UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

Joint Meeting of Site Characterization and Repository Panels

On Seismic Issues

February 24, 2003

Best Western Tuscany Hotel and Casino
255 East Flamingo Road
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen, Roundtable Discussion Moderator
Dr. Thure Cerling
Dr. Ronald Latanision
Dr. Priscilla P. Nelson, Meeting Chair
Dr. Richard R. Parizek

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

CONSULTANTS

Alfred J. Hendron, University of Illinois
Peter Kaiser, MIRARCO
Alfred McGarr, USGS
Anestis Veletsos, Rice University

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P R O C E E D I N G S

8:00 a.m.

NELSON: Good morning. My name is Priscilla Nelson, and I am Chair of today's joint meeting of the Nuclear Waste Technical Review Board's Panel on the Natural System and Panel on the Engineered System. Those of you who have been following the Board will realize that these constitute new panels that have been organized to encompass focus areas of the Board as it's now configured.

The meeting today will be devoted to seismic issues. For those of you who came to hear about the Waste Management System, that meeting will be held tomorrow, but in this room. However, you're certainly welcome to stay for this meeting if you came to the wrong one initially.

Let me give you a brief background on the Board itself. Our Board was created in the 1987 amendments to the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of the activities of the Department of Energy as related to the disposal of commercial spent nuclear fuel, and defense high-level radioactive waste. The Board is required to report its findings and recommendations at least twice each year to Congress and to the Secretary of Energy.

The Board is, by law and design, a multidisciplinary group composed of eleven members with expertise covering a wide range of disciplines. Members of the Board are appointed by the President from a list of nominees submitted by the National Academy of Sciences.

Now, let me introduce you to the other members of the Board that are present at today's meeting. As I introduce them, I would ask each to stand briefly and be identified. Let me remind you also that we each serve in a part-time capacity. In my case, I'm Senior Advisor in the Directorate for Engineering at the National Science Foundation, and my areas of expertise include rock engineering and underground construction.

Mark Abkowitz is Professor of Civil Engineering and Management Technology at Vanderbilt University in Nashville, and he's Director of the Vanderbilt Center for Environmental Management Studies. His expertise is in the areas of transportation, risk management, and risk assessment, and he's very interested in seismology.

Dan Bullen is Associate Professor of Mechanical Engineering at Iowa State University. His areas of expertise include performance assessment, modeling, and materials science. Dan chairs our new Panel on Repository System and Integration.

Thure Cerling is Distinguished Professor of Geology and Geophysics and Distinguished Professor of Biology at the

University of Utah at Salt Lake City. He is a geochemist with particular expertise in applying geochemistry to a wide range of geologic, climatological, and anthropological studies.

Ron Latanision is a Professor of Materials Science, Professor of Nuclear Engineering and Director of the H.H. Ulig Corrosions Laboratory at MIT. His areas of expertise include materials processing, and corrosion of metals and other materials in aqueous environments. Ron is also Founder and Chairman of the MIT Council on Primary and Secondary Education. Ron chairs our Panel on the Engineered System.

Richard Parizek is Professor of Geology and Geoenvironmental Engineering at Penn State University. He is also President of Richard Parizek and Associates, Consulting Hydrogeologists and Environmental Geologists. His areas of expertise include hydrogeology and environmental geology. And Richard chairs our Panel on the Natural System.

I'd also like to call your attention to the Board Staff, who is arrayed at the side of the room, and particularly at this point to recognize Leon Reiter and John Pye as the two leads in assembling this panel meeting.

Thank you very much.

The subject of today's meeting is seismic issues. Earthquakes have long been a concern in repository siting. The Department of Energy has devoted much research to assessing the seismic hazard at Yucca Mountain, including characterization of possible damaging earthquake ground motions and fault displacements.

In 1998, this culminated in perhaps the most extensive probabilistic seismic hazard analysis, or PSHA, ever carried out for an engineering project. Most recently, the DOE has been concentrating on applying, and to some extent, extending the results of this PSHA to pre-closure design and post-closure safety analysis. The purpose of this meeting is to focus on these recent efforts, the methodologies used and the results to date.

These can be highly technical discussions covering a wide range of disciplines. For that reason, we decided on having a panel, or a joint panel, meeting that combines each scientists and engineers, and that would allow for more detailed discussion than that found in the typical meeting of the full Board. We have also asked four consultants to join us and help the Board in reviewing the material being presented today.

I would like to introduce these consultants and ask them to stand briefly and identify themselves as I call their names.

Dr. Alfred Hendron is Professor Emeritus in Civil Engineering at the University of Illinois at Urbana. During his distinguished career, he has amassed a great deal of experience in the design and review of major geotechnical engineering projects, including underground excavations at the Nevada Test Site.

Dr. Peter Kaiser is a Professor of Mining Engineering at Laurentian University in Sudbury, Ontario, President of the university's Mining Innovation, Rehabilitation and Applied Research Corporation (MIRARCO), and Director of its Geomechanics Research Center. His expertise is in the geomechanics, tunneling, and mine design.

Dr. Arthur McGarr is a seismologist with the U.S. Geological Survey in Menlo Park, California. His expertise is in characterizing earthquake ground motion and he has extensive experience concerning earthquakes in underground mines and their associated ground motions.

Dr. Anestis Veletsos is Brown and Root Professor in the Department of Civil and Environmental Engineering at Rice University in Texas. He has extensive experience in the dynamic response of structures to earthquake motions.

And now let me say just a few words about today's agenda. First of all, in order to facilitate discussion, the Board has urged DOE not to limit itself to published material, but also to provide draft or preliminary information. So, please remember that a portion of what you hear today is preliminary, will be preliminary, and does not necessarily represent any final position of DOE on these issues.

Bill Boyle will start off with a short description of the Department's approach to seismic issues. He will be followed by Carl Stepp, who will tell us about the probabilistic seismic hazard analysis conducted for Yucca Mountain, and Ivan Wong will then summarize the geotechnical investigations at the site.

We will then hear several presentations on preclosure seismic issues. For those of you who are unfamiliar with the terminology, "preclosure" refers to the 100 or so years during which the proposed repository would remain open to receive and emplace spent fuel and high-level radioactive waste. Richard Pernisi will introduce the general topic of preclosure analysis and design. Ivan Wong will then discuss the ground motion estimates, and Richard Pernisi will follow with information about the preclosure seismic design and analysis to date. The last talk before lunch will be by Mike Gross who will provide some general background on postclosure seismic analysis. "Postclosure" refers to the period of 10,000 or so years during which the closed repository has to meet government criteria.

After lunch, Ivan Wong will talk again, this time about ground motion estimates for postclosure seismic analysis and some studies the DOE is undertaking to see if some limits can be placed on ground motion estimates at very low probabilities. Jim Brune will then describe some of his work that could place limits on ground motions at Yucca Mountain.

Mark Board will then discuss the stability of the drifts affected by earthquakes and thermal loads and M.J. Anderson and Mike Gross will describe the response of the underground engineered components to seismic events and their incorporation into total system performance assessment. Following these talks, we will have a roundtable monitored by Board member--moderated by Board member, maybe monitored as well, Dan Bullen, but more about that later. At the end of the day, we have set aside time for public comments.

I must say a few words about public comment and the ground rules of our meeting. We have scheduled our public comment period at the end of the meeting in the late afternoon. Those wanting to comment should sign the public comment register at the check-in table in the back where Ms. Linda Coultry and Davonya Barnes are seated, and they will be happy to assist you.

Let me point out, and I will remind you again later, that depending on the number of people who sign up for comment, we may have to limit the length of time you have to make your comments during the comment period.

As always, we welcome written comments to the Board for the record. Those of you who prefer not to make oral comments or ask questions during the meeting may choose the written option at any time. We especially encourage written comments if they're more extensive, and our meeting time would not allow them to be spoken orally.

So, finally, I have to offer our usual disclaimer for the record so that everybody is clear on the conduct of our meeting, and the significance of what you're hearing. Our meetings are spontaneous by design. Those of you who have attended our meetings before know that the Board members do not hesitate to speak their minds. When they do so, they are speaking on behalf of themselves, not on behalf of the Board. When we are articulating a Board position, we will be sure to let you know. You can find the final Board positions in our written letters and reports, which can be accessed through the Board's website.

And, I would like to put a special request in that as we go through the presentations today, that we avoid acronyms, because there are many of them these days, and clarity early in the presentations will really help in understanding what's going on.

So, I'm ready to invite our first speaker up. He's Dr. William Boyle, who is Director of Postclosure and License Acquisition Division in the Office of License Application and Strategy, Office of Repository Development.

Before joining DOE, Bill was a geotechnical engineer for the U.S. Nuclear Regulatory Commission, and he had been involved in site characterizations and design activities for several other previously proposed or considered repositories. Bill has been with the project for quite some time, knows everything, and we invite him to make the introductory statements.

BOYLE: Thank you for that introduction, Priscilla.

Good morning. Thank you for this opportunity to make a presentation on the Department of Energy approach to Yucca Mountain Seismic Issues, or earthquakes and the effects of earthquakes.

We have some challenges today. We had a meeting along these lines last summer with the Nuclear Regulatory Commission. It took two and a half days. So, what took hours in that meeting, will have to be done in minutes today. So, we're going to get the Readers Digest condensed version.

Dr. Nelson brought up acronyms. I bet there's a lot of people in this room that are neither seismologists nor structural or civil engineers. This area of seismicity and its effects is very full with technical terms, and you're going to hear words like ergodic, response spectra. So, I have a request. I know Jim Brune has followed up on this already. If you're going to use such terms that although the experts may recognize, there are non-experts in the room, please provide, whether you are a questioner or a presenter, you know, a brief description of the term.

As Dr. Nelson already mentioned, we are not done with our work on seismic issues. It's still a work in progress. But we will show preliminary results today.

One last general item. Where is Tim Sullivan that for more than ten years, Tim Sullivan was the DOE Manager for these efforts. And if this meeting had been held last summer, as the meeting with the NRC was, it would have been Tim Sullivan up here and not myself. Tim retired a couple months ago. So, he's now in sunny Florida enjoying himself, and with his retirement, the work has, for now, gone to Drew Coleman, who's in the audience, DOE and myself.

This is a slide I borrowed from Tim's presentation. You can still see his name even right there. This is from the presentation, the meeting with the Nuclear Regulatory Commission last summer. It describes our seismic approach. One change I made to this slide from last summer is is I put today's presenters in red. Jim Brune isn't shown on this slide, even though he's a presenter today, and Jim is quite involved with the Yucca Mountain project running the seismic net for us. But that's in a box off this chart, if you will.

The basic approach is, starting from the left, as with any complex project or system, you start with data gathering. You eventually go to modeling and analysis of the parts, and then finally modeling and analyses of the whole.

And in our work, as Dr. Nelson has already mentioned, we split it into postclosure and preclosure.

The green boxes on here represent work that's already completed. The yellow boxes represent work that's underway, and we will show some of those preliminary results today. And the blue boxes represent work yet to be done.

Now, for postclosure, the modeling to date, we're looking at the biggest effects. Not all the models are coupled to each other. Like later in the day, you will see presentations by Mark Board and Mike Gross. Their models are not fully coupled, you know, Mark's results will show rocks coming into the drifts, whereas, Mike's do not. And we're aware of that and we're just taking a simple approach here first to examine separately the major effects.

Now, even without this coupling, and even without going all the way to the end for the postclosure calculations, we have done similar calculations many times in the past. And although it's a bit of an apples and orange comparison, our preliminary results to date for the postclosure back in this area give us indications that when we finally do go all the way through, that seismic effects on postclosure dose results will not be a major concern. We'll still probably stay comfortably below any of the standards that are applied to the Yucca Mountain system.

And this is comforting, because in the past, we haven't really considered postclosure seismic effects because we felt that they would be small concerns, and it's in part intuitive. For those that aren't familiar with the system, in the postclosure, it's simply thick walled cans sitting in a hole in the ground. There's no moving parts. There's no pumps, no fans, no cranes. There's not really much that can go wrong.

So, why are we here today? What, in part, always gets people's attention are low probability things. Well, how bad can it be? Or how large can it be? Or how small can it be? And in our case, it's the sizes, if you will, of the rarest, very low probability events that gets people's attention. Now, in our Probabilistic Seismic Hazard Assessment, PSHA, that's probably one acronym you'll see in here a lot this morning, which, by the way, the paper that described that effort has recently been granted an award by the Earthquake Engineering Research Institute, that PSHA was set up to generate unbiased estimates of the future ground motions at Yucca Mountain. And by its definition, an unbiased estimate means that there's a 50 per cent chance that the estimate is larger than what the real number is going to be, or a 50 per cent chance that it's going to be smaller than what the real number is going to be.

In our case, fortunately, the estimate is on the high side. It's on the conservative side. Our estimate has come up with numbers that even our own experts, when they look at them, they like to deem them conservative or physically unrealistic, and there will be some discussion today of some of those numbers.

The good news is, back to the apples and orange comparison that I mentioned earlier, knowing what we know now from all our prior years of working, when we look at the results, the preliminary results we have that are being done in those yellow boxes, that even with these physically unrealistic numbers, the system still passes. So, I think that's good news.

Now, nevertheless, we want to put those physically unrealistic or conservative numbers into perspective, so we do have work underway, or being considered, that's going to shed light on just, well, how conservative are those numbers. And this afternoon, both Jim Brune and Ivan Wong will talk about some of the work.

But, in general, the work can be thought of in three parts, and I think most importantly, and you'll see this in Jim Brune's talk, is what does Yucca Mountain itself tell us. It's been there for roughly 13 million years. It's had an opportunity to be shaken many times. What does it tell us? So, that's one part. What do we know from the field?

The other two parts deal with modeling and lab testing to put that natural reality of Yucca Mountain into perspective.

So, this is a really fascinating technical topic, and it's also, for us, even in terms of coming to grips with the technical challenges of this problem, we have to do all our work to make sure that we stay compliant with all the requirements of the applicable regulations.

Now, I've got two slides added to my talk here, and it was in an effort to speed things up. How many have never been to Yucca Mountain? Good. This just confirms what you should already know then, that focus on the cross-sections in particular. The geology of Yucca Mountain can be thought of as a layer cake. You can see the layers, if you will, in different colors, and they have been tilted. They are no longer horizontal. And they have been broken up by faults, which is what you see here. Yucca Mountain itself is right here in the middle of the figure, and these are two cross-sections, slices, vertical slices through the earth that show the layers. So, it's a layer cake that's been tilted and broken up.

Now, the seismic hazard, the earthquakes of concern at Yucca Mountain, some of it comes from these nearby faults, which--I was born in San Francisco. These faults aren't the same as the San Andreas. They can generate earthquakes that we need to be concerned about, but not earthquakes as large as what California sees on the San Andreas Fault.

There are faults not shown on this map, and they're in California as well, Furnace Creek, Death Valley Fault that is capable of much larger earthquakes. It contributes to the hazard as well. But because it's further away, it contributes in a different sense.

Now, I'll say right up front, the color schemes between these two slides are not the same. So, each can be viewed in its own. This, again, gets across the layer cake nature of the rocks at Yucca Mountain. This down here, this legend down here is important. The more any individual layer sticks out, is an indication of its erosion resistance. The layers here tend to be either hard, strong, brittle, and capable of being fractured, or less strong, less brittle, and less fractured. I like to describe it, think of an Oreo cookie. These layers are like the dark chocolate cookie layer. You know, you can break it, crumble, it's harder, stronger. The intervening non-welded layers are the cream filling, if you will. It's not as strong, not as brittle, not as fractured.

Now, the reason I'm showing this slide is the engineers and scientists, they need to know the properties of these different layers, because they will take an earthquake, the seismic energy from an earthquake, and propagate it up through these layers based upon the physical properties, and get different responses for different sites, either at the waste handling building, that's what WHB stands for, or at the repository itself.

NELSON: Thank you, Bill.

Okay, any questions or comments, realizing that Bill will be a member of our Panel this afternoon, and can be engaged in conversation then as well? First, Dan Bullen.

BULLEN: Bullen, Board.

Can you go back to the diagram, the first diagram you showed? Keep going all the way.

BOYLE: That green, yellow, blue?

BULLEN: Yes, green, yellow, blue. The comment that you made basically that the models are not coupled, but you're essentially still conservative or physically unrealistic? The follow-on question is when will the models be coupled, and will it be done prior to LA?

BOYLE: I don't know that they are going to be coupled. I think we're going to finish, you know, these initial calculations first to find out how large the effects are, and then people will make determinations as to whether or not to fully couple them.

BULLEN: Bullen, Board. Again, just a quick one there.

If you're overly conservative, are you spending too much money? I mean, would it be better to have more realistic representations of the models so that you could actually build the underground and the surface facilities to an adequate standard as opposed to a concrete bunker standard?

BOYLE: Well, this will come out this morning. You know, the preclosure design, you know, when you get to those things like cranes and fans, and things like that, they are not designed to these very low probability earthquakes that generate the physically unrealistic numbers. The numbers used, those probability levels, which people just, you know, for shorthand call the preclosure calculations, or preclosure earthquakes, they're, you know, they're large numbers, but they're certainly nowhere near as large as the much rarer events that must be considered for the postclosure. So, the large postclosure motions don't drive the design at all.

BULLEN: Thank you.

NELSON: Richard?

PARIZEK: Parizek, Board.

Now, is there any, say, confirmation testing tied to any of these boxes? I'm just sort of thinking ahead in terms of the whole contribution.

BOYLE: Yes, we have a performance confirmation meeting with the Nuclear Regulatory Commission on Wednesday of this week, and my guess is the answer is probably yes. But I don't know the complete answer.

NELSON: Any other comments?

(No response.)

NELSON: Okay, Bill, thanks very much for that lead-in. We've met concrete bunkers and Oreo cookies so far this morning. We will have other analogies following.

Next, Carl Stepp. Carl Stepp has had more than 30 years experience in earthquake hazard assessment, and was the lead author to that referenced paper that received the award at the EERI meeting last week. He was a research scientist at the U.S. Coast and Geodetic Survey, and then Chief of the Geoscience Branch of the U.S. Nuclear Regulatory Commission.

For ten years, he was Manager of the Seismic Center at EPRI, the Electric Power Research Institute. Since 1993, Dr. Stepp has been a private consultant, and has a very special association with my old place of work, the University of Texas at Austin, where he is working as a research scientist, and is involved in the NEES project, Network for Earthquake Engineering Simulation, which I had to get a plug in about.

So, welcome, Carl, and thank you.

STEPP: Thank you for the kind introduction. In the next 20 minutes or so, I want to talk about, in a very high level way, the Probabilistic Hazard Assessment that we performed at Yucca Mountain. This is going to be a fairly high level talk, and I'm going to go pretty fast through it because I have lots of illustrations here to show you. So, I hope you will take notes, and we can then respond to your questions.

I'll make the presentation in these headings. I will talk about the objective for the seismic hazard analyses, the guidelines, and methodology that we followed, how we implemented the methodology and guidelines. I will show some ground motion hazard results, and describe to you what controls those results in terms of the parameter inputs, and I will show fault displacement results in a summary way, and then make some summary comments.

First of all, Part 63 is a risk-based regulation, which means that hazard needs to be transmitted--I should say uncertainties in the hazard estimations are transmitted through the system's response to the risk results. So, the emphasis of Part 63 is very strongly on quantification of uncertainties at all stages of the evaluation and the transmission of those uncertainties through to the performance assessment and the design of the facility.

We obtain ground motion and fault displacement hazard results for preclosure seismic design and for postclosure performance assessment. And these will include a special emphasis on capturing epistemic uncertainty and the input interpretations. By epistemic uncertainty, we mean knowledge uncertainty in the state of knowledge for our assessment of the input parameters.

We quantify the uncertainty in hazard results based on current uncertainty of the informed scientific community, and I will explain a little better what we mean about that later, about seismic source interpretations, earthquake recurrence and maximum magnitudes for seismic sources, for engineering estimation of ground motion, that is attenuation of ground motion and variability about the attenuation of ground motion, and for the assessment of fault displacement potential, how do we model the potential for fault displacement.

We quantify, I should say minimize unquantified uncertainty due to date and limitations of data by using a common, uniform database for all interpretations. There's a very strict requirement of the project, and an important one that has evolved over recent years in developing and standardizing probabilistic hazard assessment.

We quantify the uncertainty by conducting a formalized expert elicitation of all evaluations and input to the hazard computations.

It's long established that regulatory decisions of public safety are based on reasonable assurance, the reasonable assurance standard, and the foundation for reasonable assurance is found in Standards of Practice. So, we took some particular care to implement a standard practice in developing the PSHA. In particular, we implemented what is referred to as a Level 4 PSHA, as defined by the Senior Seismic Hazard Analysis Committee, the Chair of whom is here in our audience.

This work by the committee was reviewed a committee of the--or I should say by a review group of the National Academy of Sciences, and it has been accepted by the Nuclear Regulatory Commission for generalized implementation in assessing PSHA for nuclear plants.

That is important to us, and I think to the project as a whole, and to the decision making about the project for seismic design, that these results are really developed a period of 30 years, or so, in a combined effort by the Nuclear Regulatory Commission, the industry, and a whole body of interested and concerned scientists.

We also followed the NRC's Branch technical position on expert elicitation. The NRC staff technical position on identification of fault displacement hazards, or seismic hazards, evaluation, and the NRC's staff technical position on consideration of fault displacement hazards in the design of the repository.

DOE elected early on in the process to develop a series of three topical reports that would describe the methodologies that would be implemented for Yucca Mountain, the seismic evaluation of Yucca Mountain. The first of this is a methodology for probabilistic seismic hazard assessment.

That was reviewed by the NRC and accepted provisionally based on its application at a later time. And the second one was a topical report on preclosure seismic design methodology, also accepted by the NRC provisionally for subsequent, or pending subsequent application and review of the implementation results.

The SSHAC methodology, Level 4 methodology, as I've emphasized, focuses on quantification of epistemic or knowledge uncertainty, and it focuses on achieving this through alternative interpretation by multiple experts. In our case, we elected to form six expert teams for seismic source and fault displacement evaluations. These teams consisted of three persons each, one an expert in basin and range tectonics with broad experience, a seismologist, and, of course, a quaternary fault displacement expert. Those three focused expertise make up the range of expertise in each of the teams.

We asked that each team function as a virtual expert, recognizing that the quantification of uncertainty across all of the input interpretations, the parameters that had to be evaluated, required their collective effort, so that the uncertainties in their results reflect their composite uncertainty in each of those interpretations.

We engaged seven ground motion experts. This number was determined by the fact that there are six models, alternative models that are generally considered to be viable models for estimating ground motion. And we had empirical experts involved as well. Common databases were used by all.

The structured expert interactions in multiple workshops and field trips, we wanted to ensure that all of the experts had common understanding of the available data, had common exposure to the data and investigations in the field. And, so, we implemented a series of steps to accomplish that. We went through a comprehensive identification of the issues that were related to the interpretations that had to be made, again, to ensure that we had fully identified the issues, and to ensure that all of the experts understood the issues at the same level of detail, and that they could take that forward to their evaluations, independent evaluations.

Workshops presented alternative viewpoints about conceptual models relative to the various issues, what range of alternative interpretations are important relative to the issues. So, there was a sampling there of the state of knowledge of the scientific community.

We had ongoing participatory peer review at all stages of the project. What this means is that the peer review panel was present. It was active and participating in the actual workshops. It met with the project management team at the end of each of the workshops, and provided feedback to the project management team, and made, of course, adjustments, and so on.

And we take the integrated expert evaluations as being representative of the current state of scientific uncertainty in our ability to make these evaluations.

The project was structured, as you see here, I want to just make a couple of emphases. There was a management team basically of experienced people in various aspects of hazard evaluation. It was constantly advised by the peer review panel. But the meat of the project really is at the next level, the data management. We had constant in-flow of uniform data. And then we had technical facilitation teams for both the seismic source and the ground motion evaluations, made up of a group of people who were able to provide certain analyses and assist the experts in massaging data where that happened to be needed.

We had parallel participation by the calculations group led by Gabriel Toro, and then, finally, there was the experts themselves who produced the results on which these probabilistic hazard assessments rely.

This simply shows the experts. I won't do more here than make the point that the experts were selected from a pool of larger experts. We selected them in ways that emphasized the broad strength that we needed for the interpretations, and we also gave some weight to distribution of their private sector, university base and public research institution base.

The next slide simply shows the series of workshops that we went through. We went through a total of six workshops for seismic source and fault displacement, plus one facilitation meeting with each of the teams.

The ground motion was conducted similarly, but emphasis was about a mix between workshops and individual meetings with the ground motion experts.

After we had preliminary interpretations, we began to give feedback from the computational side of the project to the experts so that they were aware of how their various interpretations affected, in some degree at least, the hazard result.

This slide shows a schematic of the mountain to illustrate to you where we did the computations. Point A is a point, it's an actual geographic location within the repository area. Point B is at the emplacement level. Point C is at the top of the mountain, not shown here. And then we have Points D and E, which are locations of the surface facilities.

Ground motion hazard was computed at this control location, Point A. And for the rock properties at the repository emplacement level, the rock properties are defined, as you see here, by shearwave velocity, and the parameter, high frequency parameter kappa.

And in computing the ground motion hazard, the aleatory variability about the median ground motion, magnitude and distance, was not truncated. Indeed, we did not truncate or place bounds on any of the experts uncertainty distributions. So, what you see here in the hazard results truly reflect the total uncertainty as we received it from the experts.

This is going to be a little bit difficult for you to read at the lower annual frequencies. For preclosure, as we have laid out in Seismic Topical 2, the design will be for 10^{-3} and 10^{-4} for our frequency Category 1 and frequency Category 2 components. And you will hear an elaboration on this later on from Richard Pernisi.

The mean hazard for preclosure is used for both preclosure seismic design and for probabilistic safety assessment, again, as you will hear more about later in both. We compute the hazard at Point A, shown here, this is peak acceleration, I believe. Spectral acceleration at 10 Hz. Okay, we compute the hazard at Point A in the free field, and we used that to compute hazard at other locations, or facilities, throughout the repository. And we implement means of doing that that transmit the full uncertainty in the hazard through to those results for design.

Note here that I would say at 10^{-4} level, that the uncertainty distribution, that is, the probability distribution about the mean hazard, or median hazard, is pretty well behaved, in the sense that it's reasonably symmetric.

Let's see. We're not going to be able to go backward, are we? Okay. The starting point here is at Point A for deriving motions at other locations as to compute uniform hazard spectrum. We obtained the uniform hazard spectra by computing hazard curves for range of structural frequencies that span the structural frequency range of the facility. And what you see here is the uniform hazard spectra. I guess this is the mean 85th median and 15th fractile spectra.

These spectra are used then to derive through disaggregation the ground motions at Point A, that is, the controlling earthquakes, and then we move forward with the computation of the ground motions at Point A following NRC's Reg Guide 1.165. And then those motions are transmitted through to other locations with the full uncertainty being transmitted.

At 10^{-4} annual frequency, we can disaggregate to show where the hazard is coming from in terms of magnitude, distance and parameter. Epsilon, which is a measure of the standard deviation of the motion from the median, and as you can see in this illustration, the hazard at 10^{-4} is coming from a wide range of earthquake magnitudes, down to the lowest magnitude that we include hazard integration, which is 5. And we have some small contributions from earthquakes as large as magnitude 7, to 7 1/2.

And the distances here are below 20 kilometers dominantly, but we do have a blip of contribution coming from distant sources, Furnace Creek, and so on. Probably those are the magnitude 7 1/2 earthquakes. And the hazard is coming dominantly from above the median attenuation, but it's not unreasonably behaved. It's dominantly from about two standard deviations above the median.

Now I'd like to go back, if I may.

NELSON: Carl?

STEPP: Yes.

NELSON: This is Nelson. Just a cautionary. We're coming up on 20 minutes into the presentation.

STEPP: Okay.

NELSON: So, I wanted to make sure that we get to the fault displacement, too.

STEPP: Okay, we can push ahead.

I just wanted to make one more point about the hazard results. We used these for our postclosure, as well as preclosure, and NRC's regulatory policies have established that we will use mean hazard in both pre and postclosure to transmit uncertainties from the hazard through to risk assessment.

When we go into the postclosure, you will see from this curve that the hazard becomes poorly behaved, in the sense that it's very asymmetric and increasingly asymmetric in the probability distribution about the mean or median, with decreasing annual frequencies. We nevertheless, as Bill pointed out, are using the mean hazard to transmit the results through to the performance assessment, in keeping with NRC's policy, even though we don't believe that these motions that correspond to the mean hazard at very low annual frequencies are realistic, they do capture the uncertainty in our ability to estimate the motion. So we used them.

I will go ahead and skip through this, I believe. I want to make a point from this slide, to simply show to you at 10^{-7} annual hazard, there is a very great difference from 10^{-4} annual hazard, and where the hazard is coming from. It's dominated now by larger magnitude earthquakes, and they are predominantly less than 5 kilometers from the site, and the variability in ground motion about the median peaks at about 3 standard deviations. So, we're getting really on the very extremes of the hazard estimation.

This slide shows nine locations where we did fault displacement modeling. These locations are representative of some 15 faulting conditions that were identified in the mapping of the repository. So, we did calculations for those

15 faulting conditions, and obtained hazard curves that can be applied then to any feature or location within the repository.

The fault displacement hazard for preclosure at 10^{-4}, 10^{-5}, in this case for Category 1 and 2 events, as defined by Part 63, is negligible except for the block bounding faults at Bow Ridge and Solitario Canyon, from which the repository facilities will be set back.

This shows--I'm going to just walk through some slides of the different major features, faulting conditions that we modelled. On your right, there is the Solitario Canyon model. As you can see, Solitario Canyon does have a significant hazard out to 10^{-6}, or so, or at, I should say, the preclosure design level, and it characteristically becomes very much more asymmetric, with decreasing hazard.

On the left, is a typical interblock fault, Ghost Dance. You will note, the 15th fractile hazard doesn't even show on this plot, so it's a very highly asymmetric, it's insignificant for preclosure. Has to be analyzed for postclosure.

On the upper left is a point which we call 7a. It represented faulting conditions with two meters of offset, cumulative offset. And as you see again, there is no significant hazard for preclosure at 10^{-5} annual frequency. The 15th fractile does not show up. It's highly asymmetric.

On the top right is the same location, with 10 centimeters cumulative offset condition, and even the median doesn't show up on that plot.

At the bottom is the condition of non-faulting in the repository, and while there is some very low probability that unfaulted rock could become faulted, that is negligible here.

I'll stop there. I'm sorry to run over.

NELSON: Thank you very much, Carl. This was a lot of information to convey and we did have the benefit of the paper that described the PSHA that I think provided really great background for our discussions here.

I'll open it to questions. Parizek?

PARIZEK: Parizek, Board. Congratulations again on the award. It's a first of its kind effort, and it began in 1994 and ended in June 1998. So, it's dated perhaps. There must be new data. There must be new theories, maybe new hypotheses.

STEPP: Yes.

PARIZEK: And, so, as good a paper as it is, and the credits you received for it, what new information is there that you might say is still a credible result, that you can think about this in more depth. Surely, the repository horizon, 70 per cent is in the lower lith. Do you have data on the lower lith, as an example, and so on?

So, the question is not negative. It's just saying can you still live with it?

STEPP: The new data cover a wide range of features of the repository, as you just mentioned, from the rock properties and state of, I guess, deformation of the rock quality of those rocks in the repository, to new data about tectonics, and so on, we have not identified. There is an ongoing effort, the project does have as part of its QA plan, an effort to continually evaluate new data, and that has been ongoing. It is ongoing now, and we have not identified new data which would suggest to me to revisit any aspect of the repository.

No doubt--I mean, of the PSHA evaluation. We expect that PSHA will evolve with time. But, as I mentioned at the very beginning of my presentation, we make regulatory decisions based on established practice, and unless something happens by way of new data that challenges that established practice, that's what we will go forward with. So, we don't anticipate redoing this PSHA.

VELETSOS: Dr. Stepp, would you be good enough to display that curve of ground motion hazard, the plot of the annual exceedance probability?

NELSON: Do you know which slide that is? This is Anestis Veletsos.

VELETSOS: This will do. There's one thing that really

I find very difficult to accept, and this is the fact that these curves, even at the extreme values of acceleration do not level off. Do you feel comfortable with that? That leaves me very, very, very uncomfortable.

STEPP: I really appreciate what you're saying. We took some consideration in the project of possibility of bounding motions, placing bounds on the uncertainty distributions. If that had been done, we would see most likely some curving over of these curves. That is, they would not be quite a flattened as you see them here.

We don't know at the moment just what behavior would have taken place. We elected not to do those things for the project because they're not at this point standards of practice, and the difficulty in getting consensus agreement within the seismological community on what constitutes bounding motion, is really significant. It's a matter that's a function of the rock types. There are a lot of issues involved there that made it very difficult for us to do that in the time frame of this study. So, we elected not to do it. You're right, though, that that is the next step I think of improving hazard estimation methodology.

VELETSOS: The Standard of Practice may not involve these very low probabilities.

STEPP: In general, that's a problem here, which I kind of skipped over. NRC's practice is based on nuclear power plant experience, where the operating life is tens to hundred year, and where the interest in hazard input extends maybe to 10^{-6} annual frequencies. We have not previously had experience working at these low levels, 10^{-7} and 10^{-8}, and I suspect if we had had that experience, we would have been developing some other measures to put bounds on ground motion and bounds on the distribution of aleatory variability in particular. But we didn't do that here.

NELSON: Let me ask each of the consultants as you speak to pull the mike close and identify yourself. Peter Kaiser?

KAISER: Actually, I think I've answered most of the questions I had. In one of your statements, you're saying that the results of the ground motions are physically unrealistic. And my question was what were the experts asked to contribute?

STEPP: Yes. That's a very important consideration and an important question. Experience since people began to look at seismic hazard has pretty clearly shown that the experts are not generally probabilists, so we asked the experts and emphasized to them that they were to make evaluations of their input parameters within their expertise, that they should not consider the use of these results when they made those valuations. Any consideration of whether you agreed-- or I should say whether your results were proper for hazard results, or your interpretations were proper for hazard results that may extend to 10^{-8} annual frequencies could do nothing but bias the inputs of the experts. So, we asked them specifically to stay away from any consideration of probability in making their evaluations.

HENDRON: In your database, were all the records rock records only, or did you mix straw records and rock records?

STEPP: There's a range, for the ground motion records specifically, there's a range of recording conditions. We had, and I think Ivan may talk about it later in more detail, but we developed a means--there's actually no straw motion data in the basin and range. So, we took the records that we have, which are dominantly in California, and developed transfer functions for those records to use them at Yucca Mountain. And, so, we were transferring also from California rock conditions, which are must softer than Yucca Mountain, to the Yucca Mountain rock conditions. That's what led us to do the specific definition of the properties of the control motion site. You will hear I think a little more about that later.

HENDRON: Okay. Because I was just going to suggest that maybe if you had used all rock records, maybe some of the scatter would be done away with.

STEPP: It was reduced by doing a transfer function of those motions, and that was a significant part of the ground motion evaluation.

HENDRON: And another point, I assume that you've done similar graphs like Figure 14 here for accelerations for velocity, because we're really more concerned in velocity for the vulnerability of the tunnel.

STEPP: Yes. And, in fact, as you will hear later, the scaling of records for postclosure analyses are really based on scaling velocity hazard.

HENDRON: And have you done the same thing for the ground motion displacement, so that we could get some idea in what you would have for both peak acceleration, peak particle velocity, and peak displacement? Not fault displacement, but displacement associated with the ground motion.

STEPP: We did not do an independent curve displacement hazard curve, no. It comes from the spectrum only.

NELSON: Any additional questions at this point from Staff?

(No response.)

NELSON: Okay, thank you, Carl.

Our next speaker is going to be Ivan Wong, who is with BSC/URS. He's been with the project since 1992, and is Senior Consulting Seismologist and Manager of Seismic Hazard Group of URS Corporation. He's been involved in many seismic hazard evaluations, including more than 200 critical facilities worldwide. He is currently Principal Investigator for the Development of Seismic Design, Input, Ground Motions for DOE's Yucca Mountain Project. And, as I said, he's been with the project since 1992.

Welcome, Ivan.

WONG: Thank you, Priscilla.

I want to absolve DOE of any responsibility for this presentation. They told me that my presentation was twice the length that it should be. However, if you have spent two years in the field swinging a rock hammer in 110 degrees, by damn, we're going to see the results of the study. So, I'm sorry, DOE, here we go. And I know many of you are similar to me in age, so I hope you all took Evelyn Wood, because this is going to go very fast, and I'll set my timer.

What I'm going to talk about simply is a two year program that we undertook to characterize both the subsurface geology beneath the waste handling building--excuse me--surface facilities, and the repository block to characterize those properties, and particularly velocities and dynamic properties, such that we could take the ground motions that were defined by the experts at Point A, propagate them up through that geology, so we could come up with the ground motions at the places where we need a design.

So, we did this not only for preclosure seismic design, but we also calculated ground motions and used the properties that we investigated to assess the postclosure performance of the repository block itself.

Specifically, what does one need? What does an earthquake seismologist need to calculate ground motions once the hazard is defined at this Point A? And let me emphasize Point A is a hypothetical location at the repository. It has the same properties as a point at the repository level, but we've basically stripped off all the geology above that. So, it's, indeed, a hypothetical spot. Why did we do that? We defined Point A because it allowed us a much more efficient and accurate way of calculating the ground motions at the other locations.

What do we need for the seismic design ground motions? We need velocities, in particular, we need shearwave velocities and P-wave velocities, again, both at the surface facilities, beneath the surface facilities and the repository block. We need to know something about the lithology and stratigraphy. We need to know about the nonlinear dynamic properties, because when a material is subjected to seismic valoning, then the material may behave in a nonlinear fashion. And, to a very much lesser extent, we need to know something about densities.

A secondary objective of our investigations, and the investigations occurred basically in the cool summer of 2000, and the cool summer of 2001, we also investigated and obtained properties for the foundation design of the surface facilities.

In designing the program for the emplacement area, or the repository block, there were several issues that we had to address. First, the program focused on the upper block. And I'll show in the next diagram what I mean by the upper block. One thing is on top of the mountain, for obvious reasons, there's limited boreholes, and these boreholes in many cases were already plugged, so they weren't available for us to go back in and do any investigations.

If you've been on top of Yucca Mountain, then you know the topography isn't exactly flat. And, so, that of course constrained places where we could go with our investigations. And it seemed like everywhere we went, there was someone doing an environmental check, and that also constrained what we could do, including leaving the car.

In general, the geology across the repository block is rather uniform, and so that, in a sense, allowed us to make some predictions of what the subsurface geology is.

Again, because we didn't have very many boreholes on top of the repository block, we relied heavily on a seismic technique called Spectral Analysis of Surface Waves. I don't have time to go into an explanation of what that is, but if you would like to learn more, you can speak to Ken Stokoe at the University of Texas, who did our work.

