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NUCLEAR WASTE TECHNICAL REVIEW BOARD

STRUCTURAL GEOLOGY & GEOENGINEERING
PANEL MEETING

SEISMIC VULNERABILITIES

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Hyatt Regency Hotel
17900 Jamboree Boulevard
Irvine, California 92714

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman
Nuclear Waste Technical Review Board

Dr. Clarence R. Allen, Chair
Structural Geology & Geoengineering Panel

Dr. Leon Reiter, Nuclear Waste Technical
Review Board, Senior Professional Staff

Mr. Russell McFarland, Nuclear Waste Technical
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Dr. Edward J. Cording, Consultant

Dr. Robert Kennedy, Consultant

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PROCEEDINGS

(8:30 a.m.)

DR. DEERE: Good morning, ladies and gentlemen. I'm Don Deere, Chairman of the Nuclear Waste Technical Review Board and a member of the Panel on Structural Geology & Geoengineering.

Welcome to the Board's first panel meeting of 1992. The Nuclear Waste Technical Review Board has four meetings of the full Board per year. The winter meeting was just held two weeks ago today and I'm sure many of you may have been there. It was held in Arlington, Virginia. The spring meeting will be held in Dallas from April 7 to 8.

However, most of the Board's work is actually done through panels in topical meetings, such as this. There are seven panels in total and each meets from one to perhaps four times per year. At the panel meetings, DOE and their contractors and collaborators present their studies, findings, and future plans, and answer questions from the Board, from other interested persons in the audience, and enter into discussions. Speakers from other organizations are often invited to speak to specific topics and we certainly wish to acknowledge the considerable effort that all of the scientists and engineers make in preparing their remarks and their slides to present to the Board and to the rest of the community. We often have invited speakers from NRC, EPA, State of Nevada, the utility industry, and, occasionally, environmental organizations and others.

I wish to thank all of you who are making presentations and are going to enter into the discussions today and tell you that the Board certainly does appreciate this opportunity of exchanging information. It is the main way, together with your reports, that allow us to make our evaluation and report our findings to Congress and to the Secretary of Energy.

Before I turn the meeting over to Dr. Clarence Allen, Chairman of the Panel on Structural Geology & Geoengineering, I'd like to remind all speakers to speak directly into the microphone clearly and to identify themselves and their affiliations.

Clarence?

DR. ALLEN: Thank you, Don. First, let me introduce, in addition to Don and myself who are the only two members of the Board here, our consultants who are with us today, as well as our staff members. Ed Cording is with us today. He's been with us many times before with this panel. And, for the first time, Bob Kennedy is with us as a consultant to the panel. Bob is internationally known for his work on vulnerability analyses. Our staff members include Leon Reiter, Russ McFarland sitting over there in the first row, and Bill Barnard was to have been with us, but was recalled and had to go back to Washington early this morning.

The purpose of our meeting today and tomorrow is to further explore the issues of potential seismicity at Yucca Mountain with special, but not sole emphasis on the consequences of earthquakes. That is the seismic vulnerabilities of both the proposed underground and surface facilities. We will thus be paying particular attention to the engineering of these facilities in addition to the physical natures of the hazards themselves which have heretofore been the primary focus of our attention.

You will recall that in its second report, the Nuclear Waste Technical Review Board stated that the mere presence of active faults near, and even within, the Yucca Mountain block should "by no means imply that the site is necessarily unsuitable. Suitability should be judged on the basis of potential risk (the likelihood of such adverse consequences as the release of radionuclides to the accessible environment), not just on the potential occurrence of a natural phenomenon alone, such as earthquake ground motion or fault displacement, independent of their consequences. . . . In this light, the Board suggests it would be wise at this early stage in the site investigations to assume that relatively large local earthquakes may occur during both the pre- and postclosure periods of the repository life and to investigate the engineering and safety consequence of such events."

This is the framework in which much of the present panel meeting has been planned, and it is our hope to hear relevant responses from a variety of groups and individuals during the next two days.

The Technical Review Board is among those who have urged the Department of Energy to retain flexibility in its planning for the proposed repository as long as possible and not to become locked-in too early to plans which later may turn out to have been unwise. From its earliest days, the Board has also urged the DOE to use more of a systems approach in its planning than it has in the past, whereby final decisions will be made in the light of optimizing the entire operation, rather than considering individual elements such as seismic risk independently of other concerns.

Let me give just two examples how such a systems approach might radically affect our consideration of seismic risk. The current reference or baseline design calls for transfer of spent fuel from transportation casks to disposal containers at the surface loading facility. However, also under consideration is a universal cask which would be used for both functions, thereby eliminating the need for a hot cell and greatly reducing the seismic exposure at the loading facility.

Secondly, the vulnerability of the stored spent fuel to underground fault displacement could be a function of the method of canister emplacement within the bedrock of Yucca Mountain. The current reference design calls for emplacement in vertical boreholes, only slightly larger than the canisters themselves, drilled into the floors of the branch tunnels. But, ongoing studies of optimal thermal loading and engineered barriers leave open the possibility of storing the canisters in different configurations, such as in the much larger branch tunnels themselves, with or without backfill. Such schemes might have a significant impact on the canisters' ability to withstand local fault displacements, although this could be open to question.

In any event seismic risk obviously cannot be considered independently of other factors in the overall system, such as thermal loading, tunnel configuration, the nature of the engineered barriers, transportation systems, et cetera. It must be an integral part of a total systems approach.

Some topics less directly related to seismic vulnerability are on the agenda for tomorrow, such as the likelihood of new faulting, ongoing relevant activities by the NRC, EPRI, and the ASCE. But, in the long run, we emphasize that it is the seismic risk and not the hazard alone that should determine whether or not the Yucca Mountain site is indeed suitable from the seismic point of view, and this is where our attention must ultimately focus.

I was most impressed at the recent ACNW meeting with the informality and the give-and-take between participants. I hope we can achieve some of the same spirit here, although I must remind you, as did Don, that this meeting is being recorded and you must identify yourself when you step to the microphone.

At the termination of the formal part of the meeting tomorrow afternoon, we will have an informal roundtable wherein the various speakers will face each other and the audience, and have at it. During the meeting, I will be attempting to formulate some provocative questions that can be considered at that time, and I hope all of you will be thinking in somewhat the same way. We do not expect, needless to say, to reach any formal conclusions or recommendations from this meeting alone, but we do hope that the most critical seismic vulnerability issues will surface and will be thoroughly aired.

With that, let us proceed to the first speaker, who will be Ardyth Simmons of the Department of Energy. Ardyth?

DR. SIMMONS: Thank you, Dr. Allen, and good morning, everyone.

Carl Gertz and Max Blanchard were sorry that they could not be here for this meeting today. So, the Department of Energy will be represented at this meeting by me and by

Tim Sullivan who I would like to introduce now. Tim is the DOE manager in the Tectonics Program and has had extensive experience previous to coming to this program with the Bureau of Reclamation in the siting of dams and the investigation of seismic hazard with respect to that. In addition, we have with us John Whitney of the USGS who is head of the Tectonics Program for the Yucca Mountain Project and we have additional speakers here who have technical expertise and I will introduce them as their presentations come up.

As Dr. Allen correctly pointed out, our previous discussions with the Board have been primarily in the area of seismic hazards, both faulting and ground motion. The last panel meeting that we had with the Board on this subject was in April of 1990 and it accomplished several things. At that meeting, we presented the current knowledge of seismic hazards that we had to date. We also presented the conceptual seismic design basis that was described in the SCP and a summary of probabilistic assessments that had been done to date. In addition, there was a detailed presentation on the preliminary cost benefit analysis for the waste handling facilities which had been conducted by Sandia and Bechtel. And, finally, we proposed an approach for further development of the seismic design basis.

Since April of 1990, repository design has not been an emphasis of our program and, therefore, it has received rather minimal funding and attention. So, we have not done a lot of new work in the area of seismic design. In addition, we have also experienced a major shift in personnel in 1991 in the area of the Tectonics Program.

However, we have made some progress and there are a number of activities that you'll be hearing a bit more about today in our presentations. The first is that a technical assessment review or TAR is being initiated to evaluate the seismic design basis for the exploratory studies facility as a part of the Title II design. And, this technical assessment review will consider the items, that is the structure facility and components not important to safety.

Furthermore, a report was prepared by SAIC as an early site suitability evaluation and this has been received by the DOE for review and is under review right now. The report recommended that based on knowledge we have to date for faulting and ground motion for both pre- and postclosure tectonics that the site would not be disqualified on this basis. And, we will address this a bit more as the presentations go on today.

Finally, although we feel that seismic hazard is not of concern for a determination of site suitability at this time, it is a good topic for issue resolution. And, to that end, DOE has convened a working group in the area of seismic hazard tectonics to establish and write a topical report that would be later submitted for issue resolution. And, it is the additional purpose of this group to evaluate new data as it comes in, as it's collected by investigators in tectonics, and to provide recommendations for focusing site characterization studies.

The purpose of today's presentations from the Department of Energy are multiple. First, we will be discussing new data that has been collected in the past year and we will also rely on some additional expertise that has provided knowledge from nuclear testing that's been conducted at the Nevada Test Site and damage that's been done to the tunnels there. Also, experience that's been gained from the nuclear power industry.

For the purpose of this meeting, we were able to use this information in a preliminary sense to evaluate seismic vulnerabilities for both the surface and the underground proposed facilities for pre- and postclosure. And, we will be considering in our discussions how the knowledge of seismic vulnerabilities can be used to focus this aspect of the site characterization program.

In our presentations, we will be suggesting possible methods that can be used to reduce seismic vulnerabilities of structures and we will through these discussions be showing that the seismic risks, that is the radiologic risks, to the public by both the surface and underground facilities would be low.

And, finally, this is a good opportunity for us to introduce the new investigators that we have had this year beginning in our program. And, I will introduce them as their presentations come along.

Reiterating very quickly, the first part of our meeting on the agenda today will be the summary of seismic hazards to date and the new information that we've collected with a consideration of multiple event scenarios, as well. We will hear an update on the work that's been done, both mapping and trenching, in Midway Valley this year and we will consider the work that was done on the effects of nuclear tests on tunnels at the test site. From there, we will go into discussions about the seismic vulnerabilities and design issues for both the surface facilities and for subsurface excavation. And then, finally, we will put this information together to see how it can be used to drive or focus certain studies in the site characterization program.

If there are questions, I can entertain them now. Otherwise, I will introduce our first speaker.

DR. ALLEN: Thank you, Ardyth.

Let me ask if there are questions from the Board itself or from anyone in the audience.

Leon Reiter?

DR. REITER: Ardyth, I wonder if you could--we've heard this issue resolution word before. Is it a formal process that you go through and what happens after you do it? Could you give us some background on that?

DR. SIMMONS: Okay. We're just starting to develop a strategy, if you like, for resolution of various issues that have been defined as being of greatest concern. And, so far, a list has been identified of about eight different issues. What we propose is to prepare what is known as a topical report on those issues that would clarify a methodology, primarily, that would be used to demonstrate that we are able, through the process of site characterization and up to licensing, to obtain the information that we need and to reach a resolution of those issues.

The report, itself, would not be something that would say here is all the data and we now consider that it's closed, that the issue is closed. It's primarily a preparation of the methodology by which we would go through the period up to licensing to demonstrate that we can provide the necessary information. And then, I believe it depends on the individual issue, but I know that in the case of this seismic hazard issue resolution we plan to meet with the NRC to get their comments on the process and eventually ask for their support of our methodology.

DR. ALLEN: Okay. Thank you, Ardyth. Go ahead?

DR. SIMMONS: Okay. Our first speaker then will be

Terry Grant who will summarize the data that we have, so far, in the area of seismic hazards. Terry is the Senior Geologist with SAIC and he's one of the people who has been part of the program for a long time and will continue to work in this area.

MR. GRANT: Good morning. As Ardyth mentioned, I'm basically going to give a rather brief summary of what we believe the seismic hazards are or what the range of hazards may be in the Yucca Mountain area. As a lead in to the following speakers who will then discuss the vulnerabilities of various repository facilities and structures to seismic hazards. So, this will be an introduction just to go over what we believe the hazards may be in the Yucca Mountain area.

I divided the talk into four areas. I'll discuss in order preclosure ground motion, preclosure faulting hazards, and then postclosure ground motion and postclosure faulting hazards.

I'll start with the ground motion preclosure. Basically, I'm going to give a different talk than we've given in the past. I'm not going to talk about design basis or how we calculated design basis. I'm merely going to talk about bounding conditions; that is the worst case conditions that would be of interest for you in discussing seismic vulnerabilities, the succeeding papers.

For this part of the talk, I'll be referring to this slide here. Basically, what I've done is taken several publications that are based mostly on remote sensing and identified the major faults that we are concerned about in the area for the purposes of this talk. Now, these are rather generalized at this scale, but it gives us a basis for looking at the location and amount of hazard that we are facing. To go from faults to ground motion or earthquakes, I've prepared a series of relationships using historic basin and range earthquake data that relate fault length to seismic moment.

The first of the several relations I'm showing here, this relates length of fault zone. So, this is the zone of rupture that occurs related to seismic moment. I've also done something a little different in that I've related cumulative fault length to seismic moment. So, this is the cumulative length of ruptures whether they occur in parallel or form a long zone. The reason for doing this is to be able to evaluate the size of earthquakes that may occur or may involve rupture on several faults that are parallel to one another, rather than just a single fault forming along the zone.

There are differences. For instance, this earthquake right here happens to be Hebgen Lake and it has a rather short zone length, but when you consider the total length, the cumulative length, of the ruptures, it moves over into this area here. So, that will be my basis for predicting earthquake size in the succeeding slides. I'll also use maximum displacement related to seismic moment for predicting earthquake size in the succeeding slides.

I'll go through several different scenarios or models on the way earthquakes may occur in the area. The first one I'll talk about is the one that's been talked about most often and that's movement occurring on individual faults without any co-seismic movement occurring on any of the other faults, so that each of the faults is considered to behave independently of the other. And, I've divided the faults into four groups that I'll talk about; Paintbrush Canyon, Solitario Canyon, Windy Wash-Fatigue Wash, and finally across Crater Flat, the Bare Mountain Fault. So, using the relationships that I put up previously, I estimate the zone length, or a cumulative length in some cases, of these faults and used that to predict seismic moment and a moment magnitude.

For the three faults or fault zones actually in the Yucca Mountain area, I come up with maximum events or full rupture length events, at any rate, of up to about moment magnitude 6.8. I've also used displacement--that's 1.5 and it didn't come through on the viewgraph--which is larger than anything that John Whitney has currently found in his studies out there as far as single event offsets as a maximum offset and get about the same result, 6.8.

For Bare Mountain, if you look at the full length rupture, 23 kilometers, you get an event of up to about moment magnitude 6.5. However, if you look at Merith Reheis' work, she postulates that there may have been a holocene event on the fault with up to 1-3/4 meters of offset which would be larger than would be apparently accounted for by the length of the fault. So, we seem to have possibly some fault length missing in that relationship.

And, that leads into my second scenario which is that movement may occur on more than one fault at the same time. There is some evidence that faulting has occurred on several faults that at approximately the same time. Many of the faults in the area have evidence for a holocene event. Also, many of the faults in the Yucca Mountain area have ash deposits within the fault zone that have been hypothesized to indicate that the faults have all moved at the same time.

For this case then, what I've done is assume that the full length of the Bare Mountain fault ruptures and at the same time we have rupture occurring on the faults in the Yucca Mountain area. I used two cases where I arbitrarily selected 10 kilometers and 15 kilometers each for each of the three faults, used the cumulative length in the cumulative curve I showed, and come up with moment magnitudes of around magnitude 7. So, this is another variation on possible occurrence of earthquakes in the area.

A third scenario or model would be that the faults in the Yucca Mountain area are related to Walker-Lane strike-slip faulting. On this viewgraph then, I'm hypothesizing that Crater Flat and Yucca Mountain are actually involved as part of a larger strike-slip system, what has been called the State Line Fault which is known in Pahrump Valley and Stewart Valley as a significant strike-slip fault. And, for the purposes of this discussion, I assume that the fault may continue across the Amargosa Desert into Crater Flat and that Crater Flat is really a pull-apart basin--say, similar to Fish Lake Valley at the northern end of the Furnace Creek Fault.

I was first intrigued by this possibility on a field trip by Tom Sawyer a couple of years ago in Fish Lake Valley where he showed this faulting pattern at the north end of Fish Lake Valley where the fault dies out. And, what struck me was the similarity to Merith Reheis' map on the Bare Mountain Fault, particularly looking at these features in here and features up at the top of the map here. Basically, the idea is similar to an idea by Lauren Wright put forward in a paper a couple of years ago that you have a significant strike-slip fault here at the State Line Fault and pull-apart basins in Pahrump Valley and also a large pull-apart type basin extending through the Amargosa Desert and up into Crater Flat.

For the purposes of this presentation, I basically just assume that there's a fault extending from Bare Mountain. We know the Bare Mountain Fault is mapped to cross the hills at the southern end of Crater Flat and exit Crater Flat, but is not mapped any further south than Highway 95. However, this area here is basically covered by very young eolian and alluvial deposits. So, there may be some connection extending down through this area here. For the purposes of coming up with some bounding values, I assume that the entire length shown in yellow there represents a single event rupture and that basically down in Ash Meadows you have a segment boundary separating that from the rest of the strike-slip system that's in Stewart Valley and Pahrump Valley off the map to the south. When you do that, you can postulate earthquakes up to moment magnitude of about 7-1/4.

You'll notice that I did not involve the Yucca Mountain faults directly. So, again, looking at the apparent synchroneity (sic) in fault movement, another possibility for movement on these faults is that they don't occur at exactly the same time, but movement occurs on several faults as a sequence of events that are relatively closely spaced in time. My model for that would be the Dixie Valley Earthquake where in July and August there were two events of moderate magnitude that were separated by about a month that occurred over the range in the Carson-Sink area, and then about four or five months later in December, we had two main events, two larger events that were separated by about four minutes, the main Dixie Valley and Fairview Peak Earthquakes. So, it's possible that you could see the same pattern in the Yucca Mountain area; that is movement occurs, say, on the Bare Mountain Fault, followed or preceded closely in time by events occurring on individual faults in the Yucca Mountain area, so that you have a sequence of events.

Lastly, it's also been suggested that the Cedar Mountain Earthquake of 1932 represents an analog to the Yucca Mountain area where the distributed faulting that was in evidence in 1932 would be postulated to occur as distributed faulting along the several faults in the Yucca Mountain area and that could also get a large earthquake by that mechanism. To date, we haven't found the amount of offset in a single event that would indicate that this has actually happened. Craig Depolo indicates about two meters of offset in the Cedar Mountain Earthquake and looking at historical earthquakes, the relationship between maximum displacement and earthquake size seems to be fairly good. So, we would expect that if this was the case, we would see somewhere in the Yucca Mountain area evidence for a large single event offset. To date, we haven't seen that, although it's still possible that some strike-slip faulting has yet to be detected. In any case, the Cedar Mountain Earthquake was a moment magnitude event of about 6.8 or 6.9. So, it falls in the range of the bounding conditions that I am proposing for considering seismic vulnerabilities.

In summary, bounding worst case events appear to be in the range of moment magnitude 7 or 7-1/4 and that these events would occur on faults that are very close to site facilities, so that these facilities would be in the near field for these events. Jay Merritt will talk about the impacts of ground motion on underground facilities and Phil Richter will talk about impacts of ground motion on the surface facilities in the following talks.

Given these bounding events, it's important to note that the probability of these events occurred or the exceedance probabilities for these events is quite low. As a rough estimate, we'd say into 10^{-5} to $4x10^{-6}$ range. This is a result of the very long recurrence intervals and very low slip rates that occur on these faults. So, when Phil Richter talks, he will place those kinds of numbers in context with current engineering practice for other facilities.

DR. ALLEN: Excuse me, Terry. Where do those probabilities come from?

MR. GRANT: I estimated them basically just assuming the standard Gutenberg-Richter relationship for occurrence of the faults.

DR. ALLEN: Based on the local sites, it must be during the last few years?

MR. GRANT: No, based on the slip rate and using a moment release rate.

DR. ALLEN: Oh, slip rates on the local--

MR. GRANT: Right. I wouldn't follow the exact numbers very closely. They're just a rough estimate. I don't want you to focus on their numbers as consisting of a precise estimate of the return, but it would appear for an event this large, you would have to get up into that range since the probability of just having an event would be in probably the 10^{-4} range.

DR. REITER: These are annual probabilities, right?

MR. GRANT: Right.

And, finally, that we may not have just a single earthquake occurring, but we may find that when an event occurs, it actually occurs as a series of events on several faults with time intervals between events as short as a few minutes or as long as several months.

All right. Moving on to preclosure surface fault rupture, we have two areas of concern, the surface facilities and the underground facilities. Starting with the bottom bullet first, the current designs for the repository, such as the location for the north ramp, indicate that faults with a potential for movement will be crossed by these facilities; namely, the Bow Ridge Fault will be crossed by the north ramp. Although the possibility of movement is not great, these faults are not all that active, there is some potential then for movement on the order of maybe 10 to 15 centimeters in the preclosure period. Jay Merritt will be talking about the vulnerabilities and design of underground facilities through fault movement as part of his talk.

I'm going to talk about the surface facilities very briefly since Bert Swan will follow me and give some more detailed information on the current studies in Midway Valley. Basically, all I'm going to say is that we don't have the main trenching facility that we're going to use. Basically, our primary strategy for the surface facility is to avoid known faults where they occur and place our facilities in the area where we believe faulting is absent. Our method of doing this will be to excavate long trenches across the facility location to demonstrate the presence or absence of faulting. As I said, we haven't excavated these trenches yet, but when we do, they can be quite extensive.

I don't know how well that will show up. This is an aerial view of a project I worked on several years ago where we wound up with about 5,000 feet of trench for basically the same purpose, looking at surface faulting. The point I want to make in this talk--this is actually from that other project--is that our main concern then is locating any faults that are of interest. And, this resolves itself into a concern with first detecting the fault and that in your trenching study you need to have the stratification available--that is marker units or marker horizons--that are distinct enough so that you can detect offset. In Midway Valley, this will be then you need to look at the alluvial units in the valley and the soils that are formed on those units as marker horizons for detecting faulting.

Secondly, the concern is with the age of the unit that you're looking at. Obviously, if you're looking at very young units, you may be missing events that have a longer recurrence interval than the age of the units you're looking at.

Basically, in Midway Valley, we believe that site characterization, trenching studies can provide the necessary confidence in siting surface facilities. The degree of this confidence will be a measure of the age of the units which we believe will be several hundred thousand years old that are exposed in the trenches. That is we'll be detecting any event that might have occurred with an annual probability or exceedance probability of 10^{-5} or 10^{-6}. Phil Richter will be talking about surface design considerations with respect to surface faulting and what we might do about them as far as if special designs are necessary.

Moving along to postclosure ground motion, basically the faults haven't changed any, but our area of concern has changed. Our primary concern will be with the engineered barrier system and the performance of the system through time. And, the other factor that needs to be considered are the long performance period during the postclosure. We actually have two periods; the 300 to 1,000 year substantially complete containment period and then the full 10,000 years where we're interested in controlled releases from the engineered barrier system. I'll be referencing all of my subsequent discussion to 10,000 years. So, it's important to note that we're not expecting absolute containment during the 10,000 year period. That we are allowing for failures to occur in individual waste packages over that period. Our main concern then is with not having a large number of packages fail all at one time.

Also of concern is that from a regulatory standpoint we're not assuming that continuous monitoring and repairs occur over this 10,000 year period. And, the reason for mentioning this is that our main concern, when considering these long periods, we'll have to include a consideration that more than one event will occur. That we shouldn't consider just a single design event, but we need to consider the possibility of cumulative damage occurring as a result of several events scattered through this 10,000 year time period.

We've considered this in a paper by Rich Lee that was presented at last spring's ANS meeting. What Rich did was take the probabilistic hazard estimate from the URS/Blume Reports and use that as the basis for considering the possibility of multiple events during the postclosure time frame. This probabilistic estimate does include a factor assuming activity on the local faults, although the method that they use to do that is different than the presentation I gave in the preclosure talk. But, at any rate, this is the source for the consideration of multiple events. I should point out that we did not put in any factor for attenuation with depth. So, this is basically surface acceleration, although we're applying it to the postclosure case.

Rich Lee used Campbell's Curve for attenuation to come up with acceleration values. What Rich found was that when you consider 10,000 years, the most likely number of events that would exceed .4g was in the four to five event range, and that when you considered then the cumulative probability, even exceeding eight events in 10,000 years had about a 10% probability. So, we feel that it is important that engineered barrier system designs consider the occurrence of multiple events and not just the occurrence of a single design event, and that probabilistic evaluations would be the best method for coming up with the design basis for the engineered barrier system in the postclosure.

Finally, I'll talk about postclosure of fault rupture in the repository. Again, our concern is with the performance of the engineered barrier system. Basically, fault movement that would occur and intersect a waste package and compromise the waste package by the fault impinging on it. Again, our strategy, as with the faulting of the surface facilities, is to avoid placing waste packages in locations where we think faulting is a hazard and where faulting may occur. If that is our strategy, then our main concern or consideration has to be how well can we detect any faults that we wish to avoid when we are underground, basically in a repository drift looking at offset in 13,000,000 year old tuffs? So, any method of evaluation we use will have to consider the limitations that we'll have when you're in a drift determining emplacement locations. That is you'll be looking basically at the drift wall detecting apparent vertical offset in 13,000,000 year old tuffs.

And, I don't have the final answer on that issue. We'll need to do the ESF facility to get some of the answers to the questions that are raised by this issue. But, I will show sort of a method of looking at what the issues might be and how you might think about them. What I'm showing here is a plot that shows on the bottom scale here total fault displacement in the Miocene tuffs that you would see in the repository drift and that would be actually--it's the vertical component of displacement and probability on the vertical axis.

The first half of your concern then is what kind of fault can you detect down there? What size fault can you detect? This will be a function of the stratification that you see in the drift wall. If you have very good stratification, very sharp contacts, presumably you could detect very small offsets. If you have a massive tuff unit with virtually no stratification and if the faults themselves are not well-defined or cannot be separated readily from other fractures and joints, then we may have a problem in detecting the faults. But, what I'm showing here is just a hypothetical value that you might get after you've done the ESF facility and a look at what you're going to see down there and come up with what you feel is your detection limit. For the purpose of the example, I just picked a number. It happens to be 20 centimeters in this case here.

The other half of the concern then is what kind of fault do you have to worry about? What size fault? What amount of displacement on a fault would cause you concern? First of all, you have to decide whether you're really concerned about any of the faults. Are the smaller faults actually capable of movement? We don't have a good handle on that yet.

Bert Swan and the Midway Valley trenching and other surface studies will give us some idea on the distribution of faulting and whether movement occurs on the smaller faults or not. For the purposes of this example, I'm just going to assume that they do have some potential for movement. If that's the case, then going again from the designs in the SCP where we have an air gap around the canister, I'm looking at then what's the potential for having displacement in excess of five centimeters in 10,000 years that is closing the air gap and possibly pinching the waste package itself?

But, what I've done and my assumptions are that the slip history on these smaller faults that we'd be mostly concerned about is proportional to the larger faults in the area. That is if we have a Quaternary slip rate of .01 millimeters per year on a fault with 200 meters of Miocene offset, then a fault with 20 meters of Miocene offset would have 1/10 the Quaternary slip rate. I'm also considering that all of these smaller faults are actually secondary faults to the main faults in the area or the larger faults and that when the larger faults move, the smaller faults may move. However, not all of the faults would move in each event. An individual fault may only move every fifth time there's a major event on some of the larger faults.

Basically then, taking all this together, what you can do is come up with a mean value of fault displacement per event if you assume a given average time interval between events and a given average slip rate. When you do that, you can then assume some sort of distribution about that mean, as far as the actual displacement that occurs, to give you that average of mean offset and calculate a probability of exceeding it. In this case, I simply used a normal distribution about the mean, although you could use exponential or other distribution.

So, basically, all I'm doing here is then showing if you assume that faults move, the fault you're interested in, at a given known Miocene offset, moves every 10,000 years, it would fall in this range here exceeding five centimeters. If you assume it moves only every 100,000 years, you move over and include this range over here. This is just a means of looking at which faults we want to worry about. As long as you have a gap between the two here, you have confidence that you're detecting all the faults that you're concerned about. If, however, your ability to detect is poor and you start considering events with a much longer average interval between--faults with a much longer average interval between events, you may get an overlap which would indicate there's some probability that you're not detecting all the faults that you're concerned about.

In summary for all of this, something like this is what I believe we'll come up with, although that will have to be qualified until we get to the underground with the exploratory studies facility. And, it would appear that we would really be concerned about faults with apparent vertical displacements in the Miocene of about five meters or at least several meters, which from an intuitive standpoint would also be your conclusion since we know these faults have been moving since the Miocene. That any amount of movement that would occur on a fault that would be of significance would have to have presumably a fairly significant amount of total offset.

