UNITED STATES
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 K. McFarland, Nuclear Waste Technical
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Dr. Edward J. Cording, Consultant

Dr. Robert Kennedy, Consultant

I N D E X

SPEAKERS: PAGE NO.:

PROCEEDINGS

8:30 a.m.

DR. ALLEN: Good morning. This is the second day of the meeting of the Panel on Structural Geology & Geoengineering, of which I am chairman. The Agenda says that I will make some opening remarks. My opening remarks will be to turn over the microphone to Don Deere, Chairman of the Board, who, I think has some comments of his own.

DR. DEERE: Thank you, Clarence, ladies and gentlemen.

Yesterday, when I gave my opening welcoming remarks, I had another page and a half of comments I was going to offer but, decided it would be better to wait until we had a chance to hear the speakers and to have some discussion. Since the topic of the damaged tunnels was fairly well covered yesterday, I will now make my comments about that particular topic.

I would like to offer some technical comments about the use of case histories pertaining to the damage to tunnels by earthquake ground motions. Case histories could offer much on this subject, but all too often little or no data are given regarding two of the key elements.

One, the quality of the rock expressed in some term or description and not only the quality of the rock where the damage occurred, but the quality of the rock in the other 80 or 90 percent of the tunnel where no damage occurred while subjected to similar ground motions. Two, the type and intensity of ground support that was used. Was it little to occasional rock bolt? Or was it pattern bolting four meters long? Or pattern bolting five feet long? And were the bolts 7/8ths inch in diameter or 1 and 1/4 inch? Were they high strength steel or low strength steel? Did they have the ability to elongate with the wave or would they go brittle and fail?

These are types that really localize the failure; the type of support and the quality of the ground. And you might think, oh, but the mining engineers, the civil engineers, engineering geologists who are driving these, they put it in exactly like it should be. They make the decisions to put it in. Too often they do not. It depends on the type of contract that is written and the advantage to the contractor. If he is getting two dollars a pound for setting steel sets in a tunnel and he is making money at that, the ground looks awfully bad to him. On the other hand, say if he is given a contract which says you shall supply all support at no additional cost, then the ground looks awfully good. When the inspectors go in and they look up, they get a little scared.

So, there are a number of these elements that make it extremely difficult to just take a point from failure out of tunnel and say that it was at this depth and this was the ground motion, approximately, and that was 700 feet deep, therefore 700 foot deep is good or it is bad. So a terrible lot of caution has to be used with this us.

Under static loading conditions, the factor of safety in a fault zone may be only, let's say 1.1 to 1.3, if we could calculate it that closely, but it could very low, enough for the static conditions to make you feel comfortable and for the miners to work. That particular area may be stable for the construction, but it may have a very low factor of safety.

On the other hand, in the remainder of the tunnel, we may have widely spaced joints, joints that are slightly irregular and very tight. In these areas, the static factor safety is probably greater than five and maybe fifteen or twenty. Now if we have a superimposed dynamic event, whether it be from explosives or whether it be an earthquake, we know which area is going to fail. It is going to be the one with the very low factor of safety. However, in 700 case histories, you may be lucky if you have information on five or ten of them that allow you to have any comfort in really what does the point mean.

Also, the tunnel in some cases when approaching the fault zone, may be extremely well supported, to the point that you have a factor of safety that may well be in the range of two, three, four or five. And then when the dynamic motion is superimposed, it comes through very beautifully.

So, it is this existing factor of safety and that is a combination of the quality of the rock and the quality it details of the support that has been placed. Then, the other variable, of course, is the magnitude and the type of the ground motion that the tunnel is subjected to.

In light of these two crucial factors in static and dynamic stability of the tunnels, that is the rock quality and degree of tunnel support employed, it is not surprising that statistical studies of tunnel damage versus earthquake or other dynamic loading parameters, and versus depth of tunnel, type of rock, size of tunnel, etc., have not given too much insight into the problems. And certainly, not enough for design. The scatter would be so wide and the really good data points so few, that it is extremely improbable that leaving a site and extrapolating to another site based on information from around the world, is really not sufficient for us to understand the problem to do the design.

Now I am very pleased that a number of the studies to be presented at this meeting, and I feel I can add, those presented at this meeting to-date, have contained many of the critical parameters. And particularly when we are in a site in a geological medium where some rock quality indices are available, we found yesterday four were used for the tunnels at the Nevada Test Site by DNA, and these do give you some idea of the ground quality. They have assigned some numerical value to those and another system might have other numerical values, but at least, when you are looking at one-third of the case histories in the worst ground, you know which ones they are, and were you looking for the best, you know which they are. And, you have a very good control of the ground motion parameters.

Therefore, I think that the results obtained and the conclusions that have been drawn really were of great interest to all of us and can have some value. It is also clear that there are some differences in the loading criteria in the type of ground motion and these make a complicating factor that also has to be taken into account in any extrapolation. Those are my main comments on this.

I think case histories, which is really experience is highly helpful, but it is very difficult to take a broad-brush trend and put it immediately at a given design at a given site.

Thank you.

DR. ALLEN: Thank you, Don.

We have had a request for a very short statement this morning by Ron Ballard of the Nuclear Regulatory Commission, which will come next. I would just as soon though not prolong this particular discussion, and further responses let's defer until this afternoon, which we can return to this subject if we wish.

Ron Ballard.

MR. BALLARD: Thank you.

I would like to just take a couple of minutes to comment on Dave Tillson's presentation yesterday, particularly the aspects where he believed that NRC's regulations are contradictory when comparing repository regulations to those of the MRS and reactors.

I gave it quite a bit of thought last night, and I really don't believe they are contradictory and I would like to make a very few points here to summarize that. Most of these points I believe you are aware of. First of all, as most of you know, Part 100 does apply directly to reactors and these are surface facilities with lifetimes on the range of decades. The Part 72 regulations apply to Monitored Retrievable Storage Facilities and again these are surface facilities with lifetimes that are very similar to reactors.

Even the pre-closure aspects of Part 60, the repository regulations are of similar lifetimes, perhaps a little longer. We talk about a hundred years versus maybe forty or fifty. But, they are surface facilities with similar lifetimes. And if you will note in the regs, we do apply similar requirements. We have in 60.111 for example; the requirement for applying Part 20 regulations to the preclosure period. This is very consistent with our treatment of reactors in MRSs. Also, we have requirements related to structure systems and components important to safety. Again, a consistency, I believe.

A little later on this morning Keith McConnell will be giving a briefing on one of our technical positions, which is the first of a series of technical positions he'll indicate on what we consider reasonable approaches to tectonics and seismic issues. And in there, you will note that our approach to investigations for seismic matters is very, very similar to Part 100 requirements. In fact, it even referred to the investigation of Part 100 in the early drafts of it. But, recognizing the unique aspects of the repository which I'll touch on in a moment, we removed the direct reference because of the complications involving capable fault zone.

For all of the above, neither our regulations nor our regulatory guides, I believe, specify any minimum set-back distances or any minimum requirements to avoid faults.

To be sure we have discouraged siting on fault zones, or in the immediate vicinity of fault zones, just because of the regulatory complexities. That is consistent. We feel that fault zones should be avoided for the repository.

Power reactors are not located near capable faults just because of the difficulty in proving that their complex designs and such can meet the appropriate regulatory requirements. But this is not because the rules specify minimum separation distances from faults.

There are unique aspects of the repository though, in terms of the underground components and the post-closure requirements. As you all know, the EPA standard requires license periods in the range of 10,000 years, versus the decades that we are more accustomed to in surface facilities. And there are attendant difficulties such as model validation. We have no experience in trying to validate models that predict out to these ranges. Not in the normal sense, anyway. And we don't have a good handle on how to entreat in a licensing environment, predictions of future states of nature. These are unique problems that require a little bit different approach regulatory speaking, but I don't believe that we actually have contradictory rules and regulations at this stage.

I was asked yesterday afternoon by Clarence Allen, if I disagreed with any of the comments in the Dave Tillson paper. At that time I indicated that I didn't see any problems with the direct quotes of NRC, but I had questions about the conclusions. I looked it over a little more closely last night, and I did note one area that may be what I would consider an inaccurate comment, and I would just like to read on top of page nine of his paper a sentence.

"However the NRC and its 10 CFR Part 72 Regulations for non-reactor nuclear facilities including Managed Retrievable Storage Facilities, incorporated Appendix A siting criteria including the fault exclusion criteria."

And as I have indicated I believe earlier here, I don't think we have the exclusion criteria built into the rules, so although Part 100 was referenced, it does not necessarily include an exclusion criteria.

I do agree with the conclusions that the repository is a unique one-of-a-kind facility and I think we are going to have to take little different approaches. We are attempting that as Keith will indicate in our technical positions that we have planned. And, in the primary efforts within the staff now on iterative performance assessment techniques, develop skills and see if the regulations can be implemented in a licensable way.

The bottom line is DOE site investigations will have to determine whether or not Yucca Mountain can be licensed. I hope that our regulations are not contradictory in trying to reach that goal.

Thank you.

DR. ALLEN: Thank you, Ron.

I realize that your comments may also stimulate some response, but I would like to put that off until this afternoon if we might, in order to get on with the morning's schedule.

The first speaker this morning is David Schwartz of the U.S. Geological Survey. He has been very active in field studies of "active" faults throughout this country and elsewhere in the world.

Dave.

MR. SCHWARTZ: Thank you, Clarence.

I think most of you probably know me. I am with the USGS in Menlo Park. I guess I have been looking at active faults around the world for about the last 18 years.

In October I was on a field trip in Idaho with Leon, and we were standing at a trench. I said something about faulting repeating in the same place. Leon said, well how do you know there can't be a new fault. Then we got into an interesting discussion. I thought I had left it in Idaho, but about a month later I got a call from Leon and he said there was going to be a workshop in Irvine and would I mind coming down and continuing the discussion of new faulting. So, I said fine.

Of course, when anything is two months in advance, you can say okay. Then we came close to the meeting and I had to get serious about what I was going to talk about.

What I would like to do today is really based largely on my experience looking at faults in the western

U.S. and in other places, run through a number of examples of looking at the idea of the repeatability or non-repeatability of faulting in the same place during successive earthquakes.

In the examples that I will show, I have either actively worked on or visited all but two of the examples and those are from Central Australia, but I think they are kind of relevant to what we are talking about.

To sort of kick things off, first of all I would just like to point out that I am going to be telling you about primary tectonic faulting that is seismogenic, coseismic rupture. And there are a number of things that I think is important to keep in mind when we are doing this, when we talk about repeatability of faulting, there is a scale concept, largely in space, but also I am going to introduce the idea of time a little bit.

Certainly, when we look around the world, there are many major long-term zones of crustal weakness that have been reactivated through a variety of different tectonic regimes that have been convergence boundaries and have turned into strike-slip boundaries, that have turned into normal fault zones. So that is sort of a large scale type of reactivation and repeatability.

We are probably more interested in the math scale where there is sort of a general correspondence between a surface rupture with one style of faulting and perhaps preexisting bedrock. And then we get down to the outcrop scale where we have a fault trace here and three meters away we have a fault trace here and there is another event. Is it going to follow this trace, that trace or break through new rock?

These are some ideas that I will hit on as we go through the series of slides.

I would like to start off with the Lost River Range in Idaho. In 1983 the magnitude 7 Borah Peak Earthquake; what I am actually going to try to do in assembling the slide is to use examples of faults that produce the kinds of displacements, the range of displacements and magnitudes that we might expect in the Yucca Mountain area.

We are going to take a look at the 1983 surface rupture. If you lower the lights a little more, everybody can just kind of dose off.

Here is an aerial view of Double Spring Pass and I picked this out because this is in many ways typical of the fault zone and it is a very complex zone of faulting that you can see there is a large graben developed here. These are 12,000 year old alluvial fans that have been displaced in 1983 and by one prior event about 6,000 years ago. You will notice the main fault scarp, the antithetic scarp bounding this graben and then lots of small displacements in between. When we look at that in detail on the ground, this is what you see. You are looking at the main fault here in shadow. This is the main, the free face from 1983. This beveled surface on top is the degraded scarp from the 6,000 year old earthquake. And, I'll point out, here we have the faulting occurred in exactly the same spot in these two events.

You'll also notice that the size of these are the same. It is a beautiful example of a characteristic earthquake, repeating of the same slip in the same place during successive earthquakes.

We will stand here and we'll look back a the other side of the graben. Here is the main antithetic fault. The scarp is already degrading. But, if you look, there is a beveled surface on top which flattens out here and this is a scarp of a 6,000 year old earthquake exactly in the same place.

Let's look at a little more detail. Shortly after the earthquake we excavated a trench across here. This is a view down into the trench. It's a large horst block. This is small graben within the larger graben. This is Tony Crone from the Survey in Golden. This is very interesting because Tim Hait in 1976 had excavated a trench prior to the earthquake. So, what we wanted to do was to compare what happened before and after.

This is a log of Tim's trench purposefully put in reverse; you notice all the faults. Here is a schematic of our trench log and you can see the complex zone of deformation, the main scarp, the main antithetic scarp here, many more small displacements. Those color-coded in orange represent minor traces within the larger graben that slipped both 6,000 years ago and in 1983. So, even on sort of the outcrop scale, we have repeatability of smaller displacements in the same place.

DR. ALLEN: Were there no examples where it broke recently, but not 6,000 years ago?

MR. SCHWARTZ: The only possibility is these are fractures without any displacement. These are fractures that occurred in 1983, without any displacement.

That is an example for fault for which have historical rupture. Let's take a look at another normal fault. This is the Wasatch, in an area just south of Salt Lake City, Little Cottonwood Canyon. I picked this particular slide because, here the trace of the fault zone is very complex. There are, as you can see, many parallel and en echelon scarps with large graben developed. Here is a view of that area from the air. You can see the large main fault. It cuts across these 18,000 year old moraine in a series of very complex parallel scarps and large graben developed through here. In the next slide I will be standing here looking back towards this.

Here I am. I am standing on top of the main antithetic scarp. This is a big graben in here. Obviously the people in these condos don't care about the repeatability of faulting.

Some lovely homes sitting on tope of the main scarp for a better view down the valley. You can see the series of parallel traces and here is the large antithetic scarp forming this big graben. Now, we have not had a historical event here. But, clearly this topography is built up by repeated slip in exactly the same place. We have trenched these and you can see one for one correspondence in the location of the repeatability of successive earthquakes.

Just south of that location is another point where we have spent a lot of time looking t the fault. It's a place called Dry Creek; one of the probably 10,000 Dry Creeks around the world. And this is a series of about five parallel scarps, a very complex zone. We have trenched these; we have profiled these. Let's just show a log from this trench over here. These represent scarps from two earthquakes. In each of these events, each of these scarps was reactivated. So, even when we had the zone of complexity, the zone of complexity is repeated. Here is just an example of that trench log. Here is the main fault slipped here during two events. We are able to date buried soils. We have two earthquakes about roughly at a 5,600 years and somewhere around 1,400 years in here.

Another part of the Wasatch you see slightly similar but slightly different relationships. This is at Kaysville, just north of Salt Lake City. Here is the main scarp. There is a large graben in here. It has been built up by repeated faulting in the same place. But, when we look in detail across the graben, we can see some variability. This was our trench. You can see the size of the main scarp. Actually I used this slide, some of you might notice and might be able to recognize this person. This was taken when he was still able to do this. This is Bert Swan.

We spent a lot of time logging this trench. Here is sort of a large view of what this graben looks like with back tilted deposits. In detail this was the log of the trench. This was actually our first trench across the Wasatch. Here is the main fault. We see at least three events in this very well-defined narrow zone. But what I want to point out are these red lines running through here. All of these little faults formed only during the most recent earthquake. So, in a sense, these are new faults. These deposits were here for three events, but only during the most recent event did whatever structure was below the graben work its way up and finally break through. This is young unconsolidated deposits.

DR. ALLEN: So, the implication is, that maybe in the underlying materials those were not new breaks.

MR. SCHWARTZ: Exactly.

DR. ALLEN: Okay.

MR. SCHWARTZ: Let's leave the U.S. for a second and go visit a normal fault in another part of the world. I have been involved with a project in Italy helping the Italians develop their skills in paleoseismology. I had the opportunity to help them put in the first trenches across the normal fault.

What you are looking at here is the surface rupture from the 1980 Irpinia earthquake. This was in magnitude of 6.9. It killed about 3,000 people. There was about 35 kilometers of surface rupture with displacements up to a meter. We are going to look at a site right here just north of the town of Colliano at a place called Piano di Pecori. A wonderful place to do field work in the active trace of the fault. It is just on the other side. This town was actually very heavily damaged in 1980 and they are presently reconstructing it.

Here is the site, here is the part of the 1980 scarp. At this point there is a brittle rupture with about 70 centimeters of displacement. And when we trace this to the self, we are able to trace it to this location where we spent most of our time.

I would like to point out the following: If you look at the surface here, you can trace it along to about this point, you see a little inflection it kind of steps up and flattens out. Well, the brittle scarp, the brittle deformation over there gradually changed to a warp. So at this point, the surface displacement in 1980 was about a 55 or 60 centimeter high warp of the surface.

When we look down below the surface, you can see this sort of orange feature coming up like this, this orange slope. This is weathered limestone. What this represents, this is the buried fault scarp which sits directly below the surface warp.

Here you can see a series of light colored deposits. These are lake deposits, little lacustrine deposits that have all lapped onto the scarp and been sequentially warped each time there has been an earthquake. You can look at that in a little more detail.

Here is the buried fault scarp; the surface one on top. And these deposits are laid down and there is an earthquake which warps them and there is a series of unconformities that are developed within here. We have been able to actually refold or unfold this and we can work out five earthquakes in around the last thousand years. My point is here, that at this particular place, even though we didn't have a brittle rupture we had a warp, the previous five earthquakes were the same style of deformation. These were all warps in exactly the same place with roughly the same amount of displacement. So, we are getting repeatability of the style and the amount of displacement in the same place during repeated earthquakes.

Here is just an example of the log from that trench. In addition to the warping, there are minor little faults that have been developed along the zone of extension as this buried scarp continues to warp during each successive event. So, again, repeatability in the same place.

The last normal fault I'll talk about is down in Peru. It doesn't make a difference where you are, normal faults behave the same. Look at the Cordillera Blanca fault zone which is hidden above the top of the screen a few hundred kilometers north of Lima. This is the surface trace of a fault. It is about 250 kilometers long up in the high Andes. Here is a location where we can see lots of different aspects of reactivation. This big face, which is close to two kilometers high, is basically the exhumed or bedrock trace of the fault. The repeated slip has raised this bedrock face and the young trace is right along the bottom.

