal investigators.

And I should point out that, you know, I get up here and talk about this document, I've done it several times already, and it does represent the work of several hundred scientists and engineers on the project, quite a massive undertaking and lots of people worked on it, it wasn't just me. I'm not that prolific or smart.

Updates in scientific information, Mark talked to you about some of those updates in scientific information. We did take the test data that the testing was reflecting and what was available we tried to incorporate into the new models, and that's always a good thing to try to constrain our models by data. And so we tried to do a little bit of that in the updates.

And the thermal operating modes, Jim Blink covered that in some detail yesterday.

Next slide, please.

So what do these documents look like? We'll have an introduction and the methods and approach, describe what they were about, how we went about business, how we went about collecting new information and new distributions in some cases.

The content and level of detail for each section--this is in Volume 1--is quite variable and it can be somewhat troublesome to the reader when you look at the unevenness of the documentation. And there's a couple reasons for that. One is that, you know, just the extent of the analysis that had to be performed and the amount of new information that was collected during that time frame between when the AMR's and PMR's had been published and when this document was published, it was dependent on the process area, the data generated, which is different, and the amount of information that was necessary to evaluate the range of thermal operating modes. So we had a lot more detail with respect to couple processes and the rock and the EPS and drift environment than we did for, say, biosphere, and that was--each section contained a summary of information and recommendations for use in Volume 2.

Next slide, please.

Again, Volume 2 racked out somewhat similar to Volume 1, where you had introduction, methods and approach. Section 3 was sensitivity analyses, and these were system-level evaluations for the nominal scenario, looking at basically one-offs, also subsystem-level evaluations, and these were against the TSPA-SR Model, so it gave you basically a delta analysis between performance with these different model adjustments and the TSPA-SR. So that was mainly to inform us on where we were with respect to uncertainties in the TSPA-SR from individual adjustments and then also used as the basis for what process models we carried forward into the supplemental analyses that we had to do for the comparison of the range of thermal operating modes.

Section 4 of Volume 2 contained the supplemental analysis, the analysis that we used to capture all the information we felt was appropriate and have available at this time for the range of thermal operating modes for the nominal scenario, we looked at the subsystem results for the nominal scenario and the evaluation of disruptive events and conclusions.

Next slide, please.

To touch on what the relationship is between Volume 1 and Volume 2, it's similar to the relationship between the AMR's and the PMR's and TSPA Rev. 00, ICN 01, where Volume 1 provides the technical basis for those total system analyses that were documented in Volume 2. The one-off sensitivity analysis in Volume 2 and the guidance that I just mentioned that's in each section from Volume 1 determine the content of the TSPA supplemental models.

Next slide.

UNIDENTIFIED SPEAKER: Rob, can you talk a little bit louder, it's a little difficult to hear you.

MR. HOWARD: Sure. Attributes of the repository performance, all I wanted to do was remind people--and Carol Hanlon touched on this this morning, that we documented our work in these analyses similar to the way it was organized in the science and engineering reports, so we went through the different expected processes that we think we're going to see at a potential repository at Yucca Mountain and documented our results in that manner, so trying to make it easier for reviewers of both documents to have a correlation and present the information in a somewhat systematic way because it does tell you where we are with respect to the science and engineering report. And I'll go through these areas in a little bit of detail.

Next slide.

For unsaturated zone flow, what have we done? Well, we have examined lateral flow in the Paintbrush Tuff, we have expanded the 3-D flow fields. What we had to do in the consideration of the range of thermal operating modes was look at the fact that we probably would have to expand repository footprint into other areas. So Bo Bodvarsson and the folks at Lawrence Berkeley extended the model domain for the UZ flow and transport models to capture a larger area of real estate. And we found that the flow fields when we did that were similar to the flow fields that we had done in the past. There were some differences when we looked at what's going on itself as far as transport times, and I'll talk about that in a bit. We included the lithophysae properties, thermal properties, in these analyses. We saw results that Jim Blink showed yesterday on the importance and the sensitivity of those, and I'll also show a little bit later why that's important because of the real estate that we're occupying.

The new THC model development we're working on was in the scope of the previous AMR's and the PMR's in that area. And the THM model--and Jim showed you one result of the THM model in his discussion yesterday--we addressed multi-phase flow and calculated stress-induced permeability changes, which could have an effect on the flow fields.

Next slide, please.

The flow fields, just to point out and orient everybody, the lower lith, if you look at how much real estate that occupies and what the material properties are, it's important that we recapture that. Also note that in Volume 1--this does not occur in Volume 2 when we did the total system analysis--we were looking at that larger footprint area extending to the south, and I'll note that the extension to the north here is further than it was considered in the previous AMR's and PMR's. It had limited effects on the UZ flow fields, but it did have some effect on UZ transport times through the saturated zone.

Next slide.

For THC mountain scale, I just wanted to show one result, and this was a result that Bo Bodvarsson had shown to the panel in June and it does correlate a little bit with what Jim showed you yesterday on the drift scale chemistry results for the high-temperature and low-temperature operating mode. But the pH of the waters forming above and around the drifts in the repository went up there on the order of 7-9, were those pH values. And the CO_2, because of the degassing, was going down. So CO_2 goes down, pH was going up. Chloride concentrations that were passed onto the drift scale modeling reflect reductions and dilution from the condensation and the increase was owing to boiling and evaporation through the gas-based convection for the high-temperature operating mode, not as extensively the low-temperature operating mode. And then the effects of seepage chemistry will propagate it through to the TSPA.

Next slide.

Seepage development. We expanded the seepage model to include the lower lith, and again, because as I showed you two slides ago that was important because a considerable amount of repository real estate is in that unit. We reduced the conservatism and the flow focusing factors. For the flow focusing factors that we use in the TSPA-SR were only 40 or 50, in these analyses, when we try to take a more realistic approach, we reduce those flow focusing factors down to in the order of 4 or 5. So we dropped them about in the order of magnitude.

THC and THM, we looked at the range of thermal operating modes. We had to do multiple sensitivity analyses for high temperature and low temperature, and then we developed a fully coupled THM Continual Model and improved the Distinct Element Model. So we have two different models in that area.

Next slide, please.

The EBS system, the main improvement was the propagation of the chemistries that came from the mountain scale UZ down to the drift scale chemistry into the TSPA. The soil horizon CO_2 concentrations, we looked at the sensitivity of that, and then those were variable for both the high-temperature and the low-temperature operating mode.

Next slide.

Just to give you a comparison, this is kind of a shorthand of some additional tables that you have. As backup information, the same tables are also in both volumes of the SSPA in the front of both volumes. But, you know, where we hadn't included the uncertainty in those AMR's and PMR's that we had documented in the science and engineering report, we did try to address those more extensively in these analyses. So we looked at, again: compositions of liquid and gas entering the drifts; seepage invert mixing and interactions, and yeah, we didn't include that in the TSPA model; trace element compositions and effects on chemistry; sorption on the corrosion products; generation of colloids; and cement leachate effects. Cement leachate effects on drift chemistry, since we don't have a whole lot of cement in the current design, wasn't a whole lot of point in propagating that all the way through.

Next slide, please.

UNIDENTIFIED SPEAKER: Excuse me, Rob?

MR. HOWARD: Yes?

UNIDENTIFIED SPEAKER: I'm having a hard time hearing you. I don't know if it's volume or just you need to project a little bit more.

MR. HOWARD: Okay.

UNIDENTIFIED SPEAKER: Sorry.

MR. HOWARD: Do you want me to go back to the last slide?

UNIDENTIFIED SPEAKER: No, that's all right.

MR. HOWARD: I apologize for that. Waste package corrosion--

DR. RUNNELLS: I didn't hear the last thing you said on the last slide.

MR. HOWARD: Okay, let's go back to the last slide.

DR. RUNNELLS: You mentioned generation of colloids from corrosion products.

MR. HOWARD: Yeah, I mentioned it, but the last thing I said was with respect to cement leachate and effects on the in-drift chemistry, and what I said was we don't have a whole lot of cement in the placement drifts, so we don't really need to propagate that through.

DR. RUNNELLS: Thank you.

MR. HOWARD: Okay, waste package corrosion developments. I guess this is where the beating continues. But since we've talked about it quite a bit already, all I'll say is that, you know, we did look at additional range of water chemistries, considered to a limited extent the effect of soluble lead and other minor constituents in the natural systems. And April showed you results that Catholic University had done that had what we consider minor constituents.

The question was raised, "Well, what's the relevance of those results and why are you showing us these results if these aren't the expected conditions?" Another way to get at the problem, I think that those results are useful for us to look at at the Project because what she demonstrated was that, you know, these materials will corrode under certain conditions, and we'd better understand why or why not we have those conditions. And I think that it's just a different angle of tackling the problem, and that's useful in many ways to try to formulate or look at what could go wrong or what could happen in a different way just so that you understand why you don't think that it could go wrong. Because it does show that these materials are not, you know, no corroding, they will corrode.

We considered sources of other soluble salts, and Gerry touched on that a little bit yesterday about the programs that we have to better characterize the rock dust and the in-coming ventilation dust. We're, you know, continuing to look at a dust sampling that's been generated since the mid-'80's and also what we can generate at the site.

Next slide, please.

Phase Stability in Alloy 22. Tammy Summers and her team did some additional theoretical modeling of the base metal. We did not show any phase stabilities under repository conditions. We did not show any evidence of long-range ordering as long as temperatures were below 300 degrees C, so that's an important temperature dependency to keep in mind. And preliminary weld data did not indicate instabilities below approximately 200 degrees C.

Alternative lines of evidence. The degradation in mechanical and corrosion properties due to aging did not appear to be likely below 300 degrees C. And Alberto talked a couple minutes ago and Tammy answered him to some extent on looking at these other natural alloys, if you will, that may indicate a stability of passive films over geologic time frames. And so we've taken those issues to heart, we'll look at them, we think that they're important and we're going to continue to look at them. We have a little bit in the SSPA on that.

Next slide.

Waste form mobilization. In-package chemistry, we looked at the effects of high-level waste degradation rates and steel degradation rates and how those can change to in-package chemistry over time, which could in turn affect dissolution rates.

We looked at dissolved concentration limits of thorium, neptunium, plutonium and technetium. We looked at the different controls on that and we had updated the solubility models that have lower means and wider ranges. That's one of the things that Bill showed you yesterday as part of the uncertainty analysis that we did.

For cladding, creep rupture and stress corrosion cracking, we looked at different failure criteria for those models, localized corrosion rate uncertainty, tried to characterize a little bit better seismic failures. We have updated information on seismic analysis, so we included those sensitivities in there, and unzipping velocity uncertainty.

We did develop a simplified model that expanded the range of reversible and irreversible colloid plutonium attachment. That's another sensitivity analysis that we've done in the waste form area.

Next slide.

Flow and transport modeling in the engineered barrier system. Things that hadn't been looked at in any detail for the science and engineering port was the seepage evaporation rate in the drip shields. We took another look at our drip shield and waste package flux models and the splitting algorithms that we had for where the fluxes were going to go.