I'm hoping you're looking at your handouts, but you can't see it very well. This slide basically shows the area of investigation. There are three symbols shown here. One symbol is for the SASW lines. I believe those are the circles. The squares are some boreholes where Lawrence Berkeley Laboratory conducted vertical seismic pole filing in 1996 and 1997. And then triangles are some shallow boreholes which we were able to get in and do some down-wave velocity measurements.

Again, as you'll see, most of the investigations were concentrated in the western part of the emplacement area. This, at the time, was the area that we thought was the major emplacement area, and this is the area called the emplacement block. As it turns out, the potential repository has been expanded out to include a lower block, and so we have very few measurements in this portion. These measurements are basically from the LBL study, which at one time, we weren't going to use, until the area had been expanded out. And there is an area up here where we have done no measurements at all.

The reason I point this out is because this is a major source of uncertainty in the characterization of the velocity structure for the repository block, and that has, in a sense, resulted in some of the very high ground motions we see at the small exceedance probabilities, because we've incorporated this epistemic uncertainty into the velocity models.

For the surface facilities, we didn't know at the time the number or the locations or even the classifications of individual facilities. So, we had a very large area to work in. Because of the very large area we worked in, we combined not only the classical approach of using boreholes, but also we supplemented this extensively with SASW surveys.

This is the area that we investigated. To give you some reference, here is the north portal, and this of course is the developed area. Here is the muck pile here. So, the area we investigated was this area shown in blue. The boreholes are indicated by the white circles. There were 16 boreholes in total that we drilled. The deepest borehole was at a depth of about 668 feet. There's a combination of the shallow boreholes, shallow being anything up to about 200, 300 feet. About half the boreholes were shallow, and the other half were deep. The yellow lines were the SASW surveys that we conducted along the site. These SASW surveys were by and large connected for the boreholes, so that we could calibrate the SASW results with the measurements that we were actually doing in the boreholes themselves.

Two types of seismic surveys were done in the boreholes, a down-hole survey, classical down-hole survey, as well as suspension blocking. So, we had two types of down-hole velocity measurements that were done in the hole.

This is just a compilation of the SASW lines that were done on top of the mountain, and I'm just showing this to illustrate the variability in velocities. Here we have on the vertical scale depth below the ground surface. Here we have shearwave velocity and feet per second. And, you can see the variability that one gets from the various measurements. This variability is what we'd expect when we look at other velocity profiles from other different sites.

NELSON: Ivan, can you just clarify? Where do you think the transition is between alluvium and rock? Or is this all alluvium?

WONG: This is basically all rock, Priscilla, because we're at the top of the mountain. There's very little alluvium.

NELSON: Yes.

WONG: This is the VSP that was done by LBL. There were only six holes done. Again, this data was data we were originally not going to use because it was outside the upper block. But because we have to consider a larger emplacement area, we have used this data. This is P-wave data, but we've converted to a shear wave profile using Poisson's ratio.

I think the important feature of this figure is that by and large, because of the limited extent of the velocity measurements we were able to make in the lower block, we ended up using two base case velocity profiles to characterize the repository block. This blue line here is the median results of the SASW surveys that were done. We simply drew a smooth version through this. So, this model is what we call Base Case Number 1.

The Base Case Number 1 was anchored. We did SASW surveys within the tunnels themselves, the ESF. And, so, we've taken the data from 700 feet and simply joined it up with the velocity measurements in the ESF. So, this is our first base case model that we used in our ground motion calculations, which I'll talk about in a subsequent talk.

The solid line here is a smooth version of the VSP. Now, again, because there was an area of the emplacement area that was not characterized by any shear wave velocities, we were concerned about incorporating an adequate amount of epistemic uncertainty into the velocities. So, we decided to increase this VSP by one standard deviation of the VSP measurements, and this resulted in this dash line into the Base Case Number 2.

So, again, in the design ground motions for the repository block for both preclosure and postclosure, we considered two velocity models. And the use of two velocity models has resulted in increase in the epistemic uncertainty, and increase in the ground motions.

This is the location of the boreholes shown here in the surface facilities. Also shown here are three test pits that were dug--actually, four test pits that were dug at the surface facilities to get geotechnical properties for the foundation design.

This is an interpretation of the geology using the borehole data. One of the observations we came upon, which was not surprising, is that we uncovered a number of faults underneath the surface facilities. None of these faults appeared to penetrate the bottom of the alluvium and colluvium beneath the waste handling facilities. And, so, we do not believe that any of these faults are in any way active or earthquake generating. Therefore, we don't think there's any surface rupture displacement potential here. But this does give you an idea, this cross-section goes from, looking to the south, it gives you an idea of the thickening wedge of alluvium and colluvium. Here's the muck pile that underlies the surface facilities and the basic tipping nature of the faults and their faulted configuration.

This is just an example of a test pit where we were doing ring density measurements and collecting samples for static lab testing.

This is just an example of the downhole measurements. We did downhole measurements in each of the 16 boreholes. So, what we're looking at is, again, here is shear wave velocity in terms of feet per second, and I've just tacked on the lithology that was observed when pulling out core from these boreholes.

This is an example of suspension logging, very similar in the sense that it gives you shear wave velocities or P-wave velocities as a function of depth. Again, the lithology is shown on the right.

These are the SASW measurements that were conducted at the surface facilities. I'm just showing the measurements for the tuff. The alluvium at the surface facilities ranges anywhere from zero thickness to about 100 feet, and I'm just showing the tuff velocities. I have a similar figure for both the artificial fill that's at the surface facilities, as well as the alluvium.

Actually, the base case model that we're using for the surface facilities is shown here. The blue line is the Vs profile for the alluvium, and the black line is the Vs profile for the tuff. So, these were the two velocity profiles that were used in the calculations of the ground motions for preclosure at the surface facilities.

Now, to handle what we know will be the variable nature of both the subsurface geology at the waste handling building and the repository block--I keep on saying waste handling building. I'm not supposed to say that. I'm supposed to call it the surface facilities. We used a probabilistic scheme that was developed by Gabriel Toro. This is a scheme or model that we've used on several DOE projects. It basically takes the data that we've calculated, observed or measured both at the repository block and at the surface facilities, and we've developed this probabilistic representation from the statistics from that data.

So, given any base case model, we can use this approach to develop any "X" number of models to run our calculations. As I will show later in a later presentation, we have taken the one base case model at the surface facilities, and the two base case models at the repository block, and we've used this model to calculate 60 profiles.

So, instead of using a single profile to calculate the ground motions, we've actually used 60 for the surface facilities, and 120 for the repository block. And, again, what we're trying to do by using this large number, or large suite of profiles, is to try to capture the variability that one would expect at a location.

A crucial portion of our study was dynamic lab testing that was performed by Ken Stokoe again at the University of Texas. This just summarizes the number of samples that were tested. What we're trying to do here is to get two very important properties of the subsurface geology, again at the waste handling building and the repository block.

What we're after are what in geotechnical lingo are shear modulus reduction curves and damping curves. And these curves simply show the nonlinear behavior of a material when you subject it to seismic, or increased strains. So, up in the upper curve, we have normalized shear modulus. These are our lab results. The curves were basically developed through a subcommittee of experts, including Dr. Silva, Bob Pike, Dr. Constantino from New York, and Dr. Stokoe.

Shown in the lower graph is material damping, again, as a function of shear strain. Here's our lab results. As you can see, there is some extrapolation here, because our lab results are basically confined to shear strains of less than .1 per cent. This portion of the curves is very important when you get to the very high strains that we're encountering in the postclosure, and as well as some of the high strains we're encountering at the surface facilities in the soil.

Okay, let me summarize. So, for the repository block, we used a combination of SASW, vertical seismic profiling, and some very limited shallow downhole measurements to come up with the Vs and Vp profiles for the repository block. It resulted in two profiles, because of our limited data for some portions of the repository block. For the surface facilities, we had SASW data, quite extensive amount of SASW data, downhole data, suspension data. That resulted in a single base case profile for shear wave velocity and compressional wave velocity.

To capture the variability in these properties, what we would expect in any site, we developed a probabilistic representation of those velocity profiles. It's a site specific probabilistic representation, and that was used in the calculation of the preclosure and postclosure ground motions.

Shear modulus reduction and damping curves were developed both for the tuff, the alluvium and the fill. And those were used in the calculations. For the repository block, we only had to deal with the tuff modulus reduction and damping. For the surface facilities, we had tuff, alluvium and fill.

The uncertainties in the velocity structure and the dynamic properties in the emplacement area, and to a lesser extent the surface facilities, have been incorporated into design ground motions, to a greater degree than if more site specific data were available. In particular, I'm talking about the limited dynamic lab testing because of the fact that we were constrained to strains of less than .1 per cent, and the fact that we weren't able to cover completely the enlarged emplacement area.

The incorporation of the uncertainties, because of that let's say lack of data, has resulted in our ground motions being conservative. But we think they're defensible. They're conservative. And we move forward from there.

That's it. Thank you.

NELSON: Thank you, Ivan. A tremendous amount of information. I've got a general question, which almost always comes up when you're combining or using laboratory and field measurements. The agreement in velocity is between the vertical borehole measurements and the SASW is interesting. But the question about working with laboratory reconstituted or cored specimens, and how you take measurements made in the laboratory in a resident common test, I assume was used?

WONG: Threshold shear as well.

NELSON: And compare that to the field. You've presented here modulus ratio, g over g max.

WONG: Right.

NELSON: How do the absolute values of velocities or modulus compare between the laboratory specimens and the field measurements?

WONG: That's a good question. That was one of the issues that the experts struggled with. But they did look, in particular Dr. Stokoe, did look at, for instance, we compared the shear wave velocities from the field measurements, as well as the shear wave velocities from the laboratory testing, and used that sort of observation to be able to extrapolate the curves. So, there's always this issue of taking laboratory results and transferring it to the actual in situ field conditions.

NELSON: Well, in particular, for the lithophysal zones and the lower lith, where some of the lithophysaes are fairly large.

WONG: I don't even think we testing anything--the testing was limited to the Tiva Canyon, the upper portion of the Tiva Canyon. So, we didn't actually get down into the area where the lithophysaes were.

NELSON: And for Stokoe's downhole in the ECRB, were those primarily in the middle lith and the non-lith zones? Did he get into the lower lith in field testing in the ECRB?

WONG: I don't believe so. I can't remember. But I'll have to look. I don't believe he did. There were just limited tests that were done in the tunnel.

NELSON: Okay. Questions? Skip Hendron?

HENDRON: I'd like you to go back to your Figure 10, or your Page 10. I assume from that graph that you don't have any field measurements of shear wave velocity at the tunnel level.

WONG: Except where that little vertical rectangle, where we did SASW in the tunnel. That was sort of our anchor point.

NELSON: Identify yourself.

KAISER: Kaiser, consultant. Can you explain the reasons for that jump in velocity between the 4,000 and the 6,000 foot level? Is it due to measurement method, rock type? What is happening in between that causes that major jump?

WONG: We don't know. We don't have observations below 700 feet, so we simply used the measurements in SASW, and the deepest measurements we had at the top of the repository block, and we connected them up.

Now, remember, these are base case models. So, when we actually do the calculation of the ground motions, we're calculating through our randomization scheme, 60 profiles from this base case. We have also done some sensitivity analyses to see how we should handle this connection. We could have brought, you know, extended the profile down and brought it over here. We could have brought it, at any particular depth, brought it down to connect up with the ESF. We simply chose to, after doing those sensitivity analyses, simply chose to connect the deepest observation point here with the deepest observation point here. And then using that as the base case, we randomized.

KAISER: Can you then explain to me what the difference is between Base Case 1 and the dotted Base Case 3?

WONG: Well, actually, the difference one should observe is the difference between the SASW and the VSP. It's a solid line. The dash line is the final model that we came up to use in the ground motions. Why there is this difference, it could be simply because the VSP was done in the lower block in an area where we didn't have any SASW measurements, so there was no overlap. That could be. That difference could be lithologic. We don't know. It could be, we don't believe so, it could be because of a difference in the technique.

When LBL was performing the VSP, they weren't concentrating on the shallow velocities. What they were trying to do was get a tomographic image of the repository block. So, there could be a difference in technique.

We are discussing, because of this difference and because it's resulting in conservative design motions, not so much conservative design motions, but it's resulting in conservative motions for the postclosure, we are discussing going back in in the repository block and using SASW for those areas where we either have no data or we just have the VSP data. So, we consider those to be confirmatory studies.

Again, I want to emphasize, we accounted for this lack of data through our incorporation of the epistemic uncertainty through the use of these two base case models. So, the motions are conservative. But, again, we believe because they're conservative, we have a high degree of confidence in them.

NELSON: Okay, back to Skip Hendron, and then to Andy. Hang on, Andy.

HENDRON: Two things. When I first talked about shear wave measurements in the tunnel, I meant more direct shear wave measurements from the tunnel itself, since you've got one. I take it you didn't take any measurements from the tunnel itself, from boreholes and propagating shear waves back behind the tunnel.

WONG: I'm not aware of any what you might say direct measurements in the tunnel.

HENDRON: Because it's more direct in the spectral method. The other thing is you have reiterated several times using this envelop off to the right is more conservative, and I wish you would explain that. It may not necessarily be more conservative in some aspects of the ground motions for the tunnel.

WONG: Well, based on the sensitivity analysis we've done using both base case models, the use of the Base Case Number 2 results in higher ground motions. So, that's what I mean by conservative.

HENDRON: Okay. Tell me what you mean by higher ground motions. I assume right now that you're referring to accelerations.

WONG: That's correct.

HENDRON: And we're concerned about velocities for the tunnels, and when you divide the velocities by the shear wave propagation velocity to get strain, it's not conservative to have a high shear wave velocity in that case.

WONG: That's correct. We've done those calculations.

It has resulted in higher peak ground velocities, peak ground displacements, and strains is a function of depth.

HENDRON: So, when you say it's conservative, it's only conservative for acceleration.

WONG: No, excuse me. I said the calculations we have done have resulted in higher peak ground velocities, higher peak ground displacements, and higher strains and curvatures as a function of depth. So, the ground motions, in a global sense, are higher because of the use of these two base case models. We have investigated the sensitivity to the use of the two models.

HENDRON: Higher than what?

WONG: Higher than if you had used a single base case Model Number 1.

NELSON: Let's go to Andy.

VELETSOS: Will you please display the Figure 21?

NELSON: Figure 21, Andy?

VELETSOS: Yes.

NELSON: Can you talk into the microphone, please?

VELETSOS: Yes. Will you please display Figure 21? Yes. For the lowest probability events that you have considered, what were the maximum developed shearing strains in the calculations for the ground motions?

WONG: Okay, I believe they're upwards to 1 per cent strain.

VELETSOS: 1 per cent?

WONG: Correct me, Walter.

SILVA: Walt Silva. Hi, Andy. I need a little bit of clarification. The median strains?

VELETSOS: Well, let's talk about median strains.

SILVA: Okay.

VELETSOS: What was the level for these low probability events? What was the value of the shearing strains that you ended up with?

SILVA: By low probability, do we mean the 2000 year?

WONG: I think he's talking 10^{-7}, 10^{-8}.

VELETSOS: Yes.

SILVA: But you have alluvium up there. That was never run in the alluvium.

WONG: Right. That's correct.

SILVA: So, we have to differentiate between preclosure and postclosure. The alluvium, which would be sort of the preclosure, the waste handling building, there was a 2000 year that was run, and I think we're just finished running the 10,000 year. So, for the 2000 year, the maximum median strains in the alluvium is probably about .3 per cent, something like that.

VELETSOS: It is to the right of the data points that you have?

SILVA: Yes, definitely.

VELETSOS: These are the extrapolated values that are being used?

SILVA: The maximum median strains exceed the data.

WONG: And, again, that is a source of uncertainty that we've addressed, which results in higher ground motions at the waste handling building.

NELSON: Andy, did you want to ask the same question about the tuff, the 10^{-8}?

VELETSOS: Well probably those are higher values. We are further to the right of the data points. Is that right?

WONG: In the tuff, for 10^{-6}, 10^{-7}, the maximum median strains are about the same level. You know, we just have higher base case velocities. So, it turns out that the strains are about the same cases. There may be excursions in some of the randomized cases which get up to 1 per cent, those kinds of numbers, in the tuff at 10^{-7}.

VELETSOS: One point of clarification. What does this median Number 1 and median Number 2 mean? In other words, the solid line and the dashed line.

WONG: Those are the two base case curves that were used in the calculations.

VELETSOS: You mean the velocity profiles?

WONG: No, I'm sorry. There was actually two models that we used in the calculations because of the data. So, we had a median or Base Case Number 1 model, and a Base Case

Number 2. So, similar to what we've done in the velocities for the repository block, we're using two sets of curves.

SILVA: You know, we do complete analyses for each set of curves and each profile. And for the tuff, we had two sets of curves as well, two sets of modulus reduction damping curves to accommodate epistemic uncertainty and nonlinear dynamic material properties. So, we do a complete set of analyses, so you basically wind up with around four sets of median ground motions. Okay? And then we envelope those.

NELSON: Bullen?

BULLEN: Bullen, Board.

Can you go to Figure Number 5, please? This is just a quick question from a non-geologist. You mentioned in your presentation that you had no data in the area of sort of the northeast region, basically the region where we're looking at expanding the repository block, if you will, and you say that that's a major source of uncertainty. You did also mention that maybe in the confirmatory studies period or the confirmation testing program, you'd end up with data.

I guess the question I have is how long does it take to get it, and what are your plans to obtain this data, and how will that impact license application, license to close, whatever? I guess I would like to know a little bit about the plans to get that data.

WONG: Well, it's just in the discussion period. I can't make any commitment that we will do it. We believe that the design motions that we've come up with, we will stand by it at this time, because they are--we haven't--the uncertainty through the Base Case Number 2. We would, however, like to go back in and do measurements here. It would probably take a few months of time to actually do the measurements in here, and we'd like to go back into this portion of the lower block where we just have VSP data, and see if we can try to understand the difference between the two base case models.

BULLEN: Bullen, Board. Just a followup to that. Would that reduce your uncertainty for preclosure, postclosure, or both?

WONG: Depending on the results. I guess we favor Base Case Number 1, which is the data from the SASW. If that difference between Base Case Number 1 and Number 2 were to close down, the uncertainty, the ground motions would reduce, that would impact both preclosure, but in particular, it would impact postclosure.

BULLEN: One last question then. If you did these few month tests and you could reduce that uncertainty, wouldn't you save money in the design?

WONG: Probably.

BULLEN: Thank you.

NELSON: Last question? Richard?

PARIZEK: Parizek, Board.

On Figure 11, you show four test pits. Were these all in the colluvium and the alluvium as shown, like in Figure 13?

WONG: Yes, they were.

PARIZEK: Stratigraphic units?

WONG: Yes.

PARIZEK: So, none of those penetrated bedrock, as such?

WONG: No. No, they were all in the alluvium, colluvium.

PARIZEK: Now, are there datable materials shown in, like Pit 13, or Slide 13, rather, Pit 1, where you really might show ages of any of those layers to constrain the lack of movement? You made a statement there was no evidence for displacement faults uncovered by boreholes, and boreholes are shown in the cross-section on Figure 12, and I was trying to connect the borehole evidence for lack of displacement of those red faults versus your survey lines on Figure 7, all yellow, almost all of them are in disturbed areas. You ran those after this facility was developed, so they're in that stage; right? But you only have a few lines that go out in undisturbed ground. So, I'm trying to say what's the evidence that the faults are not active, or were not active to displace alluvium?

WONG: Several years ago, there was a major seismic investigation that was done across Midway Valley to clear the site of any active faults. In that study, extensive trenching was done, dating the materials of the alluvium and colluvium at the deepest portions of their excavations, which I believe in some places got down to about I think 8 or 10 meters. That dating resulted in the fact that the material was very quaternary in age. So, by our standards, since the faults don't penetrate the bottom of alluvium, they're not what we would consider to be active faults.

PARIZEK: And that Midway Valley study is how far relative to the footprint of this shown on 11, Figure 11? I'm trying to get distances. In other words, it would be nice to know that you really have no active faults in this area in order to kind of constrain this whole question of likelihood of faults and the magnitude, and so on.

WONG: I can't remember right now where the trench was, but it was near the ESF, the entrance to the ESF.

NELSON: Okay, thank you very much, Ivan. We'll be hearing from you again a little later.

And just to let everybody know, we did hear during that presentation from Dr. Walt Silva, who has been a consultant to the project, and he is President and Senior Seismologist at Pacific Engineering and Analysis, and we welcome him and his input.

The next presentation is going to be given by

Richard Pernisi. Richard has been working on Yucca Mountain since February of 2000. He's a civil structural engineer and he works with analysis, design and licensing of nuclear power plants for the past 28 years, and he's been assigned the project seismic coordinator to manage the project's efforts to identify and develop the necessary seismic design inputs, both in the preclosure and in the postclosure time frames. And we welcome him to the Panel meeting. Thank you.

PERNISI: Thank you very much, Priscilla.

Before I get started, I'd like to take an opportunity to elaborate on your question that Ivan tried to answer relative to the Yucca Mountain design. If, in fact, the design solutions that we're talking about are strictly driven by seismic, then any reduction in the seismic design basis ground motions could in fact achieve some reduction in the overall cost of the facility.

However, I will use an example here. The shear walls, the reinforced concrete shear walls used as part of the confinement structure are also being driven by the ability to provide shielding, and the overall thickness of those shield walls are governed somewhat by the shielding as well as the seismic forces.

So, a reduction in the design basis ground motions, if we could achieve, let's say, a 10 or 15 per cent reduction by doing some additional analysis of spectral acceleration of shear waves may not, in fact, provide that much of a reduction in design of those shear walls, and the overall cost of the facilities may remain about the same.

However, it could help. I just wanted to make that clarification.

BULLEN: Bullen, Board. I appreciate the clarification. The point being, though, if the ground motion is less than the size of the shielding, I mean, the crane design, the air handling design, all those kinds of things get a lot cheaper, because you don't have to have the crane falling off of the rails at a lower G force. It's a lot cheaper than it is to, you know, try to nail it up there.

PERNISI: That's correct. Again, in our preclosure safety analysis, if those structures, components or systems are designated as important to safety, then that again would be true. However, if we can demonstrate through that preclosure safety analysis that those structures, systems and components have no affect on safety, then they would not have to be designed to seismic criteria. Okay?

Good morning, everyone, and thank you for coming. I'm here to provide a presentation on the approach to preclosure analysis and design. Later on, we'll cover this in more detail with respect to seismic.

Just to provide some perspective, because most of the Board has been dealing in the postclosure time frame. In postclosure, we develop a total systems performance assessment in order to evaluate those components that are important to waste isolation. That assessment is done to demonstrate the ability of those components important to waste isolation to protect public health and safety through the postclosure era.

In preclosure, similarly, what we do is a preclosure safety analysis, and that's performed in order to evaluate those structures, systems and components that have been designated as important to safety. And this is done in order to protect both the worker and the public health and safety.

This presentation will discuss our approach requirements that drive how we do the work. The work is prepared by engineering groups that produce a preclosure safety analysis and develop the structures, systems and components that are input into that preclosure safety analysis that represent the repository design.

Finally, we'll cover how this work is integrated between preclosure safety analysis and repository design. And, finally, we'll present a brief summary.

In our approach, the preclosure analysis and design has to address several requirements to successfully complete the work. The Code of Federal Regulations, Part 63, requires that the project prepare a preclosure safety analysis. This is done to address the site, design of structures, systems and components that make up the facilities, potential hazards, those being either natural or human induced hazards, the event sequences based on accident scenarios, and the dose consequence analysis.

The project has prepared a PSA guide document to ensure the consistency of our preclosure safety analysis, such that the end products demonstrate compliance to the regulatory requirements under Part 63. The project approach to meet the objectives of Part 63 and the Yucca Mountain Review Plan include a coordinated and integrated effort through the preclosure safety analysis and repository design to meet these requirements.

NELSON: Can I just ask you to go back to that slide? Should that first word be preclosure?

PERNISI: Oh, yes. Good catch. Okay, we're on the next slide, please.

Okay, there are numerous specific safety objectives that have to be addressed by the preclosure safety analysis in order to comply with Subparts 63.11 and 63.12. The important objectives include the formulation of Category 1 and Category 2 event sequences.

Now, event sequences are a series of actions and occurrences within the engineered components that could potentially lead to the exposure of individuals to radiation.

A Category 1 event sequence is one that is expected to occur one or more times prior to postclosure. A Category 2 event sequence is one that has one chance in 10,000 of occurring prior to postclosure.

Now, the event sequences then are formulated using natural or human induced accident scenarios as initiating events. Those event sequences are then run through an analysis, and as part of that, this allows a consequence analysis to be done in order to identify those structures, systems and components that are important to safety. And those are the ones that are credited in the preclosure safety analysis to either mitigate or prevent dose consequence to workers and the public.

These analyses eventually lead to the identification of those structures, systems and components that are important to safety. Again, they're important to safety because they're credited in the safety analysis for mitigating or preventing dose consequences.

In a moment, I'm going to walk through this chart to demonstrate how the preclosure safety analysis and repository design are integrated and coordinated. But, first off, I'd like to go to Slide Number 8 and talk about the repository design as some background to this figure.

The repository design, Design Engineering has the responsibility for developing the design solutions that make up the structures, systems and components for the Yucca Mountain project. The Repository Design Group uses design requirements, design basis, design criteria and methodologies in order to come up with the design solutions for the structures, systems and components that make up the facilities.

The requirements are first provided by the DOE. However, the Repository Design Group then refines these design requirements into engineering solutions to formulate the structures, systems and components that make up the facilities. The design basis documents are prepared by the Repository Design Group, and they document the operational and functional design considerations.

Additionally, the repository design comes up with a design criteria document that outlines the acceptance codes and standards, as well as the details for development of design solutions.

Now, as part of these details, the design criteria looks at things like loads, load cases, the load combinations, and where those load combinations are to be applied. The design methodologies used are those that are accepted within the nuclear industry, and are basically those that have been applied for nuclear power plant construction.

So, if we could go back to the chart, please, on Page 6? I'm going to use this to explain the integration slides on Sheets 9 and 10. As I discussed here, Repository Design prepares and provides to the Preclosure Safety Analysis Group the initial design solutions for the structures, systems and components, as well as their descriptions and functions. That's this first circle here. Okay. This becomes the input to the preclosure safety analysis, and as part of the event sequence scenarios and accident scenarios developed by this group, they look at the initiating hazards, either natural or human induced, and begin the analysis, the event sequence analysis, to determine the frequency assessments and screenings to go on and do the categorization of the event sequences. And that's the Category 1 or Category 2 event sequences that I mentioned earlier.

This work then leads to the consequence analysis. Now, in the process of doing these analyses, and the consequence analysis, if one of the structures, systems and components in the event sequence scenario is removed from that event sequence, or allowed to fail, and a dose consequence results, then that structure, system or component, is this identified as important to safety.

So, as we go through these analyses and we get down here and we ask the question are the doses within the regulatory limits, if we can answer that successfully, because one of the structures, systems and components either allowed to fail or removed, which leads to the dose consequence, has been demonstrated to be able to perform its designed function in this analysis, then it's designated as important to safety, captured on the Q list, and is documented fully.

If in this process we've determined that a structure, system and component is removed or allowed to fail, and it does result in a dose consequence, then that needs more work. So, then, we have to answer the question no, it can't meet that requirement, it goes back through the Repository Design Group for an enhancement in its design or performance characteristics, comes back into the preclosure safety analysis process until it can successfully mitigate or prevent those dose consequences and, again, we can answer the question yes. Here, it becomes designated as important to safety, captured on the Q list, and it then is afforded an appropriate level of design, inspection, fabrication and construction in order to ensure its performance within the safety analysis.

Okay, going on to the summary page then. The approach outline then demonstrates that the preclosure safety analysis objectives and requirements for Part 63 are met. The PSA develops the event sequences and consequence analysis to identify those structures, systems and components that are, in fact important to safety.

The PSA and repository design uses a coordinated and iterative process to achieve the design solutions to mitigate or preclude those consequences to the workers or the public.

And, finally, the goal of all the work done for the preclosure safety analysis and repository design is prepared to demonstrate that the facilities can safety operate to meet the performance objectives of 10 CFR 63 to ensure worker and public health and safety.

And that concludes the presentation. Are there any questions?

NELSON: Okay, thank you. Any questions? Bullen?

BULLEN: Bullen, Board.

Would you go to Figure 6, please, the one that's the flow chart? As you did a determination of dose in that little diamond at the bottom there, I assume that for the surface facility or the waste handling building, or whatever we're going to call it now, you use the standard practice that's used in the nuclear industry for radiation exposure to workers and release to the public.

PERNISI: That's correct.

BULLEN: When you do preclosure safety analysis below that at the repository horizon, how do you deal with the dose in that case?

PERNISI: Well, again, you're using the same processes there to look at accident scenarios to make a determination as to whether there's an accident that can lead to some sort of dose, either to the workers or to the public health and safety. Okay? In that scheme of things, we're looking at the underground structures, systems and components, such as the transporters, the ground support, the way we actually off load the waste into the emplacement drifts and place it in order to make that determination. So, in the underground, it's following this same process.

BULLEN: Bullen, Board. I guess the question I have falls into the transporter and the unloading. If you have an event where you have to go back and mitigate, I mean, you drop a container off a pallet, for example, how do you design for that beforehand, I guess is the question, so that you can mitigate dose?

PERNISI: Okay. Well, right now, the way we're looking at that is the robustness in the design of the waste package is such that currently, it's being designed for a drop accident well in excess of the height that would be associated with it rolling off the transporter in the underground as part of the emplacement process. So, at that point, we don't believe that even though there is a postulated failure of a transporter which would lead to a roll-off type scenario, that it would actually breach the waste package and lead to some sort of exposure.

BULLEN: Bullen, Board. I agree. I was just worried about how difficult it would be to recover from such an accident.

PERNISI: Yes. Well, I agree with that. It would be difficult. But, in that scenario, if the waste package has not been breached, then it's just a matter of putting the appropriate equipment into the drift in order to recover it, place it back on some sort of transporter, bringing it back out for a series of detailed inspections to see what kind of damage has been done to make the assessment as to whether that waste package can be used for emplacement, or if the waste in that waste package has to be removed and placed in a new waste package prior to emplacement.

BULLEN: I guess the last followup question I have to this is where does the seismic play into this? Because this is a seismic meeting. I just kind of want to make the connection here.

PERNISI: Yes. In my next presentation, we'll cover that in detail.

BULLEN: Thank you.

NELSON: Okay. Followup, Ron and then McGarr.

LATANISION: Latanision, Board.

My question is a corollary to Dan's. It has to do with the use of the nuclear power plant siting precedent. What kinds of experiences can you point to that would give you some confidence that these are precedents, if there are precedents, that are useful here? Have there been issues that you can point to that would make one feel warm and fuzzy about using the nuclear power plant siting precedence as a vehicle here?

PERNISI: Well, in these scenarios, we're actually looking, in addition to the siting, to the specifics of the design, such as reinforced concrete shear walls to act as confinement for the hot cells where the wastes are actually processed. Okay? And then in that scenario, you know, we have a lot of experience with the types of load cases being driven by seismic or human induced hazards, as well as the design codes and standards used by the nuclear power plants in order to come up with satisfactory design solutions and to provide adequate margins. So, we feel as though the application of these processes here, as well as the design methodologies that were utilized by the nuclear power plants are more than adequate to prepare design solutions here that are able to ensure the worker and public health and safety.

LATANISION: Latanision, Board. A followup.

If you looked at Diablo Canyon, would you look at that as a useful precedent in terms of--

PERNISI: Yes, because actually in the preclosure safety analysis and design we're going to have to do, the seismicity and the level of seismicity at Diablo Canyon is very similar to what we're seeing here for the 2000 year earthquake event. So, the types of things that we did there and the analysis and the evaluations that were performed, using a risk informed graded approach here for Yucca Mountain, we should be able to apply those same types of methods and techniques to demonstrate the seismic safety of these components.

LATANISION: Thank you.

NELSON: Okay. Art McGarr?

MC GARR: McGarr, consultant.

I'm not at all familiar with the nuclear power industry and their safety procedures, so I'm having difficulty coming to grips with what you've proposed here in this Figure Number 6 of coming up with an exhaustive set of scenarios or event sequences that can lead to a dose. Could you give us a few more examples where a human mistake somehow triggers a sequence that leads to a dose?

PERNISI: Yes, that's fine. Okay, the Repository Design Group would forward to the Preclosure Safety Analysis Group the overall design for the hot cells, which would be the reinforced concrete structures that enclose the hot cells where the waste is to be processed from the transportation casks into the waste packages. Okay? In that process, there's several lifting and transportation occurrences that are part of that hot cell process.

As part of one of these human induced accident scenarios, we would look at a load drop during those processes. Okay? And that load drop then could lead to an uncontrolled release of radioactivity. Now, in that event sequence scenario, we would look at an initiating event such as a natural phenomena, let's use seismic, as leading to that load drop. Okay?

Now, in these event sequence scenarios and the analysis that's being done here, what we would look at is the failure of one of those reinforced concrete shear walls as a loss of confinement. And this would be probably something that would happen. If we had that loss of confinement due to a failure of that reinforced concrete shear wall, that reinforced concrete shear wall would obviously be important to safety.

What would happen there is that if, based on these analyses, we can't demonstrate that that reinforced concrete shear wall has the adequate performance to withstand those seismic loads, it would loop back through our Repository Design Group and go through an enhanced design process. In other words, we'd provide additional thicknesses, more rebar, until that reinforced concrete shear wall was able to successfully perform under the seismic conditions that were being analyzed here.

MC GARR: Thank you.

NELSON: I have a question, or two questions. This is

Nelson, Board.

First, when we go through these important to safety evaluations, it's similar to the kinds of thinking that was done on perhaps on one-off, one-on analyses where dependence is important in terms of what happens in what sequence, and what ultimately gets labelled important to safety or not. Do you find that kind of a situation evolving in these scenarios that you're doing for preclosure?

PERNISI: Well, we're just starting down that process. Okay? And we fully intend to do an exhaustive event sequence scenario and fault tree analysis in order to make that determination. But, we're just getting started with this. It's in process, and I don't have any specific examples that I could go through right now.

NELSON: What's the target date for the establishment of the event scenarios?

PERNISI: The event scenarios established with regard to the structures, systems and components that are provided for that analysis, that is an ongoing process as we speak.

NELSON: So, this summer?

PERNISI: Yes, during the summer. Obviously, it will be completed prior to our submittal of the LA, of the license application.

NELSON: Okay, let me ask one other question. I would expect that there would be additional faults identified in the subsurface as tunnels are excavated, and that there would be some sort of a stand-off distance, or some other way by which poor rock quality would be recognized and avoided in the placement of waste packages.

During the preclosure time period, that would present an additional source of non-uniform thermal loading, in addition to the general heterogeneous thermal loading, because not all waste packages are the same. And that could move some water in the preclosure time frame, I would expect, from warmer zones to cooler zones within the repository that might precipitate some response that may be involved. Is that kind of thinking included in your preclosure safety analysis?

PERNISI: Yes. In order to address the first portion of your question with regard to lining up with drifts that, how should we say, are unacceptable because of faulting or poor rock conditions, there is contingency in the development of the underground block to allow for additional emplacement drifts should that occur.

The scenarios relative to, let's say, moisture coming into one of the emplacement drifts, at least in the preclosure time frame, we feel that that particular aspect is mitigated due to ventilation and the rather dry air that's coming through in the preclosure time frame, would keep the humidity levels in the emplacement drifts to very low levels.

NELSON: Okay. There's quite a bit of water that may move. It would be very interesting to see an analysis that supports that the ventilation would get rid of all the water for you.

PERNISI: Well, I don't know that--

NELSON: I understood that you don't have it now.

PERNISI: Yes. I don't know that it would get rid of all of it. But based on some of the analyses we've done, it's very low levels, the humidity profiles in the emplacement drifts, as long as the ventilation is occurring.

NELSON: Just for clarification, is it possible, as you understand things right now, and this is Nelson, Board, that there may be a part of the drift that is not occupied by waste packages, because only that one part of a drift is susceptible to poor ground condition impacts?

PERNISI: Actually, I'm really not the one to answer that particular question. As I understand it, if we wind up with, like, faulting in one of the drifts, there is some contingency for having considerable stand-offs from the faulting in order not to have to--

NELSON: Given that most of the faults run north, south, and the drifts are roughly east, west, you're probably going to get a fault in more than one.

PERNISI: That's true. If I could, I think that Mark Board will be here later. He can probably better answer that question.

NELSON: Right. Okay. I thank you for a very closely successful morning, and we're just a little bit off schedule. Let us take a little break here, and plan to reconvene at 10:25. So, that's 12.3 minutes.

(Whereupon, a brief recess was taken.)

NELSON: We're going to continue on with preclosure, but before then, Dr. Bill Boyle has asked to have the mike for a moment to address the issue that I left hanging at the end of the first session.

BOYLE: Right. William Boyle, Department of Energy.

And the questions dealt with stand-off from faults because of fault displacement. In general, provided the geology warrants it, and the analyses warrant it, we would just as soon not resort to stand-off distances from faults.

If you look at Page 20 of the presentation that Carl Stepp made, you see that there's 15 different types of faulting conditions, and we would have to first determine which one of the conditions applies. We'd much prefer to make a case that even if the faulting occurred, the system is still safe. We don't need to stand off. That's our going-in approach for seismic. But, in a general sense, that's true for all the rock conditions we might encounter underground, is we'd much prefer to make the case that whatever it is we encounter, if the analyses indicate that it's safe, we don't need to stand off.

NELSON: And my followup question to Bill was that and none of the rock as they're encountered in the underground have constituted rock that you would avoid?

BOYLE: Not any that I'm aware of yet. We haven't found any that we've said we'd stay away from.

NELSON: All right. We're back on schedule, and I'm confused, is Ivan or is Walt Silva going to make the presentation? Ivan, are you going to make the next one?

WONG: Unfortunately, Ivan.

NELSON: We have unfortunately, Ivan, speaking about proposed ground motions for preclosure seismic design and analysis.

WONG: Okay, please, someone knock me out so I don't have to give the next talk. You're going to get tired of seeing my face.

It's a pleasure to be the opening act for Dr. Silva, who's really the brains behind this operation. So, we're going to go through this fairly quickly.

This is the first of two presentations where I'll be talking about the development of the ground motions for design for preclosure and postclosure. In this presentation, much of the methodology that I'll be talking about can be also applied to postclosure. Where the departure is between preclosure and postclosure, is in the calculation of the time histories.

Again, what we're talking about is the development of site-specific design motions for preclosure and postclosure, but I'm going to focus on preclosure here. And the development of these ground motions are consistent with the NUREG CR-6728, which Dr. Silva helped develop.