Now, it is almost impossible, if you were right here you could see a little break in slope to give you some sense of scale. We are going to look in the next slide at a point right over here (indicating), and this is that scarp. We could be in Utah, we could in any number of other places. It is a 23 meter high scarp, again formed by repeated slip all in the same place. Here is another example of that. Here is the scarp. It has been buried by debris flows. And up in here you can see a little terrace. This is a tectonic terrace that formed during an individual earthquake. We have done some trenching and again all of the deformation is confined to a very narrow zone that is built up this topography.

One other point, on a larger scale old structures tend to control the location of newer structures. Here is another location where we can see actually the fault plane and bedrock, this surface. And when we get up closer, this is the edge of a large granodioritic intrusion of pliocene age. When we get up closer to the edge, you can see there is this strongly foliated ductile deformed margin. This is the margin of the batholith, and within that margin you can see these brittle shears. These are the young normal faulting planes. So all along this part of a fault zone, the margin, the older zone of deformation in the pluton, controlled the location of the younger normal faulting.

In this last slide of the Cordillera Blanca, just another example of repeatability in the same place. This is roughly a 14,000 year old alluvial fan that grades out into younger lake deposits. This surface is roughly 16 meters of displacement across here. As you take this scarp out into the younger deposits, the scarp height decreases and you can follow the same trace out onto the horizon where we have a much larger graben developed in older moraines. So again, repeatability in the same place.

Let me move away normal faults for a minute and go to strike-slip faults, since perhaps at the site we are seeing a combination of the two types of movement. Art Sylvester sent me this slide. Art is at Santa Barbara. He is the editor of GSA Bulletin. He said, David, I flew over here and I can see your trenches in the slide. He is a better man than I am, because, I can't see the trenches. But, this is the San Andres Fault running right through here just south of Palmdale, California at a little place called Little Rock. We have been looking here trying to develop information on the slip rate along this part of the San Andres. So, we have a series of trenches in through here, in fact you can see this stream comes and makes a right bend and goes out through here.

This is a map of the site. These are one meter contours for scale. I am not going to go into any of the details, but these are some things we think might be piercing points on the fault. What I do want to point out are the two traces. They are both geomorphically well expressed and expressed in the trenches. There is a vertical trace through here and there is a dipping trace through here.

Now in the next two slides we are going to look at this trench, a view and then a log. Here is a view. This hump is between the two fault traces. So the vertical trace is here, the dipping trace is on the other side. This is a pressure ridge that is built up by repeated movement. These light colored deposits are pond deposits that have been stuck behind the scarp. And like Italy, actually, they have been sequentially warped. Here is a log.

So, what we're looking at is the main San Andreas that ruptured through here in 1857 with perhaps four meters of slip. The faults that you are seeing here, the deposits are about 1100 years old. The faults represent anywhere between 20 and 30 meters of right lateral slip, five to seven large earthquakes. The deformation is basically confined to these two very narrow well defined zones, which, just below the surface will probably coalesce and become even a narrower zone with depth. And there has been a little bit of warping off of it.

But again, repeatability of large slip events with large amounts of displacement in a very, very narrow well constrained zone. What is interesting here is that we actually were very close to bedrock. There is just a very thin veneer of alluvium over the bedrock.

All strike-slip faults aren't always that nice and neat and you can find situations where faulting is much more complex and spread out over a broader zone. This is a trench excavated across the San Andreas at Wrightwood, California, which is just north of San Bernadino. If you follow the trench down, you might be able to see sort of a little antiformal shape ridge here. The main trace of the San Andres which ruptured in 1857 and 1812 and 1655 and back on down, runs through here as a broad zone. And you can actually take the trench out and you come up to cross a scarp that runs out through here. This is largely a secondary normal fault associated with the San Andres.

This is just some representative trench logs. This is across the secondary normal fault. This is across the main trace. You can see here we have very complex zone of faulting. But, this is all in unconsolidated sediments, peats, which at one time were very wet. And most of this deformation actually occurred while this stuff was very wet, this was an old swamp. But you can run into zones of complexity to a large degree that are controlled by the materials near surface.

Another strike-slip fault is the Motagua Fault. Here we are. We are looking at the surface rupture from the 1976 earthquake. Roughly 240 kilometers of faulting with a magnitude of 7.5. That is George Plafker in the background trying to measure one meter of left lateral offset on this cactus fence. I spent a lot of time down here.

DR. ALLEN: It's Guatemala. You haven't stated that.

MR. SCHWARTZ: Excuse me?

DR. ALLEN: It's in Guatemala, right?

MR. SCHWARTZ: Guatemala, yeah.

DR. ALLEN: You hadn't stated that with this.

MR. SCHWARTZ: Oh, I'm sorry. Yes, Guatemala.

I spent a lot of time on the Motagua and one of the really interesting locations is a series of terraces that is cut by the fault. This is the oldest terrace and we are working down in elevation. You can see that as you move down to the younger terraces, the displacements decrease, which is what you would expect. What I would like to point out, you are looking at about ten thousand years of history here in these terraces. Look how narrow the surface trace of the fault is. It is really confined to a very, very sharp zone. Then take a look at this trench excavated right over here, and here is a schematic of the trench. Here is the main fault zone where it ruptured in 1976. There is a little bit of older faulting off to the north and there is some warping off to the south. But, basically, this zone represents 23 meters of displacement. Almost all brittle displacement; almost all occurring here. And you notice, as we start to go down, we are even getting narrower. Here is a view of that fault, the Motagua fault and the trench; roughly a meter at the surface and narrowing down in depth. As it goes into the bedrock, it is just going to be a very, very skinny zone.

One last strike-slip fault, and this is the 1986 Superstition Hills earthquake. A fascinating event for a number of reasons. There are really two earthquakes. There was a magnitude of 6.2 on which surface rupture occurred across a zone roughly a six or seven kilometer wide zone of northeast trending, left lateral strike-slip faults. Then about eleven hours later there was a 6.7 on the Superstition Hills fault and there was roughly 22 or 23 kilometers of right lateral faulting.

This is one of the, at least for a large part of its length, one of the cleanest, neatest strike-slip faults you will ever see. It looks like somebody came down and just took a knife blade and cut it across the surface. Here is an example of the surface rupture. You can just see how clean and narrow and well-defined this is. It followed a preexisting zone of bedrock. It had been mapped before by Bob Sharp. It also has some geomorphic expression in places, and again, it just looks like a sidewall curve. This is how neat the fault was and it was roughly 70 centimeters of coseismic slip that occurred.

Linvall and Rockwell and others spent some time looking at evidence for the pre-1986 event. They think it occurred somewhere around 300 years ago. They measured a number of features and one of the more fascinating is this; this is a little dune that had formed and been, a little sand dune that had formed around some brush and had been offset 70 centimeters in the earthquake. Next to it was another dune that was offset 140 centimeters. Double the amount; the two events. And again, notice how this slips is just repeated in exactly the same narrow spot during these two earthquakes.

Let's go back to the northeast trending faults for second because I think they are really relevant to this problem at Yucca Mountain.

In '86, these red traces represent where the surface ruptured. The darker traces represent faults that had been mapped. This is an area of Pleistocene Brawley formation, and Bob Sharp had mapped these faults in the bedrock. The absence of faults out here doesn't mean that they don't occur, but this is an area which is covered by some eolean sand and some younger deposits. But you get a feel that there is a broad zone of deformation in the bedrock. Now, in '86, not every one of these bedrock faults was reactivated. But, the ones that did occur followed preexisting bedrock structures. So, we had a broad zone with a lot of choices. Some of them were reactivated. The investigators really looked carefully to see if there were new faults and in all of the literature on this event, everybody says each of these faults is part of this Elmore Ranch fault zone, occurred along a preexisting zone of faulting.

Before I get to this, I would like to move to Australia for a second. Maybe after this talk I'll want to move to Australia for good.

In 1986 there was a magnitude 5.8 earthquake at Marryat Creek. In '88 a series of earthquakes up at Tennant Creek. These are very interesting, because they are some of the few examples of surface faulting events within stable continental interiors.

Tony Crone and Mike Machette from the Survey in Golden had the opportunity to go over and excavate some trenches across the surface ruptures. They were interested in two things. Number one, was the resident of preexisting Quaternary faulting on these features, and number two, what was the bedrock structure like below. So, this is a map of the Marryat Creek rupture. These are thrust faults. It had up to about 67 meters of displacement. This is where they excavated one of their trenches. Here is a schematic log of that trench. Here is the '86 scarp. The red represents fault planes that moved in '86. The orange represents fault planes, older fault planes and basically these are Precambrian granites.

Now in talking with Tony, he said they found no evidence of any other Quaternary faulting. It doesn't mean there wasn't any, but there is nothing recorded at this location. And in fact, this could conceivably be a first-time rupture of this fault zone in the present regime.

DR. ALLEN: What do you mean in the present regime?

MR. SCHWARTZ: I don't know how long this has been a stable interior. These faults were likely formed during emplacement of these granites in Precambrian or to a large degree they may have been. This may be the first time that this has ruptured as a thrust fault, a seismogenic thrust fault.

DR. ALLEN: Except, clearly, it was along the preexisting break.

MR. SCHWARTZ: Okay. That's what I want to get at. Regardless of how many events we've had, it occurred along a preexisting, a recognizable preexisting zone of deformation juxtaposing different bedrock types and containing preexisting faulting. So, even if this is the first time that it slipped as a thrust in this regime, it occurred along existing faults.

Then we go up to Tennant Creek. Another fascinating series of events. There were actually three earthquakes here. The first one was a 6.3, then about three and a half hours later there was a 6.4, and then later that evening there was as 6.3.

The first 6.3, during that event, this part of the fault slipped. During the 6.4 this part of the fault slipped. During the last event, this part of the fault slipped. Also, these two segments of the fault dipped to the south and this segment of the fault dips to the north. So it is a very complex, structural setting. They put in a series of trenches, and I'll show you a trench located right here at site 2. This is what they found. Here is the 1988 faulting; here is the 1988 fault scarp based on thicker deposits of alluvium on the down thrown side. They infer that there may even have been a preexisting scarp here. The point is that the 1988 faulting followed preexisting zones of faulting in the older bedrock.

Now, interestingly they tried some dating and they have done TL dating at the base of the sands, and it is 60,000 years old. So, this is the first event in at least 60,000 years; maybe considerably longer.

Even if these are very rare events, which they appear to be, they are following preexisting zones of weakness. Let me get on to the last few slides.

My last example comes from the Sierran Foothills in California. I think many of you sitting in this room were involved in this work in one way or another. This evolved out of the Auburn Dam study and work for PG&E looking for potential reactor sites in the Sacramento Valley. And, in the mid-70's we went in and took a look at the Sierran Foothills for the siting purposes. The Sierran Foothills is a zone of Mesozoic compression. It is a very, very strong structural grain which is dominated by northwest trending bedding, northwest trending foliation and major northwest trending zones of brittle faulting in the older basement rocks. Superimposed on that is late cenozoic extensional faulting. All of these dots of different colors, they are different types of evidence. But, they represent locations where, in our work we are able to show evidence of late Cenozoic faulting and you can see the general relationship to the older Mesozoic structures.

Here is a little more detail of some of these previously mapped bedrock faults and the locations of the younger superimposed normal faulting. I show a slide from up at Oroville, up in Spenceville and then down here at Auburn Dam.

This is what started it all, this little crack in the ground. Maybe the kind of slip you might find at Yucca Mountain. This is the surface rupture from the 1975 Oroville earthquake the magnitude at 5.8. A few centimeters of vertical displacement and maybe just a fraction of right lateral. We were able to follow this into bedrock with a very distinctive preexisting zone. You can see here that this particular soil horizon was displaced. That got everybody excited and we said, well maybe we can use this as a basis for looking around the rest of the foothills to see if we can find evidence of other types of features. And we did.

This is a place called Spencerville. I remember Clarence spent quite a bit of time at this trench. This too. Bert you keep getting into all of these photographs.

Here is a place where we have young colluvium an alluvium. You can see this well-developed fault plane in here. Here is a log of that trench. What I really want to emphasize is that we have two different types of Jurassic bedrock. We have a broad shear zone in between. This is sort of older shearing by and large. The young faulting is defined by this very thin zone of gouge or paper thin plane which is within or near the boundary of this preexisting zone of faulting and can be traced up to the surface.

Well when you talk about structural complexity, I think there is probably, well I mean, Auburn Dam comes close to being one of the most complex places you'll ever want to look. And you can see there are lots of different types of structures that we had to deal with in trying to come to grips about surface faulting potential; where it was going to occur, how much was going to occur. Actually at that time we actually had the audacity to say where we though renewed faulting would occur within this larger mass of preexisting structure.

What we did as we looked throughout the foothills, we looked for late Cenozoic deposits such as this Mehrten debris flow, which was cut by normal faults. We were able to trace the normal faults down into bedrock. This is what we often saw, preexisting zones of deformation with gouge and the young fault, the reactivated fault occurring at the boundary or within the older zone of deformation. And that is sort of represented schematically by this, where we can see a regional foliation which is deformation of one regime, perhaps at the late stages. A crenulation cleavage was formed with brittle shearing, cataclasis crumpling and gouge formation. And within this narrow zone of older bedrock faulting, these were the preferential places for the young faulting to be reactivated.   I think it is basically the same point that I have made throughout the series of slides.

So, let me try and sum up and see if there is any time for questions or we can keep that for later.

One of the questions, I think it is very clear that certainly from my experience and I think a lot of geologists would agree that future events are going to occur in the same place where they have before. Is there anyway that we can quantify this? I think that is really kind of a difficult problem.

Back in 1979, Doc Bonilla at the Survey did some work for the NRC on various aspects of faulting. One of the things that he talked about was the relationship of surface rupture to preexisting faults. This is sort of a widely cited USGS open file report.   Just let me read what he wrote. At that time back in '79, he had 108 examples of historical surface rupture. He went through the literature and he tried to see if he could really understand the relationship between historical rupture and preexisting structures from what was in the various papers.

It says: "Of the main faults in 108 examples of world-wide historic surface faulting on land, 91 percent occurred or probably occurred on preexisting faults; 8 percent are indeterminate in this regard based on available data; and, one percent, that is one example, this was Inangahua in New Zealand of a magnitude 6.2 back in 1968, apparently occurred where no fault existed previously. In a few other cases the main or subsidiary faults apparently penetrated unbroken materials to a limited extent. The correspondence and position of the historical ruptures with prehistoric ruptures has ranged from exact to approximate and emplaces the surface rupture as elected to follow one of two or more available, pre-existing faults."

Well since that time, I sat down and I came up with another 26 or 27 historical ruptures, so that the data set is probably up into the 130's now. Somebody, I am sure, can spend some time going through all those and seeing if there are other suggestions of new faulting.

Out of this actually, the Inangahua case which he calls the one example, I talked with Doc last week and he said since he wrote this he talked with the authors and they are really not very sure that the conclusion they made was really accurate. So, that would be kind of indeterminate too. So, basically on a sort of a semi-quantitative basis, we can see that it is just not a very common occurrence.

Let me sum up with these last two. With regard to new faults, I think that in a sense new faults do occur and they are most common in a couple of examples I showed, in unconsolidated deposits and particularly where preexisting slip surfaces have been buried and there is a long recurrence interval relative to the age of the deposits. You may have no preexisting geomorphology and then something pops through.

I think you get new faults in particular where you have refraction at material interfaces. So, if you have alluvium over bedrock and rupture plane comes up and hits that interface, it can refract and go in various places that we may not expect during repeated ruptures. I sort of mention this idea for some faults we may have no prior expression because the faulting has been buried, but still there is an existing fault at depth.

I think there are some possible examples in the literature of coseismic rupture propagating into unfaulted bedrock. I think that you certainly have to expect say, for strike-slip faults. I mean, they come to an end. There is a finite end. And, they move laterally. So, over time that rupture surface is going to propagate laterally.

It may take advantage of other preexisting structures conceivably at times; it may go through new rock. I don't think you can rule out these possibilities. But, that generally is not what we see in most surface ruptures. And you always have to remember that this takes less energy to move existing planes, especially when they are properly oriented in the stress field, than to break through new rock.

So, the final sort of Schwartz's rule of thumb, I'd like to end with this, that I think I'd say that the collective geologic experience is that future slip on faults is most likely to occur along fault claims that have been active during the present stress regime or on planes that are favorably oriented with respect to it. I think there is always the possibility that new faulting will occur in previously unfaulted bedrock. You can never rule that out. And I think it really largely takes place at the very ends of propagating faults.

And right now, I don't think there is any real quantitative basis for saying what the likelihood of new faulting is, but qualitatively, I think based on our experience, I think that is exceedingly low.

That is where I will end it or open it up for any discussion or if you want to save that for later.

DR. ALLEN: Thank you, Dave. Are there questions from the Board or consultant staff?

(No audible response.)

DR. ALLEN: From the audience?

Yes. David Tillson.

MR. TILLSON: This is David Tillson.

David Schwartz, somewhere back in history I recall you saying that you had some experience on a fault that was reactivated and had encountered an engineered structure. I think it was in Nicaragua; Managua. Can you relate what occurs when a preexisting fault is reactivated and encounters an engineering structure? What happens?

MR. SCHWARTZ: I think the one you are referring to is the case of the Banco Central in downtown Managua where there was the Tiscapa Fault ruptured up to the bank and hit the vault. The vault was much stronger than its surrounding pyroclastic deposits and the ruptures just went around it and went on its merry way. That is the example you are referring to.

MR. TILLSON: Do you have other examples of what happens?

MR. SCHWARTZ: I think that you can go back through the literature, you can look at any historical particularly strike-slip event. You can look at roads. You can look at places where pipelines have been crossed. You can look at where structures have been offset. There is a full literature. Things in certain places, you are amazed at how little happens to a structure, and others there is damage.

I haven't systematically made a search of the literature to try and categorize the styles of deformation. But, you go from very little to big surprises.

MR. TILLSON: So what you are really saying is that it is very unpredictable.

MR. SCHWARTZ: What's unpredictable?

MR. TILLSON: The effect.

MR. SCHWARTZ: I think the effect is variable, not necessarily unpredictable.

DR. ALLEN: Other comments?

John Whitney.

MR. WHITNEY: Do you have any examples of reactivation along faults where there has been some rotation of the least principal stress in an area or the style of faulting has changed over time?

MR. SCHWARTZ: You mean individual structures?

MR. WHITNEY: Right. Individual structures.