Now, that I've probably thoroughly confused everybody, you can ask questions.

DR. ALLEN: Thank you, Terry. A good deal of food for thought there.

Let me ask, first of all, if the members of the Board or the consultants or staff have questions?

MR. GRANT: I did thoroughly confuse everybody.

DR. DEERE: I'm wondering what you and your colleagues think will be the difficulty or the ease of detecting these small faults underground looking at the core borings that you already have? I would think, based on my experience, it would be rather simple to--

MR. GRANT: It may be. It depends on what we see. Looking at the surface exposures of the faults, we get a fairly wide range from very narrow features to kind of distributed fracture patterns. I really wouldn't want to say until we get down there in the ESF facility and get a chance to look because we'll be so limited to a 15 or 20 foot high, whatever the height of the drift is, slice, it depends on how the fault is behaving at depth. If they're quite obvious, we might not have any problem, at all.

If, on the other hand, they look a lot like the fractures and joints down there and we have difficulty in determining offset, if the units are not well stratified, it may be more difficult or there may be more uncertainty at any rate. In any case, it's important to remember that at least in the 10,000 year time frame, our concern is not with total containment, but there is a consideration that failures will occur during that time frame from corrosion or seismic sources or other sources. So, there is a--we don't have to be 100% accurate in coming up with total containment. Our main concern would be faulting that would rupture a large number of waste packages in one event.

DR. ALLEN: Do I understand, Terry, that at least in your current thinking you were willing to dismiss from consideration any earthquake that was, say, greater than 7-1.4 of which the site would be within the near field?

MR. GRANT: Well, there's always some possibility of larger events when you're dealing--talking about maximum magnitude. You can always bump it up another notch for some reason or another. At the moment, I don't see a source, a fault that would seem to come up with magnitudes larger than that, remembering those are moment magnitudes. So, those are really pretty good size events. Depending on our studies, though, we may come up with somewhat larger events, but I would say that those are reasonable bounding events given our current information on the size of earthquakes.

DR. ALLEN: I noticed on your map of potential faults that might potentially break and contribute to that earthquake you didn't include the Ghost Dance Fault.

MR. GRANT: No, I didn't. That would be kind of a short little secondary break if it is indeed active, probably related to the movement on the other faults. As I said, given the scale of those maps, I generalized them quite a bit. So, there are other little potential breaks here and there that could occur on other smaller faults.

DR. ALLEN: As I understood it, the way you were getting this cumulative displacement, you were taking all these little breaks and adding them up?

MR. GRANT: That's right.

DR. ALLEN: So, the more little breaks you have, the longer your cumulative--

MR. GRANT: That's correct and I--again, I was generalizing a bit. I did not show the very small faults. That could bump you up a little bit, but I don't think it will affect you too much.

DR. DEERE: What is the offset at the south end? Isn't that over 100 meters?

MR. GRANT: On the Ghost Dance?

DR. DEERE: On the Ghost Dance?

MR. GRANT: Oh, no, it's much less than that. It's probably around 10 meters.

DR. DEERE: I don't believe so, not from information we've had presented earlier.

MR. GRANT: On the Ghost Dance Fault?

DR. DEERE: Yes?

MR. GRANT: 130 feet? Oh, that's right, about 30 meters.

DR. DEERE: 130 feet, okay.

MR. GRANT: 30 meters then.

DR. ALLEN: Who was contributing that information, please? Mr. Mike Cline.

MR. GRANT: Oh, that's Mike Cline back there. Sorry.

DR. DEERE: About identifying the faults, because you have not only the phenomenon of offset where you're looking to see if you can see a bed displaced or another joint displaced, but you also have the characteristics of continuity of the faults and the little gouge on the side, the striations, often small seepages of water. Many of the faults in the Nevada Test Site that we've looked at, time after time they were damp or have water coming in and had a little gouge and alteration in it--

MR. GRANT: The water coming in, of course, raises another concern that it's possible that the faults will be readily detectable underground. Until we get down there and actually take a look at what we're dealing with, we want to hold open the possibility that we want to look at this in detail and be sure that we have the ability to detect the faults we need to. But, it's quite possible we will readily be able to detect the faults. At the moment, there's some possibility that we may not. We just want to leave the issue open for the moment.

DR. DEERE: I'd ask--

MR. GRANT: If your strategy is avoiding faults, that has to be the question, though, that you look at, is can I find the faults I need to avoid?

DR. DEERE: I'd like to ask Professor Cording to give us his experience. About three years ago, he and I were involved in four of the tunnels being excavated at the test site and you spent many months out there, Ed. Did you have any difficulty finding the faults?

DR. CORDING: We were working in the nonwelded tuffs in G-Tunnel and we could quite clearly see fractures and faults. They were really widely spaced in that case. And, you could pick them out when you got to a fracture zone and there was, in some cases, I think, there was movement on them, but we had a very low frequency of any jointing at all there. So, I think you can see some of the features. There's going to be a lot more fracturing, I think, at the repository level because there's more brittle material. But, I'm just kind of wondering as to what size of offset or fault one is concerned with? For example, for how long would a fault splay or a push of a fault have to be before you'd say this is something that could have significant movement on it?

MR. GRANT: Well, that's the reason why I went through the little exercise I did at the last there. I would say that we're really only concerned about faults that have Miocene displacements in excess of about five meters, as far as faulting during the postclosure time period. Those, hopefully, would be readily detectable and we don't have a problem. I'm not saying that we have a problem. We just want to address the issue. We want to have a structure for looking at it. We need to consider whether we can detect the faults and then what kind of faults we need to detect.

DR. CORDING: You could certainly have very small en echelon features with very small displacements out of it. That really isn't what you're concerned with as much as the potential for the larger--or something that's had larger movements within a zone. I believe that sort of thing is something one could start mapping across the facility and define fairly well.

MR. GRANT: We would hope so, yes. Our main concern is if you use the reference design is that you have a vertical emplacement borehole that has about a 7-1/2 centimeter air gap around the canister. And so, the reason I used five centimeters was that was a round number we chose as to when that air gap would be closed by fault movement substantially and could start causing problems. So, we're probably looking at displacements on the order of five centimeters as a threshold of concern during the postclosure time period. And, relating that back to how big a fault you'd have to have that gave you a significant probability of having five centimeters, I'd say you're up in the five meter or more range of faults.

DR. DEERE: If this five centimeters turns out to really be important, you probably would change your design.

MR. GRANT: Well, that's the other option is you could look at other emplacement designs as Clarence talked about in his introduction. Another way to address the issue, if you're really concerned about it or felt that it was a significant possibility of loss of containment due to faulting, would be to alter your design concept for replacing the canisters somehow.

DR. ALLEN: Other questions? Bert, do you have a comment or question?

MR. SWAN: It was on the detection issue. It's so much whether you have a fracture or fault. The problem is going to be to quantify how much cumulative slip is on that fault given the limited exposure and lack of marker beds and discriminating fractures from faults that actually have slip on them.

DR. ALLEN: If one could be assured that any fault of less than five meters in total--was not going to be of concern, that I suspect would be very comforting. But, I suspect there are a lot of people that would question that.

MR. GRANT: You might.

DR. ALLEN: But, I wonder if I might ask people in the audience here--Steve, certainly, you must have some comments on this methodology? Steve, where are you?

MR. GRANT: He's thinking about it back there.

DR. ALLEN: Steve Wesnousky said he had no specific comment.

MR. GRANT: All I was trying to do there was illustrate what the concerns would be and how you might want to look at them. That was not necessarily our only method for addressing the issue later on.

DR. ALLEN: We do have some time available. So, let me encourage these people to speak up. Robin McGuire?

MR. MCGUIRE: Robin McGuire with Risk Engineering. I would just ask for some clarification on the plots of log probability versus log total fault displacement. I just really am having trouble understanding what those portray.

MR. GRANT: Oh, the last ones I showed?

MR. MCGUIRE: In particular, two questions. What units are there on the abscissa in terms of displacement and--

MR. GRANT: Oh, I'm sorry, that's in meters. I forgot to put that on there.

MR. MCGUIRE: And, for those curves on the right hand side, why--I understand what their level is, why their level is at zero and -1 log probability. Why do they curve down?

MR. GRANT: Well, this is because as your total Miocene fault displacement gets smaller and you're distributing that displacement among a given number of events and your average value per event gets smaller and if you look at that as then a probability of exceeding a given number, five centimeters, that probability drops off. I got a blank look. Do you want me to try that again? It's a function of the total displacement on the fault. That is as the fault gets smaller, you have to have basically smaller events through time or else you have a larger fault.

MR. MCGUIRE: So, you're saying as you go to the left on the abscissa towards lower and lower total fault displacements--

MR. GRANT: Right, that's what you're looking at down here--

MR. MCGUIRE: --that one event every 100,000 years might be of magnitude 4 that implies a half centimeter of displacement? Is that the sense that you're portraying?

MR. GRANT: Yeah, I'm just looking at rates of displacement here, and basically if you have a fault that has only moved 10 meters in 10,000,000 years and you have events every million years, then your average displacement is going to be one meter. And, so your probability of exceeding five centimeters is pretty high. But, if you then divide that up into 100 events, your probability of exceeding five centimeters starts to drop because you're dividing that displacement up amongst more events. So, your average displacement per event is smaller. The probability of exceeding any given value is low.

MR. MCGUIRE: All right. That implies the event is smaller and smaller, too?

MR. GRANT: Right.

MR. MCGUIRE: Okay. All right.

MR. GRANT: Now, I'm assuming these are not acting independently, that they're really secondary faults that are moving in response to a larger event occurring on some of the larger faults in the area. They're not little independent faults all on their own.

DR. ALLEN: Bob Kennedy?

DR. KENNEDY: I'd like to get a clarification on one point. During the presentation, you indicated there would likely be in a 10,000 year period multiple events of .4g ground motion. Would there be potentially multiple events of fault movement on any one fault in that period? In other words, are we really worried about five centimeters per movement or is there a chance for multiple movement on a fault?

MR. GRANT: There's a chance if you particularly reconsider that events may cluster in time. If you consider they're more random, it's pretty low because the event recurrence intervals are quite long. One of the things you'd have to consider is--which I did not in the presentation I made--is that multiple events may occur if you have clustering of events and they're not randomly distributed. If that happens, then you'd have to add up the total that you get from each of the events and it might be a little larger.

DR. ALLEN: Don't forget about the rocks that are 13,000,000 years old.

DR. KENNEDY: I know.

DR. ALLEN: So, we're looking at something accumulated over 13,000,000 versus 10,000.

Kevin Coppersmith had a comment or a question.

MR. COPPERSMITH: I'm Kevin Coppersmith of Geomatrix. I just had, Terry, two questions basically. The first deals with the issue of slip rate. I think the key cornerstone of any of the evaluations of recurrence intervals or the probability of occurrence of either the ground motion or the fault displacement has to do with these assessments of the slip rate. You said those are the primary constraints on the frequency of occurrence of earthquakes on these faults?

MR. GRANT: It's also something we can measure out there.

MR. COPPERSMITH: Exactly. But, I think the question that I have deals with that assessment. What assumptions or what data that you used to arrive at those slip rates at this point? Are they all based on post-13,000,000 year old displacement or do you have or are you using information on Quaternary of more recent slip rates?

MR. GRANT: Well, John Whitney is here. Basically, the slip rates I'm using are the Quaternary slip rates that are based on the work by John Whitney at the Windy Wash trench and, more recently, Busted Butte--over on Bare Mountain. So that the slip rates are not averaged over 13,000,000 years. They're Quaternary rates which are actually different than what you'd get if you did an average over the entire period.

MR. COPPERSMITH: Right. I think that's an important point. I think it also is important when looking at--going back to the concept of five meters or some sort of threshold of displacement in a 13,000,000 year old unit. That five meters or 10 meter displacement on a 13,000,000 year old unit may not mean a thing if the slip rate has changed in the post-13,000,000 year period. In fact, that's not unusual to see a change in a lot of the--gone on show that there is a change over that period.

MR. GRANT: We know that the slip rates apparently have changed through time in that the Quaternary rate that we see is different than what we'd see for the full length of the 13,000,000 year period. What I did in constructing the whole thing, what I did there was I assumed the Quaternary rate was the current rate, not the average rate over the full 13,000,000 years and I used that to proportion out for smaller faults what the slip rate might be. I assumed that the history was proportional, whatever the history was.

MR. COPPERSMITH: Okay.

MR. GRANT: And, we know that a lot of displacement occurred on these faults in the Miocene while the tuffs were being deposited. The work by Will Carr and Bob Scott indicates that with local unconformities, angular unconformities, that a lot of the offset in these faults occurred in the miocene during the period that the tuffs were being deposited. So, that's another reason for saying that we probably don't need to be concerned about a lot of movement on one of these smaller faults since we know or have a pretty good indication that the faults have been moving throughout this time frame or at least during portions of the 13,000,000 years. And, that some, probably, fairly large component of whatever total offset we see belongs back in the Miocene rather than in the Quaternary.

MR. COPPERSMITH: Okay. The other question has to do with the preclosure ground motion assessment, looking at your various scenarios for rupture on Bare Mountain, essentially getting into maximum earthquake and so on. In those scenarios where you have synchronous movement on the Bare Mountain, as well as faults in the immediate site vicinity, as you know, most ground motion continuation laws use closest distance to seismogenic rupture or something like that to assess the ground motion level. In this case, when you have these types of ruptures--let's say, your third scenario with the Bare Mountain ruptures and Solitario Canyon, Bow Ridge, and some others, and the Paintbrush Canyon in the nearby site region also rupture, are they seismogenic ruptures occurring in the immediate site region? Is there seismic energy released or is it essentially an earthquake occurring at 15 or 20 kilometers on Bare Mountain and just sympathetic non-energetic rupture in the immediate site vicinity? There's a big difference to ground motion.

MR. GRANT: I realize that. I don't know that we have an answer to that right at the moment. But, for the present, I think we'd have to assume that they're seismogenic and that would impact your design basis for the facility. So, at the moment, I think we'd have to consider them seismogenic.

DR. ALLEN: What's your answer?

MR. COPPERSMITH: To me, I would think that basically the geologic record is if those faults are active and showing activity they would have to be assumed to be seismogenic. So, the near field--you can say it's near field with the difference between 15 to 20 kilometers--the Bare Mountain versus within two or three or one kilometer is a big difference.

MR. GRANT: Oh, well, I realize that. I generalized the near field because we have a lot of facilities that are scattered over an area and I didn't want to give specific distances from specific faults to any specific facility because it will vary, but they are particularly for like the Paintbrush Canyon Fault and the Solitario Canyon Fault. You are very close to a lot of your facilities and you are definitely in the near field, the very near field in some cases.

DR. ALLEN: Jay Smith?

MR. GRANT: Boy, I thought I was going to get off easy there and then everybody started coming up with their questions.

MR. SMITH: Two questions. First, what is your current expectation of the spacing between faults that might be found either at the surface facility and in the subsurface facility?

And, secondly, you've talked about avoiding the hazard by avoiding the faults. What approaches will you be considering for determining how much setback or how much distance by which you will avoid particular faults?

MR. GRANT: In answer to your first question, actually I'll let Bert answer it for the surface facilities because he's up next as far as his expectations there. In the underground, I don't know that we have a good handle yet. All we have is the surface mapping. With the limited exposures, we'd probably find more faults in the underground than we currently know about in the surface mapping. The spacing, I'm not quite sure what that will be. I don't think it will be that many significant offsets from some of the work that we've done on a technical assessment of the--when we were doing shafts, we had a technical assessment and looked at that area. There did not seem to be many faults, other than the Ghost Dance in that particular area when we looked at it. But, as far as very small faults, there are probably some number down there. We really wouldn't care to hazard a guess until we're down there with the ESF facility.

MR. SMITH: What about an order of magnitude guess? Would it be meters, tenths of meters, hundredths of meters?

MR. GRANT: Oh, between faults? What size fault?

MR. SMITH: Well, I'll add that question to my list.

Well, obviously, Terry, the problem in an area where there are numerous faults is the fact that the farther you stay away from one, the closer you approach another. And, there needs to be some criteria for obviously identifying those that require first a setback and then those that require more or less setback.

MR. GRANT: Your second question, the setbacks, we really haven't come up with any set criteria on setbacks yet. I would largely depend on again what we see the character of these faults is, whether they occur as rather distributed zones or very well-defined zones. And, it may be a case-by-case basis.

MR. SMITH: I expect so and I think I would acknowledge Bert Swan's statement that it's the quantitative determination for each of these faults as they're identified in the subsurface that might be the toughest problem.

DR. ALLEN: I think we perhaps better move on here.

MR. GRANT: All right.

DR. ALLEN: All of these same questions we can and will return to tomorrow afternoon if anybody wishes to do so.

MR. GRANT: The next speaker is Bert Swan who is going to give us an update on the preliminary work on surface faulting hazards in Midway Valley.

MR. SWAN: Okay. Our topic is the status of the ongoing investigations in Midway Valley. I'd like to just comment it's nice to be talking about ongoing, as opposed to planned investigations for a change, and emphasize the fact that they are ongoing. So, the results are preliminary.

The Midway Valley studies are part of site characterization plan study for evaluating the location and recency of faulting near the prospective surface facilities in Midway Valley. The study plan consists of two activities. The first activity consists of mapping the Quaternary deposits, the soils, geomorphic surfaces in Midway Valley--that to provide a tool for assessing fault activity. It also includes excavation of soil test pits to characterize the map units we delineate. And then, the excavation and logging of exploratory trenches to investigate any possible fault related features that would be in the vicinity of the surface facilities. The objective of that Activity 1 would be to identify locations within Midway Valley where late Quaternary faulting is absent and then that would be the proposed locations for the surface facilities.

Activity 2 is primarily a confirmation phase, as Terry described earlier, through the excavation of long continuous trenches in the immediate vicinity of the foundations of the prospective facilities to document the presence or absence of faults. If faults are encountered, then they would also be the excavation of some supplemental trenches to investigate those faults and to quantify what that hazard is.

The primary objective though is to pick an area that's free of hazardous faults.

Okay. This is a generalized geologic map of Midway Valley. The area outlined in blue here is the conceptual repository boundary, Midway Valley located in this area. The dark blue line delineates the area that we've conducted a detailed quaternary mapping in and we've completed a preliminary map which is published and coming out as a SAND report.

The area shown in red here is the reference conceptual site for the surface facilities identified by Neil on the east flank of Exile Hill. This is an aerial view of the Midway Valley area, the crest of Yucca Ridge through here, Exile Hill in this area. This is the area of the reference conceptual site. Midway Valley is an alluvial filled valley and it's bounded on the east by the Paintbrush Canyon Fault along this area in here and on the west by the Bow Ridge Fault up through this area.

I'd just like to briefly summarize the units we've described in the Quaternary geologic mapping. In the preliminary map, we've just arbitrarily designated these units from oldest to youngest. To the oldest being one up through the youngest. We've identified seven major alluvial fan and terrace surfaces. Also, identified a previously unidentified remnant of an older, either lower Pleistocene or Pliocene terrace that occurs in a single outcrop. Also, delineate areas of bedrock or bedrock covered with undifferentiated colluvium. We have not attempted at this stage in the mapping to break out the colluvium units, although in the field we can recognize different age colluvium units that correlate with our alluvial fan surfaces.

The paper that's coming out or in preparation by Taylor and others based on the direct age dating regional correlations, they've defined some Quaternary boundary dates for the deposits in the Yucca Mountain or Nevada Test Site area. Within Midway Valley, the surfaces we have to work with range in age generally from middle Pleistocene up through latest Holocene. Some of the basin fill deposits themselves may be Plio-Pleistocene in age, but the surfaces we generally feel range from, as I say, middle Pleistocene and younger or age of about the Bishop Ash on up to the present.

This is just on this side just a generalized geologic map of Midway Valley. Here's Exile Hill in this area, the reference conceptual site, Bow Ridge Fault which is the feature we investigated in our first exploratory trench. This box is the area of the detailed area Quaternary map in this area. And, what I'll be talking to you about later is the results from our first exploratory trench, Trench A/BR-3, north of Exile Hill which lies across the map trace of the Bow Ridge Fault and across some vegetation alignments.

I want to illustrate here is, in terms of the stratigraphy, the oldest surface we have to work with in the vicinity of Exile Hill and the reference conceptual site is our Unit 3 alluvial fan surface. We picked this site, one, because it was across the trace of a known quaternary active fault, and secondly, because it's in similar age and character materials that we expect to find at the reference conceptual site itself, our Unit 3 alluvial fan surface.

If we look at a schematic cross section--and this is not in the packet, but a copy of this viewgraph is on the back table there for those that want it. If you go north of Sever Wash, north of Exile Hill, the stratigraphic relations, we see a series of inset terrace surfaces below our Unit 1. This is equivalent to the Swadley-Hoover Unit QTA and then successfully younger inset terraces below those. It's at this Sever Wash these terraces grade to a series of alluvial bands and the oldest surface south of Sever Wash in the vicinity of the reference conceptual site is this Unit 3 alluvial fan surface.

This is a detailed topographic map showing some of the lineaments we've identified in the vicinity of the reference conceptual site in the northern part of Midway Valley. This is Exile Hill, and this area for reference, Trench 14 here. This is the trench we've excavated and I'll talk about the results of that in just a moment. The alignment along here is the proposed trench and location of soil test pits which we plan to be excavating during this next phase. Beginning in February, we hope to excavate a number of soil test pits to calibrate what materials will be encountered in the area of the reference conceptual site and then we staked out an alignment for the continuous trench which goes through the prospective surface facilities and the area of the prospective surface facilities. And, in February, we plan to excavate the western part of that trench.

I'd like to devote the rest of the talk on what we found to date in the one trench we have completed, the Trench A/BR-3. As I said, it's along the map trace of the Bow Ridge by Scott & Bonk. In this area, the map trace is not based on any direct outcrop evidence, but was based on interpretation of aeromagnetic data and an electromagnetic survey. We've identified several weak vegetation lineaments along this same alignment and along the projection of the last outcrop exposure of the fault which is in the suite of Trench 14, suite of exposures in the Trench 14 trenches.

Now, this is a generalized cartoon of that based on the detailed logging we've done on that trench. Oh, I didn't talk to this photo. This is an aerial view, oblique aerial view, looking south along the Bow Ridge Fault. Trench Area 14 is in this area here and then the Unit 3 alluvial fan surface, remnants of it in this area, that surface wraps around Exile Hill to the area of the reference conceptual site located over in this area here.

DR. ALLEN: Bert, prior to the excavation of Trench 14 and those trenches adjacent to it, was there definitive evidence of a fault there?

MR. SWAN: There is a weak topograph--or linear scarp associated with it. I mean, it's very degraded and subdued, and if we go back to the lineament map, there are a number of tonal contrasts, break and slopes, vegetational alignments coincident with a fault. The fault is pretty well defined on aerial photographs down through here. The expression of it on aerial photos the north end of Exile Hill is almost non-existent and we have identified some lineaments, but they're very weakly expressed. You can't identify them on the ground. You have to use the 1 to 6000 aerial photos and then actually locate yourself by individual bushes to identify where it is when you're on the ground.

I should also comment on the approach used in drawing these lineaments. These are not all features suspected of being Quaternary faults. We took a very conservative approach and anything that could be construed as a lineament that we could not conclusively rule out as being a cultural feature was a line drawn on the map. So, the objective there was to draw it--draw anything that could be construed as a lineament so we wouldn't miss some features.

Okay. The trench is about 105 meters long going from east to west. The area of the photo lineament is shown in orange in this area in here and the map trace of the Bow Ridge Fault by Scott & Bonk goes through the trench in this area. We identify three principal stratigraphic units in this trench which we arbitrarily numbered and we used Roman Numerals instead of Arabic numerals in this case to hopefully avoid confusion between our mapping and the trench mapping. The oldest being identified as Unit I is a crudely stratified to moderately well stratified alluvial fan deposit in the lower part of the trench. It's a well developed Stage 5 calcic soil developed on this unit which is indicated here by the blue. That unit has been truncated by erosion overlain by younger alluvial fan deposits, coarse grain alluvial fan deposits, that grade up to the present surface and this unit includes the eolean cap associated with that surface.

And then, in the east end of that trench, Unit II has been cut--there's a cut and filled channel that cuts out Unit 2 and we have a third or our Unit III younger alluvial surface. That's more clearly illustrated in the detail log here where we have our Unit I alluvial fan, crudely stratified, a number of cut and fill channels on the eastern end of it. That was capped by well developed end paleosol, truncated by erosion. Unit II in this area and then the inset, Unit III, in that area. The geologic unit that corresponds to the map surface would be this Roman Numeral Unit II.

If we look at more detail in the central part of that trench, this area through here, again we have the lower unit, crudely stratified alluvial fan deposits. You can actually map out fine grain beds within this. We've got the calcic soil and then the erosional unconformity between our Unit I and Unit II and then the younger alluvial fan deposits overlying that. We see no evidence for any displacement or faulting of any of these units through this reach or through the entire length of the trench.

And, just to sort of clarify the correlation between our log units and the trench exposure versus our geologic mapping, I reiterate we have three primary stratigraphic units exposed in the trench. Unit II correlates to the surface that we mapped in that area or our alluvial fan unit surface in this area. In terms of correlations, our Unit I, we feel, correlates with our alluvial fan surface 2 which is just slightly inset below Swadley-Hoover's QTA surface or our Unit I which we feel is middle-Pleistocene in age. This is the one that has the well developed Stage 5 calcic soil on it. Our Unit 3, we feel, is late Pleistocene. It has a well developed textural B horizon with accumulation of clays, carbonate, and silica. We feel it's probably lower late Pleistocene in age approaching Oxygen Isotope Stage 5e.

So, if I put, just off the top of my head, a number age on it, we're probably looking at something 70,000, plus or minus 30. I think that would be a conservative age estimate on it. And then, the youngest cut and fill channel on the east end of that trench which is Holocene to late Pleistocene in age.

DR. ALLEN: How then does all the vein material and so forth or whatever it is we see in Trench 14--how does it relate to this?

MR. SWAN: The vein material is probably equivalent in age to this and older. There is carbonate precipitation associated with the soil on the Unit 3. The carbonate precipitation is ongoing and even going on today. The bulk of it pre-dates the erosional unconformity and is associated with our Unit 2 surface, but it is ongoing. It's a complex soil exposed in the trench where we see younger soil forming processes, as superimposed on the older ones, but there is clearly an erosional unconformity between the two units in places that's matched by the carbonate precipitation and soil--

DR. DEERE: Could you go back for a moment to the plan view that shows the location of this A/BR-3 with respect to Trench 14?

MR. SWAN: This figure?

DR. DEERE: Yeah?

MR. SWAN: Okay. Actually, I should have thrown this up. That's the explanation that goes with that figure.

DR. DEERE: Where did it go? The Bow Ridge Fault?

MR. SWAN: Pardon? Oh, you mean, where does it go?

DR. DEERE: Um-hum?

MR. SWAN: Well, I haven't gotten to my conclusion here, but--

DR. DEERE: Oh, I'm sorry.

MR. SWAN: But, not finding it in the trench, I mean it begs the question where does it go if it isn't here? We have basically four options. It either goes east, west--it doesn't make it or it makes it with a resolution. The displacement could be dying out just as the topographic relief of Exile Hill dies out. And, the fourth explanation would be that it does continue on through this area, but below the threshold of detection which was the point I was going to get to in the summary and conclusions here.

DR. DEERE: Those pink shaded lineaments, those were vegetation lineaments?

MR. SWAN: Not all of them. The ones marked in V are vegetation; BIS, break and slope; LD, linear drainage; T is the tonal contrast.

DR. DEERE: What's the one that cross the trench?

MR. SWAN: The ones that cross the trench are very weak vegetation lineaments. I mean, you really--

DR. DEERE: And, on the section, could you see anything there?

MR. SWAN: No. Through that part of the trench, those lineaments go through this part of the trench and there's no evidence of faulting, at all, through there. And, incidentally, it gets what's our degree of resolution--maybe I should just throw out the conclusions and then discuss it further. But, the trench does cross to vegetative lineaments and it crosses the map trace of the Bow Ridge Fault and the map in this area not based on outcrop evidence, but based on interpretation of the aeromag data and an electromagnetic survey by Scott & Bonk. In Trench 14, the Bow Ridge Fault displaces colluvial deposits that we would correlate with our map Unit 3 and 4, or in the trenches, our Units I and II. And, it's displaced by multiple events.

Taylor and Hutchins are putting a paper out now where they interpret that there are two faulting events within the interval of time represented by our Unit I in the trench and pre-dating the erosional unconformity and then another event post that. So, we're looking at the same stratigraphic record or interval in time where we've had three surface faulting events; yet, we see no evidence for the faulting in the trench. Which gets to this bullet here where the fault crosses the same age deposits as exposed in Trench 14, but the deposits are unfaulted.