Just a simplified chart to show where the annual exceedance probabilities that were calculation, the ground motions were calculator for. For preclosure, we're calculating ground motions for 5 x 10^{-4}, what you may call the 2000 year earthquake, 10^{-3}, and 10^{-4}.

For postclosure, we're calculating ground motions for 10^{-6} and 10^{-7}. So, for locations, you can see the checkmarks are the ground motions that we've completed to date. TBD simply means that we haven't gotten to calculating those ground motions yet, but are in the process of.

Ground motions were calculated for two primary locations, Point B, the emplacement area or the repository, as well as the site of the surface facilities, a point that we call D/E. D sits over the central portion of the surface facility area, characterized by soil thicknesses that range from about 30 to--well, actually 20 about 100 feet in thickness of the alluvium/colluvium. Point E is over towards the edge of that area, and it's characterized by either having exposed bedrock or exposed tuff, or soil thicknesses less than 10 feet.

Again, PSHA was defined at Point A with, in particular, two parameters that we defined, the shear wave velocity of 1900 meters per second, which corresponds to the velocity, shear wave velocity underlying the emplacement area, also with kappa. Kappa is a parameter that is characterized the near surface attenuation probably to a depth of about one kilometer.

So, these are assumed properties, but they are based on some limited geotechnical data. We use these assumed properties such that we can get to Point B and Points D and E using out site response analysis approach.

What of the products of the ground motions? Well, we want response spectra, so we have two component response spectra, horizontal and vertical, at various dampings. The frequency range covers the range of engineered structures going from .3 to 100 Hz. We're defining for this site peak ground acceleration at a frequency of 100 Hz.

For postclosure, as well as preclosure, we're developing and calculating three component time histories. The C time histories that are being used come from a subset of NRC strong motion database, which was also developed by Dr. Silva.

For preclosure, which we're concentrating on here, we match the target spectra or the design spectra, consistent with the criteria in CR-6728, just some guidelines on the component correlation. We're also calculating peak particle velocities for horizontal and vertical, and we have calculated these but have not finalized it. We're calculating three-dimensional strains and curvatures as a function of depth. So, basically, going from the top of Yucca Mountain down to the repository level.

Some of the issues and some of the criteria that we're trying to keep in the perspective in doing these ground motions. Remember the Point A ground motions are being defined for specific annual exceedance probability. We are doing a site response analysis to get the ground motions at Point B, or the emplacement area, and D and E. So, we want to maintain this consistency and hazard level.

It's very important in this analysis to be able to incorporate the uncertainty and the variability in the site-specific dynamic material properties, and also the velocities.

One of the issues that has been discussed in 6728 is that in the rock UHS, i.e. the uniform hazard spectra that's been defined at Point A, there is already some site variability that's been accommodated in that. So, to some degree, we may be double counting when we compute our site-specific ground motions. That amount of conservatism, we haven't been able to quantify at this time.

One of the things we want to do in our site response analysis, which is rather state of the art and has not been done previously, is that we believe there is a magnitude dependence on nonlinearity, not a strong dependence, but there is a magnitude dependence, so we have included this magnitude dependence in our calculations.

As Carl showed, the probabilistic hazard at Yucca Mountain can be basically divvied up into two types of earthquake scenarios. Remember, probabilistic hazard is looking at the levels of ground motions associated with a specified annual exceedance probability. There may be different sources contributing to those ground motions at different frequencies.

Looking at the range of ground motions at 5 to 10 Hz for the 2000 year annual exceedance probability, we've done a horizontal deaggregation, and not surprising, the major contribution of hazard here, and contribution is shown in the vertical scale, distance on this scale, and magnitude, as you would expect, most of the hazard is coming from earthquakes that are within about 15 to 30 kilometers, mainly a distance of 15 kilometers of the center of the repository block, and they're in the range of magnitude 5 to somewhere up to magnitude 7. The source of these earthquakes are the block bounding faults around Yucca Mountain, as well as the background seismicity.

If we look at another frequency range, if we look at longer periods, 1 to 2 Hz, we see the contribution of the close-in faults, as well as the background seismicity, but we also see a fairly significant contribution coming from earthquakes that are in the range of magnitude 6 1/2, you can't see it very well, up to about magnitude 7 1/2. And this seismic source corresponds to the Furnace Creek/Death Valley Fault, which is at a distance of about 40, 50 kilometers.

So, at long period, we are getting a contribution from one of these more regional, more active faults that can generate earthquakes upwards to magnitude 7 1/2. At high frequencies, or moderate frequencies, the hazard is being dominated by the close-in earthquakes.

So, one of the things, in addition to the deaggregation and the uniform hazard spectra, that we have taken from Point A for this annual exceedance probability, again, this uniform hazard spectra is the result of the calculations from the PSHA. This shows the uniform hazard spectra, the horizontal spectra for the uniform hazard spectra, and this shows the vertical spectra. This was the original result of the experts, and at Point A, you can see the peak ground acceleration was about 27.27 G, and the vertical was .17 G.

The issue with the vertical spectra here that came out of the PSHA is they both peak, the horizontal and vertical, both peak around the same frequency. This is something that has not been observed empirically, so it was the decision of the project to readjust this spectrum using the horizontal spectrum that was taken from the experts.

This is what we would expect for a Western U.S. earthquake. We would expect the vertical spectra to exceed the horizontal spectra at close in distance at some frequencies, usually at high frequencies, and that they would peak at different frequencies. This is an average of Western U.S. earthquakes.

If we go the next slide, which shows the Central and Eastern U.S., this is based on modelling, since there's very few strong motion records for the Eastern and Central U.S. We see, again, that the vertical spectra exceeds the horizontal spectra for Central and Eastern U.S. earthquakes, and they peak at different frequencies.

Yucca Mountain can be characterized as a site that's sort of midway between a typical Western U.S. rock site, and the typical Central and Eastern U.S. rock site. So, using the procedure that was developed in 6728, we modified the vertical spectra, the original Point A vertical spectra, so that it would have the same characteristics as the empirical data.

So, doing that modification, you can't see it very well, but this is the revised spectra, the vertical spectra, and this is our horizontal spectra, and this is the original vertical UHS that we started out with from the experts.

So, this vertical spectra, revised vertical spectra, and the horizontal spectra are the two uniform hazard spectra that we started out as a basis for our calculations.

Okay, if you remember, when we did the deaggregation, we were getting contributions from seismic sources at different frequencies. We had the more distant, more active Furnace Creek earthquakes contributing the longer periods, and we had the close in earthquakes occurring on the nearby faults contributing to high frequencies and moderate frequencies.

Because of that observation, we have to deal with the fact that we're really dealing with two sources of earthquake contributions at two different frequencies. So, again, we've done a deaggregation. This spectrum here shown in the, you can't see it very well, the dotted line, which is here, that is our uniform hazard spectra, as we've defined it, 1 to 2 Hz, or long period. Uniform hazard spectra at 5 to 10 Hz shown here, which represents the contribution from the close in earthquakes, is shown by this symbol here.

So, we've actually taken the uniform hazard spectra and decomposed it down to a uniform hazard spectra at 1 to 2

Hz, and a uniform spectra at 5 to 10 Hz. And those are the two spectra that we're using in the calculations.

Major source of input for the calculations are our velocity models. And, again, for the repository block, we're using two base case models, base Case Number 1, which is based principally on the SASW, and Base Case Number 2, which is based on the VSP data done by LBL.

Later on in future slides, I've just abbreviated this as BC, and this is UR, which is upper range. So, these are the two shear wave velocity profiles that we've used in the calculations. Similarly, we have P-wave velocity models for the repository block.

For the surface facilities, we have two models. We have our shear wave velocity profile for the tuff, and we have our shear wave velocity profile for the alluvium. Similarly, for compression velocities, we have two P-wave models, one for the tuff and one for the alluvium.

We have our shear modulus reduction and damping curves. For the repository block, we just have the curves for the tuff. For the surface facilities, we have modulus reduction and damping curves for the tuff, alluvium and fill.

In doing the site responsive analysis, we're using a RVT based equivalent linear approach, very similar to the classic and traditional approach used in the program SHAKE, which was developed about 30 years ago. We're decomposing the analysis to take into account different wave types for the horizontal component of ground motion. We're calculating ground motions for both vertical and inclined horizontal component of the SH wave.

For our vertical component of ground motion, we're using vertical and inclined incident P-waves, as well as inclined incident vertical component of the vertically polarized shear wave.

For the vertical component, we're assuming just solely a linear analysis, in contrast to the horizonal component.

Okay, I'm just going to quickly step through the major steps. We don't have time to get into much detail. Maybe we can answer your questions in the question and answer period.

In the first step, again, we have our 5 to 10 Hz spectrum, and we have our 1 to 2 Hz spectrum that we've decomposed from the uniform hazard spectra. We come down to this first step. Because there is a magnitude dependence on nonlinearity, we've taken each of these and decomposed them down into magnitude dependent spectra. So, we have a high magnitude representing the 95th percentile of the distribution of the magnitude deaggregation. We have the median magnitude, and then we have a low magnitude.

So, for each of the two earthquakes, the 5 to 10 and 1 to 2 earthquakes, we have three what we call deaggregation earthquakes. These two earthquakes, by the way, for terminology, we call reference earthquakes. These we call deaggregation earthquakes.

So, in the calculations, we're dealing with six deaggregation earthquakes that we actually calculation through the site response.

This is just a portrayal of some of the randomized velocity profiles. As I mentioned, we have this probabilistic representation that was developed by Gabriel Toro, and we took the base case model, used that procedure, and generated 60 velocity profiles.

So, for the repository block, we have 60 profiles associated with Base Case Number 1, BC, and we have 60 velocity profiles for Base Case Number 2, or the upper range.

To incorporate the variability and shear modulus reduction and damping, we also randomized 60 curves using a similar procedure. So, this is just an example of those 60 curves.

Okay, so, we have six earthquakes, three deaggregation earthquakes with each of the reference earthquakes, and we drive those deaggregation earthquakes up through the soil column through this randomized profiles, as well as using the randomized dynamic properties. So, 5 to 10 Hz, we have this suite of response spectra at the ground surface, and similarly for the 1 to 2 Hz, we have the suite of spectra.

Actually, can we go back? I made a mistake. This suite of spectra is actually for Base Case Number 1, and this suite of spectra is for Base Case Number 2. They're both for the 5 to 10 Hz earthquake. Similarly, for the two base case models for the repository block, for 1 to 2 Hz reference earthquakes, we had the same suite of spectra.

Okay, given this suite of response spectra, we divided by our input rock motion, and the ratio of that is something we call spectral amplification factor. So, we're simply computing the ratio of these spectra, and this gives us the ratio of the ground motions at the point at which we're calculating the ground motions, divided by the rock input motion.

So, for each deaggregation earthquake, each of those six earthquakes, we have a ratio of 60 tuff outcrop responses, because this is being taken from the repository block, divided by the input motion.

So, what we get here is we get a mean spectral amplification for both the high magnitude, the median magnitude, and the low magnitude. And, again, we do it for the various reference earthquakes.

After going through all that black magic, we have the mean spectral amplification for the two profiles. Again, we have the base case model and the upper range. We compute the deaggregation, the mean spectra amplification factor using a weighted average.

Once we get that weighted average, we apply it to the rock input spectra for the two velocity profiles, and as a result, we get the response spectra at either Points B or D/E.

So, for the repository block at 2000 years, these are the design spectra after going through the various steps. So, this is the design spectra for the vertical component. As you can see, they peak in different areas, and we see a slight exceedance of the vertical component, very consistent with empirical data where the major contribution of hazard is coming in from the close in earthquake. We can see the vertical component. You see the horizontal. And then we have the horizontal design spectra. These are basically the envelopes of all those deaggregated earthquakes and the reference earthquakes.

So, for the repository emplacement area at 2000 years, we have a ground motion of .19 g horizontal component, and for vertical, the PGA is .165 g, rather modest motions.

At the surface facility, the next slide, I'm just showing an example of some of the various cases that were calculated. Because we wanted to include epistemic uncertainty in the material properties at the waste handling building, and the variability in site properties, we felt that we needed to be conservative and, therefore, we enveloped all the various cases that we've calculated. And I'm just showing here the examples of the various cases, and that we've enveloped that.

This is a result of this enveloping. So, this is the design spectra at the surface facilities for the 5 x 10^{-4}, the 2000 year earthquake. The ground motions at the surface facility at the horizontal component are .63 g, and also for the vertical component, you see the ground motion is also characterized by peak vertical ground acceleration of .63 g.

In addition to the response spectra, the design engineers wanted time histories. So, we simply took the standard approach of doing a spectral match to our target response spectra. The seed time histories were again taken from the NRC database. This shows the spectral match for the surface facility at 5 x 10^{-4}. The criteria, again, that we used is the criteria that's spelled out in 6728.

This is an example of the design response spectra at the surface facility for 5 x 10^{-4}, and I'm just showing an example of the horizontal time history. This is the time history and acceleration, velocity, centimeters per second, and the time history in terms of displacement in centimeters.

So, we've calculated for preclosure, a single set of time histories for the surface facilities, and for the repository at Point B.

This is an example of the strain-compatible shear wave properties that comes out of our calculations. These strain-compatible properties are given to the engineers doing the SSI analysis to use in their calculations.

So, for preclosure, 5 x 10^{-4}, just to summarize. For the repository block, we're getting a horizontal peak ground acceleration of .19 g, vertical component of .17 g, and at the surface facilities, .63 g for both the horizontal and vertical component.

Thank you.

NELSON: Thank you, Ivan. Questions from consultants or Board members?

PARIZEK: Parizek, Board. There was that western type signal, and then there was that central and eastern type, and you shifted yours to sort of a bastardized system.

WONG: We adjusted for it.

PARIZEK: Well, what does that mean about--does it tell you anything about the earthquake likelihood or--

WONG: It simply describes--okay, can we go back to that slide? That was a complicated slide. I was hoping to slip that one past you. It would be 9 or 10. Okay, 10, this is, again, this is a typical run of the mill Eastern or Central U.S. earthquake based on modelling. But the modelling has been calibrated. So, this is the type of response spectra we would expect, vertical and horizontal component for the Eastern or Central U.S.

And if we go back to the previous slide, this is the typical run of the mill, based on empirical data, strong motion data, of what the vertical and horizontal component would look like for a magnitude 6 1/2. By the way, this is a magnitude 6 1/2 at a depth of 5 kilometers.

So, our site is sort of--it's not a Western U.S. site, it's not a Central or Eastern U.S. Site. And we characterize these sites by the parameter of Kappa. A typical Western U.S. site will have an average Kappa of about .02 seconds. A typical Central or Eastern U.S. site will have a Kappa of .006.

Now, what does Kappa mean? Kappa describes the attenuation we think in the top kilometer of the crust. We know that attenuation in the Central and Eastern U.S. is much slower. Wave propagation acts more efficiently. That's probably because the crust in the Eastern and Central U.S. is more dense. It's more solid, so wave transmission is much more proficient.

In the Western U.S., we've got a lot of crappy rock, it's fractured, it's soft, highly attenuating. And, so, when we talk about Kappa for the Central and Eastern U.S., and Kappa for the Western U.S., we're using a parameter that sort of tries to describe those physical properties.

Yucca Mountain, surprisingly for being a site in the Western U.S., has a very low Kappa. It has properties that are not quite Eastern and Central U.S., but they're not Western U.S.

So, what we've done, what Dr. Silva has done, is he's taken the 6728 procedure, which has v over h ratios for Central and Eastern U.S. earthquakes, and Western U.S. earthquakes, and we've used a weighted average to come up with a set of weighting factors to take our Yucca Mountain site specific horizontal spectra, and compute a vertical spectra.

NELSON: Let me ask somewhat of a followup question. Nelson, Board.

The difference between strike slip and normal faults is also reflect in different spectra; is it not?

WONG: It depends on who you talk to. Walt always tell me, he gives me a seismogram and tell me can you tell me this is a strike slip earthquake or a--I haven't ever been able to do it--but, you know, there are some people who think that.

Yucca Mountain is characterized by an extensional tectonic regime predominantly normal faulting. Almost all the faults that we consider to be seismic sources at Yucca Mountain are normal faulting. There is some strike slip component, but in terms of seismic source parameters, I don't think you can distinguish to an extensional regime.

If you go to California, there's definitely a difference between normal faulting in California, and particularly there's definitely a difference between reverse faulting and strike slip in California. I think it's more of a wash here in Yucca Mountain. But, again, you know, most of the earthquakes we're dealing with here are normal faults.

NELSON: And that varies with distance. So, most of the near faults are normal?

WONG: Yeah, the nearest--well, I think there's like of the 54 faults that were characterized in the PSHA, probably less than five are strike slip, and most of those are the ones that are on the eastern portion of California, like Death Valley, Furnace Creek. Those are strike slip.

NELSON: Okay. Parizek?

PARIZEK: I just wanted to feel comfortable about having made that shift, whether that creates a design difficult, or does it improve design, or is it an error really, or is this accepted practice?

WONG: Well, you know, we're using a NUREG. It is a process that's gone through the review process. It's based in, you know, on empirical strong motion data. So, we definitely think it's a valid process. We use it on other projects.

NELSON: Latanision?

LATANISION: Latanision, Board.

I have absolutely no experience with computational modelling in a seismic context.

WONG: Me either.

LATANISION: I do have some in terms of computational modelling of materials properties. And in the latter, what one typically does is to choose a model to perform some calculations to calculate some properties that you know and, therefore, it allows you to build some confidence that your model is accurately representing what you're trying to calculate, and then to go on and calculate something that is unknown.

WONG: Right.

LATANISION: That confidence building is an important element of at least that experience.

WONG: Absolutely.

LATANISION: What is the equivalent here? Maybe I misunderstood, but what confidence do you--how do you go about generating the same sense of confidence that your modelling is representative of what you're trying to calculate?

WONG: I mean, that's a very good question. The model that we're using is an equivalent with the RVT based model. It's very similar to the SHAKE. Have you had any experience with SHAKE?

LATANISION: No.

WONG: Okay. The equivalent linear model has been around for easily 30, 40 years. And, in particular, the version that we're using, RVT, has been calibrated against thousands of strong motion records. Walt, through work mainly supported by the NRC and EPRI and DOE, has calibrated the heck out of that process. Others have used SHAKE. They've calibrated it with actual strong ground motion records. So, we have a long history of calibration and comparing it to actual data.

So, the answer to your question is do we have high confidence in the model? Absolutely.

LATANISION: Just a followup. If you could point me to some literature on that issue, I would be very interested to read it.

WONG: We would be more than happy to send you boxes of reports.

NELSON: Okay, the last question, Bullen?

BULLEN: Bullen, Board.

Could you go to Slide 15, please? And I know I'm an ignorant, non-seismologist here, but I'm looking at the extrapolation of data, and obviously there's some model that underlies this, but as I look at the data and I try to extrapolate, say, down here, I guess I don't--where do these trends come from? I mean, I know there's got to be a model that describes it, and I'm sure you're deriving, you know, from some data back here, you've got to get this trend that comes up. So, can you tell me sort of in layman's terms, realizing the limitations I have as an engineer, that why you can do that extrapolation and you end up, based on the data that you see there, you end up with those types of curves?

WONG: Okay, since I'm part of a tag team, I'm going to hand off this one to Dr. Silva.

BULLEN: That would be great.

WONG: So, Dr. Silva, wake up.

SILVA: This is Silva. We basically use--well, all of these curves follow a similar pattern, modulus reduction and damping curves. That is, they tend to come down at the higher strain levels. In modulus reduction and damping, they go up. So, we use a general shape to extrapolate, and then we take multiple mean curves to accommodate the uncertainty with that extrapolation. We do the complete analysis with the multiple curves.

BULLEN: Bullen, Board.

So, are there data beyond that, what, .1 per cent strain that you can use to benchmark it?

SILVA: Yeah, the shape of the curves is actually based on data that goes out to strains of 1 per cent, and sometimes beyond.

BULLEN: Okay. So, there are data, and so you're basically just overlaying a curve that you got from some other set of data, and it behaves in this manner, and so with this limited set of data that's less than .1 per cent, I can extrapolate, and that's within the realm or the bounds of what you see?

SILVA: Yes. And we've tried this in practice with real earthquakes actually with this extrapolation to sites that have recorded ground motions that have strains beyond the range at which we have data for that particular site. And it seems to work pretty well.

BULLEN: If I had to put uncertainty bounds on this, what would they be?

SILVA: Well, again, we use multiple curves. So, if you want to have the uncertainty under a single set of curves, or about a single set of curves, we use a range of--well, we have a sigma, natural log units of about .3 at a strain of 3 x 10^{-2} per cent, and then we take bounds on that of plus or minus 2 sigma. So, we allow randomization about a median curves of plus or minus 2 sigma. Okay? And the sigma is empirical.

BULLEN: Bullen, Board.

So, that last sigma gets bigger, or stays the same as you go to higher strains?

SILVA: That stays about the same.

BULLEN: And why is that?

SILVA: Well, because we picked up that uncertainty in the mean curves or the extrapolation with multiple mean curves.

BULLEN: Okay, thank you.

NELSON: Okay. One last, last question from Andy Veletsos.

VELETSOS: I've got more than one.

Referring to your black magic--

WONG: I'm sorry, I don't want to be responsible for that one.

VELETSOS: One question of clarification for my information. What is a 5 to 10 Hz earthquake? What is a 1 to 2 Hz earthquake?

WONG: Okay, if we could go back Slide Number 6? Okay, this is the deaggregation at 5 to 10 Hz, at 5 x 10^{-4}. So, what we've calculated is basically that mean or modal magnitude and the mean and modal distance for this distribution here. And that roughly translates at 5 to 10 Hz to about a magnitude 6 1/2 at somewhere between 5 and 10 kilometers.

On the next slide, the 1 to 2 Hz earthquake, the mean or modal M and D get shifted because the long period. But here, we're looking at an earthquake at the high end of the 6 range, but more out at 30, 40, 50 kilometers.

So, when I talked about the 1 to 2 Hz reference earthquake, I'm talking about this sort of longer distance, higher magnitude earthquake. And when I talk about the 5 to 10 Hz earthquake, I'm talking about the close in earthquake.

VELETSOS: Is your frequency the frequency of the motion you are dealing with? I'm at a loss.

WONG: The frequency is the frequency of the range when we look at the uniform hazard spectra and we deaggregate it, we're deaggregating it between the 5 to 10 Hz, at 5 to 10 Hz, and we're deaggregating the hazard at 1 to 2 Hz.

VELETSOS: Coming back to your Page 9, I like this curve, and please notice that you have a break in your curves in the high frequency range. You have a horizontal segment of the curve in both of your curves.

WONG: Yes.

VELETSOS: By contrast, I don't see these in your other curves.

WONG: That's correct.

VELETSOS: Even though you are going to frequencies as high as 100 cycles per second.

WONG: Absolutely.

VELETSOS: Why not?

WONG: Typically, as you know, for a Western U.S. earthquake, the ground motion saturated peak acceleration at lower frequencies in the west than they do in the east. Sometimes earthquake engineers will assume peak acceleration in the west is like 33 Hz. I mean, that's a classical marker. And that's just because of probably the nature of the crust. In the Eastern and Central U.S. where we have low values of Kappa, where the rock is denser and more proficient in transmitting seismic waves, high frequency ground motions get transmitted very well. And, so, peak acceleration of where they saturated goes out to higher frequencies. So, we're out at 100 Hz when we get to the Eastern U.S.

VELETSOS: All right. You also gave results for peak velocities, and presumably peak displacements. Did you use the velocity acceleration relationships from the Western USA?

WONG: No.

VELETSOS: No?

WONG: No. The final design values for peak velocity and peak displacement come straight out of the site response calculations.

VELETSOS: Thank you.

NELSON: Do you have a question, art?

VELETSOS: One final question on this. The largest of the accelerations that you gave for the probabilities that you considered are certainly very compatible with past experience.

WONG: Wait until you see my next talk.

VELETSOS: Yes. We have now other values, you know. Do you have that information on the largest acceleration record that has been obtained.

WONG: Can I answer that right after lunch?

VELETSOS: Surely.

WONG: I'd be happy to answer that one.

HENDRON: It can wait, I think, Priscilla.

NELSON: Okay. Well, then we'll let you relax until after lunch. Thank you, Ivan.

And we're now going to hear from Richard Pernisi again talking about the preclosure seismic design and analysis. And this is the last preclosure talk, just to get everybody's minds oriented.

PERNISI: This presentation will provide an overview of the project's preclosure seismic strategy for classifying and designing structures systems and components important to safety. This will be similar to the last talk I gave on the overall approach, but focused on the seismic considerations.

We will cover the background that formulated the strategy, mainly by knowing the documents used to provide the basis for the project's seismic strategy, and briefly noting the team members that helped develop the strategy.

We'll cover in some detail the purpose, approach, and key elements, and in order to demonstrate how the strategy is implemented, we'll present an example using the strategy slide.

We will demonstrate how the application of a project strategy develops seismically designs for the structures systems and components important to safety that are at least equivalent, and often more robust, than those designed for other nuclear facilities.

As a background, the project has been working for years on developing the approaches, analytical methods and documentation to develop its strategies by site specific seismic design inputs, and the appropriate ways to apply them to ensure the seismic safety of the facilities. The project prepared and issued to the NRC, Seismic Topical Report Number 1 and Seismic Topical Report Number 2 to document the methods used to assess both fault displacement and ground motion hazards at the Yucca Mountain site, and to outline the methods that would be used for preclosure seismic design which was to apply the risk informed performance based approach to the design that is endorsed by the NRC.

This work led to the development of the project's probabilistic seismic hazard analysis, which was used to determine the site specific seismic hazards, and the PSHA was covered by Dr. Stepp earlier in the presentations. This work led directly to the site specific design ground motions that have been developed and are being used, and in some cases are still being developed, as the work is still in progress.

These results of the ground motions work will be documented in our Ground Motions Input Report and in Seismic

Topical Report Number 3, which are all due to be completed this year.

Now, the strategy team. The members of the team that developed our current strategy are well versed in the various aspects of site specific seismicity and design methods for ensuring the seismic safety of the designs. Most have worked on the project for years, and participated in the development of the background documents. Several members of the team, including Dr. Cornell, Dr. Kennedy, and Jeff Kimball of the DOE, are members of the project's Seismic Review Board, which oversees the project development of site specific seismic inputs, and advises on the methods to be used to apply the work to realize safe designs of the facilities for seismic conditions.

These members are also nationally recognized experts in these subject areas, as is Dr. Stepp, who presented the presentation on the probabilistic seismic hazard analysis.

Okay, the purpose of the strategy, As noted in the background sections, the documents used to establish the project's approach to seismic safety were developed in the mid Nineties. Additional work in this area, as well as updated and new guidance and rulemaking from the NRC has occurred since then.

After reviewing this information, the project realized that its existing seismic approaches and the methods used should be updated to include this information. So, our purpose here was to include this information to update and enhance our seismic strategies. This includes the risk informed methods to develop design basis ground motions that are input to the preclosure safety analysis used to determine the structures systems and components that are important to safety.

It also included a determination of the appropriate levels of design basis ground motion to be used to develop those solutions for those structures systems and components that are important to safety.

The purpose of our strategy is to be consistent with the methods that were outlined in our Seismic Topical Report Number 2, as this is risk informed and performance based, and is the current accepted method for performing this work in the nuclear industry, and is endorsed by the NRC and has been applied to nuclear power plants.

The strategy includes the requirements necessary to be completed to demonstrate that the final design solutions are seismically safe and will meet the performance objectives under the Code of Federal Regulations, Part Number 63 to protect our workers and public health and safety.

Approach to developing the current strategy was to first remain consistent with our Seismic Topical Report

Number 2, which is a risk informed basis, and which has been reviewed and conditionally accepted by the NRC. This document defines two levels of design basis ground motion in terms of frequency categories. Frequency Category 1 at an annual frequency of exceedance of 1 x 10^{-3}. Now, this correlates to 1000 year return period on an earthquake. Also, there's a Frequency Category 2 at an annual frequency of exceedance of 1 x 10^{-4}, which correlates to a 10,000 year return period. Both of these are to be used as design inputs to our structures systems and components.

It's important to note here that the approach adequately captures the seismic performance of structures systems and components important to safety, as a combination of the level of design basis ground motions used, and the procedures, codes, standards and acceptance criteria apply to achieve the design solutions.

Our Seismic Topical Report Number 2 and the project have committed to use those procedures, codes, standards and acceptance criteria that provide a high level of seismic safety and are consistent with those applied in other nuclear facilities, primarily those endorsed by the NRC that are applicable to nuclear power plants.

Finally, the current strategy. For the Yucca Mountain project, we have decided to include an additional level of design basis ground motion designated as Frequency

Category 1-A. The A is just for additional. This has an annual frequency of exceedance of 5×10^{-4}, which correlates to a 2000 year return period. It includes additional analytical work to confirm the capacities of the designs prepared through more detailed confirmatory analysis and limited risk analysis. This is done to demonstrate the overall capabilities of the structures and systems and components determined to be important to safety, to meet the performance objectives, and to ensure work and public health and safety.

To include the acceptance criteria, the NRC standard review plans, based on nuclear power plants, ensures that our design solutions for those structures systems and components important to safety have adequate margins of safety in order to protect work and public health and safety. This approach has resulted in a strategy that we believe enhances our implementation of the existing analysis and design methods that were documented in our Seismic Topical Report Number 2.

Now, using this chart, I'd like to go through an example, which I think is the best way to illustrate how the strategy will be used in order to develop solutions for structures, systems and components that meet our safety criteria.

So, the example we're going to use, we'll go back to a reinforced concrete shear wall that we talked about in the first analysis. Repository design, we'd go ahead and develop a design for the reinforced concrete shear walls, and let's say that those are the ones that provide the confinement for a hot cell where the nuclear waste is processed. As part of the initial design, based on the functioning of that, we can tell by our judgment that that shear wall is going to be important to safety. In order to provide an adequate design for that, the first design basis ground motion we'd use would be at the FC-1A level, or the 2000 year return period.

We would perform a design using the methodologies that are consistent from nuclear power plants to demonstrate that that design in fact meets the code design allowable limits, which is what we're showing here for FC-1A, that the eventual computed stresses would be below the code of design allowable limits. Okay? Now, once we did that, we would say that that is an acceptable design and it's able to function to provide important--to be an important to safety component.

Now, in subsequent analysis in the PSA, we may postulate that that particular shear wall now has to be evaluated for an initiating event in an event sequence using the design basis ground motions associated with an FC-2 level earthquake, and that would be the 10,000 year. Now, this would have higher seismic loads associated with it.

In the re-analysis that the Repository Design people would do, they would go back and recompute the stresses based on these levels of design basis ground motion for FC-2. If we can demonstrate in that analysis that those stresses are still below the code design allowable limits, we can accept that shear wall as being adequate, and move on to the next one.

If for some reason in this analysis we determine that based on the Frequency Category 2 design basis ground motions that the computed stresses in that shear wall, either the concrete or reinforcing steel, go above the code allowable design limits, we go to Step 2 of our strategy, which allows further confirmatory analysis using more realistic strength properties of those materials. And that would be based on test results from compression tests of the concrete, a poll test of the rebar that demonstrate that the material properties are above those minimums assumed in the original design.

If using those, and we apply the methods and procedures to determine the code design allowable limits, and we're still below that, again we could say okay. If not, then we would look at some additional analysis to see what it means if we exceed the code design allowable limits, and allow some limited inelastic behavior to occur. If we can demonstrate in those analyses that we're still primarily within the elastic limits or elastic behavior of that shear wall, then we can say that the confinement capability of the shear wall is maintained, and seismic safety is maintained, and the worker and public health and safety is maintained.

If for some reason we're still demonstrating that we're exceeding this, in our Step Number 3, we can use some nonlinear evaluations to demonstrate the performance objectives are still intact. This can be done by demonstrating strategies of some limited inelastic behavior within minimum distortions that can be easily repairable. And if we can do that, and I'll stick with the chart for a while, if we can do that, then again we're demonstrating that the overall performance of that shear wall is capable of withstanding these seismic design input demands, and still able to perform its function.

Now, in order to demonstrate the overall safety of these kinds of systems, if we have to go to this level of analytical work, we would also include some limited risk analysis using the methods to determine seismic fragilities of the structures. These fragilities would be convolved with the seismic hazards in order to define the annual probabilities for the seismically induced damage states. Those would be the damage states associated with that inelastic behavior. The goal here is to demonstrate that the annual probabilities are so low as to be an incredible event, and we can demonstrate that the function necessary is still maintained.

Now, if all of that fails, our last recourse is to go back and using the design basis ground motion loads, based on a Frequency Category 2 event, redesign that shear wall to meet the code design allowable limits, such that we demonstrate that we are below those code design allowable limits and we have adequate margin in that design to withstand any of the demand forces associated with this level of earthquake.

So, going to the summary page, in summary, the seismic safety of the preclosure facilities will be assured using this strategy. The strategy is consistent with our risk informed regulatory policies that are outlined in the Code of Federal Regulations, Part 63 and the Yucca Mountain Review Plan. The strategy is consistent with our Seismic Topical Report Number 2, and we feel it represents a more detailed implementation of our approach to establishing design basis ground motions based on their risk significance.

The seismic design strategy is based on the identification of those structures systems and components that are important to safety, again, using our preclosure safety analysis methodologies that I explained earlier. And the goal here is to provide assurance that the preclosure performance objectives out of 10 CFR, Part 63.111 are met using either confirmatory and limited risk analysis in order to demonstrate the overall safety of the structures.

And that concludes the presentation. Do I have any questions?

NELSON: Thank you very much. Let me just ask you one question off the top.

This risk reduction ratio, how is that evaluated?

PERNISI: That's in the backup slides. And the key parameter here on the risk reduction ratio, the risk reduction ratio considers such things as allowable stress limits, use of material properties that are well within the elastic behavior. Conservative estimations of the applied loads, and conservative development of the applications of the load combinations. All of this goes into the development of this risk reduction ratio in order to ensure that the designs that are produced have an adequate margin against failure, so that we can ensure that their performance is there under any of the conditions in which they're designed for.

NELSON: Okay. Nelson, Board.

But how is it evaluated? I mean, is it a judgment call? Is it calculated?

PERNISI: It's very calculated. Actually, I'd like to defer the answer to that question, you're looking for probably some more details than I can provide on that, to Dr.

Cornell, who's in the audience. Dr. Cornell, would you mind?

CORNELL: I think Dr. Kennedy.

PERNISI: Maybe Dr. Kennedy can answer that. I'm going to hand this one off to somebody.

NELSON: Thank you. And identify yourself.

KENNEDY: Bob Kennedy. Basically, you have a hazard curve that gives ground motions as a function of annual frequency of exceedance. Ground motions are obviously higher at the 10^{-5} level than they are at the 10^{-4} level. You also analytically develop a fragility curve for your structure that defines conditional probability of unacceptable performance. You define what constitutes unacceptable performance, conditional probability of unacceptable performance, versus ground motion level.

So, at one ground motion level, you estimate a 1 per cent chance of unacceptable performance. Higher ground motion, 10 per cent, higher, 50 per cent. You integrate these two curves together. We call that convolution of the curves, and you calculate the annual probability of failure.

This term that a number of years ago we called risk reduction ratio, and I prefer to call it now just probability ratio, it's simply the ratio of the annual probability of unacceptable performance to the ratio of your design ground motion.

There's conservatism in our design codes. And as you can see on this slide here, typically, the annual probability of unacceptable performance for things that are designed to the kinds of design criteria that these structures are designed to have relatively low unacceptable behavior and an annual probably of about a factor of 10 less than the annual frequency of exceedance to ground motion you designed to, and that's because of the conservatisms in the design codes.

These are calculated values. What's been put up here is some examples of kinds of results that have been produced on previous calculations. On this project, risk reduction ratios are not going to be used. They're basically if, as Rick Pernisi said, if you go to inelastic behavior at this higher ground motion beyond design basis ground motion, there will be a limited risk assessment made. A fragility curve will be developed, and it will be convolved with the hazard curve, and the results will come out whatever they come out. Now, we expect that we will see these kind of ratios.

NELSON: Okay, thank you. Additional questions? Yes, Mark?

ABKOWITZ: Abkowitz, Board. If you could go back to Slide Number 8, please? And this is another non-seismologist asking a question.

I'm a little bit confused about the graph on the left-hand side of this, in that we're looking at 1 x 10^{-4}. But aren't there another set of criteria for ground movement that brings us down to 1 x 10^{-8}?

PERNISI: In postclosure space, but not in preclosure space.

ABKOWITZ: Okay.

PERNISI: This is just for preclosure space.

ABKOWITZ: And is this same frequency distribution used for the postclosure phase?

PERNISI: No. In the postclosure, we're looking at annual probabilities that are much lower.

ABKOWITZ: I understand that. But the distribution that you see there, if I was to carry that out to 10^{-8}, is that the same distribution that's used for postclosure?

PERNISI: Yes. And that will be covered later this afternoon.

ABKOWITZ: Okay.

PERNISI: This is a representative hazard.

ABKOWITZ: Just as a point of information, a little bit of trouble with the tail as it relates to that.

PERNISI: Okay. Well, this is just supposed to be a representation, not the details of that.

ABKOWITZ: The other question I have, sort of the 30,000 feet, why is there such a big concern about supporting preclosure and postclosure in the design process?

PERNISI: Well, because in the postclosure space, all of the facilities associated with the processing, handling and placement will be removed. So, the facilities designed to these levels of ground motion will be removed in postclosure space, and they won't have to be subject to any lower probability design basis motions.

ABKOWITZ: I understand what you're saying, but it would seem to me that the postclosure criteria, for consistency sake, should have probably been applied across the entire domain.

PERNISI: No, that's, under the regulations, that is not what we're doing.

ABKOWITZ: Thank you.

NELSON: Okay. Leon Reiter?

REITER: Leon Reiter, Board Staff.

I'm trying to find out, and maybe you could help me out, what's the basis for the 10^{-3}, 2 x 10^{-4}, 10^{-4} criteria? How does that stem from the NRC criteria for preclosure, the 15 millirem and the 5 rem. Is this some sort of a connection? How were those numbers derived? This is supposed to be in a risk informed evaluation.

PERNISI: That's part of the--oh, how are they derived? Carl, can you help with that? I think that came out of the preclosure safety assessment, didn't it?

STEPP: I'm Carl Stepp. I will give a partial answer to that, and I may ask Bob Kennedy to join in the answer.

The regulation, Part 63, as you know, identifies Frequency Category 1 and Frequency Category 2 components for the repository. Those are defined--the performance criteria for those frequency categories are defined in terms of dose exposure for preclosure. And the actual choice of those annual frequencies to represent those components that are defined by the exposure criteria, the dose criteria, was made in consistency with the nuclear plant system design.