MR. SCHWARTZ: I think a lot of the ones I showed actually fit that description. The last series, the faults in the Sierran Foothills were formed by regional compression at depth in the Mesozoic and are now undergoing east-west extension. So, a totally different stress regime acting on the same planes. And, almost all of these structures were at one time or another in the bedrock, some sort of other type of Wasatch went through an older perhaps thrust period and that zone was active from the Paleozoic. There are a lot of examples of individual faults that have gone through one, if not more different styles of deformation over the history.

DR. ALLEN: Certainly, a famous example from geologic text books is the Bright Angel fault in the Grand Canyon where the Precambrian rock in the inner gorge is offset one way vertically and Kaibab limestone at the top of the rim was offset the other way vertically.

MR. SCHWARTZ: That's a beautiful example of that. It is very common.

DR. ALLEN: Leon Reiter.

DR. REITER: Just two questions. I think the example of the Banco Central brought up that was a key item in the case of the NRC hearing on Vallecitos Reactor. There was a concern about a fault going through the reactor. That was one of the things that eventually led the Board eventually to rule in favor of keeping the reactor there. Unfortunately, the ruling took so long the company went out of business.

DR. ALLEN: With a board of experts sitting at a table.

DR. REITER: Yes.

But, Dave, one example which has been cited often as possible new faulting is the Meckering Fault in Australia.

Could you comment on that?

MR. SCHWARTZ: Yes, I will comment. It is not a new fault. There is actually a preexisting scarp there. There has been some trenching and it shows evidence of prior faulting.

DR. ALLEN: Bob Kennedy.

DR. KENNEDY: I would like to comment on Dave Tillson's question as to whether this was highly uncertain or whether the behavior can be predicted. There are several examples of faults going across vault type structures near the ground surface where because of the strength and the stiffness of the vault type structure, and the lower strength of the ground surface, it went around.

There are also cases where you have brick walls, un-reinforced masonry walls or wood structures where the surrounding soil is stronger than the structure and it goes through the structure and breaks the structure.

For structures like hot cells in soil media, it is in my opinion nearly certain to go around as long has there has been proper engineering design.

Now at Nevada Test Site we find, and I think that is not surprising, you cannot build a structure at a fault crossing that isn't going to still allow the tunnel to have to displace the fault crossing. You can't stop the fault crossing. You can't stop the fault moving. But we have built structures that have withstood fault movement. In fact when we have critical cabling that crosses faults and blocks and joints and we have had situations where there has been movement of over one meter, we can protect that cabling by putting it in a thick-walled steel pipe and grouting it and filling the steel pipe with grout, we have never had a cable break. It just simply, the pipe deforms and this is in rock. The pipe has enough deformation capability and we protect the inner cables for over a meter of fault movement.

So, it is a matter of which is the stronger and more ductile system. If you build a structure that is stronger and more ductile, it will survive these movements. I think it is very predictable.

DR. ALLEN: I think we probably ought to move on. We are a little bit behind schedule. Thank you very much, Dave, for a very clear presentation.

The next presentation is by Keith McConnell of the Nuclear Regulatory Commission, the Identification of Faulting and Seismic Hazards at a Geologic Repository.

MR. MCCONNELL: Thank you, Clarence.

My name is Keith McConnell. I am with the U.S. Nuclear Regulatory Commission. I am here this morning to give the NRC staff's perspective on the identification of fault displacement and seismic hazard at a geologic repository. And by necessity, that is a regulatory philosophy or perspective.

The perspective that I am going to give is formalized in a staff technical position that is now in draft form and is soon to be in final form on the investigations to identify fault displacement and seismic hazards. And that staff technical position is going to form the basis of my presentation today.

Now the staff technical position on Investigations to Identify Fault Displacement and Seismic Hazards at a Geologic Repository, is one of a series of staff technical positions that we have under consideration or development at the present time. The one we are going to speak on today, the upper most one, investigations to identify the hazards, is followed by a companion staff technical position, the Analysis of Fault Displacement Hazards and Seismic Hazards at a Geologic Repository. Now the separation of these two has caused quite a bit of consternation among our reviewers of the initial STP.

However, we base the separation on two things. One, the controversial nature of the topic that we are dealing with, we felt that in order to get something through in an expedient manner, we had to take a small bite at first. Second, the split also reflects a split in Part 60, 10 CFR Part 60. Part 60 has requirements that relate to the investigation of potentially adverse conditions at the site, and it also has requirements that relate to meeting performance or the analysis of those potentially adverse conditions and whether you can meet the performance objectives. So, there is a basis for the split.

A third staff technical position under development at the present time with the staff is the use of Tectonic Models in Performance Assessment. Then, there is a fourth technical position under consideration which is, the Application of Fault Displacement Hazards and Seismic Hazards to Design.

A question frequently asked of us is why is the staff taking it on itself to develop a staff technical position for the Investigation of Fault Displacement and Seismic Hazards at a site? And basically, it is because in our staff review of the site characterization plan that the DOE published several years ago, we identified what we thought were very significant concerns with respect to fault displacement and seismic hazards as to whether the investigations outlined in the SCP were sufficient to fulfill the Part 60 requirements. Again, these are the requirements that relate to the identification and investigation of potentially adverse condition at a proposed site.

As we saw yesterday, site characterization has begun at Yucca Mountain in earnest. While we have no objection to DOE starting site characterization, the staff concerns have not been resolved at this point. We felt that it would be inappropriate for us to sit around and wait until we received a license application to address the issues or what the staff felt was sufficient and necessary to meet Part 60 requirements.

To get back to one of the questions that Leon had yesterday, in this pre-licensing stage of the process, the staff feels that the most appropriate mechanism or issue resolution is to go through and address the SCA concerns that were identified in our site characterization plan. At the same time in Bob Bernero's cover letter where we emphasized those issues, we felt were of highest priority, if DOE has a consideration where they think we should change those priorities, they could come to us again in the form of the response to the SCA to change the priority.

This slide is to just give you some idea of the chronology of development of the staff technical position on investigations to identify fault displacement and seismic hazard. Basically the main point I wanted to bring out was the stage where we are now. We are in this area on December 17th and 18th of last year. We met with the Advisory Committee on Nuclear Waste to discuss the staff technical position. And my presentation today and the aspects of the staff technical position that I'll be discussing, do reflect some of the changes that were made in response to comments made at the Advisory Committee Meeting. We hope to issue the final STP sometime in the next couple of months.

Now the objective of the STP is to provide an acceptable approach to the collection of sufficient data related to fault displacement hazards and seismic hazards for both pre-closure and post-closure performance assessments, basically putting on the table what the staff considers is necessary and sufficient information to identify those adverse conditions that relate to fault displacement and seismicity of at a geologic repository.

What's required to meet Part 60 requirements? The purpose is again to describe an acceptable approach to meet 10 CFR Part 60 requirements for investigation of fault displacement hazard and also to provide one path, although there may be other paths, to the resolution of the SCA concerns with respect to those issues.

The approach adopted in the staff technical position has several aspects to it. One, that it does benefit from the past regulatory experience with reactors. It does not ignore the experience gained with the implementation of Appendix A, to Part 100, in that it does use explicit criteria for identifying fault hazards. However, there are very clear regulatory and technical reasons why Appendix A to Part 100 is not applicable to a geologic repository. From a regulatory perspective, Part 60 does not refer to Appendix A to Part 100, therefore there is no specific requirement that DOE needs to address the requirements in Appendix A of Part 100.

From a technical standpoint, Appendix A concentrated on the seismic hazard of nuclear power plants. It did not necessarily put the emphasis on fault displacement hazard that is of a concern with respect to a geologic repository.

The STP uses deterministic criteria, not unlike Appendix A to Part 100, to determine which faults require detailed investigation. However, from our perspective we do recognize that there is utility in using probabilistic techniques in determining which faults are of concern outside the controlled area.

Finally, the STP recognizes the need to perform iterative assessments and it also recognizes that our crystal ball is not completely clear, and that there may be things that come up in the iterative performance assessment that may require additional investigations of fault displacement and seismic hazard.

The key provisions of the STP that again relate to some of our SCA concerns, the site characterization concerns, are one, staff technical position identified the entire Quaternary as the period of geologic time that should be considered with respect to identifying fault displacement and seismic hazard. It also provides a methodology and criteria for identifying and investigating those faults that are of potential concern to the repository. Again, this is the criteria that is parallel to the approach used to define capable faults for nuclear power facilities.

Also, it specifies that faults or fault zones previously removed from further consideration may need to be reconsidered based on the results of site characterization. In other words if your basic assumptions change as a result of some of your site characterization activities, you may need to go back and revisit some of these faults you have said that you didn't need to investigate.

Finally, the staff technical position recognizes that it is proper and prudent to err on the side of conservatism. In other words, there may be some faults that DOE will investigate and on further analysis may not be of importance to repository performance. It is better to err on the conservative side rather than risk overlooking something that may be significant, some fault or fault zone that may be significant.

What I would like to do now is to attempt to walk you through the position using this diagram that illustrates the position outlined in the staff technical position.

Basically there are a series of steps that need to be passed through, or gates that need to be passed through.

First of all, there has to be the definition of the geologic setting or at least the faulting and seismicity component with respect to the topic today of the geologic setting. There has to be an identification of the region to be investigated, that area for which faulting and seismicity could possibly affect repository performance. After having identified that region and with the existing data and the knowledge of faults at the repository there has to be some sort of screening mechanism about which faults need to be continued as candidates for detailed investigation.

Based on the requirements of Part 60 for potentially adverse conditions, which state that a potentially adverse condition such as faulting in a Quaternary is an adverse condition if it is characteristic of the controlled area, or based on that, all faults inside the control area are considered to be candidates for detailed investigation.

Faulting in a Quaternary is also an adverse condition if it occurs outside the repository only if it could affect repository design or performance. Therefore, those faults outside the controlled area that could possibly affect repository design and performance continue to be candidates for detailed investigation. Faults outside the controlled area, even though they may show Quaternary displacement, if there is no potential that they may affect repository design performance, then they do not require any further consideration or detailed investigation.

Now one of the changes we have made in response to comments from several reviewers is, we have basically changed our approach to naming faults and faults of concern to the geologic repository. Those of you who are familiar with the history of the STP know that we started out with tectonically significant fault. We were criticized for that term because it appeared to be prejudicial. We ended up then with susceptible fault which is a parallel to capable fault. We were basically criticized for the same reason. So now we've gravitated to basically categorizing fault levels. This is preliminary because there is again some discussion that calling things Category 1 could confuse things with Category 1 type structures in reactors. So it could be Type 1 or Category A or something like that. But it is going to be similar to this type of categorization scheme.

A Category 1 fault would be a fault that does not require detailed investigation. A Category 2 fault is a candidate. And, after we have determined what the candidates are, then we go through a third step where we identify those faults that require detailed investigation. They will eventually be Category 3 faults and then you go into the investigation of the faults and then the input into the probabilistic and deterministic assessments of performance.

So, just to reiterate, basically there will be three categories of faults. Category 1 faults do not require detailed investigation. Category 2 faults which are candidates and they have gone through the initial screening. Category 3 faults, faults that should be investigated in detail and we'll provide the basis for input into probabilistic and deterministic analyses of performance.

To provide some of the criteria for the various categories, and I'll describe some of these criteria in a little bit more detail later. Particularly, Category 1 faults are faults that are not subject to displacement and I'll discuss what subject to displacement in the staff views is in a few minutes. We are also looking at such that are of sufficient size such that, they will not affect repository performance or will not provide significant input into models of repository performance.

Category 2 faults are faults inside the controlled area. Again this is these two branches, faults inside the controlled area, and those faults outside the controlled area that are determined to be located such that, and are of sufficient size such that, they may have an affect on repository performance or may provide significant input into repository performance models.

Finally, Category 3 faults are faults that are determined to be subject to displacement and they have the potential to affect repository performance or provide significant input into models used to assess repository performance.

Now the key factors in determining what a Category 3 fault is defined in the STP as a two step process. First, there is a consideration of whether the fault is subject to displacement and then two and three here, a judgment whether it will affect repository design or performance or provide significant input. That is illustrated in this rather dark viewgraph.

Step 1 up here, again we have the candidate faults coming into this two step process. Step 1 is the fault subject to displacement, and then Step 2 an assessment of the affects of fault displacement on repository design and performance.

So, Step 1 is the determination of whether the fault is subject to displacement. A fault is considered to be subject to displacement if there is evidence of Quaternary displacement. That is the first block up there. In those cases where the Quaternary record is incomplete or unclear, basically middle block, then you should consider secondary criteria. In other words if the entire geologic record is not present for the Quaternary such as the Ghost Dance Fault scenario, then you should consider the secondary criteria in determining whether the fault is subject to displacement.

Another modification we have made since the Advisory Committee Meeting, is that if there is documented evidence that no Quaternary faulting has occurred, then the fault does not require any detailed investigation. But that does not relieve all responsibility for considering that fault, because, again, it is an iterative assessment.

Based on results of site characterization, you may need to go back and reconsider those faults that you have excluded from site characterization. But just to reiterate, the primary criteria is evidence in the Quaternary. If the answer is yes, then you continue on in the process. If the answer is yes to any of the other or any of the secondary criteria after you have passed through the first block, then again you continue on. But, you haven't reached fault Category 3, yet.

Basically, again an assessment of effects of fault displacement on repository needs to be considered before you determine which faults require detailed investigation. This is to address the point where you may have a fault that is a foot long, it may have Quaternary displacement on it, but it is insignificant. Does DOE have to do an extreme amount of detailed investigation to address that fault issue? And the NRC position is no, if they can show that it is not going to have a significant affect on repository design or performance.

I think we heard yesterday that DOE was proposing five meters of offset for those faults that might be of significance. I think if DOE plans to propose that formally, we would be very interested in commenting on it. So there is a second assessment of affect on repository design or performance.

Only after you have gone through those two steps do you have fault Category 3, which are those faults that require detailed investigation and would serve as a primary input into repository assessments of performance.

Now having said that, there are some questions that came up yesterday and have basically come up for quite awhile, with respect to whether the presences of what we called up until a couple of months ago susceptible faults in the controlled area or what we would not call Category 3 faults in the controlled area, would remove a site from consideration or would make it unacceptable in the NRC's eyes. It is quite clear from Part 60 that that is not the case. The DOE would have to demonstrate with reasonable assurance that the siting criteria, the design criteria and performance objectives in Part 60 could be met.

That is a very general statement. But, there is specific guidance in Part 60 related to adverse conditions. And again, fault displacement and seismicity are adverse conditions. With regard to how they should be addressed, Part 60 indicates that although you may have the presence of an adverse condition, as long as you can demonstrate, one, that it is balanced by favorable conditions at the site, or that it can be designed for, or basically that it can--I'll have to get the correct term. But, basically there are two steps to resolving potentially adverse conditions at the site. One is that you have favorable conditions and the other is that it can be remedied. The term remedied implicitly says that you can design for these adverse conditions.

From the NRC staff's perspective, the presence of Category 3 faults does not make the site unacceptable as long as you can demonstrate that performance objectives can be obtained and met.

I would like to skip down to the fourth bullet here. What Part 60 doesn't do is it is not specific about how you remedy the adverse condition. In other words, Part 60 contains no requirement for a set back or an avoidance philosophy as far as remedying the adverse condition. It is up to DOE to come up with the remedies of potentially adverse conditions if they exist at the site.

Now, being realistic about the situation and knowing that at Yucca Mountain fault displacement and seismic hazards may be quite pervasive in the controlled area. The staff has developed a philosophy as far as what it would expect from DOE to address these hazards and potentially adverse conditions. First, we would suggest that prudence suggests caution regarding design to accommodate fault displacement. I think it is kind of a motherhood statement.

Also, design for fault displacement must provide reasonable assurance of meeting of performance objectives. Again, you've got to meet the performance objectives.

Finally, if DOE does intend to design for fault displacement as seemed to be indicated yesterday, then they should come to the staff for early resolution of fault related design and performance issues. I guess that is basically as far as we want to go.

DR. ALLEN: Thank you, Keith. There have obviously been some very significant modifications since the time of the ACNW meeting that you have obviously been very busy.

Do we have comments from the Board?

Ed Cording.

DR. CORDING: In regard to the statement, the motherhood statement that you referred to, is it caution in deciding to embark on a design or caution in regard to the conservatism and what one puts into the design? And then in terms of design, there is a lot of different aspects that design could involve setbacks, it could involve things such as these vault-like type structure that tend to move or could involve things that accommodate it. It could involve an access drift that gets offsets and it is not near emplacement holes and things like that and you just go in and re-mine it. There is a lot of different aspects to fault movement in a large facility like this and I think those are things that certainly would be looked at assuming there are faults that have those possibilities anywhere in the repository.

So, I guess part of my question is, the statement seems to say, well it is not clear what the statement is really saying, but are you really saying that one should not be involved in designing for fault offset?

MR. MCCONNELL: I don't think we are saying that. I think the history of the agency has been to take a very conservative approach to design for fault displacement. I would expect that philosophy to continue. But, we've recognized that adverse conditions like fault displacement can be remedied. How those adverse conditions are remedied is up to DOE to propose to us, how they are going to remedy that adverse condition if it exists at the site. That is why we ask for early resolution. If they do intend to design for it, they must be aware of this conservative philosophy that the agency has taken and will continue to take. So they should come to us very early in this process to resolve those concerns. But, the burden is on DOE.

DR. ALLEN: Other comments?

(No audible response.)

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

Yes, Ron Ballard.

MR. BALLARD: I would just add in response to the question on caution is that the statute, you may recall, provides a three year license period. That leaves the staff with something like 18 months to do a review and prepare a safety evaluation report. Now, based on the experience we have had with faulting and such in the licensing for reactors, I think that the caution indicates that let's get these matters out on the table during this consultation period, long before we get to a license application review.

DR. ALLEN: Thank you.

Let us take a 15 minute break and reconvene at 10:25 a.m. and proceed with the program then.

Thank you, Keith.

(Whereupon, a 15 minute break was had off the record.)

DR. ALLEN: The next speaker on the morning's program is Kevin Coppersmith of Geomatrix, who will be talking to us about the EPRI Studies.

MR. COPPERSMITH: I'm here today representing a study by the Electric Power Research Institute, known for short as EPRI. This is part of their high-level waste project. I am going to be stepping through a brief summary of their program and focusing in particularly on the elements related to the fault displacement hazard and giving an update on where we are as an ongoing program. We will be going through the latter part of this year.

The EPRI-HLW project objectives are essentially two. One is to develop an integrated methodology for performance assessment and use that to identify and prioritize crucial issues. The second and equally important objective of the study is to involve the Department of Energy and its contractors in this methodology development and its implementation.