Our degree of resolution, it depends on what unit we're talking about. If you're talking about the coarse grain alluvial fan deposits themselves, our degree of resolution is anywhere from zero to 10 centimeters. You can express degree of resolution in two ways. What's the smallest feature you think you could detect or what's the largest feature that you think you might miss somewhere in the exposure? And, it's the latter case that we're representing here. We feel that in most places, we could detect very small, one or two centimeter, features along the trench, but in some areas we don't have as good stratification. If I had to say what's the largest feature that could go through the trench somewhere undetected in those alluvial fan gravels, it would be somewhere on the order of 30 centimeters. So, less that five to 30 centimeters.

If we're looking at the erosional contact at the top of our geological map Unit 3 or at the base--at the top of map Unit 2 or base of Unit 3, this is the well developed calcic soil that's been truncated by erosion--that erosional contact, we feel the degree of resolution for vertical displacements is between zero and 10 centimeters. Most places, we feel we can preclude any faulting going through there.

So, given the four working hypotheses, where does the Bow Ridge Fault go, I think the least likely of those four is that it goes through the trench undetected. I can't absolutely preclude it, but we're looking at Trench 14 at displacements on the order of 10 or 15 centimeters per event and we're looking at a record of three events which fractured this calcic soil in Trench 14 and it was readily apparent there. My judgment is it doesn't go through the trench here. So, we're left does it go east, does it go west, or does it just not extend this far? If we have to resolve that issue, the way to do it would be to back up closer to Trench 14 and trace the fault out into this area. That would be the next step if we had to resolve it. I wouldn't recommend, for example, extending the trench further east or west at this location.

DR. ALLEN: Well, Bert, on the basis of what you now know about this figure here, what you see in the trenches, are you optimistic that at the loading facility site that we will be able to document the presence or absence of faults of significance?

MR. SWAN: With the deposits we see in this trench, we expect to find the same age deposits at or near the surface in the area of the surface facility. In some cases, they're going to be buried by thin, younger alluvial fan deposits, but we expect to encounter them in the near surface and I think we'll have comparable resolution. If we find this same calcic soil going across that area, it's a good--it's our best marker horizon. And, the age of that is--the uranium dates that Emily Taylor gets out of Trench 14 puts it in the age bracket of a half million to 250,000 years for the Unit 2, but the overlying unit is younger than that and probably somewhere around Oxygen Isotope 5e of about 100K.

DR. ALLEN: So, basically, you're optimistic that we will be able to establish the presence or absence of--

MR. SWAN: Yes. And, our resolution is going to be on the order of a few centimeters. In a worst case scenario, it may be 20 or 30 centimeters.

DR. ALLEN: Other questions from the Board? Steve? Steve, would you go to a microphone, please?

MR. WESNOUSKY: Bert, with respect to resolution, is it worse if it's a strike-slip fault in predominant mode?

MR. SWAN: I labeled it as vertical displacement. In these coarse grain gravels, it's going to be difficult to detect a pure strike-slip movement. If we had a significant one, a meter or more, I'm confident we'd see it. We'd see it in the fabric and the gravels and the fracturing of the calcic soil. But, in terms of very minor amounts of strike-slip, we don't really have the ability in this without--until we find a--if we find a fault we can do lay backs, investigate it, and come up with estimates of the amount of strike-slip. But, trenching in an area where we don't see any faults, it's hard to say there's zero lateral slip component going through there. If we look at how these faults are behaving, the Paintbrush Canyon Fault and the Bow Ridge, they all have a predominant vertical component. They have lateral component also. But, the dominant component is vertical and we can resolve that.

DR. ALLEN: Other questions or comments?

(No response.)

DR. ALLEN: Okay. In that case, thank you, Bert.

We'll take a break now for 20 minutes until 10:40.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Okay. May we reconvene, please?

Ardyth, do you wish to introduce your next speaker?

DR. SIMMONS: Yes. The next speaker will be Jim Phillips with SANDIA National Laboratories. Presently, Jim is working on assessments for transportation of hazardous material produced by the Defense Waste Programs. And, in the past, he's had extensive experience in analyzing ground motion for underground nuclear explosions.

Jim?

MR. PHILLIPS: Thank you.

The presentation I'm going to give today presents the results of an experiment that was conducted a few years ago in the tunnels of Rainier Mesa in Area 12 of the Nevada Test Site. The primary motivation for this experiment was the need to obtain the behavior data on the dynamic behavior of tunnels at ground motion levels of interest to the Yucca

Mountain site characterization project. The event that this experiment was fielded on had a body wave magnitude of 5.0 as determined by the USGS and it was approximately half a kilometer away from the source.

A fairly large body of tunnel response data exists. Observations of damage are generally linked with estimates of ground motion rather than measurements. In the case of explosion generated loading, the observations are generally at ground motion levels much higher than of interest here. In earthquake generated loading, there is no actual measure of ground motion linked with damage observed. In general, results that you can find in the literature show that damage to tunnels resulting from seismic loading is less than surface structures, and unless a fault intersects the opening and there is motion along that fault, damage is usually minor.

The experiment that we fielded was an imperfect analog for the conditions at the potential repository in many respects. It was conducted in an area where the rock type is different from the Yucca Mountain. The compression dominated ground motion generated by the explosion is different than shear dominated ground motion that you find in earthquakes. Tunnel support systems in or near Mesa are likely to be different than that planned for the potential repository. So, questions about the use of this data arise. For example, how do the differences in rock type affect the conclusions? Can tunnel response to underground nuclear explosion loading be used to conclude anything useful to the Yucca Mountain objective? I hope to supply some answers to those two questions.

The presentation and the material within this presentation is based on a paper entitled "Tunnel Damage Resulting From Seismic Loading" by Phillips and Luke. This paper is contained in the Proceedings of the Second International Conference on Recent Advances in Geotechnical Earthquake Engineering and Soil Dynamics held in March of 1991.

The objective of this experiment was fairly simple. We wanted to try to correlate measured ground motions with observed tunnel damage. The experiment was fielded in this particular tunnel. It was a horseshoe shaped tunnel lined out with a tunnel boring machine. The tunnel was initially driven with a tunnel boring machine. As it was driven, it was reinforced along the back with 1.8 meter long rock bolts, 2.2 centimeter diameter. Those were placed in a random pattern. Later, the corners were mined and the main rock bolts were installed at that time. They were 4.9 meter long, 2.9 centimeter diameter rock bolts, nominally spaced at 1.2 meter centers. There was four to 10 centimeters of fibercrete lining sprayed around the outside of the opening from spring line to spring line over the back. The fibercrete was sprayed over the 5x5 centimeter mesh. Spring line on down, the wire mesh was outside of the fibercrete. The host rock in this particular tunnel is identified as a nonwelded ash-fall tuff and the tunnel access was approximately perpendicular to the direction of the incoming shock wave.

Here's a more detailed description of the experiment itself. We chose a 12 meter tunnel section. In the center of that section, we installed triaxial acceleration measurements on the ribs, on the back, and the invert, and in the free field. The free field gauge was nine meters below the surface of the invert. The direction of loading is shown here coming in towards the left rib. In the remainder of the presentation, I will call this the rib closest to the shock front and this is the rib furthest away, for obvious reasons, I guess.

In addition, we had some permanent displacement measurements where we used the rock bolts that were installed on the 1.2 meter spacing. Tunnel convergence measurements were made at anchors shown at the numbered locations here. The objective of those measurements were to try to understand how the dimensions might change as a result of the loading. There were borehole observations made in two boreholes and those were done pre- and post-test with a televiewer. In addition, we had still and high-speed photography. The still photography was used mainly as documentary and the high-speed was used to--we digitized the information and compared that to our measured ground motions from our accelerometers.

I was primarily responsible for the transient measurements and the permanent displacements and Barbara Luke was responsible for the convergence measurements and borehole studies.

This is a photograph of the section pre-test. I've done kind of a bad thing to you all here in that now we sort of flipped it so that the incoming shock front is coming in in this direction. This is where the photo-target is for our high-speed photography. Our ground motion gauges are here, here, here, and down in this area. Both the free field and the invert gauge was over on this side of the track. This is essentially the view that we have on our high-speed photography which is what I'll show next.

As you watch this, you might keep in mind a couple of things. At half a second before the detonation of the device, the camera was started rolling. At the point their detonation occurred, there will be a flash of light down in this section of the picture, and at that point about 180 milliseconds later, you will see the incoming shock front on this particular target. So, let me go ahead and--I hope this will be visible. We had two cameras. I'll show both of them. It's a redundant view. So, there really isn't any difference.

(Whereupon, Mr. Phillips commented on a videotape of the Tunnel Dynamics Experiment.)

DR. DEERE: Tell us about the golf balls?

MR. PHILLIPS: Well--

DR. DEERE: They were free to move which direction? It looked like they were clamped in.

MR. PHILLIPS: Well, what we were concerned about was that the lead brick was going to fall out of the frame before we actually got the motions. We wanted to maintain some kind of a reference so we could digitize that film. So, the technician came up with kind of a clever answer, I thought, in that he put in a couple of PVC pipes with the two colors of golf balls. As the brick dropped, it pulled a couple of pins out from the PVC pipe and then the balls fell down and they were allowed--you know, there was a slight incline into that. He ran two or three kind of dry runs, if you want to call them that, to make sure that the rate of that fall or stream of golf balls would give us kind of what we thought we would need. We received a lot of ribbing from the fellows down in the tunnel about that, but those particular golf balls became quite the souvenir after the test was conducted. Everybody wanted to take those home with them.

So, that's a pre-test on the right, post-test on the left. Basically, what we saw was that the rib closest to the shock front, incoming shock front, showed some degree of cracking and distress along this fibercrete reinforcement. You see some of that as you go up towards just about the vent line here. There are a few cracks up along the back. As you come down over here on the other rib, you'll see that there is no apparent sign of any damage. If we look at the rib that was furthest away, pre-test is the one with the fire extinguisher, post-test doesn't have a fire extinguisher. There really is no apparent damage in terms of anything on the surface of that particular rib.

We look at the other rib and we'll get a sense of some of the kinds of things that we saw with this being really the most visible element of damage. There was one other thing that I wanted to point out. Here, you noticed all of those white chips kind of flying all over the place and the perspective of the camera would lead you to believe that those could have been some fairly large chips, but bottom line is this is kind of the rubble that you see down there and it's all fairly small and so it wasn't as big as it appeared on the film.

So, the assessment based on our photography and from the borehole work that was done was that there was very minor surficial damage. Borehole inspection supports the assessment. Marked differences in the near surface conditions appeared. On the rib closest to the event, apertures of pre-existing fracture increased by as much as three centimeters. On the other rib, a new fracture five centimeters wide appeared parallel to the opening. Noticeable changes, however, were limited to within 6/10 of a meter of the tunnel surface.

Okay. Additional results from our permanent displacements of the rock bolt markers that we had used, as well as the gauge. We used our gauge bounce as permanent displacement markers. Essentially, I've done it to you again. I've changed my direction of loading. We're now coming in this way. Essentially, the unit appeared to move as a whole somewhere on the order of five to six centimeters.

If we look at the tunnel convergence measurements trying to look at how the dimensions of that particular drift changed, this plot shows the change in those measurements from 30 days prior to the event to 60 days after the event. Zero time here means when the event went. Basically, what you see here is that the maximum closure on that particular section was at about 45 millimeters which is relatively small. The overall dimensions were essentially six meters by six meters.

In terms of the ground motions that were measured, this is the free field, these are the free field ground motions. We saw about 28g of acceleration, radial acceleration. Maximum velocity was 2.3 meters per second, maximum transient displacement was on the order of 13 centimeters.

And, the duration of the ground motion was somewhere between eight and 10 seconds.

Okay. Basically, what we concluded from the results of this particular experiment, we had a self-consistent data set which we felt good about. We only observed what we would call minor damage and we felt like the damage that we observed was consistent with the case histories present in literature. The major question remains though. Are the results applicable to Yucca Mountain considering the major differences that we have in source, geology, and the ground motion levels? So, in an effort to try to get a handle on that, we did some analysis.

Source differences, I'll talk about first. I guess, the major ones that you would think of are the compression dominated versus the shear dominated aspects of the wave forms, the duration of shaking between a bomb source and an earthquake source, and the frequency content of the two.

Hendron and others have found that the ratio of wavelength of the motion to the tunnel diameter, if that's greater than eight, then the problem becomes pseudo static. And, if you have a pseudo static problem, then the frequency of the ground motion is not really an issue. In this particular case, we had a wavelength to diameter ratio of 20. So, that kind of addresses the frequency content.

Compression versus shear, we have a wave front that has a large radius of curvature relative to the tunnel diameter. We can treat that as a plain wave. If we make some simplifying assumptions about the material, things like it's an elastic homogeneous isotropic material, then we can estimate strains as the ratio of particle velocity over media wave speed. Dowding in a 1984 paper postulated that both shear compression and shear waves induced circumferal strains in tunnels and further postulated that the explosion data could be used to predict earthquake response.

The duration of shaking issue, some work done by McGarr in 1983 from South African experience with small earthquakes and rock burst, he developed a model from which you could calculate duration of shaking. If I used his model to come up with an estimate of what a magnitude 5 earthquake would produce in terms of duration of shaking and then compared that to what we observed in the tunnel dynamics experiment, and from that, that's where I get this estimate that our duration of shaking of the major pulse is within a factor of 2, what we would expect from a small magnitude 5 earthquake. Now, I don't want to give the impression that we feel like we've simulated an earthquake here. We haven't. What I think we have done is simulated a tunnel response that is similar to what you might expect from a small earthquake in the near field.

If we look at geologic differences, the repository is a moderately to densely welded ash-flow tuff and it's highly fractured. The tunnel dynamics experiment is non-welded, partially saturated ashfall tuff. Rock type is misleading because it's really not a true measure of the behavior of the rock mass. What we're really interested in is the rock mass behavior. At the time this particular work was done, I chose to use the rock mass rating system of Bieniawski and this kind of put these particular rocks on a common basis. That particular rock mass rating system uses strength, drill core quality, joint spacing, joint condition, groundwater, and orientation to come up with a value. It may have been more appropriate to use a rock mass modulus at that time, but at that time I really didn't have the information that I needed. I took these rock mass rating values from Langkopf & Gnirk from a 1986 SAND report. Basically, I guess the conclusions that I drew were that although these rocks are vastly different in their description, that as a rock mass they may be comparable in their behavior. And, if you take further another leap of faith assuming that this rock mass rating captures important aspects of the dynamic behavior, then if we feel that that experiment in the Yucca Mountain material with the same tunnel that we had in the tunnel dynamics experiment--I guess, what I'm trying to say is our damage, we think, would have essentially been the same.

Okay. I guess, the other thing we needed to address besides the source and the geologic difference is the magnitude of the motions. When we initially went to design that experiment, we expected something a little less than what we actually got. So, our ground motions in terms of velocities are about an order of a magnitude greater than what we had initially anticipated, and if you look at the design basis that I took from RIB Version 4--again, at the time I did this work, the design basis for the exploratory shaft that had ground motions of .3g and .3m/s--well, we saw 28g and 2.3m/s. And, as an aside, that's a typo on the slide and that's entirely my fault. I was given the opportunity to proofread these and I just pulled a no-brainer and didn't get that one. So, if you'd make that change, I'd appreciate it. If you look at the design basis for the other facilities, they're specifying something like .4g and again you compare that to 28g.

If you look at a prediction of strains using these philosophies that we recorded, then the strains that you calculate from these motions are an order of magnitude less than what the TDE had. And, what I'm--and, I guess this picture kind of shows the comparison of the--the design basis that's shown here that's specified as earthquake is out of a Blume Report, a 1985 Blume Report, in which they provided pseudo-relative velocity response spectra for various components of motion. That's the orange dotted line there. And, if you look at the tunnel dynamics experiment, it's clear that we exceeded that by a fair amount.

So, I guess, in running to a conclusion, the first one is that we feel like we've stimulated a tunnel response similar to what you might expect in the near field region of a small to moderate earthquake. Comparison of the rock properties indicates that a similar level of damage would have occurred in a tunnel constructed in the repository host rock subjected to the same loading. I think that the bottom one is probably the most important one to bear in mind. That is the ground motions used for design of the repository tunnels are likely to be much less than those observed in the tunnel dynamics experiment. And, I think it's safe to say that those motions can be accommodated in the design and probably fairly easily accommodated in the design.

DR. ALLEN: Okay. Thank you.

Questions from the Board or people--Don Deere?

DR. DEERE: Yes. It's, I think, very noteworthy that you had ground motions approximately 10 times those associated with the design ground motions. Is this correct?

MR. PHILLIPS: Correct.

DR. DEERE: And, yet, the amount of damage that you got was just absolutely minor.

MR. PHILLIPS: Right.

DR. DEERE: This, I think, is in a great part due to the very good support system that was used in the tunnel. But, people don't realize and I think experience has shown more and more that a system of rock bolts and shotcrete with mesh, or in this case the mesh reinforced or the fiber reinforced shotcrete, is a very tough system that can keep working even though there's a considerable amount of additional force and perhaps deformation involved.

I mentioned to you before at the coffee break an experience we had in Washington. I happened to be at the Nevada Test Site and got a telephone call that one of the tunnels we were constructing in Washington Metro had just suffered a collapse and could I get back fast and take a look at it. I flew overnight and got in the next day and had the chance to go directly from the plane to the failure and I think what I saw was correct. The failure had taken place because they had encountered a small fault, or at that time we called it a shear zone, in the roof running, more or less, sub-parallel to the tunnel and they were using rock bolts for their support. And, as they followed it along for 15 or 20 feet putting rock bolts on both sides of the fault and crossing them, they said, well, you know, this fault is not getting any better. Maybe we'd better change to steel sets, circular I-beams or wide flange beams of fairly heavy size. And, the tunnel opening would have probably been on the order of 20 feet in diameter or something like that. But, when they advanced a certain distance, the last rib that they put in started to take some load, some blocks started to shift on the side, and loaded that steel set so that it was getting asymmetrically loaded and it started to twist. And, once it started to twist, it had very low resistance. So, it then passed the load or the rock passed the load to the adjacent one and it started to fail. So, the failure that had taken place the day before was progressing. One steel rib contorting, failing, coming down, and the next. It just kept working back. And, the miners said it was just amazing to see this thing fall every five minutes to 15 minutes, another failure, another failure, another failure. Considered to be very strong support. It came right back to the rock bolts and all motion stopped and we had a very, very stable structure. And, I think Jay Merritt will probably tell us about some of the rock bolt tunnels that were used out here in Hard Hat, was it, Jay, that behaved extremely well, even better than some of the steel ones.

So, my point is this. Where you have the ground motions, where you have some idea of the geology and the rock quality, you're able to relate with a reasonable degree of certainty the behavior under these ground motion parameters. But, if we go to the literature and we look about tunnels and earthquakes, what you find is, well, we don't really know for sure what kind of support to use, but there was a moderate failure effect. Well, many of these tunnels are 10 miles long and there may have been rock falls at fault zones, but those have not been recorded. The degree of support, whether it was very well supported or whether steel sets had been put up with very bad bracing or whether rock bolts on 2.5 meter centers instead of 1.4 meter center, all of these things are lost and they're not in the literature. So, you could have 700 case histories and get practically nothing out of it.

MR. PHILLIPS: That's right.

DR. DEERE: And, therefore, I am very, very pleased to see the information that you have here. It shows that with a well designed, supported system, rock bolts, and reinforced shotcrete, you can take a lot of ground motion. You had 13 centimeters displacement and you came back again and ended up with a permanent displacement of about four or five centimeters. Your acceleration in terms of gravity was how much?

MR. PHILLIPS: About 28g.

DR. DEERE: About 28. Particle velocity?

MR. PHILLIPS: 2.3m/s.

DR. DEERE: 2.3 meters as compared to 2 inches per second which is considered the maximum for buildings--I am in an old, old sort of rule of thumb--not to suffer damage. So, these things went under terrific ground motion.

MR. PHILLIPS: It was quite a ride, I think, yeah.

DR. DEERE: And then, you look at what happened and you can say, well, the combination of that support and that rock just behaved beautifully.

Now, up and down the tunnel, what happened? Did you have similar types of supports or did you just have the original rock bolts without the additional--

MR. PHILLIPS: Basically, what we did was we just took whatever, you know--what DNA had fielded at that point. What they used, that was what we came in and instrumented. And, we were concentrated all in that one specific area of the tunnel. The rest of the tunnel was supported in the same way.

DR. DEERE: And, behaved equally?

MR. PHILLIPS: Yeah. Obviously, if you get closer in, things get a little more--it damages a lot worse as you get closer to the source. As you get further away, then it was really no big deal. I think we were probably kind of at the mid-point as far as that was concerned, as far as damage. You had to go closer to the source by quite a bit before you got into any real serious kind of damage. The function of that drift was not impaired, at all. I was on the re-entry team on that one and was allowed to go down within a day or so after the detonation and it looked--with the exception of some dust and some of those little pieces on the ground, it didn't look that much different than the day before at button up.

DR. DEERE: I'd like to ask Professor Cording if he has any observations on this, particularly the rock bolt system and its efficiency?

DR. CORDING: One question, the drift, was it angled so that as you went down the drift you got closer to the working point or where the device was?

MR. PHILLIPS: Really, what happened, this was running essentially perpendicular to the shock front. And, I guess, my comment was that, you know, what you did was you went down the drift a few hundred feet and then you made a right turn. And, as you started going down that particular drift towards the emplacement point, that was where things really started to get more interesting.

DR. ALLEN: Bob Kennedy?

DR. KENNEDY: I guess I agree with most of your presentation, but I do have some comments. Now, first of all, this is the standard design that has been used for the last number of years on the tunnels shocks of these 1.2 meter spaced rock bolts with shotcrete and wire mesh. And, the performance of your experiment is in general agreement with many, many other places where this design has been used in these tunnel beds. But, there has been large variability in performance. And, I can name one or two shocks, at least one shock, in which this 2.3m/s ground motion level, this design did not perform very well. This is about the lower bound threshold. You wouldn't go much below this and have substantial damage with this design. But, there is a lot of variability in these tunnel beds. This event, I think, performed near the mean or near the average.

One thing we've observed with this design in these tunnel beds is that when we start getting invert heave, which always seems to be the biggest closure is invert versus the back, when we start getting invert heave of roughly this percentage of the tunnel diameter, at ground motion levels roughly double this, we would expect very severe damage to the tunnel. In other words, I don't think there's a lot of margin even in this case beyond the ground motion levels you were mentioning. It doesn't take too much beyond this before you would have rather serious damage.

One other area of concern I have is this was primarily a compression wave. The strains around the tunnel are sort of closely proportional to the peak particle velocity divided by the wave speed. Now, for seismic, the waves are primarily shear waves. So, the wave speeds are quite a bit less. So, for the same peak particle velocity, we'd have quite a bit higher strein. I think as you try to extrapolate this data to seismic, that needs to be taken into account. So that for a seismic event, you'd have to knock these results down some. The other thing, this was essentially one velocity pulse, as you show?

MR. PHILLIPS: Right.

DR. KENNEDY: Where in a magnitude 7 seismic event, I think we'd expect something in the neighborhood of 3 to 5 near peak velocity excursion during the time history and that we might not have quite this same performance. So, I guess I have a great deal of concern about the statement that there's an order of magnitude margin. I do certainly think there's a substantial margin. I don't want to be mislead on that, but I think the margin may be more like a factor of 3 over their seismic design--your seismic design was .3m/s, is that--

MR. PHILLIPS: Yes.

DR. KENNEDY: There is a significant margin, but I question if it's really as high as an order of magnitude.

MR. PHILLIPS: I guess the other thing is that those particular numbers are what's in the reference information base now for surface facilities. There is nothing really for underground. So, I was pulling numbers that probably don't apply.

DR. CORDING: You made a reference, I think, to a comment to a point that I'd like to follow up a little bit on. You were talking about the comparison of the two rock types and the rock mass rating showed about the same. Well, of course, the rock mass rating combines several different parameters, some of which may or may not have as much significance in terms of a blast of--the effect on damage and also wave propagation. And, I think that one would expect to have higher propagation velocities whether it be a shear or P-wave in the welded tuffs than you do in the nonwelded tuffs where you did the experiment. So, it would seem that you're strains around the opening would actually be smaller when you got to the repository site. So, that would be a situation where your strains are actually less at the repository site than they were in your experiments. So, that would be something that--compensation going the other way from the difference between a P- and S-wave. So, I think there's several parameters there that are affecting this in comparison from the nonwelded tuffs to the welded tuffs. And, perhaps, we would have less strains certainly in the free field and there might be some more local slabbiness around the tunnel, but I think we have a better situation in terms of strains at the repository.

MR. PHILLIPS: Like I said up front, this is really an imperfect analog that we have here and I think it's--the major thing I think that we can say is that we've documented the ground motion here, we know what the design is here, and now we know very well what the environment that that tunnel survived was. And, it's clear that a tunnel will survive an environment such as that. And, I think, beyond that, you know, you start getting a little tenuous on some of the conclusions you can make. But, you know, it's clear that it can--a tunnel can be designed to withstand substantial motion. And, the motions that are considered as--at least, from the RIB version 4 are kind of a chip shot in terms of design for ground motion.

DR. ALLEN: I think we'll probably go ahead here if we're to stay reasonably on schedule. So, thank you.

MR. PHILLIPS: Okay.

DR. ALLEN: Ardyth, do you want to introduce the next one?

DR. SIMMONS: The next speaker will be Phil Richter who will discuss the seismic vulnerabilities with respect to the surface facility. Phil is with Fluor Daniel and he comes with extensive experience in seismic risk analysis and the design of defense and nuclear facilities with regard to earthquake engineering.

MR. RICHTER: We may want to turn down the lights a little bit, please. Thanks.

Good morning. As Ardyth said, I'm Phil Richter with Fluor Daniel. I come to you as a structural engineer with 37 years of practice in civil and structural engineering. I guess I'm going to keep on for a while because I've been practicing and I haven't gotten it right yet. So, I'm working on it.

The topic today is seismic vulnerabilities and design issues and this is basically the agenda for my talk. By way of background and objectives, I'm just going to talk from a design perspective and talk about the issues of seismic design and what possible vulnerabilities I see and background that's covered in that scope that we talk about right up there, seismic design considerations-general. What I want to do because I'm talking to primarily geotechnical people, I want to establish a general view of seismic design considerations to form a base for talking about the more specific issues on the waste handling building and other surface facilities of the repository. So, we'll talk about the waste handling building concept, design criteria issues, design for vibratory ground motions, design for fault rupture, and some summary and conclusions.

Again, I've been in seismic design for quite a few years. I spent about 23 years on design of special facilities considering seismic effects and blast design. For the last 14 years, I've been with Fluor Daniel and we've been involved in a number of nuclear process facilities. And so, they're very similar to the waste handling building. I haven't been involved in the repository design at Nevada directly. So, my background and the details for that comes from studying the conceptual design.

Now, as I said, I'm going to give a real brief rundown on seismic design considerations, in general, just to establish a base. I'm going to talk about the design approach, the design process, structural systems, earthquake response, and earthquake effects. That's a tall order to talk about in just a few minutes, but just give us a little --as I said, just give us a little commonality of understanding for the more detailed talk.

Design for more industrial facilities has a certain level of protection and expected behavior. The basic hazard for normal industrial and commercial facilities is put in terms of a return period of 475 years. That's the uniform building code type of criteria. When we design for that, we expect facilities to--when the design level earthquake occurs, we expect facilities to have significant damage, structural damage and considerable non-structural damage. So, that's kind of the level of code protection that we have in normal industrial facilities. When we work on special or safety related facilities, the design ground motion level for vibratory ground motion is anywhere from 500 to 5,000 or for nuclear power plants we can be looking at the maximum credible earthquake and it's defined with respect to a fault that's active and they talk about 10,000 and 20,000 years.

The expected behavior of this type of facility, there is additional conservatism in that type of facility. So, the expected behavior is basically elastic with maybe some small inelastic behavior; in other words, very little damage is expected to perform its mission.

Next slide, please. Regardless of what type of facilities that we work on, this is just a general rundown on what the design process consists of. We start by establishing risk levels. If we have codes or other types of criteria, they're already pre-established. We have to define the inputs and the loads and forcing functions. We have to select the acceptable levels of response and then choose a structural systems, materials, configuration, and sizes. We then go through a determination of structural response for that design or configuration and we iterate on that analysis as we've changed the design. We can change the design because the layout of the building is changing for process or architectural reasons or we can change it because we've found some weaknesses when we did our analysis. We also can refine our design to try and remove certain levels of conservatism. We also have to consider non-structural systems and components very important. For normal buildings in earthquakes, we have given them a certain measure of consideration, but not a high level of consideration. We're talking about architectural elements. We're talking about heating equipment, ventilating equipment, and electrical equipment, and also piping systems.