So, we elected to take the 1×10^{-4} as being approximately equivalent to the, or is equivalent to the average experience of annual frequency of ground motion exceedance for nuclear plant designs. And then we adopt the nuclear plant design criteria to carry that forward then to risk base. And the 10^{-3} is just backed away from that by structural considerations. I'd ask Bob to comment on that, if he would.

KENNEDY: Bob Kennedy. Basically, current criteria for nuclear power plant design is to design safety significant items for 10^{-4} mean annual frequency of exceedance ground motion. And there's historical reasons for that selection. That selection leads to ground motion at our existing nuclear power plants that are pretty consistent with what they had been previously designed for. So, it doesn't greatly change the design criteria from earlier design criteria. And a very large number of our existing nuclear power plants have gone through probabilistic risk assessments.

As part of those probabilistic risk assessments, structures, systems and components have been shown that based on them being designed for their design basis earthquakes, which have averaged the mean 10^{-4} ground motion, the annual probability of unacceptable performance of those individual components have typically been in the 10^{-5} to 10^{-6} range.

Therefore, back at the time that Seismic Topical Number 2 was developed, we decided that a good ground motion level for the most seismically significant preclosure structures, systems and components would be to design them for the same level of ground motion that we would design components of the nuclear power plants for, in the aim that this would give us probabilities of unacceptable performance in the 10^{-5} to 10^{-6} range, and a nuclear power plant for a shear wall structure unacceptable performance would be loss of it as a confinement barrier.

Certainly designing to 10^{-4} will get us down very close to 10^{-6} if we're talking about collapse of that structure. So, that established the one bound. The other bound was put in at 10^{-3} to have this idea of risk consistent design to allow certain things that led to less risk to be designed for a lower earthquake level. Now, when Seismic Topical Number 2 was written, we thought there would be about a factor of 2 difference in ground motion between 10^{-3} and 10^{-4}, and said that there's no reason to have any intermediate category.

Now that we have hazard curves, and you'll notice that there's more than a factor of 2 difference between 10^{-3} and 10^{-4}, and that difference in fact from a structural design standpoint is more than a factor of 3 difference in ground motion level, which is very, very important to structures design, and felt that it's really important to have an intermediate category.

We expect that if you design for the 5×10^{-4} ground motion, these structures will have annual probabilities of serious damage that might result in some kind of a potential release down very close to 10^{-6}, maybe slightly higher than 10^{-6}. That's why we have to go through the confirmatory analysis stage and see, and if we don't achieve the goals, we'll have to change some of those structures from 5×10^{-4} design, possibly to the 10^{-4} design.

NELSON: Leon?

REITER: Yeah, let me see if I understand this correctly. I may misunderstand it.

I think what Carl and Bob are saying is that essentially, the way that these numbers are derived was an assumed equivalence of risk between nuclear--it was good enough for the nuclear structures, it's good enough for the waste repository. Rather than as a nexus between a specific criteria, like the 15 millirem or the 5 rem, between that and then a probability of ground motion. Did I misunderstand that?

KENNEDY: Bob Kennedy.

Basically, if we design for the 10^{-4} ground motion, we would have pretty high confidence that we're going to be able to demonstrate that these structures are not sufficiently damaged at a 10^{-6} level, not significantly enough damage that we would get releases to the boundary. As we back off from that kind of a ground motion design level, we will have to demonstrate what the consequences are.

NELSON: Okay, thank you. Seeing no other hands, we will move on to our last presentation before lunch.

PERNISI: Okay, thank you.

NELSON: Thank you very much. And that presentation is the first on postclosure. It's going to be given by Michael Gross. Dr. Michael Gross has been working on the Yucca Mountain project since February of '98, and he brings the project an expertise in Total System Performance Assessment, computational models for structural response, flow and transport, and geomechanics response.

And we welcome you and look forward to your discussion on postclosure seismic approaches.

GROSS: Thank you. Good morning.

This first talk is the first of about five or six talks on the postclosure seismic approach and our results to date. This talk is primarily intended to be a programmatic overview in the sense of I'll tell you what are our major tasks we're doing, what's their general status, where we're at in the process. I hope this provides a context for the detailed technical talks that are going to follow it later.

I want to give a general disclaimer. Almost all the results you'll see from this point on are preliminary data. They haven't gone through the project's checking documentation, and other QA processes. It's not final data yet.

The scope of our technical approach for seismic is driven by a number of compliance and regulatory issues. The first one is that we're primarily focused on the 10,000 year postclosure regulatory period. In other words, if you were to drive us into 50,000 or 100,000 year simulations for seismic effects, we would have to represent the degradation of the structures and engineered barrier differently than we have done so far. So, the current work you've seen has been more or less designed for the first 10,000 or 20,000 years.

Another constraint is from probability. We've talked about the fact that the NRC regulations basically require us to consider annual exceedance probabilities down to 10^{-8}. So, our work tends to focus on very low probability, but very large amplitude seismic events that could destroy the system.

The third consideration is we're using the mean seismic hazards. Previously, seismic was screened out of the site recommendation because it was based on the median hazard curves. Basically, the median is much less than the mean. The mean is typically at the 90th percentile for some of what we have to deal with. So, since we have to deal with mean hazards, it has driven us to a much more detailed evaluation of structural response and seismic response.

Final point is in all the seismic work, our ultimate goal was to represent the damage to the barriers as a failed area that allows flow and transport. In effect, there's a parallel between the nominal scenario and the seismic scenario. In the nominal scenario, you get damage primarily from corrosion processes that degrade the performance of the drip shield and engineered barriers over time. In the seismic scenario, we get damage to those same barriers, but this comes from structural deformation in response to a seismic event.

The technical approach can sort of be summed up in four very simple questions. How likely is the ground motion or fault displacement? How big is it? When it occurs, is there damage to either the drift, or the drip shield, or the waste package or the cladding? And if damage occurs, what's the impact on long-term performance?

Unfortunately, the answers, the methodology is a lot more complex than the questions. I've indicated the questions over on the left-hand side. And we start first with ground motions and fault displacements. For postclosure, we actually deal with a suite of 15 vibratory ground motions. It's essential that we do calculations with that full suite because basically, that captures the uncertainty in the system that we have to propagate down through all the subsequent analyses.

I think Ivan is going to come up later and talk about how we derive those time histories at Point B, which is within the emplacement area of the repository.

In effect, the ground motions are boundary conditions for the later calculations that I'll talk about.

A similar situation with fault displacement. That work is currently going on. We are hoping to screen out most effects of fault displacement, but I am not sure what's going to happen at the extreme low probability end.

Anyway, the vibratory ground motions are boundary conditions for the rockfall analysis and for the structural calculations. We've probably done on the order of 500 rockfall analyses. And the calculations are done with several Itasca codes that represent the state of the art in rock mechanics and underground response.

The rockfall analyses not only include the uncertainty from the ground motions, but they also include uncertainty for rock compressive strength and for synthetic fracture pattern.

The results from the ground motions and the rockfall feed into the drip shield structural response. Primarily what we're looking at here is, in the nonlith, is the potential that the ground motions will eject large rock blocks, almost like a rock burst that can impact the drip shield and cause structural damage.

The drip shield calculations are also done for vibratory ground motion. For the waste package, we just used the ground motions as the boundary conditions, because we assumed that the drip shield will protect the waste package.

Finally, when we have the structural response, we use a failure criterion to interpret the permanent deformations of failed area on the surface of the structure. The failure criterion we're using are basically comparing residual stress to yield stress. I have some details later on. And once you know how much of the surface of the structure fails, we represent that as a failed area abstraction, and that's what goes into TSPA.

The seismic scenario is basically a separate scenario, primarily because we have to consider low probability events. There is no computationally efficient way to represent an event that happens at a 10^{-7} per year annual exceedance probability in our nominal scenario.

Some of this I've already touched on, but let me go through it again very quickly. For the ground motion and fault displacement, that was the box on the top, we've defined actually three ground motions at Point B, which is at the emplacement drift. The first set was 15 time histories for the 10^{-6} per year seismic event, or seismic hazard. We actually did this process iteratively. We first took the 10^{-6} per year time histories, calculated the structural response, saw what sort of damage we got, and used that information to help pick 10^{-7} as the next level. And by the same token, I suspect that if we go to a next level, we'll probably be 10^{-5}.

We also get a three time history, so to speak, from the preclosure work that was just discussed. In that case, it's only one time history, so it really doesn't provide much uncertainty or variability in the boundary conditions.

Fault displacement comes directly from the PSHA report, whereas, in the PSHA report, they define things at Point B and we go through a fairly elaborate process to define time histories--excuse me--PSHA defines the seismic hazard at Point A, and we go through an elaborate process to develop ground motions at Point B. The fault displacement in the PSHA report is at sites within the repository block. So, we have directly applicable information for that.

Rockfall analysis, I think I've covered that. So, the next slide, please?

The drip shield, we analyzed the response to rock blocks on the drip shield, and we also are analyzing the response to the drip shield to vibratory ground motions at these various levels. The drip shield calculations, as well as the waste package calculations include the variability of friction coefficients in addition to the variability caused by the ground motions.

Waste package is primarily vibratory ground motion. We include damage from both waste package emplacement pallet impacts, as well as waste package to waste package end on impacts in our analyses. I'll have more details about that at my later talk just before we end.

The structural response is computed with the LS-DYNA code. It's originally developed for defense applications, with impacts in penetration. It's been used a lot for simulations of auto crash tests, and things like that. It's an appropriate tool for these analyses. And one final thing is the residual stress from the structural deformation.

The failure criterion. What we've done is we get failure stress from permanent deformation with accelerated stress corrosion cracking. And, basically, we're assuming that the damaged areas where the residual stress exceeds this criterion have the potential to form pathways for flow and transport.

We anticipate the accelerated corrosion rates will occur for residual stress below the yield stress, levels like 80 to 90 per cent of the yield stress of Alloy 22, that's appropriate to the waste package, and we're using 50 per cent of the yield stress for Titanium Grade 7, which is the drip shield.

And we're assuming--not assuming--what we've seen in the calculations is that this failure criterion is the restrictive one, so to speak. In other words, another failure criterion you could look at is just ultimate tensile failure of the material, strictly mechanically. But, those levels are not reached in our current calculations, and this would occur in any case well after you get to these residual stresses. So, this is a more conservative failure criteria than the ultimate tensile failure, conservative in the sense that failures happen sooner.

The results for failed area is basically interpreted for performance assessment or total system performance assessment as a failed area abstraction. We're currently using a distribution that defines the failed area as a function of the magnitude of the ground motion. In this case, we're measuring the magnitude of the ground motions by peak ground velocity, and this distribution or response curve is similar to a fragility curve that people use in typical PRAs for NPPs. But, there is a difference. The response curve allows a continuous variation in the amount of area that fails, whereas a fragility formulation tends to be a failure or not failure. It's sort of on or off. And since we're getting fairly low levels of damage to particular the waste package, we feel it's more appropriate to represent it this way.

The seismic scenario, I mentioned that we need a separate low probability scenario to do things efficiently. The seismic event assumes to cause failed areas. This is similar to the patches that are generated by general corrosion, if you've heard WAPDEG type discussions. And we compute the mean dose as a probabilistically weighted sum of the dose for the full range of ground motions that can cause structural damage. I have some more details in my second talk about what that weighted average looks like.

There are a number of conservatisms that I want to point out that are built into this analysis. The first is, as we've mentioned, the ground motions do not saturate at high strain levels, high ground strain levels. This is particularly a consideration for the 10^{-7} time histories. 10^{-6}, people my describe as a conservative, but I don't think anyone would call them physically unrealistic. Whereas, at the higher levels, it would be very useful to be able to cap or define how the rock behaves at these levels.

The structural response has a number of conservatisms built in. The first one is that the material properties are used at a temperature that's conservative over most of the 10,000 years for the waste package. We use materials properties at 150 degrees C., and this is conservative for over 97 per cent of the 10,000 year period.

Degradation of the values is represented, and we use a thickness reduction of 2 millimeters that corresponds to a high percentile corrosion rates over the 10,000 years.

For the Alloy 22, the waste package, we're using an 88th percentile, and that 88th percentile also includes the effect of MIC, microbial induced corrosion, and the aging factors. In other words, the project has a corrosion rate distribution that it uses to represent other processes. That is multiplied by conservative factors. One of them is called the microbial, the second one is for aging. So, this 88th percentile actually corresponds to a corrosion rate that's greater than any that the project has used in its distributions.

We use a 73rd percentile rate for the Titanium Grade 7. That's a bit lower, because we assume the corrosion takes place on both the top and bottom surfaces of the drip shield.

We also believe the damage assessment is conservative in the sense that as soon as a zone--there's a structural response calculation. Cells are typically represented as zones or cells, however you want to call it. And, typically, it's five or four cells through the thickness. We basically assume that even if a single surface cell fails the residual criterion, then everything beneath it is assumed to fail. In other words, we assume the cracks propagate through instantaneously. We're not checking for cracked propagation conditions at this point.

So, in summary, for postclosure, we've primarily used ground motions defined for 10^{-6} and 10^{-7} levels. We are doing structural response and rockfall calculations on each of those levels with a full suite of 15 ground motions. Degradation is included. The damage to the barriers is represented as a failed area for flow and transport that basically comes into being at the time of the seismic event. And the failed area abstraction will be included in a separate scenario for the TSPA/LA. Total System Performance Assessment License Application.

Thank you very much.

NELSON: Thank you. I have a question straight off the top. Slide 5, please. The rockfall analysis only interjects between ground motion and drip shield, not between ground motion and waste package.

GROSS: Yes.

NELSON: So, I assume that the drip shield is assumed to be there?

GROSS: Yes.

NELSON: Do you do any analyses without the drip shield, or is that a part of the repository that's been decided upon?

GROSS: We have not done calculations without the drip shield, in part because over the 10,000 year period that we're looking at, the drip shield, at least in the current analyses I've seen, is predicted to survive on the order of 25,000 years.

NELSON: Nelson, Board. Assuming it's installed. I mean, I was under the understanding that the decision about installation had not been made yet.

GROSS: Well, these analyses are based on the baseline design, if you will, and that baseline design as it currently exists includes a drip shield. So, the DOE may make a management decision sometime in the future, a technical decision to remove the drip shield, but my analyses, and the whole team, works with what the baseline is right now. And the baseline now has a drip shield.

NELSON: Is the seismic input the primary design control on drip shield design?

GROSS: No.

NELSON: It's corrosion?

GROSS: It's a combination, I believe, of corrosion and possibly there were some rock block analyses that were done several years ago. And, so, some of the bracing on the design of the drip shield does reflect those.

NELSON: Okay. Questions? Art McGarr?

MC GARR: McGarr, consultant.

Maybe this question will be answered in a later talk. But I was curious how you relate a given level of ground motion, like say a 5 meter per second peak velocity, to the probability of rockfall.

GROSS: Okay, the probability of rockfall, let me separate it out a little for you. We basically used the PGV hazard curve to relate the probability to the magnitude of the ground motion. So, in other words, you tell me you want to look at a probably of 10^{-6} for the seismic hazard, I'll tell you at Point B, that the peak ground velocity is about 2.44 meters per second, horizontal. And for that ground velocity, we then develop a suite of 15 time histories that are all consistent with that peak ground velocity, but they will have vastly different accelerations. Okay? That suite of time histories is then used as a boundary condition on the rockfall calculations, both in the lith and the nonlith.

So, let me talk about the nonlith, since I think that's how you're thinking. So, in the nonlith, we have this suite of histories for ground motion. We also have various synthetic fracture patterns. And we vary those both stochastically together, more or less in a Monte Carlo procedure, and just calculate how much rockfall we get over perhaps on the order of 100 calculations. And that's what we use to develop the probability of rockfall occurring.

I don't know if I helped to explain that or not.

MC GARR: Thanks a lot. Is there any way to confirm that type of analysis, which is based purely on modelling, as I understand your response? Are there any actual physical experiments that tend to confirm it?

GROSS: Probably you're better off repeating that question with Mark Board.

MC GARR: Okay.

GROSS: I know there is an activity to try to validate the results from the rockfall codes, and compare them to both lab tests and experiments. But he would be the best one to respond to that.

NELSON: Okay, Latanision, Bullen, Abkowitz.

LATANISION: Latanision, Board.

I have a couple of questions. The first one relates to slide 9. In the seismic scenario, the second bullet there, seismic event causes failed areas similar to the patches generated by general corrosion. I guess I'm unfamiliar with the concept. What patches are you thinking about?

GROSS: Okay. The nominal scenario, the way it's currently structured computationally is, and let's just take the waste package for simplicity, the waste package is represented in the WAPDEG model, and it's represented by 1000 different nodes on the surface. And the different nodes include package to package variation, patch to patch variation, various uncertainties that are around.

So, in the WAPDEG model, you will find that you get failures, individual nodes can fail as a function of time because of corrosion processes. And loosely, I probably should have taken the word off, those are referred to as patches on the project. In effect, it's an area of the waste package that can fail.

LATANISION: Latanision, Board.

But it does refer to the phenomenon of general corrosion as opposed to localized corrosion?

GROSS: I actually think it includes both. I know we considered general corrosion, pitting corrosion, and stress corrosion cracking, I think, as the mechanism. You need an expert on this. Not me.

LATANISION: Okay. No, that's fair enough.

GROSS: But patches, unfortunately, has come into the lingo, because that's how the waste package failures, you know, has ten patches failed or has 100 patches failed on the surface. In effect, what you get from the seismic scenario is you'll damage the waste package, and really the way it would be expressed is that a percent of the surface area that's failed.

LATANISION: Latanision, Board.

I'm only concerned about the use of the word general there, because to a corrosion engineer, that means something different than what you've just said.

GROSS: Okay. I agree.

LATANISION: Let's go on, if I may, to Slide 8. Your failure criterion has to do with effectively the correlation between residual stress left from, for example, a rockfall as a function of the yield stress?

GROSS: Yes.

LATANISION: And the criterion then goes on to say that stress corrosion cracking at accelerated corrosion rates. Maybe you get into a matter of semantics, but you're not saying that the rate of corrosion is accelerated, but the rate of cracking is accelerated.

GROSS: Thank you. I agree with what you're saying. But the net effect for the model is that once this damage occurs, we basically assume that that area fails as a barrier to flow and transport.

LATANISION: Latanision, Board.

And it would fail as a consequence of stress corrosion cracking rather than accelerated uniform or general corrosion?

GROSS: You're correct.

LATANISION: Okay. The latter point is important because calculations would show that the influence of plastic or elastic stress on general corrosion is very insignificant. However, if residual stresses are left behind, and a material happens to be susceptible to stress corrosion cracking, then in fact you have a much more problematic situation, and it's a form of localized corrosion.

Now, just to continue that, that leads to the second bullet. The comment is that accelerated corrosion or stress corrosion cracking rates occur for residual stresses below the yield stress, which I agree with. But I don't understand the criteria for Alloy 22.

On the other hand, for Titanium Grade 7, there certainly is evidence from project testing that in representative repository environments Grade 7 will stress corrosion crack. I don't know of any evidence in terms of project data, or any other data, that would show that in representative repository environments, that Alloy 22 will crack.

So, how does this criteria--somehow, it seems that this--

GROSS: I think your information is correct. But, Gerry Gordon will be here in the afternoon and will cover it. I'm sorry, I'm not a corrosion expert. The structural side I can talk to.

LATANISION: Yeah, I'm not a seismic expert either. So, we're on good terms.

GROSS: But, really, Gerry would be the best one to give you the basis for the 80 to 90 per cent.

LATANISION: Okay. I can live with that. Thank you.

GROSS: Okay. Sorry.

NELSON: Okay, Bullen, Abkowitz?

BULLEN: Bullen, Board. I'll defer to this afternoon if Gerry Gordon is going to be here, because I had similar questions to my colleague Dr. Latanision.

NELSON: Thank you, my colleague from Iowa. We will go to my colleague from Tennessee.

ABKOWITZ: Abkowitz, Board.

Could we go to Slide Number 3, please? I was curious about your comments on the third bullet. You mentioned that initially, you were working off of the median seismic hazard, and now you've moved that to the mean seismic hazard. Do you happen to know what percentage of the hazard observations actually fall in excess of the mean?

GROSS: Well, if you look at the PSHA report, and they have hazard curves for PGV, you'll typically find that the mean curve is on the order of 90 per cent, it's at the 90th percentile. But don't hold me to that as a final number. I think in some cases, it's closer to 100 percentile, and a bit less, but it is pretty far out on the extreme end.

ABKOWITZ: Okay. That helps a lot because if it was not nearly that close, it would be a conservatism--

GROSS: Agree. No, no, it is quite far out there.

ABKOWITZ: Okay, thank you.

NELSON: Additional questions?

(No response.)

NELSON: Okay, then it is now five after noon, and we are going to break for lunch. We are going to eat very fast, and we're going to be back here, and we'll call things to order at 1:00 p.m.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

NELSON: This morning, we heard the project overview, and introduced the pre and postclosure considerations, and spent most of the morning considering preclosure background and design. This afternoon, we're going to move on through postclosure analysis, and hear about the geological observations from Jim Brune and Mark Board, and then discussion of the consequences related to the waste package drip shield and engineering barrier performance from Anderson and Gross.

So, let's get started. Do we have the slides ready? Ivan Wong again.

WONG: I hope you all go into a metabolic lull. If you don't, this talk will do it for you. And, by the way, if Dr. Silva tries leaving the room during my presentation, please someone tackle him. Again, I am the front man, the opening act, and please reserve your questions for Dr. Silva.

Okay, this morning what we did was I basically laid out the methodology to calculate the preclosure ground motions. Again, there's a lot of strong similarities to calculating the preclosure ground motions, very similar to what we've done for the postclosure.

Differences. We're dealing with very, very small annual exceedance probabilities, probabilities less than

10^{-4}. Right off the bat, we don't believe them. We know that we're getting to levels of ground motions that are physically unrealizable. This has been a problem for seismologists. We've always wondered what is the physical limit that ground motions can actually obtain. So, we're working in an area that no one else has worked before. We're working in annual exceedance probabilities and ground motion levels that we admit are getting pretty, pretty strange.

So, for postclosure, what I'll be showing you are the ground motions we've calculated for 10^{-6} and 10^{-7}. Please don't make us go to 10^{-8}. We're calculating the ground motions only at the repository level, Point B, not the surface facilities. We're staying in the repository.

The ground motions are being calculated, as Mike Gross earlier gave a presentation on, simply to provide us postclosure performance. We're looking at rockfall, drip shield, waste package, structural response, and also this thing called the seismic scenario abstraction.

Actually, did we skip something, or did I miss something? There was a slide for approach. Okay. Again, 10^{-6}, 10^{-7}, same process as for the preclosure, with the exception of--well, not with the exception--using our equivalent linear process, we calculated a response spectra at Point B, so the emplacement area, and we also determined or calculated the peak ground velocity at the waste emplacement area.

As Mike explained, for postclosure, we calculated a suite of time histories, 15 suites. Each suite consists of two horizontal components and one vertical component time history, so a total of 45 time histories were generally. Actually, we generated a few more than that just to have some spares, so to speak.

To generate the time histories through spectral matching, we actually have to start with a suite of real honest to goodness recorded ground motions, and we did that, as I've mentioned a number of times, we did that by selecting the time histories from the NRC database that Dr. Silva put together. Once we got those time histories, those 45 time histories--excuse me--not quite 45, 15 sets, we conditioned those to the response spectra at Point B, the emplacement area, it was a weak conditioning, just to sort of get a rough estimate of the shape of the time histories after we converted them. We took the response spectra from those time histories and conditioned them.

Once that was done, we simply scaled the ground motions to peak ground velocity at the emplacement area. Again, we have two components. We scaled the horizontal component, what we'll call Component 1, to the peak ground velocity at Point B for the annual exceedance probability that we're looking to. We scaled the second horizontal, as well as the vertical component, simply to maintain the inter-component variability that was actually extracted from the original set of time histories.

So, this is one set. This happens to be at 10^{-7}. This is a horizontal set of time histories in acceleration, velocity and displacement. So, this is just one of the 15 sets of time histories that we've generated for this annual exceedance probability.

And to give you an idea of the ground motions, you can come over here to the acceleration time history, and see that it's somewhere around roughly 15 g, nice modest ground motion.

Next slide, this is the vertical, and you can see we're peaking here, and peak acceleration, about 10 g's, in terms of centimeters per second, peak ground velocity is somewhere around, it looks like around 30 centimeters per second, maybe a little more.

HENDRON: Those velocities don't scare you at all? I'm sorry to interrupt.

WONG: Go ahead.

HENDRON: They're very low. I'm not insinuating they're too long. They're just very reasonable.

WONG: Okay, next slide and I'll explain what's happening here.

Okay, one of the things we want to do is we want to capture the variability in the time histories. So, we had a target spectra, but we selected a suite of 15 time histories, and those 15 time histories are shown in the background information. If we compute the response spectra for each of the horizontal time histories, this gives you the range of ground motions that those time histories represent.

Now, you can't see it on the slide, but if you see it on the figure, you'll see that at peak ground acceleration over here, if you read the vertical scale, you'll see some of the peak ground accelerations get up to 20 g's. Some of the time histories are, oh, let's say 2 g's at peak ground acceleration. So, I just showed you one example of the suite of 45, and it was a random selection. I may have just picked a low one, but it wasn't intentional. The figure here shows the range of ground motions that we're looking at.

If you go to the next slide, 10^{-7}, this shows that same distribution, but I'm just simply showing the distribution in terms of a median 84th and 16th percentile. So, at 10^{-7}, the median PGA is 7 g's. The 84th percentile, it's 14 g's. And that reflects the distribution of those time histories. If they're reasonable, that's okay with me.

10^{-6}, next slide, this is the distribution again. We're looking at an acceleration response spectra, so we have spectra acceleration on the vertical scale, and frequency on the horizontal scale. This is the response spectra of the time histories. From the scale time history for 10^{-6}, if we go to the next slide, then we see the same distribution that we did for 10^{-7}. The median ground motion is 3 g, 84th is 5 g, 5.4 g.

So, the results of our time history development and calculations of ground motions at these two annual exceedance probabilities can be summarized thusly in terms of peak ground velocity. At 10^{-7}, we're talking about a horizontal peak ground velocity of 535 centimeters per second, vertical is 625. At 10^{-6}, we're looking at 244 for the horizontal and 233 for the vertical.

So, the question is is are these reasonable? Are these ground motions that we've seen, observed in nature? And I hope to answer that in the next few slides.

In terms of what we identify as issues with these ground motions, obviously, in the curvilinear process that we've used, when we get up to 10^{-6} in some cases, and 10^{-7}, we're calculating strains that are sufficiently high that we think that the rock mass that we're working with at Yucca Mountain can no longer sustain those strains.

Several of the cases, this was observed for 10^{-6} and for several cases 10^{-7}, the strains are getting just, as I think Walt said, in some extremes, up to 1 per cent strain, which, you know, begs the question can rock sustain those strains without fracturing, and just basically failing. If the rock fails at much lower levels, then you obviously cannot get ground motions as high as one might predict with these annual exceedance probabilities.

We've done some sensitivity analysis, and I'll get into that in the next few slides. We've done some numerical modelling of ground motions using a point source approach. This is an approach that's been around for at least the last ten or twelve years. It's been used by a number of investigators from the USGS and other institutions. And in trying to get to the ground motions at 10^{-6} using this ground motion modelling approach, we're having to deal with stress drops, earthquake stress drops in excess of 1000 bars. And I'll explain that a little later on, whether 1000 bars is credible or incredible.

So, the question is asked can these calculated low probability ground motions that we're dealing with, can they be realized in nature?

Now, in the background information that we've included, we've included a couple figures that show the largest ground motions that we are aware of based on actual empirical data, strong motion records. The largest peak ground accelerations that have been historically recorded are up around 2 g's. The 1985 Mohani earthquake in Canada I believe is the record holder at slightly more than 2 g's, and that was a vertical ground motion. So, in recent terms of the empirical data, 2 g's appears to be, you know, that's the largest recorded.

In terms of peak ground velocities, we're seeing peak ground velocities up around 250 centimeters per second. The record I believe is for a strong motion record from the recent Chichi earthquake, which had a peak ground velocity of about 250. So, that's the empirical data.

HENDRON: Was that on soil or rock?

WONG: I believe it was on--Walt, was it on rock, the Chichi record?

HENDRON: That doesn't count. That doesn't count, really, for this problem.

WONG: Okay.

HENDRON: It's totally irrelevant.

WONG: Well, the point here I'm trying to get across is what are the largest reported ground motions. One can always make a case from the empirical database that it's not site specific. I'm just trying to give a perspective. So, whether you think it's relevant or not, I'm still trying to give that perspective.

The geologic evidence at Yucca Mountain, which we'll hear a little about the precarious rocks from Dr. Brune, and there is also some discussion of whether the deformation of the lithophysaes are some evidence that these very high ground motions may not be physically realizable.

So, the task at hand is this. Can we demonstrate the ground motions at Yucca Mountain at these very small annual exceedance probabilities? Will they saturate at some level? I mean, intuitively, as a seismologist or in the seismology community, we do feel that there's a physical limit. But this question has been asked by seismologists ever since strong motion recordings were made, and there's just never been a really definitive approach to come up with what those ground motions might be, or, you know, what that physical limit might be.

We're in the midst of some scoping studies. Scoping studies are very preliminary in their nature, and those scoping studies can basically be divided up into two types of approaches. One is what we call a strain threshold approach. And, again, this is what we're trying to get a handle on, what is the strain threshold for rock fracture using the approach of an equivalent linear analysis.

So, in that approach, what we want to do is we want to look at the material properties of the rock mass. At what strains will the rock fail, therefore, providing sort of a natural cap to what ground motions might be.

The other approach is what I briefly described, was using this stochastic numerical modelling technique, using the point source or possibly a finite fault, what are the ranges of source parameters one would be dealing with to find out what those ground motions might be.

Obviously, we have a limitation on the strong motion records. If you look historically, the ground motions that we've seen recorded on our strong motion records have increased with time. I remember ten years ago when many engineers felt that 1 g was an impossible ground motion to obtain. But, as we know from the Northridge earthquake, we had several records which were in excess of 1 g.

So, I think we have to resort to numerical modelling, well calibrated approaches of numerical modelling, to try to get a handle on what those ground motion levels might be, and what are the source parameters one would have to have in an earthquake to get to those ground motions.

So, in terms of a little more detail on the strain threshold approach, what we want to do is we want to try to reduce the Point A ground motions, again, those ground motions at that hypothetical location in the repository, and we want to reduce them such that, you know, they don't exceed some fracture strain threshold, whatever that may be.

Now, one of the observations that's been made, and Mark Board can expand on this, is that we have these lithophysaes, these volcanic cooling features that have a very fine thermal structure within them. And if you examine those on a very microscopic level, we notice that they're basically undeformed. Now, if we had really strong ground shaking at Yucca Mountain, we would have expected those thermal features to have been deformed.

So, one might make a case that at least the 13 million years of existence of Yucca Mountain, that those lithophysaes may be some empirical evidence that at least very high ground motions have not been obtained.

The other approach in terms of strain threshold, in calculating the ground motions, we have these shear modulus reduction and damping curves that we have to deal with. Those curves were used the in equivalent linear process, but they haven't been truncated, truncated in the sense that after some strain threshold is reached, maybe those curves are actually truncated. And one would like to be able to get a handle on those curves through maybe dynamic testing, and one could use those sort of modified curves in some numerical modelling to see what kind of ground motions come out of that.

I just wanted to summarize what the Point A ground motions were at these annual exceedance probabilities. Again, this is without the site response. So, at 10^{-7}, we're dealing with 6 g's horizontally, 8.6 g's vertically at Point A. And, again, some very high peak ground velocities.

Now, in doing the strain threshold approach, we can look at two areas. We can look at the area that goes from the emplacement area to the top of the mountain, the area we call B-C, which is basically just the volume of rock mass above the repository. We can take this approach of just scaling the Point A ground motions, somehow averaging the strains at the fracture levels. And this would sort of put an indirect limitation on what the Point A motions might be. That's supposed to be 10 to the 7th, there's no evidence of deformation.

Another approach, one that we've done a scoping study on, is that we can try to apply the fracture strain threshold at some point below Point A. And I'll show in the next figure what I mean. In other words, what we could do is we define the ground motions at Point A based on the PSHA, but if we start at some lower point, let's say hypothetically the location of an earthquake, and take into account what we believe would be the nonlinear properties of the tuff below Point A, then there may be some limiting factor here, or the nonlinearity of the rock mass below may limit what the ground motions are to getting to Point A.

Looking at this familiar diagram, what Walter has done is some scoping studies, and we just defined a Point A prime, and we've put it down at a depth of 680 meters. This distance was based on the available crustal velocity model that we have for the mountain. And, so, what we did is we just calculated the ground motions going from A prime to A.

And, to do that, we have to start with our modulus reduction and damping curves. These are the set of tuff curves that we've used for our normal ground motions--I shouldn't say normal ground motions--but the ground motions that we've calculated for preclosure and postclosure.

And we've come up with five models, and these five models were again developed by our subcommittee of experts, and these five models take into account a fracture threshold that the committee felt was appropriate. And, again, we have no data at the high strains. It's based on their experience and their judgment on how to handle these curves.

The crux of this preliminary scoping study is this. If we start out with our two reference earthquakes, again, our big magnitude earthquake at low frequency, and in this case, we just used the magnitude 7 1/2 at a distance of 51 kilometers, and our high to moderate frequency earthquake at 10 Hz, magnitude 6 1/2 at a distance of 1 kilometer, to get the ground motions at A, starting from A prime, using those modulus reduction and damping curves, we would have to have a stress drop for the low frequency earthquake of 15,000 bars. For the high frequency earthquake, the stress drop would have to be 2,500 bars.

The average stress drop of a typical Western U.S. earthquake is 60 bars. The average for a Central and Eastern U.S. is 120 bars. So, again, I think what this shows is that to get to the ground motions that we're dealing with at these small annual exceedance probabilities, from the standpoint of reasonable range of stress drops, or source parameters for these earthquakes, we're dealing with very, very, you know, extremely high values that we don't think are realizable.

Again, this is a scoping study. And the purpose of these scoping studies is to try to give us a handle on what we feel are the parameters that are most sensitive to, and once these scoping studies are completed, we hope to continue on and come up with what we hope is a defensible case, a case where we can say, or hopefully define where these high ground motions should be truncated.

The next slide simply shows the response spectra. The dash line is the uniform hazard spectra that we started off sometime this morning with, and the other response spectra are our high frequency and low frequency earthquakes to get to Point A.

These are some of the strains that we're encountering. This is using the upper mean tuff, set of degradation curves. You can see the median value. We're getting up to about .2 per cent strain, again assuming the strain fracture threshold that was in the modulus reduction and damping.

That previous slide was for the upper mean tuff. This is the lower mean tuff. You see the large amount of uncertainty here. For this set of degradation curves, we're getting strains of in excess of .3 per cent strain, the 84th percentile is way out there, close to 1.

Okay, there's another approach that we're calling the geotechnical approach, and Mark Board is the one who is in charge of this investigation. What we want to do is we want to estimate the intact mechanical properties of the tuff units below the repository level. So, he started a few weeks ago making some observations, looking at core, and hopefully this will lead to some laboratory testing.

There's some nonlinear codes that are going to be used, in this case, UDEC, to try to model the effects of fractures, which we know exist beneath the repository level, and see what that may be, how that may lead to capping the ground motions, because it is a fractured rock mass and we're not sure. We need to investigate the influence of these fractured rock masses on the modulus reduction and damping curves. And that's the ultimate purpose here of these geotechnical studies, is to be able to develop some modulus reduction and damping curves for the tuffs to input into the ground motion estimates.

The next slide is simply just sort of a diagram that shows the steps that Mark is carrying out to, again, try to get information on modulus reducing and damping.

Okay, so where do we stand? We realize these ground motions are probably not physically realizable. So, we've embarked on a series of scoping studies to provide us some insight into the problem, and to find out what parameters our calculations are most sensitive to.

So, we're still running calculations at Point A prime. We need to characterize the rock properties between A and A prime, hopefully by further in situ strain measurements. Mark has been carrying out some measurements in the ESF. We hope to get some additional dynamic lab testing. Of the samples that were tested by Dr. Stokoe, the tuff samples, we only have one sample that failed, and that failed at a shear strain of about .2 per cent. So, we think, at least based on that, that in terms of the strength of the rock masses in the repository, that we're thinking the rock is going to start failing around that range of shear strains.

Numerical modelling of the rock mass using 2D/3D codes, and then Jim Brune of course is carrying out his investigations with the precarious rock observations.

So, that's it. Thank you.

NELSON: Thank you. I'd like to invite Skip Hendron to speak more about the issues that you were raising.

HENDRON: Just another point. We've got a great big thick length of stuff to read, and I remember someplace in there, something was said about two faults that were in a reasonable distance, one very close, and one a little bit farther away, and certain magnitudes of earthquake on those faults. Do you remember what those were? It was buried in that mass of stuff someplace.

WONG: It was probably Solitario Canyon.

HENDRON: They were both greater than magnitude 6; correct?

WONG: Right. Those are dominant sources at Yucca Mountain, two of the local faults that are probably contributing most to the hazard at Yucca Mountain.

HENDRON: And do you remember what those were?

WONG: Solitario Canyon and Bow Ridge Fault.

HENDRON: I want the magnitude and the distance.

WONG: Oh, remember, the PSHA has a range of magnitudes based on the experts. Roughly, I would say for Solitario Canyon if you bring in all the link faults, it's somewhere between 6 1/2 and 6 3/4. The same for the Bow Ridge. It's a very wide distribution.

HENDRON: And the distance?

WONG: The distances are somewhere within 1 or 2 kilometers. They're the two faults that bound the repository block.

HENDRON: It's something very close to what we had when we studied and reevaluated Hoover Dam for an earthquake here a while back for a three year period of time, and Jon Ake is here, and they conducted studies and the Lake Meade Fault was a normal fault and it was 3 kilometers away, and it had 6 3/4 magnitude on it, and they did both empirical extrapolations, but they did a lot of actual calculations from fault plane and stress drops, and so forth, and they kept the fault offset and the stress drop consistent with what the magnitude was. It seems to me like they had like 1 1/2 meters fault offset, and 100 bars stress drop, if I remember, and they propagated about .63 peak horizontal acceleration to the dam. But they were calculating from a model like that, and it wasn't too far from the maximum drop--at that particular time.

The one diagram, back on Figure 5, the only one that showed acceleration, velocity and displacement. Acceleration is around 10 g, and the velocity there was around 47 a second. But it definitely is too low, because in your table later for this case, you come up with around 400.

SILVA: I think there's perhaps a plotting error, a draft person error there. I think velocity and displacement have been interchanged.

WONG: Good point. Thank you.

HENDRON: Displacement is way high, and the velocity for this case was way low.

WONG: You're right. The report hasn't gone out, so thank you very much.