In-package diffusion, we developed an in-package diffusion model. A couple things to point out about that model. One thing is yesterday Abe told you his unauthorized view of the TSPA Peer Review Panel's thinking on this and they thought that, you know, our continuous film model for diffusion in the waste package was unrealistic or incredible. I wonder what they would have thought of our model before that one.

The NWTRB I think about a year ago, September 20th of last year, in their letter to Department of Energy, one thing that I do recall about that letter is they mentioned the fact that--or they suggested that we could develop a transport model within the waste package and look at that as a way to look at different performance. So that was one thing I remember. There's a couple other things I remember. That's one I wanted to point out. So we did take a stab at that and maybe it is incredible to have continuous film, but it's better than what we had before, which was an instantaneous pathway, as the Board appropriately recognized over a year ago, so we did take that to heart.

And radionuclide sorption within the waste package and the sensitivities to that. We developed models that we had not included in the TSPA-SR.

Next slide, please.

Some other things, the drip shield condensation. Alberto was asking a little bit about that, about the temperature differentials and how water might condense in the drip environment, so we developed some analyses of that to look at whether or not that was going to be an important process that might effect transport. We did some sensitivity analysis on alternate conceptual models, what we call the bathtub model, versus the flow-through model. Diffusion through the invert, we did additional work in trying to develop how that process is going to occur, and then microbial sorption and transport, we did some additional sensitivity analysis in Volume 1. We didn't carry those through to Volume 2, but there is work in there on that.

Next slide, please.

Transport times. This is UZ transport. I mentioned that the experimental repository footprint didn't have major effect on UZ flow. The flow fields were in fact similar, but there were some differences in transport. The drift shadow model, we did some preliminary development of how that model might work and predicted that the transport times, if you include the drift shadow effect, could be on the order--five minutes, okay.

Next slide, please.

Including the southern extension as far as transport goes would result in slightly longer transport times to the water table.

Next slide, please.

For the saturated zone, we included new data that we got from the Nye County work, looked at the hydraulic head and water level elevations, so we recalibrated on that.

Mark talked about the Alluvial Testing Complex, so I won't get into that in any detail.

We had in the SC portion of the SSPA an alternative conceptual model for the large hydraulic gradient and we had alternative representation of the Solitario Canyon Fault.

Next slide. Next slide.

Biosphere, I want to touch on biosphere. We updated our FEPs analysis in the biosphere area for what we thought were relevant processes. Relative exposures to receptor groups, we knew the issue was coming up with respect to a critical group of reasonably, maximally exposed individual, so we did some head scratching in that area that helped us prepare for the calculations that we're doing right now with respect to 197.

Climate effects on water usage and ingestion exposure, per 197 that's one of the things that you're supposed to look at in the biosphere, is the climate. Transfer coefficients. Revised biosphere dose conversion factors based on this information.

The question was asked yesterday which has more uncertainty, the waste package performance or the biosphere model. The biosphere has much more uncertainty than the waste package, but regulatory uncertainties seem to dominate that issue, and so a lot of those uncertainties are taken out by regulation. I think that that's an important thing to consider as a modeler. I mean, when you do do the calculations, consequence calculations, at a specific target, you have to formulate the problem. It's an Eularian formulation, so you're looking at the problem differently. If you weren't looking at consequences at a specific location, you know, have a Lagrangian formulation of the problem, it could produce different insights. I believe that's what Abe was talking about in his unauthorized translation yesterday with respect to looking at the problem as a fate of contaminants problem rather than a consequence problem over long periods of time. So it does give you a different insight into how the system behaves. It's not formulated with respect to a consequence to a receptor.

Next slide.

Disruptive events. We updated wind speed information, and that was related to a KTI agreement we had and that had effects on the disruptive dose consequences. Probability of dike intrusions were reconsidered. We had scaling factors for different layouts, but they weren't propagated through Volume 2. Evaluation of dose sensitivities to waste particle size distributions in an igneous eruption, that again was related to a KTI issue that we had with the NRC staff.

Next slide.

Volume 2, again, the one-off sensitivity analysis, I know that there is some trouble with how we use these analyses where we looked at the result at the subsystem level and we made some what I would consider rational decisions about whether to move them forward or not. Results are directly comparable to the TSPA-SR, so it tells you where we are with respect to that document.

Next slide, please. Next slide.

NWTRB priority areas, you guys know what your priority areas are, I don't need to tell you that.

Next slide, please.

We did try to look at meaningful quantification of uncertainties and conservatisms in the nominal performance. As Bill showed yesterday, supplemental models show significantly wider ranges of doses, i.e. maybe more uncertainty than we had shown in our previous calculations at a given time and times to reach the given dose. After the first 10,000 years, the base case model appears to be conservative. The other way to look at that is before 10,000 years the SSPA appears to be more conservative. It depends on your frame of reference, again. And then just looking at mean results, and I don't mean to affront anybody on the Board, we're just looking at the mean results for the SSPA, they were on the order of 10^{-4} mrem per year as opposed to 0. So that was an area where we weren't conservative and it was a useful exercise to get at this information.

Next slide.

Thermal operating mode, Jim Blink talked about that yesterday.

Next slide.

Corrosion processes. We did document, you know, where our current understanding at the time was of the corrosion processes. We developed a framework for the conceptual model for long-term passive film stability. It was one of the models discussed at the workshop that the NWTRB hosted last month. Stress corrosion cracking, we've got updated information for our parameters and models there. And then we already talked about aging and phase stabilities. Temperature dependent general corrosion model, Jim went through that yesterday as well.

Next slide, please.

Multiple lines of evidence. The idea of multiple lines of evidence wasn't new to the project. The way to capture it was new. We readily admit that we hadn't done a good job of articulating what those lines of evidences and why we think the way we think about processes by using what we've gotten from past experiences and analogues. We did try to be explicit about this in the SSPA. I think just about every section of Volume 1 does touch on multiple lines of evidences. It tells you what we're thinking about them. I know it may not be the way the NWTRB Board or as individuals might define multiple lines of evidence, but it gives us now a point of discussion, and I think that that was good.

Next slide.

What have we learned by doing this? Quantification of uncertainties, improved our understanding of both conservatisms and non-conservatisms in our process model representations, so that was useful. Post-closure impacts of range of thermal operating modes and a variety of operating mode configurations can be evaluated by selecting the appropriate thermal initial conditions of model representations. What I mean to say there is, you know, we were looking at the thermal implications, we weren't looking at all the design detail implications in this analysis, so we chose thermal initial conditions that would get us at the lower temperatures and then did sensitivity analysis to show that you could get at that by a multitude of repository operation configurations. I know that's not quite the same thing as what Priscilla was thinking, but that's what we did, and I want to be honest about that.

Next slide.

Waste package degradation evaluations with respect to thermal operating mode need to consider the thermal dependencies and the local chemical environment. It's important it's not just a temperature parameter, it's a temperature and a chemistry that's going to give you waste package failures by any number of corrosion mechanisms.

Multiple lines of evidence, capturing helped us with our thought process and improving our own understanding and communication of what we believe to be repository process. I for one really don't know what it is that I know or don't know until I write it down and it was useful to start writing this stuff down. I'll note, as was noted in our meetings with the Panel in June, that we focus primarily on lines of evidence that support the thinking of the processes that we have. We do need to do more work with respect to going out and looking for lines of evidence that are contrary to what it is that we're thinking, make sure that we address the whys and wherefores of that as well. It's not the end of the story, it gives us a point of reference for continuing work.

That's it.

DR. WONG: Thank you, Rob. Dr. Runnells.

DR. RUNNELLS: Runnells, Board. I got ahead of the 19,000 questions of my colleagues this time, so I'm going to go back and Mark may want to address this, I wanted to ask it, but it's on one of your slides, though, Ron.

MR. HOWARD: Okay.

DR. RUNNELLS: Your Slide 16.

MR. HOWARD: 16, please.

DR. RUNNELLS: The second bullet, "Dissolved concentrations of thorium, neptunium," and so on, the second line there, the updated solubilities, we're talking there specifically about neptunium, I guess, as opposed to new solubilities for thorium, plutonium and technetium; is that correct?

MR. HOWARD: Christine Stockman, you want to shed some light on that? There she is. Thank goodness you didn't have to travel from Albuquerque today.

MS. STOCKMAN: We did do new ranges for all four of them.

DR. RUNNELLS: Experimentally or evaluation of thermal data?

MS. STOCKMAN: We reevaluated the data we already had and we made different assumptions about the redox chemistry within the package and the controlling solids.

DR. RUNNELLS: Okay. And some of the work is ongoing at Argonne in experimental work, is that correct?

MS. STOCKMAN: Exactly, yes.

DR. RUNNELLS: Just on neptunium?

MS. STOCKMAN: We're looking at both neptunium and plutonium. Right now we have some experiments planned where we will take spent fuel, fully oxidize it, and then do batch tests to see if the solubilities we would get under those conditions are similar to the drip or the Wilson batch tests that were done with most of the spent fuel not oxidized.

DR. RUNNELLS: Thank you.

MS. STOCKMAN: Um-hum.

DR. RUNNELLS: I don't have 19,000, but I have another one or two. If we could look at your Slide 15.

MR. HOWARD: 15, please.

DR. RUNNELLS: The very last line about Josephinite. I think that may be taking us down a misleading path unless we're careful. Until I know what the geologic situation is in which we're finding Josephinite in that creek in Oregon, I'm not going to trust anything about two-phase metastable structures for any number of years. Those nodules, as I understand it, are weathering out of a serpentinite, which is a rock that forms under reducing conditions and high pressures. And if those nodules weathered out last year from the serpentinite and now we find them in the creek, to infer that the metastable structures have existed under conditions that we care about, which are lower temperature, lower pressure and oxidizing, would be misleading. So my only point is, in this discussion of Josephinite and oxidized surfaces and metallic phases and so on, we have to know what the geologic environment was, is and was, for those materials.

MS. SUMMERS: Summers, Livermore. Actually, I think a lot is known about how Josephinite formed, and it forms at high temperatures under reducing conditions. The point here is that because the two-phase structure can be fit to the diagram, phase diagram, you can tell what temperature it formed at. What that tells me is that it has not changed. If it had changed after it formed, then it would not fit to the phase diagram anymore at those temperatures.

DR. RUNNELLS: I agree 100 percent, but the question is, how long has it been in the creek under oxidizing low-pressure conditions? It could have been there a year, and therefore it's--

MS. SUMMERS: No, it formed during the igneous intrusion, so--

DR. RUNNELLS: I agree with formed, but where we're finding it today is in the sediments of the creek downstream from where it formed.

MS. SUMMERS: Correct.

DR. RUNNELLS: And that's the information we're trying to apply to the metals in the repository. If it's ten years old, it may change. If it's one year old, it may not. The conditions under which it formed are extremely important, but equally important is how long has it been in the creek.

MS. SUMMERS: Here we're talking about changes in the internal structure, okay, and those changes are more likely the higher the temperature. It really is irrelevant how long it's been at room temperature.