In nuclear types of facilities, we have a lot of concern to design and evaluate process systems. There can be a great amount of attention paid and should be in nuclear process facilities and nuclear power plants to non-structural equipment that relates to the process. That may be 90% of the cost of the plant, for example.

Other issues that we have to look at in the design process include looking at the details of the structural engineering details, connections, ductility issues--that is the ability to deform beyond the elastic limit--and we also have to look at the construction aspects and the construction situation of the site in completing our design.

Now, very fundamentally structural systems for all types of structures, especially for buildings, consist of vertical systems and lateral systems. And, oftentimes, the vertical and lateral systems' mesh are identical, but in many cases they have different elements. The vertical system include space frames, columns, beams, trusses, walls and slabs, foundations. And, the lateral system, in addition to the foundation poured, includes frames and shear walls. The primary lateral force resisting systems are either moment frames or braced frames or shear walls or sometimes combinations and they're tied together by horizontal members that we call diaphragms. They're usually the floor and roof slabs.

Again, for just a kind of a simple view of how buildings perform and structures perform in earthquakes, for the lateral force resisting system--well, with mention of the vertical system, we don't pay too much attention to it normally in design because when we design for the floor loads and the vertical loads, the dead loads, then the earthquake vertical ground motions tend to be taken care of. So, most of the attention is paid to the horizontal ground shaking and we have what we consider a box like action and we have either a braced frame or a moment resisting frame--that's what the symbol is, that means we have rigid joints--and you can have a shear wall that covers the whole side of the building. And, we have to have those in two orthogonal directions to take care of all the earthquake forces where the earthquake shaking can be in any direction, any horizontal direction. It might be important to realize that this box like action really acts like a deep beam. Each of these frames acts like a deep beam. And, if we have a braced frame, it's a truss. If we have a shear wall, it acts like a deep girder. And, if we have a moment frame, it acts like the so-called virendeel truss with the rigid joints. And, so the building basically under lateral inertial loads performs in story to story distortion that's much more linear than if it was a flexural beam which is a thinner beam and that basically is how the structural engineers look at the design of these types of elements.

Okay, time for my slide projector. Just real quickly, I'll get into some nuts and bolts here. This is a rigid frame building under construction. Basically, the idea of the rigid frame is that we have a rigid connection between beams and girders and that's what gives us our lateral force resistance in modern high-rise buildings or even in low-rise buildings when we have a rigid frame. Rigid frames can be designed both as ordinary or ductile frames, which means that they have special--the ductile frames have special design characteristics that allow them to distort much further and rotate these joints under a heavy earthquake lateral loading.

Next slide. Okay. This is a braced frame building. Actually, most braced frame buildings can't be observed. This is an architectural feature, also. Many of them have the braced frame interior to the building. And, one thing to remember is that any of these lateral forces, since most of them are inside the building, they can either be on the exterior walls or interior walls or they can be a combination of both. They can be interior column lines or on exterior walls. This is a concrete shear wall. A lot of them, as I said, might not ever--a lot of buildings, you couldn't tell by looking at it. This one, this whole side, is a concrete shear wall.

Those are basically the three kinds of lateral force resisting systems that are primarily used for design of buildings.

This is just a quick view of structural components and elements that was meant to show non-structural elements and these are architectural ones, such as ceilings and glazing and wall cladding, and also some furniture. And then, there's other things like piping and HVAC ducts and then, as I said, in process type buildings, we have a lot of other equipment. In these buildings, we might have generators, electrical equipment, and pumps, and so forth.

Okay. We'll turn that off for a minute. I'll go ahead with it on this. Basically, a consideration of the structural engineering earthquake response is important. I think most everybody here knows that buildings respond. When they're responding elastically, they respond in modes of vibration. Basically, it's like looking at a vibrating string. They have modes of vibration. Fortunately, for simplicity, buildings primary respond to earthquakes in their fundamental modes of vibration in each orthogonal or horizontal direction. We have to consider the ground motion input and then the ground motion input is normally amplified by the building because of the relation of the period of the ground shaking to the period of the building and we have amplification. And, the damping is the frictional force that tends to resist the motion.

When we look at inelastic response when we have high levels of ground shaking, we get into inelastic response of buildings in normal situations. We have to consider the ductility, that is the ability of the building to distort beyond the normal elastic range. In other words, the force is no longer directly proportional to the deformation. In moment frames, we have distortion which is primarily induced, if it's well designed, by hinging of the beams and columns. Many times, we hope to have a hinge in the beams.

Then, one thing to remember is that once we have inelastic response, that prescribes that there's a certain level of permanent deformation in there; therefore, some level of damage in a building once we have inelastic response. As I said, normal industrial buildings, we expect to have considerable or significant damage during the design level earthquake.

Next slide, thank you. Okay. This basically is a diagram of a multi-story building showing how hinges form in beams during a lateral loading and basically that results in some level of distress or rotation of the beam at the column joint. This is a concrete beam that illustrates the hinging effect or the rotation into the inelastic range of a concrete beam due to earthquake effects. And, one thing that we would like to call out is that shear walls normally are damaged--once we're getting into the non-linear range, we get damage that's normally manifested by diagonal tension cracking. And, oftentimes, it's over places such as--this is a stair-well or a stair door. This happened to be a picture of the damage of the Kaiser Hospital in the 1971 San Fernando Earthquake. So, just a quick rundown of these items. I think that finishes the slide part. Okay. Now, basically, that's our general design considerations and issues for general building considerations.

And, now the waste handling building concept. It's a three story concrete shear wall building. I'm really addressing all of surface facilities that we might have, but I want to focus attention primarily on what I would call the high hazard facilities. So, I'm using as an example the waste handling building and I'm using the example from the recent conceptual design performed by Bechtel. So, we have a three story reinforced concrete shear wall building that's partially buried and it's a very heavy building.

We'll go on to the next one. I don't expect anybody to be able to read this. This is a print of the design drawing for the conceptual design. It's pretty complex and this is a plan view. We'll go on to the elevation section. I didn't even get enough detail on there, I think, to show the complexity, but it is a quite complex layout with a lot of elements. But, in order to get something that we can use, I prepared a simplified version of the plan view and the elevation views. And, this basically shows the principal elements of the waste handling building.

On any of the nuclear process facilities, we normally have hot cells and there are some more hot cells. And, those normally for radiation protection have concrete walls that are between four and six feet thick, reinforced concrete. The exterior walls have to be designed to maintain the mission and protect the internal workings of the building and those are normally 18 inches to about 42 inches thick. So, basically, what we have is a heavy concrete building, often partially buried, that has a lot of heavy shear walls.

Next slide, please. This is cross-sectional views of the same building. You can see some of the hot cell locations. This is the receiving and shipping bay that we discussed a few minutes earlier.

Next slide. Now, I think it's worth spending just a few minutes talking about design criteria issues and current DOE practices. Basically, the criteria that I'm relating to for seismic design is something that's been developed over a period of several years by the Department of Energy for use in natural phenomena considerations and design for their facilities. And, we have considered the categorization or classifications of buildings by use and occupancy and, therefore, we assign risk levels which relates to the input and the resistance of the facility. And, the DOE also has requirements for analysis. We have to look at elastic analysis primarily and we look at cell structure interaction.

And, the level of documentation follows pretty much nuclear power plant type of practice following NQA-1 quality approach. So, we have in place some DOE design criteria that would make a lot of sense for us to use to design this waste handling building and I believe it would work very well.

Next slide. This, to me, is kind of a summary that talks about the levels of risk for different categories of buildings. These are the four occupancy categories; general use, important or low hazard, moderate, and high hazard. Primarily focus on the high hazard facilities or perhaps we could judge that our facility is moderate hazard, but let's assume that it's high hazard.

This is the hazard exceedance probability, in other words, for the earthquake ground shaking, and we would have a $2x10^{-4}$ requirement if it was high hazard. So, basically, the return period for moderate hazard would be 1,000 years and for high hazard would be 5,000 years.

The performance goal is basically the risk of failure to meet the mission of the building or the facility. And, for the moderate and high hazard facilities, the DOE has established high ratios of performance related to the input. So, basically, for a high hazard facility, such as a waste handling building, we could say that the performance that we're looking at that we're trying to attain this probability of protection would be containment of nuclear materials and radiation. And, so if we can achieve that mission, we have to do it with this level of assurance, 1 in 100,000.

Okay, next slide, please. Now, I've looked at the site-specific input which is very important for design of our waste handling building and other surface facilities. We have to look at the risk or the hazard which is the probability of peak ground acceleration, and then once we establish the level of peak ground acceleration that corresponds to the pre-determined level of risk, then we can develop a design response spectra for the site that really defines how we do our analysis for earthquake motion. We also have to define vertical ground motions so that we can consider those in the analysis. We also have to look at underground nuclear explosions that are in the adjacent areas to see what effects they may have on the design of our building. And, last but certainly not least, is the effects of local fault rupture.

Okay. Real quickly, these inputs that I have here or criteria come from the conceptual design reports that were prepared earlier in the last several years. This is basically the risk of ground shaking in terms of peak horizontal acceleration and it's presented and goes much higher than normal curves would because we went out to a very long return period, perhaps because we're relating to a repository that has a long design life and perhaps because of the nature of the fault mechanism that may occur at the Nevada site at the Yucca Mountain site.

So, at any rate, the 10^{-5} occurs about here and it's probably about .8g or something like that. But, if we go to a 5,000 year, it would relate more to something like .5 or .6g, as I understand it. The current design that was accomplished was at .4g. So, we would have to go from a 2,000 year return period to a 5,000 year return period if we consider this a high hazard facility. That has yet to be determined, I believe.

Next slide. These are examples of the response spectra that we had used for design. This is the well-known NRC generic response spectra developed by Newmark & Kapur and it's used in a number of nuclear power plants and scaled to the ground acceleration. This is a site-specific response spectra. I believe it was developed by Blume for the Nevada site for the repository.

Now, in considering underground nuclear explosions, we compare response spectra to see what kinds of strength of shaking we might have. The lower curves in each case are the mean value of the underground nuclear explosion and the upper one is the design earthquake response spectra. As we can see, in general, the earthquake motions provide much stronger ground shaking. However, in one of the reports that I read, it indicated that the nuclear explosion should affect the response spectra or cause the design response spectra for vibratory motion to be adjusted in the longer period range. So, that would have to be looked into, also.

Now, the risk of faulting as judged in the conceptual designs looks like this. It starts at a very low probability of exceedance and goes to a further low. We start at almost 10^{-7}. And, at that low value, it's only one centimeter for the amount of vertical rupture that would be predicted. However, the point is that there's no known faults, no identified faults, that would be expected to rupture at the site. And, so this probability study was based on the risk of unknown faults rupturing and that contributes pretty much to the low probability that would be expected.

Okay. Getting down to the real nitty-gritty and wrapping up pretty close here, the next five slides, these are the issues that relate to the waste handling building designed for vibratory ground motion. We have to establish the design criteria and the level of conservatism. That directly relates to the site studies that we're carrying on now and we need to also develop what the level that is appropriate to design to, what the criteria will be. Once we have that, then we can provide the analysis, develop the structural resistance, or prescribe which levels of resistance are appropriate. We can address ductility issues, all very much like they're done in UCRL-15910.

The main things that we have to do and this was discussed in the conceptual design is looking at the designs that--you basically can provide increased design assurance and have a better design if you provide structural regularity and you have to have continuity of the structure and you provide the ductility and connections appropriately. We also have to account for the effects of embedment, and normally when we're looking at designs for vibratory ground motion, the effects of embedment are generally a plus and that's because the effects of embedment tend to attenuate the ground shaking that affects the structural response.

Design for fault rupture is another issue. That's a very important issue. First of all, I think it's important to say that we should make every attempt to design the structure in a location where there's no known faults. And, I think that can be accomplished in talking to people. It sounds like that's a fair target. I don't think it would be appropriate to locate the building on a fault if it's at all possible.

The issues for design of fault rupture are what is the fault displacement that would be expected? What kinds of strength do we have to build the building to to accommodate that displacement including the consideration of the orientation of the slippage of the fault. There is a limiting force that earth can apply to a rigid concrete building. So, we have to be able to determine at what point will the force be limited, to what level will the force be limited on the building, and the effective embedment is important there. In the case of design for fault rupture, the effective embedment is somewhat negative because you tend to get more forces applied to the building because of its embedment. If it's totally on the surface, then there's less forces applied.

Now, in the conceptual design studies, the estimated level of resistance for fault rupture were vertical slip, 1 to 2-1/2 inches, and horizontal, 5 to 15 inches. That seems to be a pretty conservative estimate based on what I looked at in the studies. It seemed like quite conservative. One particular report that I'm familiar with was done by EDAC a number of years ago on the Vallecitos test reactor and it showed that that particular reactor structure, that test reactor structure, could withstand one meter of fault displacement. Of course, they're not the same building, but the point that I'm making is that we can design or we can find that we can get pretty high levels of resistance for buildings.

The main issue, I think, in design for fault rupture is that it isn't done. There's very few examples of design for fault rupture for building structures and we may end up having to do it in this case and it seems like we have all the tools in place to accomplish it, but we are hampered by the fact that there is a lack of experience. And, it may mean that we have to do a certain level of testing and further analysis to assure ourselves of our designs.

Mitigative measures, next slide, please. There's a number of possible mitigative measures when we design for local fault breakage. First of all, you have to determine the orientation of the fault and the limiting forces. We can do what I call the direct approach and just provide sufficient strength and also include ductility, so that the structure can basically accommodate the full fault slippage. We can also reduce the effect of the fault slippage by modularizing, breaking the structure up into a number of discrete modules, and that allows each of those modules to move relative to the other so when the force is working on it, it's basically moving and not breaking anything. And, that requires very careful planning and design details for the joints.

A number of isolation techniques might be available. One item that was mentioned would be granular bedding. That would allow for embedment of the buried portion in a well known, well behaviored media that allows a certain level of slipping and you can predict the slipping of the material and therefore have a good handle on your design forces that the motion is inducing.

Another item would be to put crushable bedding.

That's kind of like isolating it and the crushable bedding--therefore, the ground would move and you'd have a gap where this bedding material is or packing and that would allow the structure to--the ground to slip around the structure and just break the bedding. You can also just have air space or clearances and also there's schemes that I believe are feasible where you could have mechanical isolation. And, mechanical isolation would provide for a double foundation really. You basically separate the structure and have a double foundation and I would put the isolator separated on individual foundations and allow them to move under the structure and the isolators would accommodate the distortion that each one would deliver to the structure.

Next slide, please. Now, one more slide before I wrap it up. People have been talking for a long time about experience with these kind of facilities. And, I've done a fairly brief, but quite wide look at a number of facilities to try and find experience of heavy shear wall type structures in earthquakes. We have a number of instances where we can relate, but there's no real good experience that's been recorded. What I mean by good experience, I'd really love it to say we've got 10 buildings that we've looked at that have undergone .6g ground motion and there's no problem. But, we don't have records of that and there's probably two reasons for it. One of them is that these buildings don't get recorded. These types of buildings don't get recorded too often because they behave so well. Another one is probably that we may not have been fortunate, or unfortunate as the case may be, in having earthquake strong motions occur at the site of such buildings. However, I did look at these different earthquakes. There are heavy industrial facilities that relate to most of them. I can go over the experience with you. I'm running low on time. So, I won't take the time to go over the individual experience.

The one that I'd like to mention most was not in my initial review, but was provided by my colleagues at Woodward-Clyde. They gave me a document that I'll just reference here. I think it's kind of interesting. It's called the "Environmental Assessment at Yucca Mountain Site", Volume 2, and it's for the nuclear waste repository. Basically, it's an assessment and somebody in that document tried to do the same thing I did which was look at past facilities of this nature and see what kinds of experience we had. They came pretty much to the same conclusion. I didn't see any significant damage of any major structure and that was a heavy reinforced concrete shear wall structure. The closest example to this kind of structure was the Fukushima Nuclear Power Plant in Japan in the Miyagi-Ken-Oki Earthquake of June 1978. Don't ask me to repeat that again, please. And, that was a magnitude 7.4 earthquake and, evidently, the ground motion at the site that was instrumented was felt at .12g and the response was .25g, fairly significant, no significant damage. And so, basically, that's the story on experience.

So, in wrapping up, the fundamental issue is that we have to be able to quantify as accurately as we can with as great assurance as we can the seismic environment which includes the fault locations and fault slippage and the vibratory ground motion for the site, plus we need all the other standard types of geotechnical information for design of a structure, foundation information, and so forth. We then have to establish the level of risk that's appropriate for this facility and we have to build on the current concept and the current concept may change as the process and the architectural requirements change. The design approach for vibratory ground motion is very clear. The good things that we build into the structure are the design details, the details for ductility, and we have all the tools in place and all the criteria in place in the structural engineering community to provide a very highly defensible, highly acceptable design for this type of building.

For fault rupture, I believe we have all the tools in place to understand how the building works and to design for it. We have the handicap of not having done this type of work in any great extent before. So, that's a little bit of a negative, but I do believe we can handle that design very well. And, the other good news is let's--at this point, I would say we aren't going to design for a known fault because we should site the building accordingly and, therefore, we may want to consider design for some unknown fault that may occur which would have a small level of motion or movement and a very low probability. That might be a safety feature we want to incorporate in the design.

So, that's basically the remarks that I wanted to make this morning.

DR. ALLEN: Thank you. Let me ask you if Bert Swan's optimism is misplaced and we really cannot document the absence of significant fault beneath that site, the proposed site or any other site that's practically located, could you design a loading facility that's currently--with hot cells that could withstand a meter of displacement in any direction beneath any most critical part of that facility? Could you do it economically?

MR. RICHTER: Well--

DR. ALLEN: And, guarantee public safety, not--

MR. RICHTER: Right. Well, that's a tough question. I'll answer it. Basically, I think we could design for a meter of offset, but I don't know how economical it would be and I also think we should be able to find a local site that would not have that type of offset. That's what I would like to target, of course. So, I'm kind of begging the question.

But, if I had to come right down to it and said my life depended on designing it, I guess we could try and do that.

Bob?

DR. KENNEDY: If you had a design for something as large as a meter, do you think you could convince the engineering community, the wide engineering community, of the adequacy of the design? I mean, I tend to concur with you. I think individual engineers would feel they could do it, but I'm not convinced that we could convince people that--

MR. RICHTER: That's the tough part. I think that's a more important issue. I think you've raised the real issue. I think we could probably design for it. I think as we develop the design and did it, we'd gain confidence in it and we could probably provide our assurance, but you might have a heck of a time convincing the whole public and all of the engineering community. So, that's a good point.

DR. KENNEDY: On the other hand, if that could be kept down below a foot of movement, I think you probably could convince all of--a very large percentage, anyway. I'm not going to say all, but a very large percentage of the engineering community of the adequacy for these thick shear wall designs. Would you concur or not?

MR. RICHTER: That's right. Yeah, absolutely. I believe so. I think there's always going to be some that will be on the opposite side of the fence and I do agree that

10 or 12 inches is a better target if that's what we have to do. I'd prefer the one to two centimeters, of course, but I think we can handle that.

DR. ALLEN: I would again point out though that if we could ever come up with a universal cask, we could do away with the hot cell problem and, therefore, the hazard there is no greater than it is anywhere else in the transportation system.

MR. RICHTER: Right. In that case--sure.

DR. ALLEN: I think we--the fuel from where it's stored into the repository.

MR. RICHTER: In that case, we might have more of a metal shed building or something. It may not have to be a heavy concrete building.

DR. REITER: Phil, just a quick question following what Bob said and, Ardyth, maybe you can correct me on this. I think in 960, the site suitability criteria talks about reasonable available technology or some phrase like that. Again, the question that Bob says, using the criteria reasonably available technology accepted by the community is one foot or what number do you think you could design? Would it take extraordinary efforts that would contradict the--

MR. RICHTER: Well, I think there's reasonable available technology, but I'm not sure how well it's accepted by--you had kind of two parts of your question. I'm not sure how well it's accepted by the whole community. I think we could do it and I think we could have pretty high confidence. I do think it would take significant analysis and some further testing to develop our approach. You know, to really have a sound approach and be satisfied with it. I think there's a lot of things we could do. It wouldn't be as much of a slam-dunk as far as the design for vibratory ground motions. I don't mean it's a real slam-dunk because there's a lot of work to be done, but it's all very straight forward.

Bob?

DR. KENNEDY: Is the waste handling facility going to be designed in accordance with the requirements of UCRL-15910, the seismic requirements, or has that decision been made?

MR. RICHTER: I'm not aware of that decision being made. I would recommend that it would, but I haven't heard any--I don't know of anything that has established the design guidance and that's what my point was basically in the talk.

DR. SIMMONS: In response to that question, at the present time, the decision has not been made as to whether to use the UCRL Report. However, this is one of the topics that would be considered by the working group that I described earlier and a decision would eventually be made on that.

DR. DEERE: Russ McFarland, didn't we look in Sweden underneath one of the pools that had displaceable joints on it that the thing could move around with earthquakes, designed and built, I think, five years ago or four years ago? Do you know something about that, Russ?

MR. MCFARLAND: At the last trip to Stockholm or to Sweden, we visited the nuclear power plant at Aspo about 100 kilometers south of Stockholm. They showed us the structures for their waste fuel pool that were underground. And, the arches tied back--the roof--there was support arches tied back with rock bolts and the pools were supported on isolators. The swimming pool, 30 foot deep--30 to 40 foot deep pool were supported on isolators, seismic isolators, as I recall. Is that your recollection, Don?

DR. DEERE: That's right.

MR. RICHTER: Do you happen to know if that was primarily for the vibratory ground motion protection? It sounds like it probably would be.

MR. MCFARLAND: Yeah.

MR. RICHTER: But, that still doesn't necessarily answer experience in the fault design, but it--

MR. MCFARLAND: No, it was strictly vibratory, my perception.

DR. ALLEN: Any comments or questions from the audience?

DR. DEERE: One here, another one. You talked about building in some kind of a joint or dividing the thing up. A recent concrete gravity dam about 250 foot high was completed the last couple of years in the South Island of New Zealand

and this has crossed a fault zone around 10 meters thick of very heavy clayey fault gouge which just a kilometer upstream runs into a regional fault that is known to be active. And, the question was could you generate sympathetic movement, the same thing we heard this morning, on the one fault. And, the answer by the geologists and seismologists was, yes, it probably could and they even predicted what that movement would be based on strain measurement--or stress relief measurements of the in-situ state of stress. And, they predicted that there would be a movement in this direction, right hand, this side up, this side over of 20 centimeters. So, they consulted then their structural consultant from Switzerland and he and the New Zealand department designing this dam decided they would make a joint that would allow a movement of one meter in any direction. Because they said, you know, we just don't know about these seismologists.

MR. RICHTER: Sure.

(Laughter.)

DR. DEERE: And, seismic geologists. So, this became quite a structural detail and they had to bring in a little soil mechanics to help along the way. So, they do cover in the foundation area. They excavated down about 60 feet and backfilled that with clay to give them a little bit of an impermeable zone in case there were movement. And then, downstream, they put in a long trench, backfilled with gravel, which again was go down to the foundation, so to try to keep the water from coming out, try to keep the water from getting to the crack, and if it gets out, let it come out in hopefully a very controlled condition. But, that is certainly a novel--I wouldn't say it's a common practice--but I believe it's the first really major concrete gravity dam which is an un-reinforced structure that is built with the slip joint to allow this type of motion.

DR. ALLEN: And, every engineer agreed it would work.

(Laughter.)

DR. DEERE: This information has been presented to the international community and to large dam conferences and there has been a great deal of interest.

MR. RICHTER: I suspect not too many engineers have heard about it yet, but it sounds like a good model. I mean, there is at least a situation where people have designed a really major structure. That's good.

DR. DEERE: Absolutely.

MR. RICHTER: I think Jay has been designing--haven't you been designing some structures for the Hyperion? Does that relate to fault motion? You're going to maybe talk about that later?

DR. ALLEN: Whoa, whoa, whoa. Could you go to the microphone, please?

DR. MERRITT: Yes, this is Jay Merritt. What Mr. Richter is referring to is the north outflow replacement sewer for the city of Los Angeles to carry the sewage from the valley north of the city to the Hyperion Treatment Plant. we cross the Englewood Fault at the northern reaches and we have designed a system to cross that fault to accept approximately seven inches of displacement.

MR. RICHTER: I think there's probably a number of examples of linear structures that have been designed for fault displacement. I'm not aware of specific examples, but I know I've read about those in the literature. I think Bob has been involved in some of those designs related to design of lifeline types of structures, too. So, there are examples, but they're pretty few and far between, and I'm not sure of building examples.

DR. DEERE: In the field of subsidence engineering due to either a solution mining of salt or coal mining, particularly in the older days when there was a great deal of surface subsidence, when one studies the structures one is amazed how a structure really can often bridge across this opening. You can find, for instance, the fault that will have been created perhaps with four or five inches of opening and then a down-throw of one or two feet in a zone of subsidence and find that the house is intact because it has a floor slab with just minimal reinforcing. And, the same with some of the commercial buildings. That doesn't always happen because there's several houses that are two halves now.

MR. RICHTER: Well, you're talking about a subject that's kind of dear to my heart because in my earlier days--I won't say how many years ago--one of my earlier assignments was with the Port of Long Beach and you're talking about subsidence. We had approximately, if I remember, 30 feet of vertical subsidence on facilities. Basically, they went down pretty much together, but in the 30 feet of subsidence, there was some horizontal ground compression that I was working on on the transit shed which is a long steel structure. It was about 1,000 feet long at Pier A and that built in a lot of compression and actually looked like a classical rigid frame deformation that structural engineers draw. It built in something like a foot of displacement and caused distortion in that structure. We basically refit the structure after that, but it still stood up and was a valid structure.

DR. DEERE: In these bowls of subsidence where the inner part may undergo a compressive strain, as I recall, values of six inches per 100 foot are rather common in areas that have subsided four, five, or six feet, something like this.

MR. RICHTER: Right.

DR. DEERE: But, in the outside of this bowl of settlement, there is a tensile or an extension strain region. And, that's where we pull pipes apart and break the telephone lines as they cross these particular zones. But, that magnitude is only between a half inch and one inch per 100 foot of length.

DR. ALLEN: Baldwin Hills is a good example.

DR. DEERE: Yes, exactly.

DR. ALLEN: Thank you very much. Let's break for--oh, Bert, did you want to say something?

MR. SWAN: One comment with respect to--you threw one viewgraph up, the DOE performance goals. And, my understanding in terms of NRC and regulation, everything is couched in terms of consequence and release. And, without the interface between the probability of the hazard occurring versus the consequence, as a scientist or as an engineer trying to come up with design criteria for use in the conventional practice thinking of low probability of occurrence of a hazard, rather than the consequence, right now I'm not aware that there are target probability levels. But, if we could define acceptable low probability levels, one of the approaches for the faulting hazard will be or could be to say that the hazard is acceptably low. Then, you follow that through to consequences and that's even still lower. But, without those probability goals for the occurrence of a hazard, not the occurrence of the risk, it's hard to conduct your investigative program because you don't have the target that you're shooting for. We can define what that number is, but there's no way now of taking probability of hazard and converting it to a design parameter. It's one of the stumbling blocks we're coming up with and it's going to be easier to avoid the faults than it is to design for them. But, if it became impractical to avoid them, one important step in the process would be to define what the level of acceptable hazard is and see if we can define that as a low number.

DR. ALLEN: From a probabilistic approach, you could incorporate the 90% of the engineers who agreed and the 10% who didn't.

Okay. Thanks again. Let us break for lunch, and after one hour or at 1:20, we'll try to reconvene.

(Whereupon, at 12:20 p.m., a luncheon recess was taken.)

AFTERNOON SESSION

DR. ALLEN: May we come back to order, please? We are still missing a number of people, but it is necessary for us to evacuate this room by 5:30. And, I think it's best we get on with the schedule.

Ardyth Simmons would otherwise be introducing our next speaker. Our next speaker is J.L. Merritt. Jay Merritt, an expert on tunneling who is with the Woodward-Clyde organization in Las Vegas.

Jay?

DR. MERRITT: Thank you, Dr. Allen.

Although I shall be talking about numbers of structures in the briefing, I've also been asked to put that number into a time context because, of course, it's one thing to have 50 structures in one experiment, it's another thing to have 50 structures in 20 experiments over a series of years.

So, the experience at the Nevada Test Site which I shall be talking about began with the Rainier event of 1956. That was the first underground explosion test which involved data from the structure of the tunnels in the same medium. Since that time, there have been more than 20 such experiments, two dedicated experiments, and six experiments in which a large structural program was added onto the experiments. So, it's 20 experiments over the period since 1956 that I should be talking about when I get into the UNE's.