VELETSOS: I was going to ask you if you believe that in that diagram--

WONG: Well, I just noticed it.

HENDRON: If that's true, that takes away what I was going to say. It makes more sense. Because a lot of things make more sense. I usually do this fingerprint of a record of V squared over AD, and I was getting like 4/10000ths for that, and it's not possible.

VELETSOS: Also, the frequency content is not realistic.

NELSON: Andy, do you have a question?

VELETSOS: On this Figure 5, that has been answered, I think.

WONG: Yeah, I understand that this mistake was intentional. We wanted to see if you guys were awake.

NELSON: While everybody is thinking about their next question, I want to just ask you a question about what you are really going to do regarding this g over g max, and the modelling of the intact rock properties as opposed to the laboratory properties? I can't conceive there not being a bias on the laboratory test results, particularly compared with the full scale, the scale effect. And I'm interested in how the modulus as it's evaluated in the laboratory compares to the modulus as it's evaluated by SASW or field shear modulus, and also if you have any indication of the strain at which you begin what might well be a precipitous brittle failure in the material in the laboratory. And that would not come from the resident column tests probably. It would probably come from other tests.

WONG: Can I hand that one off?

NELSON: Yeah. Who are you going to hand it to?

WONG: Dr. Silva.

NELSON: Okay.

SILVA: The strains at which the rock appears to fracture, or have fractures coalesce from laboratory testing is about .2 per cent, and that does come from the resident column. You can get a little bit higher strains there than at torsional shear. And I believe Mark Board's observations from some of the more full-scale testing, that that strain level where things start to come apart is about the same strain level, about .2 to .3 per cent shear strains.

So, we would expect to see some sort of catastrophic effect on modulus reduction and damping curves at around those strain levels.

Let's see, to your other issue, there's two sets of modulus reduction and damping curves for tuff. One set of curves is lab test driven, and that's the set that's the more linear of the two sets. We looked at the ratio of lab to field velocities or moduli. In this case, the field velocities are lower than the lab velocities, which is opposite the effect we generally see with soils.

And that's probably due to disturbance effect for soils, that is, lab, you see lower velocities than you do in the in situ. And, so, some recent projects have developed to scaling of lab produced modulus reduction and damping curves based upon this ratio of velocities to make them more linear, because in the lab testing, if you have sample disturbance, you might wind up with more nonlinear curves than are appropriate for the field, for in situ.

Well, for the tuffs at Yucca Mountain, the opposite was the case in terms of the ratios of velocities. So, our second set of curves was developed to be much more nonlinear than the lab based curves, and those were really based on the assumption that the nonlinearity is due to large scale fracturing, which small scale lab testing just can't accommodate.

NELSON: These are the ones you're talking about?

SILVA: No, it's a separate set. That's a set that was an attempt to come up with some scenarios of sort of a catastrophic effect of inducing fractures. Okay? Those are not the base case tuff curves. I think what Ivan is showing up there are the two--the middle one is an average of the two. So, the top one, which we can't see, is really the laboratory test driven curves. So, it's more linear sort of model for the nonlinearity in the tuff samples.

The dash curve then is what we would assume would apply if large scale fracturing was contributing to the nonlinearity.

NELSON: But if you look at that, I mean, the experience with the brittle response where you would have a sudden--

SILVA: This is not intended to model that.

NELSON: No, it's not.

SILVA: No. That group of five curves has built into these perhaps a catastrophic effect. That was just a side study to look at the possible saturations.

NELSON: Were there any tests that supported this, I think we're on 20, tests that supported this?

SILVA: No, the only thing that's driving this are tests on other materials where you have perhaps a cemented sand, maybe even baby sands. And if you drive them up to high enough strains, you see a fairly catastrophic effect. So, that was a model we used to try and develop these kinds of curves.

NELSON: And you also get strain rate effects, too?

SILVA: Oh, yeah, sure.

HENDRON: I have something I'd like to--

NELSON: Skip Hendron.

HENDRON: Something I'd like to say to answer your question. It's an idea that I've done before at Nevada Test Site, but I need something to write with up here. I don't think I can put it in words.

But to answer your question, with rock masses, with joints and everything, it's hard to do the curves like for sand and for clay. A number of years back, we had, and Bob Kennedy and I worked in this area, Climax stock, which in granite out here at Nevada Test Site, several highly instrumented experiments where we had tunnels, we knew what fell in, what didn't fall in, what survived very well, and we also know how the propagation velocities changed as a function of the stress level propagating out. The stress level propagating out is a function of the particle velocity increment. And you can back calculate from that behavior a reduction curve like this without ever worrying about doing it in the lab, and even though it is for a P-wave regime, it's not for a shear mode, you can get some idea. And there is a point at which it kind of falls apart.

Unfortunately, I don't have all those numbers with me, but I can tell you conceptually how it was done, and maybe people here could go back to some of the experimental stuff for the explosions in tuff, and try to back out a similar code.

NELSON: Was that because of a--and this is Nelson, Board--was that because of a fundamental material change that occurred, or was it because of a strain--

HENDRON: We know the stress wave propagation affects the measuring, the changes in particle velocities associated with the shoft, and from having instrumental points at a certain distance apart, we knew what the propagation velocities were, as well as the incremented particle velocity jump that took place. And when you got all that information, you can calculate what the strain jump is, and you can calculate the change in modulus, and you can document the modulus reduction as a function of strain level.

I can remember some of the numbers, but not all of them. And I don't know--

NELSON: You're fine.

HENDRON: I'm afraid I need something that stays there.

Okay, sorry to disrupt, Priscilla. Okay, if this is the weapon point, the explosion is here, on a radius, there were gauges at various distances to measure the particle velocity at each of these points. Okay? So, a high particle velocity here. If we go out on a radius and the wave front is going out, you would find, for example, here where we measure the particle velocity v1, we would have a wave propagation velocity, V1, here of a certain value. And the seismic P-wave velocity in this medium was around 20,000 feet per second. And at very high stress levels here, we had high particle velocities. At the highest stress levels, you would find that the propagation velocity was around 14,000 feet per second.

Okay, if we went out here, and by the way, at this distance, call it R1, if you want to, the strain level is roughly equal to the particle velocity measured divided by the propagation velocity. So, we know the strain level. We know what the particle velocity is there. And we can also calculate what the constrained modulus is there, because it's the density times the propagation velocity squared. So, we can get what the effective modulus is there governing the propagation velocity, and we know what the strain is.

So, we've got a strain, and we've got a modulus calculated there, and we can calculate a seismic modulus just from Rho times this 20,000 feet per second squared, the seismic squared. So, we can calculate a reduction factor for that point at a certain strain level. When we go out here, the measured particle velocity, V2, is smaller, you will find that the propagation velocity, V2, increases to higher than 14,000 feet per second, and eventually as you get out here and approach the elastic case, the particle velocity that you measured here is a given value, and the strain is a given value, and you revert back to the 20,000 feet per second when you get below a certain stress level, or below a certain strain level.

And from a series of points like that, some of these shots had five, six, seven, eight points, you could back calculate out a modulus reduction curve versus strain, reduction factor versus strain. That's all I wanted to say. And even though this is not shear behavior, this is P-wave behavior, I don't doubt that similar reduction shape would probably be valid for the shear case, because you're bringing in the relative movements along the joints, and so forth, at the higher stresses, whereas at the seismic levels, those things don't come into play that degrade the modulus.

NELSON: Okay, thank you. Do you want to make any other points, Skip?

HENDRON: I guess I agree with what was said there that these motions are probably too high. I think in the real world, I think that these relationships have got to be truncated by physics. We've got certain shear strength on barrier planes. We've got certain plausible stress drops possible. And with a given magnitude of earthquake, you can go back to some of the models that are available and I think truncate it at some level. I'm not saying I know what the level is. But conceptually, and according to physics, I think there has to be a truncation.

It's kind of interesting that some of the records that are included in this table are kind of interesting. On Hoover Dam, the Bureau guys used some of these same records, like the Morgan Hill/Coyote Lake record, for example, that's real close. It's a tenth of a kilometer away from the fault. And, so, the peak ground velocity was like 80 centimeters a second. The Pacoima Dam record, that probably is a little bit suspect because of it being on that tipsy turvy piece of rock that was up there. And then there's another close one here, Landers earthquake 97.

So, we do have some that show that are pretty close to the fault. Certainly some of these are closer than what your facility is to the nearest fault that could produce magnitude 6 3/4 earthquake, and these values might be taken as physical measurements to show that it can be truncated. You're very close to the fault with those. The more of these records we can find, the better. Because, as you well know, there aren't too many.

WONG: This is it, as far as we know, of all the strong motion records. These are the highest.

NELSON: The point that you brought up about not mixing soil records with rock records is just really important.

HENDRON: Some of that, we have a lot of number of records, like I think you have--starting to mix firm ground and rock records is one of the reasons for all the uncertainty, and I'd rather do with massaging fewer data points rather than more data points and make sure that they're all really rock, and sort of set a minimum value of the shear wave velocity in the area of a seismograph before you even accept the record.

SILVA: All of the records used in the analyses here, the 17 sets and three components, are from rock sites. That soil site, that was just a table to show large values of motions that have been recorded in the past. That's all it was for.

NELSON: Andy Veletsos?

VELETSOS: One point has already been made of not mixing the rock motions with ground motions. I had another question, though. Earlier, you showed a cross-section of the site near where we are interested in, and then you showed these many layers that had these big slides on the movements and the faults, and then there was another layer that was not disturbed.

WONG: That's correct.

VELETSOS: Could that be used, that information be used as a basis for determining what was the maximum event that has occurred in this location?

WONG: The slide that showed the cross-section went through the surface facilities. So we really have to go into the block, for instance, the Solitario Canyon and the Bow Ridge fault. We have measured displacements on those faults, and we have some inference on what the event displacements are. Those faults do show that the displacements are consistent with earthquakes that have typical Western U.S. stress drops.

So, you know, empirically there's no animal out there, there's no fault out there that we would expect would, you know, give these very high ground motions.

VELETSOS: Well, isn't that the basis--

WONG: There's a lot of what I would call circumstantial information. I think the strongest piece of circumstantial information is the strong motion database itself. If you look at all the rock records worldwide, you know, it's not site specific, but I think it gives you the range of rock motions that one would expect to see.

Yes, I think it's empirical evidence, but it's circumstantial. And as I pointed out, as the strong ground motion database has grown, the upper limit to ground motion seems to have crept upward. So, we get to a point at where can we say this must be it? This must be the physical limit, given the site specific conditions at Yucca Mountain. And, right now, that's what we're trying to achieve.

NELSON: Okay. Was there one more question? Because that question itself was a pretty good lead-in to our next presentation.

KAISER: Kaiser. I just have a somewhat speculative question. We listened to what you present, and if we looked at the numbers, which are 2 g, maybe 3 g, and how many meters per second, and then you go back to the presentation this morning from Carl Stepp and we look at what the predictions were from these many experts and what kind of probabilities, and when we put the cap on, we would end up at the 10^{-4}. So, it wouldn't be--going towards very high accelerations.

WONG: That's correct.

KAISER: So, how are you going to resolve that contradiction that the physical evidence is going to show very low values compared to all this probabilistic work that you did?

WONG: Well, as Carl pointed out, we asked the ground motion experts for their assessment of, in this case, ground motion as a function of magnitude and distance and fault type in a biased way. Because they're not probabilists, and few of us are probabilists, we said give us your functions without any truncation. That truncation was not carried out in the PSHA. Often in standard practice, that assumption is used that you can truncate the attenuation relationship at 2 or 3 sigma. But that has been a standard of practice that no one has really been, let's say, pushed to the wall and had to defend. And we just used it.

So, that's I think the dilemma that we're in. It would have been nice to truncate the ground motions and the PSHA, and the curve would have flattened out. You wouldn't have continued out to 6 g's at 10^{-7}. But, we felt at the time that there was no defensible position to be able to truncate those. And, so, this is the price we pay.

So, you know, obviously these ground motions have drawn a bit of attention. We realize that. We realize they're not physically realizable. But what we have to do, what we feel we're compelled to do is come up with a strong enough case that we can present to you folks and the NRC that says we believe the cap, the saturation which physical realizable ground motions is X g's, X centimeters per second, and then we move forward from there. So, that's why we're carrying out these scoping studies.

NELSON: Okay.

HENDRON: I think you should consult with the Bureau guys that did the Hoover Dam thing. I think they did an excellent job of mirroring the empirical measurements with the calculations to try to make a consistent picture. And they also tried to calibrate some of the models with the micro earthquakes they measured.

WONG: Well, actually, the approach that the Bureau uses and the approach we use is very similar. We have worked for the Bureau of Reclamation for the last ten years doing the same thing that we're doing out at Yucca Mountain. We've done that for the Bureau. So, we take very similar approaches. And we're coming up with the same conclusion, that these ground motions are crazy. But, you know, that's the burden we have right now.

NELSON: Okay, thank you, Ivan three.

Now we're going to hear from Jim Brune, who received his Ph.D. from Columbia University, I won't tell you when, and was at Scripps Institute of Oceanography for many years before becoming Director of the Seismological

Laboratory at the Mackey School of Mines at the University of Nevada, Reno, and he's presently Professor of Geophysics, still involved with the laboratory, and also associated with the Department of Geological Sciences. So, we welcome.

BRUNE: Thank you. My co-author here is John Anderson from our lab.

When you're talking about something that repeats once every 100 million years, you've got to be talking about models. And, in particular, of course we can talk about statistical models, but I'm going to talk a little bit about physical models and physical constraints.

I'm going to talk a little bit about precarious rock, so I'm going to start right off by bragging about the fact that I published an article in 1996, I showed these pictures, I said these would be knocked over if there was shaking of 2/10ths g, and a Hector mine earthquake, a once in 10,000 year earthquake, on that nearby fault occurred, knocked these rocks over, a nearby strong motion instrument recorded about 2/10ths g. So, I claim a success.

Of course, I might say that this is definitely a scoping study. Nothing is QA'd, except one thing that is sort of QA'd is we originally did some stuff on the program to estimate the toppling acceleration. Now, that did get Q'd.

I don't think I need to spend a lot of time on a lot of this because it's already been discussed. But, we're going to argue that there's a good chance that the uncertainty is not handled correctly rather than the mean values being wrong.

And when I said model, okay, I'm going to talk about a very unusual model. This is a foam rubber model where big blocks of foam rubber are stressed up to create earthquakes. And the advantage is I can repeat these over and over. So, I can get real statistics on this. In a case of the earth, we don't have any good strong motion records for normal fault earthquakes anywhere near at high magnitudes in close to the fault. So, we're extrapolating from small earthquakes.

In this model, I can create what's called a characteristic earthquake. Everything is similar, so it repeats over and over. And I put in an accelerometer in the backward directivity, that is, directing away from rupture, forward directivity, and intermediate, and I drew a lot of events, and I get a Gaussian.

Now, distinct from the Gaussians we've been talking about here in earthquakes where we've got essentially zero data, these are real Gaussians. That is the real shape of them, except maybe the tails way down there don't mean anything. But, I've got dozens of events to constrain them.

Now, the hypothesis we're proposing here is that what's happened in the PSHA is only having a few values up near the peak there, and not knowing at a given value whether it's forward, backward, where it is on the radiation, these were all thrown into a Gaussian and the whole set was fit to one Gaussian rather than--we didn't have the information to separate them out into the individual Gaussians.

Of course, that puts a tail out there. It's a lot higher out, and the question is is that tail real. And, in this case, we know for sure it isn't. No matter how many times I repeat this, we're never going to get those values out there. And that's a purely statistical problem. We fit a broad Gaussian to a sum of narrow Gaussians, and that's a mistake.

If we plot this on a log-log plot instead of a linear plot, then you will see this is exactly the same thing. To explain what we're hypothesizing, say it might be the other extreme from what we're calling the ergodic assumption, you have the three real Gaussians there. You fit this other Gaussian to it, and by the time you get down to 10^{-6} times the repeat time of one of these characteristic earthquakes, you can see, and I've scaled things to match the case of the real earth, if you'd used the real Gaussians, you're talking about maybe 2 g, 2 or 3 g down here.

If you used this erroneous Gaussian, where we've put "the epistemic into the aleatory," in other words, we've put too much aleatory ground motion in there and then integrate it in the time domain, you can see you're getting up to 20, 30 g's. So, if this model is right, that explains a large part of what happened in PSHA.

So, as I already said, the other extreme is the real physical foam rubber model where these earthquakes repeat, and that's what we're calling the characteristic ground motion model, because the ground motions more or less repeat every time. There's good physics behind that. There's not a tremendous uncertainty in it.

I'm going to go through these definitions quick, because you can read them. Aleatory is a random uncertainty from event to event at the same site. So, that's those narrow Gaussians. Epistemic is knowledge uncertainty. We don't know whether we're in the forward, backward direction, where we are, radiation pattern.

Ergodic process is one where you assume that the scatter that you see in space domain from the very few data that we have right now, very few earthquakes, we have a lot of scatter, if we assume that it goes into the time domain, then you're getting that broad Gaussian, you're getting those high accelerations.

This is a discussion on how it's dealt with in PSHA.

We conclude from looking at this that certainly it's true for the foam rubber model, and it may be true for the earth. We're hypothesizing that. The aleatory uncertainty should only include the effects that vary in time for these particular sites. And that means that narrow Gaussian is where you have to put the aleatory.

The effects of the spatial variability should go into the epistemic category. If you mix them up, you're going to have this problem.

Now, I'm going to discuss for the sake of whether this applies in the real earth or not, I'm going to make two assumptions. The reason I have to do this is because it's not clear what a priori assumptions went, at least in my mind, and I think to a certain extent John and Gabe Toro and I have gone back and forth at great length about what's actually in the program. It's not totally certain. But we know what's in our model.

The first thing we're going to assume is the experts are approximately correct in their estimates of the mean ground acceleration. It's the tails that go way out that's possibly the problem.

The second assumption we make is the appropriate statistical model is likely to be somewhere between the ergodic extreme and the anti-ergodic extreme. That's those two models I talked about.

Given this, we need to look for field evidence to determine the more appropriate models. That's just a background on why I'm going into the next data.

Okay, the first question is at Yucca Mountain, the earthquakes don't repeat very often. So, it's hard to make a test of the ergodic versus anti-ergodic proposition there. But on the San Andreas fault, we have magnitude 8 earthquakes that are occurring 100 times more often. We have a bigger data sample. And I've done a lot of study of balanced rocks around the San Andreas fault. In the background here is some of the rocks. We've tested a lot of them. We know the age dates are thousands of years.

We plot the values of those on this graph, and the constraint, because the ground hasn't shaken much more than somewhere around the 10 per cent in 50 year values from the hazard maps, we're somewhere near the mean values of ground acceleration.

But if you look at these hazard maps for the 2 per cent in 50 years, they go way up here. And why do they go up? Because you're moving out on the tails of that ergodic assumption. If you wait long enough at this particular site here, these curves just keep going up to infinity, and as Ivan said, there's probably got to be somewhere to truncate this.

So, this seems to say that you are truncating it somewhere around the 500 year repeat time, not at the 2,500 year repeat time here, which is inconsistent with these rocks. So, that suggests that the ergodic assumption is incorrect, at least in one place where we have frequency enough earthquakes, 100 times more often than San Andreas fault, to test it.

Okay, this just translates over to the Yucca Mountain hazard curves, where I've taken the hazard curves, but I've shifted the axis by two orders of magnitude to take into account that at the San Andreas, we have 100 times more samples than we do at Yucca Mountain. And that's the kind of constraints that you would get on the ground motion from those values. It constrains you down quite a bit less than the median value for the toppling rocks.

This point is an estimate from the shape of the cliffs at Yucca Mountain. And this last point down to the right, around 2 g, and maybe at more than a million years, is based on non-shattered rock, which I'll give you a discussion of in a minute. But, that's how these kind of probabilities fall on the curve once you correct for the factor of 100 difference in frequency.

So, I've given you the ergodic hypothesis, anti-ergodic. The next bit of evidence of the shattered rock. This is shattered rock that you typically see on the hanging wall of all thrusts in Southern California, Banning, San Gabriel, White Wolf, everywhere you go, on the hanging wall of the faults, you see this shattered rock.

Now, we know from a couple of records that we're talking about peak ground accelerations of around 1 or 2 g, probably, and velocities of a couple hundred centimeters per second. Unfortunately, in most of these cases, we don't know for sure how many of these earthquakes this rock has been exposed to, but it's been exposed to at least a few. And I'm arguing that this is the consequence of it, and one of the bits of information for that is you go across to the footwall, you see something totally different.

On the footwall, lo and behold, you find balanced rocks. So, the same cases where we had the White Wolf fault, this is only a few kilometers from the trace of the fault on the footwall side, and it was a magnitude 7.6, 7.8 earthquake. It didn't knock this balanced rock. There's balanced rocks all over here. The footwall of these thrust faults does not shake very much.

But the other point is they're not shattered either. This is about a 15 foot high rock here, and there's no shattering evidence. It's jointed and weathered, but if you go around here, there's nothing like that shattered rock. So, hypothesis is if you go from the typical ground velocities and accelerations that we expect on the footwall of thrust faults, namely probably less than 100 centimeters per second, and less than half a g, you don't get the rock shattered. You go to the values you get on the hanging wall, they get shattered.

So, we may be able to use this as actual field evidence to support some of the things Ivan was talking about. At strains of 10^{-3}, you expect to start seeing shattered rock, and you do in the field.

Okay, what evidence do we have at Yucca Mountain? It's kind of hard to test the ergodic hypothesis because the frequency of earthquakes is so low there. You don't have many earthquakes to repeat to test this. This is the mechanism by which--this is from a paper by John Witney and myself published some years ago--this is the mechanism which these rocks form.

This is a stack of rocks in Solitario Canyon, and it's to indicate two things. One is you have this stack of rocks, which indicates the ground hasn't shaken very much there in the last we're claiming a few thousand years. In fact, the age date, the cosmogonic age dates of the rock behind this stack of rocks are about 80,000 years, indicating that whatever was there before has peeled off something like 80,000 years ago.

But the other point is there were huge blocks of this rock that are not shattered at all. So, they haven't been exposed to the kind of stresses and strains that we saw in the hanging wall thrust. And Mark Board is going to talk a little bit more, but there's evidence that what fractures do exist there are really old. There's no evidence of any recent fractures in this rock in the last several million years for sure. So, that indicates, if you follow that indirect argument through the thrust fault in California, these rocks have not been exposed to any kind of strains like that.

Okay, there's a lot of these balanced rocks up and down Yucca Mountain, so they're not just small occurrences.

We've tested some of these in the field. We've done numerical tests on them, computer simulations, and also gone out and put a force through the center mass to see typically what some of these would tip over. And you can see that they're pretty small fractions of a g. They're around 2/10ths g, would knock a lot of these rocks over. I think that is a pretty solid conclusion.

More controversial is the question of how long you can prove they have been there like that, and that's indirect, definitely not QA.

Here's another stack, and actually, the bottom rock is split, and this one is standing up on top of it. The cosmogonic age date on the pedestal right here is about 250,000 years. The next slide shows you some of the cosmogonic age dates of these rocks. That one was Whitney 1, which is 242,000 years. That says that cosmic rays have been coming in, and the thing has been uncovered by at least half a meter, or so, for the last 242,000 years.

Of course, that doesn't prove it was exactly in the shape it is right now, but it does prove that the erosion rate is very slow, and that these things have been there a long time. The rock varnish on them, which covers the whole rock and tells you that the whole rock has been exposed to air, they're all greater than 12,000 years old.

So, how does this translate to ground motions at Yucca Mountain? Well, I'm scaling the peak of the foam rubber to the recurrence time for one of the big earthquakes at Yucca Mountain, a magnitude 7 earthquake occurs about once every 10,000 years, and has ground motions somewhere around some fraction of 1 g, and I've put those three Gaussians for different places on there, and then put on where the precarious rock constraints are.

The fact that the cliffs, I don't have any data on that, but we've got a paper coming out where we show that the effect of the ground motions from the nuclear shots just completely knocks the cliffs down. And based on the fact that the cliffs at Yucca Mountain are very steep and they have stacked rocks on them, so forth, we think that that's around 100,000, maybe more, that you can prove that the accelerations that you've seen in the nukes has not occurred. So, that's that dot there.

And then the non-shattered rocks, here scaled to Yucca Mountain, is given by the crosses there, and that gets you up around 3 g. So, this is new data. It's a totally scoping study, and it's totally un-QA'd, except for, as I mentioned, the early part of it, but it suggests one I think reasonable explanation for those very large tails going out there. This is 10^{-6}, but if you take the 10,000 years as the peak there where the magnitude 7 occurs, then you're talking about somewhere down here for your 10^{-7} and 10^{-8}, and that does give some constraint. The fact that the rock is not shattered gives us constraint on the ground motion.

This is a plot, the same kind of a plot, on the hazard curve for Yucca Mountain. Basically, it says if that argument is correct all the way through, that somewhere around 2 g limit on the strains, or accelerations, and roughly at 1×10^{-7}, so it tends to constrain you down there near the lower part of those curves.

Now, the last thing I wanted to talk about is we have evidence from trans-tensional and normal faulting earthquakes in other areas. I've published a couple of papers in recent years where we have evidence that a big earthquake, like a magnitude 7 has occurred, and we have balanced rocks on the footwall, or fairly close to a trans-tensional fault. And this is an example at Honey Lake where we're only about less than 1 kilometer on the footwall from a scarp, which is about 2 meters high, a fairly young scarp.

Now, that confirms that the ground motions on the footwall of normal faults are very low. Unfortunately, I don't have much data to say anything about the hanging wall side. But, there also is the Honey Lake strike slip fault that goes nearby here, just a few kilometers away, and we know it's had three events in Holocene, and it has not knocked these rocks down.

So, like I say, I've got a couple of papers that have been published arguing that normal faults and trans-tensional strike slip faults are much lower in acceleration.

This gives my estimate of the peak ground acceleration compared with some of the standard curves for a number of these faults in the basin range where we know there have been big earthquakes on these faults recently. And you can see that the precarious rocks give a constraint that's considerably lower than the projected values for these large earthquakes.

And, lastly, kind of a summary of the data constraints going back to these original curves for the San Andreas fault, where you have the dark line in the middle is the median regression curves from Abrahamson and Silva. The LJB is the constraint I already talked about at Love Joy Buttes, which we think is several thousand years old, and puts an upper limit. And you can see that when you make what we're calling the ergodic assumption, that accelerations go way up and are inconsistent with the rocks.

But I've got a bunch of points plotted down below that which are my constraints for trans-tensional earthquakes, like Honey Lake, Beaumont and Turkey. Now, Turkey, the recent Turkey earthquake is one of the few cases where we have a lot of ground motion data for a big magnitude 7 plus earthquake, 7 1/2. One of the characteristics of that fault trace is that it does have a bunch of trans-tensional step-overs in it, and the ground motions constraints for that earthquake are way low. They are less than 1 sigma below the median curve that's predicted. One of them has a ridiculously low value of only a tenth g. These are instrumental recordings on a magnitude 7 plus earthquake.

So, that tends to support that at least in trans-tensional and normal faulting earthquakes, the ground motions are quite low. But, I think it also supports the idea that the ergodic assumption is probably wrong, even in the case of the strike slip earthquakes, like the San Andreas.

Well, you can read the conclusions. Precarious rocks may provide a constraint on low probability ground motions. And they're smaller than those determined by the PSHA.

NELSON: Thank you very much. Very interested in this. Richard?

PARIZEK: Parizek, Board. The illustration in at least one publication I reviewed implied that a lot of the spherical weathering was occurring with the soil chasm, and then somehow you stripped the soil away later to leave the precarious rocks.

BRUNE: Right.

PARIZEK: There are a number of examples around the world where you can see rounding and boulders forming in front of your eyes, and it doesn't appear there's any need to have any soil present at all, I mean, granite blocks falling apart. In Egypt, you'll certain relics there that are falling apart and sticking up in mid air.

So, the question about the role of the soil and not having soil present, trying to constrain that in terms of the ages of how long these have been exposed raises an interesting question. You talked about a Carbon 14 date covered by an outer crustation. You talked about the varnish, and I guess there's some arguments about how useful the dates from varnish are.

Can you comment further on where you stand on putting age restrictions on some of these?

BRUNE: Well, it definitely depends on the type of granite. Some granites are kind of loose and they're almost decomposed granite to begin with. They erode very fast. In many of these cases, the only thing you can say is they survived the last few earthquakes, because they could have been changing with time.

In other cases, the granite is so hard that it just lasts thousands of years without any change. I've got a picture that I usually give in the talk showing a statue of Osiris carved out by the Egyptians and put out on the desert that's 3,500 years old and it looks brand new. But that's really good granite. They picked out good granite. Now, a lot of these rocks are good granite. Some of them are not very good granite.

Another bit of information you might tie into this is the fact that in most of these areas, when the rocks get knocked off and fall down on the ground, they dissolve very fast. In many of these areas, you don't find any rocks down at the base of a cliff where they should be, because they get in the ground and the acids in the groundwater start dissolving them, and the sediments, and so forth. But this only applies in the desert, of course. We're talking about desert areas. You don't even find these things where there's high rainfall and it's non-desert. But in those areas, we're claiming, and this has to be reviewed, of course, by everybody, that these things stay essentially the same for thousands of years if they're good granite.

PARIZEK: Parizek, Board.

Were you working on the lithophysal cavities that the previous speaker talked about, this little chilling around cavities that are 13 million years old?

BRUNE: That's a point that maybe I didn't make as much as I should have. But the fractures that are there at Yucca Mountain always have this--Mark Board is probably going to talk more about this, which has little features on it that indicate it has not slipped in, say 10 million years, just for an argument, since it cooled, basically.

Now, if any of that kind of shaking that I'm claiming created the shattered rock on the footwall, the hanging wall of thrust faults occurred, that would have caused fractures to move all over the place. And there's no evidence--well, I leave it up to Mark to say what his constraint is. But, that seems to me a pretty strong argument that very high strains have not occurred, because they would have caused the faults to move.

PARIZEK: If no one else has questions, I have a picture, if you would indulge me for a minute. It has to do with precarious crystals that maybe gives us a new opportunity, all the work on mineral date and lithophysal cavity fills and joint fills. Many of you obviously have seen the secondary mineral discussions.

The point here was that with these very delicate mineral in-fills in some of the lithophysal cavities, it would seem like you really have these precarious crystals with a bulbous top, with opal, which are age dates, and it would be possible perhaps to look to see if any of these have snapped off. All I knew is the U.S. Geological Survey used to say if I drill a hole, I could never get any of these crystals preserved, because the drilling disturbs it so badly.

Once the tunnels are put in, they could then go in and do systematic sampling. But, it seemed like a perfect place with some of these needles being as small as they are to maybe do simple tests. Like you were pulling with your pulley, this would be something, making a shaking table, or you pull on one or push on one and snap it. I think maybe it's useless.

On the other hand, they're present at all depths in the rock, I guess, and they might say something about the accelerations at the repository level and maybe help constrain this problem of the concerns that we seem to have today.

Does anybody else know where I'm coming from from this? I think it's--there's an age date or two, but I didn't get the right slides on it. But the point is those of you who follow the mineral dating information realize there's a lot of very excellent dates on the opal part of the story, and the age dates are systematic with regard to the different layers that have been stripped off from the mineral surfaces.

So, you, say, needles lying down, buried over, earthquakes of the past, or whatever. There's an opportunity maybe to go back in time and look for evidence like this, or just show it's impossible, that these things are tough as hell. You couldn't shake them loose.

NELSON: Comment by Bill Boyle?

BOYLE: I'm glad you showed that slide. We've already had discussions within the project with Zel Peterman about the possibility of using these crystals, exactly as you described. He mentioned using a frictionless pulley and using a special glue to attach a pulling mechanism to them, and dynamically test them, or put them in a shaking table.

The interesting thing about these secondary minerals is they are hundreds of thousands to millions of years old. And I asked Zel in the absence of measurements, you know, of the type we're discussing here did he believe that their fragility indicated qualitatively that they couldn't possibly have been subjected to ground motions larger than have ever been measured anywhere on earth yet, and he said yes.

And in addition to these, we also have the vapor phase minerals, which Jim Brune was alluding to as well, Hematite and other minerals that I discussed with Steve Beason. These, again, are fragile minerals, either blades or needles, that we don't have any measurements on, or anybody shaking them, or anything. But I asked Steve did he believe that the presence of these old minerals 12 plus million years old that are fragile, if their inherent fragility indicated to him that they couldn't possibly have been subjected to ground motions larger than anybody has ever measured anywhere on earth, and he indicated yes.

PARIZEK: That's very interesting, because we talked to Zel about this at the Board meeting in September, and then also Joe Hale with the Geologic Society of America meeting about opportunities maybe around this area. So, I'm glad to see someone is following up on it. But, it's a great place to spend some dollars.

NELSON: Thank you, the Senator from Pennsylvania.

One last very fast question? We're 25 minutes late now.

KAISER: Kaiser, consultant.

In your paper that I briefly read for the Southern California, you had a map showing the contours, you had it on the slide as well, and if I remember right, the accelerations were less than .3 g's, about 50 meters from a fault, and less than .1 g, about 75 kilometers from a fault. Were you able to compare that to measurements and do the measurements to confirm that map that you showed?

BRUNE: Could we go back to the slide that has the San Andreas fault?

KAISER: It doesn't have the scale on that one. Yes, that one.

BRUNE: Well, the answer is that the rocks are consistent with the 10 per cent and 50 year maps, which are the 500 year repeat times. But, they're not consistent with the 2 per cent and 50 year hazard maps. Does that answer your question?

KAISER: Well, my question is there must be measurements in that area, measurements of ground motion and acceleration.

BRUNE: You mean instrumental?

KAISER: Yes.

KAISER: No, forget it. Everything we've been talking about here, there are no instrumental ground motion records for big earthquakes. That's part of the problem. We're extrapolating from small earthquakes at large distances, trying to guess. We've never had a big magnitude 8 earthquake on the San Andreas fault since we've had modern instruments. So, there's no data. We're extrapolating in.

The Turkey data and the Taiwan data are the first cases in history where we have a lot of strong motion data in close to big faults, and I showed some of that.

HENDRON: From the Landers?

BRUNE: The Landers had a record at 1 kilometer at a site nearby, but that doesn't really constrain the motion out at the distances we're talking about here. And, also, it had a layer of low velocity stuff, which they're still debating how much it amplified the motion. But, I think it was around 1 g accelerations on that. But, that's only one point, one record, though, for that earthquake. And the Taiwan earthquake, which is a thrust fault I think in soft rock, again has very little accelerations compared to the mean predicted by the curves used at Yucca Mountain.

So, all the data we have is quite a bit below the mean values, and it's in foreign earthquakes, so some people dismiss Turkey and Taiwan.

I think to use a little argument about the precarious rocks, is so far, the precarious rocks have been totally consistent with all the instrumental measurements we've made on these other earthquakes. That last graph, or just a couple of graphs from the last, makes this point. Go back one. I've got several points here. By the way, the recent Alaska earthquake had only .3 g at about 3 kilometers. So, that falls right here. All the data we have from Turkey, Taiwan and Alaska is consistent with the precarious rock data, and indicates that peak ground accelerations are quite a bit lower than even the mean estimate on the attenuation curves. Now, how much weight you want to put on that, it's going to be debated, I can guarantee you.

NELSON: As will all things like analogues, which the Board has always supported.

Okay, thank you very much. Very interesting. Our next presenter is the often referred to Mark Board. Mark got his Ph.D. from the University of Minnesota, and worked in the mining and consulting industry in the Western U.S. for about seven years before joining Itasca, and working with the mining and geological engineering industry for the last 19 years. He's been with the project since September 2001. So, anything done before that is not his fault.

BOARD: Thanks, Priscilla, for that vote of confidence. I hope my voice makes it through. I lost it last Wednesday. I don't know what's going on, but I'm having a little trouble.

Anyway, what I'm going to talk to you about today is work we've done over the last eight or nine months, or so, to look at the stability of the emplacement tunnels under both seismic and thermal loading.

The objectives of this work that we've been doing, it's a bit different than things that I think have been done before, that we've attempted to do before, and that typically in trying to look at seismic damage to tunnels, or things like that, it's always been in much more of an empirical sense about whether the damage levels would be major, minor levels, things like that. But, unfortunately, the requirements that we've had is that we're actually attempting to calculate a bit more detail about how much rock might be displaced, what the size of the pieces of rock are that might be displaced, because our calculation enter directly into the estimate of stability of the drip shield that is going to be placed over the waste packages in the postclosure time frame.

So, all our goals here really were to produce, as I put on the top here, a geologically based estimate of the distribution of rockfall for the lithophysal and non-lithophysal rocks as a function of the ground motions that we've been supplied by the people that you've heard talk earlier. So, basically, our input are those ground motions, and what we're attempting to do is to understand how the geology of the site affects the ground motion that we get.

What we're really aiming at is an estimate of rockfall. I've given you some of the items that we're attempting to calculate here, or estimate, and that's total tons of rock that might be displaced per unit length of the tunnel, the distribution of block sizes and masses, and what types of velocities they might com from the host rock mass at. So, it's a pretty steep, I think, requirement of calculation, and I hope you keep that in mind when we look at the results.

The other thing I'm also going to talk about is determining the impact of thermal loading history and time-related degradation on the strength of the rock.

Just to some of the people that we're working with, obviously we're attempting to take rock properties input, ground motion inputs, thermal loads, and our goal and again is to determine what kind of loading you might get over what time periods on the drip shield that covers the outside of the waste package.

The contributors here, I don't know if you can see them down there, but we've had a lot of people working on this over the last nine months, or so, from a wide range of organizations, from BSC, Itasca Consulting Group in Minneapolis, and you see the people here have contributed with the calculations. The Bureau of Reclamation and USGS has assisted us with the geologic description of the rock mass. Sandia has been working with us on testing here, Ron Price, Larry Costin, on estimating rock mass properties, and John Kemeny from the University of Arizona has also been involved.

The first thing I wanted to do is discuss the different types of rocks that make up the Topopah Spring and the repository horizon. As most of you know, there are two distinct types of rock that we're dealing with here. One is non-lithophysal, welded tuff. It's a typical hard, strong fractured rock with a uniaxial compressive strength for 50 millimeter samples of about 150 mega pascals. The modulus is somewhere around approximately 30 GPa, and its rock mass quality is in the range of about 60 to 70, for those of you who know what that value means.

This plot I think is very hard to see on this slide here, but what I'm trying to show you here is the Topopah Spring formation, which the proposed repository horizon will go through many of these different units. And in the center of this unit, we have the middle non-lithophysal zone, which is given this distinction Tptpmn, if you see that a little bit later. This is in the center of the flow and it's your typical hard jointed rock mass.