DR. RUNNELLS: How about reducing conditions?

MS. SUMMERS: That doesn't affect the phase stability.

DR. RUNNELLS: Oh. You and I will talk about it independently because I'm taking too much time.

MS. SUMMERS: Okay.

DR. RUNNELLS: And we will, please. And my colleague, Dr. Sagüés, is going to pursue it, I can see that. One last question, please. Your Slide 10.

MR. HOWARD: 10, please.

DR. RUNNELLS: Can you explain why there are higher pH zones in that top illustration so far away from the repository cross-section to the lower right and to the upper left? That one, um-hum.

MR. HOWARD: The lower right and over here?

DR. RUNNELLS: Yeah, right.

MR. HOWARD: No, I can't, but maybe Dr. Houseworth can. And if he can't, then--

UNIDENTIFIED SPEAKER: Is this a north-south cross-section?

DR. RUNNELLS: I'm just wondering if you really attribute it entirely to the degassing associated with the projected repository if it's happening that far away and lower.

MR. HOWARD: You want to take a stab at it, Jim?

DR. HOUSEWORTH: Jim Houseworth with Lawrence Berkeley. Rob, I don't think I can help you on this one.

DR. RUNNELLS: Having covered that one, let's go to the tough one.

MR. HOWARD: Thanks, Jim.

DR. RUNNELLS: Kind of wish he'd gone home. In the last diagram on this page, if I look at the chloride concentrations, the color is a little hard for these old eyes, but it looks like 10_5 mg. per liter right in the center of that dark blue, at least on the print that I have, it's easier to see. It's harder to see on that slide.

MR. HOWARD: Yes, it is.

DR. RUNNELLS: But in the xerox print, the center is about 10_5 I think, maybe $10_{4.5}$. That's somewhere around 100,000 mg. per liter of chloride. But in the presentation that Gerry Gordon gave yesterday, the highest chloride concentration that was used was in the 1,000 XJ13 water, which is about 5,000 mg. per liter. So this illustration--I sense motion out of the corner of my eye--is about 20 times higher in chloride, and if you apply that to fluoride, you get up to about 30,000 mg. per liter fluoride in a sodium chloride carbonate brine. So I'm asking about the comparison between this model and the experimental work that's being done with simulated waters.

DR. HOUSEWORTH: Jim Houseworth, Lawrence Berkeley. The only thing I can say is that with these THC models we're finding these extremely high concentrations when we're down to very low water contents, typically, and that water is generally not mobile because it's at such a low water content. So in that case those high concentrations and very low residuals of water exist, but maybe not moving.

DR. RUNNELLS: Just make that comparison for me, you know, later when you have time.

DR. HOUSEWORTH: Okay.

DR. RUNNELLS: Because if you do that extrapolation, you get 30,000 mg. per liter fluoride. Okay, my 19,000 questions are over, Dr. Wong, thank you.

DR. WONG: Okay, we have four people who want to ask questions. The first person will be Dr. Craig, the second person will be Dr. Sagüés, the third one will be Dr. Bullen, and the last one will be Dr. Parizek.

DR. CRAIG: Paul Craig. The relevant figure here, John, is No. 19, and this has to do with the drift shadow, which we talked about previously. Now, one of the things that we know is that the Payer Panel told us that there's no reason to believe that the C-22 won't work, which is sort of a weak statement. Maybe it will. But if it doesn't, the mountain needs to do something. And back a year and a half ago Bo Bodvarsson educated me on their modeling on the unsaturated zone and had me read a famous Phillip's paper, which shows that the drift shadow effect is absolutely a real phenomenon if you're dealing with a homogeneous medium. Very compelling. Here you're not dealing with a homogeneous medium. You get a very large effect, which turns out now to be really extremely important in this new document. As a matter of fact, even with the new effect, while it helps a lot, you still have transport times which are comparable to or substantially less than 10,000 years. You now seem to be relying on a brand-new silver bullet which has, the best I can tell, almost no experimental validation underlying it. This is almost pure modeling. Why should we trust it?

MR. HOWARD: Well, why should you trust it? The short answer to that is that you shouldn't trust anything, you should do exactly what you're doing, which is examine it very carefully and see if the weight of evidence would convince you that it's a reasonably expected thing to happen. I'm going to give Dr. Houseworth the chance to save my job one more time.

DR. HOUSEWORTH: Well, I don't think that you can say we're relying on it at this time.

MR. HOWARD: That is a good point, it wasn't propagated through, so--

DR. HOUSEWORTH: Well, there was a certain part of it that was propagated through just in an attempt to look at it, but if we were to rely on it, we would certainly be doing some testing, and we in fact have been looking into ways to do testing for this.

DR. CRAIG: So when you say you're not relying on it, does that mean that we should consider that it's not included and we should ask for data that does not include it? Is that the point you're making?

DR. HOUSEWORTH: It was put in as I think a one-off in the SSPA, right?

MR. HOWARD: It was a sensitivity, yes.

DR. HOUSEWORTH: That's all I'm saying as far as the TSPA, but it's not, you know, baseline.

DR. CRAIG: It is not in the baseline?

DR. HOUSEWORTH: No.

DR. CRAIG: Okay. Thank you.

MR. HOWARD: Well, now Leon is going to put the nail in the coffin on my job and say that that's not true, so thanks for trying, Jim.

DR. REITER: The assumption is that according to your own table you did include it partially in effect that you assume that all the diffusive releases went into the matrix and all the invective releases went into the fracture. And that is listed as part of the drift shadow effect.

MR. HOWARD: Yes, that is a good clarification. I was thinking more of the implementation side with respect to the modeling in Volume 2 with respect to the process level modeling in Volume 1, and we didn't go at it the same way. The effect I guess you could correlate that way.

DR. HOUSEWORTH: Right. Jim Houseworth. There were certain aspects of the drift shadow process model that weren't carried forward except for the splitting that you are referring to.

DR. WONG: Dr. Sagüés, you yield your time?

DR. SAGÜÉS: Well, I'll respond to the benefit of Dr. Bullen and if we have time afterwards I would like to ask my question, but I promised to him that--

DR. BULLEN: Oh, go ahead, go ahead. I just have one quick one.

DR. SAGÜÉS: Well, actually, I wanted to bring up this thing that we left at the end of the presentation of the "Preliminary Site Suitability Evaluation". If you can find Picture 19 from two presentations ago, the one on preliminary site suitability evaluation. It should be listed under Sullivan in the printout. Sullivan. Yeah, 19.

(Pause.)

DR. SAGÜÉS: Okay, very good. This brings us up to the issue that we had a little while ago, and the question here had to do with uncertainty on corrosion models. This line in here represents what would happen if one would assume, I believe, that 1 out of every 4 realizations, 1 package out of 10,000 would experience some kind of a massive material problem or a severe material problem because of that weld maybe that didn't get--now suppose that that kind of an effect were comparable to the presence of, say, significant pitting in the package, elemental things will be more or less equivalent or not. Some year it will help them, somewhere around year 2,000, something in that order, relatively early in the stage of the system.

Now, if that were to happen because the signs were severely wrong on corrosion, and now we have all the packages having that kind of a problem, then am I correct in saying that that would be sort of comparable to lift in one, two, three, four cement, we're then multiplying by four, which will take us around there somewhere? Is that sort of like a reasonably ballpark way of thinking about it?

MR. HOWARD: That's exactly the calculation that Jim Blink and I went through.

DR. SAGÜÉS: Okay.

MR. HOWARD: So yes.

DR. SAGÜÉS: Okay, so that will take us dangerously close to the 15 mg. kind of that's somewhere around there, I believe. Now, suppose now--and I just want to continue a little bit farther--suppose that the science is okay, but not quite okay, maybe 90 percent okay, then in 1 out of every 10 sort of alternative features, this science was wrong and the high temperature will result in excessive pitting. So then that will take us from there to there. And if the science is very good, 99 percent good, what it will take us up to there, a couple of us know what we'd really like to be, but still, you know, it begins to look pretty bad, even if the science is 99 percent right. I mean where am I wrong with this chain of thinking? I would like to hear what you have to say about that.

MR. HOWARD: Okay, I'll tell you what I have to say about this, then. I don't understand if the science is 99 percent right how I could correlate that to those high doses. But I will tell you that as far as localized corrosion goes, the corrosion scientists do have models for localized corrosion, we do know that localized corrosion can occur with these materials, the question is whether or not we have the right environmental conditions for it to occur. So in the ESPA we did not screen it out as Dr. Craig had implied with the low probability. In the FEPs process, localized corrosion can occur and we allow it to occur if the conditions exist. So that's one part of the story. It's in the model.

The other part of the story is, although it may not be convincing to everyone whether or not the science is right, that's the fundamental question that we're going after with the waste package peer review. It's a fundamental question that is gone after with the TSPA peer review panel looking at the science behind it. The NRC and the Center are also going after that fundamental question. We, ourselves, continue to ask that question whether or not the science is right. So the probability of the science being wrong is, what, it's one minus probability of waste package is right. Before we assign numbers to it--and I'm not sure if that will be a useful exercise--we have the waste package peer review, we have the TSPA peer review, we have the NRC staff reviews, we have the Center reviews, we have the State of Nevada.

DR. SAGÜÉS: Now, I may have to back up and go the other way, but yes.

MR. HOWARD: No, well, I mean we have to consider all of these--

DR. SAGÜÉS: Yes.

MR. HOWARD: --inputs I think. We have the NWTRB, we have the Department of Energy and contractors. And I believe all of those organizations are after the same question, whether or not that science is right. And I hope that everybody is after that question.

DR. SAGÜÉS: Yes.

MR. HOWARD: I have a fundamental interest, we're talking about the safety of my daughter, right?

DR. SAGÜÉS: Right. Now, what is interesting, though, is could this be the beginning of some way of addressing uncertainty on models being drawn and incorporating them in the performance models?

MR. HOWARD: Jenny, can you help me with this one? I don't know how to get at this kind of elicitation, which I think it is. Karen, maybe you can help me.

DR. WONG: Alberto, is there a way that you can have your question answered later?

DR. SAGÜÉS: Sure. Certainly. I just wanted to bring up the issue.

MR. HOWARD: It's a very important issue and there's a lot of people looking at it, and I think we all owe it to ourselves to make sure that we rack it out.

DR. WONG: Dan Bullen, if I give you time, are you going to buy me dinner?