I think that it is important to note that the EPRI's involvement in this program is spawned by the strong interest the electric utilities have in the Yucca Mountain program and in the high-level waste program in general. It was felt a couple of years ago at the time of the evolution of this project, that the EPRI and its contractors would have an opportunity to help the Department of Energy to develop methods for having an integrated performance assessment that would help the process move forward both for purposes of early site suitability assessments as well as for ongoing performance assessments, iterative process of looking into the variety of technical issues, using those at any one period of time, that the performance assessment would tell you what the important issues are and use that evolving information as more data are gathered to help prioritize and to move the process ahead.

The Electric Power Research Institute has no long term objective of carrying out the full performance assessment. It is interested in developing a methodology, demonstrating its usefulness and the fact that it works and ultimately having DOE and its contractors take over that methodology and carry it forward.

The significant project milestones for the project, I have shown here. We have made it through Phases 1 and 2 and are in the middle of Phase 3.

During Phase 1 a methodology for integrated performance assessment was developed and it was demonstrated to be a useful methodology. I'll show a little bit what that looked like. The results of that first phase were published in an EPRI publication. Many of you have seen that.

Phase 2 involved a refinement of that methodology, the inclusion of some additional parameters such as gaseous release and the consideration of a number of other isotopes for example, and the refinement in the various components of the model. I'll show some detail on that.

Phase 3, which is ongoing right now is basically a demonstration of how uncertainties can be quantified and incorporated into the analysis. Phase 1 and 2 was designed to help establish and set up a methodology that could be used for integrated performance assessment, without the clear objective of trying to quantify uncertainties in that treatment. Obviously it is a probabilistic analysis, but the treatment of uncertainty and incorporation and quantification of our present level of uncertainty was not the goal in Phases 1 and 2, it is the goal in Phase 3.

To do that we are focusing in on one element of the performance assessment and that deals with earthquakes and tectonics. We are going through a process as I'll show in some detail, of incorporating the present levels of uncertainty regarding those issues.

Well, let me show, without getting into too much excruciating detail and performance assessment methodology, let me just show basically what the EPRI model looks like. It is shown schematically here in what we call our master logic tree. The components that are considered here are a variety of things that can influence the performance of the repository system. We are dealing here in the post-closure period over approximately the next 10,000 years or so and looking at the influence of a number of environmental factors like ground-water flux, earthquake caused canister failures. These are particularly fault displacement. This is the node that I'll be talking about in some detail. Change in water table due to earthquakes. Volcanoes. And, moving into the impact on the repository itself, borehole stability. I get into details of the canister and its design. And then moving through a variety of transports, pass from the repository system to the accessible environment.

These types of considerations are the common considerations being integrated now into all the performance assessments being done for the repository.

The important point here is, number one, we have tried to be very explicit and very careful to incorporate all those elements that could potentially affect the repository performance. And secondly, we are using a tool called a logic tree that allows us to incorporate the uncertainties in each one of those elements and ultimately have a full distribution of uncertainty in the final answer. The logic tree approach in a nutshell, is essentially one that allows for alternative hypothesis and probabilities associated to those alternatives and I'll show some examples of that.

I should point out here, I am going to be focusing in on particularly this node of the logic tree. And of course to get a full distribution on the probabilities of earthquake induced canister failure due to fault displacement, a larger analysis and in turn a much larger logic tree is involved. I'll get into some of the details of that.

But, earthquakes show up not only in the fault displacement part of the problem, but the considerations of vibratory ground motions, the affect on the water table borehole stability and other places along the way. The description of and characterization of the earthquake environment becomes important, and there is feedback between some of these elements.

The way the methodology, and again this is in Phases 1 and 2, the way the methodology was set up was to develop a methodology development team that had met for about a year for Phase 1 and another year for Phase 2. These are the individuals involved in that team. So, I think it is important when you look at the expertise involved or basically looking at essentially a single individual for any one element of the performance assessment. We asked that these individuals describe in the best way they could the particular models that might be most appropriate for trying to quantify the particular element of the performance assessment. And, to make their best estimates of the types of uncertainty that might exist in the community at the present time. We didn't, though, use multiple experts or try to fully quantify the uncertainty in these first two phases.

You'll see, for example, I am the only one up there whose is involved in the seismic geology part of the problem. The model that I will show is essentially one person's model. What we ultimately would feel is appropriate for a full probabilistic performance assessment of course is a better and fuller description of the uncertainty.

Just to give you an idea of what these logic trees end up looking like, this is an example to give you an idea of how the results of the performance assessment come out. Essentially, the logic tree as I showed is a large tree with a number of nodes which in turn are composed of a series of smaller trees. If you look at just in general at this example logic tree, this is the basic scheme of how things are done. If we look at a particular environmental factor external impact, let's say this is the likelihood of a particular type of volcanic eruption or dike intrusion probability or dike intersection with a repository, a particular state of that hazard shown here as E_1 and an alternative state shown as E_2 and each of those alternatives are associated with a probability of being the true state of nature. This is a typical way of breaking down the problem into component parts, but, in turn would lead to certain types of radioactive releases in terms of a source term here S_1 or S_2 in turn associated with probabilities.

Hydrologic properties are also uncertain as we well know. We could show those with different alternatives as H_1 and H_2. You can see as you work your way through this logic tree you have a combination of particular scenarios. These are arranged in such a way that the elements, the components of the model that exist to the right are dependant on those that exist to the left. So given, a volcanic dike intersection probability, that would lead to some source term and move on through the tree.

Essentially, the combination then of parameters that find that particular end branch and in this case E_1, S_1, and H_1, the probability of that particular combination of parameters is simply the product of the probabilities of the branches that got you there. And it is a very simple technique to go through and it is very convenient and efficient for scientists and engineers to quantify their uncertainties this way.

The way this gets into the calculations essentially those particular combinations, end branches and their probabilities, then go through a series of source and hydrologic transport calculations and lead to a distribution of chemical release that looks like this. It is a function of time. As I'll show most of the examples go from essentially zero out to 10,000 years or so, we look at the release rate as a function of time.

This to get at the actual accumulative distribution or probability distribution, we need to of course look at the likelihood of this scenario and that is essentially the probability associated with that branch. So, when the probabilities are convolved with these distributions, we have a cumulative complimentary distribution function, CCDF which people in performance assessments are used to looking at that expresses the release rate. Here are the curves that express the release rate from zero time out to 10,000 years. These are the individual paths or end branches. We can see the increasing release as a function of time. When we convolve those with the probabilities for each one of those, we come up with CCDFs that look like this (indicating). This is shown for one isotope Cesium 135.

One of the important things, I think to point out, in doing this for Phases 1 and Phase 2, the actual location of this curve, its level or amplitude relative to the EPA criterion is not the important part of what we are doing. What we are trying to do is to show that we have developed a methodology that can be useful. We have also done sensitivity analyses to try to get a handle on in the first cut what some of the most important issues are.

For example, what is shown up here and it is difficult to see, I understand, but looking at different flux levels of ground-waste flux ranging from four millimeters a year down to half a millimeter a year, if we could just look at it in general, these are the scenarios. You can see those scenarios in the heavy lines or those scenarios that lead to the highest and earliest releases. Those are the dash lines that are clustered more down in this area. The one in the half a millimeter case are essentially down with very little or no release in the 10,000 year period.

This type of dissection and sensitivity shows that essentially these types of factors flux in particular is one that comes through as being a very important element of the model. This is the spirit so far, of what the methodology has attempted to do, is to use this to show what might be important and to demonstrate that it works.

Well, let me move on then to the good stuff, and that is basically dealing with the fault displacement node of this logic tree. Basically, I want to deal only with the fault displacement hazard and outline a methodology that was put together to try again, primarily by myself, with the help of Bob Youngs and Donald Wells to try to capture what I think are the major concerns related to fault displacement hazard at the site. I'll talk about at the very end, we are in the process of asking now, several experts to develop their models and to assign their level of uncertainty to the various components.

The basic fault displacement model that we envision has two parts. The first part is basically the earthquake source model. This is standard for probabilistic seismic hazard analysis of any type for laboratory ground motions or whatever. Essentially it is defining where earthquakes are going to occur, what types of geometries the faults are going to have, the maximum earthquakes that would be expected for each individual source and the earthquake occurrence rates.

That basically defines the earthquake occurrence part of the problem.

From that we are dealing specifically with fault rupture and not vibratory ground motion or some other element. And to get at that, I would say this is very standard and is done all the time; this part is done very rarely. There are a few models available that give us a full description of the fault rupture part of the problem and it leads to, I think the good opportunities for a lot of insights and alternative modeling procedures.

One of the things that we thought was important as I'll show is that typically fault ruptures at the surface or at the near surface are complex and are often composed of primary ruptures as well as what we call secondary faults. We need to look at that distribution of secondary faults and look at the probability of intersection of either primary or secondary faults with the repository. And we need to establish that probability that that will occur. We also need to establish the probability that certain amounts of displacement will occur within the repository.

The way we approach the problem is to first look at the distribution of faulting in the repository area. I think we apologize for the difference in scale. The conceptual repository boundary is shown here and the faults that have been mapped in the vicinity of the site are also shown. What we are trying to capture here is the probability that the individual faults either those that are mapped or those that are not mapped will intersect the repository and that is treating the repository in three dimensions. And that will occur during some individual event. We treat the problem by first assigning some faults, assuming that some faults are what we call primary faults and are those that have the most displacement and appear to be more major features in the region. Those are outlined in yellow here. And then, looking at secondary faults that may occur around those.

I think you could treat the problem in two ways. You could say that basically the primary faults is where the action is and that is what we need to consider or we could say that you basically should consider the possibility of secondary faults or other types of deformation around these primary faults. As I'll talk about, I think our present level, again this is a snapshot in time, our present level of uncertainty about where future ruptures will occur demands that we treat more than just the primary faults in the model. I think we will see we haven't gotten our opinions back from the experts yet, but I think we will see that several of them fill with this secondary faulting part of the problem is something that needs to be incorporated into the model.

What we do to start out is for the first part of the earthquake occurrence or the earthquake source model is to basically develop the types of source characteristics that were familiar for any type of probabilistic analysis. We look at, for example, this is the logic tree for the earthquake source part of the model for the Ghost Dance. We have considerations of whether or not it is active. Again, these terms, these particular branches are ones that can be assessed by any individual. If you would like to use another term like, I hate to use capable or susceptible or Category 3 or some other term, basically the assessment here is whether or not this has the potential to undergo the seismogenic slip or cause fault displacement in the repository. That is the important part.

We leave open other elements like the geometry and dip and depth as I'll show are going to be important to this three dimensional probability of intersection of a fault with the repository. For this purpose or methodology purposes, we use essentially a single value that comes from the average of what we see in the Basin & Range. Obviously there is an uncertainty here that needs to be further characterized.

Estimates of maximum magnitude which come from considerations of false segmentation, ruptured length and so on. The slip rate which for the Ghost Dance Fault is particularly poorly defined. I can imagine a considerably broader range of uncertainty here, but it certainly needs to be included. Our model as most other models for earthquake occurrence and probabilistic analyses now rely very heavily on the estimates of fault slip rate.

Finally, the assessment of what type of recurrence model, given the slip rate, how do we partition out that slip into earthquakes of various magnitudes, various seismic moments, do we use the characteristic distribution or an exponential. I think that after Dave Schwartz' in sightful talk this morning we obviously would all use characteristic. But to keep that open, we allow for uncertainty in that component as well. A totally unbiased view.

One other thing I just wanted to point out, in the characterization of some of those elements, there are these types of considerations. The best estimates now are the models for understanding slip rate on some of the faults in the local area, like the Paintbrush Canyon, Bow Ridge Fault, argue for changes in the rate of slip back into the time presumably even 10 million on out to older time periods, the rates of slip might have been higher than they are in the more recent time periods. As a geologist in making predictions about the next 10,000 years, our best estimates and the ones that we would like to rely on are those that have been taken from the most recent geologic past.

In this particular case, we have very few data. John Whitney and others have been developing as much information on Quaternary slip rates as we can get. I think that will be very important when we have indications like this of a change in slip rate over a geologic past.

One of the other elements here (indicating) we use that to develop the earthquakes source model and then we begin to look at the problem of the pattern of rupture, that we might expect. This is probably the trickiest part of the problem and this gets into the second component of what I call the fault rupture model. We know from historical surface ruptures not only the Basin & Range, but elsewhere on normal fault systems which we are concentrating on for this model, around the world that the pattern of rupture at the surface and presumably the near surface, say the repository depth of a few hundred meters, we have a broad range of observed behaviors.

We see some ruptures like the Pleasant Valley rupture in 1915, that I would consider to be relatively simple. The pattern is one of a large linear trace without two much deformation in the upper plate or the foot wall. Probably the other end of the spectrum at least for the Basin & Range would be that of the pattern of the 1932 Cedar Mountain earthquake. We are hard-pressed to define what you would call the primary fault in this case and have a shattered zone of deformation and individual traces, some of which are not aligned parallel to the overall rupture, but are as much as 10 to 12 kilometers wide. I would say that these probably define the sort of spectrum of what we have seen in the Basin & Range. We need to capture this part of the problem in a fault displacement model. We simply can't say that the primary faults that have been mapped or all that is going to happen. I think we have other cases where we need to consider enough other historical cases that force us to consider the possibility of secondary faulting as well.

Now the predictability in detail of that secondary fault is what we have been talking about and I'll have some other comments about that.

The way we decided to model the problem was to say okay, let's deal with a primary rupture, a primary fault that is shown here and again these are the more major faults and larger amounts of cumulative slope and so on. And to allow ruptures to occur along those, of course the size of that rupture both the length and down dip width and in turn the rupture area is directly magnitude dependant by some very well established empirical relationships, will then allow that rupture, magnitude dependant rupture and size to appear randomly along the fault. Then, we will consider the possibility of deformation of secondary faults off of that main fault.

Now we could do this by saying we are going to assume the secondary faults will occur where other map faults are if we believe that, or we can assume that secondary faults will occur randomly within a zone about that primary fault. We have chosen the latter. We have chosen a model that says that the primary faults are where the main action will be; the secondary faults will occur randomly within a zone about the primary rupture we will get into and we will have more discussions about that later.

Again, looking at the primary rupture itself, we have relationships like this to give us a good handle on the area of rupture that would be expected for a particular magnitude. So, coming out of our earthquake source model is the frequency of occurrence of various magnitudes and can directly relate that to ruptured area and randomize the location of the primary rupture on the primary fault.

What about secondary faults? It's a little bit tough. What I am going to be showing are a series of plots that we have put together based on, and Donald Wells is the fellow that did all the leg work, based on a series of normal fault ruptures world-wide, but most of which come from the Basin & Range province. You'll see in many cases the large scatter in the data. We are not trying to regress information one parameter on another and to try to arrive at linear relationships. What we are trying to look at is the range of observed behaviors on historical ruptures. What you are going to see is that some of these are basically shotguns. If it is a shotgun we will incorporate that range in the modeling.

What we do, I should say overall, the model follows now a simulation. We have a earthquake occurrence. We will then lead to a series of simulations which means we allow for earthquakes at various magnitudes on all the faults in the region to be occurring and each time will be running simulations that vary the width of the zone of deformation and the amount of slip and so on.

Basically, this is a plot that shows several historical ruptures. When we did this the first time we looked at the width of the zone of deformation at the surface as it was. Then we had some indications and we would expect mechanically that the width of the zone of deformation and the hanging wall should be wider than that in the foot wall. So, we looked particularly at the width of the zone on the hanging wall and on the foot wall.

What we are seeing here is a function of magnitude for a series of earthquakes. The width of the fault zone as shown in the hanging wall is the width of the hanging wall deformation. The diamonds are showing the foot wall deformation. So, for any particular earthquake, you have two widths up here; one for the hanging wall and one for the foot wall.

We see in general an increase in the width of deformation as a function that is a function of magnitude but there's certainly a lot of exceptions. What we will do then in the simulation is allow that width to vary from essentially zero up to an upper bound of observation.

The other way we treat the problem is to try to look at, if it looks like the data are telling us something a little bit more strongly than just a uniform distribution, we will try to assign a distribution to this. We looked at the ratio of the foot wall, the hanging wall, the fault zone width and as you can see in almost all cases or virtually every case, the width of the zone of deformation of the hanging wall is broader than in the foot wall. And that is obviously consistent with what we expect for normal faults with antithetic faulting in the hanging wall, graben formation and so on occurring almost entirely in the hanging wall of the deformation.

In this case, we see that we have most or, many of the observations are occurring with a ratio of about .4 between the foot wall and hanging wall fault zone width. So we model this as a distribution that we can show as a discreet distribution that looks like this. It allows us to include that consideration.

So as we go through the simulation for a given magnitude, we'll have a given fault zone width, and we will have a ratio between the hanging wall and foot wall.

We want to look then at how much secondary faulting occurs. What are the lengths of secondary faults as opposed to the lengths of the primary rupture. We might have a primary rupture that is 20 kilometers long and we add up the length of all the secondary faults and it is half a kilometer. That is very different than if it is 20 kilometers of primary rupture and 20 kilometers of secondary. So, essentially that is the ratio that we are looking at here. We tried to see whether or not that ratio varies as a function of magnitude; it didn't seem to. The only thing that seems to be associated with is fault zone width; the overall width.

That is basically saying that the wider your fault zone is, the longer the length of secondary faulting. The more secondary faulting you have. This why view all this, and the reason for it is we are going to be dealing with the likelihood of faults intersecting the repository, which is a three dimensional space. So, we need to deal with how much secondary faulting actually occurs to quantify that. That is what these relationships do. In this case we used three probabilities or three levels that describe that distribution of fault zone width as a function of the ratio.

I should say there is another handout besides the viewgraphs that is a preprint of a paper that Bob Youngs and I have in the High-Level Waste Conference in Las Vegas that would go into a good bit more detail in this part of the model.

Just to give you a feel for this for particular, just looking at just the secondary fault part of the problem, looking at the length of secondary faulting that occurs within the repository, this is what it looks like. We are dealing here with the length is over here in kilometers or we are dealing on the order of 100 or 200 or 300 meters of secondary faulting through the repository in three dimensions. Remember we are allowing our rupture through three dimensionally along the primary fault as well as long secondary faults.

We see for example that a more distant fault, the Paintbrush Canyon exists way out to the east of the site, the probability or the likelihood or the length of secondary faulting that occurs within the repository is low. But, even though this fault does sit well out, would not rupture as a primary fault through the repository, there is a finite likelihood of secondary faulting related to that fault through the repository itself.