Above and below that, you have the upper lithophysal and the lower lithophysal zones in which the rock mass changes fairly dramatically and abruptly, and you have much fewer long consistent length trace length fractures. They become much smaller and you have porosity that occurs in lithophysal porosity, which is essentially a cavity in the rock.

I've shown on the side here the percentages approximately of where the emplacement drifts in the repository are found within that sequence. And, right now, the current design is in the lower lithophysal zone. We have approximately 80 per cent of the emplacement area of the repository, and about 10 per cent in the middle non-lithophysal zone, and then very minor amounts in other units.

So, by and large, the most important rock unit that the emplacement drifts are found in is this lower lithophysal zone, and I'll show you some pictures of what that looks like in a little bit.

The lithophysal rocks, in particular the lower lithophysal unit, have high porosity values of anywhere from 10 to 30 per cent in these cavity spaces in the rock. This is distinct from the matrix porosity which you can't see with the eye. Lithophysal porosity are cavities that are typically on the order of 10 decimeters, things of that size. They can be up to over a meter in size.

The rock strength of this unit of the testing that we've been doing is around 7 to 15 mega pascals uniaxial compressive strength, and it varies by porosity. And the modulus is ranging somewhere on 5 GPa, again porosity dependent. This plot, in color you can see it much better if you've got color slides, but it just shows what happens with the long trace length fracturing, and the lithophysal porosity is a function of distance across these four units.

Where we have high lithophysal porosity in the upper lithophysal zone and the lower lithophysal zone, we have very low density of long fractures. When you cross over into the middle non-lithophysal zone, which you see here, this is fracture frequency in red and fracture per 10 meters, we suddenly get a jump. It's much more highly fractured, with longer trace length fractures. So, we have these two distinct rock types.

In our analyses, it's very obvious that we have to look at these two rock types differently in our calculations of stability. Currently, the ECRB drift and also portions of the ESF pass through all of these different rock types. Currently, under the conditions of excavation and stress in the mountain, the drifts are quite stable and nice. There's no stability issue at all.

In the middle non-lithophysal unit, there are very few key block wedge type failures that have formed, especially in the ECRB, which is 5 1/2 meters in diameter. So, right now, there's very little failure that we can see.

It's obvious, though, that in the non-lithophysal rocks, the rock mass response is largely going to be controlled by the jointing or the fracturing, since the rock in between is quite hard and strong. In the lithophysal rock, on the other hand, the lithophysal cavities themselves have an impact on the strength of the rock mass, and we have to account for those in our calculations.

The first thing I'd like to do is show you the approach we're using for the non-lithophysal rocks, which are the jointed hard rocks. We felt that it was very important here to get a very good understanding of what the fractures are in that rock mass, and how they occur, because we feel that the fractures, and our conclusion thus far is that the fractures themselves control the size of blocks of material that can be released under shaking or under thermal loading.

So, the first thing we did was take this very extensive fracture database that's been developed over the years. The U.S. Bureau of Reclamation primarily, and the USGS, did a tremendous mapping campaign when those tunnels were driven. We have a fracture database of observations of over 35,000 fractures, which I've never seen anything like that in my career here where you've had such a detailed examination of fractures, their trace length, their roughness, their orientations, and all that.

What we have to do is we feel that we have to do a numerical analysis primarily to examine the size and range of rocks and the variability of the rock mass, rocks that can be released under seismic loading. So, one of our first goals is to create a statistically equivalent and geologically realistic rock mass that we can use for our numerical modelling. And we're using this FracMan program to do that. It's a program that's typically used in the oil industry to describe fractures and their distribution within a rock mass.

It's important here, in that the joints in this rock are highly discontinuous in nature. The average trace length of these joints is somewhere on the order of a couple of meters, something like that, but it's less than the tunnel diameter, and you very often see fractures start and end in solid rock, or they will stop up against another fracture plane. So, they're non-persistent joints, and it's not the typical blocky rock mass that one might see in terms of a rhyolite, or something like that.

That has real implications on the stability of the middle non-lithophysal unit. In particular, I think it controls to a great extent why you see very little failure, key block failure in those tunnels right now.

So, we use the FracMan to generate a statistically representative rock volume around the tunnel. We've been doing direct shear testing and field testing on joints with the Bureau of Reclamation to get fracture properties. We feed those into this three dimensional discontinuum model called 3DEC. We feel that this is truly a 3-D problem. It's not something that lends itself well to two dimensions.

We have been using the time histories that Ivan talked about as input data. We've been using all 15 of these different ground motions to drive this model, and we're estimating a rockfall distribution at each annual exceedance level that you might get from that.

This shows a great picture just to show you what we've done with this FracMan program. We've got lots of data from both detailed line survey, that's what the DLS stands for, as well as full periphery geologic maps. And the first thing that we wanted to do is make certain that this FracMan program, which is this statistical joint generator, is what it is, reproduces the right kind of distributions of set geometry spacing, trace length, things like that, just to show some contoured plots that show that the program can quite nicely reproduce that data.

What we've done with this FracMan program is we've produced a cube of rock that's 100 meters on a side, that sort of is our representative rock mass that we're using. And this just shows a very cluttered picture. It's difficult to see it here because the fracture density is so great at this level.

But we've generated this rock mass, and then what we do is we've used a Latin Hypercube sampling technique to apply one of these 15 ground motions, actually I've got 16 here because we substituted one ground motion in this case, we select a random location of a tunnel central within our 100 meter cube at which to drive a tunnel so we can get a completely different joint distribution every tunnel that we have, and then we apply one of these 15 ground motions to that analysis and examine what kind of rock mass failure that we get, what kind of block distribution from that model.

We've conducted for each annual exceedance frequency, and we looked at three levels thus far, we've looked at the 5×10^{-4} preclosure case that Ivan talked about, the 10^{-6} and the 10^{-7} cases. I don't hope to understand the statistics here, but we're using a base case of 76 representative cases that I've been told will give you a reasonable distribution of responses. And, to me, that's a lot of analyses anyway. We've actually conducted in excess of 100, because what we did we conducted 76 representative cases with a given set of fracture properties. Then we went back and did a sensitivity study to examine the effect of joint dilation roughness, friction angle, our estimate of cohesion on the joint planes, to see how that impacted the results that we calculated.

This just shows a model, a representative of our 3-D model. In this case, I removed the rock mass outside the tunnel. This just shows, for a particular realization, that we had the type of blocks that you form. You have the tendency to form fairly high angle, spiny block structures here because we have two sub-vertical joint sets that are quite smooth that are striking, one north, slightly west, and the other north, slightly east, and we have one set of anastomosing vapor phase alteration structures that are sub-horizontal that are very rough. They've got roughness values of Barton, roughness values of up around 16, or so, and dilation angles of 13, 14 degrees.

They're very rough and they've got tritomite and crystobalite on those joints, and, so, if you look at them in situ, they've got these anastomosing fingers like this that make them very difficult to shear. But, those are our sub-horizontal sets.

So, what we do is we actually put in a block in our model here that's fixed to the invert that represents the drip shield that we have. We essentially shake this with one of those ground motions, and we just simply add up and calculate how many rocks fall out. So, it's a pretty simplistic approach, I guess.

What we do is we do enough analyses that we can generate some sort of distribution curve from it. And then, as I said, we go back and look at variations in things like dilation. Our base case is extremely conservative in that we've assumed planar, zero dilation joints, which means that they can fall out quite easily.

We've also, although I'm going to show the seismic results, we also thermally load this tunnel, and we've examined what impact thermal stresses alone can have on the failure of the material, and also thermal and seismic as well.

One thing I'll point out that we did make some changes to this. This 3DEC model was developed by Itasca back I think it was around 1984, or so, and it's since become a very I think popular and standard type of program to look at discontinuum problems in three dimensions. One thing that we did do to change it is we put in some logic to be able to examine partially penetrating cracks. I know Peter is well aware of the old version of the model. The joints had to be completely penetrating through the model, which means that we would end up cutting this thing up into many, many small blocks, no matter what the trace length was. Trace length didn't come into it.

We made changes in this to be able to have fractures that partially penetrated the rock, and would be bonded with a solid rock bridge that had a given strength beyond that. We looked at all ranges. We said okay, what if we throw the rock bridge out, even out completely, and just say, you know, we're going to let it fill wherever we went, or wherever it wanted to go along those joints, and looked at all those possibilities.

The output we have from the model when we shake it, is that we give our stuff directly over, our results directly over to the drip shield calculation people. So, what we did is we wrote a little algorithm to be able to determine the contact location on the drip shield as a function of time, and to record the block mass and shape and velocity as it hit the drip shield. You can see some results here from the 10^{-6} calculations, where the blocks actually exited the site of the rock mass and hit the side of the tunnel.

Pretty much, you see that you can get impact locations from all directions, not just from the crown in the tunnel, but also from the side walls.

One thing that we did also, which is quite conservative, is that when a block would exit the back and hit the drip shield, we would delete the block and get it out of the way. In reality, if they started coming out and piling up on the top it would prevent blocks following them from coming. But we decided we wanted to determine all the block sizes that could possibly come out, so we deleted those.

This is what the results look like. It's quite interesting. It's the first time I've ever done a study like this, and it was quite interesting, the results. You get essentially a negative exponential distribution, which you would expect because of block sizes generated by these joint planes that has roughly the same shape no matter what the exceedance frequency of the events are. You can see obviously for the 5×10^{-4} case, there are much blocks than you get for the 10^{-7} case. But the general shape of that distribution is roughly the same.

This is the total number of blocks for those 76 base simulations that I talked about. The median size of the rock block that we get generated in the middle non-lithophysal unit is about a quarter of a tonne, so it's quite small. The rock has a density of 2 1/2 tonnes per cubic meter. So, that can give you some picture of the size of these blocks, and it's consistent with the key block sizes, the few that we do see in the tunnels. That's consistent with the size that we get.

Basically, what this curve shows to me is that the rockfall is largely controlled by the block geometry, by the joint geometry, and that's why it makes it so important, I think, to make a reasonable attempt to actually represent the true geometry of the fractures in the model that we're doing. I don't think we can just simply go out and make a few measurements of strike and get orientation, average orientation of joint sets, and hope to come up with a statistically relevant curve. We actually have to do our best to try and model the statistical variation in the fractures, and that's why we spent so much time doing that.

What we found out is that really, the only other important parameter, as you can imagine, that seems to be important for this is the dilation angle of the fractures. It turns out that if we assume only just a few degrees of dilation, it's in many cases very difficult to shake some of these blocks out. And you see the same shape of the curve, but it reduces down in number

So, what you're seeing is quite conservative. And we did find out, too, under thermal loading, I don't think anyone has talked about it, but the thermal load, the stress and the temperature both reach a maximum only about 20 years after the closure of the repository. After the ventilation has shut off, the temperatures peak at about, in the simulations we're doing, at about 135 degrees at about 70 years from first emplacement, assuming a 50 year ventilation cutoff.

So, the reason I point this out is that then the thermal load decays over the next few hundred years, and, so, really where the thermal load mixed with seismic only comes into play is very early on in the game after it's closed out. After that, you're pretty much back to the original in situ conditions.

And what we find out in the middle non-lithophysal unit is the thermal load actually almost eliminates rockfall from occurring. The reason for that being rock expands and it puts normal stress on these joint planes, so it locks them in place. Then, of course, the thermal load decays and runs back to the same particular thing that we have here.

The lithophysal rock presents a very much different issue. I'm sorry these slides don't come out very well. It's too bad we couldn't stay an extra day or so and go underground where we can take a look at this stuff and show you in detail what it looks like. But, the lithophysal rock--

NELSON: Mark, you're 20 minutes into the talk.

BOARD: Okay.

NELSON: So, take about the next ten.

BOARD: Okay. The lithophysal rock is high porosity again. Block size in this lower lithophysal rock is really controlled by the lithophysal spacing, these void spacings, and the inter-lithophysal fracturing that we have.

And in the lower lithophysal unit, this rock is very highly fractured, and as Jim pointed out--we know that these fractures, and I don't know if Dave Buesch is here or not, the geologist, but anyway, he can talk about it in more detail, but the majority of these fractures in the inter-lithophysal--or inter-lithophysal fractures, have vapor phase alterations on them, which is al alteration product that was formed during the cooling process of the material. So, we're quite certain, and we know that these fractures are cooling fractures, and they also are relatively weak. And whenever we try to drill core from this material, or anything, we find that this breaks into small blocks. And, so, we're quite certain that when this material does yield, it's going to form blocks that are very small in size, on the order of inches to a foot type size, and they aren't going to be forming large blocks.

Now, how do you model such a thing and try and determine how much of that material might come out? There's two basic modelling approaches that you can use. Well, first of all, there's a straight empirical approach. And we did start with that, trying to use things that people have learned from the mining industry, subjected to seismic events, underground openings.

Since we had to get a little bit more detail than that, we're trying to understand the properties of this lithophysal rock a bit better. The problem with the continuum model is is you develop a constitutive relationship, and then you calculate what materials are actually yielding or failing around the outside. But the problem with that approach is it's difficult to estimate how much of that material actually dislodges and comes off the side walls.

And, so, we've developed a method. It's actually similar to this. It looks much different, but it really isn't. We take the discontinuum program, which means that all these little blocks can break away from one another, and we give properties to the fracture plane such that it mimics the behavior of this model, and we calibrate it against our laboratory tests.

We've done a lot of laboratory testing. The first thing we do is calibrate the model against that. We estimate the block size distribution simply from fracture density, and we can go ahead and shake it again just like we did the other one.

I'll just pass very quickly through this. We've done a lot of testing work in the last year to try and estimate what the properties of this material are. It's obviously a function of the size of the sample because of the large lithophysal porosity. This is a 12 inch diameter core. We've done some 1 meter plus sample size compression experiments underground where we've measured the strength properties of the material. We've actually driven these blocks to failure under various load tests.

This just shows an example plot of uniaxial compressive strength against Young's Modulus. But, it shows that all these samples that we've done tend to fall on a range that looks like this. The lowest porosity samples are up here. The highest porosity samples here. These are in situ tests down here. What we decided to do, because we have as few samples as we did, is to conduct calculations across this entire range of values rather than try and do some statistical mumbo jumbo here, which I didn't think we had enough data to do. So, we just decided to look at the entire range, and see how robust the calculations we had were across the entire range.

This just shows a calibration. What we did is take our model, our block model, and we actually calibrated it using numerically generated tests, and compared them to our lab measurements.

This just shows an example of what a sample looks like in our numerical model as we compress it in uniaxial compression. And in uniaxial, it forms axial splitting failure mechanism, which is what we see in the lab, to give us some confidence that we're doing what we think we're doing.

We've also verified the model against the drift scale test, which was a heated experiment that was over driven, and it produced back-parallel fracturing in the roof at year 2000, when the rock temperature hit about 185 degrees. We found out that we can quite nicely reproduce that mechanism in our model here, it spontaneously produces fractures.

Just to go over very quickly what the thermal drift degradation analysis shows, we ran many scenarios where we looked at 50 year ventilation, shutting the ventilation off, reaching temperatures of 135 degrees, as I mentioned earlier, and examining what kind of failure mechanism you'd get.

What we see here is at 50 years when the ventilation is shut off, we initially have some side wall failure in the tunnels, and we actually see this underground. This is a pre-existing condition. We see some slight spalling to a shallow depth around the springline of the tunnels underground.

At the peak temperature at 70 years, what happens is is essentially that that previously spalled material tends to simply slough off, and we get this kind of behavior, with a small amount of rockfall from the springline. It essentially stays that way. As the temperature decays, we don't get any more failure in the model.

From a seismic standpoint, we've run these cases, as I mentioned, 5 x 10^{-4}, 1 x 10^{-6}, and 10^{-7} exceedance frequencies, annual exceedance levels. The 5 x 10^{-4} is a preclosure earthquake. We examined unsupported tunnels, and essentially we get very similar to what we see the thermal analyses when we shake it. This is with about 2/10ths of a g, and about 190 centimeters per second velocity. Essentially, that pre-fractured material that we see currently underground just simply sloughs off under the shaking.

At 10^{-6}, this very large event that we've been talking so much about that we think is excessively conservative, we essentially see that the rock mass tends to fail in tension. When the compression wave passes, we get a tensile portion of the wave, which tends to fail this rock mass all along the fracture planes and tension, and it just simply drops by gravity and cover the drip shield. We essentially see this same behavior for 10^{-6} and 10^{-7} events, and it forms a dead weight load on top of the drip shield, which we've calculated then from the dead weight load of the material.

We find out that we get this same level of damage essentially for any one of those--for that entire range of rock properties that we've shown, and the reason being that the event magnitude is so large in comparison to the strength, that we get the same essential response.

I wanted to point out that the damage levels for these low probability events don't appear to be consistent, as Jim was pointing out, and others, with the observations that we have underground. This would be what would be good about an actual trip underground, is because you can see that these lithophysae are essentially undamaged since they've been laid down. Some of them are in excess of a meter in diameter.

We see sort of delicate structures within them almost looking like mud cracked types of structures that were formed from expansion of the material, and then when it contracted, it formed these sort of mud crack looking structures, some of which have actually fallen out with time, and caught behind the wire mesh that we have down there. But, there was no evidence that they've been disturbed prior to that. We also see no evidence of shearing or extensional failure on any of the lithophysae, or the joints that form this fine fracture network.

Okay, my last slide then is that the results that we see in the non-lithophysal rock is a median rock size of about a quarter of a tonne, and there's a relatively small rock volume that falls off, although I did not show them. The total volume that falls off is really not tremendously significant in the non-lithophysal rock because of this interlock nature of the rock mass.

In the lithophysal rock, thermal stressing in the postclosure time, we see a small amount of rock displaced. We have not looked at the problem of time dependency yet, so I can't tell you that. I suspect that's more significant in the thermal loading, but we're right now doing testing to look at the effect of static fatigue strength of the materials, so I can't give you any information on that.

Approximately the same amount of damage from loosening of the springline and unsupported conditions in the preclosure. That's something I believe that we can easily take care of just with typical rock support that we would use.

In the postclosure, because of the large ground motions that we're currently dealing with at 10^{-6} and 10^{-7}, we see significant damage.

So, that's it.

NELSON: Thank you very much, Mark. Boy, is this a moving target trying to keep track of all the things that are going on.

BOARD: Quite a bit of stuff here.

NELSON: And really interesting. Questions? Art

McGarr, and then Peter.

MC GARR: McGarr, consultant.

Mark, it seems like the horizontal stresses are quite important to the outcome of these calculations, especially since the lithophysal zone is deeper and would be under greater, more compressive stress, which would tend to stabilize it, as you pointed out earlier. What sort of stresses have you taken for your model?

BOARD: The stresses have been measured by a couple of different researchers over the past. One was Mark Zoback, and somebody named Stock, who I don't know, from the USGS. They did a really nice study back in the early 1980s, did hydraulic fracturing, measured borehole break-outs, and things, on the site. And, also, Sandia Labs measured in situ stresses from within the tunnels out there. And basically, what you have is the vertical stresses maximum, and at that repository horizon, it's about 7 mega pascals at about 300 meters depth. So, it's just, you know, a weight thing.

The minimum horizontal stress is about north 115 east, and it's about 3 mega pascals. So, the ratio is about seven to three, max to minimum. So, that's about .3, .4, something like that. In the intermediate stresses, about 6/10ths of a vertical stress. And I do have some confidence in that, because the measurements by Zoback and Sandia both gave very similar results, and the borehole break-out at depth I think also lended some credibility to the direction now that we have.

But, you're right, it has a big impact on it, because we've got a high stress ratio. The normal load in the crown, the confining load, if you want to look at it that way, is very small. And that's why you have some fracturing that we see at the springline down there in this weaker rock, is because the stress concentration of the springline is much more significant.

NELSON: Okay. Peter?

KAISER: Kaiser, consultant.

First of all, I have to say it's nice to see that finally somebody addresses the issues which we thought should be addressed in '98. I'd like to start at the back, maybe the slide just before this one, but the second figure shows that there is cracking. I assume that's all cracking. Have you had any samples--

BOARD: Yes.

KAISER: --that that kind of earthquake destroyed the rock mass?

BOARD: Yes, just the free field motion that you have is sufficient to actually fail this lithophysal rock. I think that's the first thing that we looked at, and said these motions are very high and they don't necessarily fit with what we observe underground. And that is that--yeah, that's the response that we get.

KAISER: So, indirectly, the analysis is proof to the type of questions that were addressed earlier about if there events of this magnitude, the rock mass should have persistent fractures?

BOARD: I would think so. I would think not only just persistent fractures, but the lithophysae and solids I think would show damage in some form, and we just don't see that.

KAISER: Going to the Overhead 9, you show the impact location from the block falling?

BOARD: Yes.

KAISER: Did you do any study on what velocities those blocks are coming out? Are they just gravity?

BOARD: It's just gravity, yes. What we did is we determined--we essentially gave these people doing the structural calculations, I think you'll see some of that next, we essentially just, as a function of every time step, we monitored all those potential locations around the drip shield, and we determined what the impact sites were, and for each impact site, we recorded the block number so we could get the shape of the block, but also the mass and the velocity of that particular block. And then we also determined the distribution of forces at the impact location as well, and we fed that information off to the people doing the structural calculations.

KAISER: So, there is no momentum transfer?

BOARD: We assumed that this drip shield, it's quite conservative, it's a rigid block. And, so, that's the reason we gave mass and velocity of that block, was to let them do the--

KAISER: What I meant is there is no momentum transfer between blocks that causes high speed impact?

BOARD: You know, I don't think we saw much of that. We made movies of this stuff, and when you see it shaking, all these things just essentially are falling out. The one place that that is different is is that for these blocks along the side wall down in here, some of them actually get kicked out. I think you made the observation back in '98 when you were out to the site for that ground control workshop, and that was that with these high angle joint planes, that a large number of potential key block wedges would actually be formed in the side walls. And, in fact, they do. Those are the ones that we actually see come out. But the velocities are quite low.

KAISER: Last question. On Overhead 10, you showed the distribution of number of blocks and block mass in tonnes. First of all, the number of blocks, is that per meter or--

BOARD: No, that's actually the total number for our 76 base calculations. So, each analysis, each model that we had was 25 meters, or five tunnel diameters long, the axial length of the model. And, so, I didn't divide it up here, but you could express this, I could sum up the total mass, express it as, you know, tons per kilometer, for example, which we have done, but I don't have that here. But this is just total numbers.

NELSON: I'm following up on that. Would you be able to get an idea of the variability of the rockfall across your tunnels in all of your various realizations?

BOARD: Yes, I think that was a very important part of what we tried to do, is capture how variable it might be by taking those 76 different tunnel locations within that mass.

KAISER: The last part to that question. You talked about that you did these with rock bridges?

BOARD: Yes.

KAISER: Or this is without rock bridges?

BOARD: No, this is with.

KAISER: Oh, this is with rock bridges.

BOARD: We also did them without. We just assumed the joints were fully penetrating and ran it as well, and I don't recall off the top of my head how that affected the distribution or the shape of it. But, I don't think it affected it too much. I think the distribution pretty much looks the same.

KAISER: Thank you.

NELSON: Dan?

BULLEN: Bullen, Board, just a couple quick questions.
You mentioned that thermal load decreases the rockfall in the lower lith basically because you locked it in?
BOARD: Yes.
BULLEN: That the peak occurs at about 70 years post-emplacement with a temperature of 135 degrees C.
BOARD: Yes.
BULLEN: After the thermal pulse, do you get an enhancement in the rockfall because you had the thermal pulse, or is it pretty much the same?
BOARD: It's pretty much back to the same it was, and that is because we're not actually failing or yielding, with the thermal load, we're not yielding those joints. So, in other words, there's not any permanent set, you know, a hysteresis in the response where it goes up and then comes down to a different state. It pretty much--it comes in a slightly different position, but as I recall, I don't think it's particularly different.
BULLEN: Is that the same as you see in the repository now in the drift scale test?
BOARD: In the drift scale test, yes. Yeah. Other than when the test was over driven specifically to cause that back fracturing, it's now cooled for how much time? It's over a year now, I guess, and we've seen no rock fall-outs, no

changes in that year.

BULLEN: But that happens to be in the non-lith, not the lith; right?

BOARD: Right, that's in the middle non-lith, which is what we were talking about here. In the lithophysal rock, which I did not show, you get slightly--it's a different effect because it's uniform. You get slightly more fill. You're thermally loading. It doesn't tighten the rock up. It doesn't reduce the amount of failure. You get slightly more because it's a different failure mechanism. But on cooling down, it's hard to say, I'll have to say, because our constitutive model that we've developed, you know, it's a little difficult to say on the hysteresis because we have dilation effects when the material starts to fail. I believe that it's not going to have much impact at all. From our results, it certainly doesn't show that. But I think at some area, we have to investigate a little bit more.

BULLEN: Okay. Last followup. Figure 14, please?

You show in this lower left figure basically the springline failure. And I guess--

BOARD: No, that's actually--

BULLEN: Lower left, yeah.

BOARD: No, that picture is one of those--but it looks similar to that, though, yeah.

BULLEN: And is that driven thermally, or is that just--

BOARD: You mean in the lower lith?

BULLEN: Right. In the lower lith.

BOARD: Yeah, in the lower lith at a depth of about 300 meters and below, we see occasional sidewall spalling right now that looks somewhat similar to this. You get free parallel fractures that extend to a depth of about 18 inches, or so, into the rock, and it's a typical sort of fracturing parallel to a free surface that you see in mines. This doesn't happen to be that. This is actually in our tests that we did in the upper lithophysal unit in the compression test.

BULLEN: Okay. Does this type of failure suggest that maybe you will need more ground support in the lithophysal regions?

BOARD: You mean than what's currently planned?

BULLEN: Well, the ground support seems to me to be sort of evolving. So, what type of ground support do you expect to see?

BOARD: It's not something that is particularly bothersome to me. I mean, having looked at--it's a typical sort of thing you see at shallow depth of fracturing in the side wall. I think it's interesting from my standpoint only that it makes a nice calibration level that we can compare our models to to make sure that we have the proper kind of strength range.

In the ground support, when we're talking about the whole preclosure time frame, I believe we'll have to bolt the walls, yeah, which isn't done right now, just to make sure that this material is maintained. Because it's pretty obvious that the failure mode that this stuff will undergo is going to be a ravelling type of a failure mode, different than in the non-lithophysal rock, which is a T-block type of a thing. And, so, we're going to have to control that, and so the ground support methods you use in that ravelling type failure method are different than what you'd use there. We have to have a surface type support method to do that.

BULLEN: Thank you.

NELSON: Richard?

PARIZEK: Parizek, Board.

Figure 9, you have some hits on the bottom of the drip shield. That looks bad. It either came right through the drip shield or--

BOARD: What that is is that the drip shield, it actually starts a little ways back into the tunnel, you see here, and actually the block that's bounced out and then comes back--

PARIZEK: Okay. Well, I stand corrected. On the pages that show failure 50 years and after shutdown, or 70 years later, you have a lot of rock falling.

BOARD: The thermal?

PARIZEK: On the drip shield; right.

BOARD: Yeah.

PARIZEK: And then this is after ventilation, peak stress change, and so on. This is what the models predict, all that rubble?

BOARD: Excuse me?

PARIZEK: All that rubble on top of the drip shield is what the models are predicting?

BOARD: Let me look at what you're looking at first. Which figure is that now?

PARIZEK: It's on Pages 20, 21, 22.

BOARD: Oh, keep in mind that this is run without any ground support in place. We've run this as a purely unsupported tunnel. And what we predict is that a small amount of material will, as you thermally heat the thing up, the thermal stresses here that are generated by this 135 degrees in the lithophysal rock is quite low because the Young's Modulus, the material is quite low. But, yes, we predict the occasional surface materials falling off. What we are predicting there I think is similar probably to what you'd see in that drift scale test with the roof fractured off. And if you look right now in that test on that tube that covers the heater that's done there, you will see small pieces like that that are resting out on the tube or fall on the flow.

PARIZEK: I have a question then to follow-on. From a corrosion point of view--I just, if you have that rubble starting to accumulate on a drip shield, then it raises a question about corrosion and moisture at contacts, is what I would think. And then the other question is whether you can get data out of places like the Nevada Test Site with miles of tunnel and all kinds of rock type to sort of see if this is really how it works years later, 50 years, 20 years, 30 years of tunnel exposure.

BOARD: That's a very good point. I think Peter brought up a point earlier, too, that what we really need to do is we've got probably some really valuable data from the test shots at the site, that we have not compared these dynamic models to, and I realize it's not the same type of ground motion shaking from a seismic event, but one thing we should do is compare some of these models, because there's no reason we can't compare these to that kind of damage from the shots at the site. In fact, we have compared some before in the work we had from the Defense Nuclear Agency, but not as part of this particular project, and I think we should do that.

NELSON: Okay, I get to ask one question. With all these new tools and new understandings that you've evolved over the last couple of years, are you going to revisit the spacing of the drifts in the footprint?

BOARD: You mean make it smaller? I think that spacing was generated primarily from thermal and hydrologic concerns, not from stability concerns. And, so, if I'm not mistaken, Bill could probably clue in better than that, but it really wasn't a stability issue at all. Sure, I mean, from a stability issue, those things could be pulled way, way in, because they're essentially so far apart now that they're completely non-interactive. The drifts are 5 1/2 meters diameter, and they're spaced at 81 meter spacing, and, so, one drift doesn't know the other one even exists.

NELSON: right. But the field tests that you've gotten have really made it clear that there are other properties of the rocks than have been assumed in terms of the thermal conductivity and some of the other.

BOARD: Oh, there's certainly, from a thermal conductivity standpoint, right now there's calculations going on to examine for that range of conductivity as a function of porosity that's going on. You know, I think just regarding that there's lower thermal conductivity because of the lithophysal porosity. I think that's definitely going on.

NELSON: So, this isn't really directed towards you so much as to Bill, is that I don't think we ever really, the Board ever really understood why 81 meters was the right number. And now that the rock properties have changed and our understanding of several other things have changed, the question as to why it's the right number remains.

BOYLE: William Boyle, DOE.

I think Mark was exactly right. I think the spacing probably came out of the study of the license application design selection, and it certainly wasn't for ground support issues. It was hydrology and thermal issues. And I think with respect to the thermal conductivity values, it all depends on which values in that range you want to look at. I think there are some people on the project that would believe that the thermal conductivity values really haven't changed that much through the years, or as a result of recent measurements.

I mean, you can develop different models that take the lithophysae into account in different ways, but most recently for the calculations we did in the Supplemental Science and Performance Analyses, we looked at the effect of how to handle the lithophysal porosity, or porosity in general with respect to thermal conductivity, and although models show it has a pronounced effect, we don't find those effects present in measurements in the field, that there's a more restricted range, is what we see with measurements.

BOARD: I think the bottom line, Priscilla, is that the stuff that I've seen recently, and somebody is probably going to shoot me for this, but the temperature difference based on if you look at the mean levels of porosity, don't change very much. I believe that the temperature predictions only vary by something on the order of 10 degrees, or less, I believe, assuming the highest porosity level of the lithophysal unit, and based on the in situ tests that have been done to measure thermal conductivity. It wasn't a huge difference between the current prediction and if you assume some of this new variability that you're talking about.

BOYLE: I'm not even aware of the calculations that Mark is referring to. But, it corresponds to, I think, my remark, or what I hope my remark got across with respect to field measurements. We do have in situ measurements of temperatures, which reflect earth's geothermal gradient, and it reflects all the lithophysae present, where there is a lot of them and a little of them, and there are no distortions in the temperature measurements people have made, you know, indicating that the lithophysal porosity or presence or absence of it is distorting the thermal conductivity such that it would greatly change the temperature measurements in situ.

NELSON: Okay. You're off until the panel.

Okay, our last speakers before break are Michael Anderson and we're going to hear again from Mike Gross. Michael Anderson has been with the project since June of 1997, and he's been managing the design of the waste packages for the repository since April of 2000. And we welcome.

ANDERSON: Thank you very much.

As you know from the agenda, Mike Gross is on, too, so you get two presentations for the price of one.

Happily, though, a lot of things that he said already in his introductory discussion are things I was going to say anyway. So, we're going to try to accelerate through this.

We talked about the dichotomy that you've seen before where we look at segregate vibratory ground motion for both the waste package and the drip shields, and look at rockfall on the drip shield separately.

Again, the representation of the vibratory ground motion seen in at least one of those acceleration time histories, and the way we decide where do we start and end our simulations is we look at that part of the ground motion after the first 5 per cent of the total energy in the wave form occurs, up to, say, 95 per cent of the total energy content, and then we stop. The vibration at that time is usually low magnitude, has little effect on the final results.

We represent the deformation process, and I'll show you some analysis representations in a little bit, and we'll talk more about that. Generally speaking, the simulations are run 15 to 30 seconds. So they are computationally intensive.

In general, deformation is localized with contact regions, whether it's the waste package on the pallet tiers or impacts between adjacent waste packages, or, say, the drift wall or the top of the invert.

I think this has probably been pretty well covered by Mike Gross. I would say that with regard to friction coefficients, we do have separate samplings for metal to metal and metal to rock contacts.

We use typical mechanical properties. We believe it's a good assumption that those effects are small compared to the acceleration time history variability and the friction coefficients, and Mike has already talked about that.

Next point there, as is the next one, about 150 degrees C. Finally, a note for those who have seen a nuclear power plant seismic analysis, we use no system damping. Certainly the regulatory fractions exist for elastic analyses, but we have an unanchored structure here, and it's rather challenging to come up with a defensible definition of critical damping.

Very early last summer, almost a year ago, we looked at some initial simulations we did with some ad hoc accelerations. We found that by and large the problem was divided into two acceleration ranges. If it's less than 3 g's, you see that most of the effect is a hammer and anvil effect between the waste package and the emplacement pallet. You have repeated impacts on the same location. There's very little waste package interaction and there's very little effect on the drip shield either.

The higher ground accelerations, you still have this hammer and anvil effect, but what happens is that there's increased rigid body motion. We see more interactions among the waste package, drip shield and even the drift wall.

This is a finite element analysis representation of the waste package. This is just representative, depending on the severity of the ground motion, these do change. However, you can see that we finally meshed this region where you would expect interactions with the pallet pierce. In some cases, especially for the low ground motions, we can represent part of the waste package as rigid. When we get into more ground motion where there's more rotation, we have to make the whole waste package elastoplastic.

Also, these regions tend to increase in size, both around the circumference and along the axis of the waste package in order to capture the hammer and anvil effect.

For the drip shield, this particular representation shows no waste package inside of it, and that's appropriate from what we've seen for the 10^{-6}. When we get to 10^{-7}, we have to put a representation in here of the dynamics of the waste package and the pallet that it rests on. What we're really looking for is this drip shield in the center. We have these adjacent drip shields that are meant to represent the effect of all of those waste packages in the line, and constrain it. You can see the fine mesh in here to try to pick up residual stress by the ground motion. Also, we're looking for separation between the adjacent drip shield segments.

For the waste package as a whole, when we focus in on its damage, you can see the waste package resting on its pallet. You can see that the drift has been cut away here, and there's an end plate here and here. That represents adjacent waste packages, and we conservatively model those or represent those as rigid bodies, so that the waste package hits this rigid surface rather than another waste package that may be retreating in phase with the one that's represented here.

You can see the drip shield is there to give the proper dynamics of the drip shield. We don't have the adjacent drip shields there, but at the moment, we think this is a defensible assumption because all of those should be moving in concert with one another also.

These are the results, and of course I have to repeat the mantra that this is all preliminary and unchecked. This for 10^{-6}. This is quite an eye chart here, and I hope that you can read it much more clearly from your copy there.

What you see here are realization numbers, and that's the information that we received from the science folks that provided this to us. It's in no particular order with regard to the total energy content or the severity, but it does give us traceability back to the source.

In general, you can see that for 10^{-6}, we get much more damage from this waste package to waste package interaction than from those with the pallet. In general, the total stress area, or total area on the waste package-- anyway, what we have here, as you remember from Mike's presentation, we have 8 per cent yield, 90 per cent yield strength that we're using as a threshold, and we report the areas that are above those thresholds in terms of both square meters on the surface, and down here as you see these percentages are a percent of the total outside surface of the waste package. For 10^{-6}, we have results that are less than 1 per cent of that total area.

I should make one other comment about it. Some of these areas, particularly in the impact areas, tend to be on the edge of the lid of the waste package. And, so, what you see is damage that doesn't necessarily contribute to eventual break-through due to accelerated corrosion.

Here you see again for 10^{-7}, the one interesting thing is here that the waste package pallet interaction becomes more comparable with the waste package to waste package interactions. In general, these results you see down here are less than 2 per cent of the total waste package surface area, or the outer surface.

For the rock fall, I think Mark Board has covered most of this, although I would like to say a couple of things about what we assume about the rock. It's a rectangular prism, and the center of gravity is located above the point of impact. Some of you have seen rockfall calculations that have been done in the past. They're based on previous understandings of the shape of the rocks. They tend to be very long, tetrahedral shapes, very long tails, and so that sometimes the center of gravity was not even above the target location on the drip shield.

This has the advantage of transferring the maximum linear momentum of the drip shield, and also the sharp edge on the rock tends to maximize damage. As far as the base of the drip shield, we don't constrain them except for a friction coefficient there. So, they're free to move.

This is pretty much what's been said before. So, let's go on to the next slide.

Again, this is what's been said before. Let me make one distinction between the seismic calculations and these rock fall calculations. The seismic calculations are very much dependent on the details of the acceleration histories.

For the rock fall, it's a singular event, and so as you saw from Mark's presentation, he gives us the location on the drip shield of the impact, the kinetic energy in terms of mass and the velocity of impact. And, so, rather than running many, many such rock fall calculations, instead, we've created a catalog of results that are a function of the independent variables here. And, again, it exceeds 50 per cent of the titanium yield strength.

Here you can see a finite element representation. Here's the drip shield, and there is the biggest rock that we've got, 14.5 metric tons, you can see falling on the center. There's that angular surface right at the edge there.

Here is the--you can actually see the finite elements. In this one, you can see where we've concentrated here below the impact point. You can see there where we're allowing it to slide.

These are the results for 10^{-6}. These are actual areas. The surface area, outside surface area, and the drip shield is I think 38 square meters. So, you're looking at, like, 10 per cent damage there from that largest rock in the catalog.

As Mark talked about earlier, you can see we have rock fall at different locations. That's on the corner. And here we have one ejected into the side wall. And as he noted, the energy is a lot lower, and so we don't accrue much residual stress from that.

And then finally, this is the 10^{-7}. Because we're creating this catalog, all we had to do was add an additional rock with higher kinetic energy that would cover the 10^{-7} results. Here, you have a little bit more damage than we saw in the last largest rock.

That pretty much wraps it up. As has been noted before, we are decoupling the ground motion and the rock fall from one another, at least at the present, showing you some results to date which are encouraging in terms of the amount of residual stress on the waste package and the drip shield.

And, with that, I'll entertain questions before Mike Gross comes up.

NELSON: Do you want to separate the questions?