DR. BULLEN: Bullen, Board. And I'm going to actually try to tie into what my colleague, Alberto Sagüés, just brought in, and I'm happy that he brought up this diagram, because even though you thought you got saved by the bell with respect to going by the NWTRB priorities, one of the questions that we have is an evaluation and comparison of the base case repository design with a low temperature design. And my interpretation of the presentations in the past two days and the SSPA and all of the evaluations that we've done is that your interpretation that these curves and that comparison to that answers the issue. And I guess the question that sort of stuck in my mind and it didn't strike me until, you know, a day after Jim Blink's presentation, but Jim talked about the comparison of LTOM and HTOM and noted that there were a couple of errors in the thermal hydrologic models that were presented earlier that got rectified, and that's fine. But he made a statement, and I guess it didn't strike me until I had lunch with a colleague and we talked about things, that his statement was--and I looked at my notes--"If there's no permanent changes in the natural or the engineered barrier system, then these things happen." And so I guess the thing that strikes me is, if I heat the mountain up in boiled rock and I should be able to get data from all types of experiments that we've been running, including the drip scale heater test, that would show me the kinds of changes that I get when I do and don't boil the mountain. And the kind of changes that I would expect to see at a high-temperature design that may have localized corrosion more effectively operational versus one that doesn't have more effective operational, then I don't think I'll see such similarities. And I know there's, you know, a couple orders of magnitude at different locations and the like, but I guess the issue there is, we've asked you to take a look at a low-temperature repository design, you've made great strides, maybe you're not quite there. Would you like to respond to that, I guess is what I'm saying.

MR. HOWARD: Yeah, I'll respond to that. You specifically did say in your letter lower temperature designs. What we did was a range of operating modes, and there are probably both semantic and conceptual differences with what we did. This is what we could do to address the issue with the information that we had. It's not starting with a clean sheet of paper, as I think Priscilla has suggested, more than once, and I acknowledge that. I wouldn't want to make it more than it is.

DR. WONG: I apologize, Dr. Parizek, I have to pass you by.

DR. PARIZEK: Parizek, Board.

DR. WONG: Dr. Parizek, I have to pass you by.

DR. PARIZEK: Oh, you have to.

DR. WONG: Yeah.

DR. PARIZEK: You said you apologize for passing me by.

DR. WONG: I'm passing you by. Debra Knopman has a statement to make but not a question.

DR. KNOPMAN: Knopman, Board. Now, you put in some backup slides on the large hydraulic gradient. This is a big question the Board has. We don't have time to address it, I just wanted to note that I didn't want you to think we didn't care about that.

MR. HOWARD: No, I know you don't.

DR. KNOPMAN: And we'll talk about--

MR. HOWARD: That's why I put them in there, I wasn't sure what we were going to get to.

DR. WONG: Okay, thank you, Rob, you still look well.

Okay, we are scheduled for a break about a half an hour ago, and we will take a break, but first April Gil has an announcement that she would like to make.

MS. GIL: Yes, I just wanted to say that quite a few people are asking about the range of operating temperatures, key technical issue, technical exchange the Department of Energy and NRC were going to have on Thursday and Friday. We're evaluating whether or not we're going to be able to have this meeting in light of the tragedy that happened this morning. We're trying to set up a video conference in VTEL with the NRC at Rockvale and the Center in San Antonio if possible. We won't know the answer until sometime tomorrow whether or not we're going to go ahead and have the meeting, and as soon as we know, we will let you know. Thank you.

DR. WONG: Thank you. All right, it's 3:38 by the little clock that we have here, ten-minute break, expect everybody back at 3:50.

(Whereupon, a break was taken.)

DR. WONG: All right, our next presentation is from Dr.

Dale Hammermeister, who is working with Nye County. Dr. Hammermeister has extensive experience in hydrogeological processes and site characterization related to both solid hazardous or radioactive waste. Dr. Hammermeister, please.

DR. HAMMERMEISTER: First I wanted to thank the Board for giving us the opportunity to let you know what we've been doing lately. This work is funded, as you know, by the Department of Energy. We have a cooperative agreement with the Department of Energy. Drew Coleman is the technical lead. I am one of three members of the Nye County group, technical group, Rena Downy and Kathy Gilmore make up the rest of the group. Most of the work is done by Nye County Consultants, a talented group. Jamie Walker is our senior geologist, Tom is the lead hydrogeologist, Jay is our drilling engineer, and Dave is with well design and aquifer testing.

But anyway, today, very quickly I wanted to just go over our EWD DP Drilling Program, the Phase III primarily, and also to talk a little bit about some ventilation work that's going on.

Could I have the next slide, please?

I'd like to talk about our overall program objectives, where we are in the program. The location of our Phase III wells that we're putting in, drilling and completion objectives, our geologic sampling and testing objectives, and then really spend most of the time talking about some preliminary results that we've got from our drilling which we think are real interesting, and then some proposed additional wells.

Next slide, please.

I'm not going to talk about a whole bunch of other things that Nye County is doing associated with putting wells in the ground.

Next slide.

The overall program objectives have always been to develop a capability for early warning groundwater monitoring network between Yucca Mountain and Amargosa Valley, the populated areas, to establish a baseline water quality information, and to fill in hydrogeologic data gaps in a bunch of areas: flow paths between tuff and alluvium; nature and continuity of alluvial textural layers. We're interested in the layering in the system--Nye County, of course, is interested in the health and safety of the Nye County residents and we're interested in potential preferential flow paths through coarse grain layers and also finer grain layers that may retard the movement of contaminants. And of course we're interested in hydrogeologic units underlying alluvium, hydraulic gradients and flow and transport parameters.

I'm going to primarily talk about--I mean the data we'll present today, we'll talk primarily about layering in the system, some information we've obtained from our drilling program about layering in the alluvial system, a little bit about hydrogeologic units underlying alluvium and flow and transport parameters.

Next slide, please.

You've been updated before on Phases I and II. I won't go into that right now. Generally, we did some good technical work we think and produced some good data and also learned an awful lot I think, and we're trying to put this into Phase III, which we started in July. And we're focusing primarily on filling data gaps in the zone of uncertainty, and that is the zone between Highway 95 and Yucca Mountain.

Next slide, please.

The Phase III wells are shown here. I want to talk about their location. We are drilling two wells, the fact is we completed two wells at the ATC location, IM1 and IM2. We have two other optional wells at that location and we have two wells at the 22 location, two at the 10 location and two at the 18 location.

Next slide, please.

This is the ATC location. The existing wells are 19D and 19P. We finished drilling and completing IM1 as a multiple screen monitor well. We'll show you, again, completion a little bit. And we just finished drilling and we're in the final stages, the final day or two, of completing IM2.

Next slide.

This is the typical completion, it's a multiple screen well. The screens, of course, are sealed with bentonite seals between the screens. The wells are suitable for sticking in package systems to isolate zones so that we can do tracer tests, we can pump from these zones, we can do hydraulic tests, and we can obtain different water quality samples from the different screens. Nye County is actually going to install some Westbay equipment, which is a removable package system that allows us to sample and to monitor from individual screens.

Next slide, please.

At the upgrade in locations, the 10, the 18 and the 22 location, we plan to drill two holes, a P-hole, a piezometer, which is really a dual completion piezometer, and we'd like to think about it down the road as potentially a tracer injection well, and about 60 feet downgradient we're going to install in each of these locations a 1,000-foot multiple screen monitor well very much like we saw just in the previous slide. Nye County intends to at least have the capability to do tracer tests at more than one location. Currently tests are only being conducted at the ATC location. Nye County has proposed for future work that perhaps we could do some tracer tests at other locations in the

Fortymile Wash.

Next slide, please.

This is a typical piezometer. It's a dual completion piezometer. We are interested in vadose zone, too. Mainly we are interested in characterizing bulk permeability, bulk air permeability, so we plan to install some air piezometers and do some monitoring. Again, we're interested in the layering. We feel that a vadose zone can supply a little bit of information and might be useful that we could transfer it to the saturated zone. And again, looking at the atmospheric barometric pressure wave we can back out bulk scale permeability, and we feel that the larger the scale of the estimate probably the more useful the data is.

Next slide, please.

This just summarizes the types of the wells we just talked about. We have basically two types of wells we're going to be drilling, multiple-screen monitor wells and the dual completion piezometers. Piezometers are limited because of our limited budget. Those are only at the upgradient locations.

Next slide, please.

Our objectives for both the piezometers and the monitor wells are to design them and to construct them in a manner to support tracer tests. We feel that obtaining the hydraulic and the transport parameters from tracer tests are extremely valuable. We also liked both of these well types since they're located at the upgradient locations, they're located close to each other. It would be nice to collect representative drill cuttings from each and look at the correlation of different layers between different holes, and also where possible we'd like to obtain in situ density data where possible, bulk density data.

In addition to that, the monitor wells must be straight and must be stable, so if we wanted to put in our retrievable instrumentation system.

Next slide, please.

The monitor wells we can't good samples from monitor wells and we also can't drill a hole at the same time, so we really have to use two different drilling methods to get the samples. We're using dual wall reverse circulation, and to get the straight hole we're using the flooded mud method. And to the piezometers we're using a casing-advance air-percussion hammer method. We get excellent drill cuttings in these cases and this particular method allows us also to get core samples.

We don't have the time to go into the details of these methods, but we feel that these are--we've thought a lot about them, we've had a lot of experience in the past with these different methods, and we feel this is probably the right approach to go about our drilling.

Next slide, please.

Basically, our drill cutting sampling and logging and testing objectives are just primarily to maximize information about textural layers. There's a whole bunch of activities that we have undertaken to try to maximize the information we can get from drill cuttings, and drill cuttings aren't the best thing in the world and they aren't-- they're oftentimes contaminated, but with a lot of care you can get a lot of good useful geologic information from them. Again, I won't go into any of these in any detail, but some of the methods we're taking are described in the backup slides.

Next slide, please.

We're also attempting to get some core and demonstrate that we can get core from both the unsaturated zone and the saturated zone. We're going to demonstrate this in the actual casing-advance drilling system in the piezometers. We'll obtain core from the saturated zone where well screens are potentially going to be located. Also, we'll try to get core just generally from representative units throughout the unsaturated zone also.

Again, this is primarily a feasibility study. Again, it would be nice to have samples that haven't been chewed up by the drilling bit that we could do some laboratory tests on.

Next slide, please.

Now I'd like to get away with the preliminary stuff and let's move towards talking about some of the results and where we are in our program. Once again, these are the locations, just keep in your mind the 22 location, the 10 location and the 18 location. The 22 and 10 are located right along the channel, the existing channel, and Fortymile Wash. And of course we have a couple holes down here at the ATC side. And then we have over here (indicating) an area that's more in the fractured rock area.

Next slide.

To date we've actually drilled the sampling holes--we call these A-holes--at the ATC side, which is the IM1, IM2 at the ATC side. This is the same location where the monitor wells are actually being installed, and also at the 10 and the 22 location we drilled exploratory boreholes, got good cuttings back, and some of the results we'll show here in a little bit.

In addition to that, we came back and we abandoned these holes and we came back and actually drilled the IM1 hole with the flooded mud system for a multiple-screen monitor well, and we actually have drilled and are completing also IM2. We're in the process of just starting the first piezometer hole. In short, we've made some good progress over the last couple months.

Let's move on, next slide, and look at some of the results. Want to look at some of the drill cuttings, but before we look at drill cuttings, we just want to--I'm sorry, I want to talk about some of the actual data we're going to talk about here. Particle size distribution of depth profiles, cementation HCL reaction depth profiles, and electrical conductivity, and this is a 1:1 water extract to look at soluble salts. We don't have data back yet on silt and clay percentages. The particle size distribution we're going to talk about is just a very gross particle size distribution. We'll show you the data here, and we haven't had a chance yet to process in situ bulk density data.