Likewise, for Solitario Canyon which exists very close, it is off to the west of the site, basically the repository sits in the foot wall of that. But at least to a relatively significant links of secondary faulting through the repository.

The Ghost Dance was the only primary fault that we modeled through the repository itself. Again, this is the link of secondary faulting, it is relatively less because it would be the secondary faulting just around Ghost Dance.

Well that gives you the likelihood of various amounts and various lengths of faulting through the repository. What about the amounts of displacement? Well we have relationships like this one, that relate the amount of displacement, average displacement in this case. We have similar relationships for maximum displacement as a function of magnitude. These are surface observations, empirical relationships that have got a good bit of scatter, but basically show the amount of displacement on primary faults as a function of magnitudes. This gives us a direct indication; we can tie it back to the magnitude on a primary fault.

For secondary faults, this is one of my favorite plots, we have seen, well number one, I think it is important to show that we have had virtually no cases where the amount of secondary displacement has exceeded the amount of primary rupture. That's nice. We have seen many cases where the amount of secondary displacement has been very significant, up to 80 percent of the primary. And this would be cases of large graben formation for example; big antithetic faults that rupture at the same time. And, you've got three meters of displacement on the primary fault and two meters on the back facing antithetic scarp. Those types of things we do see and should be incorporated into a model that is trying to capture that secondary faulting part of the problem.

So here, I think in the fact that there is a uniform distribution, our simulations basically allow for the amount of secondary displacement, the ratio to be anywhere from zero to 80 percent of the primary displacement. So some of these secondary faults can be very significant.

Why deal with the amount of displacement? Well, I think that the issue that hasn't been resolved to us is how much displacement to canisters these boreholes can withstand. Is it a centimeter? Ten centimeters? What is it? Right now we are not sure so we will assume that the amount of displacement is important and we quantified it for a couple of values for one centimeter and for ten centimeters.

Well the process of looking at the likelihood then of canister intersection is a simple geometric one. We are assuming vertically emplaced boreholes. We looked at fault geometries that have been used and we just looked at simply the likelihood for 35,000 canisters, the likelihood of intersection through these scenarios of the faults with the repository. We are just assuming right now that there is no design aspects to the canister configuration that will allow you to move away from the Ghost Dance, for example, or to avoid other faults in the excavation that you would say could potentially have movement. We are using what we call, what Bert Swan called figuratively a "dumb model", that basically says, I don't agree with, basically says you have got a primary fault and you have got a halo of secondary faults around you. You are dumb in the sense that you are assuming we don't know where those secondary faults exist. They occur randomly within that zone.

Others may feel that maybe we should still be dumber, we are not even sure where the primary fault would be; it is just a zone. Others might say I think we can define where both are; the primary and the secondary faults and we don't need to randomize the problem at all. Intact rock will stay entact rock. So, it's kind of dumb.

The results look like this and I will try to wrap up. Basically we are dealing with a couple of, and let me just break the problem down. On the left-hand column this is looking at the likelihood of the canister failure probability for one centimeter of displacement. An engineer plotted these so we call them offset. But really we are looking at dip slope, so it is a displacement.

The ten centimeters of displacement are shown on the right side. These boxes essentially represent that the integrated contribution, the fault displacement hazard from primary and secondary faults. So, this is the failure frequency here is essentially annual failure of frequency or annual probability of canister failure. And we are looking at numbers that are on the order of three or four times 10^{-4} of annual probability.

Again, I think the absolute level of the numbers isn't so important in this analysis, as for demonstration. But, it is important that when you dissect it you see that the contribution related to secondary faulting is almost equivalent to the combination of the two and the contribution to fault displacement hazard from primary faults is relatively low. An order of magnitude less. When we move into the probabilities for the ten centimeters of displacement, again the probabilities get lower, and the annual frequency of occurrence or probability of occurrence gets lower because of a larger displacement. But the contribution related to primary faulting is relatively small compared to secondary.

I think that may be the most important message here is that basically we have a situation at least, inasmuch as this model might be realistic to real world fault cases, our biggest problem right now and our biggest concern would be the halo deformation around the primary fault, which we have termed secondary faulting.

Let me just then give you a brief update of where we are on Phase 3. What we are going to do in Phase 3 is to attempt to quantify uncertainty. We have gone through I think and shown that the methodology works and is appropriate and is performance based, which I think is the way to deal with earthquakes or any of these issues, but now we need to really try our best to get a better handle on the uncertainty. I think one thing that is important to recognize and probably everyone realizes that the uncertainty at any one point in time is going to change. We hope that the site characterization program and so on will help reduce the uncertainties of certain aspects of the model. We are trying right now to not only show the level of present uncertainty, but allow that to focus and prioritize the program that will lead to the greatest reduction of uncertainty in the future. This is the type of process that should be done periodically and updated.

The way we are handling this part of the problem of quantifying uncertainties particularly for earthquakes and tectonics is through a couple of workshops and through the elicitation of expert judgment. We are trying to show two things. First is how that expert elicitation can occur, and secondly to quantify the uncertainties actually for the performance assessment and recalculate it and show how it works.

I think the issue of uncertainty definition of quantification is one that is almost obvious, but, for probabilistic performance assessments, it is essential. We have different ways of doing that, but it has to be done. I think we are dealing with single valued parameters and we are kidding ourselves and we are really trying to show that we have a perfect knowledge about these characteristics and it is very unusual to have that type of definition.

The use of expert opinion, notwithstanding Dave Schwartz' slide is a very effective way of trying to quantify uncertainty. I think it is important to express our concept about the use of experts is not one to supplant data collection or the understanding and gathering of new information. It is one that is a process of taking the available data at any point in time and allowing it to be digested and the different points of view to be considered and expressed and incorporated into analysis. I think there has been a misunderstanding by some people that expert opinion elicitation is a process of saying, hey, I don't need data, I've got experts. That simply is not the case here and I think that in the several probabilistic studies that I have been involved with in using expert opinion, that has not been the focus there. I think we are simply trying to at this point in time see what our level of uncertainty is and to demonstrate the use of formal expert elicitation and then to go forward with site characterization. In fact, I think a very good thing to come out of a program like this would be the clear expression of what the important issues are. We see that when we run this through, these are the most important areas. The site characterization data collection could be focused on those issues, not to supplant that collection of data.

One thing that is important in many of these studies that have gone on, is a whole field that deals with nothing but expert elicitation and so on. I think it is felt right now that one expert can assign a range of uncertainty and I tried to in my first pass at this. Multiple experts get at something that is called diversity, which these days is something that is seen to be a very good thing to have.

Well selection of the expert panel, basically any expert panel again, most of these things I think are motherhood statements, but I think this it is important to point this out. The panel has two purposes and one is to get at and to quantify the uncertainties associated with these issues so they can work their way into the performance assessment. Secondly, we are trying to demonstrate how you do this, how experts of elicitation can be dealt with, expert judgments, workshops can be held, how there can be a free interchange of scientific discussion, regardless of what institutions or government representations are involved. We are trying to demonstrate the process just as much as to actually carry it out.

The guidelines for selection are shown here and they deal with experience and capabilities and of course willingness to participate. I think the panel is a balanced one, but I think it should be pointed out that other people could be identified with equivalent skills who would be candidates for this type of panel. We simply didn't have the opportunity to have a lot of people. In fact, there are problems with very large panels in carrying out this type of work.

We have asked the individuals to represent themselves and not necessarily their institutions. We found, at least in the first workshop that they have been able to do that. They have been able to represent their own opinions and not worry about whether or not they'll get sign-off from headquarters or somewhere else later.

The Panel on Earthquakes and Tectonics is shown here. A lot of people that you have seen before, some of which are in this room. I think it is a good balance between those that are very close to the project and working on it now and those that are somewhat detached and have not been particularly involved in Yucca Mountain, but have been involved in Basin & Range tectonics or probabilistic seismic hazard modeling and so on. So, I think it represents a good group. So far the dynamics of this group in the first workshop have been very good and the interplay has been excellent.

To help guide through the process, one of the issues in expert elicitation and there are several, deal with a lot of the dynamics of having experts, how much anchoring takes place, how you get them to interact and how you elicit their opinion and so on? To get that part of the problem, to guide us through that, we have three so-called normative experts, experts on experts who have been involved in this before. They have been helping us through the process.

A key element for example would be the aggregation of all of these opinions. Many of the big studies for the Eastern U.S. Seismicity for example, have stumbled through the process of trying to figure out how you take, say eleven experts, and aggregate their opinions. Do you give them all equal weight? Do they weight each other? Do you weight them? How is it done? Some of these people, Bob Winkler for example specializes in the expert aggregation procedure.

Finally, our schedule looks like this. We have gone through the first workshop in November. We will be having sample elicitations coming up in a week or two to basically familiarize the individuals with the elicitation process and what it is like having a normative expert sit there who basically doesn't know much about earthquakes but knows how to pull things out of your head and get you to quantify the uncertainties. Everyone will be elicited individually and there will be technical facilitators there too. That process is coming up.

We are in the process right now of analysis of issues and dissemination of data sets and there are quite a few related not only to Yucca Mountain specifically, but to similar tectonic environments and we are in the process of doing that now.

The March workshop will have three parts, basically begins with a focused discussion on technical issues, have the actual individual elicitations and followed it up by feed back of the assessments that have been made. Following that will be a reporting. A key part of this again throughout this process is the involvement of DOE and its contractors as observers and of participants in these workshops and with the goal of developing a methodology that is mainstreamed, that works, that incorporates the technical issues as we know them now and allows for the Department of Energy to pick this up and to carry it on.

Thanks.

DR. ALLEN: Thank you, Kevin.

Questions from the panel?

Bob Kennedy.

DR. KENNEDY: Kevin, in your presentation plus in some previous presentations, there seems to be a great deal of concern about predicting fault movements of one centimeter or five centimeters or ten centimeters in an implication that movements of ten centimeters or less are likely to break these canisters. Now, in my opinion rather than putting the burden on the geologists in predicting annual probabilities of fault movements of ten centimeters or less, it would be far better to change the emplacement design if the design is really that sensitive to such small fault movements.

In my opinion, we have got a problem with the design if it is really sensitive to such small fault movements because there are so many other ways that over any large number of years we can get deferential displacements around these canisters of ten centimeters or more.

If that fault displacement issue became one associated with the significantly larger amount of fault movement, I am not sure that the secondary faults would still dominate over the primary. They may or they may not. But, I am very worried about all these studies being done at such small offsets. There is an implication there that these canisters, this emplacement design is vulnerable.

MR. COPPERSMITH: I can only comment that our guidance was essentially the air gap concept that would be one that with a centimeter or two we could close the air gap. We are not looking only at canister failure directly, we are looking at the performance assessment model also incorporates essentially closing the wall, closing the air gap and that leading to additional pathways and so on too.

DR. KENNEDY: Well, is someone else in this program looking at ways to make the emplacement design more forgiving of movement?

MR. COPPERSMITH: I believe so, yes. I can't directly comment on that, but, yes I believe so.

DR. ALLEN: But, you yourself, Bob, pointed out yesterday that this is an area where engineers themselves might disagree as to whether some imaginative design is really going to be effective 9,000 years from now.

Ed Cording.

DR. CORDING: Going further with that air gap question, this is really perhaps not so much directed to you but just a little bit further on here to other people that are concerned with this, is how many air gaps could you lose and not change the performance of the facility. I mean, that is the sort of thing one has to look at. I don't know whether DOE has presented that sort of information to us at this point. But, you can lose air gaps from a little slab in the sidewall of a hole collapsing against the wall of the hole. It's not a big deal, but is it going to be a major concern for maintaining this air gap in isolation?

DR. ALLEN: I think we better move on here. There may be comments that we will again take up this afternoon. We are running a little bit late.

Thank you, Kevin.

The next presentation--well, a group of three will start out with Quazi Hossain all on the ASCE Seismic Design Proposed Guidelines.

MR. HOSSAIN: Good morning distinguished panel, ladies and gentlemen. My name is Quazi Hossain. I am here on behalf of the American Society of Civil Engineers. We formed a working group to look into the seismic design aspects of the high-level waste repository. I will spend five or ten minutes introducing the activities that we are involved with, and I'll be followed by Bert Swan and Walt Silva this morning, and in the afternoon by Mike Hardy and Ken Mark. They are going to present in a little more detail about the different aspects of the seismic design of waste repository.

For those who are not familiar with American Society of Civil Engineers and our different committees, let me now briefly announce a few words. This particular working group is part of the Dynamic Analysis Committee, who used to be part of the Nuclear Structures and Materials Committee of the Structural Division. The Dynamic Analysis Committee of that particular division was chartered to look into various aspects of nuclear facilities design, primarily concentrating on nuclear power plant design.

Dr. Kennedy used to head that particular committee for many years. Presently, Bob Kassawara of EPRI is the chairman of that inner group. A few years back, you know, some of the committee members expressed some desire to come up with an ASCE special publication that will summarize the state of the art for the seismic design of high-level waste repository, providing some guidelines which can eventually be used by the industry or DOE to develop a detailing of criteria.

We went through some difficult times of forming that group. Finally, about two years back, you know, the committee was formed and we met about eight or nine times, and we drafted a rough--or the first draft of the document which is being reviewed by various committee members, as well as our certain peer groups.

Our objective was to summarize the state of the art and providing some recommendation where possible, and familiarize the reader with the different controversial issues that are presently being worked on by different groups. We are also trying to have a consensus on different issues and see whether we can come up with some recommendation which can be useful for the industry.

The process through which we want to develop this in a publication is as follows: From the working group, we are going to develop the draft guideline, which we have now with the first draft, and before this draft is reviewed by the higher ASCE organization or committees, we are planning to hold a conference or symposium, specialty symposium where this draft would be presented in the form of about ten papers. We are also inviting outside industry experts to submit and present papers in that conference, and based on the discussion that will go on in that conference and the proceedings, we are going to modify the draft that we have prepared, and then it will go through ASCE peer review before it is eventually published as an ASCE special publication which, we think, will have the status of a guideline, not a standard.

Presently, the scope of the document or the contents of the document will be--will have six chapters addressing the various issues of high-level design, high-level waste repository design and primarily from seismic consideration, seismic and other analysis and design considerations.

The first two sections will be introductory and description, general description to establish the terminal loads and different components of the repository. Section three of the document will provide the latest research on fault characterization and ground motion characterization, with some recommendation on the methodology.

Section four and five will provide some general criteria on the design aspects of both subsurface as well as surface facilities, and Section six will provide some guideline for instrumentation for monitoring purposes.

The schedule with which we are working presently is that we have our draft, first draft complete. The topical conference to get industry's input, as well as a discussion on our original draft will be in August 19 and 20. It will be in San Francisco, California. After the conference, based on the discussion, the draft guideline will be finalized from the working group by September of this year, and we plan to get ASCE's review within the next three months following September, and our target date is to publish this document by the middle of next year.

Before I turn it over to Bert Swan, I want to express my thanks to the Board and especially Leon Reiter for inviting us here and giving us this opportunity to explain what we are now doing, and we would also like to invite all those who are interested in it to our August 19 and 20 symposium where we'll be presenting the details of our draft, as well as other industry experts will be presenting papers on the seismic and dynamic analysis and design issues of the high-level waste repository.

With that, I will turn it over to Bert Swan.

DR. ALLEN: Thank you.

Bert, you're on again.

MR. SWAN: There are advantages and disadvantages of being late in the program. The disadvantage is we're rapidly approaching lunch and we want to try to move through this stuff quickly. The advantage is, Kevin showed most of my slides so I can move through it very quickly. Also, I'd like to thank David for the presentation he made earlier. It made, I thought very eloquently, one of the points I wanted to make here today; also thank him for pointing out that I'm not as agile as I used to be when David and I did a lot of the work together along the Wasatch Fault.

The topic is going to be talking about assessing the potential for fault displacement for high-level nuclear waste repositories. We've seen this figure before, and it just illustrates one point; namely, that because of the size of a repository, we're going to encounter faults. I think it was Jay Smith who pointed out, you know, if we want to set back from faults, what it's going to do is push us closer to the next one. It's almost a moot point with a repository, but just because of the dimensions of it, we are going to have to address the issue of faulting and how do we accommodate it in siting design and performance.

This is not a murder mystery, so I'll just start right up front with the conclusions of the--well, actually, before the conclusions; just define what we mean by potential for fault rupture, and by fault rupture we mean it includes the displacement that may occur along the primary faults, any displacement associated with secondary faults during a seismogenic event, and also any associated deformation, either drag folding or folding across the leading edge of a fault propagation fold. So we're talking about tectonic deformation associated with earthquakes.

We define potential in terms of the location and three-dimensional geometry of the faults relative to the location of the repository, the sense of slip or style of faulting, the amount of net slip per event and/or the cumulative net slip that could occur during the design life of the facility of concern, and also, very importantly, the likelihood of occurrence.

Now, we can define likelihood of occurrence in the old Appendix A approach, where it is implicit that if the repeat time is less than--in the case of Appendix A--multiple events in the last half-million years, the hazard is low enough that we aren't concerned about it, or we can do it explicitly by defining the earthquake recurrence characteristics either in terms of recurrence interval of events or in terms of rate of slip on the faults.

In terms of what the ASCE working group's recommendations are with respect to fault displacement, we've seen a lot of discussion over the past two days about the relationship between Quaternary faulting and potential for fault, and we feel the best way to characterize the potential is to characterize the Quaternary history of fault displacement in terms of its location and geometry, sense and amount of displacement, and likelihood of occurrence.

The committee feels there are accepted direct and indirect methods that can be used to quantify these parameters and their associated uncertainty.

There are two basic approaches for assessing the potential; either a classical deterministic approach, or a probabilistic approach, a la the discussion of Kevin's previous talk. The committee advocates, recommends the use of both approaches, and with a strong emphasis on the probabilistic approach for quantifying the fault hazard. This is particularly important where the regulatory guideline or, in a sense, low probability of release expressed in terms of ultimate performance. To arrive at that final end value number, we need to quantitatively assess what that potential is.

We feel the advantage of the probabilistic approach is that it explicitly quantifies the uncertainty in both the input parameters and in the analytical models or methods that are used. It allows you to use alternative analytical methods and test the sensitivity of the results. It also allows you to test the sensitivity of the results to the uncertainties in your input data and then, importantly, the probabilistic approach should be an iterative approach that goes on throughout the consideration siting design of the repository. Done early in the investigations, it allows one to prioritize the issues and focus the investigations and analyses on those significant factors that are most significant to the performance.