In this briefing, I shall be talking about the subsurface facilities, as indicated here, but I would emphasize that I'm really talking about the tunnels. I'm not talking about the contents of the tunnels. That's a different subject. I'm merely talking about the response of the tunnels, themselves.

As I started to put this briefing together, I was reminded of an experience of about 10 years ago, a little over 10 years ago, the fall of '81. Several of us had the opportunity to visiting the Seikan Tunnel, the staff at Seikan Tunnel. This is the tunnel that was intended to carry the bullet train between Honshu and Hokkaido in Japan. And, someone in the audience asked about their experience with earthquakes and they indicated that there had been a devastating earthquake mid-70's which had totally destroyed the town of Hokkaido, and when the staff, the miners, and others came up from underground at the end of their shift, as a matter of fact, they were nonplussed to find so much there at the station because they hadn't even felt it in the Seikan Tunnel just 2600 feet below the town of Hokkaido.

This is the items I will be discussing, the historical information, observations, UNE's, potential modes of failure, and a summary.

Under the historical experience, I've just given some highlights here of the U.S. literature. There is more than 200 documents in U.S. literature. There is, of course, also the Japanese experience, the Greek experience, the South African experience, and so forth. I've only hit the highlights of these. The summary is that our experience indicates that we've had very good experience with behavior of tunnels to earthquakes. I shall later be referring back to these two numbers, .67 FPS and 3.1 FPS, from the paper by Dowding & Rosen.

And, I'm afraid Jim Phillips caught the disease from me. I had the opportunity of correcting this and please correct it in your notes. It should be no damage for less than .19g which corresponds to .67 FPS, minor damage for estimated pre-ground accelerations of less than .5g which corresponds to approximately 3.1 FPS for the model used by Dowding & Rosen.

I would also emphasize before moving on, the most recent paper by Sharma & Judd in which Bill Judd concludes--well, the authors conclude that there is considerably less damage below 50 meter overburden and no heavy damage below 300 meters of overburden in all of the cases they've looked at, the 192 cases they've looked at.

I should probably also mention in this <u>Proceeding</u>

<u>of the Workshop on Seismic Performance of Underground Facilities</u>, 1981, by Wendell Marine, the editor. There are a number of authors in that; Tom Kuesel, H.R. Pratt, Cole McClure, to mention just three. And, because they address different questions, there are various indicators of damage used by each of them.

The historical information is quite broad. It includes traffic tunnels, highway tunnels, railroad tunnels, water tunnels, sewage tunnels, personnel tunnels, and so forth, and a tremendous number of different types of line and type. I would highlight from the 192 cases and 85 earthquakes reported by Sharma & Judd that, given the many hundreds of miles of tunnels around the world, there have doubtless been innumerable cases of no damage and consequently no reports. The 192 cases are, of course, only of damaged tunnels. The information--because they were strictly targets of opportunity--represented by the tunnel damage created by the earthquakes, there is literally no data. Even P-wave velocity and geologic features were seldom given for the cases summarized.

Again, to emphasize the points that I shall be picking up later, there's no damage for an estimated peak surface particle velocity for less than .67 FPS and minor damage at an estimated peak surface particle velocity less than 3.1 FPS. And, little damage below 50 meters and essentially no damage for overburden depths greater than 300 meters.

Partially as a result of the fact that there's very little data from the experience of the 192 reported damage cases and partially because there is a wealth of data from the UNE's, we have included a great deal of information in the remainder of this briefing on the UNE's and then we move from there into postulated modes of failure.

As shown here, the plan view and the elevation of a typical experiment at the Nevada Test Site. The plan view here, these are the Nevada state coordinate system in meters. This is at the tunnel level, so that you're looking at Paleozoic material here overlain by the various ashfall tuffs. These are all ashfall tuffs. Within the range of this, we go from Tunnel Bed TT2 to TT4I. It shows some fairly large fault displacements, but having said that, I would emphasize that the amount of displacement on most of these faults is very small and peters out, for that matter, as you get above the tunnel level in many, many cases.

The typical experiment is as shown here with the so-called working point, WP, at this point and the main experiment is normally arrayed along this drift. Because there's a tremendous amount of construction involved in creating one of these experiments, they will put in a bypass drift which allows construction to be supported at various points along the main drift off this bypass drift, but also allows lay time access to the front end. If there is a dedicated experiment, as was the case in Diablo Hawk, the dedicated experiment is usually arrayed in drifts such as these off of the bypass drift.

Later on, I shall refer to briefly the possibility of pre-shock condition rock. What I'm referring to there is often a working point or the vice emplacement point will be in proximity to later constructed tunnels. And, what I'm referring to is the possibility of this stress wave propagating outward from this working point having modified the rock, the possibility of having modified the rock in this area.

Moving to the elevation over here, this elevation is right along the access drift of the experiment I referred to here. So, this now shows the tunnel beds above this, typically 1200 feet of overburden over the working point and the principal experiment. Tunnel beds ranging in thickness and solicified beds, as little as a few millimeters in thickness, but they were very unusual. More often, you'll find them at least tens of feet on up to hundreds of feet in thickness. And, a rhyolite cap rock over the entire area.

These two pictures will give you a feel for what the geology looks like in that ashfall tuff. This is a series of sub-beds, obviously. This is about an eight foot high opening here. So, obviously, you're looking at like a foot thickness of the sub-bed in that particular case. So, a series of sub-beds as illustrated here, as defined by the difference in coloration.

We move to the one over here. A different set of beds including the red beds up through here. This is a double jack in here, a small--in the picture here. This drift was originally 18 feet wide and was done in two lifts using a road header. The first was a bottom heading and then this was the upward excursion from the road header in order to begin the top lift. You're looking up at the series of beds, again different in colorations, some inclusions within the bed.

I would point out that we do have a minor fault at this point. You can see a little offset at that particular point. I illustrate that for a number of reasons, no the least of which, as I'll in a moment discuss, the various parameters that have been found to be significant in a regression analysis of these data.

Here is a typical tunnel ready for execution of the experiment. This is an 18 foot wide drift, 18 feet high. The high pressure tubing you see coming across here, running up the wall, goes to instruments that are imbedded back into the rock here as much as 50 to 60 feet in some cases. These are targets for high-speed photography. This is the set of flashbulbs used to eliminate those targets and then these are the cameras that were used to get those data. These two crossing pipes are passive gauges to measure closure in the horizontal dimension and closure in the vertical direction in that tunnel.

This particular tunnel, the 18 footer, was one of a series of experiments which involved the 18 foot, the 13 foot as you see right here, the 9 foot structure behind that, and then coming back towards you there's a five foot ring drilled opening, five foot diameter, a two foot drillhole, and a nine inch drillhole at the end coming back this way. An intentionally designed set of experiments to determine "size effects".

The miners were instructed in all drifts 13 feet and smaller to only put in those bolts required to provide safety. These were not pattern bolts. If they crossed a minor shear zone or something of that sort, they'd perhaps put two bolts up with some pure steel mesh between them, so that there was no bolting in the 13 foot and smaller except for those that required, as in this case, for static safety. In the 18 footer, there were bolts at approximately 1.2 meters, four feet, spacing, but in most cases, mesh was not provided in here unless we had a truly unstable condition, and in that case, we did put mesh in. And, the fault that I showed you before runs right through this drift from about this point on back to 45 degrees over here.

DR. ALLEN: Jay, where was the device itself?

DR. MERRITT: The device itself in this particular picture is off to the right.

DR. ALLEN: Off to the right?

DR. MERRITT: Off to the right, yeah.

DR. DEERE: What distance?

DR. MERRITT: Can't tell you that, sorry. The reason I can't tell you that is I'll be subsequently referring to particle velocities and accelerations and that sort of thing. And, when you combine the two, that's information I can't divulge.

The regression analysis that I mentioned used these definitions of damage. These definitions of damage were created by DNA for the deep basing program. The deep basing program was pursued by the Air Force in the early 80's. It was a method of providing a secure reserve force. This was a series of missiles that were going to be placed fairly deep underground and were able to take the full postulated attack at that point, and then following the attack, come to the surface and retaliate. Level I damage being none ranging up to Level IV requiring complete re-mining and rehabilitation of the tunnel with, of course, II and III being lesser degrees of damage between those.

Now, all from a specific series of experiments, I've picked some pictures to try and illustrate Level I through Level IV damage. Here is an instrument alcove. The instrument package being here supported on bungee cord in an alcove off of a much larger drift with shotcrete on the surface of the alcove. As Jim Phillips has indicated, you do get minor dropouts of the shotcrete, but these are of the order of a half to an inch in maximum dimension ranging on up in a few cases to about three and a half inches in size. Typically, from areas like this where the shotcrete has pulled away.

Level II damage over on the left side here, this is in the size effect experiment again. Block sizes here ranging up to two feet or so in size which, of course, fell out of the back indicated on here and that would have required, of course, some rehabilitation.

Level III damage, still a larger and more continuous failure, and in the background approximately a 50 ton block of rock which fell out as a result of opening of that particular fault that we saw in the headway of starting to create the large drift. This incidently saw approximately seven feet per second in the actual experiment and in excess of 19g, not unlike the situation that Jim Phillips talked about this morning.

The next picture was taken at a much closer range. This structure saw about 25 feet per second around 300g.

This originally was a nine foot drift, side to side, and nine feet high. The original crown of the drift was right there. The back of the drift was right there. This cavity extends about six feet into the top and this rubble was created, of course, by the fallout at the back. These rock bolts, as you may have already inferred, since this is above the original crown of the drift, these were put in for safety when we reopened that drift and went back in. As I mentioned, this is about 30 feet per second, 25 feet per second, and several hundred g. This was a totally unlined drift. So, even a totally unlined drift--by unlined, I mean, no rock bolts--even a totally unlined drift in this tuff was still passable after the event, once the rock bolts were put in there to provide safety.

Now, to give the results of the regression analysis, this regression analysis was done by Tom Kipp under the direction of Dr. Kennedy in the audience today and Mr. Steve Short. They postulated the series of 11 parameters of what was considered to be potential for creating damage and did a multi-variant regression analysis and came up with this equation for damage level. The damage level in this case might be 1.25, 2.8, 5.6, but it was measured against the Level I, the Level II, the Level III, the Level IV damage that I presented from defacing. The constant turned out to be 1180, opening width to the .41 power, geologic setting to the .21 power, fault proximity in the denominator to the .04 power. A very important equation to illustrate the importance of the various parameters as indicated here; opening width, geologic setting, ground shock orientation, and so forth.

DR. DEERE: What did you actually measure or characterize in your geologic setting? You had a numerical value of something.

DR. MERRITT: Maybe I should refer this to Dr. Kennedy since he was responsible for putting it together. You have --well, maybe I should start and then Bob can correct me. You have reworked tuff in some of the areas where a fair amount of a volcanism or tectonic activity occurs. So, you have from the weak side reworked tuff to fairly pristine, unworked and so forth at the high side.

DR. KENNEDY: We basically--I mean, all of these shots occur in limited number of tunnel beds. And, those tunnel beds can be classified into about--I think it's three categories from basically pristine to reworked tuff and then there's a category in between. That parameter was just given a value of 1, 2, or 3, depending on which one of these types of rock you were in. It was relatively imprecise, but it seemed that the data we were reviewing could be broken down so that there was more damage in the reworked tuff than in the fairly pristine. And, so we just classified the tunnel beds. And, so it's really a classification associated with tunnel beds and what type of rock is in those beds. None of these tunnel beds are like where the repository is. So, I mean, I'm not sure how much this is extrapolatable. I don't think it is.

DR. MERRITT: However, I think it is important, Dr. Kennedy, to note, as I tried to in the briefing, that it does show the dependence of damage level on various parameters here like opening width. But, I would re-emphasize what Dr. Kennedy has already said and that is one must use it with caution which is the purpose of the next chart. It's very good for the 256 cases that were used, but don't lose sight of the fact that this was developed primarily to give guidance to the test site staff on how to proceed subsequently for designing support facilities like instrumentation off of--and data are still being accumulated in that area.

I mentioned the other parameters, the independent parameter being damage deep in the parameters as indicated in earlier equations. Other parameters considered is indicated here. The first two turned out to be uncorrelated. The next two turned out to still be of import, the pre-shock rock conditioning that I mentioned, but there was insufficient data in order to do the regression analysis against, the cases available.

The next chart takes another slice of these same data.

DR. DEERE: Let me stop you for one second, if I may, before we leave the topic that I just brought up. When you showed the picture of the fault and a major block had dropped out in one of the things, obviously that could have been in one sequence of beds or another sequence or another sequence. But, that particular are was certainly worse than the rest in that bed. So, you really needed something on rock quality perhaps over and above just bed type, even though your experience showed that one bed was different than the other.

I mean, this is a question or a statement, what do you think?

DR. MERRITT: I certainly agree, Dr. Deere, that there are a number of parameters that have to come in to play in evaluating these data and certainly feel more information has to be considered when we try to extrapolate these data to the welded tuffs at Yucca Mountain.

DR. KENNEDY: On that issue, our experience has been if the fault crosses the drift pretty much perpendicular to the drift and if no movement is triggered on the fault as a result of the detonation, the fault does not seem to affect the tunnel performance, in general. If the fault is at a shallow angle across the drift, then it has a substantial effect. If movement occurs on the fault as a result of being triggered by--I mean, these are close in to these detonations, very high ground motion. But, if you trigger movement on the fault, then you can get the local damage right by the fault. But, if it's perpendicular to the drift, that damage extends maybe five feet each side of the fault. It's very localized.

In response to the other, I said wrong and I've got out my notes now. The three categories were primary ashfalls, that was the best; reworked ashfalls, the middle; and fractured reworked ashfalls were the worst. There are two of the tunnel beds that are fairly notorious for lots of fractures. The performance of these minimally hardened openings is much worse in those.

DR. CORDING: What do you mean by reworked?

DR. DEERE: Must be rubble. Rubbly?

DR. KENNEDY: You're getting a little bit out of my area. The are the names that the geotechnical people have given to these beds and we have no--there must be someone here who can give that. I can't.

DR. CORDING: Basically, it was a natural geologic feature, not something happening during the--

DR. KENNEDY: Yes. No, no, it's natural.

DR. CORDING: That's all I--

DR. KENNEDY: That they classified the three different --the beds into these three different categories and our experience is the performance does differ significantly in which bed you're in.

DR. MERRITT: This is an attempt to--also from deep basing--take another cut at this information. We have found for years the Hendron & Aiyer procedure a very powerful one for doing trade-off studies. If you want to make a decision of just how unconfined compressive--affects a problem, Hendron & Aiyer allows you to do literally hundreds of solutions in the matter of an hour on a not very sophisticated desktop computer and then allows you to make decisions on what thicknesses of material you want to use. If it's one type of lining as compared to what spacing of rock bolts you might want to use for another type of lining.

As a result of deep basing, Dr. Kennedy and his staff looked at our experience with safety bolts and I would emphasize this was back in '81-82 time frame. What light pattern bolts and mesh--by light pattern, I mean a spacing of something in the neighborhood of a meter by a meter, three feet by three feet; heavy pattern bolts down to two foot by two foot. And, back in the '81-82 time frame using the Hendron & Aiyer procedure, the strain at the interior surface for those three cases, safety bolts of approximately a 4x4 pattern, light pattern bolts of 3x3, and heavy pattern bolts of 2x2, this turned out to be the Hendron & Aiyer calculated strain at the entry of the surface, .5%, 1.5%, and 2.5%.

My staff back in the '81-82-83 time frame also were looking at the composite integral liner and backpacked and I'll be showing examples of those subsequently. So, let's hold in abeyance what a composite integral and a backpacked structure are. But, at the interior surface of the composite integral lining, we came up with a Hendron & Aiyer strain of 5% and for a backpacked structure, I'll also show a picture of a backpacked structure, 7.5% strain at the backpacking rock interface.

The Hendron and Aiyer procedure is a closed form solution developed back in 1970 and published, a closed form solution assuming a hydrostatic stress field around an opening with various slanting types on the interior.

The question, of course, is how do you relate that to other conditions in the rock surrounding the opening and I shall get into strain gradients in a moment. I've added a chart over the lunch hour to get into the impact of strain gradient. But, I took these as invariants--and I'll also discuss in some detail why I consider those to be invariants --and then applied the straight P-wave strain because it gives a lesser value for the result than you get for the S-wave coming into play and you get 38 FPS from a P-wave, just 7500 feet per second times a .5%. You get 19 FPS if you assume a stress concentration factor of 2 or a strain concentration factor of 2 and you get 13 FPS for the stress concentration or strain concentration of 3.

If, on the other hand, you take for Yucca Mountain a P-wave seismic velocity of about 11,000 FPS, you'll get to two significant figures, 55 FPS and 28 FPS and 18 FPS. The final overlay for this brings the same sorts of calculations for the other values of strain here again assuming these to be invariant. And, we have data for structures that have actually seen those kinds of particle velocities out there on the right hand side assuming these kinds of strain levels.

Now, why did I assume those to be invariant? Again, back in--I believe it was '82-83 time frame--five agencies of Government labs and independent contractors were asked to make their best estimate of what was going to happen in the tunnel experiment which was about to proceed. The results of the calculations using the then more sophisticated non-linear finite element programs ranged from just under 2% on the low side to 28% on the high side and yet all of the five agencies said that they agreed that there was going to be light to moderate damage despite that large range of strain values.

A rather long digression to merely emphasize the fact that even today's finite element programs don't give you the mechanism of damage. All it gives you is the result of running a stress wave across the opening, shaking it with the El Centro Earthquake or Loma Prieta Earthquake. It merely gives you the response of that particular structure or that particular tunnel in this particular case for that given--one still has to make the decision of what is the mode of failure.

So, over lunch time, I added--and, I'll come back to that chart--this handwritten, highly simplified situation. If I have a stress wave, a P-wave running straight down the axis of the tunnel, there's no angle of incidence whatever--it's going straight down the axial direction--axially, there's no gradient. Circumferentially, however, you get the hydrostatic condition behind the stress wave propagation, in which case approximately three radii from the center, you'll get within 5 to 6% of the magnitude of the incident stress and you'll get twice that at the edge of the opening. So, an implied gradient of one sigma and one R at that condition. A P-wave even in the nuclear events is not the only thing that occurs. You get very significant off-axis motion indicating that it's certainly not just the pure P-wave which strikes these various tunnels.

However, if I jump to another pure situation of an S-wave intersecting the tunnel at an angle, it's going to create in the worst case a magnification of four at the entry to the surface, and again out at like 2R from the edge, it will be within 5 or 6% less stress. You have fairly strong gradients either from a P-wave or the S-wave and, of course, in the real case, you get a very complex system of SH, SV, and possibly P-wave coming into the picture.

But, further, to try and put into context my assumption of the condition of .5%, 1.5%, and so forth, I look at conventional reinforced concrete, and if you've got a column of conventional reinforced concrete--and, I emphasize conventional--this is not mesh imbedded in the concrete, at all. This is just rebar and rebar alone creating the reinforcement. ACI Code 318-89 explicitly for--gives a failure strain of .003 and it implies for no gradient or column, a strain of .0015. So, I argue with myself, at least, and staff that we certainly don't have the pure situation that's represented here. We're always going to get a super position of a great complex series of waves even for the fairly clean case of a completely contained explosion. And, consequently, I took those strains as a variant.

Now, the other side of that would be to say that the condition created by an earthquake is a very complex one and its complexity is going to create stress and strain gradients depending upon whether you're looking at the elastic problem or the plastic problem which are a heck of a lot more complicated than what I've indicated there. And, if that were the case, these should probably be adjusted depending upon what the condition of your particular wave--but, if you assume that it's invariant, you end up with the numbers that I've summarized over here.

As a number of people have already said, we have to be very cautious of extending these data. I believe in my digression I've already talked about most of the caveats over there. But, I come to this summary for the raw data. That for safety bolts, the 4x4 pattern indicates the capability to withstand shaking of up to 13 FPS. We've harked back to the historical data.

As already emphasized, there was no damage for the cases of indicated peak surface velocity of 2/3 FPS over the surface above the opening and only minor damage for estimated peak particle velocity over the surface of 3.1 FPS. But, I would also hark back to the fact that the most recent look at these data indicates very little damage below 50 meters and essentially none below 300 meters in depth.

But, now if we just take the straight ratio of this 13 to the .67, you get a raw knock-down factor, as I've called it here, which shows that ratio of 19 or the lesser raw factor of 4.2 as the ratio of the 13 to the 3.1. Which leads me to the conclusion, as indicated here, it strongly suggests shaking produces no problems for the openings of Yucca Mountain if we use safety bolts on a 4x4 pattern. But, of course, we have to be aware of some of the things that a number of people have alluded to, shear zones, faults, and that sort of thing, where even static conditions are going to perhaps create problems for us.

Now, I'm changing horses rather dramatically and postulating possible or potential modes of failure due to earthquakes and we postulated five such principal modes; spallation and flyrock, general structural failure from shaking, two subdivisions of local structural failure. One from shaking due to weaker generalized zones, and secondly, by discrete motion along bedding planes, as already mentioned by Dr. Kennedy, a so-called block motion and the weapons effects area. We also postulated inundation from perched water as a potential mode of failure and triggering of incipient rock bursts as the final one. The rest of the briefing I've attempted to address each of those items.

However, harking back to the statement that we strongly--that the information strongly suggests that shaking by itself is not a principal cause of damage, I shall only be in the subsequent briefing looking at the remaining four items; spallation, block motion, inundation, and possibly triggered motion rock bursts.

In most cases, I'm just trying to scope the problem and then summarize the indications from that scoping. And, the first such attempt is shown here for spallation and flyrock. Let's take the right hand side first. The underground nuclear explosions normally characterized, as already mentioned by a number of people in reference to Jim Phillips' briefing, usually much longer duration, but there frequently is some high frequency hash on top of that long duration, a fairly simple wave form. Only one case in approximately 500 such examples where there's been fairly likely mode of failure was one of spallation.

If, on the other side, we look at the earthquake and assume we have an incident wave as shown here, solid, and a reflected wave over the free surface, the geometry shows that we're dependent upon peak velocity in the medium, effective tensile strength, unit weight, and frequency, and I've assumed a 60 cycle dominant frequency. Of course, it depends upon what earthquake you're looking at and what you do to it, whether you count zero crossings or you do a spectral analysis to determine--primarily used with a scoping calculation, 60 cycles per second, which is consistent with apparent spectral density before the earthquake down in El Centro. You go through this, you compute a spall depth, the reflected wave exceeds the stress in the incident wave by the effective tensile strength. It creates the spall of 1.6 feet.

If on the other hand, I assume that the cross-sectional area affectively associated with that spall is 6x6 inches, of course, it's going to be probably very ragged. That gives me a kinetic energy trapped in the spall as a result of the failure and the momentum trapped in it of 72 foot pounds. If, in turn, I take two crossing wires not meshed, just two crossing wires of a four foot pattern and vary the yield strength and diameter of the wire, it takes four to six inches of displacement to absorb at 72 foot pounds of energy. Of course, this would not necessarily happen, at all, first of all. Second of all, it would be very unlikely if it were to occur right at the middle of the four foot span of welded wire fabric or chain-link fence. We consequently summarized that production from flyrock that occurs is readily provided by wire mesh and rock bolts.

Now, to get to the possibility of block motion and discrete motion along planes of weakness, I'll show a few photographs, first of all. Almost all of the lined structures used in the tests at the Nevada Test Site in recent times have been pre-fabricated and moved to the tunnel and then moved underground. So, this is a fairly typical structure shown at the portal. These are just picked up--near the portal being prepared, ready to go underground. I shall return to that to give you more details on what it really represents.

But before I do that, let's look at a case where we had a fault that intersected a drift at essentially right angles to the axis of that drift. You're looking down the tunnel here. The actual structure goes from off the view over here to a point right down here as the end of the structure, right in here. This was the adjacent structure and the bolt circle for the reinforcing the adjacent structure.

These are passive gauges--these are mounts for passive gauges within the structure and they are two feet apart. And, the fault passed coincidentally--this wasn't by design--coincidentally fell between the passive gauges and the displacement you see here is continued on over here so that the total. So that the total displacement from right there to right over here was 19 inches, about 50 centimeters.

I'm not saying that 50 centimeters is the criterion that we're going to have to design to. In fact, I'd be surprised if we end up with anything of that magnitude. All I'm saying is here is a strong structure that was guillotined, as Dr. Kennedy has already pointed out. These markings here are for passive gauges. These are eight inches apart. So, the maximum extent of this displacement on this particular fault crossing at essentially 90 degrees is eight, 16, 24, and about half the next one, about 28 inches. So, the devastation created by that fault on this particular structure was encapsulated within just over two feet of the length of the structure. Again, it crossed at right angles to the particular structure.

DR. DEERE: On the left side, there's no displacement. Am I looking at it right?

DR. MERRITT: That is correct. That is correct. This is still relatively pristine and you've led me into the next chart.

This is a companion structure further down the drift. This one is the 11th structure on further down the drift. All 10, between the one you saw guillotined and this one, had no perceptible damage on the inside. This one--and again I intentionally pointed out the gauge mounts here near the center of the structure--the fault that you saw started right there and extended for 28 inches along that structure. This structure is a much weaker structure. This one has only three and a half inches of concrete surrounding it. The one you saw that was guillotined had 12-1/2 inches of concrete, so almost four times as much concrete. This one, incidentally, saw two loadings. One of almost 50 FPS moving--a stress wave of moving across right to left and then it was reloaded with a loading of about 30 FPS with the stress weight moving axially along the length of the tunnel. But, here is a structure much weaker than the one with the--that by happenstance crossed the fault; yet, it withstood the loading with no difficulty whatever. You might say what happened here--you see the weld right there and there's displacement along the weld--part of that is due to the lack of precise alignment of the weld by the manufacturer. The principal criterion for emplacing these was to make sure the passive gauges fell exactly at the cardinal points, zero degrees, 45 degrees, 90 degrees, and so forth. So, if the fabricator, as it turned out, was not precise in aligning his field, he missed his angular position by that.

But, more importantly, the slight relative displacement you see right here is a result of the fact that this structure in the foreground, from here on back, was a stronger structure and did not have as much displacement as this one right here. That deformed to a greater extent because of the lesser amount of structure there.

MR. TILLSON: Is the geologic conditions in the two tunnels exactly the same or the rock types?

DR. MERRITT: To the degree that that's possible, yes. They were all in--

MR. TILLSON: The general quality of which--

DR. MERRITT: Yes, yes, yes. All of these were in, I believe, the four bed, but I'm not sure. But, to the degree that the geologic setting could be made the same over a length of some hundred feet of drift, they were the same.

To further put that into context and to lead to the bottom line of this segment of the presentation, here is a structure that saw in two loadings--in the first case, almost 90 FPS with the loading moving across this way, and then subsequently, almost 120 FPS with the loading moving from that dome structure back this way. So, this structure saw in round numbers twice the loading of the one that you saw of the guillotine and then the 111 structure further down. No obvious distress when there was no fault proximity. Again, here are the passive gauge mounts. So, the structure actually goes from there to there. Again, you've got relative displacement here, but that's due to the fact that this structure right here was stronger than this structure running from here, this way, and consequently, it had the lesser response than this one adjacent to it. These are the gauge mounts. So, the actual structure goes from here to here.

I would also draw to your attention this upstanding 2x6 inches, it turns out, steel bolt circle which was used to put the end closure on this particular structure. The end closure was a dome with a steel manway through the end of the dome.

So, now, let's go back to the pre-fabricated structure that I showed you before and I'll show you the difference between the structure that saw the 90 to 100 FPS versus the one that saw half that. This structure--this is that bolt circle I just showed you, the two inch wide by six inch high steel plate merely welded to a 3/4 inch steel plate that formed the interior lining of this particular structure. That structure was surrounded by 12 inches of conventional reinforced concrete, but with so-called Nelson studs--studs welded to the outside of the steel plate--to create a shear transfer between the steel plate and the concrete.

And then, the last of the series of structures I showed you, the one that saw, let's say, 100 FPS had surrounding it one foot of cellular concrete. Cellular concrete in this particular case had a nominal yield strength of 1,000psi. It was 12 inches thick. And, that 1,000psi represents the yield plateau, if you will, and that yield plateau extends for this particular material to a strength of almost 30%. So, had this structure been back in the case with the one that was guillotined, it could have withstood something in the neighborhood of four to six inches of displacement without putting any distress on the interior structure.

But, what's the difference between the one that saw the guillotine versus the one that saw twice the loading and saw no guillotining? It's this frangible, crushable, breakable packing material surrounding it. I'm not saying that that's the solution that we're going to have to adopt because, as already indicated, that was 50 centimeters of displacement that we had to accommodate. It's more than likely that the displacement is going to be much smaller. But, if we have to over a very short length of tunnel for, let's say, retrievable purposes or whatever purpose--I need get back in the tunnel following an event--this short length of structure provides a potential solution to having to cross those kinds of conditions.