Next slide, please.

Just a word of caution about drill cuttings. We all know there's a well of possible errors in drilling that could create some misinterpretation of drill cuttings. I won't even go into each of those. But basically, if you're careful, drill cuttings can be of use to identify major trends within and between boreholes and to provide indication of the thickness and textural variation between layers.

Next slide, please.

This is a graph of the depth profile of drill cuttings from the two boreholes at the ATC site. These were two exploratory holes that were drilled and abandoned. The yellow is the first hole that we drilled, the 1A location, and the blue is the 2A location. What we're plotting here is percent passing in two different sieves, a 200 sieve here and a No. 4 sieve here, and simply, to the left of these lines translates into the amount of fine silt plus clay, between these two lines is the amount of sand, and to the right of these lines is our gravel. There are some general trends you can see with depth. The water table is right here (indicating). That is generally increasing fines and a general decreasing amount of gravel with depth. There are some correlations to some extent at different points. Here we have a higher amount of fines (indicating) at this location, and as you go down further, there seems to be some shifting of the peaks. There does appear to be some correlation between--these holes are located about 60 feet apart and this data was just plotted last week, we haven't had a chance to analyze it in any depth, but there does appear to be a little correlation, some continuity, in some of the layers over 60 feet.

I want to emphasize that the differences may in part be due to slightly different drilling methods. I know the first hole we drilled primarily dry and there was some caving and there's some mixing of the samples, and the second hole the upper portion of the hole was conditioned a little better and we probably had less contamination from up hole locations.

Next slide, please.

This is the data from up the hill a little bit from the 10 location and the 22 location. By the way, these samples were taken on 5-foot intervals. The 22 location the lab just did at 20-foot intervals. We haven't got the lab data back. Again, this was just plotted last week. There are some similarities in these two plots, the previous plot and this plot. Again, there's a generally increasing trend of fines with depth. There seems to be a fairly thick--and previously we haven't seen this, there's a strong variation as you go from 5-foot. Between 5-foot layers you have maybe 10, sometimes even 20, 15, 20 percent variation between 5-foot intervals of the amount of finds and also the amount of gravel in the sample, which suggests there aren't thick layers of gravel and they aren't really thick layers of finer material, either. However, in the lower portion of the hole there's some indication that there's relatively thick layers of fine units in the lower portion of the hole. We get virtually no gravel.

Next slide, please.

This slide is a slide that was put in to demonstrate a comparison of lab versus field. We do do field logging and we attempt to estimate the amount of fines, the amount of sand, the amount of gravel. This shows a relatively good correlation between field estimates and laboratory measurements. I made a mistake in this slide, I really meant to plot--we actually in the vadose zone we actually measure on 2.5-foot intervals and we actually estimate fines, we actually log the sample on 2.5-foot intervals in the vadose zone, and I wanted to demonstrate that. This slide does not show that, it's still on 5-foot intervals, but basically the slide that I had in mind does show a variation of 10 to 15 percent fines and 15 to 20 percent gravel with every 2.5-foot interval, basically.

Next slide, please.

It's always informative to compare with other locations. There is a lot of work on a test site that has been done in Frenchman Flat, there were holes that were drilled by casing-advance method to 1,000 feet, drill cuttings were sampled and also core samples were taken.

And how are we doing for time?

DR. WONG: Oh, sorry. Well, you've got 12 minutes.

DR. HAMMERMEISTER: Let's skip a few slides in a minute here. But basically these are--we can go back to this if we have time, but there is data on a test site. Frenchman Flat is also filled with volcanic sediments and it's always nice, it's always informative and we can learn a lot by comparing with other previous work that's been done.

At any rate, can we skip ahead about two or three slides, please? Let's go through these. Some conclusions about some of the preliminary data that we found. We generally have generally increasing fines and decreasing gravel with depth in each of the boreholes that we've drilled so far. Fine percentages average about 14 percent in the hole by the ATC complex up to about 21 percent in the two holes located up the hill a little bit. I think it's important that we don't see any thick fine texture layers except in the lower portions of 22 and possibly 10, and we do see a large variation in particle size fractions on 2.5-foot intervals.

Next slide, please.

And finally, we do see some correlation between layers in closely spaced wells, the 1A and 2A well at the ATC complex. And this refers to Frenchman Flat and we're going to skip these slides so we can stay on schedule.

Next slide, please.

We also, besides doing laboratory tests and besides--our logging consists of a lot of detailed description and we tend to geologically describe the samples in great detail in the field, and part of the logging involves visual estimates of cementation. And in the lab also we do a simple 1:1--I'm sorry, in the field we also squirt some HCL on the sample to look for presence of calcium carbonate as a cementing agent. In the lab, a very simple measurement is a simple 1:1 water extract of the sample.

I'd like to show the next slide, please. This simply shows, again, a field logging description of depth profile of Well 22. And what we see here is when we see some weak cementation, this is typical of most wells, in the upper 100 feet, 150 feet we see some weak cementation, and the only well we see moderate or strong cementation is in 22. If you look over here (indicating), this is the ACL reaction. There's some correlation between cementation and ACL reactions suggesting that maybe calcium carbonate at least plays some part in cementation.

Next slide, please.

This is the EC profile of the two wells that are located at the ATC site. Notice that this is a water extract electrical conductivity and the peaks more or less match up in each case and for both holes the majority of the peaks actually lie on top of each other.

Next slide.

For the two holes located upgradient we see a decrease in electrical conductivity and of course we don't expect the peaks to line up, these wells are separated by several thousand feet.

Next slide. I'm sorry, several miles.

Some conclusions about cementation and electrical conductivity. We do see some weak cementation in all boreholes, like I said. There is some moderate cementation in Well 22, and there's a good correlation between EC in Wells 19 1A and 2A, and the EC peaks and valleys may reflect a whole bunch of things. They may be due to periodic salt accumulation during sediment deposition and/or periodic infiltration events not great enough to flush the profile of soluble salts.

Next slide, please.

I'd like now to turn to a preliminary cross-section done along Lower Fortymile Wash that has been put together by Jamie Walker. Incorporates borehole data from the deep Well 2DB and the shallower Wells 19D, 22SA and 10SA and incorporates the U.S. Geological Survey regional geologic framework units in the northern portion of the section.

Next slide, please.

This cross-section is shown in the back of the room, it's actually printed out. I apologize, when Jamie put this cross-section together for me, I forgot to mention that I wanted one to be able to put it on a PowerPoint slide, so this is not his best work, it's not his fault, it's my fault. But basically the cross-section goes from 2DB to 19 to 22 to 10 at this location right here. Some basic overall trends in the cross-section, we go from a volcanic faces to a sedimentary faces over here (indicating). There's a lot of uncertainties. These wells are, as you can see, separated by more than two miles. We really don't know what--this is the alluvium layer right here (indicating) the QAL. There may be a lower conglomerate unit in the base of the alluvium, we're not sure. The actual tuff unit is probably the Paintbrush tough, but the actual unit has not been identified. And I should also point out that this long section is only intersected two major possible structural features, Highway 95 fault, which is inferred from surface geophysics, and a major lineament at this point right here (indicating). So these are the only two major structural features that have been intersected. There is a major point here, there's a lot of uncertainty.

Next slide, please.

DR. WONG: Five minutes.

DR. HAMMERMEISTER: Five minutes, thanks. Next slide, please.

I just went over this just now, I won't repeat this. We would like to go back and drill some deeper holes. We have a limited budget this year. We had to terminate our holes at roughly 1,000 feet, and in future years we'd like to go back, propose additional wells. In future years, we propose the Department of Energy would like to continue our drilling program. And this looks like a scatter diagram of proposed wells, but basically the proposed wells are the black and the bull's eyes. The black hexagons are a combination of wells that could be either exploratory wells or they can be completed as a monitor well or piezometer well depending upon what we'd find. The bull's eyes are the combination monitor wells/piezometer wells that we talked about at the 22, 10 and 18 locations previously.

Some of the rationale for these holes, we'd like to learn more about the alluvial flow path along the main access of Fortymile Wash. These holes would help us with that. We'd like to look a little bit deeper. We'd like to look at the crater potential of volcanic fractured rock flow path over in Crater Flat. These wells would address that particular issue. We're interested in the flux boundary for the site-scale model, trying to get a better handle on that flux boundary. I think Tom mentioned that the last time he talked to the Board. These wells would help with that. We have a line of wells we'd like to deepen along the Highway 95 road in the actual fault. Very little is known about the fault and we'd like to look a lot more closely at the transition into this basin. And finally, we have a second fence of wells down here, further down toward potential receptors in Amargosa, and again, to try to get a better handle on flow paths closer to actual receptors.

Next slide, please. Next slide.

I'd like to spend just a minute and update you about our ventilation activities that are going on. Parvis

Montazer has been doing this work. Quickly summarize some of the background, past work, some of your present work and some proposed work.

Next slide.

DOE models do predict high temperatures and humidity after backfill and closure, and that of course results from Nye County's perspective in increased uncertainty and corrosion flow and transport simulation, performance assessment and safety demonstrations. And primarily Nye County is primarily interested in performance assessment and safety demonstrations. Every since 1995 Nye County has studied natural ventilation as a means to lower temperatures and humidity and reduce some of this uncertainty. So this has been going on since 1995.

Next slide, please.

Some of the previous work that was done, Nye County did conduct some monitoring in the ESF and the ECRB during the actual drilling, hung instrumentation on the back of the actual tunnel boring machine, and collected a large amount of temperature and pressure and wind speed data and concluded that ventilation clearly can remove a lot of heat and a lot of vapor from the system. Parvis Montazer is also a mining engineer, has a mining engineering background and has a considerable amount of experience in that area.

We also did some preliminary modeling of highly simplified repository with ventilation shafts, and the results indicated that natural ventilation can keep host rock relatively cool and dry for the first 1,000 years when we think it's supposed to heat up. And this is assuming we can keep the repository open.

Next slide.

This is just a summary of some of that modeling. The simplified axisymmetric modeling was completed in 1996. The simplified three-dimensional site-scale modeling was completed in 1998. Results have been presented in Nye County Annual Reports and a ventilation workshop in 1998 which DOE helped co-sponsor with Nye County. The code A-TOUGH is used to simulate heat and vapor flow both in rock and in tunnels and shafts, so basically DOE uses two codes, one for the rock and one for the ventilation. Important to note is the heat and vapor transfer between the rock and the actual ventilation system is accomplished by using a transfer coefficient called eddy diffusivity. And eddy diffusivity, of course, is highly dependent upon temperature and other variables. Eddy diffusivity was actually calculated by Nye County. Both these modeling methods suggested a much cooler and much drier repository could be achieved.

Next slide.