We also need to clearly define the relationship between hazard and risk. Faulting, surface faulting is a hazard, and what we're worried about is the ultimate risk, which is an end-line result of that hazard, and that's dependent on design and how you accommodate the hazard in the design.

There's several ways--implicit in this is at some point you have to have an explicit level of acceptable hazard, which for a repository really hasn't been designed--defined. The definition is in terms of the probability of occurrence of release to the environment. Given that there is going to be some finite probability of the hazard occurring, there are appropriate design measures to mitigate the unacceptable effects of the fault hazard.

Criteria related to a potential for faulting have to be flexible enough to allow for the different functions of the different elements associated with the repository. The surface facilities, for example, have a--it's primarily a preclosure issue. We're concerned with an interval of roughly 100 years; whereas, with the repository itself, we're concerned with preclosure retrievability in that 100-year period, and then the postclosure performance over the long time frame. Also, different elements of the repository have different risks associated with them. Your access ramps and tunnels don't particularly pose a high risk to the performance of the repository in terms of radioactive release to the environment. You may be able to accept some faulting hazard just in terms of low probability of occurrence through those elements, whereas you may or may not be able to accept it for the waste packages themselves.

So given the different functions, different elements of the repository, there are different ways that the risk to fault displacement can be mitigated. As I say, one approach may be to just determine that the risk is acceptably low. Another approach would be to locate the facility's waste packages to avoid the active faults; and the other would be to quantify what the potential for fault displacement is in terms of amount of slip per event, cumulative slip over the lifetime of the facility, and then to design for that displacement.

I want to touch just briefly on areas of investigation. In terms of studying faults, major tectonic features out to 100 km are probably only an issue in terms--or are an issue only in terms of the ground motions at the repository. Because an underground repository is not particularly susceptible to long period motions, the main emphasis of concern is going to be earthquake sources within about 50 km, but then for fault displacement itself, the real issue is--should be a focus on faults within about 10 km, 10 to 20 km of the repository itself. To go out to distances beyond that in assessing potential for fault displacement, what you're really doing is gathering data to better understand your tectonic modeling faults within the region, but it's not going to tell you a lot about the potential for slip in the repository itself.

Implicit in any potential for fault displacement investigation is a basic premise, and we've talked a lot about this today, and David's presentation focused on it a lot, and the basic premise of this--and it's implicit in Alquist-Priolo fault zone studies in California, it's really implicit in Appendix A--and that is that future fault slip will reoccur at the same locations and in the same manner as geologically recent--and by geologically recent we mean Quaternary past displacements. Accordingly, then, future fault displacements will only occur on preexisting faults. The likelihood of future fault displacement is going to be related to the frequency of the most recent past displacements, and it's also based on the premise that the tectonic forces that cause faulting are assumed to be constant over the geologically short period of concern; which in the case of a repository is the next 10,000 years.

Corollary to this is that unfaulted bedrock will remain on bedrock, will remain unfaulted. That is a basic premise that intuitively we know must be false right from the outset. We know--I guess it was Clarence in the coffee break yesterday morning pointed out the San Andreas Fault wasn't born in one mega event. It hasn't always existed. It has evolved through times, so we know faults do grow along strike and along projection at dip. They have to evolve. But the practical experience, our experience in studying quaternary faults--and David addressed this point earlier today--is that the faulting reoccurs along preexisting fault zones.

In every introductory geology course, we learn the basic premise that the present is the key to the past. By studying present day processes, we can then interpret the geologic features in the past, but in terms of the fault displacement, I sort of inverted that, and it's really the past is the key to the future. What's happened on these

Quaternary--at the site or on faults during the Quaternary defines what we can expect to see in the future.

David gave a lot of examples where faulting reoccurred along--demonstrating where it reoccurred along preexisting faults. One example he didn't show was the surface rupture associated with the 1980 earthquake in El Asnam. I'll just briefly describe this one simply because it's probably--David emphasized how finite and narrow many of the fault zones are. This is probably one of the sloppiest cases where we're dealing with a low angle thrust fault with a wide zone of deformation on the upper plate of that thrust, and a secondary fault, the Beni Rached Fault that ruptured up in this area, and in putting trenches across this fault, this is a--

DR. ALLEN: But is this not true of thrust faults in general, pretty low angle thrust faults, whereas Dave was emphasizing normal faults and strike slip.

MR. SWAN: Yeah. This is typical of thrust faults, but one of the concerns is the secondary faults on the upper block, and in trenching the primary fault we saw multiple Holocene events along that, Trace 3 events within the last 1500 years, and even in the case of the extensional faulting, normal faults on the upper plate which had displacements, vertical displacements comparable to those along the primary trace, those were along preexisting faults that could be defined in bedrock and also in terms of repeated quaternary displacements, and then most notably, the Beni Rached Fault, which extends out several kilometers, about 10-15 kilometers away from the fault. That was the location of displacement during a prior earthquake in 1954, and then there was also geologic, geomorphic, and stratigraphic evidence for prior Quaternary displacements on that secondary fault.

So accordingly, the most direct approach for assessing the potential for fault rupture in the repository is going to be to determine the locations and three-dimensional geometry of the faults in the vicinity that could affect the performance of the repository if they were to experience displacement, and then to reconstruct the history of Quaternary displacement on those faults that could impact the site.

And in summarizing the recommendations, I allowed that there are two basic approaches to this; deterministic, where you could classify faults as either active or inactive, and then you worry about only the active ones, and typically in a deterministic approach you define for the maximum event scenario, regardless of its likelihood of occurrence.

Now, there is implicit in a deterministic approach a definition of acceptable risk, and that is your definition of what faults you classify as active. Now, for the repository, I think implicit in Keith's presentation is all Quaternary faults are considered as active. What's missing in this guideline is what do we do with those in terms of design, and he's clearly placed the burden on DOE to define how will we accommodate Quaternary faults in the design of the repository or mitigate hazards due to them.

The other approach is probabilistic, which as I said earlier, explicitly incorporates the uncertainty in the analysis; both the analytical models and the input parameters. A further advantage of the probabilistic approach is that it considers the full range of possibilities, not just the maximum credible event, but also the range of possibilities from the maximum, minimum, and to assess what the most likely event scenario would be. And it allows one, as I said earlier, to test the sensitivity of the results to the various input parameters and prioritize the most significant issues and focus on those that would affect design and performance.

I talked about the--or mentioned the most direct way to assess the potential is to look at--reconstruct the history of Quaternary faulting. There's several limitations to the direct approach. Often we have incomplete structural information on the location and geometry of faults. The issue of threshold of detection comes up. Some of the faults maybe have small amounts of displacement or--and do we know where they all are and how do we address that issue?

One of the main limitations to the direct approach, the primary one is that we have limited distribution of Quaternary deposits, soils, and geomorphic surfaces that can be used to reconstruct the history of Quaternary faulting. We do not have, in most locations, a complete record of Quaternary deposits going back to the beginning of Quaternary time to get a complete history. And also, there are uncertainties in the ages of the deposits themselves.

However, there's several indirect approaches that can be used to get a handle on the potential for fault displacement, and we aren't advocating the use of one or the other. In reality, we think you look at all these approaches and try to learn as much as you can about the faults. Three basic approaches would be the use of regional tectonic models and local structural models to predict future displacement; comparison to historical fault ruptures. Kevin showed a couple of those earlier with the Dixie Valley, Fairview Peak, and the Cedar Mountain Earthquakes, or analogies based on paleoseismic investigations of similar faults in the vicinity of the repository itself.

I'll talk just quickly about the use of

tectonic/structural models as predictive tools. There's been a lot of discussion in prior meetings about Yucca Mountain about what the appropriate tectonic model for the region is. In terms of two-dimensional models, both listric fault models for the Basin and Range faulting have been proposed, and block rotational or domino-style models have been proposed, and there are advantages and disadvantages to both models.

In the listric fault model, it's shown in a little more detail here. It has the appeal from the standpoint we don't end up with space problems in the lower part of the fault that we have with the domino fault. If you look at oil company seismic reflection data, there's a lot of evidence suggesting many of the Basin and Range faults are listric and shallow out at depths of three to five kilometers, and this was a very popular model probably about five years ago.

The big problem with a listric fault model is our experience with Basin and Range earthquakes is that the focal depths of the large Basin and Range earthquakes are typically down around 10-15 kilometers, and the location of the hypercenters suggests fault planes with average dips of 50 to 60°, which is contradictory to a listric model with the plane going listric and shallowing out at shallow depths.

If you look at a domino-style block rotation model, it has the appeal that it may fit the geometry of planar faults going down to seismogenic depths. It's unappealing from the standpoint that you end up with some real space problems in the bottom of the model that you don't have with a listric fault model.

Just a quick example of one use of these tectonic models, given or assuming a model, you can use these tectonic models as a predictive tool to predict amounts of displacement on faults. For example, in this case, you could look at this as being Fran Ridge, Midway Valley, Exile Hill, and Yucca Mountain. We can, on the Paintbrush Canyon Fault bordering Fran Ridge, we have good paleoseismic data giving us slip rates, slip per events and aged timing of events.

I put another one in here, the Ghost Dance. We have no Quaternary cover to evaluate it, but using the geometry of the blocks fault with ratio, you can scale off and predict ratios of expected amounts or rate of a slip on adjacent blocks as just an example.

While the Yucca Mountain area is not as simple as all that, as was pointed out by Dave Tillson in his talk yesterday, we actually see the overprint of different tectonic regimes. We've got the north/south style Basin and Range faulting. We've got the strike slip Walker Lane Belt and the east/west seismogenic zone. One model that's been proposed to account for these different tectonic styles is that we're looking at strike slip at depth and that these blocks at the surface--and this would be a planned view model then--this is a block rotational model. Given this type of model, if we have data on one of the faults, you can use it as a predictive tool to predict descents and rate of displacement on adjacent blocks.

We've talked a lot yesterday and earlier today in Kevin's talk on comparisons of fault ruptures associated with historical earthquakes, and already talked about two examples of Basin and Range faults that could illustrate the range of conditions or range of pattern of surface faulting one might expect with the Dixie Valley, Fairview Peak Earthquake, and the Cedar Mountain Earthquake.

The goal in applying this to the repository would be to--or the approach would be to try to fully understand what the structural relationships are associated with these two different styles, and then understand the structural relations at the site to say which one is more applicable.

Kevin already talked about how we could use compilations of data as a means of probabilistically forecasting what the, for example, what the width--zone of secondary faulting would be to the zone of fault--to the length of fault rupture. I'm not going to go into it further. He also talked about this and explained it in more detail than I can, so I'll just move over these.

The other approach, and one I'm particularly fond of, is just comparisons based on paleoseismic investigations of similar faults in the vicinity of the repository, and that's--as I said earlier, we have quite a bit of data along the Paintbrush Canyon Fault, Windy Wash Fault, and some of the other faults. What we don't have, because of the lack of Quaternary cover, is much data on how the Ghost Dance Fault has behaved during the Quaternary, but by looking at the geometry style of faulting in the Tertiary bedrock, comparing amounts of cumulative slip on the Ghost Dance Fault to the amount of cumulative slip on the Tertiary bedrock on the Paintbrush Canyon Fault, we can infer certain ranges of values in terms of slip rates and behavioral styles that won't--and they can't violate the basic constraints of what the cumulative bedrock slip is.

Given the small amount of slip on the Ghost Dance Fault, we have to expect either fewer events than we have had during the Quaternary on the Paintbrush Canyon, or the same--or we could model it as having the same number of events, but in that case, the events have to be smaller. So you can put constraints on the size and frequency of events based on the structural comparisons of the two faults.

Kevin has already gone through the probabilistic approach for estimating slip per event on cumulative displacement on faults. The best approach is from direct observational data, obviously; would be to reconstruct the Quaternary history on those faults. But because of the limitations and the lack of Quaternary cover, we can't always do that and we have to rely on indirect approaches, and in the probabilistic analysis on primary faults, we can arrive at the estimates of slip per event and Kevin explained the procedure before where we look at the inferred slip rate times the fault geometry to get a moment rate.

You combine that moment rate with an earthquake recurrence model and you can use alternative models, along with what your estimate of maximum earthquake and minimum earthquake are to get a frequency distribution for different size events, and then those different size events can be related either using analytical relations or empirical models; for example, those proposed by Bonilla and others, and if Kevin will--I'll put a challenge here that you'll get this paper out this year that would relate earthquake magnitude to slip per event.

And that is applicable to the primary faults. The result is essentially the same or identical on the secondary faults, except that you need some sort of scaling relationship to relate amount of slip on the primary fault to the amount of slip on the secondary fault, and as Kevin showed in his view graph, there are a lot of scatter in those data. The only consoling factor is we don't seem to see secondary faults with displacements larger than those observed on the primary faults, so we can do it by looking at just the historic data sets and the scatter in the data there, or we can look at fault-specific structural models.

The main source of uncertainty in the probabilistic analyses, the hazard is primarily driven by slip rate and there are uncertainties in the slip rate both in terms of the amount of displacement. That we can generally fairly tightly constrain, although there's some uncertainty as to, for example, what the net slip is on the faults in Yucca Mountain. We have a good handle on what the vertical displacements are on these faults, but we don't have a good handle on the strike slip component on some of them. We see evidence that there is a strike slip component, but that gives us some uncertainty in the amount of slip. The biggest uncertainty comes in with the ages of the deposits that are displaced.

The other source of uncertainty is then just how do you model your data. This is a plot of cumulative displacement over time for the Paintbrush Canyon and Bow

Ridge Fault. Here we've plotted it, you know, it is either a continuously decreasing function with time. The data argue and tectonic models for the area could support that we've had an abrupt change in rate over time. Others have argued that the slip rate should be more linear. But in your probabilistic analyses, you can model the full spectrum of conditions and take into consideration the uncertainties in the data.

So sources of uncertainty, probably the most important one are the fault slip rates, the amount of cumulative vertical displacement, sense of slip, and the ages of the displaced quaternary horizons. There's also uncertainty in terms of the slip per event and, you know, defining at any one point along the fault what the maximum slip, you know, does that measured value represent an average value, a maximum value for the slip along the entire length of the fault.

The main source of uncertainty and one that David Tillson alluded to yesterday is the earthquake recurrence models; what is the correct magnitude frequency distribution? How do we apportion out that slip rate into slip events over time? We can use, as David was alluding to, the characteristic as being the favored one for predicting fault displacement on particular faults. You could use the classical log normal. One of the problems that has not been addressed in prior probabilistic ones, but certainly can be very readily included in the model, and that's: what is the effect of temporal and spacial clustering and how would that affect the results.

Another source of uncertainty is a lot of our data in investigating these faults comes from surface exposures, and there's uncertainty as to how does the behavior at these faults at the surface relate to the amount of displacement we would get at the repository depth, and we can analyze this by looking at historical examples of tunnels that have been ruptured. There are actually very few of these and, to my knowledge, there's been no comprehensive review and compilation of the literature, but in most cases--and I think Bob's comments earlier about behavior of tunnels and ground motions is applicable here. We have a number of case histories, but they're very poorly documented, but we should take a look at those literature and see what they say in terms of what are the likely range of conditions we might expect.

One source of data that hasn't been looked at but could be a source of what the variability in the three-dimensional geometry of the fault would be, would be data from underground excavations, primarily from mining where oftentimes the source of mineral deposits are aligned along faults and some of the coal mines in Great Britain, for example, are very well documented and you can get a handle on what the expected variability in the geometry fault--if the fault is down dip.

We can also get a handle of it by detailed mapping right there at Yucca Mountain. There are already detailed strip maps being constructed, for example, of the Ghost Dance Fault, and we see that in different stratigraphic levels, different behavior of the fault. In some of the less competent zones, we see wide zones up to 10-30 meters wide of shearing. You move up section into one of the ridge form units and that zone of faulting may taper down to less than a meter wide. So we can expect variability down dip in the faulting and there are ways of getting a handle on it. Ultimately, the data to address that are going to come from the exploratory shaft where we'll have hands-on data to look at what the pattern of faulting is at the repository level.

I think in closing, in presenting this to other groups earlier, there's a lot of discussion about the use of probabilistic, and Kevin talked about it, and just to emphasize it, using probabilistic method of analysis to assess fault displacement is not a replacement for data. In fact, in my view, it requires more and better data on the behavior, timing of Quaternary faults than would be required for a strictly deterministic view.

I think with that, we're approaching lunch. I don't know if you want to open it for questions or save them for later.

DR. ALLEN: Okay. Bert, let's go right on into the final presentation of the morning, because I think that'll put us out of here about twelve-thirty. Then we can take up questions later, if necessary.

The final presentation is by Walt Silva, also representing the ASCE group, on ground motion.

Thank you, Bert. Indeed, there may be questions, but let's take them up this afternoon. In fact, I have some questions.

MR. SILVA: Well, this presentation is going to be on the aspects of the design guide, or consider the assessment of seismic design loads, and contributing authors to this part of Section 3 of the design guide is Carl Stepp and Robin McGuire.

We're dealing with vibratory ground motion. Sources of vibratory ground motion due to earthquake, of course, but we also have to consider vibratory ground motion due to excavation, thermal loading, and explosions; both UNE's, underground nuclear explosions, surface explosions, and missile impact as well.

The vibratory ground motions due to excavation are just induced seismicity due to stress perturbations resulting from material extraction. Significant levels are generally associated with very deep excavations, greater than a kilometer depth and high volumes of material extraction, and due to the relatively shallow depth of the repository, we don't consider that this excavation-induced seismicity to be really a significant or a controlling issue.

The vibratory ground motions due to thermal loading is again induced seismicity due to stress perturbations, but really resulting from thermal load. Here we're recommending a combined analytical approach to estimate levels of activity to get some kind of handle on frequency magnitude statistics. The combined analytical approach then is thermal mechanical modeling to estimate expected stress perturbations due to the thermal load, and then go about--in areas of the world where there's reservoir-induced seismicity, to try and relate the seismicity to the effects of the reservoir impoundment on the local stress field just to try to get a handle on expected levels of activity.

So we're back to, then, vibratory ground motions due to explosions. Certainly, the underground nuclear explosions we have to consider. We'll get into that very briefly, and then we have surface explosions and missile impacts. Surface explosions really are due to construction, and those ground motions due to surface explosions are very well characterized with typical blasting curves, and in missile impacts we don't think are a significant issue because very little energy gets into the ground from missile impacts. It's mostly a pressure, an over-pressure problem or an issue.

The approaches for specification of design ground motions, we have deterministic approaches, probabilistic approaches, combined deterministic and probabilistic, and that's what we're recommending is the combined deterministic and probabilistic, primarily because the probabilistic permits a formal treatment of uncertainty.