Now, this in a few words, I think, summarizes what I just tried to say. That we're not saying that block motions are going to occur of large magnitude, but should a very isolated condition develop, there is a engineering solution to that particular problem.

Inundation from perched water was the next to last postulated condition from earthquake damage. Again, I relied on a study done for deep basing; this one for ballistic missiles off of the Air Force done by Joel Sweet and myself back in '86--well, actually, late '85 and the report was published in '86. Extensive series of calculations were done using the SATURN Program; SATURN for saturated non-linear. It's a true effective stress model. Treats water as water, the porous rock as porous rock, allows the water to flow in the in the interstices if you have cracks in the pores, if you've got only pores. It also invokes a critical state or a cap model to both control the dilatency, as well as induce potential for compaction within the material.

We always said if a crack existed, at all, a single crack, and these are the typical conditions--again this is for deep basing where we were looking at high stress waves, high level stress waves, .4--6,000psi, typical input, 40 millisecond rise time, 1.5 second duration. However, we also did three problems with 3,000psi, 1500psi, and 750psi. These are the variables that we considered, at least in summary form--there were some variations in this. Since we didn't have a three phase model to include air, we adjust the bulk modulus of the water in order to approximate the conditions for air flow voids.

If you have 6,000psi stress imposed on a 16 foot tunnel, you get five gallons of water per foot driven into that tunnel if you start with a fully saturated condition in the rock, or 5,000 gallons for 1,000 feet of tunnel. It's not quite linear in terms of size of opening. Applying the same stress levels and so forth for half that size opening, you only get two gallons, .4 as much water. And, you have to go to a totally open crack, no impediment whatever to the flow, in order to get 59 gallons per foot entering a 16 foot tunnel, again driven by 6,000psi. And, for the 1/8 of 6,000, only .8 of a gallon. So, if perched water is a problem, pumping will suffice during construction and grouting will be adequate for the long-term.

I might mention at that point that Mr. Joe LaComb who is in charge of the DNA operations at the Nevada Test Site has developed a number of means of injecting grout into these ash-flow tuffs and he's a real resource in terms of coming up with practical solutions for handling the multitude of problems that are encountered at the test site.

Finally, triggered rock bursts, the bulk of the information from triggered rock bursts comes from the South Africans and they, of course, back in the 60's and before were having a great deal of damage due to rock bursts. And, they found out that the principal problem was excessive compressive stress and they were able to largely eliminate the problems of rock bursts at their 10,000 foot depths--as an indication of the kind of depths they were looking at--and were able to get around those by planning their stope development to avoid regions of suspected or measured high compressive stresses.

Now, this is a very simplified look at Yucca Mountain. I've assumed a Kirsch sort of solution, took the maximum lithostatic stress like 1600psi. The Kirsch kind of solution gives you about 4500psi locally around the opening which says that you've got to reserve even against the pessimistic 6,000psi about 1500psi around the opening before you start to get concerned. But, I would quickly add that this is a very--quickly emphasize, as already said, that this is a very simplified thing. You'd have to worry about the tectonic stresses on linear behavior and so forth. However, you know, the current RIB, as it's called, the data base, is showing unconfined stress capabilities more like double the 10,000psi, more like 20,000psi. So, in summary, triggered rock bursts look unlikely at this site for the depths that we're currently considering.

The final summary of all the material that is included is given up here and that is shaking and spallation are taken care of by using safety bolts on approximately a 4x4 pattern. Block motions, if the developing criteria indicate they may be a problem with very localized conditions, we have engineering solutions for that. Inundation by perched water, there are engineering solutions for that. And then, triggering of incipient rock bursts seems unlikely at this site and at this depth.

DR. ALLEN: All right. Thank you, Jay.

Do we have questions from the Board or--

DR. DEERE: I have a comment. I might have another question later, Jay.

With respect to the rock bursts susceptibility, I don't know what the South African experience was, how they could predict zones where they thought it might take place. But, in a recent tunnel that was driven for a distance of six kilometers in Chile, a rather large tunnel about eight feet in diameter, through some Tertiary granite which was moderately jointed, but in general you would have to say it was of good quality, seismic velocities on the order of 5,000 to 6,000 meters per second, and they were at a depth of 400 to 600 meters--so that means we had a lot of stress available--and, as soon as the jointing became more widely spaced and tighter, we found no water was coming in, although on either side you would run into water, and the tunnelers soon learned that that was country-rock because the rock quality was so good, the jointing being almost absent and what was there was just incipient, so that indeed there were higher stresses in those zones. And, even though the rock was so great--I mean, it's strength was so great, it came to the point that the stress concentration exceeded that. And, the failure that took place usually took place from the area that had been blasted the day before. They were not from the face where you had a three-dimensional stress field and, therefore, not as high a stress, but about one tunnel diameter to two tunnel diameters behind them where they were going into a true two-dimensional effect with higher stress concentration. And, they had a whole series of rock bursts to the point that it became very dangerous to work there.

But, my key point there is wherever they had normal jointing, they knew they were home free because their only problem was putting rock bolts in to keep rocks from dropping out. Where the rock quality got so good that it was dry and practically no jointing, they were very scared.

DR. MERRITT: I was just trying to think, Dr. Deere. First of all, I'm not familiar with the case that you were referring to, the eight foot tunnel in Chile.

DR. DEERE: 28.

DR. MERRITT: I'm trying to recall the specific paper and authors that I used. It was a '68 paper out of South Africa and the specific conditions they found to stay away from was the footwall of a normal fault. They were getting suspicious, and in some cases, measured very high in-situ stresses.

DR. DEERE: Right. I'd like to give a second case then. The first case, the one in Chile, was where the rock quality improved and, therefore, we had higher stress. And, when the factor of safety became one, it failed.

The other case is in Panama in a thick andesite flow. This again was considerable depth, about 400 or 450 meters. There, the andesite was very massive, very strong, not too many joints. And, yet, the rock was strong enough that it could take this high stress that's peaked up and they had no particular problems. However, from time to time, they would run into a zone where the andesite was vuggey. It had a lot of porosity, some secondary calcite in-filling, good enough to transmit the stress. So, we still had a lot of high stress. But, in this case, its strength went down. So, in those areas, we got continual problems with rock bursts.

And, the interesting thing, the solution there was a little bit easier than the one in Chile. The solution was instead of using for a temporary support rock bolts, mesh, and one layer of shotcrete, it was to use rock bolts, mesh, shotcrete, another layer of mesh and shotcrete. And, just a difference in the support capacity was sufficient to stop the failures.

DR. ALLEN: Bob, do you have a comment?

DR. KENNEDY: I had a comment, but first on this rock burst issue, Jay, are you aware of whether there has been any rock burst problems in any of these tunnels there at the Nevada Test Site?

DR. MERRITT: I have seen none.

DR. KENNEDY: I have seen none either.

DR. CORDING: Well, we've had--the rock fracturing occurs down there around the openings and there's failures that develop with excavation. And, we've had one case where there was a small, you might call it, a bump or a small burst or pop or whatever, where the miners walked in and they said it felt like it was, you know--came off the floor. It must have been a little bit of acceleration. But, it was when we were opening up a very large opening and we had some stress concentrations and the opening was about 50 feet wide. It was fairly quiet. We were getting high stresses near the sidewall, but the rock is weak enough in the end tunnel at that depth that it will relieve, but there's not much energy. And, so the dynamic effect is almost non-existent. And, I think that's, you know--with the rock bursting at the very large depths, there's a lot of energy released, enough to even close drifts. And, so you just don't see that.

DR. KENNEDY: Yes, I had some things I wanted to mention with regard to the minimally hardened tunnels, not your super hard, but your minimally hardened tunnels where, as you indicated, you gave a set of criteria which by your calculations would indicate about 13 FPS, peak particle velocity capability. I think, you know, that we may quibble a bit. I may think that's a little bit more optimistic than a median. You may think it's a little less optimistic than a median. But, it's probably out there close to a median estimate.

I do think we do have to worry about large scatter about those estimates. For instance, that particular shot you mentioned where there were five agencies to predict the damage of one of these minimally hardened tunnels, yours was one, mine was another, we all predicted light to moderate damage. The actual damage was moderate to heavy. In other words, all five missed on the prediction. Since then, I know we have lowered those strain limits that--the strain limits that you showed there were back from 1982. We've lowered those some since. I do think we've got to worry about this scatter at depth.

I had one other thing. The Dowding & Rosen paper which indicates minor damage below 3.1 feet per second, that's very consistent with the kind of Nevada Test Site experience. In other words, we would never predict damage below that kind of number, no matter which of these approaches. So, that's all consistent, but it is worrisome to me that the more recent Sharma & Judd paper indicates that you have to get down to about .12g for an 80% confidence. That is outside of my range of experience if you'd have to get down that low. Do you have any explanation for why they're--

DR. MERRITT: The principal reason, I think, that it gets down that low is they, like others, have put to put the E.G. Wright Tunnel, for example, into their overall regression analysis. E.G. Wright Tunnel was damaged in the 1906 San Francisco Earthquake by four and a half feet of block motion along its length. E.G. Wright #2 which was parallel and removed by about 60 feet did not have the fault crossing it and saw no damage, whatever. So, they in putting these data--information, I prefer to call it, rather than data--had to include all things by virtue of the fact that if you start eliminating things--for example, if you start looking at only brick lined tunnels, my recollection is there are nine of those 192 cases that are brick lined tunnels--there are 14 un-reinforced concrete tunnels. So, if you start eliminating things from the 192, you end up with potential statistically insignificant--by the other side of that coin, when you start mashing the whole 192 together, you have a number of situations which probably shouldn't be attempted to correlate. And, their results of the rather pessimistic value of acceleration level include all those things.

DR. DEERE: I had this morning two pages of comments regarding the statistical use of tunnel collapses and faults. I'm going to give that tomorrow morning and I hope that a couple of the points will apply to the inadequacy of many of the case history studies.

DR. ALLEN: You know, I think we're just going to have to move ahead here. I'll remind you once again we don't have the option this afternoon of going on forever. We've got to be out of the room by five-thirty.

Thank you, Jay.

Ardyth, do you want to introduce the next speaker?

DR. SIMMONS: Thank you. The next speaker will be Richard Quittmeyer with Woodward Clyde. Currently, Richard is leader of the Seimology and Geophysics Group for the Yucca Mountain Project. He has extensive experience with seismologic monitoring networked in the area of New York and New Jersey, and has conducted seismic hazard evaluations around the world.

MR. QUITTMEYER: Okay. The last couple talks that we've just heard have discussed some of the seismic vulnerability aspects of the surface facilities and the underground openings, and I'm going to kind of shift gears a little and bring the discussion back to more of a geologic focus, with particular attention to the site characterization program and how that can gather the information we need to address the seismic vulnerability concerns.

The scope of the presentation will be first to identify what the seismic vulnerability concerns that we derive, that we shift out of the previous talks are, and then to go over how the site characterization activities will address those concerns. We'll be concerned with both the surface facilities and the underground openings, and we'll be concerned with two different time periods; the preclosure and postclosure time periods.

Based on the previous discussions today, we feel that there are two main concerns that we can derive from the seismic vulnerability discussions, and the two concerns are fault displacement and ground motion. The reason why these are concerns come from various types of damage that can occur. For fault displacement in the surface facilities, we're concerned with release from, say, the waste handling building, release of radioactivity caused by damage associated with actual fault displacement. For ground motion, we're really interested in the same thing, but in this case, the damage from ground motion, not from fault displacement.

In the underground, during the preclosure period, our primary concern with seismic vulnerabilities are maintaining the ability to retrieve the waste if that's required, if the decision is made to do that. In terms of fault displacement, we're concerned with things like displacement in the emplacement borehole that perhaps could jam the waste container and make it hard to get out. For ground motion, it may be things like spalling of rock into the air gap around the emplacement borehole, again making it more difficult to remove. We also could be concerned here with rock fall in the tunnels that provide access, that we would need to maintain the access to get the--to retrieve the waste.

For postclosure period, we're concerned with--again, in terms of fault displacement, here we're concerned with actual fault displacement and rupturing the waste containers and allowing releases beyond those which we can accommodate.

In the rest of the talk, I'm going to address first fault displacement as it applies to surface facilities and then to the underground, and then ground motion for both the surface facilities and underground, and I'm going to try and give a very brief assessment of sort of the current state of affairs with respect to the seismic vulnerability concerns that were expressed in the previous talks; then to present a strategy derived from the site characterization plan on how to address these seismic vulnerability concerns; and then indicate the types of information that will be required and then show how the site characterization activities will gather that information and try and show how the current focus of the site characterization plan is gathering the information we need now.

So the preliminary assessment of sort of the state of affairs concerning seismic vulnerability is that reasonably available technology will allow us to design to accommodate small displacements, and here we're not even really thinking of fault displacements, but there are displacements associated with differential displacement and settlement, things like that, which it's just standard practice that buildings can accommodate these things.

If we go back to Phil Richter's talk earlier where he mentioned some of the preliminary assessments of the amount of resistance that is inherent in the current waste handling building design, we find that this current design can accommodate several inches of vertical displacement and up five-ten inches, 15 inches of horizontal displacement.

We realize that we'll probably never know perfectly without any doubts that there would never be any new faulting or faulting along unrecognized faults, but we believe that given our site characterization program, the amounts of displacement that we can expect from these types of faulting will be small and that they'll be accommodated by the design of the facilities.

So our strategy, then, is to detect and avoid the fault locations, and any residual uncertainties that we have concerning the possibility of fault displacement will be accommodated in the design.

So rephrasing this, we can put this as a question. What we need to know is: Where are the faults? What types of displacements have occurred on them? And here we're concerned with the preclosure period, so we're identifying faults that will be of interest during that time period. To do this we'll need information on detection of faults, on their displacement, and on the tectonic framework in which they exist.

By detection, we're concerned with the locations of the faults, with their lengths, their orientation, their width, and part of detection is also understanding the bounds on detection, or our ability to detect faults down to certain--well, just that some faults we probably will not be able to detect and we need to understand the characteristics of those.

In terms of displacements, we need to know rates of displacements. We need to know displacements during individual events. We're interested in whether displacements are associated with secondary faults or with primary faulting. We're also interested in segmentation of the faults and as that relates to the amount of displacement that may be expected in any one particular rupture of any one particular segment.

In terms of tectonic framework here, we're interested in the interrelationship of the faults. If one fault goes, are other faults going to also slip at the same time?

So how will the site characterization plan studies and activities gather this type of information? The types of--I tried to group the site characterization activities into broad groups rather than going through in excruciating detail every single activity and how it would relate to this problem.

The four main types of studies that will deal with the fault displacement issue at the surface facilities are Quaternary geologic mapping, paleoseismic studies, some geophysical studies, and tectonic model studies. Most important, probably, here will be some of the work that the preliminary--the work in Midway Valley, which the preliminary results, the results from the early trenches was presented earlier by Bert Swan.

I can see that we're going to excavate trenches across the proposed site of the surface facilities, and this will be to either--hopefully to document the absence of faulting there, but when we do it we'll see what we find.

In terms of identifying the locations of some of these faults with quaternary motion, a lot of that work is ongoing and has been ongoing for a number of years now.

The next topic will be fault displacement as it affects the underground facilities. Here our preliminary assessment is that the most vulnerable areas are those where faults are going to intersect the underground openings, and we do have technology available that allows us to design the tunnels to accommodate some of this fault displacement if we deem that that's necessary.

Our strategy, again, will be to attempt to avoid the faults in siting the emplacement of the waste containers. We'll provide an appropriate design for the ramps and tunnels to maintain the retrieval options, and when we say appropriate, it may be that we don't need to do extensive reinforcement of the tunnels given the low probability of fault displacement. We may decide that the best and most cost effective way to go about it is just to do standard reinforcement, and if you do get some fault displacement, you can go in and mine out the debris and if you need to retrieve the waste, you can do that.

Also, finally, part of the strategy is to design the emplacement to accommodate any of the residual uncertainties we have concerning detection or amount of movement on faults.

So what types of information do we need to implement this strategy? Pretty much the same as for the surface facilities, except now we're concerned about both the preclosure and postclosure period. Again, we'll be requiring the same types of data; detection of faults, displacement on faults, how they fit in the tectonic framework, how the faults are related to one another.

For the underground, a lot of the same site characterization activities apply. Additionally, though, we'll have the studies carried out in the Exploratory Study Facility which will provide sort of the ground truth on the faults that are occurring in the--beneath Yucca Mountain. So we still will rely some on the surface studies because they will also be telling us about where the faults occur, but we feel that our primary ability to identify faults and fractures in the underground is going to come from the studies carried out in the Exploratory Study Facility.

Okay, now we'll shift over to ground motion, and here, as Terry discussed this morning, we think we're dealing with earthquakes somewhere in the magnitude range of 6¼ to 7¼. We also believe that probably the types of ground motion that's going to be driving our assessments of seismic vulnerability are going to be those from earthquakes rather than underground nuclear explosions. There may be some periods of interest where the UNE's may contribute, but it's going to be primarily an earthquake ground motion problem. And we also have reasonably available technology that allows us to design for the levels of ground motion that can be expected at the site.

This figure over here just shows--this is a figure taken from the cost benefit analysis showing the difference in costs of designing for different levels of ground motion, and you can see that between about .24 and .53 it's fairly insensitive. Even as you get up to higher levels, the cost doesn't become too bad. The point here is that we've been designing engineered structures for ground motion for quite awhile. There are a number of nuclear facilities that are designed for ground motions, accelerations on the order of, say, .7 g. This is not something where we're going to have to go out and reinvent the wheel. It is part of the standard practice of today.

So what is our strategy? Our strategy is to carry out the studies needed to determine the appropriate design basis for the surface facilities. To do this, we're going to have to answer the standard questions for, you know, seismic hazard analysis: What are the sources of the earthquakes? This will include evaluations of which model or models are credible. What is the rate of recurrence of these earthquakes? How big are they going to be, and what is the level of ground motion that they're going to generate and how is that going to attenuate with distance?

To gather that information, we have a number of activities and studies in the site characterization plan. Some of the studies we've already seen because they also apply to the fault displacement issue. We also, though, now have seismic monitoring studies. These will help us in terms of evaluating our ground motion model.

In terms of evaluating the seismic sources, we'll be using the geologic mapping studies, the paleoseismic studies will be giving us information on recurrence rates. Our tectonic model studies will be helping us to evaluate the different models that we'll incorporate into our hazard assessments, and in terms of hazard assessments, we'll be looking at both deterministic and probabilistic approaches.

In terms of the underground and vibratory ground motion, just showing a figure from the Sharma & Judd paper in which they indicate the type of damage that they noted in different cases versus the depth of overburden. We see that heavy damage is primarily occurring for the shallower depths. As the overburden gets up to about 300 meters, the occurrence of heavy damage is no longer observed.

As we've seen from some of the previous discussions today, the levels of ground motion from UNE's are quite a bit higher, although we recognize that ground motions from UNE's is not the same as ground motion from earthquakes. But they seem to indicate that the tunnels can withstand quite large ground accelerations and velocities.

So again we come to what our strategy is here. We're going to design the underground openings to remain stable for the expected ground motions. We have the technology available to do this and we'll provide seismic design values for design of the waste container and emplacement.

Moving through this fairly quickly, it's again the same as for the surface facilities. Here one difference, though, will be that for the ground motion model, we'll also be very interested in the effect of depth on the ground motions that are observed. Data to address that issue will be coming from the seismic monitoring, which will be gathering site specific data to develop models or to calibrate models that exist.

And just to summarize now, one of our basic strategies is going to be to identify and avoid faults in siting both our surface facilities and the waste emplacement boreholes. The site characterization activities that we're currently carrying out, the geologic mapping that's going on, the studies that are underway in Midway Valley directly address this issue.

A second point, secondary faulting, undetected faulting, and new faulting, these are issues that we need to evaluate further, to think about some more, but it seems that right now the rates of--the amounts of displacement and the rates of occurrence for these types of faulting are going to be small.

And finally, the anticipated ground motion levels at the site, both for the surface facilities and in the underground, can be accommodated and designed for using reasonably available technology, and our site characterization plan includes a number of activities that are aimed directly at coming up with the value that is appropriate for design.

So those are the end of my remarks.

DR. ALLEN: Thank you, Richard.

Your presentation brings up a number of questions of prioritization, and so forth, and I think they're the kinds of things we might want to be talking about tomorrow afternoon in the session. We might all be thinking about it.

Any specific comments from Board members? If not, Ardyth, I'd like to suggest maybe we take a break and then have your summary at the end of the break. Is that okay, or would you rather do it continuously right now?

DR. SIMMONS: It's okay, but my summary's going to be so brief that we can get it over in like a minute or two.

DR. ALLEN: Okay. Let's do it right now. You were allocated 15 minutes.

DR. SIMMONS: I know.

Richard has said much of this more eloquently than I'm going to say right now, but I just wanted to leave the audience with the highlights that we would emphasize in the presentations, and that is that for the ranges of seismic loading that have been given and are anticipated at the site, we believe that they're within the design capabilities and experience of common practice.

Also, the underground facilities, as you've heard through presentations that have drawn analogs to Yucca Mountain through experience with some of the tunnels at the test sites show that the underground facilities would be naturally robust and could readily accommodate the expected seismic events that we would find during the postclosure period. However, considerations that are important are the geologic setting of the tuffs and the fault crossing. Those will definitely have to be accounted for and localization of minor damage would have to be dealt with as a possibility.

Finally, the surface facility is inherently robust in its design, and the design for anticipated ground motion is well within the available technology.

As Richard pointed out, we have a number of ongoing studies. For FY92 the things that are either beginning or ongoing will be additional Midway Valley trenching and mapping that Bert Swan described to you to understand fault displacement near the location where the surface facilities would be. We're also going to be starting a seismic reflection line across Yucca Mountain this year, and that will help provide information that will go into the discrimination of tectonic models.

We have Quaternary faulting studies and paleoseismic faulting studies that should be added to this that provide information on fault detection with the fault zone length, and so forth, the amount of displacement and tectonic setting, and then, finally, the seismic monitoring network which is ongoing to provide information on contemporary seismicity, earthquake recurrence and size.

So these are the things that we will be doing in a large sense in this area. We recognize that once we are able to study the underground area through the Exploratory Studies Facility, we'll have a great deal more information and be able to refine our knowledge of the hazards, and that's all.

DR. ALLEN: Thank you, Ardyth.

Questions, comments from the Board?

DR. DEERE: I guess just one question. With your trenching across the Midway Valley area where we have some deeper alluviums, are we sure that when we get movement in the bedrock on the same fault that the surface displacement will be on a previous one, or is this a little haphazard? Maybe we have some parallel ones simply because the surface break is in a little bit different position, and--I haven't heard that referred to, I don't think, today.

DR. SIMMONS: Dave Schwartz, that's something that you're going to address. I don't think we can necessarily make that assumption, but you'll be hearing more about that.

DR. ALLEN: There are several things you've said I want to get at tomorrow afternoon.

MR. WHITNEY: John Whitney, USGS.

We do believe there are faults at depth in the bedrock at Midway Valley, both from our balanced cross-sections of Yucca Mountain, and there's been some shallow gravity traverses which actually do show up some faults so it's a good test to do this trenching across the Valley because if these deposits are hundreds of thousands of years old and they're not--don't demonstrate any fracturing or faulting, we can be reasonably certain what their activity is in the late Quaternary.

DR. ALLEN: Other comments?

(No audible response.)

DR. ALLEN: Okay. I'd like to suggest we take a fifteen-minute break and be back here at 3:25, and continue with the program. We'll be back a little bit more on schedule by then.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Okay. Our next presentation is by Dinesh Gupta of the Nuclear Regulatory Commission.

DR. GUPTA: Thank you. Good afternoon. First of all, I'd like to thank the Board for giving us this opportunity to express our views on the vulnerability of the geologic repository to vibratory ground motion from a regulatory perspective.

Earlier this morning, the Board has heard from various speakers, a discussion on this subject from a designer's point of view and what I intend to do is discuss the same subject, basically, from a regulatory perspective. My presentation will cover the principal regulatory requirements that are related to vibratory ground motion and fault displacement, and then I would like to discuss a frequently raised question, whether the NRC staff are going to impose the requirements of Appendix "A" to Part 100 for the seismic and faulting design of the geologic repository.

Then finally I'll discuss from a regulatory point of view what components of the geologic repository are vulnerable to the seismic hazards and displacement and should be considered in meeting the regulatory requirements.

Starting with the contents of the regulatory requirements, the requirements that deal with the geologic repository design for vibratory ground motion and for displacement are contained in 10 CFR Part 60, and as you all probably know, they are stated in very general terms. There is no specific requirements stated which give detailed guidance to DOE as to how to proceed with consideration of vibratory ground motion and fault displacement in the design.

What this staff intends to do is provide the specific guidance to DOE through pre-licensing interactions on as-needed basis. We start currently developing a number of staff technical positions, and my colleague, Keith McConnell, is going to present one of the technical positions on this subject tomorrow morning.

Also, we've started also developing license application format and content guide, and a license application review plan that will provide some guidance on this subject and other subjects.

The key requirement in Part 60 deals with the performance objectives, and the performance objectives deal with the time period of preclosure performance and the postclosure performance. The preclosure performance objectives require DOE to design the geologic repository operations area so that the radiation exposures, radiation levels and releases of the radioactive materials will meet 10 CFR Part 20 requirements, and then the rule also requires that the design should preserve the option of waste retrieval throughout the period of waste emplacement. Retrieval can start any time up to 50 years after the initiation of waste emplacement operations, and to meet this requirement, the preclosure time period may have to be about 100 years.

For postclosure performance objectives, Part 60 has basically requirements in two categories: One category requires the performance of the overall system. The requirement is that the geologic setting should be selected and the engineered barrier system should be designed along with the seals for shafts and boreholes so that the release of the radioactive material to the accessible environment conform to the EPA standard, and with regard to that, the other part of the requirements state that individual components of the geologic repository also meet certain requirements.

To this effect, the requirement that the containment of the high-level waste within the waste package should be substantially complete for the first 300 to 1,000 years after closure, and after the containment period is over, the release rate has to be gradual for the next time period following the containment period.

Finally, there is a requirement for the geologic setting which is stated in terms of pre-waste-emplacement groundwater travel time.

In addition to the performance objectives, Part 60 also specifies certain design criteria and siting criteria. The design criteria specify minimum criteria for the design of geologic repository operations area, for the design of seals for shafts and boreholes, and for the design of waste packages.

It requires that the engineered barrier system should be designed to assist the geologic setting in meeting the performance objective for the postclosure period, and for the preclosure period the requirements include specification that says that the structures, systems and components important to safety shall be designed to protect against natural phenomena.

As far as the siting criteria are concerned, identify certain favorable and potentially adverse conditions for the site which need to be considered in evaluating the post-closure performance of the site.

Let me turn my attention to the applicability or inapplicability of Appendix A to 10 CFR Part 100. A question is often raised to the staff: "Are we going to require DOE to use the requirements of Appendix A? And you know that over the years nuclear power plants have been designed using very conservative methods, and the issue is often raised: "Is this same level of conservatism necessary for the design of geologic repositories?" And Part 60 is silent on this issue.

The NRC staff position is as stated in the technical position that Keith is going to mention tomorrow. We say that Appendix "A" does not provide for the differences in the function and periods of performance between geologic repositories and nuclear power plants.

There are some specific differences that I would like to point out here. Basically, the nuclear power plants are designed for a 40-year life span, and even with some life extension of these spans, the hazard only applies to a period of a few decades. The geologic repository, on the other hand, must meet performance objectives for many thousands of years.

Also, the geologic repository would be basically a passive system. It's not going to be a pressurized system, and therefore, there's no risk similar to a nuclear reactor core melt down.

Thirdly, the portion of the geologic repository for which performance objectives must be met for the postclosure period will be deep underground, and as many speakers stated this morning and as is common knowledge, that underground structures have been observed to be more resistant to ground motion than surface facilities.

Also, there might be a need to consider the effect of repeated ground motions because of the long time period for which the performance objectives have to be met. Now, repeated ground motions and fault displacements could have cumulative adverse effects, and those may have to be considered for geologic repositories.

Also, there would be a need to combine the effects of heat generated by the waste emplacement and the seismic loads. There's no such case for the nuclear power plants. Thermal effects may increase the loading because seismic loads may have to be superimposed on thermal loads, and the heat effects may also reduce the strength of the underground rock mass and could thus reduce the system's resistance to earthquakes.

Also, the damage modes for a geologic repository may be quite different from those considered for a nuclear power plant. For example, vibratory ground motion and fault displacement can widen or close existing fractures and can create new fractures between ground surface and underground facility, and from underground facility to the water table.

In addition, the earthquakes can cause possible change in groundwater tables, hydraulic gradients, and degree of saturation of the repository horizon and those may need to be considered.