We have some ongoing work right now. We are attempting to identify a more realistic range of design parameters for a natural ventilated repository and develop a more realistic conceptual model and a mesh, a model mesh, and at the same time refine the estimates of this important transfer coefficient, eddy diffusivity, and then conduct simulations to demonstrate this more realistic natural ventilation design. And we would like to come back and present these results to the WTRB in January of this coming year.

Next slide.

Also what's going on right now, and this is not funded by Nye County, it's actually funded by DOE through UNR, there's a co-comparison of A-TOUGH versus MULTIFLUX plus NUFT. And there is a large difference in the temperature predictions between these two models, and some preliminary discussion and preliminary work suggests that it's possibly due to differences in the heat and vapor transfer coefficients used, among other things.

Next slide.

This is probably the most important slide of the day. Nye County believes that DOE and also Nye County--let me start again. Heat and vapor processes in rock are being validated in the ESF. In ELKO5, heat and vapor flow processes in the tunnel, ventilation systems are being validated at this facility, but as yet these two processes have not been coupled together. The rock, processes in the rock, and the processes in the ventilation system have not been coupled together and models have not been validated, including Nye County's model.

Next slide.

Nye County proposes a low-temperature heater/ventilation experiment in the repository, a block to actually measure the necessary parameters, both in the rock and in the actual tunnel ventilation system, and to actually validate their model and the data would also be available to the Department of Energy. Once that was done, if you were able to validate the model, the model would be used to hopefully reduce some uncertainties, possibly reduce footprint size and possibly reduce costs.

Thanks for your time. Questions?

DR. WONG: Thank you, Dale. Questions from the Board? Dr. Parizek.

DR. PARIZEK: Parizek, Board. You mentioned about the geophysics proposal. It seemed like at one point you had an opportunity to maybe develop some geophysical work in order to guide the placement of some of the drill sites. Is there anything going on in that area?

DR. HAMMERMEISTER: Yes, there is. We have proposed--have not shown, of course, proposed future work, some detailed future work. And the Department of Energy has approved a significant surface geophysical program in the order of--anyway, it's a lot of money and it allows us to do some deep seismic work, so it's really some--all the necessary geophysical work that we had proposed DOE has tentatively approved some funds in the coming years.

DR. PARIZEK: That would be sometime in Fiscal Year 03?

DR. HAMMERMEISTER: Yes, 03 and 04.

DR. PARIZEK: Were there any temperature surprises in the drilling you've done to date? By surprise I mean anything that was abnormally either cool or warm based on other holes that already had been drilled in Phase I and II.

DR. HAMMERMEISTER: No, the most recent round of drilling was very shallow. We had a limited budget this particular phase and really couldn't afford to go deep. So no, we didn't come across any temperature anomalies.

DR. PARIZEK: EDWDP 18 is fractured, I guess, one of the first bedrock sites you have for--

DR. HAMMERMEISTER: It has some shallow alluvium we think.

DR. PARIZEK: But eventually you hope to hit some fault zones? Would that be the first place that maybe some fault permeability data could come out in terms of going in an orderly direction closer to the footprint of the repository?

DR. HAMMERMEISTER: Yes, yes.

DR. PARIZEK: That's to be drilled this year?

DR. HAMMERMEISTER: Yes, it is.

DR. PARIZEK: Thank you.

DR. WONG: Dr. Knopman.

DR. KNOPMAN: On one of your very last slides, 45, where you just talk about the comparison of computer codes, perhaps you could just explain a little bit more what you think you might find from this comparison and what difference does it make if the codes changed. And then I'm not sure if Bill Boyle is still here, but perhaps someone from the project could respond to this work and say something about the kinds of code comparisons or conceptual model comparisons that have been made within the program so far on this point. It's really important.

DR. HAMMERMEISTER: Yes, I'd really like Parvis Montazer to come up and address that, if you would.

MR. MONTAZER: Parvis Montazer. This work has been funded I guess as part of the workshop that we had in December 1998, a Nye County and DOE sponsored workshop, resulted in doing this code comparison because of the differences in the predictions. And I have been working just basically this year with Dr. Danko on making this reconciliation as far as the differences. And what we are hoping to find here is--there are some basic differences that the way we are doing this in relationship. The AT2VOC is a simplified version of the ventilation process, but it's fully coupled. What Dr. Danko uses and other codes in the project, MULTIFLEX is, what Dr. Danko has been using, is an externally coupled code with NUFT. It's more complicated, more sophisticated in simulating the process in the drip, but it's externally coupled with NUFT. And that's where we see the differences. And the main difference--and we're both making this assumption incorrectly--is in the transfer correlations. ATOUGH allows us to do the variation with time and the conditions, but in all of our previous simulations we have used one eddy diffusivity number that we obtained from calibration of the code to the tunnel conditions. Basically, Dale mentioned some of the early data that we got by putting up instrumentation at the end of the GBM. We use that to calibrate and we got one number for eddy diffusivity, and we have used that to simulate a long range of temperature and pressure in time. And that has resulted in overestimating heat and moisture removal.

On the other hand, the MULTIFLUX and NUFT external couple simulation, they're using the standard transfer coefficients, and they're constant, too, but they're at the other extreme end. So somewhere in between this whole process of what Dale was talking about as far as our proposed work, we want to see what are the range of changes in the transfer coefficients at different temperature and moisture conditions of the rock and velocity of the ventilation.

So I don't know if I answered that question, it was long winded.

DR. KNOPMAN: That sounds more like a parameter estimation issue than a model discrimination problem.

MR. MONTAZER: That is more or less correct. There have been questions, but we more or less knew NUFT and TOUGH, they use the same kind of equations. They're very much the same, do the same thing.

DR. KNOPMAN: Okay.

MR. MONTAZER: And so do MULTIFLUX and the eddy diffusivity concept. They use similar kind of processes. So we didn't expect to have to be surprised by the differences, and it's basically boiling down to the fact that the parameters that we've been using--there's a dynamic parameter we've been using as a constant parameter.

DR. WONG: Okay, we must move along. Thank you, Dale.

DR. KNOPMAN: Jeff, could someone from the Program just very quickly respond; do we have time?

MR. BLINK: Jim Blink, Livermore. We are also doing a code comparison of ventilation calculations. The Project baseline has used the ANSYS conduction only code in the rock with a post processor using the spreadsheet for the evolution of the temperature in the airstream. We've been comparing that to the results of the MULTIFLUX code. The MULTIFLUX code developed by Reno has been added to the Projects suite of codes, and that code has a more complete coupling between the air and the rock and keeps track of the mass transport in the rock, which ANSYS is not able to do. We see some differences and we're currently working our way through trying to isolate the specific causes of the differences, but in our case it may be more conceptual and model related than parameter related.

DR. WONG: Thank you. Okay, our next speaker is Linda Lehman. She's president of Linda Lehman & Associates. She's a hydrogeologist by trade, and she's made many presentations before the Board and she's provided scientific support to the State of Nevada's High-Level Waste Review Program.

MS. LEHMAN: Thank you. Is that working? Hopefully. I don't know if this is going to work on me.

Thank you very much for the opportunity to present some of the latest results that we have on our efforts to calibrate a larger area model. The last time I spoke to the Board, several years ago, we were working on a smaller area, which involved mainly just the mountain, and now we've expanded the three-dimensional model to go all the way down to the Amargosa.

I'm first going to give an introduction and then talk about the conceptual model, our calibration targets, the model gridding and boundary conditions, and then discuss the results and some conclusions. I'd like to try to spend about ten minutes on the first few items and then another ten minutes on the results.

This should be considered a work in progress as opposed to a final product, and we shouldn't think of this model as an exact replica of Yucca Mountain but rather as a learning tool to study the flow and transport mechanisms at the site.

In the draft EIS, the Department of Energy indicated that this flow model was a credible flow model, but they did not look at it in detail because they claim that as long as they have a waste package that lasts for over 10,000 years, or the compliance period, then there was no need to look at this particular model. And while that might be true, if the package is robust and we have no package failures over the first 10,000 years, then perhaps the role of a saturated zone is diminished.

But if in fact we do have premature failures--and like Dr. Sagüés says, what if some of the science is wrong, even a small probability, then you have package failures within the compliance period, then the saturated zone pathway does become important and it also becomes important for calculations out beyond the compliance period, for example the peak dose calculation that's being done and the Environmental Impact Statement.

So basically we have a number of concerns related to the DOE work, and basically we're concerned that DOE is proceeding with site recommendation: without demonstrating that they have an understanding of the saturated zone; without conducting uncertainty and sensitivity analyses of this particular flow model or a flow model through the mountain, fracture flow; without utilizing all available and relevant data that had led to their site recommendation, and by this I mean primarily the heat information; they have not demonstrated that their flow model can match the detailed potentiometric surface that we see in the data, nor have they demonstrated that they can match the ambient temperature distribution; and further, they have not utilized a fully coupled heat and flow formulation in their model to determine their flow paths.

For calibration targets we're using the potentiometric surface that was developed from USGS data, not the USGS potentiometric surface. And I'll show these differences later. We're using the temperature distribution at the water table of Sass, 1988, and also using the heat flux at the water table that was done by Sass, 1988 also.

I've talked to the Board on several occasions about the USGS data. As you know, they did go back and recalibrate their wells using temperature corrections and releveling surveys and corrected the water level data. However, when they came out with their revised surface, which is shown here, they did not believe their data or chose not to use all of their data in this map. What I have here is their smooth potentiometric surface--this is, of course, the mountain and the repository site--and you can see that there are some mistakes in their contouring. Basically, they have smoothed these surfaces, and this is different from their preliminary map that they had published earlier.

We took their data, used all of their data, replotted the potentiometric surface, and came up with this picture of embayments, where you say there's basically three embayments. This is a 730-meter contour line. And you see these little embayments are lined up with Drill Hole Wash Fault, Sundance Fault, and a fault that runs below the repository. You can see the repository outline here (indicating).

This is the potentiometric surface that was generated by the U.S. Nuclear Regulatory Commission, Neil Coleman, and he was kind enough to give me this overhead. He also sees the embayments. This is in the 729-meter contour line.

One thing that I do want to point out is that there really are no data points down here (indicating) until you get to the Nye County wells. And this area is important, I believe, in determining which flow model is correct.

The Sass temperature distribution is shown here. And what I want to point out to you is there's a linearity in these temperature measurements that's important. This is Fortymile Wash. You see cold plume of water moving down here (indicating), this is Midway Valley, you have warm water here, Ghost Dance Fault you have a cold plume, and Solitario Canyon you have warm water again. So cold, hot, cold, hot, basically, across the site.

The heat flux information that was calculated by Sass--and he concludes that this negative heat flow is from recharge of groundwater, and this area here, these squares or rectangles (indicating), represent the steep hydraulic gradient. And in an area that's coincident with Drill Hole Wash and its intersection with Ghost Dance Fault, you see this very negative heat flux area, smoothing out more over the mountain and then becoming more normal away from the mountain. I've also just for your information plotted our potentiometric surface here in the dots on that to show you that the heat flux also seems to be coincident with the Drill Hole Wash, Sundance and other fault down here.