On the deterministic approach, requirements are explicit identification and evaluation of all the seismic sources in terms of magnitudes and distances, propagation path effects or ground motion model attenuation relations, and site effects, local site specific amplification which applies to both rock and soil sites.

The approaches or methods that are available in the deterministic approach, we have empirical methods, theoretical methods, the calibrated theoretical methods. We kind of separated out the theoretical methods into two types, stochastic methods, and a recommended approach, and our recommended approach is really a combination of the above, with a strong emphasis on the empirical.

On the empirical, it can further be separated. To look at data needs, it's perhaps easiest to separate the empirical then into a couple of classes; the site independent, which is really a direct regression on recorded data and sometimes referred to just as the empirical approach, then a little different approach, which is also empirical but is called site dependent or referred to as statistical, or the average of representative data, and that's where you basically try to get ground motion data representative of the magnitude, distances, source path and site conditions under consideration, and then scale them to the correct magnitudes and distances and do straight averaging and generate fractiles and response spectra.

Then there's also the indirect empirical relationships and calibrated empirical relationships, and the indirect empirical, it's really an attempt to expand the strong ground motion data base to areas where there are little strong ground motion data; try to make use of velocity data, or perhaps Wood-Anderson data, displacement data, applied to low seismicity areas. It's an attempt to determine how motions scale with distance for a given region, and the basic inference is that strong ground motion scales in a similar manner.

The calibrated empirical has a basic assumption that you assume the ground motions are region independent at close distances. You develop an attenuation relation where there are data at close distances for the magnitudes under consideration. Then you use the attenuation model which accommodates the regional differences in attenuation to correct or scale to the region of interest.

Theoretical methods, we can separate these out into a few methods. There's the purely theoretical, and in this method you have a complete analytical model for strong ground motion. That includes a source, a path, and a site. Currently, I believe that these methods are most useful for studying source physics. They have generally too many parameters. They are generally sensitive to a couple of the parameters, or overly sensitive in that case and they're non-robust, and to date, they're generally poorly calibrated. They're overall too deterministic.

In an attempt, then, to kind of de-tune the purely theoretical, another class of models has come about; the semi-theoretical, and in this class of models they combine the analytical with the empirical. You use a simple analytical model combined with recorded ground motions to reduce the number of free parameters. In that sense, then, the model becomes much more robust and this class of models is very well-calibrated.

There's also another theoretical approach which we separated out because it's basically quite different than a purely theoretical, and that is the calibrated theoretical, and here they assume an extremely simple earthquake source scaling relation. The relationship is then calibrated with small earthquakes. This class of models was really only applied in early attempts in predicting strong motions in the central and eastern U.S., and it's, as a consequence of being a very simple model or class of models, it results in an unacceptably high degree of uncertainty, and so they're just not used anymore.

A fairly recent class of models which is showing good promise in capturing elements of strong ground motions is the stochastic methods, and in this technique, the earthquake ground motions are considered random "gaussian" noise, a theoretically-based seismic source and way propagation parameters are used. An advantage of the method is that one can estimate these parameters with small earthquake data. You don't need to record large earthquakes to predict ground motion from large earthquakes. The model is extremely simple. It has few parameters. They're robust, and the model is also well-calibrated.

Recommended approach then is a strong emphasis on empirical and statistical, backed up where there is uncertainty on the empirical or statistical-based relationships, the modeling should be done with a stochastic and/or the semi-empirical approach.

The probabilistic method, the advantage of the probabilistic approach is that you can explicitly include alternate models, source models in terms of style of faulting, activity rates, ground motion models, attenuation relations, and also site effects. It also allows you a formal treatment of uncertainty, and you can accommodate dispersion in ground motion models.

The actual method of doing the probabilistic, you have to characterize the earthquake sources. You don't sort of defer that because you're using probabilistic. As Bert had indicated, you have to do just as much work, and perhaps more, in trying to characterize sources in the variability, the location and geometry, earthquake recurrence, maximum magnitudes of these expected events. You also need the ground motion models of site effects, then one does a probabilistic analysis and it results in a hazard curve where you have a formal treatment of uncertainty, as well as dispersion in the models.

Kind of a schematic of the process. In the first step, then you define the source geometry, the source and site geometry in terms of a distribution of distance and from the source to the site. You also have to define a magnitude distribution or occurrence rate for each source, which relates the magnitude and the number of events. Then you need a ground motion model, which here schematically is perhaps maybe peak acceleration, some peak ground motion parameter and how it varies with distance and with magnitude. Also shown here is an example of dispersion of the ground motion model.

Then one does a probability analysis where you integrate over all magnitudes and all distances, and it results in a hazard curve which relates the probability of a peak ground motion parameter, say peak acceleration being exceeded at a time interval T as a function of that peak ground motion parameter, and schematically, we have here that perhaps this is a median value and the fractile is a 1Σ, representing the uncertainties.

An example of alternative models, ground motion models, for example. We have three empirical ground motion models for peak horizontal acceleration versus distance, and for three different magnitudes. So the probabilistic approach allows incorporation of all three ground motion models, and as well, for example, at magnitude five, one can see some rather large dispersion in the models at close distances, and so one can also incorporate dispersion in these models that's formally correct because it's based upon regressions of empirical data.

The recommended approach then is again the combination of the probabilistic and the deterministic approaches, and the rationale for combining the two could perhaps be easily, or most easily demonstrated with a figure here. We just spoke about the probabilistic in the sense that it integrates over all magnitudes and distances, and that's really an advantage because you've correctly accommodated for the contributions of large magnitudes and small magnitude earthquakes. But then you can go back and de-aggregate and look at the contribution, and that's what this figure depicts here, where we have a per cent contribution versus magnitude for a given ground motion parameter; here peak acceleration and spectral acceleration at a couple of different periods, and this is for three different frequencies, and the point I just wanted to make here that, for example, if you look at peak acceleration, we can see that most of the contribution is coming from small magnitude earthquakes, less than five, and certainly less than five and a half.

If we, on the other hand, go and look at spectral acceleration, say, at one second, the converse is true. Most of the contribution is coming from large magnitude, or magnitude seven earthquakes, and this is where the deterministic then comes into play. One can go back and look at this in detail and to see what aspects of the source path or site are controlling these kinds of contributions, and if these things are internally consistent, both the probabilistic and the deterministic.

Now, characterization of ground motions due to underground explosions, well, you have to construct a ground motion model so it's identical to that of earthquakes. We need to have a model of how the ground motions change with yield, source yield, depth, and distance. It requires a large data set of recorded motions and we feel that that's reasonably well-constrained at the Nevada Test Site.

Subsurface ground motions. The procedure we're recommending here is that we specify the control or designed ground motions at an outcrop of competent rock. We define the competent rock as that having shear wave velocity exceeding 2,000 to 3,000 feet per second, and we would like this control point to be preferably located at the repository ground surface. We're recommending then that ground motions be propagated to the depth of interest for analysis purposes, with proper accommodation of appropriate wave fields; that is, vertically or inclined, compressional and shear waves.

A very, very important aspect of this propagation of the motions is the site technical characterization. We need an accurate representation of the three-dimensional variability of the dynamic material properties. That includes the P and S wave velocities, P and S wave damping and densities and how they vary in a three-dimensional manner because the best ground motion model in the world still won't give you the correct answer unless you have the right inputs to it, and it would also be of importance to have the degree of uncertainty in the dynamic material properties.

So the recommendations, then, again, the theme still comes through, I guess; the combined probabilistic/deterministic approach, and we want to have a strong emphasis on empirical ground motion models, and of course, you never have enough data of the region of interest, but these empirical models should be supplemented only with well-calibrated analytical methods, and that's all I have.

DR. ALLEN: Okay. Thank you, Walt.

Are there questions or comments? Russ McFarland?

MR. McFARLAND: I wonder if you would amplify on your comments with regard to seismic motion induced by thermal loading?

MR. SILVA: What kind of amplification do you--

MR. McFARLAND: I don't understand what you're saying.

MR. SILVA: Oh, okay. Let me put this slide back up again.

The idea here is that the seismicity would be due to perturbation in the stress field as a result of thermal expansion due to the thermal load. So you can do thermal mechanical modeling to try and see what the size of the perturbation you might expect from the thermal load. Then if you go to areas of the world that have, say, have had reservoir-induced seismicity, one can calculate the stress perturbation due to the impoundment of the fluids, and to try and get an idea, if you can relate the same sort of stress perturbation to the frequency of earthquakes, the level of seismicity.

DR. ALLEN: Are there any initial models or calculations that indicate this is realistic? I mean, since the area of the thermal load, the stress perturbation really is very small as compared to seismogenic depths, and so forth. I just--

MR. SILVA: Well, the type of seismicity we expect here from the thermal load is going to be very, very small earthquakes; magnitude zeros, minus one's, minus two's.

DR. ALLEN: Very low, I see. Okay.

MR. SILVA: Yeah, that are contained within the repository volume. We don't really expect them to induce earthquakes of a diffuse nature at great depths below the repository.

DR. ALLEN: So in that case, it might be very different from reservoir-induced seismicity--

MR. SILVA: It could be.

DR. ALLEN: --where we apparently have generated or triggered earthquakes of six and a half or so.

MR. SILVA: We'd be looking at this for very small earthquakes, a subset.

DR. ALLEN: Other comments or questions? Audience?

(No audible response.)

DR. ALLEN: Okay. Let us adjourn and return at one-thirty. We'll be on schedule then.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. ALLEN: May we reconvene, please?

We'll continued with the ASCE presentations and we'll reverse the order of the two on your agenda. First this afternoon will be speaking Mike Hardy on the underground facilities design.

MR. HARDY: I'm going to be talking about the seismic design guidelines for underground repository facilities. I wanted to, as background, indicate that a lot of this work that we're doing we've been doing with Sandia, and there's a report that pretty much goes through the design methodology that I'm talking about, particularly in relation to drifts for the underground drifts. The contributors in the ASCE committee that are mainly interested in the subsurface and the seismic design have been Archie Richardson, Assad Chowdhury, and Chris St. John has been involved as well.

Seismic evaluations are important in a number of items in the repository; a list there of repository design items. One that concerns me is the ground support for the openings, borehole linings and plugs, drift backfill design, seals designs. Also, there may be some concerns about particular items of the equipment for mining or waste emplacement that might be impacted if they're caught in an awkward position during a seismic event.

Seismic evaluation also, of course, is important in the performance assessment, which is postclosure concerns. Also, we may be required to do seismic evaluations of the underground repositories for--obviously, for performance assessment and to resolve issues that are raised by various people in interest.

Some of this stuff is pretty well-defined in previous speakers, but the time frame of the preclosure is 0 to 100 years. In the underground, we're concerned with worker health and safety, and maybe, in some aspects, related to cask accidents or handling. We're concerned with disruption of operations. If they were too frequent it may cause delays in the program and excessive costs. We're going to maintain the retrieval option. That's a concern in the borehole stability itself, and we're concerned about container life. That's in postclosure the container life is of concern.

The components of interest in the subsurface are the ramps and shafts, the main access drifts. Each of these components may have different requirements because they're being used more frequently and they may be open for longer periods of time. The emplacement drifts, of course, each drift will see activity during construction and waste emplacement, and then will be pretty much inactive until time of either retrieval of the waste or closure operations, and during that time frame--which is something on the order of 50 years--some events might occur, but if they don't have any impact on container life or retrievability, they wouldn't be considered severe. Some rehabilitation could take place before men or materials go back into those drifts.

We're talking about design, seismic design of underground openings and I just wanted to put this quote on the board to give the general background of the level of sophistication of subsurface design. It's not as well defined as for surface structures. There is not a lot of technical guidance and codes, design codes and things of that nature in the underground area. It's more experienced-based, analytical, and expert judgment that are involved.

The overall design process for the repository, getting information from site characterization, repository design is the thing I'm primarily interested in, but ultimately that design goes into performance assessment. There may be other off mainline activities in design confirmation or evaluation, and this process goes on in time. It's not a one-shot deal as you're aware of.

For drift design methodology, we've identified an overall approach for drift design where we consider the stability of the opening without the function of the ground support system as a first cut, and then we go on to the further design of the ground support system. This is based on the assumptions that the site is a reasonably good site and we've selected a site that is not that's going to require an excessive amount of ground support, which is generally considered to be true for the site.

But in this design methodology, if you look at that closely, you don't see specific reference to dynamic design for Yucca Mountain, although it is an integral part of the design and comes about in identification of loads and is taken account of in the simplified analysis and later on in a more detailed analysis.

Simplified thermomechanical analysis, we're going to be talking about empirical and analytical methods, and then we're going to break analytical methods into quasi-static methods and dynamic methods. Empirical methods, we've talked of rules of thumb and design charts which people have used for quite a period of time. A problem with those methods, they're not well-developed to seismic thermal loads and, of course, in a repository there are more significant loads.

Analytical methods, quasi-static methods are where we take the seismic load and calculate an equivalent static load. That's imbedded in the state of the art of design of underground openings using different rock mass models. Dynamic analysis is another layer on top of that where you've got to characterize the ground motions and the dynamic properties.

Ground support designs to resist seismic loads from an empirical base, it's common knowledge that you go down a sequence from no support to grouted bolts to reinforced concrete as the seismic load increases. Analytically, whether you're using quasi-static or dynamic analysis, you'd be considering the safety factors on various components.

The empirical methods which those of you involved in design of underground space would be very familiar with, this one is Nick Barton's Q method, and it's very useful in quantifying the ground conditions through RQD and joint numbers and joint references and joint water, and stress reduction factors. You put all those things together and for the Yucca Mountain site, I've got a shaded in box there indicating where we think conditions we're likely to encounter.

On the other axis is the equivalent dimension; larger openings usually use greater support, smaller openings lesser support, and this particular method--which is based on a lot of case studies--is rather insensitive to the stress reduction factor, which is the factor that incorporates the load or the stress on the system. It would suggest that if you have an additional quasi-static load from a seismic source, that it wouldn't really impact the ground support very significantly. But empirical methods are not based on seismic design basis.

This one is one by Hoek. This uses the same rating systems up at the bottom of the Q. The Nick Barton system at the bottom is the Bieniawski RMR system, and it gives you an idea of if you're in a no support, generally no support regime where this is the ratio of in situ stress to strength of the material, so up in this region very low ground support would be required and in this region a very high level of ground support would be required. I didn't show on this one what conditions we expect at the repository, but they are in this general region in here.

These are more quasi-static or general design methods for use in tunnels and underground openings. This one, put together by Birger Schmidt, gives another empirical index for rock quality on the bottom. It's a modified RQD. On the axis is the stress strength ratio and this one attempts to identify the modes of failure that are likely, so if we had 40 per cent RQD and this ratio of 0.4, failure would be interlocking, somewhat controlled; structurally controlled through here. As you get higher into the stress level, you end up going into the unstable due to rock bursts. At low stresses, of course, you're in jointed material, low rock quality. Then the material just falls out of the roof.

From this information at Yucca Mountain site, we're in this sort of range down here. We're not right up in this very high stress category.

From an empirical base, we've heard a little bit of empirical information from the Nevada Test Site in some of the earlier presentations yesterday, and people referred to this information here from Dowding and Rozen, but in terms of quantifying empirical information for seismic design, as Don Deere mentioned this morning, it's very imprecise and it's not well differentiated between the preexisting conditions of rock quality and stress load versus the impact of the small or sizeable seismic load.

This information was discussed yesterday in terms of peak particle velocities, number of cases, and whether or not you had minor damage or significant damage, and this is in terms of accelerations and this is the information regarding a breaking point of 0.5g. Above that, it seems that there is some evidence of damage and below that there is very little damage. But you can't--that's not universally translatable to other sites and other situations.

This one is from Owens and Scholl, and I think maybe one additional case on it. This one shows different explosive tests or underground nuclear explosions, and the particle velocity up here versus zones of damage, where there was a lot of damage or a little damage. So some of these are the particle velocities of inches per second, so some of them are fairly high. The information is very diverse and some of the empirical evidence that is reported is not--doesn't have the consistent definition of what is minor damage versus another case with what is minor damage, so it's hard to give universal rules out of that.

I show this slide as well because it's one from Sharma and Judd and a few people have used information from Sharma and Judd to try and get some information on damage in underground openings. They do attempt to break down the case studies into types of ground support, type of geological materials, depths, and a few other parameters, but the picture is still quite fuzzy in terms of where the damage occurs and how to use these as universal rules.

I think with a repository, with the added problem of we're dealing with thermal loads that add a significant amount of stress, incorporating the thermal loads and the seismic loads into an empirical-based scheme is probably not very likely.

For the more analytical type of methods, I've broken it down into quasi-static and dynamic analysis. The quasi-static analysis, you estimate peak ground accelerations and translate those into strains and stresses, and then you treat the far field stresses and strains if you like, and then look at the mechanisms of deformation around an opening, whether it be a canister or an intersection, and then also analyze the loads that you expect on the ground support systems, and then you can work in terms of safety factors on ground support. The approach wouldn't be very much different, bottom line, with the dynamic analysis, except it involves a full dynamic analysis.

To establish guidance between dynamic analysis, quasi-status analysis, I put together this flow chart of a couple of decision points. In looking at a design of underground openings, we're recommending following procedures that are contained in the report, 15910, authored by Bob Kennedy, the DOE facility's evaluation of hazards, but in that one they talk of establishing usage categories whether the consequences of some event is serious or not, so we have to establish those sorts of usage categories. I might show a view graph in a minute to identify what those sort of usage categories are.

In that, Bob Kennedy's report, they identify if you have moderate to high hazards for surface facilities, you need to go and follow a path that includes dynamic analysis. So we'd follow that same sort of logic for the subsurface; establish magnitude and frequency spectrum for the event; and then based on if the magnitude is greater than 0.5g, might need to have dynamic analysis. Then looking at the spectrum, if the wave length is less than eight times the opening diameter, yes, dynamic analysis. If the wave length is short relative to the size of the opening, you need to do this. If the wave length is very long relative to the opening, then we can go with the quasi-static analysis.

The quasi-static analysis has been used in a number of other applications discussed various places; St. John and Zahrah have a study supported by the National Science

Foundation; suggests this is a method appropriate for preliminary evaluations and when the wave length is long relative to the opening size. They essentially identified and elaborated upon, and the Subramanian Report was the working group report on the--to provide the seismic design basis for the exploratory shaft at Yucca Mountain.

Archie Richardson has incorporated into the preliminary shaft liner design for Yucca Mountain. Also, to mention just a historical note, it's also basically incorporated into other reports for the salt repository shaft, and myself and Steve have elaborated upon it for underground drift design for Yucca Mountain.