Due to these differences in the nature of the facilities, NRC staff considers that the regulatory requirements for seismic and faulting design of nuclear power plants, given in 10 CFR Part 100, Appendix A are not applicable to the design of geologic repository operations area.

Now, moving on to the vulnerability issue, the way I have subdivided the discussion of this topic is first I will discuss the vulnerability during preclosure period; and second, I'll discuss the vulnerability during postclosure period.

Now, as you know, during preclosure period there would be a surface facility and there would be--there would be surface facilities and there would be underground facilities, so you have to consider the vulnerability of both surface and underground facilities during preclosure period; while in the postclosure period, only the underground facility would remain, so that's the only thing that you need to worry about. The focus of NRC's staff review would be on the design of structures, systems, and components that are important to safety.

With respect to surface facilities, the waste handling building would be the main surface facility. In the current conceptual design, as was discussed this morning, this is a large structure. Based on the conceptual design of the repository, it would be about 500 feet by 500 feet. It is to be embedded some 25 feet into the ground and would be 40 feet above grade, and it would consist of various cask receiving areas and unloading areas, package and hot cells, and so on.

To some extent, the requirements for radiation shielding may result in a robust design for waste handling building to take care of the vibratory ground motion effect, even though the structure would need to be designed for whatever is the design ground motion at the surface, and I'll talk about the fault displacement consideration for design in a minute.

As far as the underground facility is concerned during preclosure period, the underground openings need to be designed to reduce potential for deleterious rock movement or fracturing, and the facility needs to permit retrieval of waste. To meet these requirements, the design and construction of the facility may need to consider inflicting only a limited disturbance to the underground and DOE's already working on that.

The design may also need to provide adequate roof support to withstand vibratory ground motion. In addition, as Dr. Kennedy pointed out earlier this morning, the drift orientations may need to consider the kind of known faults to the extent practical. In addition, the waste package, waste emplacement holes and waste packages may be vulnerable and need to be designed to avoid damage to these phenomenon.

There are certain vulnerabilities that apply to both the surface and underground facilities, and that might include a ventilation system, the utility service systems, the instrumentation and control systems, and they need to remain operative and their vulnerability to seismic and faulting effects may have to be considered in the design.

Now, with respect to the design of surface facilities and waste packages to accommodate fault displacement, NRC staff concurs with DOE's position as stated earlier today, that the faults should be identified and avoided in siting the surface facilities and waste emplacement boreholes, but I would hasten to add that Part 60 has no specific requirements to this effect, but it would be prudent for DOE to exercise caution regarding making an attempt to design the surface facilities and waste packages to accommodate fault displacement.

Now, coming to the vulnerabilities during postclosure period, as I just mentioned, only the sealed underground facility would remain or would exist during the postclosure period. During this period, the engineered barrier system, the repository seals, borehole seals, and the geologic setting would be of interest from a vulnerability point of view. If not properly accounted for, the waste package--the fault displacement can shear a waste package or could cause an emplacement hole to collapse and damage a waste package.

Also, if a waste package is emplaced too close to a fault, a fault displacement could open a pathway for groundwater to flow into faults and fractures, and thus, water could contact the waste packages. Also, the relative matters of whether the waste packages should be emplaced horizontally or vertically or in some other mode may be of interest from a vulnerability point of view.

If an air gap is a design feature between the waste package and the surrounding rock, the analysis needs to evaluate whether such an air gap can survive over the long period of time of interest of 10,000 years or so. The liner material and thickness need to be designed to withstand the tectonic effects.

Also, the current design concept for vertical emplacement considers the use of partial liner near the top of the waste package. In this regard, the relative matters of using a full liner versus a partial liner may need to be considered, the advantages versus disadvantages in terms of its resistance to vibratory ground motion and fault displacement.

Coming to the access drifts, ramps, and shafts, they may be backfilled at closure. Now, do we have any concern after the drifts are closed? In that regard, I would just mention that there might be a scenario that may need to be considered in that the--what could be the effect of long-term backfill settlement that could make the drift susceptible to damage due to vibratory ground motion and earthquake, and any collapse of the--just the roof portion may affect the potential changes in the pathways of water. So even though the drifts might be backfilled, the effect of backfill settlement may need to be considered.

Also, another component of the underground facility, another component of the geologic repository that may be vulnerable during postclosure period is the shafts, the seals for shafts and boreholes, and their vulnerability may need to be considered.

Those are the comments on the engineered barrier system and seals. There is also consideration for a natural system that needs to be paid attention to. The aspect that needs to be considered is changes in fracture characteristics and how to predict them for long periods of time. As I stated earlier, the vibratory ground motion and fault displacement may open up new fractures or widen or close existing fractures, and there may be a potential for groundwater changes in the hydraulic gradients or water levels.

Let me summarize by saying that the requirements related to vulnerability of the geologic repository due to vibratory ground motion and fault displacement include performance objectives, design criteria, and siting criteria. These are stated in general terms in the rule, and the specific guidance is to be provided through DOE on an as-needed basis to others.

The requirements are different from those for nuclear power plants because of the differences in the nature of these facilities, and therefore, the staff considers that Appendix "A" to Part 100 is not applicable to geologic repositories.

I may also mention that the NRC staff is considering currently modifying the requirements or adding another appendix to the rule that would update the requirement of Appendix "A" because of many other reasons.

As far as the design of surface facilities and the engineered barrier system where ground motions are concerned, the NRC staff believes that there is sufficient confidence-- that the state of art provides sufficient confidence in the ability to design the system for seismic motion. However, with respect to the design against fault displacement, prudence suggests caution to design the facilities to accommodate fault displacement.

Now, with respect to the behavior of the natural system, the staff believes that additional research may be needed to develop the ability to better predict the response of natural system to future vibratory ground motion and fault displacement and its effect on long-term repository performance.

I'll be glad to answer any questions.

DR. ALLEN: Thank you. Are there questions from the Board or staff? Don Deere.

DR. DEERE: In an earlier, one of your earlier presentations or slides, you mentioned containment of the high-level waste within the waste package, and then you commented for 300 years. I notice you didn't say 300 to 1,000, or greater than 1,000 if justified. Do you have any comment on that?

DR. GUPTA: No, that might be just a slip. I think the requirements are very clear that the containment has to be substantially complete for a period from 300 to 1,000 years.

DR. DEERE: Then Mr. Bob Bernero's later staff position, was it, or a staff paper that stated that was not a restriction if it could be proven that you could have something for a few thousand years or 1500 whatever--

DR. GUPTA: Absolutely.

DR. DEERE: --it could be considered.

DR. GUPTA: It would, yes. That may be an approach DOE can take, yes.

DR. REITER: Dinesh, this is a question that's for you or for anybody, Ron Ballard, anybody else within the audience. Earlier we heard Ardyth present something called issue resolution at which some issues would be raised and they would propose it and bring it to the NRC. I guess what I'm getting at is we've been hearing all kinds of things about seismic issues, and it appears that some are more important and some are less important. Is there some sort of vehicle by which NRC and DOE could reach some sort of agreement as to what constitute the more important issues and what constitute the less important issues? The idea is that to allowing site characterization and work in DOE to concentrate on those things that really count, putting more emphasis on that than those that don't count as much.

I guess the question to you is, is there some willingness or some mechanism within NRC to accommodate that?

DR. GUPTA: Oh, yes. Keith can correct me, or if he wants to add anything. We did receive from DOE a letter last year requesting some clarification of position from NRC on certain issues, and the staff is working on those items in consideration that those issues might be of significance to DOE for resolution.

The staff is also in the process of developing technical positions in which the staff positions would be clarified to provide guidance to DOE on some of those issues. We also have frequent meetings with DOE in which free interactions take place on various issues and of resolving many of those issues.

DR. REITER: But could there--I mean, is there a mechanism, I guess what I'm getting at, where NRC would say, "Yeah, DOE, we agree with you or we disagree with you. This item is really not as important as other items and we understand your reduced emphasis on this and we agree with that." Does such a possibility exist?

DR. GUPTA: Keith, do you want to comment on that?

MR. McCONNELL: Keith McConnell, NRC staff.

I think that mechanism exists in the resolution of the comments on the site characterization plan. The DOE wants to come in and say certain of those comments are not of high priority at this time. They can propose that and then we can make judgments based on what they submit to us at that time, but again, I think that you have to go back to the--what we call our site characterization analysis and what Bob Bernero put in his cover letter of that site characterization analysis which emphasized those aspects the staff felt were the most important at the time of the review, and I think they probably still remain the most important in the view of the staff.

Is that clear?

DR. REITER: Yeah, I guess the question is would some sort of risk-based analysis which said--let's take it with some hypothetical, a certain level of ground motion is of no concern and therefore, we think that the kind of efforts which--it would end up with trying to refine it more within that level of ground motion. Though it may be scientifically interesting, because it's of relatively little impact upon public health and safety, therefore, we would recognize a reduced emphasis on that.

DR. GUPTA: Yeah, I think Ardyth should really address this issue. Do you have any comments?

DR. SIMMONS: I can address it from the point of the Department of Energy. The fact that we agree that we are going to, as part of issue resolution, address the issues that were in this cover letter from Bernero based on the comments on the site characterization plan, that's certainly a very important element of it.

My understanding, however--and there might be people who have worked in the licensing area for a longer time than I have who could address it a little bit more precisely, but I don't know of any single mechanism that has been decided upon for issue resolution. I believe that there are really two areas or approaches, rather, that can be used.

One is, first of all, common recognition on the part of DOE and NRC that a certain issue is a topic for resolution, and then from there, one can focus on the mechanism of resolution, the approach towards it for licensing and obtain agreement from the NRC on that approach; or, number two, we could go a step beyond that and say that based on the data that we've collected to date, we feel that we have enough information to address that particular issue and not close it out, but essentially say that we've collected enough information and we're not going to go any farther.

And I believe that various approaches would be through the kind of topical report that we're talking about, and then requesting NRC's agreement; or another way of doing it is the NRC might make a rulemaking on something. So I don't think there is any single way, but it starts out with common agreement of what the issue would be to be resolved.

DR. ALLEN: Any questions or comments from the audience? Jay Smith?

MR. SMITH: I have two questions; the first one regarding the inapplicability of Appendix "A" at Part 100. It was interesting to note on your graph, of the six points of difference between a repository and a nuclear power plant, there were really only two that might be considered to indicate that the NRC staff would consider the repository to have lower vulnerability, or would be less susceptible than a nuclear power plant, which might lead one to not take much comfort in the assurance that you give that Appendix "A" or something like Appendix "A" would not be applied.

The implication might be that if an Appendix "A", perhaps, to Part 60 were developed, it would have an impact maybe even more severe than for nuclear power plants; right? Okay.

Well, it's not so much whether Appendix "A" of Part 100 is applicable, but whether the de facto effect of Appendix "A"-type criteria would preclude site suitability at a location like Yucca Mountain where there are Quaternary faults known to be present, and where consideration may need to be given to design to accommodate fault displacement.

So I'm wondering if--could you clarify what your intent is with regard to this de facto application? In other words, might a site still be considered suitable even though there is the mere presence of Quaternary faults; and secondly, in your concluding remarks where you talked about you can design for some extent for seismic motion, but then you say prudence suggests caution regarding design to accommodate fault displacement. I mean, one could read that last statement as suggesting, "Hey, fellas, don't try it."

Now, can you give some clarification on that aspect? Are you saying that, like for nuclear power plants which sites were killed if you had to consider the design for fault displacement, are you considering that fault displacement is not--design for fault displacement is not going to be acceptable for a repository?

DR. GUPTA: Okay. Let me take your first question first. With respect to site suitability, as far as the regulations are concerned, Part 60 does not have any specific site suitability or unsuitability criteria other than meeting of the performance objectives. If the site can be shown to meet the performance objectives, then there's no criteria bigger than that that need to be considered in terms of acceptability or unacceptability of the site. There are, of course, as you know there, Part 960 defines some qualifying and disqualifying conditions and with respect to vibratory ground motion and fault displacement, those criteria are specified in Part 960.

With respect to your other question on fault displacement, let me just flash this view graph. If you look at the third bullet here, the Part 60 does not have any requirement for a specific setback distance. It does not say that you can or cannot design a facility on a fault. Our position is that it would be very hard to prove that the facility is safe, and DOE should apply some caution in taking a position that they can design to accommodate a large amount of fault displacement for--whether it's a surface facility or whether you're talking about waste packages.

Design for fault displacement must provide reasonable assurance of meeting performance objectives, and that's our bottom line from a regulatory point of view. What we suggest is that if DOE proceeds to or if they have an intention of designing for fault displacement, an early resolution of fault-related design and performance issues would be needed if DOE contemplates such a design approach.

MR. SMITH: Well, I don't disagree at all that the--it is meeting the performance assessment objections that's really critical. Appendix "A" to Part 100 did not preclude site suitability if Quaternary faults were present, nor if you had to design for fault displacement, but you and others that have worked on nuclear power plants know that in a de facto sense, the presence of Quaternary faults and the need to design for fault displacement effectively precluded site suitability for nuclear power plants.

So Appendix "A" didn't say you couldn't do it, either, and you're just saying Appendix--or Part 60 doesn't preclude it or specify it, either, and I think clearly this is an area for some very specific guidance, whether it's informal or in the form of rulemaking.

DR. GUPTA: Thank you.

DR. ALLEN: Other questions from the audience?

(No audible response.)

DR. ALLEN: Okay. Thank you, Dr. Gupta.

The next presentation is by Jacob Philip and Simon Hsiung, and the presentation will be given by Mr. Philip.

MR. PHILIP: First of all, I'd like to thank the Board for giving us the opportunity to talk about our program. My name is Jacob Philip. I'm a geotechnical engineer with the Office of Research at the Nuclear Regulatory Commission, and our basic objective is to do research to help our licensing folks when they review DOE documents. I'm the project manager. The research is being conducted at the Center for Nuclear Waste Regulatory Analysis, the Center.

Assad Chowdhury, who is back there, he's the Project Manager for the work at the Center. He's assisted by Simon Hsiung; M. Ahola, who's not here; Dan Kana, who is not here; and a task force of sub-consultants, subcontractors to the Center.

What are the objectives of our work? Basically, the objectives are to just get an understanding of the important parameters that affect, that are associated with the response of underground structures, and to see the effects, how seismic pumping occurs due to the seismic motion particularly for repetitive seismic motion, and to develop methodologies to see and assess the results to see how good they are, can we make predictions, and this is particularly important when we have--look at scenarios for integrated performance assessments, that we should have some hard data with us for making those determinations.

We have a regulatory basis. I'm not going to go into too much detail on that because the previous speaker, Dinesh, has spoken a lot about it, but one thing I'd like to point out is that the integrated performance assessments is an important part of our trying to understand the phenomenon and trying to quantify it somehow.

The way we are conducting the research project is have a set of tasks. The first task, Task 1, the focused literature search. We look at the literature to see how underground structures are affected by earthquake and stuff like that, and then we have some focused laboratory work supplemented by field studies, and look at the NTS data to see if we could come up with something that could give us a quantitative feel for the effects of seismicity, particularly for our performance assessments.

I'm not going to dwell too much on the next one. It's just the logic and integration of the project tasks, so basically it tells us the lab, the field, and the ground shock data.

The projects and schedules, we have a NUREG-CR report. Anybody who's interested in looking at that, that talks about the state of the art on seismic response of underground openings. We have done some code qualification studies seeing how good the codes are by comparing it with--comparing some of these codes with codes that may be used with the semantical solutions that we have. We have published eight papers in international conferences. We have quarterly and annual reports, technical reports which are available to the public. We finally hope that we get some technical recommendations, technical position papers, or technical positions that we can give guidance to DOE, and the project time frame is from 1988 to 1994.

We have only 30 minutes to talk about our project and I will like Simon Hsiung to give some specific information on the field studies. We have some data. It's preliminary. They are making some judgments. We are going to analyze them in more detail in the future. Maybe we have to do some more experiments to really get a feel as to what is happening, but Simon will give some more information on that.

MR. HSIUNG: My name is Simon Hsiung, Center for Nuclear Waste Regulatory Analysis. The title of my talk is "Seismic Field Studies at Lucky Friday Mine located at Mullan, Idaho," which is about 90 miles east of Spokane, Washington.

This field study has three objectives. The first one is to investigate mechanical response of underground mine facilities, and here we're really talking about the underground openings associated with repeated seismic events. In here we mention seismic events. Really, it's mining and deals with seismic events. It's not earthquakes, and the second objective is to clarify and possibly quantify the relation between underground seismic events and the changing groundwater conditions; and the third objective is trying to generate a set of data that can be used to determine whether the established numerical models can adequately describe the effect of seismic activities on underground structures and groundwater hydrology.

Now I'd like to take a moment at trying to draw the similarity between the mining-induced seismic events and the earthquakes, and literature--research results has shown that basically there is no systematic differences between the mining-induced seismic events and the earthquakes, and they are similar basically in seismic signals, including the P-wave and followed by a shear wave or those type of stuff, although the origin of the seismic event may be different. In this case here, the original that we have observed at the Lucky Friday Mine, all of them are lasting 25 second--half-second.

DR. ALLEN: Excuse me. Would you define what you mean by a mine seismic event?

MR. HSIUNG: Yeah. I'm going to go through that point when I say this point here. Basically, they are similar in mechanism. By saying that, I'd like to say that for the underground mining seismic events, there's really two sources of it. One is the quashing modes, which is--where we induce some kind of rock bursts. I think Dr. Deere has mentioned that and it's a type of quashing of the rock, intact rocks near the stope, or maybe the creation of new fractures near the stope area while mining.

And the second mode of mining-induced seismic events is really a type of fault slip. It would be some mechanism that is basically similar to earthquake mechanism.

DR. ALLEN: But these both occur within the mine itself?

MR. HSIUNG: Yes, within the mine itself, and in the case of the Lucky Friday Mine, all of the seismic events really, as far as I know, were induced by a weakness of bedding plane slip and some of fault slip as well. None of them is because of the quashing mode. It does observe some type of strong vibration because of that--the fault slip in a forced rock burst. So the rock burst is really a result of a fault slip induced, the ground shaking.

It is also observed that most physical and geomechanical principles for earthquakes can also apply to mining-induced seismic events, which give--a very serious implication is that the results that we observed are from underground mining induced by the seismic event, and may be useful for assessing the repository opening responses as well, subject to earthquake motion.

The last point is the mine seismic events occur more frequently than, of course, a natural earthquake so it will give us the opportunity to observe the responses of underground openings.

DR. ALLEN: I still don't understand completely. Are these events that occur on bedding planes, say, sure.

MR. HSIUNG: Yes, because of the--

DR. ALLEN: Are they the release of tectonic strain related to the changing stress distribution because of the mine, or are they the result of stresses induced directly by the mine itself?

MR. HSIUNG: Well, a little bit of both, a little bit of both. Basically, it's because of the stress field modification because of the mining itself. So you create a condition that is favorable for the fault slip.

Okay. In this particular mine that we're looking at, the in situ stress condition with a horizontal stress, and 1.4 of the vertical stresses, and the deepening of the bedding plane and also the deepening of the orebody, when you're mining you reduce the stress field. In this case, the hypothesis is that somehow in some area you reduce the clamping force, which is the normal force that prevents slip, okay? And also, on the other hand, you increase the shear stresses, so create a situation that the fault is prone to slip.

Lucky Friday Mine currently is mining Lucky Friday vein and the Lucky Friday vein is bounded south by a south control fault, and the north and by a north control fault, and there is some dipping by 75° southeast in the bedding plane on the floor wall and the hanging wall there's dipping in the range of 60 to 70°.

There are two basic rock formations in this region. The first one is the St. Regis Member. It's found in many locations in Lucky Friday Mine. It's about 1800 feet deep. In the lower rock formation is a, they call it the Revette formation, Revette Member. The Revette Member consists of three sub-members. Currently, the mining is in the Lower Revette formation and I will talk about the local geology just a little bit.

For the mechanical response study, we are trying to take the measurement of our rock displacement around the openings; also trying to measure the closure of the excavation, and trying to obtain some of the seismic wave data at the location of the instrumentation.

The instrumentation site was selected at the 5210 feet beyond the ground surface. They call it the 5210 sub-level, and here is the first site. This is the second site that was erected and their relative location with respect to the orebody they are supposed to mine. Here is the local striking of a bedding plane and a localized syncline in this area, so this somehow changed the striking of the--strike of the bedding plane. This area was mined between February to the end of October of last year, so considerable stress modification around this area was observed and I will show the data for the closure measurement in just a little bit.

Consequently, because the first site is about 260 feet away from the orebodies, so the stress condition change is relatively minor as compared to the second site.

DR. DEERE: Question; Don Deere.

What was the overall depth of that level that you showed? 5210 is--

MR. HSIUNG: Is about 5210 feet below the ground surface.

DR. DEERE: Okay.

MR. HSIUNG: This view graph shows the instrumentation array. This is the array for the first site. That one over there is for the second site. For each cross-section, we have a total of five multiple position extensometers. On the floor is extensometer No. 1, and to your left are No. 2, No. 3 up there, and No. 5 is to your right, and for each extensometer, we have a total of five anchor points, or I call them anchor positions.

The numbering sequence is Position No. 1, 2, 3, 4 and 5, okay? All the relative distance that was calculated between the anchor, each anchor and also assembly head. In this graph, two convergence stations are also shown; horizontal convergence station and a vertical convergence station. We also have a triaxial velocity gauge installed near to the surface of the opening. It's about one feet into the rock.

This graph shows the relative distribution of the seismic events that occurred since we started taking the measurements, and these are the events that are of interest up to now. For the past 12 months, the mine has more than 40 seismic events with a Richter magnitude of more than one, and here is some of them. They are fairly close to the first site and the second site; each cross representing one seismic event, and the first number here is the event and the second is the magnitude in terms of Richter. And the third order of value--the first one is for the peak particle velocity that are calculated based on the seismic data we have measured, and the first value is for the first site and the second value is the peak particle velocity for the second site here, and the unit is in terms of millimeter per second. The last value is the depth of that event measured from the ground surface in terms of meters.

Here is some correlation between the peak particle velocity and also the scale, the distance. R here is the source location relative to the mechanical measurement site, and it was scaled by this factor. M represents the magnitude of the event. Basically, it shows the linear relationship and you can find the linear equation over there where you see R^2 close to .9.

DR. ALLEN: Excuse me. Do you have focal mechanisms on these events?

MR. HSIUNG: Pardon me?

DR. ALLEN: Focal mechanisms?

MR. HSIUNG: Focal mechanism, that's being studied by Bureau of Mines currently; that basically the concept right now is the, you know, the use of role model in trying to calculate the stress property and also the--

DR. ALLEN: I guess my question, how do you know there is shearing?

(Inaudible response from audience.)

MR. HSIUNG: You're talking about that; single or double, okay.

DR. ALLEN: Ivan Wong of Woodward Clyde reportedly says they do have focal mechanisms showing double.

MR. HSIUNG: Well, there are some events showing the single, also. It's not necessarily double, similar to earthquakes, you know, when you have single and double.

This view graph shows the displacement of the rock mass around the opening. This is for the extensometer No. 1 at 52, and this curve shows certain trends. One if the influence by the mining itself, and also by the seismic event, and also by some other mining activities; this view graph basically showing that we have three distinct displacement changes--I shall not say increase because some of them actually decreased, okay?

These are for certain seismic events, okay? For the March event, May event, also November events. For this March event, the peak particle velocity is at 140 mm/sec, and for the May event it's more than 300 mm/sec; and also, November event, more than 200 mm/sec, and in here the reason I put on two events in there is on that May 22nd and May 23rd we had two events occur side-by-side. They are three hours apart.

Here is the closer look of those two events. For the May events, these are not for displacement, I realize, okay, but for the May 22nd event, with a similar peak particle velocity, it will only see a very small amount of displacement. This tells us that maybe the fault stick-slip theory may be able to explain this type of behavior because after the first responses, that the stress drops substantially. Then you need to have a certain amount of energy in order to build up shear stress back to the shear strength level, okay, in order to cause another slip around the fracture.

Another thing I'd like to point out is that for this October 4th displacement change, there was no seismic event associated with it. Just this happened for Extensometer No. 3, No. 2, and a little bit for the No. 4 as well, and the only explanation that we can come up with right now is that's still using that stick-slip mechanism.

Here I show the closure measurement at the second site as well, and as you can see the maximum closure, that the measure dropped to five inches and the closure measurement was broken down purposely trying to show the fact of the seismic event. So the closure measurements were taken every two weeks, so there's no way that we can actually put a very close correspondence, so that's the best we can do. We can assume that a year and there's no seismic event, and the closure will continue to smoothly increase, okay, since we have this as an event and view certain amount of closure of the opening.

And a point of interest here is that on the July

31st event, that it does somehow--you can see a small amount of increase in the closure but we cannot accept that in the rock mass displacement, okay, so basically rock mass did not respond for the long-term displacement effect to this particular seismic event.

This view graph shows the damage observation, and for the May 22nd or May 23rd event, unfortunately we cannot distinguish which event caused the damage. At our second site, we observed some shotcrete cracks and in this area we observed a very small amount of bedding plane slip as well, very close to our instrumentation of cross-section, and we also observed in this one here that shotcrete cracked and bulged, and also, floor had been pushed up.

Okay. I forgot to mention the rock formation, local rock formation. Basically, the rock formation is dipping 70° in this direction and is actually a layer of quartzite. In between the layer of quartzite, we have some kind of argyllite in between which is very weak, so makes very sensitive to the seismic motion. And bear in mind, for this damage we're looking at a big particle velocity, around 332, that range. This is a typo here. It's supposed to be 332 mm/sec, and I'm going to show you another damage observation for the November 11th--

DR. KENNEDY: Excuse me; Bob Kennedy.

Can you give us a little better feel for what we're really talking about in the way of damage? What percentage of tunnel closure did you have or, I mean--or was the damage just limited to those corners, or did the tunnel have some permanent fraction closure?

MR. HSIUNG: Well, when we're talking about in here, all we observed is the crack of the shaftway, and some push up of the bore for these typical events. But for this one, for the November 11th event with the peak particle velocity only at 220 meter, mm/sec, the same location and the damage observed below this cross-section, all the shotcretes are pretty much cracked, okay? And also, in this area we observed that one to three feet of rock spalling, okay? Consequently, we lost the anchor of this extensometer No. 1 because of that.

So this give us some kind of feeling that really it's the progressive accumulation of the joint displacement that controls the damage of the opening, and although the peak particle velocity or peak acceleration are also important, but it seems to me that in this case we need to at least study the effect of the repeated seismic impact.

And my next topic is the groundwater change study. In this study, we're trying to monitor the water pressure at the selected geological features, and also we're trying to monitor ground motion using hydrophone.

In order to study this groundwater change, we drilled a 1180 feet long borehole, four inches in diameter, started at corner of 5700 level below the surface with a angle of--downward angle of 20°, and we identified four zones, each zone that contained specific features, and the Zone No. 1 is the lowest and Zone No. 4, located closer to the 5700 level, and incidentally, the packers that were used trying to seal the Zone 4 somehow malfunctioned so we cannot collect data for Zone 4.

Again, I'd like to show this distribution of the seismic event again. Based on the observations so far, all the seismic events with magnitudes of more than two have somehow caused groundwater table change. I will show the results in just a little bit, and we also observed for this event occurred right in the event with a magnitude smaller than two; 1.8 also caused some kind of groundwater responses.

Here's the one result of a water pressure change for Zone 3, and as you can see in here, that the water pressure responds to a seismic event and experiences a sharp increase in water pressure, and after that it started to--the groundwater pressure started to decrease, but after it decreased to this point, another seismic event occurred coming in and instantly the water pressure responded to have another sharp jump, then started to decrease. And normally, the groundwater change, it returned to the normal condition within a few hours to even a few days.

For most of the groundwater changes that we observed is associated with a water table--a water pressure increase, but for this typical case, okay, for the Zone 1 we actually experienced a pressure drop. Then it started gradually to recover. At this point, there's 25 minutes before we had another seismic event coming in and, of course, an instant increase in the water pressure, then gradually recover.

MR. TILLSON: Will you point out where that event is relative to the--I see two on December the 11th, but I don't see a December 12th event.

MR. HSIUNG: Oh, okay.

MR. TILLSON: Or a series of events. There was one on December 12th, and where was that--

MR. HSIUNG: There are two on December 11th. One is in here.

MR. TILLSON: Yeah, that's the 11th.

MR. HSIUNG: Yes, sir. The other one is in here.

MR. TILLSON: Where's the one on the 12th?

MR. HSIUNG: 12th? Oh, okay. This is the water pressure measurement, okay. We continuously measure the--we take the measurement every 60 seconds, okay. These are the data points that we have got.