I'm going to talk briefly about gridding. First, I wanted to say that our conceptual model is three-dimensional, it uses coupled heat and flow, fully coupled heat and flow, equations. We believe that the fracture is--I say fracture flow model, it's not actually fracture flow, it's using fractures to channel the flow, but it is an equivalent porous media model. We feel that the fault zones play a major role, as you saw from the surfaces that I presented earlier, that there may be some connection. We also feel that the flow field is transient, even though the results I'm going to show you today are a steady state.

And Dave Diodato asked me to mention briefly about the code. We are using AT2VOC, which Parvis Montazer just mentioned about earlier. We are running fully coupled heat and flow, but we're using single-phase flow, although the code has capability of multi-phase flow. But because we're dealing with temperatures that are ambient, we don't have the need to have vapor phase in our calculations. It is integrated finite difference code.

The model that we're using has three layers, upper tuff aquifer, a middle confining unit, and the last layer is the carbonates. We have about 3,030 nodes in each layer, so we're pushing up against 10,000 nodes, which is about the capability of our computer. In the past we had been using the LBL version of V-TOUGH and we had to run it on a Cray at UNLV and front end it with a cyber at the University of Minnesota. And so when Parvis Montazer developed a code that could be used on a PC, of course we jumped at the chance to use that and we are using his post-processor and pre-processor on this code. So Parvis has been helping us with runs on this code.

The gridding is the same in layer one and layer two pretty much. Basically what we have is a number of fault zones. The one on the far right is Fortymile Wash. This one (indicating) represents Midway Valley, this is the Ghost Dance Fault and this is the Solitario Canyon Fault (indicating), Drill Hole Wash, Sundance and the fault that I showed you where the third embayment lies. All the faults with the exception of the Solitario are more permeable than the tuff. The Solitario Canyon Fault is impermeable, or much more impermeable than a tuff.

As you'll see here in the cross-section, we have an implied fault, which is the Highway 95 Fault, and below that is alluvium. And we have not focused any effort on the alluvium to date, but we will as you will see in future work. Right now the alluvium is more permeable than the tuff and it exists in both layers.

In terms of boundary conditions, our upper boundary condition here is 1,000 meters of head, 29 degrees C water. Our southeastern boundary is 725 meters of head and our latest run 30 degrees water. The lower boundary, the carbonates, is set up at the pressures and temperatures that were found in P1. In other words, the whole bottom layer has an upward head, so it's set at 750 meters, and the temperature in most of these runs that you'll see is at 57 degrees, but in the later runs we've dropped it down to 50 degrees C. Fifty-seven was what they actually measured it in

P1.

These nodes on the side, these little black dots, were put in there later because in our earlier modeling efforts we found that we couldn't maintain the 775-meter head that's west of the Solitario Canyon with simply this 1,000-meter head up here. So we added these nodes to maintain the 775-meter head in that area.

Now, as you will see when I talk about temperature, we're thinking about taking out some of these, perhaps the lower ones, so that the contours will bend around a little more and allow more water to come up. But I'll explain that in more detail when we get to the results.

So now I will talk about the controls on the potentiometric surface. This is one of our first modeling results on a potentiometric surface, and we are able to get the embayments in this model. However, you'll note that the 736 contour line is here rather than what we had wanted as the 730-meter contour. So in order to adjust that, first let me just mention that this tight unit up here controls the steep hydraulic gradient and keeps these contour lines up. So in order to get to the 730-meter contour, what we did was to tighten the confining unit. And we wound up tightening it six orders of magnitude to get a 6-meter head decrease. But we're able to control that elevation through that confining unit partially.

This set of results I wanted to show you because we're changing the parameters, permeabilities in the Ghost Dance Fault. In this particular run we were trying to adjust temperatures along the fault, lower the temperature, so we tried a number of things. In this particular run we have made the vertical conductivity of the Ghost Dance Fault set equal to the tuffs. Both horizontal conductivities were the same, they are more permeable than the tuff. And basically it had really no influence at all, we're still able to have our three embayments.

So then we decided, what would happen if we took the fault out entirely? So we just made it exactly the same as the tuff, so there's no Ghost Dance Fault here, and what you'll see is our steep gradient on the west side, our medium hydraulic gradient, has disappeared. That was kind of a surprise to us because we always felt that the Solitario Canyon Fault was controlling the steep gradient there. In actuality, what happens is because we have imposed basically a north-south flow field, any impermeable fault here, the flow just goes around it. If the flow were coming this way, to the east, then that fault would have more effect. And this is consistent with some findings of Ed Kwickless that it depends on the position of the fault in the flow field as well as the fault characteristics itself.

So basically what happened is this gradient was moved over to the Midway Valley Fault. And what you see in the one I just put up earlier is that the Ghost Dance Fault is actually capturing all this water that's coming in, so it's prohibiting the steep gradient from moving over.

Now I'll talk briefly about the controls on the temperature field. If I can get this on here correctly. It was fairly easy for us to get the potentiometric surface and keep it where it should be. It's another matter entirely to do the temperature and keep the potentiometric surface.

This is what happens without any adjustments over what we did to the fault zones. And basically, the lower boundary condition in the carbonate dominates everything. If you have any permeability at all, even minor permeability differences, between the faults and the tuffs and the confining unit, then you're going to have heat coming up these faults, and that's what this shows, is that the heat is coming up.

So now the question, is how do you control the temperature? And we found that there are several ways that you can do it. You can add water in the faults to the north, you could add infiltration along certain areas along the faults, you can block all vertical communication with the carbonates by just adjusting the vertical permeabilities, you can adjust thermal conductivities. And one of the things that was helpful is about a week or so ago at another meeting

I got to spend some time with Bill Arnold, and he says that he's found now that the thermal conductivity in the tuff is quite a bit less than it is in the--it's tighter, basically, in the tuffs than it is in the carbonates. So the tuffs would act basically as an insulating blanket, and that could help to lower some of the temperatures as well.

Also, we can't assume that our lower boundary condition is correct under Yucca Mountain. We have only two data points, one from Nye County wells, which indicates that the head is less over to the east of the mountain than what we have in there, and also from what we have observed in the temperature at the Solitario. I will mention again that there may be some differences in the carbonates.

This is an example of what happens when you add infiltration. This is 10 mm per year. Infiltration was added only along Drill Hole Wash in this area and along the Fortymile Wash in this area. And as you can see, it's cooling somewhat, it cools the Midway Valley Fault, because all this is connected, but not enough to bring it to a temperature lower than the tuffs. And that's what we want, is we want a temperature in the fault zones that's colder than the tuffs, and we have not achieved that. This is Ghost Dance. So it does not by itself cool the Ghost Dance significantly.

This is the latest run that was done for us by

Parvis Montazer. And while we are not looking at the alluvium, this is just blocked out, what he has done is bypass these tight units in the top and just impose basically 1,000-meter head boundary condition at the top. And you can see that it has cooled these faults considerably. Still not enough. But we have cooler faults here and then the warm upwelling in the southern part of the block. And we believe there is upwelling along the Highway 95 Fault because the Nye County temperature data indicates very high temperature water at depth in this area.

So basically I think we need a lot more work to see. We haven't tried adding infiltration and recharge here from the north together. We haven't tried adjusting the thermal conductivities. So it's going to be a balancing act, but we really need to do this in order to constrain the velocities. Because in a situation like this, these velocities are really, really high because you have to dump a lot of water in there to cool those fault zones down. Perhaps thermal conductivity could constrain it to slower velocities. A number of things could be done that need to be examined.

First of all, I'll just say that our future work-- oh, one thing I wanted to mention that I did mention briefly is that over here the Solitario Canyon doesn't even show up here, and that's because of that high head boundary. If we can move that boundary over, maybe we can get some hot upwelling here. So that's something that we'll try.

Future work, we're going to start looking at the alluvial properties and compare some of the temperatures with work and models that Parvis has done earlier for Nye County. We're going to explicitly add the Highway 95 Fault and confining unit to the alluvium, which you heard Dale say that the lower units were tighter, so I think we can justify putting a confining unit into the alluvium as well. And look at the thermal conductivity contrasts and evaluate some of the boundary conditions. We also, thanks to Eric Smistad, have acquired a copy of ITOUGH, and Eric was also so kind as to pay Bo Bodvarsson for a few hours of his time so that he can work with us on this as well. We'll use that to better calibrate.

So in summary, we feel that some of the controls on the potentiometric surface were in addition to the tight permeability of the upper boundary, that the share zones do play a role in the embayments, creating the embayments and potentiometric surface, and that the high conductivity of the north-south directed faults in a north-south flow field are causing the tight gradient.

With respect to temperature, the head distribution is important, the heat comes up through the fractures with even a very minor Kv difference, and you have to balance the recharge and infiltration versus the heat.

And that's it.

DR. WONG: Thank you. Questions from the Board? Dr. Knopman.

DR. KNOPMAN: Knopman, Board. I didn't hear you say anything about perched water as a possible explanation for the steep hydraulic gradient. Could you say something about that?

MS. LEHMAN: Well, I guess I don't know that that in itself could cause it. My feeling about this is that this is, to borrow Dave Cox's term, a cascading reservoir coming-- a cascade coming down. And you saw the heat flux there. That to me indicates that that water is coming down as a cascade, going into the water table there. I don't know how the perched water would work. But if there were perched water there, it would be, in my opinion, not letting that water go down, and so you shouldn't see that depression. So under the perched water you might expect, if these boundary conditions are right, to see warmer temperatures rather than colder.

DR. KNOPMAN: Do you have the capacity to model--you could do something in your model to capture the perched water phenomenon in a few cells?

MS. LEHMAN: Well, we don't have an unsaturated zone in here right now, we're just running the saturated zone, so I'm not really sure how we would look at that.

DR. WONG: Last question, Dr. Nelson?

DR. NELSON: Thanks. Nelson, Board. I recall in the past talking with you about north-southness of flow as opposed to the kick-off to the east that the Project's putting forward. Do you have any comments on that? Are you still a proponent of a north-south as opposed to a eastern more flow field?

MS. LEHMAN: Absolutely. Under these conditions, I think you can see that flow will come down these fault zones, come moving down this way and then this way, through here. And to me this is significant because the repository lies pretty much in this general area right in here (indicating), and with these gradients being as flat as they are, I think a considerable amount of the repository area could drain into the Ghost Dance Fault as opposed to being moving to the east. I think the water from Drill Hole Wash probably bypasses the repository and comes out there, and the same with Fortymile. But this particular model shows that only down here does this flow come out over to Fortymile. So I still believe that it's mostly north-south.

DR. NELSON: Can you give me an idea just how much more travel you would expect in the tuff as opposed to what the Project is thinking is in the tuff?

MS. LEHMAN: Well, this pretty much shows it here. It would pretty much come down here (indicating), and if we get these temperatures right, I'm sure that this would come out in this area as opposed to in this area where everyone is looking.

DR. NELSON: So you think it would stay in the rock a whole lot longer?

MS. LEHMAN: Yes, I do.

DR. WONG: Dr. Parizek.