ANDERSON: I think it might be wise, because Mike is really pulling everything together, including all the parts together. Do you agree, Mike?

NELSON: Okay, let's keep it tight. I don't want to run too late into our panel time. Go ahead, Ron.

LATANISION: Latanision, Board.

The first question may be trivial, but I'll ask it anyway. You've shown the drip shield to have a curved surface. Mark modelled it to show orthogonal shape. What am I missing here? I mean, does it make a substantial difference? I mean, the shape obviously will make some difference, but why have you chosen to do it differently?

ANDERSON: Well, we've done it in accordance with the actual design. Mark, I don't know if you want to speak to that.

BOARD: The reason we did it that was is just because it was easier. Our calculations, these calculations we're talking about here are really time consuming, and I think it doesn't make any difference in the calculations from the standpoint that our goal was to get the approximate vicinity of where it hits the drip shield. But, really, I think more importantly is what was the size of the block, is mass and velocity. And, so, I think from the standpoint of, you know, does it make much difference, I don't really think so, as long as we have the proper dimensions and velocity.

LATANISION: Okay. I have one other question. I want to return to a point I made earlier this morning, and that was the issue of the 80 or 90 per cent yield strength which is being identified as a criterion for failure. And I just don't understand the basis of that in the case of C-22, and I know Gerry Gordon is here now, so maybe we can call on him.

ANDERSON: Do you want to hazard a stab at that, Gerry?

GORDON: Gerry Gordon, Engineer, Systems Project.

The threshold stress for initiating stress corrosion is a conservative threshold. It's based to some extent on an ASME precedent for fatigue endurance limit, where there's a factor of two below the run-out stress on stress cycles to failure. It has a very similar shape to the stress corrosion stress time to failure.

We have data up to 220 per cent of yield for Alloy 22, which is about 95 per cent of the tensile strength. It's as high as you can load it. And it's run out to 11,000 hours, however, not stress corrosion. It includes crevice, welded, notched samples. We also have U vents that have been running at Livermore for up to five years in the long-term corrosion test facility and a range of environments. They're at or above yield by the nature of that type of sample.

So, we've run through very long times, relatively, not compared to 10,000 years, but up to five years, and up to 220 per cent of yield. So, we potentially could use the Code precedent and go down to 110 per cent of yield, half of the run-out stress that we have without failure. The 80 to 90 per cent just is--we really don't like to operate at the yield, because you're getting deformation. So, we just chose to be a little more conservative.

LATANISION: I understand the point you're making now. But it seems to me as a failure criterion, if the failure mechanism that is envision is stress corrosion cracking, and yet there is no evidence of stress corrosion cracking in representative repository environments, then it seems to me to be a very arbitrary failure criterion. I mean, there is no failure, at least as far as I can tell, in representative repository environments.

GORDON: Well, under crack growth conditions with fracture mechanic samples, I think I reviewed this with the Board a while back, where you fatigue pre-frac, and then you load to a given stress intensity, and you cycle it to get active stress corrosion cracks, under some conditions, you can--you won't go to--the crack will continue to propagate.

LATANISION: I think we should talk about this off line, Madam Chair, because there's really more to say about that.

NELSON: I appreciate that. Okay, Dan.

BULLEN: Bullen, Board. Go to Figure 12, please. And this is just sort of a follow-up, because you're talking about waste package to waste package interactions. And my memory of about a year ago when we had a more flexible design, spaced the waste packages about 2 meters apart instead of 10 centimeters. So, if you have 2 meter waste package spacings, would you expect any waste package to waste package interactions with these types of vibratory ground motions?

ANDERSON: I have no idea.

BULLEN: Is that analysis hard to do?

ANDERSON: It's just a matter of we have not done it. It would be a very time consumptive calculation. These are very time consumptive calculations as they are. Now, what we have done is for the baseline design for LA, so until we change that operating mode, then--

BULLEN: We're waiting.

NELSON: Thank you, Dan. Thank you, Michael.

And now we go to Part 2, the other Michael, Mike Gross, who is going to wrap it up.

GROSS: I'm going to tie this damage data here, it's convenient that you left it on, into the abstraction that we will probably go forward with it to TSPA. I completely approve of the higher yield stress barrier criterion, and you'll make my abstraction job much easier.

I'm going to skip through the first few viewgraphs. The next one just talks about the thickness reduction. You've already heard that twice. We've already talked about the thickness reduction. There's no new information on this slide. If you could please skip the failure criterion. There's also no new information here. Gerry is a better source than my viewgraphs. So, if you could please stop here?

This is a plot of the failure data that Mike just showed you. What you've got is a graph here on the vertical axis. It's percentage of failed area per waste package.

By the way, these results are per waste package. Presumably, we have not been able to introduce spatial variability into this, so the damage that occurs to one base package effectively occurs to all the waste packages in the repository. I don't think that was clear up to now.

These are the data. You see I have two fuzzes of data. The first one is for 10^{-6} per year, and that corresponds to a PGV of 2.44 meters a second. The second fuzz over on the right is the results of the 10^{-7} per year calculations, and that corresponds to a PGV of about 5.35 meters a second.

There are both red and black points within each of the data fuzzes. The black points represent the 80 per cent of yield failure criterion. The red points represent the 90 per cent of yield failure criterion. As you expect, the red points are lower than the black, and that's consistent with what Mike showed.

If you go to the next slide, please, this is a simple linear fit to the data. I have also tried some power law fits and a modified power law. I'm probably right now most comfortable with this fit. The dark black line is simply the mean of the 80 per cent data. You get about 2/10ths of a per cent damage to the waste package at the 2.44 meters, and you get about 1 per cent damage at the 5.35, the 10^{-7}.

I have also plotted for the 80 per cent values. The dashed line above and below are plus one sigma and minus one sigma. And that gives you an idea. The red curve is the mean through the 90 per cent failure. You can see that in spite of the difference in the failure criterion, the spread is dominated, I believe it's dominated, by the uncertainty in the ground motion. That's what drives the structural response.

TSPA requires damage over a range of PGV values. You can't use the curve I showed you without being able to relate it to frequencies of occurrence, annual exceedance probabilities. I've estimated the 10^{-5} per year earthquake corresponds to about 1 meter per second, and the 10^{-8} per year earthquake--not earthquake--just seismic hazard corresponds to about 10 meters per second.

So, if you could go back to the previous viewgraph, please, you can see that essentially at 10^{-5}, which corresponds to about 1 meter per second here, basically, the damage is predicted to be zero. At 10^{-8}, where it's about 10 1/2 meters per second, we'd get a damage of about 2 1/2 per cent up there. So, that's the range we're talking about, at least with this linear fit.

I think we will probably go forward assuming damage at 10^{-5} is zero, based on the extrapolations of the linear fits at either 80 or 90 per cent of the yield stress for the residual stress failure criterion.

We also have another calculation. We did calculate the waste package response for the 5 x 10^{-4} per year level.

That was the single preclosure ground motion that Ivan showed previously. That one also showed zero damage for the waste package. So, we have not done the full spectrum of results of 10^{-5}, but the evidence I have points to the fact that it's zero.

The damage at 10^{-8}, we'll go with simple extrapolation, 2 1/2 per cent, based on 80 per cent of yield. There are a number of conservatisms in this calculation. Some of them relate to the end-to-end impacts. Essentially at 10^{-7}, the end-to-end impact corresponds to about 92 per cent of the total damage. That's the mean number.

Now, individual histories are of course different. There may be one history where the waste package to emplacement pallet is actually greater than the end-to-end. But the general behavior you see is the end-to-end impacts dominate our damage. This is probably very conservative for two reasons. One of them is that given--synchronicity is the wrong word--the coherence of the earthquake waves over tens of meters implies that the case when two waste packages are moving opposite to one another and are going to hit in the middle, is probably not physically realistic. We're using it as a convenient way to bound damage, but it should not happen just from how earthquake response is.

A second thing is we allow the waste package to effectively impact an almost rigid barrier. Again, by putting an almost rigid barrier, that also ups the damage that we're calculating.

NELSON: You have just a very few minutes.

GROSS: Okay. Seismic scenario. I have talked about this a little bit. Basic estimate, we need a separate scenario because of low probability. We are focused on estimating the mean release, and we're probably going to consider a range, such as from 10^{-5} to 10^{-8}, where we get significant structural damage.

We will also be considering fault displacements if they produce significant structural damage, as well as worrying about the cladding. That work is still in process. So, I just can't present it right now.

The TSPA in the seismic scenario is a two step process. In the first step, we're generating R realizations that basically will robustly sample the whole range of earthquakes that can occur. I'd estimate that R is probably between 300 and 500 realizations, but we won't know that until we actually see how well the mean converges. And each realization is performed for 10,000 years, and each realization has a single earthquake that occurs during it at a random time.

The second step, using the results that are generated from the first, we basically sum up the doses in a probability weighted fashion to come up with a mean or expected dose for all the time histories.

I think I'll actually skip this if time is tight. This is what I want to get to. This formula here is the probability weighted summation to find the expected dose, and I just wanted to walk through that a bit.

This D(t) here is the expected dose for the total problem. It's a sum of the $D_i(t)/T_i$. This is the dose from the ith realization at time T from an event of probability, annual exceedance probability, $Lambda_i$, that occurs at time T_i. It's weighted by $Lambda_i$, and you have a sum, the T is the duration of the calculation, 10,000 years, and R is the number of realizations, probably between 300 and 500.

This factor here, the natural log of Lambda max over Lambda min, I've defined the quantities down here. This is really due to the fact that we're using an important sampling. I propose to sample the size of the earthquakes on a log uniform distribution, so that we get robust sampling in each of the decades. By decade, I mean 10^{-5} to 10^{-6}, $^{-6}$, $^{-7}$, and $^{-7}$, $^{-8}$. By using a log uniform distribution, we'll basically get equal number of realizations in those decades. But that obviously skews your sample towards low probability events, and this factor compensates for that in the total sum.

So, in summary, we've talked about the structural thickness and the failed area. The TSPA calculations will use a Monte Carlo sampling of the abstractions. They will cover the full range of seismic hazards that can cause damage to the system, and we will define failed areas and seismic condition for each realization, and the mean or expected dose as a weighted average.

That's what I have in a rush.

NELSON: Thank you very much. I'm sure Dan is going to ask about the cladding. But, Ron first.

LATANISION: Latanision, Board.

I'm really quite concerned about the failure criterion, and I just want to say as clearly as I can for the record, I want to identify the issues that concern me. So, if we go to Slide 6, as I said this morning, I think in terms of Titanium Grade 7, you know, there is evidence of stress corrosion cracking in representative repository environments. And, so, I think that may be a useful criterion in the case of Grade 7.

But in the case of Alloy 22, the trace of 80 to 90 per cent seems to be totally arbitrary. I mean, there is no evidence that I know of that shows stress corrosion cracking in representative repository environments. I think it would be a mistake to say that--well, you certainly would not want this Alloy 22 to be deformed to the point that it's plastically deformed, just from an engineering point of view. But from the point of view of stress corrosion cracking, I just don't see the basis for choosing 80 to 90 per cent as a failure criterion. I don't think that there's a basis for doing that.

If we go then to Page 7, the comment about heavily cold-worked metal being subject to enhanced general and localized corrosion, I really don't think general corrosion is going to be materially affected. And I don't know what the basis for that is. So, I'm concerned about that.

Again, the comment about 80 per cent yield strength, I think that deserves more discussion, which I'd personally like to have and I will talk to Gerry about that.

But I just want to say for the record that you may choose to say as a failure criterion that you do not want Alloy 22 to be deformed to the extent that it exceeds the elastic limit, and I could accept that, but I can't accept that in the context of saying that you don't want to do that because it will then be subject to stress corrosion cracking. There just isn't any evidence. I think the project does itself a disservice by using that criterion. So, I don't understand the basis, and I look forward to a discussion.

GROSS: I'd like to talk about that then. Off line would probably be better.

LATANISION: Yes.

GROSS: But I understand your concern. I'd just like to hear the discussion.

NELSON: Bullen?

BULLEN: Bullen, Board.

Could we go to Figure 11, please? This is where you made the point that for 10^{-8} per year--or maybe it was 10^{-7}, 92 per cent of the damage is waste package to waste package damage?

GROSS: That's a correct statement.

BULLEN: And, so, given that--I will actually agree with my colleague from MIT that I would like to see a basis for the 80 per cent. But even if the 80 per cent is right, have you done an analysis that basically says does the repository performance improve with greater waste package spacing?

And I'll disagree with Mr. Anderson that basically--I think it's an easy calculation, because it's kind of F=MA. I know how far it's going to move if I have that acceleration. And, so, why can't I figure out how far I'm going to push these waste packages with the vibratory ground motion or the standard ground motion that you have, and say will they hit each other?

And if they don't hit each other and 92 per cent of the damage goes away, then if you go back to Figure 27, which is your process of deconvoluting or unconvoluting what you have, you will notice that I'm picking something that says I've got this ground motion, and now I go back and see what fraction of the waste packages. But if it's 92 per cent less, aren't I doing much better, if it doesn't hit the other waste package? And, so, isn't it a pretty simple analysis to determine, well, if they're 10 centimeters apart, they smack each other, and if they're 25 centimeters apart, they don't?

GROSS: You trust me after all these computer models to do F=MA?

BULLEN: Well, I don't know. I assume that F=MA still works.

GROSS: I understand your point. But part of my point is we mentioned design before. I have been instructed to use the current HTOM design as my baseline. That doesn't preclude me from answering your question. But that's why the space was chosen to be what it was.

BULLEN: I understand the spacing is chosen for that. But if a simple calculation shows that that spacing, if you just doubled it or tripled it, and I don't know what the number is, saves you from having to worry about this damage at all, isn't that a reasonable thing to do?

GROSS: I don't think that will happen. These waste packages, at least some of them are moving with 4 or 6 meters per second, and I don't think they will fall to the ground in the 1 and 2 meter spacing you're talking about. But I will work it out.

BULLEN: I would love to see the analysis.

ANDERSON: May I make a statement? Mike Anderson.

The thing about them being spread apart is we may now be introducing new interactions that we haven't considered in this situation. Suppose they're spread a couple meters apart, then they could conceivably, waste packages could walk down the length of the emplacement pallet, come off, engage in more interactions with the top of the invert, maybe different interactions with the drip shield, and certainly with the pallet. So, I don't think it's clear that all of that interaction goes away, or maybe it's replaced with something else.

NELSON: Art McGarr?

MC GARR: McGarr, consultant.

This question is probably just based on my ignorance of what you mean by--the meaning of the term dose. But I'm having trouble figuring how you relate the failed area, for instance due to waste packages bumping into one another, to dose.

GROSS: Do you know how the nominal scenario works?

MC GARR: No. I guess that's why I'm asking.

GROSS: That's part of the problem. Okay, in either of these scenarios, it doesn't matter which one you have, essentially, you require failure of the waste package to reduce radionuclides. And, you know, whatever that area is, you may get advective transport if you're in an area of the repository that has seepage. You may get just diffusive releases if there is no seepage in a particular area of the repository with a damaged waste package.

Once the radionuclides leave the engineered barrier system, they go into the unsaturated zone, they're transported downward, and then out through the saturated zone, where eventually there is dose conversion factors to figure the dose to an affected member of the public.

So, in some sense, the existence of a failed waste package is directly the cause of how you get a dose to the public, because if the waste package doesn't fail, nothing happens.

What happens in the nominal scenario is an elaborate series of corrosion calculations that define how the waste package fails. In the seismic scenario, we have another elaborate set of calculations that, in effect, define mechanical failure, or mechanical plus corrosion failure.

But once you get release from the waste package, the rest of the models are identical, in other words, transfer to the UZ and SZ and the dose conversions are the same for the nominal or the seismic scenario.

Does that help?

MC GARR: Well, it helps. As I look at Figure 16 and 17, it just looked to me like there was some kind of a robust relationship between seismic damage and the resulting dose. But, it seems like it must be a very nebulous calculation.

GROSS: Well, it's not so much nebulous. It's just not a simple function that I can write down. In effect, it's a complicated function, and there are other stochastic variables in there, such as KDs and flow fields, and other things like that. So, I can't simply write down a simple function. And all of that is compressed into a black box, if you will, that I call D_i way at the far right-hand side of that equation. So, it is a function, but quite complex, with stochastic uncertainties and variability.

NELSON: Okay.

GROSS: I'm sorry.

NELSON: It's a very hard thing to answer. But, thank you very much.

I'd like to just put in a plug that's across the board. Although it did not come up, I think in doing my homework preparatory to coming here, it was clear that there's an awful lot of work going on on the project these days at an accelerating rate. And there's an abiding question I think on most of the new data that comes in, the extent to which it is reinvested in the project appropriately in terms of, for example, how some of the deterioration in the tunnel walls may have something to do with seepage or other issues that may also need to be considered and modelled anew.

So, I realize it's a moving target, but that is an abiding question that I think most of the Board members have, making sure that what's being learned here is reconnected back into the project appropriately.

No response required. Just we'll be asking.

Now, the Board had arranged, the Panel has arranged a Panel, and we are running a bit late at the moment. What I'd like to do is, because we have public commentary set up for 5:20 to 5:40, what I'd like to do is to start that public commentary at 5:30, and if there's anyone who needs to leave promptly by 5:40, let me know and we'll put you on first. If you can stay over a little bit later, we'll put you on a little bit after 5:40 so that there's time for everybody to comment. So, we'll start the public comments at 5:30, give the roundtable something close to an hour and 20 minutes.

The roundtable will be formed here, and we're not going to take a prolonged break. We're just going to break long enough for the roundtable to be formed. Dan Bullen will call you to attention in five minutes.

(Whereupon, a brief recess was taken.)

BULLEN: I always love these scripts that they write me, so I'm going to read it explicitly. I'll never forget the first time I gave a public speech for the Board, and I got a call from our Executive Director before that, it was at a High-Level Waste meeting, and he said, "Dan, read the speech." So, I will read the speech. Okay?

It starts out my name is Dan Bullen, and I'm a member of the Nuclear Waste Technical Review Board, and the Moderator of today's roundtable.

Before I discuss how we propose to conduct the roundtable, let me introduce the participants. You've already heard from Mark Board, Bill Boyle, Jim Brune, and Mike Gross today. The new faces include Jerry King, who is a Disruptive Events Lead for Bechtel SAIC, the DOE's Management and Operations Contractor. Dr. King is a seismologist by training. Walt Silva. Walt has actually spoken many times already. He's from BSC, and Pacific Engineering and Analysis, and provides the ground motion estimates used in the design and analysis. Walt is a seismologist with extensive experience in modelling earthquake ground motion and quantifying the effects of site conditions.

We also have two consultants to Bechtel SAIC on seismic issues, C. Allin Cornell. Dr. Cornell is an adjunct professor of Civil Engineering at Stanford University. And Robert P. Kennedy. Robert is from RPK Structural Engineering Mechanics Consultants, Incorporated. Dr. Cornell's expertise is in probability and statistics applied to engineering problems, including earthquake hazard analysis. And Dr. Kennedy's expertise is in analysis and design of special purpose civil and mechanical structures, such as nuclear facilities, and the design of structures to resist extreme loadings, such as earthquakes.

Now, we want to keep the roundtable discussion as informal as possible, in order to stimulate free and open dialogue between the roundtable, Board members and the Board consultants. I have not asked each of the participants to make an opening statement. The reason is is if they do that, then we won't have any time for roundtable. So, we're not going to have any opening statements.

But I would like to concentrate on four areas. And, Walt, close your eyes because you might be blinded. Okay. The four areas that we want to concentrate on are essentially current ground motion estimates for Yucca Mountain, particularly the realism of those at low probabilities. Okay? These are summarized, by the way, in your agenda, but there's a little bit more words added to these.

The second one is alternate approaches to developing these ground motion estimates, including the validity of placing physical bounds on such motions. Now, we talked a little bit today about the physical bounds that may be placed on it based on rock strength, and the types of accelerations you expect to see.

I'd also like to talk about the current approach of using these ground motions in pre and postclosure design, analysis and performance assessment. And then, finally, alternative approaches to seismic design, analysis and performance assessment.

Board members and the Board consultants are going to ask a few questions, and actually I'll start, although we encourage interaction between the Panel members also. So, if there's a dispute or a disagreement, or you want to resoundly applaud your colleague for the comments that they've made, please raise your hand slightly or make a gesture to me, and I'll acknowledge you.

Again, I'd like to remind us all of what Dr. Nelson said earlier today, that a good portion of what we have heard, or will hear, is preliminary and does not necessarily represent final DOE positions on this issue. We would really like a very free and open discussion.

And, so, with that, I'm actually going to take this mike off and sit back down so that I don't completely give us a feedback problem, and maybe pick on Dr. Silva just because Dr. Silva was the person who spoke the most, to ask him to talk about current ground motion estimates at Yucca Mountain, particularly the realism for those at low probabilities. Do you feel they're realistic? And, please use the microphone.

By the way, I will ask that everyone identify themselves before they speak so that our transcript will be accurate, and to use the microphone and speak into it so that not only everybody in the audience can hear, but so that the tape recording gets your voice on tape. Thank you.

Dr. Silva, do you want to take a shot at that first one, please?

SILVA: Walt Silva, BSC, and my comments are preliminary, along with the ground motions.

Basically, we probably should separate this into 10^{-6} and 10^{-7}, we're talking about the postclosure. The 10^{-6} motions, the mean motions, I think are probably pretty realistic. Ivan showed recorded ground motions that were about a factor of 2 to 3 lower than the 10^{-6} motions, depending if you looked at PGA or PGV. And that's what we recorded in the last 50 years, and we're talking about 10^{-6}.

The strains, median strains that are built up in the repository block for 10^{-6} motions are about $.2 \times 10^{-2}$ per cent, sort of on the cusp of deforming a lithophysae, that kind of thing, generating fractures in the rock mass. So, they're probably reasonably realistic. I don't think you can dismiss them as being unrealistic and defend it.

The 10^{-7} motions, though, I think are getting into the area where most people feel that they're unrealistic. So, it's somewhere between, I think, 10^{-6} and 10^{-7}.

BULLEN: Bullen, Board.

Do we have consensus from the Panel, or is that sort of a threshold where we think we might see the deviation from reality, or is there a differing opinion? Go ahead, Dr. Brune.

BRUNE: Well, I don't have a differing opinion. I don't think we know for sure where we're going with this difference between the--

BULLEN: Oh, please use the microphone.

BRUNE: I'm not sure we're going to end up, or we know where we're going to end up in this difference between the hanging wall and the foot wall and thrust faults. Almost all the data with the high velocities and high accelerations that Walt showed are the hanging wall and thrust faults, or at least thrust faults, if not the hanging wall. And I think it's an open question as to whether in a trans tensional and in the normal faulting area, you can use those same situations. But, I'm not saying I know the answer. I'm just saying it's something to further look into.

BULLEN: Other comments? Dr. King?

KING: I think the important thing is to state that we just don't know yet. Maybe it's 10^{-6} where the ground motions are realistic, and below that, they start to deviate from reality. Maybe it's 10^{-5}. We just don't know yet, and we need to pursue some of the studies, the scoping studies that were talked about today, to hopefully develop a technical basis that will allow us at some point in the future to make a quantitative determination of where the ground motions saturate at Yucca Mountain.

BULLEN: Bullen, Board.

To follow up on that, how much experiment, how much money, how much time would be necessary to nail it down, ballpark?

KING: I don't even think we're ready to state that. I mean, what you heard today basically is where we are. I mean, we don't have any thoughts that haven't been expressed today. We haven't done any scoping studies or planning beyond what's been expressed here today. We have to think about what's necessary and where we go from here.

Other than doing some very preliminary scoping studies of the modelling by Itasca of the dynamic rock properties, that's really about all we've scoped out.

BULLEN: Dr. Boyle, you had a comment?

BOYLE: Yes. I suppose it depends in part upon one's definition of "nail it down." If a person, you know, is an extreme skeptic, we could be at this for many, many years. And it just raises the issue in some regards is although it's a very interesting scientific issue, and we do want some insight into the answer of our degree of conservatism ourselves, as I indicated earlier, this probably will not be a driver for dose. So, in a risk informed performance based arena, although some money as yet undetermined should be spent on this, you know, it's not one of our most critical items.

BULLEN: Dr. Abkowitz, go ahead.

ABKOWITZ: Abkowitz, Board.

Actually, Dr. Boyle, I'm glad you made that comment, because you're a good segue into my question.

My question with this whole issue of the uncertainty at 10^{-7} is who cares? And you're basically saying there's no need for us to care because whatever the uncertainty is, it's not large enough to have a profound influence on the performance of the repository.

I was just curious whether the rest of the Panel agrees with that position.

BULLEN: Dr. Cornell?

CORNELL: Yes, Cornell, consultant.

I think we have to separate several things here. One is this issue of realism in the sense of are some of these values for peak acceleration or peak ground velocity physically realizable or not. That's one of the questions which has been opened, and I think Ivan Wong said since we've been measuring ground motions, people have asked the question what might be an upper bound on the peak velocity or peak acceleration under certain soil conditions, under certain rock conditions.

That question has been opened and I'm sure it will be opened even after Yucca Mountain is opened, because it perhaps is not a driver to the design and safety of Yucca Mountain, but also because it's clearly a very, very difficult scientific question. Other people have wanted to bound or cap or use such words in the past, and my challenge to them has always been fine, give me some physics as to why you want to cut off your probability distributions as 2 sigma, is 3 sigma, as Ivan said, we often do it in practice where we're looking at 10^{-4}, 10^{-6} ground motions.

So, the challenge has always, from my point of view, has always been, well, let's continue to follow the models until we're driven otherwise, either by some physics or by a necessity from the point of view of the facility we're trying to license. That's different from the question of are these the right ground motions for these probability levels. That's a separate question. That's where Jim's question is coming. I would love it if it's true that he's got good data that says the estimated mean ground motions that the ground motion experts for the PSHA here used are high by a factor of 2. That's in the average, in the mean. If there's fundamental reason to say those numbers are too high on the average, that's going to change things a lot, too, in terms of the probabilities associated with 1 g's and 2 g's. But I think we have to keep those two questions very separate in our minds.

BULLEN: Bullen, Board. As a followup to that, Dr. Cornell, this morning, we saw Carl Stepp make a presentation that extrapolated down to 10^{-8}, and took those accelerations to, I can't remember, 10, 12, 15, 20, big numbers. I mean, big numbers. Are those realistic and should they be employed in our attempts to bound the case, or are they beyond the scope of reality?

CORNELL: That's the question we have just been asking.

BULLEN: Right. I understand.

CORNELL: But my answer remains the same. We need to see is there some physical reason why 10 g can't--I mean, as people have pointed out, you can drop your watch and get 10 g. It, of course, depends on the frequency content. So, it's not precisely .xg that's driving damage to these waste packages, for example.

BULLEN: Correct. Questions from the Board. Oh, Dr. Board, go ahead.

BOARD: Well, I would just like to add I think where the importance comes in is what Priscilla brought up before we quit, and that is how does this affect uncertainty in other areas, seepage, things like that. I think if the actual ground motions at these levels are much lower than what we're currently predicting, the effects become potential effects not to perhaps waste package damage, but maybe drift stability and things also drop away very quickly, we found out. And, so, I think that's maybe where the payoff, if you want to call it, or whatever, comes to seeing if you can cap those motions. It increases your level of confidence in other areas of prediction as well.

BULLEN: Dr. Abkowitz, go ahead.

ABKOWITZ: Abkowitz, Board.

Mark, if I can follow up on what you're saying, in some ways, it contradicts what Dr. Boyle is saying, because what I'm hearing you say is that the inter-dependencies in the whole waste performance system are such that if some of the uncertainties that we're talking about in the seismology area impact these other areas, then in fact seismology has the potential to be a significant enough driver to affect performance. Is that what you were saying?

BOARD: I don't know. No, I'm not saying that. I just don't know. I guess there are issues where, you know, I brought up the issue of drift stability and shape of the opening. Perhaps that has no impact on seepage. I really don't know. But I just say that if the calculations were such that the ground motions came down to the point where you weren't concerned about those issues of drift stability, the whole confidence in that kind of area increases, and that issue goes away.

So, I don't really know if that's an issue or not. I'm just pointing out the question you asked is what's the big deal, there might not be any big deal, but that's the payoff if it is.

BULLEN: Dr. Parizek?

PARIZEK: The big deal really would be like rockfall sitting on either the drip shield or waste package, where you now have a compact point, where again you could have some corrosion activity focus. Because we talked about secondary mineral buildup on the one hand, or dust particles, and rockfall material could be of similar consequence.

But in the shape of a tunnel, a smooth tunnel, you're bound to have some flow focusing, where water wouldn't drip in, but might, if it got in, it could still move down the walls. If you get a ragged tunnel as a result of rockfalls, there may be many places now where the water really gets hung up in the ceiling and has to drip because there's no other place to go after the ventilation period ceases.

So, these feedbacks, plus the whole humidity story in terms of what all that debris is sitting around against the drip shield or waste package, it's a whole new environment.

BULLEN: Bullen, Board.

Actually, to sort of follow up on that and to amplify something that Priscilla Nelson usually always tells us, is that we want to be able to understand the story. We want the mountain to tell us the story. We want to understand the physics behind the performance that we expect to see from the natural and the engineered systems.

And, so, when you get to the realm of incredible performance, meaning something that you don't expect to see, and you run into that ability to say okay, well, you've bounded it, but you've bounded it at a ridiculous state, it just sort of shakes the confidence in the people who are doing the review, like the Technical Review Board, in that well, do they really understand the physics of what's going on.

And I kind of want to follow on to another Board point here, is the use of natural analogues. Dr. Brune gave us a very good presentation on preciously perched rocks, and Dr. Parizek came up with the lithophysae crystals that are, you know, basically little inverted pendula that are sitting there. I guess I'd like to comment or ask for the Board's, the roundtable, I guess, discussion of do those analogues tell us the story? And I'll start with Dr. Brune, because he obviously thinks they do, otherwise, he wouldn't be doing this sort of research. And then ask the rest of the Panel to please comment on that.

BRUNE: Well, there's a potential that they have something very important to say, aside from Yucca Mountain. I mean, one of the reasons for studying the San Andreas Fault is there's a lot of people that live down there, and they've got a direct interest in what the true ground motions are, and there's going to be nuclear power plants and a hospital, and so forth. So, I definitely am going to try to pursue this to the limit to figure out what's going on.

But in terms of an adversarial situation where you have to defend it, at this stage, it's really in the review process. I've published, like, several papers, John Anderson and I have, and we've had responses and criticisms, and so forth. There hasn't been anything fundamental that would counteract what I've been saying, but I think it's still in the research stage. And I'm not sure I totally answered your question, but I'm avoiding coming into any final conclusion about it. Aside from the fact to say there's not only Yucca Mountain is a good incentive for trying to figure this out, there's also a very important incentive for figuring it out where a lot of people live.

BULLEN: Bullen, Board.

The Technical Review Board has always liked, or always aspoused the use of alternative lines of evidence in making a case for a license for a repository. And, so, this is the point that, you know, I'm looking at Point 2 up here about alternative approaches to developing these ground motion estimates, including the validity of placing physical bounds on such ground motions, and I'm trying to look at that, placing physical bounds by using analogous in the area.

And if it looks like we've got 13 million year old volcanic tuff with little tiny lithophysae in them that have inverted pendula that haven't been broken by ground accelerations, you know, of 12 g's, or whatever it would take, does that not argue for the case that, you know, we haven't even seen it in the 13 million years since the timber mountain caldera deposited the tuff, so there's probably a good probability that in 10^{-8} per year, it's not going to happen?

And I guess that's a rhetorical question to the Panel to see am I off base here. And I'll ask somebody who hasn't spoken. I guess I'll pick on Dr. Kennedy here. Am I wrong?

KENNEDY: Bob Kennedy. I would like to see some of these analogous studied. I think that so far, we've been looking at these very high ground motions at the repository depth level, but ultimately, we also have to show that we meet performance goals on these preclosure surface facilities, and those are going to require us to show that at least certain structures don't have unacceptable behavior more frequent than something in the low 10^{-6} range.

To show that, we're going to have to have some estimates of 10^{-6} ground motion at the surface. If we look at what we've seen at the repository levels at 10^{-4}, the surface motions are three times those at the repository levels, so if you're starting to worry about are the motions at the repository level credible, the surface motions at 10^{-6} will be potentially even higher.

I think we definitely need to aggressively look at trying to find some ways of, I don't like the word physical, complete physical bounds, because that's a deterministic cutoff, and we're going to have to have uncertainties on those bounds as well, but we definitely need to get a handle on whether these ground motions are realistic.

I went through this same process on many, many nuclear power plants, and we've seismically ended up being reported as one of the major contributors to risk. I don't think any of us believe that. It was again driven by the same issues that are driving here. It was almost impossible to get seismic risk below 10^{-6}, because of the way the ground motions just kept going up as you went to lower and lower and lower annual frequencies of exceedance. And I think the same is showing up here.

BULLEN: Dr. Brune?

BRUNE: I just want to point out the obvious fact that Bob was talking about. A factor of 3, the precarious rocks, by definition, are at the surface. So, those curves predict three times as much as a lot of what you've seen already, and of course that would totally knock all these rocks down.

BULLEN: Good point. Other comments from--oh, Dr. Gross?

GROSS: You know, a major point that comes out from our results to date is we are really getting, in spite of the conservatisms that we deal with, we are really getting very modest structural damage. I realize there are questions about the time histories. There are questions about the failure criteria. I personally don't like putting so many conservatisms into performance assessment because it hides the real response, but by the same token, it does give a reasonable amount of comfort that the system is quite robust, even though what everyone thinks are extreme boundary conditions on the system.

Now, I still think it's worthwhile quantifying what those uncertainties or conservatisms are, because otherwise we can't really go very far with this conversation.

BULLEN: Bullen, Board.

I have a followup question, and Bill Boyle already answered it, but I wanted to get it on the record. The doses that are determined on these types of analyses are probability weighted doses; is that correct?

GROSS: They will be when we go through TSPA.

BULLEN: So, the final product will be the same types of doses that are going to essentially be the, well, the volcanic doses were also probability weighted, if I'm not mistaken. And, so, we're going to have a probability weighted scenario, or set of doses that we'll have to calculate? Bill Boyle, do you want to respond to that one?

BOYLE: Yes, this was a question that came up during the break, and I actually asked Bob Andrews about it last Friday. For those in the audience who don't know, our regulation does require that the calculations for dose be probability weighted. You know, it's to take into account, you know, how often, you know, any bad thing might happen, and weight the calculation accordingly. And, we do.

BULLEN: In spite of the desire of our previous Chair; is that what you're trying to say?

BOYLE: We have done the calculations non-probability weighted, and shown them, you know, to getting insights.

BULLEN: And we appreciate that, too.

BOYLE: And I asked Bob, you know, bearing in mind that these calculations have not gone all the way through to dose yet, but, you know, comparing the apples to oranges, the earlier calculations we've done, I asked him for the most, you know, the lowest probability events, would we pass, without the probability weighting, because that is a much stronger case, you know, that you just say I stipulate the plane crashes and nobody is hurt or dies is a much more compelling argument to travellers than, you know, the fact that it's just a rare event.

But Bob said that no, he didn't know that we would be able to make that claim. But we certainly, with the probability weighting, will probably pass easily.

BULLEN: Could I get back to the second point for the rest of the Panel here about alternative approaches to developing these ground motion estimates? Any other suggestions besides the analogous that were already discussed?

CORNELL: Cornell. We've heard what the project is doing now in terms of trying to in fact use the same types of physics that we're currently using to get from AB up to the surface, to use that from what was called A prime, up to A. That's recognizing that at the kinds ground motions that are being discussed and the strains it implies in the rocks, there would be some non-linear behavior which would tend to--cap is, again, the wrong word--but it tends to modify, reduce, particularly the high frequencies. This will unfortunately probably not have as much effect on the PGS's that are driving the rock fall as it will on the PGA's that may be driving the waste package damage, if I understand the preliminary results.

A couple other comments related to what was just said. These are going to be probability weighted doses. And remember when you probability weight the dose, you also weight by the mean of the probability, which is already a probability weighted probability. That goes back to the question of where these uncertainties are coming from.

As Carl Stepp pointed out, if we look at the 10^{-8}

ground motion, the mean estimate of the probability associated with that, which is the one that will get used in the mean weighted doses, it's associated with about a 10 g number. Whereas, the median estimate, the one the experts would say is fifty-fifty likely to be above or below, is only down at about 3 1/2 g. And that 10 g number is associated with the 90th percentile. That means that it's, in a sense, the experts themselves think there's only a 10 per cent chance that the value was really that high.

But, because you're doing mean weighted, you are driven in these cases of broad uncertainties, with units to the upper direction towards high fractiles and low likelihoods of these estimates being correct. That's basically a conservatism of the mean that's being thrown into the exercise.

If you read it the other way around, if you say what is the 2 g scenario going to do to us, today the 2 g scenario is the 10^{-7} case that we've done the highest waste package responses of. That's associated with, what, about 3 or 4 x 10^{-6} as a probability according to the mean based estimate. So, it would get weighted by 3 x 10^{-6}.

On the other hand, the median estimate is only 10^{-7}. That's a factor of 30 lower if you use sort of what the experts' best estimate of what that number should be. And that's being reflected by statements of the ground motion experts in particular that said, well, wait a minute, we're in a zone, this is a tension zone, tension extensional zones where we don't have much data. We've heard Jim talk about hanging walls and foot walls, but I'm not sure which one I believe we've heard, but we're worried about the fact that we have splayed faulting. We're very close to the fault. We don't have much data close to the fault. Maybe the numbers are going to be much bigger than come out of typical regressions through mean data.

It's all that kind of thinking that's in the experts' minds that causes them to put big uncertainty bounds on their parameters that lead to these differences between median and mean estimates being so broad. And we're living with the down side of using the mean estimate, which I happen to agree with under the circumstances. But it does throw a big factor into what we're talking about, whether you look at it at the ground motion associated with a different probability, or the probability associated with a different ground motion.

BULLEN: Bullen, Board. Oh, I'm sorry. Dr. Brune, go ahead.

BRUNE: You asked for other analogous, and I was just thinking of John Stuckless's analogous of the caves. I don't know if Mark Board or anybody else has thought about it. I don't know what kind of seismic risks zone they're in and how many earthquakes they've been exposed to. But it's a possibility.

BULLEN: Bullen, Board.

Along those lines, not necessarily analogous, but actually data that you can acquire, are there relatively simple or inexpensive in situ tests that you could do in the ECRB that would give you the kind of information that may narrow the uncertainty bands in these types of calculations? Or are they too hard to do? I'm just asking a question from an engineer who thinks, you know, maybe you could make a measurement that would help you out, and what might those be, is my question.

BRUNE: I don't know. Are you going to answer that, Allin?

BULLEN: Okay. So, I asked a hard question. I'm sorry.