Based on the limited data that we have obtained we may come up with some summaries. I am kind of hesitate to call it a conclusion at the moment and the causes of the step displacement change may be due to several conditions. One is the seismic impact, and the other one is the mining activity itself caused the stress redistribution, and the shear stress along a weakness plane exceeds the residual stress and suddenly give in and the third type is the backfill operation, and I did not show you the results, but it is in your package.

The second point is openings did not necessarily respond to all seismic events with higher than threshold peak particle velocity, and another observation that we have come up with is the opening had a relative higher local state of stress are more sensitive to seismic events. That's been evidenced by the relatively insensitive response of--to the seismic events at the first site. This is the second site and they are very close to the mining activities.

The other point is the joint stick-slip mechanism may be used to explain certain phenomena that we have observed. The first one is the difference in displacement changes induced by events with similar particle velocities, why we have different displacement; and the second one is why openings sometimes do not respond to mine seismic events with high enough peak particle velocity.

For the groundwater pressure part, the current observation indicates that groundwater responded to almost all the seismic events with a magnitude of greater than 2.0, and water pressure normally will increase in all three areas that are packed off for the mines due to the seismic events, but we did have one observation that only one zone responded to a seismic event and the other two zones did not.

That pretty much concludes my talk. Any questions?

DR. ALLEN: Thank you very much.

Do we have questions from the Board? Don Deere?

DR. DEERE: Yes. I have a question about the groundwater rise. It seemed like it was quite uniformly between about 1½ and 2 feet of head difference, potential head.

MR. HSIUNG: The maximum that we have observed is about close to 4.4 feet. That's because of seismic event of a magnitude 3.5, which is maybe 300 meter away from our area of interest.

DR. DEERE: Did you show that in one of your slides?

MR. HSIUNG: I don't know whether that I included it in there or not.

DR. DEERE: I looked at two or three of them. There seemed to be about two feet--

MR. HSIUNG: If you can check to see whether you see that; a September 19th event. That one shows you about 2 psi increase, equivalent to 4.4 feet.

DR. DEERE: I see. I wondered if that particular one took the same six hours, more or less, to stabilized, or was it somewhat different?

MR. HSIUNG: It's a about a little bit over three hours, I believe.

DR. DEERE: In about three hours?

MR. HSIUNG: Yes.

DR. DEERE: Perhaps an explanation of the difficulty making interpretations is that you have apparently a very well-pronounced bedding and very rigid, or at least very hard--and I presume fairly high modulous quartzite beds, occasionally separated by a very weak argyllite and, therefore, the stress is probably carried almost entirely along the beds and the argyllite is just sort of sitting there, and suddenly it has a chance to get into the act because when a free excavation is made and the low strength is there, it is able to move in.

And are there folds in the area, or faults that change the orientation of the beds, or are they fairly consistent across the mine?

MR. HSIUNG: I think that I have shown that one view graph in showing one minor fold, folding around our instrumentation area. Let me put that up again.

(Pause.)

MR. HSIUNG: This is the folding that we have observed, and also, in this area, in the--the dipping is not really like a straight line. It's really like a ripple, so it's also folded in this way and folded in--folded downward and upwards, and also folded in this direction.

DR. DEERE: Okay, thank you.

DR. ALLEN: Other questions?

(No audible response.)

DR. ALLEN: Questions from the audience?

MR. WANG: Ivan Wong, Woodward Clyde. I'm a seismologist.

Just maybe for clarification, on a worldwide basis on studies of mine seismicity, we cannot differentiate between a rock burst or mine tremor and a typical tectonic earthquake. They both appear to be the result of shear failure. If you look at the seismogram of a rock burst, you see a very well-pronounced S-wave. You can do focal mechanisms and you come up with a classical double coupled failure. So we think that earthquakes and rock bursts both have the same mechanism.

Rock bursts also follow the typical Gutenberg-Richter relationship for frequency. They have very similar source parameters. The difference where we've seen rock bursts, in particular, in South Africa and earthquakes is that in some cases, we will see stress drops for rock bursts that are on the order of several hundred bars vis-a-vis stress drops for typical tectonic earthquakes, which are on the order of 100 bars, and the thinking is that these high stress drop rock bursts are due to fracture of intact rock where, in most cases, earthquakes and the majority of rock bursts are due to slip on preexisting zones of weakness, either joints or faults or those type of discontinuities.

The Lucky Friday Mine happens to have a major fault system running through it, and so there are faults and joints of varied orientation.

DR. ALLEN: Thank you.

MR. HSIUNG: Thank you for clearing that point for me.

MR. SUMMERVILLE: Paul Summerville, seismologist at Woodward Clyde.

Just pursuing the issue of the focal mechanisms, if you have a normal faulting event, you should get a pressure increase; and if you have a reverse faulting event, you should have a pressure decrease. I think it would be interesting to see whether there is that correlation of your events, a mechanism correlation with the sense of the pressure change.

MR. HSIUNG: Yeah. We are also interested in that because I mentioned the point, and right now, currently, Bureau of Mines in Spokane Center is doing that type of analysis. We will be able to get those information from them and try to correlate all those things then with our responses to see whether we can establish some kind of relationship or not.

DR. ALLEN: Okay, thank you very much.

Our final presentation today was to have been by Carl Johnson of the State of Nevada. He could not be here, but David Tillson is, and as I understand it, Johnson and Tillson are both co-authors of this particular paper. I'll be following this with some interest since I have two telephone calls from Las Vegas reporters this afternoon asking the Board's comments on what you're going to say.

MR. TILLSON: I have some good news and some bad news for you. The first is, the bad news is that Carl could not be here. He is constrained to traveling within the boundaries of the State of Nevada due to some minor bureaucratic problems that restricted the travel. He has no money. The good news is that that allowed him to travel to Las Vegas and perhaps make statements there.

(Laughter.)

MR. TILLSON: There is also some good news in the effect that I am not an employee of the State of Nevada. I am a consultant to the State of Nevada. I do not speak to them. I will--have been constrained to read the text, and for those of you who have been with me before in meetings, this is good news because I have a tendency to ramble otherwise.

Now, for those of you who also will follow to see how well I read a speech, I've put up some view graphs. You will not find these attached. They are more to set the stage for what we will be saying.

A key health and safety issue at any site proposed for the disposal of high-level radioactive waste is the seismic hazards. Of primary concern are the effects of earthquake-induced vibratory ground motion and fault movement during both the preclosure and postclosure phases. One might assume that to minimize these effects on health and safety, repository sites having low potential for seismic hazards would have been selected. However, as the case with Yucca Mountain, it is possible that a site could be selected in which little attention was given to its seismic hazards during the selection process.

A repository sited in a seismically active area is vulnerable to damage and possible loss of isolation from seismic events during both the preclosure and postclosure periods. Seismic activity can affect the site in the form of vibratory ground motion, fault displacement within the repository, or both. Such events, individually or cumulatively, have the potential to significantly compromise in an unpredictable way the integrity of the repository engineered barrier system; that is, waste canisters, seals, et cetera during both the preclosure and postclosure phases; the surface radioactive waste handling facilities (that is, the hot cells, for example) during the preclosure phase; and perhaps most importantly, the system performance (through changes in the site geology and hydrology) during both the preclosure and postclosure phases. This paper explores the potential seismic hazard at Yucca Mountain from vibratory ground motion and fault displacement.

The Great Basin of Nevada is seismically active. For those of you who don't recognize that, perhaps the picture on the right will sway you to that view. The historical record lists numerous events in the 6 to 7 magnitude range. In addition, paleoseismic evidence suggest many more large magnitude events during the Quaternary period.

The pattern of regional seismicity consists of the North-South Trending California Seismic Belt, the southern end of the Intermountain Seismic Belt in Southeastern Utah, the East-West Seismic Belt encompassing the Nevada Test Site, and Yucca Mountain. The East-West Seismic Belt is a diffuse pattern characterized by clusters of intense activity separated by areas of lesser activity. Studies by the University of Nevada-Reno have suggested both a temporal and spatial clustering of regional seismicity in the Great Basin through geologic time. This clustering response to extensional tectonics in effect produces great uncertainty in predicting patterns of future events during the postclosure period.

The casual mechanism of earthquakes in the Southern Great Basin is not well understood. As a result, seismic source zones are generally used to define the seismic hazard. Yucca Mountain is located within one of those seismic source zones, the Walker-Lane. The Walker-Lane seismic zone also contains the Cedar Mountain Fault, the site of a magnitude 7.3 earthquake in 1932. Analysis of the fault lengths versus earthquake magnitude data used in the Site Characterization Plan for Yucca Mountain indicates a maximum magnitude at the site would be magnitude 6.4. However, fault length versus magnitude calculations by the Nevada Bureau of Mines and Geology suggests a magnitude 7.0 earthquake would be more appropriate.

Translating these datas into vibratory ground motion at the site, the SCP suggests a maximum acceleration of 0.4g is to be expected at the site, while the Nevada Bureau of Mines and Geology estimates from its data that the ground accelerations could be as high as 1.0g. The differences in these calculated accelerations can be ascribed to differences in the geologic models used to define fault processes at Yucca Mountain; that is, a discrete normal-slip model versus a distributive oblique-slip fault model. Resolution of these models will be critical, of course, if a consensus is to be reached on seismic design parameters before any significant design work begins.

Clouding the issue of determining an appropriate ground acceleration for the site is the position of the U.S. Nuclear Waste Technical Review Board that, "It is the opinion of the Board that the vibratory ground motion associated with a relatively large local earthquake, and even some surface faulting beneath critical areas of the loading facility, however unlikely, may not entail untoward concerns for public safety provided they are adequately foreseen and compensated for in the engineering design."

This conclusionary statement about the seismic hazard potential at Yucca Mountain was not supported in their report by any data or analysis. Instead, it was argued in the report that if the potential occurrence of natural phenomena alone, such as an earthquake ground motion or fault displacement, should be the sole criteria for defining the viability of a repository site, then many large regions in the United States and throughout the world would be considered unfit for construction or human habitation simply because earthquakes have occurred. At the very least, this cavalier attitude suggests a level of naivete as to the hazard imposed on public health and safety by a high-level radioactive waste repository.

The NWTRB appears to have forgotten that from 1983 to 1987 there were nine potentially acceptable sites being proposed by the DOE, and only one of those sites, Yucca

Mountain, had significant faulting, active or not, present within the repository site area. It appears that political and economic expediency may have somehow changed the rules to allow for active faulting at and within Yucca Mountain, when it is almost certain that active faulting at any one of the other eight sites would have readily been used as a disqualifier. In lieu of empirical data, how can rational decisions be made on whether the effects can be adequately foreseen and compensated for with engineering design?

Yucca Mountain site and area studies have identified at least 32 faults with demonstrated or suggested displacement within the Quaternary period. This includes at least one fault, the Ghost Dance, within the proposed repository perimeter, and at least two faults--Solitario Canyon and Paintbrush Canyon--that bound the proposed repository block. The probability that other active faults will be found underground within the repository block is relatively high.

Early field studies suggested that these faults were characterized by normal dip-slip motion. However, more recent work has indicated these faults also show evidence of left-lateral strike slip motion. Field observations by the Nevada Bureau of Mines and Geology initially concluded that at least four of the faults may have moved simultaneously, possibly contemporary with a volcanic event. This is based on the occurrence of volcanic ash entrapped within the fault planes. This evidence suggests a model of clockwise rotation of Yucca Mountain in which multiple faults move simultaneously in response to either seismic events on deeper structures, or tectonically coupled processes that may accompany renewed volcanism.

Therefore, in evaluating the hazard at Yucca Mountain, the site should be considered to be within a seismic source zone containing numerous faults, any or all of which can move in response to a single tectonic event, thus requiring the consideration of multiple simultaneous fault displacements, as well as the cumulative effects of ground motion.

At a recent meeting of the NRC's Advisory Committee on Nuclear Waste, ACNW--Paul, I told you I wouldn't forget you--the NRC staff stated that fault displacement within a repository was acceptable as long as the total system performance objectives could be met. The ACNW recommended that the NRC staff develop a policy position to support that opinion. The TRB, in its second report, also concluded that postclosure faulting within a repository should not be a disqualifying condition, provided that engineering and hydrologic implications have been adequately addressed prior to waste emplacement.

These views represent a significant departure from previous positions of the NRC relative to proximity of nuclear facility sites to faults which could experience movement during the design life of the facility. Considering that the life of a repository will be 10,000 years or greater, demonstration that engineering and hydrologic implications have been "adequately" addressed prior to waste emplacement may prove to be exceedingly difficult for the DOE in light of minimal regulatory guidance.

A review of the past practices in siting nuclear facilities is useful in discussing the ramifications of this departure. In the late sixties, serious attempts were made to site nuclear power stations in California astride active faults. Bodega Bay and Malibu are two such examples. Locating the Bolsa Island Nuclear Desalinization Plant within an active fault zone was also contemplated. Nuclear facility designers and engineers argued that they could design for fault displacement and large ground acceleration. The facility applicants argued to the NRC that regulations did not prohibit such siting, and if the engineers were confident that the facility was safe, then the risk to the public health and safety was minimal.

The NRC, however, was skeptical and required demonstrated proof of minimal risk. Ultimately, the applicant withdrew claiming the cost and time required to prove minimal risk was unacceptable to their projects.

Subsequently, in the early 1970's, Appendix A to 10 CFR 100 was added, defining the criteria for siting nuclear power plants. This criteria included a de facto exclusion of nuclear power facility sites within five miles of a capable fault; that is, a fault which has the potential for movement within the life of the facility.

Appendix A was the staff's response to managing the large health and safety uncertainties generated by near and on-site displacement and associated large ground accelerations. While the exclusion zone approach adopted in Appendix A may represent the influence of societal perception of unacceptable risks, this cannot be documented. However, it can be argued that Appendix A criteria containing the exclusion is an expression of the public's view that locating nuclear power stations astride or near active faults presents an unacceptable and, therefore, avoidable risk.

Returning to the repository issue, it has been argued by the TRB and the DOE that a nuclear reactor is a highly complex, relatively short-lived surface facility where an accident could result in an immediate and unacceptable release of radionuclides, and therefore the risk is greater than for a repository. However, the NRC in is 10 CFR Part 72 regulations for non-reactor nuclear facilities, including Monitored Retrievable Storage Facilities, incorporated the Appendix A siting criteria, including the fault exclusion criteria.

Similarly, DOE Order 6430.1A, General Design Criteria dated April 6, 1989--and I think we heard reference earlier to UCRL 15910, which I think Bob Kennedy had a hand in developing--requires that at a minimum 10 CFR Part 100, Appendix A procedures be followed in establishing seismic design parameters for all non-nuclear reactor nuclear facilities, including waste repositories.

It is obvious the NRC staff viewed a reactor and a non-reactor facility as posing similar and avoidable levels of risk to the public. If the NRC views a nuclear reactor and a passive spent fuel storage facility as having similar risks relative to seismicity, then this would appear to contradict the argument that risks of active and passive nuclear facilities are vastly different.

The possibility of fault displacement within a repository involves a new level of increased uncertainty and an additional regulatory complication. Fault movement, particularly within the postclosure period, will likely generate secondary effects; that is, secondary fault movement and new fractures. If the faults at Yucca Mountain are part of a larger fault zone, then simultaneous movement on multiple faults could amplify secondary faulting and fracturing with an attendant decrease in the integrity of the engineered seals and the containment capability of the host rock.

Most importantly, however, it would also increase the pathways for groundwater flow and radionuclide transport tot he accessible environment. Compounding the problem is the need to provide an adequate physical characterization of the three-dimensional nature of the geologic system between the repository block and the saturated groundwater regime. Establishing hard data, sufficient hard data on all the parameters that can affect performance will be difficult to accomplish without drilling a considerable number of boreholes which, in turn, will likely jeopardize system integrity even further. Prediction of effects of any additional changes that may be caused by cumulative vibratory ground motion and/or fault displacement and their impact on site performance is necessarily highly uncertain. Credible demonstration of the performance of such effects on the system will be impossible.

Finally, the view that near or on-site fault displacement is acceptable assumes that no new faults will be created, especially within the repository. Nevada is an active seismic area where fault movement has occurred and will continue to occur for the foreseeable future. Field observations after current tectonic events indicates that most surface displacements occur on faults displaced during past events. Studies of active faults throughout the Great

Basin substantiate this observation.

However, as must be expected, this is not always the case. For example, observations of the Cedar Mountain Fault Zone by the Nevada Bureau of Mines and Geology document displacement at a location with no evidence of previous activity. This occurrence indicates either formation of a new fault, or extension of a previously displaced fault and confirms that in the Great Basin faults can be created during current tectonic events with a magnitude similar to that which could be expected at Yucca Mountain. Prediction of the location and magnitude of displacement of newly generated faults will be difficult, if not impossible. There can be little confidence in engineering designs that would attempt to compensate for such unpredictable events.

The DOE's 1979 Environmental Impact Statement for mined geologic repositories contained a list of geologic factors which must be considered in assessing sites for repositories. The existence of seismic hazards were considered to be of key importance, and sites with such hazards were considered undesirable. In fact, DOE siting guidelines, 10 CFR 960.4-2-7(d) list seismic hazards as one of the few disqualifying conditions. Yet in 1987, the Yucca Mountain site, which was known to be subject to seismic hazards, was selected.

The DOE and the TRB have recommended engineering design solutions to compensate for this hazard. Also, the NRC staff appears to be of the view that a seismic hazard is acceptable as long as site performance can be achieved. However, this contradicts the NRC staff position relative to other nuclear facilities.

A geologic repository for high-level radioactive waste is a first-of-a-kind facility. It has been argued that the technology required for development of a nuclear waste repository is not new; however, predicting performance of the geologic environment for 10,000 years and beyond with reasonable confidence is new, having never before been attempted. Given the long-term risk posed by such facilities and the inherent difficulty in predicting future performance, will society accept siting such a facility in the area with certain yet unpredictable seismic hazards? The answer to this question cannot be cloaked in contradictory regulatory policy.

And I have one last slide which some of you may have seen before. It's been around for awhile, and we think that this is one of the major problems.

That's all I have, Clarence.

DR. ALLEN: Okay. Thank you, David. I think you've made your position clear.

MR. TILLSON: Well, I'll be certain to stay around just in case.

DR. ALLEN: May I ask if there are questions or comments from the Board or staff?

(No audible response.)

DR. ALLEN: Let me ask, or let me make a statement. In just my hurried reading of this as you read it, as far as I can see, you quoted our Board correctly. Some of the conclusions you drew were not the same as we drew but, nevertheless, I think, as far as I can tell, we were quoted correctly.

May I ask the Nuclear Regulatory Commission is they think they were quoted correctly or interpreted correctly? There are many statements here concerning NRC regulations and so forth, that I'd be interested in knowing whether you think you've been quoted correctly on those.

MR. BALLARD: Ron Ballard, NRC.

Yeah, in general, I believe the statements were fairly accurately stated as to what NRC has said. We've been--we've commented on this just a few weeks ago with the Advisory Committee and our own Advisory Committee, and those are pretty accurately stated. I believe I agree with Dr. Allen that the conclusions we may want to debate a little bit, and that's about all I would say at this stage.

MR. TILLSON: I don't think that you'll find, if you read it, that we reach too many conclusions. We're really calling for consideration of what we consider to be one of the key issues, and that is regulatory consistency, and I think this was mentioned before, the need for guidance from the NRC, specific regulations to handle what I see is going to be one of the most difficult problems, and that's prediction of performance on fault displacement once the repository has been closed. That's very difficult.

MR. BALLARD: I would just say that I agree that's going to be a difficult problem. That's one that we've reflected very clearly in our comments on the site characterization plan, and that I guess--I believe the comments were already made that, of course, Part 100 does not exclude siting.

MR. TILLSON: I don't think we were talking about Part 100, really. We're talking about the concept, as Jay said earlier.

MR. BALLARD: Well, okay. I'm not sure what exactly that was, but we have all acknowledged that we do have a different kind of facility here and we're really relying on the site characterization to resolve the issue as far as regulations go.

MR. TILLSON: I think we could carry on this debate for a long time.

DR. ALLEN: Let me ask if there are others in the audience who would like to have some comment or questions? For example, the U.S. Geological Survey; any particular comments? Even Jay Smith doesn't want to say anything.

MR. TILLSON: I think one of the points, Clarence--just led me add one thing--that I personally would want to make--and this is--I'm not speaking for the State of Nevada--and the reason I chose those illustrations to use is to indicate that there are very large expenditures of time and money, and perhaps in this case, even more importantly is the time element than the money because the nation does need a waste repository, and the issue of fault displacement in that repository generically is no different than the issue of fault displacement in a nuclear reactor in terms of the public perception, and I would hate to see a considerable expenditure of money and time--particularly time--and then be found that it's not going to be acceptable under any condition.

But we're just calling for some more specific regulatory guidance to deal with this issue as early as possible.

DR. ALLEN: Oh, Jay Smith does have a comment.

MR. SMITH: Well, it's hard to pass up an opportunity when Dr. Allen invites one to speak or to comment.

I think that there's a lot of parallel between the points that Dave Tillson and Carl Johnson have presented here and some of the comments I have made. My comments reflect some views of the nuclear utility industry, which has been speaking for some time of its concern over regulatory consistency and a proper scope of new regulations to provide the kind of guidance that we're all asking for.

Personally, I feel that if the NRC staff, let alone future ASLB's or Appeals Board, hold the view now that designing for fault displacement is fundamentally not acceptable, then DOE ought to be advised of that now or as quickly as possible so that the impact on suitability of Yucca Mountain can be determined as soon as possible. The utility industry certainly supports the DOE program and would like to see an early determination of whether the site is suitable, and if it is, let's get on with it. If it's not, let's start considering some contingency plans.

MR. TILLSON: One caveat there. I think Jay is referring to displacement during the operating or the preclosure phase, primarily, and I'm not so sure that that couldn't be accommodated. I have a lot of confidence in the engineers. I'm not so sure that it could be effectively licensed through the regulations just as Bodega Bay had a lot of problems. However, the postclosure is another matter. I don't think anyone has the ability to say that they can effectively design for displacement in the repository during the post-closure.

MR. SMITH: Well, I haven't limited my remarks just to the preclosure period.

MR. TILLSON: Right.

MR. SMITH: But I think we certainly are in a relatively new field from a regulatory standpoint of plowing the ground with regard to the ability, fundamentally a a concept, to design to accommodate faulting.

DR. ALLEN: Yes.

MR. FENSTER: Dave Fenster, Woodward Clyde.

I just wanted to state that DOE has not chosen to interpret the applicability of Appendix A in Part 72 as an interpretation of, let's say, a disqualifying condition at a potential MRS site. So we basically looked at that as if there's large areas of the U.S. The only document out right now is basically large scale screening site requirements in considerations, so if you looked at the entire U.S., large areas do not contain faults that we would consider disqualifying.

MR. TILLSON: That wasn't the point I was trying to make. One, 10 CFR 960 refers primarily to the waste repository not the MRS; and two, it was the issue that active tectonics in faulting in the repository was considered by DOE as a disqualifier at a very early stage, and yet they chose to ignore one of the few disqualifying conditions in recommending a site. That was the point we were trying to make.

MR. FENSTER: There was another point I guess I'd like to make, which is something DOE and NRC really haven't addressed, mainly because we don't have an MRS site, and that's the interpretation that perhaps east of the Rocky Mountain front, you're really not applying Appendix A.

MR. TILLSON: It was a philosophical point that the NRC had chosen to use 10 CFR 100, Appendix A in evaluating meeting 10 CFR 72, that's all. It wasn't whether 10 CFR 100, Appendix A applies to the high-level waste repository. That was not the point. There is an inconsistency in the NRC's regulatory requirements.

DR. ALLEN: Just a moment. Leon had a question here.

DR. REITER: Dave, in the spirit of Clarence attempting to make sure things are quoted correctly, I wonder if you or anybody else would have a full statement of the disqualifying condition in 10 CFR 960.4.2.7?

MR. TILLSON: Uh--

DR. REITER: Is that a full statement?

MR. TILLSON: Hopefully. I meant to bring one along because I thought that question would be asked.

DR. REITER: Okay. I wonder if you could read that.

DR. GUPTA: As you know, there are two disqualifying conditions in 10 CFR 960. One deals with postclosure and the other one deals with preclosure guidelines that relate to vibratory ground motion and fault displacement.

The one that deals with postclosure states that: "A site shall be disqualified if, based on the geologic record during the quaternary period, the nature and rates of fault movement or other ground motion are expected to be such that a loss of waste isolation is likely to occur."

For preclosure, the guideline states that: "A site shall be disqualified if, based on the expected nature and rates of fault movement or other ground motion, it is likely that engineering measures that are beyond reasonably available technology will be required for exploratory shaft construction or for repository construction, operation, or closure."

If you look at the postclosure guideline, I think it is very consistent with the statement that I made earlier, that the bottom line is whether the site can meet the waste isolation requirement; whether the site can be shown to meet the performance objectives. With respect to preclosure, the emphasis here seems to be whether the technology is available to design the facility to account for the rates of fault movement or other ground motion.

DR. REITER: I think the point is made.

MR. TILLSON: Yeah, the point that we would like to emphasize here is not whether you could design or whether there is a credible loss of waste isolation that is likely to occur, but how will you demonstrate that? How can it be demonstrated?

We heard discussions earlier by--I'm sorry, your name is John, no, the--yes.

MR. MERRITT: Jay Merritt.

MR. TILLSON: Jay, I'm sorry--about how one would have to be able to grout for displacement in the repository to handle the problem of influx from perched groundwater, and that's fine. How would one handle the problem of those same faults that extend from the repository down to the groundwater, being able to grout those to keep the water from going further on down? I can see how you could keep it from coming in, but I'm not sure how you would keep it from going out, and therein lies the crux of the problem, being able to demonstrate performance for 10,000 years.

DR. DEERE: Don Deere. I'd raise the question that Jay really was talking about the repository in any detail at all. I think it was a generic term.

MR. TILLSON: Well, I don't know of any perched groundwater sitting above surface facilities. You specifically said perched groundwater. The only perched groundwater that I know to exist would be over the repository block.

DR. ALLEN: Yes? Larry Hayes, USGS.

MR. HAYES: Larry Hayes, USGS.

First, Dave, I'd like to compliment you on doing what I thought might have been impossible, and that's making a very exciting and interesting reading of a paper.

Certainly, you were interesting and had our attention.

Could you put back your last slide where you referenced Geologic Survey?

(Laughter.)

MR. HAYES: Okay. I've learned that I'm normally mistaken when I assume something, but I have assumed you're referring to the U.S. Geological Survey in your overhead?

MR. TILLSON: Yes, that is correct. That is actually reproduced in toto right out of Geotimes, so I won't take responsibility for that.

MR. HAYES: Could you give me the date of the Geotimes article, because in interest of getting the whole story, I'd like to sort of read that in context with--

MR. TILLSON: Well, as I recall--and Clarence probably might even remember this--as I recall, that was a comment letter or an appendix to a safety evaluation statement by the NRC, or it may have been prior to the NRC. It might have been the AEC, and it was either on Malibu or it was on one of those California sites. But yes, I can get you that reference.

MR. HAYES: I'd appreciate that and take it from there. Thank you.

DR. ALLEN: We'll take some from G.K. Gilbert.

(Laughter.)

MR. TILLSON: For those that didn't hear me, that won the People's Prize for Purple Prose from Bob Bates in Geotimes.

DR. ALLEN: Any other comments? Yes, Ron Ballard.

MR. BALLARD: Just one more. The NRC really is sensitized to this particular problem. I don't--many of you ought to know, regulations kind of--the production of regulations goes rather slowly. I think tomorrow you'll be hearing from Keith McConnell the first of a series of technical positions the staff is trying to develop that's trying to approach this problem, not necessarily the inconsistency you speak to, but nonetheless, trying to work out with DOE an appropriate way to handle it. We are also actively involved in so-called -- performance assessments. This is the process required by the EPA standard on the total systems analysis, and primarily to develop staff skills and to see if the regulations can really be implemented. I believe DOE is doing the same thing. We've worked with them for the last couple of years on this, encouraging rather strongly and vociferously a move in this direction, and I believe they're responding very well and I believe they could speak to the fact that there is supposed to be some work coming out to see if we can demonstrate, in fact, what the EPA standard and the statutes call for.

MR. TILLSON: Just to be sure that I don't only cast stones, I want to compliment Dinesh--and I'm sure the other people who were involved, and there were many--that it was very encouraging for me personally to see the reference to the kinds of problems that we are alluding to, and I am encouraged that the NRC is starting to think about this problem much more seriously than they have been in the past.

DR. ALLEN: Okay. Thank you, David, and thanks to all the other participants today. With our typical cavalier attitude, I'll declare the meeting closed for the day and remind you that we will commence at eight-thirty in the morning.

And also, since this room is going to be used for other purposes tonight, you must remove your belongings over the evening.

Thank you very much.

(Whereupon, the meeting was adjourned, to reconvene at 8:30 a.m. on January 23, 1992.)

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