DR. PARIZEK: Parizek, Board. On your Figure 4, which shows the embayments, that figure you've given to us before, but you don't have any of your drill hole control on it based on the basis for your contours with the embayments. It would be helpful to have those control points added because in Figure 3--

MS. LEHMAN: Oh. They're the same as the one on the USGS picture.

DR. PARIZEK: So every control point showing in their Figure 3 you use for yours?

MS. LEHMAN: Yeah, they're the same ones that are right here (indicating).

DR. PARIZEK: Okay.

MS. LEHMAN: And the data that I used to contour it came from their report and it's listed in the back of their report as corrected data. But those are the points.

DR. PARIZEK: And then as you work with both the head distribution as well as your temperature information you can't ignore the chemistry. I know earlier on your chemistry seems somewhat consistent with your flow field interpretations. So you have to also track the chemistry updates on your modeling efforts.

MS. LEHMAN: Yes.

DR. PARIZEK: And I guess so far there's nothing inconsistent coming out of this model.

MS. LEHMAN: Right, I think it's consistent. You know, Ed Kwickless found a flow path that came to the east, but it was down by where this lower fracture zone is. It seems that most of Zel's data, and Zel I believe is here, could say something if he would--and I'll just put this up briefly--I think Ed Kwickless did find a flow path that would come down and come out over like this (indicating). There was a thesis that Zel Peterman gave to me which looked at the flow around the mountain, basically, and it looked like the flow was coming down Fortymile and then down through Crater Flat, but this area seemed to be rather isolated, which would be consistent with this information, too.

DR. WONG: All right, thank you, Linda.

MS. LEHMAN: Thank you.

DR. WONG: That brings this part of the meeting to an end. I'd like to thank all the speakers for their information and presentations. I'd like to thank my colleagues for my aggressive involvement today, and I'd like to thank my colleague Paul Craig for being a most excellent time cop.

And one last thing is Greg Gdowski wanted to say something which he promised would only take 30 seconds.

MR. GDOWSKI: I just wanted to make a brief comment about the chloride content of the water that we're using for corrosion testing. Gerry Gordon on part of his presentation, on page 4, showed much more elevated chloride contents than were mentioned previously near 4 molar. And I also wanted to mention that we are doing test under periodic drip conditions where we allow the drips to evaporate the dryness, and so we're going from very dilute solutions down to very concentrated solutions, so that takes us through a range of chloride contents.

DR. COHON: Thank you. And thank you, Jeff, for your fine job as chair of today's sessions. We turn now to the public comment period. Three people have signed up. Given the lateness of the hour, I would ask each of you to limit your remarks to eight minutes, and I will time you. And we'll start with Judy Treichel.

MS. TREICHEL: Thank you. Judy Treichel, Nevada Nuclear Waste Task Force. I wanted to comment about something that came up a couple of times today, the first time during the EPA presentation, in which it was stated that the 10,000 years was critical to licensing and was sort of tied into legalities, and anything that had to do with those 10,000 years from the time the repository opened was part of the legal requirement that DOE had and also licensing but anything after 10,000 years was just information. And that's part of the reason that the Task Force is one of the groups that's involved in a lawsuit suing EPA about that standard, because we don't believe that it should cut off in fact there are going to be significant doses after 10,000 years, and it certainly appears that there would be.

The other very important time when the idea of legality came up was when there was a conversation going on when Carol Hanlon did Tim Sullivan's presentation, and when Lake was explaining that, you know, according to the lawyers you should probably just tell them what you need to say and nothing else. And it seems to me that that's a very clear way of saying it's sort of an "us and them" game and we're sort of pitted adversaries. And also, when Rob Howard was saying that you probably shouldn't believe anything when he listed the seven entities that are still working on the waste package. And of course the Board has absolutely nothing to do with when the Department of Energy decides to go into site recommendation, unless of course you're asked, and then I would certainly urge you to say that perhaps they're not ready. But it does seem that, you know, once again Lake mentioned that they were working on ways to better communicate with the public. And if you're being told to question everything and the public is being told to believe everything, it's a real difficult sort of thing.

And I think we've just seen an example of how the disconnect works, and I guess I, for one, would hope that the other hearings don't get rescheduled right away. I don't think we're ready for those hearings because if in fact there are other reports coming out and--I can't remember--there are supposed to be subsequent documents coming out that would either better explain or would explain further work that's not yet out there that could make the site recommendation very different from what it is, or it certainly could add to the understanding, then I believe that we should wait to see this. And I guess I wouldn't go along with the idea that we have competing goods.

Thank you.

DR. COHON: Thank you. Sally Devlin. Is Sally still here? I don't see her. John Kessler.

MR. KESSLER: I want to talk about one of Alberto's questions earlier. What if we're wrong about container corrosion, just as an example. Well, you know, obviously we could be wrong, but we will probably be wrong on some things, certainly. As DOE goes through this, these are projections over long periods of time, they'll probably be wrong about something. The question is, will they be very wrong about a lot of things?

So let's just say that they're very wrong as you try to get them to work on, Alberto, specifically about whether the containers will actually behave as they are projected to. You made a comment that, you know, gee, the doses are going to come, you know, pretty close to this 15 mrem per year limit if that's the case. I'm coming at it from that makes me feel good, even better about the site knowing that even if the majority of the containers fail, we're still at something like 15 mrem per year, and that to me is a powerful statement in favor of the fact that this site is actually contributing something, it's not all just the container. So it makes me feel that the site may be pretty suitable, just thinking about it from that limited perspective.

But then you think, well, what else should we be thinking about here in terms of, you know, what if we're wrong, we don't know what we don't know, those kinds of things. I kept thinking for a while, gee, there's really no good way to answer that question "we don't know what we don't know". And I thought some more and I thought well, there are some things the system is doing to help us out here, the system being, you know, DOE's approach to their safety case, what we have for regulations. For example, NRC is asking for multiple barriers. Right now DOE in their PSSE is showing nine barriers, they're providing nine. NRC just asked for two, DOE is providing nine. I think that's comforting to know that the waste package is just one of those nine and that there's another eight out there, all of which have to be defended, all of which they could potentially be wrong, but I feel the fact that there's nine of them out there and that I feel like they're probably not going to be really wrong on a whole bunch of them gives me some additional confidence.

Another aspect is that the current analyses do show margin. That's another way of making me feel that, you know, that this site is probably okay.

I would still argue that on the whole the DOE analyses are generally conservative. There are some optimisms, perhaps waste package design or the analyses is an optimism, we don't know. Certainly that work should continue to explore that. But I would still argue that on the whole, looking at everything, that they've still got a generally conservative analysis and are being relatively cautious about some things they don't know and tend toward a more bounding approach. So that makes me feel a little bit more comfortable, too.

The EPA regulation itself, that's 15 mrem per year. That's something like 1/20th of the natural background of citizens living in the Amargosa Valley region per year. So that's another comforting thing to know that EPA has provided that kind of a regulation that's a fraction of background, be it 15, 25, whatever. In addition, the 15 is to the reasonably maximally exposed individual. It's not to everybody out there but it's to this reasonably maximally exposed individual.

So all these analyses that you've been presented with are for this RMEI at some fraction of natural background. So that's another way of, you know, looking at this thing. And by all means uncertainties should be shown. You know, I'm talking about a mean value here, I appreciate that. I think the comments that the Board has made about how uncertainties might be presented and that some of the uncertainty work that has been presented is another aspect of this problem to shed light on it.

So I'll get back to the question about, you know, will we really be wrong about a lot? And I would think that perhaps here is where natural analogues could help to some extent, knowing are we going to be really, really wrong about a whole lot of things. In addition, DOE is committing to do some long-term R and D, some of which we call performance confirmation, another is general R and D, monitoring, things like that. That should definitely continue.

So on the whole is there sufficient confidence in Yucca Mountain to recommend the proceeding to license application, this next stage that's not throwing away all responsibility but just should we proceed to license application? When we get to that stage, assuming that we do, there's still going to be some people looking at that. There's going to be a Nuclear Regulatory Commission that I've seen DOE be very responsive to in terms of their concerns, there's yourselves, you're not going away after this, and you certainly ask a lot of good questions, and I've seen on the whole DOE be pretty responsive or attempted to be pretty responsive to what I consider a pretty good set of recommendations from you. So it's not the end of the road. I would guess that there's still--and in addition there's the long-term R and D that will help with this, and then the final thing is that DOE is ensuring with their design that it is possible to reverse course and it is possible to retrieve.

So when I look at it for myself thinking, you know, do we have all the answers, could we possibly be wrong about things, we could possibly be wrong about some things. But when I look at on the whole what we've got for regulation, that there's nine potential barriers, just as an example of the way they split them up, that we could have a long-term R and D program, we've still got you, we've still got the NRC to look at, I think that that gives me comfort knowing that we might be wrong about a few things. Thank you.

DR. COHON: Thank you. Sally, did you want to come--

okay.

MS. DEVLIN: Three minutes.

DR. COHON: I will hold you to three minutes, how's that?

MS. DEVLIN: Again, thank you so much for coming, it's been very interesting and always a learning process. I just want to say it's been a very difficult day with everything going on and I think we've held together very well and we'll all say our prayers tonight. But there is something I have to add to all this, and that is I sincerely feel that you've got to do more about my bugs with these metals and the canisters and so on. And you really haven't done enough. I hardly heard about my bugs this entire conference. And when we talk of metallurgy and we talk of Alloy-22, and I love Gerry's new thing with Josephinite. I love that. I read your congressional thing and I thought that was fun. Whatever it is, I hope it is something very nice.

So may I just say let's do some more research on the bugs, because you will get full new reports on my bugs when you get home. So I keep finding new ones and I hope everybody out there keeps finding new ones and find out what they do. And I have to say that because since Abe and I are going to be together for 225 years, when you're burying these 20,000 canisters and God knows what from DOD and their canisters and who knows what is in them, we've got to be awfully careful, don't you agree?

But I do want to leave you with a thought, and it brought back a memory and I think Russ will remember this, nine years ago when we first met, when Dr. Cantlon was head of the Board, and there was a question about what do you say at Yucca Mountain to keep off the grass, you're not supposed to come in? And when I was a little girl in Boston it said, "Irish need not apply," and in Norfolk, Virginia, they said, "No sailors or dogs." But of course in New York and Central Park we were far more erudite and we said that no one say, and say it to your shame, that all was beauty here until you came. We did say that. And so I'm thinking how are you going to get across to people in 5,000 languages "Keep off the grass"? So that is your challenge for the next year, and I think it's a taxing one, and is there an answer to this? Is there a legal answer to this, what you have to say? No? Well, we've got to work on it.

Thank you. Good night.

DR. COHON: Thank you, and thank you all for your participation on this difficult day. A long day by the agenda, but a day that felt even longer because of the circumstances surrounding it. We stand adjourned. We reconvene tomorrow morning at 8:00. Thank you.

(Whereupon, the meeting was adjourned, to reconvene Wednesday, September 12, 2001, at 8:00 a.m.)