BRUNE: What's ECRB.

BULLEN: The cross-drift, where you get into the lithophysal zone. I'm in the lower lith region, is there an experiment that I can go do that will tell me the rock response, the acceleration? What can I do besides make an earthquake?

BRUNE: You mean an in situ test?

BULLEN: Yes.

BRUNE: A lab induced--

BULLEN: No, no, no. I want to go underground and get something from Yucca Mountain that may actually help me narrow these uncertainty bands at the low probability events.

BRUNE: Well, if you believe the analogy between the thrust walls, that's a big if, supposedly we've gone beyond that and you believe that it is telling you something, then if the people who look at these fractures--first of all, are there big chunks of Yucca Mountain that aren't fractured the way the hanging wall and thrust walls are. I assume there are, because I've seen some of them, and I've been underground. Okay, then the question is when--the fractures that are there, when did they form?

And it seems to me a very strong argument if you can prove those are all old and there essentially have not been any fractures of that rock in 10 million years, that's a strong argument. And, so, I think that ought to be pursued.

If you ask me what I would pursue next, I'd like to be convinced of that, and I've heard it as a rumor, but I haven't heard any more.

BULLEN: Dr. Boyle, and then Dr. Cornell.

BOYLE: I would just offer, it's not really a test, if you will, but it's measurements or observations that we're already doing, and it's the presence of the lithophysae, and that work is underway. We know that they weren't crushed or deformed. We know that many of the fragile minerals in them are still there. And, so, it's not work related to lithophysae and the analyses associated with them that I think is--in a sense, it's a test or observation or measurement from the underground that we've already made, and now we're trying to figure it out.

BULLEN: Dr. Cornell, you had a comment?

CORNELL: Yes, just a simple comment. If we're thinking about the 10^{-7} event, and we have 10 million years, that's 10 to the 7 years. And you say this event did not happen, this event that somebody has proposed that has a probability of 10^{-7} didn't happen in 10 to the 7 years, is something like flipping a coin twice and seeing two heads and saying it doesn't have a tail.

BULLEN: Bullen, Board. I agree.

Dr. Budnitz wants to comment.

BUDNITZ: I just want to make a comment about that. If it's a stationary Poisson, and I'm not sure that it is, then if you've got something that happens every million years, on the average, the probability that it didn't happen in 10 million years is zero when you suspected 10, and in Poisson space that will never happen. There's very low probability. But if you look at the distributions we're dealing with, and then pull that through, you can't use the argument that at 10^{-6}, the thing will happen in 10 million years with as much high confidence as you think. And, in fact, it's probably fifty-fifty, or something, when you look at that very broad distribution. You have to be very careful to say that something that hasn't happened in 10 to the 7th years, isn't 10 minus 6, because that's about where it comes out. Just do the arithmetic yourself and you'll convince yourself. And, so, you have to really look at that hard before you go that far.

BULLEN: Thank you. Dr. Board, go ahead.

BOARD: I'm an engineer, and--

BULLEN: I won't hold it against you, because I'm an engineer, too.

BOARD: To me, I think these observation are quite important to me when you can walk underground and see the state that that rock is in and study it and look at it. To me, it's a very good marker horizon that indicates what's happened in the last 10 million years. Okay, I'm not a statistician, and I can't tell you if that equates to 10^{-7} or 10^{-8} probability, but to me it's a matter of confidence building, in that all these things add towards your confidence in your predictions.

And, so, that's what to me is the worth of it, not maybe necessarily to prove whether or not it's 10^{-7}. And in that regard, we are doing some work in that area as far as the testing goes. I'm not sure where we're going to take it exactly, but Dave Buesch who is over there, one of the geologists from the USGS, has studied in quite a bit of detail thus far fractures in the lithophysal zone, and perhaps will do some more work in that area.

BULLEN: Dr. Kennedy?

KENNEDY: Yeah, Bob Kennedy.

I think sometimes we maybe mean to be more I'll use the word honest in our displaying of our numbers. In reality, maybe we ought to talk both mean risk and median risk, and demonstrate clearly the large difference that exists between these two. I understand all of the arguments that mean risk is a better thing from a risk assessment standpoint, but I really believe we ought to point that different out.

I think what we're saying here is that 10^{-6}, 10^{-7} ground motions that we think have a fifty-fifty chance of being exceeded in that period of time, are reasonable level ground motions, and that these high ground motions that are in our mean hazard curves and then drive high mean risk numbers, that there's simply a reflection of our uncertainty. We don't know where to put limits on it unless we get some of these analog studies, and that our inability to put these limits on just drive up these numbers. I mean, the numbers are at the 90th percentile when you do confidence bands about the median.

BULLEN: Point well taken. Dr. Latanision, Dr. Abkowitz, and Dr. Parizek. Dr. Parizek, go ahead.

PARIZEK: Parizek, Board.

On the comment you made, if you go back to the PSHA analysis, I was sort of sitting in as a newcomer in this whole process listening to what was going on, but I don't think anybody gave any serious attention to precarious rocks. I mean, that was sort of a funny thing, you know, that's sort of like pack rats, you know. So, the way who was on that exercise really gave that a lot of weight.

The other test site precarious rocks that fell, you had Little Skull Mountain after, I guess--I guess it was after. So, there's these real factual things that you saw evidence of, but of the people who are part of this activity, who gave that great weight? And if you did it over again, it was the sense of my question this morning, would you sort of narrow the bounds a little bit? Because, I mean, everybody feels uncomfortable, and so you sort of put the uncertainty bands a little bit wider. But is this a basis to narrow them now? That's really part of the issue. Again, little crystals down inside the cavities may not help us, but somebody is going to help us. And I'm just looking for would you do it over again? Would you narrow your bands, given these new observations, new concepts, new observations, new data?

BULLEN: Comments from--Carl Stepp, did you want to make a comment?

STEPP: Just a perspective. This is Carl Stepp from the project.

During the project, Jim Brune was doing this work, and it was presented in some workshops. Jim also was doing some of the modelling work that he has done on the foam rubber modelling, which also was presented. So, there was a body of evidence available to the experts. With this data, as with other data that they were provided, didn't ask them to explain to us what weight they gave it, but some of the information was there. Of course, Dr. Brune has gone a long way in developing these ideas and reinforcing his evaluations with the new data since then.

PARIZEK: His work has gone further and further along. There's more things published in peer review literature. So, you can say, well, that's the kind of thing that may affect somebody's thinking today.

I think the rock falls in the Nevada Test Site tunnels, miles of tunnel, and in terms of rock size and frequency of falls, there's an opportunity to go look at that and build another observation basis to see whether you feel comfortable with what the models say. No matter how many joints you measure, you know, how good those forecasts might be, we want to get some field sense that that's reasonable.

STEPP: Yes. I think, you know, certainly it's a matter of building on data and observations in this instance. And if we're talking about strain limited motions, or in some way motions that are limited by physical factors, then that itself is going to be an uncertain evaluation. And any additional data that are available, such as the lithophysae and the precarious rocks, would have an important role to play here.

PARIZEK: And just interrupting, because I know I'm out of turn here, but now to go back to Jim, there's some rocks that have no desert varnish, and no caliche coatings, or anything, and there's some of these that are also on fault zones that we see when we run around the Yucca Mountain site. How can that be? Are those new cracks, new fractures, where you'd expect to see some of the signals of weathering and age?

BRUNE: You mean rocks without rock varnish?

PARIZEK: Yes, and joints without secondary mineral fillings or alterations.

BRUNE: I don't know if I can answer anything about the joints. That's going to be somebody else's--

PARIZEK: Well, let's call them cracks.

BRUNE: I've looked at a few of them and I saw little tiny colored things adjacent to all of them I looked at. But that's not my expertise.

Of course, you have rocks falling down for various reasons all over geologic time. And at the Nevada Test Site, we just have a paper in press now where we correlate the rock falls caused by the nuclear explosions as a function of distance away from the explosions, where we know the ground motion. So, it's a calibration of the methodology. And near the explosions, the cliffs are totally shaken down and all covered with white caliche. It's very obvious. In a few hundred years, that caliche will erode off. In a few thousand--well, say a thousand years, rock varnish will come back and they will turn black.

Now, aside from those rocks, everything is covered with rock varnish that's on the cliff, that's up on a cliff. Occasionally, you see a rock that looks like it might have rolled down and exposed part of the rock that didn't have rock varnish on it, but it's essentially never. If you look at the cliffs at Little Skull Mountain, Yucca Mountain, anywhere on Buckboard Mesa, outside of where the nukes have knocked them down, everything is covered with rock varnish, 10,000 year old rock.

PARIZEK: In 200 years, do you think if we flipped a rock upside down, it had caliche on the bottom side, in 200 years, it's gone?

BRUNE: I've talked to people, John Stuckless and a few other geochemists, and it's a few hundred years. But that wouldn't put the rock varnish on there. That just erases the caliche by the rain and everything. So, then you've got how long does it take to put the rock varnish on? Well, it's probably a thousand years to get it as--well, it's 10,000 years to get it as black and thick as it is there now.

PARIZEK: On what grounds do you use that 10,000 year number for varnish? Because I thought there was some discussion about whether that's--

BRUNE: There's some controversy in the literature, but we did not use any of those controversial methods. What we used is the layering in the rock varnish which has alternate yellow and dark bands, and you can identify those with various stages in the ice ages.

So, if you have this light colored band of rock varnish at the surface, that's Holocene. If you see the first dark band under it, then that's 10.5 year--

PARIZEK: Does anybody agree with you on that?

BRUNE: Nobody disagrees with it.

PARIZEK: What you're saying is a climatic record between the iron rich zones versus the manganese.

BRUNE: Well, it's been published in, John Lew (phonetic) the guy who developed it, has an article in Geology about a year ago.

PARIZEK: So, it's come out?

BRUNE: Yeah, it's come out. And I don't think people disagree with that. There is this thing about the cation ratios and the rock varnish that Dorn did, and the net effect of that is to make everybody not trust any rock varnish ages by that method. They just don't trust it.

PARIZEK: But, still, when you see a lot of varnish, that's a pretty stable surface for a pretty damned long period of time, like you have 10,000 years on it.

BULLEN: Bullen, Board.

PARIZEK: Excuse me. The other paper, where is that going to come out, the one on the Nevada Test Site? That's extremely important, the type data we've been asking for.

BRUNE: It's in press in the Journal of Geophysical Research. We can get a pre-print of it for you.

PARIZEK: I guess maybe we should have that.

BULLEN: Bullen, Board. Are you done, Dr. Parizek?

PARIZEK: Yes.

BULLEN: Okay. That's okay, we're following a trend here. Now, Dr. Latanision and then Dr. Abkowitz. Go ahead.

LATANISION: The observation of lithophysal stability over long periods, it is uncracked, suggests rock stability over long periods of time. But there is a phenomenon in the case of silicates, silicate glasses, for example, called static fatigue, which has to do with the--it's essentially an analog to stress corrosion cracking in the presence of water in the case of silicates. And I just wonder whether when the repository is being loaded with all the driving force to cause water flow, whether that changes the picture in your mind at all.

In terms of the stability issues that we're talking about here, how do we integrate the water flow phenomenon into the thinking here?

BRUNE: In the cracks?

LATANISION: Yes.

BRUNE: I don't think that's my question.

BULLEN: Dr. Brune dodged that one, so Bill Boyle?

BOYLE: Yeah, I might dodge it myself. You know, perhaps Mark knows more about it. Static fatigue testing can be done on rocks as well, as some has been done for the project by Sandia National Labs, or its contractors, and they did some years ago, but I think in the last year or so, they have restarted doing the testing. And Sandia's contractor years ago that did the work, Randy Martin of New England Research, I do remember reading the reports even after all these years, and water did have a fundamental effect. It was it the crystal level, but eventually, you know, it's these little defects or little things eventually cause the rock to fail. But, that's the extent of my knowledge, is that we are looking at the testing.

BULLEN: Mark Board? Go ahead.

BOARD: Yes, we are right now just starting a pretty extensive program in doing static fatigue tests, and rate dependent tests on the non-lithophysal rocks. We're doing it on non-lithophysal rocks because of the obvious problem of testing these large diameter cores with lithophysae, and plus you get all the stress concentration effects with lithophysae that make it very difficult to analyze.

So, it is something that we're actively looking at and concerned about, I mean, for the long-term stability. What we're doing to relate the time dependency of the lithophysal rocks is we're using this program called PFC, a particle flow coat. It's a micro-mechanical model, you know, the term I guess you're probably familiar with, in which we essentially take--the matrix material is the same, more or less, whether it's the lithophysal rock or non-lithophysal rock, so we're developing the time dependent law from the non-lithophysal rock and then we're using this numerical model to try and lump in the effects of the stress concentrations from the lithophysae to try and understand how that time to failure curve is affected by that. So, that's actively going on right now.

BULLEN: Other comments from the roundtable on the question from Dr. Latanision? If not, we'll move on to Professor Abkowitz, please.

ABKOWITZ: Abkowitz, Board.

I'm going to segue into Question 3 with this question of the Panel.

I earlier asked the question should we care what we don't know about, and the majority of the Panel seemed to think that we don't know enough to know whether we should care. And, therefore, we need to carry on.

And at the same time, it's pretty evident at this point that the performance assessment process doesn't view the seismology concerns to be critical to the outcome of whether or not the repository is going to pass muster.

So, my question then is if we need to learn more, what are the most important things we need to know? How soon are we going to know them? And is there even any possibility that that would get into the TSPA prior to license application?

GROSS: If I could clarify one point? Performance assessment doesn't quite know yet what's important and what's not important. The damage numbers you saw today have really just been available for the past month. So, we really have not assessed how all this fits together into a total model, what are the collateral effects people have asked about. For example, PA, performance assessment as it currently exists already adds an amplification factor in the seepage abstraction for drift degradation.

If I was told right, a few days ago, they basically increased it by 50 per cent. Right from T equals zero. Does that include catastrophic drift failure? I don't know. But certainly from modest blocks falling down, there's already a factor that they've tried to fold into that.

If you look at the damage numbers on a probability weighting, 10^{-7} sort of has a mean damage of 1 per cent. 10^{-6} has a mean damage of 2/10ths of a per cent. So, from a point of view of sort of integrated fragility, 10^{-6} is currently the more important number. But there are enough uncertainties and things that still need to be quantified that I wouldn't quite take that to the bank yet. And we haven't run this whole--these damage numbers through the total model. Other things can change in the model from what was before. Colloidal transport can change. So, we still have a fair amount of work to do before we can declare a victory.

BULLEN: Dr. Abkowitz?

ABKOWITZ: Abkowitz, Board.

But I'm concerned that you're working on a different time schedule than the TSPA process is working on, and by the time that you'd like to take your money to the bank vault, the vault might already be closed.

GROSS: Well, I think I am working on the same schedule, The things that you've seen and we're about to do over the next month or two are due to be folded into TSPA.

BULLEN: Bullen, Board.

Let me also reiterate our thanks for your sharing this preliminary data with us today, because it's very timely for us to be able to comment on it and to learn about it as it's being done. But we also appreciate the fact that it's taken a great deal of effort for you to put this together and to present it.

Dr. Boyle, you had a quick comment, I hope?

BOYLE: Right. It was to come back to the phrase that Dr. Nelson used, a moving target, and I think it related to Professor Abkowitz's remark, is that we always are getting more and more information, but with respect for the total system performance for the license application. We do have to draw a line in the sand at one point, do the calculation, while we still continue to get smarter and smarter, and get more information.

BULLEN: Professor Hendron had some questions here?

HENDRON: I had a few. With respect to Mark's presentation for the rock falls, he analyzed it for the height motions and for no supports. I would like to see him work on what the level of support is required to eliminate most of the rock falls at different values of the ground motion, 10^{-6}, 10^{-7}, and so on, and how much of a lining it takes, because it looks to me like you fellows haven't really decided on the lining yet, and you'd better get to that.

BOARD: Maybe I could just comment on this question while it's hot here. We're making the assumption that once this thing closes down, once the ventilation is stilled, which currently that is a little bit open, 50 years is what we're looking at right now, but I suppose it could be longer, maybe 150 years, that the repository would be closed down, and we're making the assumption that there would be no entry into it after that stage. So, we're making the assumption that all the ground support that was placed in at the beginning of that time will essentially go away over time.

That's not true in the preclosure one, that 5 x 10^{-4}. You're right. There, we haven't actually done work on the ground support. The only reason being that I think it's going to be very easy to prevent that from happening. I know we have to document that, but we're really not talking about heavy loads on the structure, or anything like that.

But, in the postclosure thing, 10^{-6}, 10^{-7}, those are presumably events that would happen way out in the future, at which time the repository would have been closed for many, many thousands of years, I guess. And, so, that was the logic there.

BULLEN: Did you have other questions, Dr. Hendron? Go right ahead.

HENDRON: The other thing is I've learned since I've been here that 80 per cent of the repository now is going to be in the weakest rock formation. And I was wondering how you felt about representative samples to be tested. I also would like to know if you think you've got enough information on that rock from the part of the tunnels that have been driven, because I understand that a lot of the tunnel that's in that formation now is bulkheaded up for other kinds of experiments, not civil engineering ones.

BOYLE: One quick comment to bolster what Mark said a couple times during his presentation. I really wish for those who have never seen the lower lithophysal unit and the tunnels in it could go in and see it. But on paper, you can do a calculation, and Mark said, well, it's failed, and yet the tunnel is fine. You know, even though it might, and it's really not the weakest rock we tunnelled throughout there, it's the weakest rock in the repository horizon. The other even weaker rocks, well, they're fine, too, even though they show the same, you know, failures in springline, they're failures, but they really don't mean anything to the performance of the repository. The tunnels are fine.

BOARD: About the data--

BULLEN: This is Mark Board. Go ahead.

BOARD: Okay. We aren't done with testing work we're doing. I've sort of showed you a snapshot of where we are right now. We're trying to get, you're right, we've got exposures in that cross-drift tunnel, and then also in the ESF in a couple of locations of the lithophysal rock. And we've done this in situ testing. We've done a lot of laboratory testing. But it's continuing right now. For example, this year, we're just now embarking on doing a lot of the index property tests on the materials with in situ modulus and strength measurements, and also we're going to do some seismic measurements in the tunnels to get more information.

The one heartening thing about it is is that the data that we are getting seems to be falling in line as far as the porosity goes. That plot I showed you up there which showed quite a range of values was for different porosity materials. If you plot that as a function of porosity, it seems like the more data we get seems to reinforce the same ideas. So, I'm getting more confidence in the fact that we understand more how this, you know, what the true strength values of this material is.

But, I agree with you that it's important as we continue, to excavate in this material and do more testing and gain more confidence.

BULLEN: Bullen, Board.

Actually, we have an interjection here of what the tunnel looks like. Do you want to go ahead and show us? First, you need to identify yourself. Grab a microphone. Yes, that's perfect.

BUESCH: My name is David Buesch.

The main reason to just give you a couple of pictures is we do end up seeing a few pictures along the way of what the tunnel looks like. But this is part of the, to address your question, again, I'm only going to show you a couple of them just to give you a sense, but over the past year, we have gone to the lower lithophysal zone. It's about 880 meters exposed in the cross-drift. Of that, we have looked and done this kind of mapping on 18 different locations of documented variations in the rocks with the rest of the way.

So, there's quite a bit of evidence we have of the characteristics of the rocks in the tunnels, and these kind of maps are of 1 by 3 meter areas. And the main thing I would want to show you there is the meter bar across the side there, so we get a sense of the scale of some of these things, and a couple of the lithophysae there, they're enhanced by the shadowing because of the low angle illumination.

And, so, you can see some of them are quite large. Some of them are quite irregular in shape. And we can have any number of these, and we can discuss this more off line if you want, but here's another example where the rocks are in phenomenally good shape. You know, the question of how much fracturing goes on, these we are tying in with detailed line surveys, and the detailed line surveys are going to be producing similar types of data that we've collected in the past with the Bureau of Reclamation.

And in those detailed line surveys, the detailed line surveys are all of the tunnels underground, again, the 35,000 that were recorded, plus we also have detailed small scale fractures in the crystallized rocks. And with this, this is the type of data that we've been collecting, or that the Bureau has been collecting. And the main point is in blue, these would be the kind of questions that could address some of the kind of questions you guys are asking.

We know the distribution of fractures. Mark showed one of the meter long, or greater, type of distributions. In the small scale fracture studies, every fracture, regardless of length, has been documented, and it's been documented with all of these types of information. And the lower, right-hand side here, the type of infilling, this is the kind of materials that are there, and the point that I'm trying to make with this is that we are documenting the kind of materials, like the vapor phase mineral linings.

And also one of the criteria they're asked to look for is whether or not the rocks have been brecciated, or broken. And, so, this is the kind of data that's in there. Currently, it's the kind of data that could be mined and extracted out of the datasets. But, I think they are ways to be able to look at some of this.

Thanks for your time.

BULLEN: Bullen, Board.

Thank you for bringing the mountain indoors. Dr. Nelson, do you have a quick followup?

NELSON: I'm not sure. Sorry I had to step out. I lost my glasses. So, this may have been asked while I was gone.

In discussions that we've had with our consultants, the issue about to what extent is the project having access to information regarding the Nevada Test Site activities, and to what extent could that information inform perhaps the validation of models, if not, inform more directly the consideration of seismic.

BOYLE: I'll only take a partial stab at this answer. But it goes back to Carl Stepp's presentation this morning, and listed amongst the ground motion experts, Marianne C. Waulk of Sandia National Laboratories. It's my understanding that she may have been included specifically for that reason, to bring in more expertise from what happened on the Nevada Test Site. And what's done now on a day to day basis with respect to gaining understanding from the Nevada Test Site, I'm not sure, but people did take that into account.

NELSON: There must be more things to be said, because I think from the standpoint of translating that experience into an engineering understanding of how the underground responds. I'm wondering if the door is open for that information, or not. And has it been used, or not?

BULLEN: Mark Board, go ahead.

BOARD: Well, we haven't, in my area anyway, haven't used it much, and I agree that it would be a good thing to do, and we should do it. And I believe that that information is really available. It sounds like Dr. Hendron knows a lot more about that, about the testing that has gone on there, and Dr. Kennedy I know does. And it's certainly the kind of thing that would be a good thing for us to use as sort of calibration or validation exercises for these numerical models that we're doing.

And I think the only thing we've got to make absolutely certain of is what kinds of rock materials that it was in, since at least the tunnels I worked on at the Test Site back in the Seventies, a lot of them were in non-welded ash fall tuffs that might not have quite as much relevance to what we're doing here. Plus, as far as I know, lithophysal tuff is really only found in the Topopah Spring and the Tiva Canyon units.

But, certainly, the tuffs like the Grouse Canyon, and things like that, are real similar to the Topopah Spring middle non-lith unit, and they would provide a good thing.

HENDRON: One of the reasons why I was asking about that formation is because it's not that I don't believe the tunnels may be in very good shape, and we haven't a chance to see them, but when you test materials like this in the laboratory, you get premature breaking, and so forth, you make a poor paper record, even if the formation in the field is good. And with some of the values I see in the modulus, I really think the modulus in the field is higher than what you're reporting in these documents that we've had a chance to read.

And I think part of that reason is some of the moduli back calculated out from the slot test is much too severe, and you're giving displacements of the material displacing, shear displacement into the wall, and maybe you should be doing a pull down test of a foundation in the invert of the tunnel. And I don't really believe that the moduli, and so forth, is materials as low as has been reported here in the field.

BULLEN: Comment on that? Mark Board?

BOARD: Well, I tend to agree with you, and that's why we're not really using those values at that very lowest end, although some of the slot test work that we did, we tried to do it in the floor of the drift where the stresses are low, and we have less, getting back to, you know, previous loading. We've also done plate bearing tests in the material, which I don't show there.

But, yeah, I think we're certainly planning this year, we're doing another--well, not this year, starting within the next few months, we're doing a whole series if modulus tests with different techniques in the material to try and sort that issue out.

The only thing I can say to try and sort it out is that we have used a whole range of values across that spectrum that I showed from the testing work to try and make certain that we didn't have some fundamentally different response based on what the selection of those values were. And, thus far, we haven't. The place it makes the most effect is is in the thermal stress calculation.

BULLEN: Skip Hendron, go ahead.

HENDRON: Yes, we've heard about these cans banging into each other, and so forth, at different levels of the motions. At what point do you just eliminate speculating and just tie them down as far as a design that's concerned?

BOYLE: I'm afraid there's not really a single designer up here to--

BULLEN: Well, there's one that's coming to the microphone.

BOYLE: Mike Anderson to the rescue.

ANDERSON: Mike Anderson.

The problem with tying things down is, for instance, if you tie the waste packages down, whatever you use to tie them down with creates a crevice, which enhances corrosion. And the question comes up how long is that going to tie it down, and what you're going to tie it to? You know, you drill things into the rock, the rock will break eventually and come out. You know, when you talk tens of thousands of years, it's quite a challenge to ensure with a great deal of confidence that it's going to stay there, which is very similar to the whole issue of lining the drifts. You know, it's fine to say yeah, that's a good solution. But to prove that it's going to last all that time, that's another matter all told.

BULLEN: Bullen, Board. I want the franchise on the C-22 tie downs. Okay?

Dr. King, you had a question or comment?

KING: Just a comment. One of the traps or mistakes we don't want to fall into is to take precipitous actions as a result of some of the indications that we're getting from the analyses of these extreme ground motions. It will be a mistake to sub-optimize the design in the repository and do something like a tie-down that might create or might unnecessarily complicate the operations, perhaps even degrade optimal performance, to preclude a scenario which is based on ground motions that we all agree are not physically realizable in the first place.

So, we just have to be careful about, you know, taking design decisions to preclude things that probably will never happen.

BULLEN: Bullen, Board.

Thank you for answering Question 4 about alternative approaches to seismic design by saying that we have very low probability of high consequence events, for which we don't want to design, over design the repository--excuse me--over constrain the repository.

PARIZEK: Dan, you didn't mean not to separate the packages, though, did you?

BULLEN: That was Richard Parizek. It had to do with thermal as opposed to mechanical. Arthur, did you have a comment or question you wanted to make? Go ahead.

MC GARR: Just a quick one. This is with regard to Point 2. McGarr, consultant.

The alternative approaches to capping the ground motion, so to speak, have been based mostly on local side effects or observations, such as precarious boulders, and whether the rock nearby is shattered or not.

Ivan Wong briefly alluded to the source part of the equation, you know, in determining the ground motion, there's the source, there's the propagation, and then there's the local side effect. And I think today we've been emphasizing the local conditions of the repository far more than the source.

Ivan suggested that these high improbable ground motions were associated with exceedingly high stress drops, something like--something astronomically high compared to our everyday experience with stress drops in any case.

We do have a much better ground motion dataset and means to analyze earthquake sources than we used to, and I think it would be possible to make some arguments about the source and the possibility, and the limitations associated with the strength of the crust, especially in an extensional environment that might also be an effective way to cap ground motion. I don't know how receptive the Board is to that, but it's an approach that I'm personally quite interested in.

BULLEN: Comments from the roundtable participants? A wholesale endorsement maybe?

BRUNE: Jim Brune. I will endorse that. I've spent a lot of time thinking about source physics, and it is sort of left out of everything we've been saying. Although indirectly it could be tied, precarious rocks tell you something about source physics on the San Andreas Fault, because you have a hundred times as many events there, and they're telling you about dynamic stress stops and stress drops. And, so, yeah, I want to second what Art said.

BULLEN: Other comments from roundtable participants? Dr. Silva?

SILVA: Walt Silva, BSC.

That is planned in the current task in terms of the three approaches to the saturation or fuzzy bound on the ground motions, is to look at the source and sort of limiting the stress drop, and also finite source as well.

BULLEN: This is Bullen, Board, with a two minute warning, because I'm going to turn this back over to Madam Chairwoman in about two minutes.

But, other comments or questions from, first, Board members? Okay, seeing none--

NELSON: Wait.

BULLEN: Oh, Dr. Nelson, I'm sorry.

NELSON: This is the last thing. I really would be delighted to hear about the plans that the project might have to get into the Nevada Test Site source of information, because I think it's more than just the dynamics. It's the condition of the reinforcement that may be in there. There may be some issues of, I don't know, like colloids and seepage or migration, and other things that could be. You may think of it as an analog, and nothing more. But, it seems that it would be interesting to know.

BULLEN: Dr. Boyle, go ahead.

BOYLE: Yeah, with respect to the tunnels, you know, Mark is here and he's heard the suggestions, but for other analogous in the Nevada Test Site, we are aware of them. Some of the same geochemists that work for Livermore on the Test Site, you know, the Bennem shot and how did plutonium move so quickly, you know, those same people work for us now and then. Ardyth Simmons, Abe Van Luik, and others, follow the work of, you know, transport as an analog, you know, the

Nevada Test Site.

NELSON: Well, but just in closing, it seems like I as a Board member have no sense of the fullness of that tie into the Nevada Test Site information. So, it might be interesting to address that in future Board meetings.

BULLEN: Point well taken.

NELSON: Am I wrong? Do you guys know?

BULLEN: Point well taken, Madam Chairman.

Seeing no other comments either from Staff, who said that we've run out of time, I would like to close my session on time, and turn the meeting back over to the Chairman, Dr. Nelson.

Before they go, I would like to express my appreciation to the roundtable participants. Thank you very much for entering the inquisition and for providing your personal opinions.

(Applause.)

BULLEN: I didn't think we applauded at these things.

NELSON: No, but they did a nice job.

BULLEN: They did a nice job. Thank you very much. And if you could turn that off, and I turn the meeting back over to Dr. Nelson.

NELSON: Okay. Yes, I want to personally thank you, and to also indicate very firmly that the reason that my sessions were late had to do with run on sentences that certain Board members asked.

And as a final, final, final question, we have reached the end of a very interesting Board meeting. I have learned a lot, and I thank the project for the level of effort and their professionalism in bringing all the information to bear.

We have three people who have signed up for the public comment, and we have time for I think about five minutes seems to be a good time to get the essence of the message across.

So, I would like to--the order that people signed up was Grant Hudlow, Jacob Paz, and Sally Devlin. So, barring any other reason, we can go in the order of sign-ups, and ask Grant Hudlow to come to the podium.

HUDLOW: I'm Grant Hudlow. I think most of you know me.

I'm concerned that I'm not hearing some things that I think need to be addressed. I mention briefly seepage. And the area, Yucca Mountain has many, many thousands of tremors every day. What is the effect of all of this jiggling on the seepage? I think I haven't heard anybody even address that.

The other thing is that in the assumption in one of the talks that titanium was going to lose 2 millimeters in 10,000 years from industrial work in the chemical industry, I can assure you that neither the titanium nor the Alloy 22 are going to be around in 10,000 years. In fact, nobody will be able to detect that they were ever in the mountain.

Alloy 22 was developed in the chemical industry as a cheap alternative for the good stuff. It has to be replaced every year for two reasons. It has nickel in it, which the microbes like. And also, nickel forms a carbonyl and evaporates. And while nobody in the government labs have noticed that, we noticed it in the chemical industry back in the Fifties.

The other reason is it has over 5 per cent chrome in it, and over 5 per cent chrome then is susceptible to chloride stress cracking, and it walks right through it, and in the use in the chemical industry is higher temperature and higher pressure, although the pressure may be disputable in this case, but it will walk right through it in two to six months, Alloy 22.

So, you can't use it at all in those applications. And where we do use it, we replace it every year, because it won't hold.

Somebody asked are you open to this kind of information, and the answer for the last several years has been obviously no. And it isn't also so much a question of are you open, it's can you possibly find somebody that can get this information. I think it appeared in the American Chemical Society Journal in about 1975. That's the only reference I remember to it.

Every chemical engineer, every chemist, every technician in the chemical industry knows this information inside and out. The government labs do not. They've never heard of it, except when I explained it to them in Sandia, and they were so embarrassed at what I used to shoot the project out, that it never got into the database. So, that's a serious problem that you're ignoring.

The other problem comes in are you open to people from the chemical industry that deal with this kind of material all the time, are you open to even having them discuss with you what is the effect of all of these various conditions on this material. The Nevada chemists pointed out essentially that you have aparegia that's going to be on these packages.

So, what do you use to contain aparegia? That's a no brainer. I had some of it just sitting around the lab in a milk bottle, polyethylene milk bottle. Is that a big deal to stop the corrosion? You could even use polyethylene, unless we're going to talk about higher temperatures, and then you use a flame sprayed ceramic on there, common ordinary stuff in the chemical industry that the government labs have never heard of, and not only are not open to it, they don't have the skills to go get this kind of information.

NELSON: Thank you very much.

I note that Dr. Sam Armijo has also signed up, and we will place him after Sally Devlin on the list. So, the second public comment to be made is by Jacob Paz. Dr. Paz?

(No response.)

NELSON: Well, then, Sally? Ms. Sally Devlin is on. Thank you, Sally.

DEVLIN: Thank you very much. Madam Chairman, Members of the Board, and members of the audience, it's always a pleasure to welcome you to Nevada. And I am Sally Devlin from Pahrump, Nye County, Nevada, where the repository is located, and I hope everybody remembers that, because I will remind you of it until the day I die, which reminds me I am so glad to be here because I'm not dead yet, and I'm so glad to see so many familiar faces, because they're not dead yet either.

And, of course, we're talking about something that's supposed to last 10,000 years, and that for you new people, Abe and I will be sitting playing gin rummy for 150 to 300 years, because there is no funding for stewardship. So, always remember that.

The other thing I'm terribly sensitive to language, being from the home county, and that is when I hear your language, and everybody knows I'm a toastmaster, and when you say, "When the repository is going to be filled, and how the repository," I take great offense. It is not a done deal, and I hope it is never a done deal, and I hope you all model until the day you die, I die, and Abe and I die playing gin rummy.

So, remember our point of view. I don't feel, and as I say, when I hear my favorite terms, the colloidal modelling, we introduced this in '95, along with the bugs. And if anybody doesn't know, they'd better know now, it's Sally's bugs, and I introduced microbial invasion to this Board. And my bugs will eat anything anybody can make in this entire world, and I will bring with me tomorrow for anybody who would like to see 60 pages from Dr. Bond from Livermore on how my bugs will eat the canisters.

And I will remind everybody one more thing, I know we're all tired, and that is that we are talking 70,000 metric tons of high-level waste. We are talking 7,000 metric tons of DOD waste. And I will say, as I always do, you cannot put classified waste in my mountain.

Now, how do all you scientists know what is in those canisters that isn't going to blow up, and my neighbor sitting here, I love him, but Jerry King, he says nothing is there to blow. The whole place can blow. We're talking sabotage, terrorism, corrosion, we don't know what, because we don't know what's in the mountain.

We also very carefully and have no testing, and

I've talked to Sandia and will talk more about it tomorrow, on the canisters, because you have no models. Now, I think this has been a revelation to learn about all your seismic experiments, and learning, and so on, and I'm so happy that Bill Boyle really came through. I was delighted to hear him speak, and Mark, I'm very, very impressed with the science, because we do shimmy and shake in Pahrump an awful lot. I have had subsidence. I have had broken patios, and so on, from the earthquakes, and we get them all the time.

When I lived in Reno, all the time with the earthquakes, and of course the houses were floating into the Truckee. So, we are in the third most seismic area in the nation, California, Alaska, and now Nevada, and I don't think we have fully anticipated the potential for earthquakes. And whether we have small ones or large ones, they still take their toll, and they do have their effect.

And since we're talking about the mine, as I call it, and it is 12 kilometers, or five miles, that's a very small portion of what will be, and we must consider this.

I read Les Bradshaw's report on storing just a small portion of the mine, with maybe 100 canisters, and self-circulating air. I was appalled. None of this stuff is being done yet, and it will take years. So, I think we're very much in advance, or behind, or whatever. We're all learning, and that's the fun of it.

So, I will end on that. We will learn more tomorrow from NRC. But I do want to remind you all where the mountain is, how large the mountain is, and what the potential is for seismic activity. I don't really feel, you've dug a little further, and that you're going to get it all together and it's going to take many years, not just of modelling, but of actual placement in the mine, and see how these things shimmy and shake.

And, with that, everybody shimmy and shake to a nice dinner, and thank you so much for coming.

NELSON: Thank you, Sally Devlin.

And our final public comment will be from Dr. Sam Armijo from the University of Nevada at Reno.

ARMIJO: Thank you, Madam Chairman.

My name is Sam Armijo. I know a few of the people here, but this is my first opportunity to attend a Board meeting. And, first of all, I'd like to tell you that I'm very impressed with the work that was presented here, scientifically very thorough. And as much as I was impressed, I am dismayed by the inability to close on some of the conservatisms.

I think Dr. Gross made a list of notable conservatisms that do affect both the schedule and the cost and the performance assessment. And I think that's the tip of the iceberg. I think a number of conservatisms are buried in these analyses and in these models, and while they may be unimportant in the final dose, since it's a probabilistic dose to the public is what's important, I believe they have a huge effect on the cost of this project.

And at $57, $58 billion dollars now, I'd just like to remind the contractors, the Board, that we could build 15 to 20, 1,300 megawatt nuclear reactors that operate at 2,000 psi, and at high temperature, with thin walls or chromium cladding that last five, ten years under extremely aggressive environments, and operate safely, yet we're trying to emplace waste, spent fuel, which is in a passive--is passive, it's not operating, it's low temperature, it's low pressure, and something is out of kilter and I would just like to urge the Board and the contractors to really look at their conservatisms very, very carefully.

I think some of these conservatisms, while engineers realize there's plenty of margin, the general public, as you may have just heard, gets alarmed by statements like we really don't know, when in fact we do know a lot. And I'm just a little bit worried that we're giving the wrong impression to the general public that what we don't know is really a fundamental problem with this project.

So, again, I'm very impressed with the work, and I really appreciate the opportunity to be here. Thank you.

NELSON: Thank you very much.

That concludes the public comment period, unless there's someone else who we have missed. No? Well, I thank you very much.

Two points at the moment. It is now ten of 6:00. At 6:30, we will reconvene next door, everybody in the front of the room, and do a debriefing.

I want to give an opportunity for, Bill, do you want to introduce tomorrow's meeting? Who wants to do that? Somebody. You don't want me to do it.

This is Dan Fehringer.

FEHRINGER: Yes, Dan Fehringer.

Tomorrow's meeting will cover the operation of the waste management system, starting with waste acceptance at power plants, through transportation to the Yucca Mountain site, and then through the operations of the surface facilities, and the underground emplacement of the material for final disposal.

The agenda is approximately evenly divided between the Department of Energy presenting its proposals, and our reception of the views of the people affected by those. We have a presentation by the nuclear power industry about their experiences in transporting materials. And then we have a number of representatives from affected units of government who will be telling us their views on the project.

That begins at 8:00, and it will be here same time, same place.

NELSON: Thank you very much. We are adjourned for the day.

(Whereupon, at 5:50 p.m., the meeting was adjourned.)