You could break quasi-static design into two other categories of whether you have soil structure interaction or not having interaction. Interaction, you could look at the strains imposed on the ground structure by the seismic wave and just assume that the structure that's imbedded in the rock is going to see the same strains as the rock without the structure being there, or else you can look at soil structure interaction, where the opening and the stiffness of the components that you're imbedding in the material are accounted for. That distinction is sometimes important in soil mechanics. In rock, because rock is stiff, that's not a bad approximation to just have a look at the contribution of the hole itself.

These just give you an indication of where we go in this. As I mentioned, we're looking at--on a drift like this we've got a--soils coming up consisting of the P-wave and S-wave. S-wave is usually broken into horizontal and vertical. Knowing the peak particle velocities, you can calculate strains, components of six strains from either a near vertical wave or a UNE, underground nuclear explosion.

It's important in applying this to look at the combination of loads that are appropriate, and often it turns out that the in situ system, plus or minus the seismic, might be as damaging to the system as, say, a later loading of in situ plus thermal, because the thermal generally increases the confinement of the horizontal stresses in the system, and the seismic can be considered as a positive or negative wave, and so sometimes that negative wave has a tensile stress or a tensile strain on the body and it, in combination with in situ stress, can lead to rock fallout or instability. But you've got to look at all those combinations of those loads on a drift or borehole liner.

In terms of modeling the rock, I show this view graph to indicate that modeling of the rock, there's a lot of different constituents of models for rock in the near-field around drifts and around boreholes, and because we haven't done extensive characterization of the site yet, it's hard to identify if there's any one of these models that would be preferable to any one other; indicate ranging from distinct block models which are U-date kind of models; interaction of blocks, these would be appropriate for highly-jointed materials in the low stress field, up to, say, equivalent continuum elastoplastic models in between those discrete joint models, ubiquitous joint models.

Currently, the program is recognizing all these may have different uses in different parts of the design process depending on what conditions we see. The design methodology is relatively simplified because of uncertainties in ground conditions. We translate the design criteria, if you like, to the ground support system and having a conservative assumption as to the loads that are applied to the ground support systems, but then we have to establish acceptable safety factors in concretes and steel liners, or generally rock bolts and shotcrete as possible types of ground support that are needed, with differentiation between the life and consider these the static loads and these are the dynamic loads, but when the dynamic loads are applied, lower safety factors are appropriate for evaluation. There is not really these sorts of things defined in the literature for underground design. That's why it's of interest that ASCE is focusing on these sort of numbers to see if they're defensible or reasonable.

In terms of the design loads or the seismic loads, and to put it in a little bit of context to the repository site, I wanted to just go through some of the logic that Bob Kennedy presents in the UCRL-15910, which basically supports the use of a probabilistic approach to establish the seismic loads, and the seismic loads are then dependent on the risk, the usage category as I mentioned earlier. Now, these words here can describe the usage categories, and this was established for non-nuclear path stations, but nuclear facilities, DOE facilities around the country.

I think for a repository, we haven't gone through the process of categorizing the subsurface space within these usage categories for the type of usage categories we think of are the opportune for the subsurface space. We don't think of the lower conditions applying to the underground repository. Of course, there may be various components or operations that will be considered on a lower level.

Associated with those use categories are performance goals for annual probability of exceedence (sic), and associated with those are, when you're talking about earthquakes for usage categories, hazard exceedence probabilities. So for the DOE facilities, if we're talking about important to low hazard facilities, we've got a probability of hazard exceedence of 1 x 10^{-3}.

Applying that sort of logic to this sort of diagram that comes from Yucca Mountain, this Blume Report that was shown earlier by Terry Grant as well, but this just gives you an idea that exceedence of 10^{-3} on this particular representation of the hazard at the site would give a G factor of 0.3g, and that is also comparable with what the working group in seismic design recommended from the Subramanian Report. It was about 0.3 for design purposes, based on a combination of probabilistic and deterministic and group therapy kind of approaches.

But if the hazard was to be low--if you consider for some operations the hazard would be low, then the design loads would be increased. Now, we've been using for preliminary design evaluations, we've been using the recommendations of the working group, which was the 0.3g, but also contained in that report was a recommendation that consideration be given to--for the subsurface design of 0.5g, so this view graph shows lots of numbers, but I wanted to show the relative magnitude of quasi-static seismic loads versus some thermal loads.

So these rock mass categories relate to the quality of the rock, and that's so we're dealing with the three-type rock, which is the most probable type of rock that we'll encounter at the repository site; our guess at the moment. The in situ stresses, these are megapascals of the order of seven vertical and three to four in the horizontal sense. The seismic load, this is now for .5g as of the order of 2 to 3 megapascals. The thermal stresses in the same rock type of the order of horizonal, additional 9 to a decrease in some locations, giving a combination of loads of this order of magnitude; 16 megapascals versus initial condition. That's a horizontal stress now. This was a--the vertical stress, there's no much change under those sorts of combinations.

So that's just an idea of, from the quasi-static point of view, that we're designing for about 3 megapascals. These ones down here represent better quality rock which has higher in situ modulous. Therefore, the effect of a strain generated by a seismic event is of larger stress. In the same token, the higher modulous rock will see a higher thermal load, so in the higher quality rock the stresses are higher. This gives still the relative importance of the seismic load versus the thermal load.

Limitations of the quasi-static methods, it does not accommodate rate-dependent phenomena. I'm referring here to a high rate dependent phenomena, not just the sort of creep or slower rate dependent properties. It does not accommodate for accumulated damage due to repeated cyclic loading; requires simplifying assumptions for combinations of wave types, and that's common to seismic design at surface facilities. It does not incorporate dynamic inertial effects, particularly related to block motion.

The dynamic analysis, the full-blown dynamic analysis has the problem that it is more complex and there's more uncertainties and more input parameters to try and define. It's not commonly used in the design of underground openings in rock. That is a truism. Very few places go to a full dynamic analysis, even in seismic and current underground openings. The methodology is not well-developed and dynamic code capabilities are ahead of material properties knowledge in general.

But on the other hand, it's needed to evaluate some of these concerns; one of them being to validate the quasi-static assumptions. Steve Bauer from Sandia was going to make some comments later on regarding direct applicability to the Yucca Mountain site, but if my recommendations at this point in time--specific to Yucca Mountain--is that it seems that given the size and dimension of the event that we're currently dealing with, that the design using quasi-static methodology would be appropriate.

However, these recommendations are more long term in nature, that they could be worked out on developing and quantifying an empirical database. For example, we had a lot of bad strains of 6.5 per cent causing collapse of openings because a number that may be relevant, or may--certainly is relevant to the unwelded tuffs, or what is the--how would you relate that to welded tuffs of higher in situ modulous; therefore, less deformation generally.

The quasi-static design methods also could be evaluated relative to some of these documented case studies that exist of seismic damage, mainly from the Nevada Test Site. Also, I think it's ultimately going to be worthwhile to develop a methodology for application of dynamic methods, just so that we're--that there is not any criticism that full dynamic analysis is not being done or that the problem of interaction of drifts and ground support in the sections with these sorts of waves that we might expect is fully understood.

That's the sum total of my presentation. Any questions?

DR. ALLEN: Thank you very much.

Are there questions; comments from the table? From the audience?

(No audible response.)

DR. ALLEN: If not, thank you, and our final speaker then will be Ken Mark.

MR. MARK: As the last speaker and possibly the shortest speaker for the ASCE group, that's not really an indication-- well, that is partially an indication of the focus that our group has placed. We've done a lot more emphasis on the underground facilities and characterization of the loads, but we could not be an ASCE committee without at least looking at some structures.

Today I'll be talking a little bit about the surface facilities, and I think the reason for this somewhat de-emphasis on the surface facilities is that there's a lot more published and available to describe the design of the surface facilities, and again, we did concentrate more on the underground facilities.

As an overview of the surface facilities, for the waste repository it is definitely different from a nuclear powerplant. The potential for dose release is much less. There may not be the need to shut down the same way you need to shut down a nuclear powerplant, and there's a lower radioactive content than in the nuclear powerplant.

There is a substantial body of design criteria and methodology already established for surface facilities, and we're not in the process of sort of re-inventing the wheel, but we would like to be able to summarize and comment on the criteria that's available.

There is good understanding of structural behavior and structural materials. There have been many types of studies from fragility studies, life extension studies that have been carried out for nuclear powerplants, and so there is a much better understanding of the structural materials in the surface facilities than there is of the materials in the underground facilities; and finally, the design life is a lot shorter for the surface facilities.

The facilities have classifications. One way of classifying them is whether they're important to safety or not, and again, the importance of safety is usually defined in terms of a level of dose consequence at the site boundary, not important to safety structures or structures with conventional designs, and the use categories that DOE has, you have the four different categories; the general use, the low, moderate, and high hazard categories.

I think as far as the current design methodology, the state of the art is well-defined. The consequences of failure of a surface facility may not be as high in the sense that a significant failure of the surface facility has to take place. You have to essentially have a large degree of collapse and breach of the walls of the facility before any type of radiation release.

Another way of looking at the design of the surface facility may be considering the cost benefit from use of a higher seismic design. One such study looked at the overall cost, not only initial cost, but the cost--the consequential cost and looked at that over the life of the facility to see if that could be a better way of establishing a criteria or a seismic design level.

There is an abundance of codes and standards that can be used to govern and provide guidance for the design of the surface facilities. There are ASCE documents, UCRL documents, the NRC documents, and concrete codes that govern the design methodology and provide guidance to the design and analysis of surface facilities.

In the area of critical loads, in the pre-operation period there is no radioactive material in the facilities and conventional design governs. Under the normal operation phase, there is some radioactivity, but governing normal operation loads are the dead and live loads and thermal loads.

In the abnormal range, you can have some accidental loads like cranes dropping and some loss of air conditioning or cooling within the facility. Under the extreme environmental loads, the seismic, wind, tornado, flood would govern, and other extreme loads may include something like an airplane crash or, in the case of Yucca Mountain, underground nuclear explosions.

We can look at a typical hot cell in the surface facilities and you'd have walls something on the order of four to six feet, and a roof on the order of three to five feet, and we could postulate, or at least get a general idea about what are the sequences and overall failure modes.

Initially, for some sort of an accident condition, initially we'd look at slight cracking, some spalling. Eventually the spalling would be large enough so that some of the reinforcing steel is exposed. Eventually, big enough chunks of concrete may fall out that we'd have holes. If you continue to load the, potentially the roof slab would start sagging. You'd have large shear definition of the walls and eventually the walls and roof structure could potentially collapse, and I think you need to look at the failure modes in terms of what sorts of levels of failure do we need before you actually have radiation release, and as long as there is no crack or hole that sort of goes through the structure, then the radiation is still confined within the structure.

We also looked at what are the component failure modes for the structures and what areas of the surface facilities, the failure of what areas of the surface facility that would have the potential for a significant release, and we've identified some of the areas that we think are important.

In addition, we've looked at some of the mechanical components that have the potential to release significant release; again, probably in combination with some sort of collapse of the overall structure so that the radiation, in combination with something else, would be able to release radiation.

I think in general, again, the methodology for the surface facility is well-defined and we will, in our approach, look at summarizing existing criteria and commenting on the criteria that is available.

Thank you.

DR. ALLEN: Thank you.

Comments or questions? Yeah, Leon Reiter.

DR. REITER: Ken, could you offer an opinion as to the reasonability or the possibility of designing against surface offset and whether that would require heroic measures?

MR. MARK: Well, I think you've had some discussion before about how rigid a structure might be able to do that. I think one study that we were involved with--and I think has been reported previous by Asadour Hadjian and others--looked at a study where we actually looked at parts of the hot cell and postulated some--a fault displacement beneath it and looked at the possibilities of designing, and I think it is possible to make a structure strong enough to accommodate a postulated fault disrupture.

DR. ALLEN: Yeah, Russ McFarland?

MR. McFARLAND: I'm curious as to the source of the critical loads in normal operation of surface facility, you've listed here "thermal" as being a critical load. Could you explain that?

MR. MARK: I wasn't--I don't think we--we weren't looking at it in terms like a nuclear powerplant-type load. I think if a certain amount of heat builds up in the structure and for--as it might effect cracking--

MR. McFARLAND: Oh, you mean hydration during the placement of five-foot, six-foot thick concrete walls?

MR. MARK: No. I mean the processes that take place within the hot cell may generate some heat.

MR. RICHTER: Phil Richter, Fluor Daniel. I'd just talk to that real quickly. There are--in nuclear process facilities there is some fairly--can be some fairly high temperatures due to the waste handling and storage, high relative to reinforced concrete and normal design practice. We've run into that type of thing on a canister storage building in the Hanford Waste Vitrification Project, for example, just recently, so it's reasonable for us to be concerned with thermal considerations.

MR. McFARLAND: I'd like to come back to this issue when we do the round table discussion. I think there's a basic question of what the problem is we're trying to solve in some of the designs that have not yet been defined.

DR. ALLEN: Other questions or comments?

(No audible response.)

DR. ALLEN: Okay. Let us take a momentary break until we get the tables rearranged here, however it is going to be done, and am I right on the--that all of the people who have made presentations are going to be up there, which means about half of us are going to be up here and half out there, so...

(Whereupon, a brief recess was taken.)

DR. ALLEN: It is not our intent here to have us against you and indeed I believe the only reason we are up here is so all of us can perhaps recognize who the culprits were who made various statements during the meetings that you might wish to question or endorse. And indeed, I hope those of you out there will not hesitate to participate just as much as those people who are up here. Some people, like Jay, I have a feeling will do so.

I made the statement that we were going to try to toss out a few provocative questions and you may have some of these yourselves. You may have some provocative answers. But, let me start out with a question that was actually stimulated and I'll direct it to first of all to Richard Quittmeyer. I am not trying to put him on the spot necessarily, but he was the first one to put up kind of a shopping list of things we might be doing here. But, I hope also others such as Ardyth and Keith and Dave Tillson and so forth might comment on this. And I pose this from playing the point of view of a devil's advocate as you will see.

At the start of the meeting, Terry Grant suggested that the maximum earthquake that might occur at the site in the near-field would be something like 7 or 7 1/4. Later David Tillson said that the Nevada Bureau of Mines suggests that maybe a magnitude 7 earthquake was the kind of thing we should be talking about.

Let's assume that. In fact let's add half a magnitude to it. Let's say 7.5 in the near-field, occurring at shallow depth, occurring in the worst possible fault orientation in terms of creating destructive ground motions of the site and ask the engineers, can you live with this? My hunch is in terms of the facilities of the site and I am talking about vibratory ground shaking, not so much fault displacement, my hunch is their answer would be yes, with the expenditure of sufficient amounts of money and so forth. And indeed, I should remind you that the NRC just recently licensed or re-licensed a nuclear plant, Diablo Canyon, assuming a magnitude 7.2 earthquake four kilometers away from the plant. That was an NRC action.

Assuming the engineers say they can do this and that is generally accepted, my question is this, why should we spend one penny more on things such as tectonic models, if we have already assumed the worst tectonic model and we can live with it, why should we do any further investigation of this field. Or something dear to my own heart, why should we run that seismographic network for one more week? What could we possibly learn in the next five years that would increase the estimate over the very large earthquake that we have already proposed. And, when I even suggested asking this question, my colleagues of Cal Tech, accused me of being anti-intellectual. But, let me toss that question out.

If we really think and I am not sure this is the case, but if we really think that we can engineer against an exceedingly large event, why should we be spending a lot of money doing work on things that might be considered, and I realize some of you might disagree, irrelevant?

Richard, since you tossed out this list of things from which I picked the seismographic network and the tectonic models, maybe you might respond first.

MR. QUITTMEYER: Richard Quittmeyer, Woodward-Clyde. I guess the first thing that would come to mind is that you would want to examine the sort of trade-offs and the cost between carrying out the site investigations that may give you a more realistic number for your seismic ground motion as opposed to the additional costs that it may take to design and license or a magnitude larger than may actually occur or be expected.

I am not sure that the long-term, that there would be a long-term cost benefit by just picking a earthquake half a magnitude larger than anybody has said so far.

DR. ALLEN: Okay. But, isn't it also true that no matter how many thousands of dollars we spend on studying tectonic models there is never going to be an agreement among the people that are arguing about it?

MR. QUITTMEYER: That probably is true.

DR. ALLEN: Well, Bob, do you want to say something?

DR. KENNEDY: I'd like to follow up on Clarence's statement.

From what I know of these facilities and that is maybe not all of the details, I don't see anything in either the surface structure or the subsurface structure whereby there would be much cost impact from raising the vibratory ground motion levels up to reasonably high numbers, let's say in the .5g, 18 inches per second peak particle velocity, maybe ten or twenty percent higher than that. As you start moving beyond those numbers, I think you probably will pay an engineering penalty and you will also pay a provability of the design penalty.

So, I guess I would like to toss it back to the geologists, seismologists and geotechnical that if you select one of these largest, very large events, what kind of ground motion are we talking about?

DR. ALLEN: Well, we selected 7.2 at Diablo Canyon in the very near-field.

DR. KENNEDY: But when we did that that got us up around 1g, tectonic acceleration.

DR. ALLEN: But the engineers were able to live with this.

DR. KENNEDY: With substantial extra engineering. There was cost penalties associated with that kind of a ground motion.

DR. ALLEN: Keith, do you have any comments on this?

MR. MCCONNELL: I think our view would be to take a broader look. In the narrow view you might be only concerned with vibratory ground motion of that magnitude of an event at the site. The questions we would raise would be, what does that mean with respect to fault displacement potentially underneath any surface facilities. What does that mean with respect to future events along faults along the repository block that could affect total system performance as well as containment.

So, obviously, from some of our slides, we do see some value to continue to look at tectonic models and their alternatives.

DR. ALLEN: You will recall that I accepted fault displacement. The reason being that I think it is a terribly important question. But, it seems to me the answers for that are going to come from these neo-tectonic studies in identifying where the faults are or what their displacements are. A tectonic model has almost nothing to do with whether or not you prescribe fault displacement on given faults. Likewise, a seismographic network tells you nothing about that.

MR. MCCONNELL: Tectonic models could become important in determining what could be the maximum credible event along a particular fault. I think if faults are connected via some mechanism that is proposed in a tectonic model, then some of the faults that could exist in an area such as Midway Valley, could have a significant maximum credible fault displacement based on the model that you do pick.

DR. ALLEN: But, no one has even suggested an earthquake as large as 7.5 might be credible, except me.

MR. MCCONNELL: Can we quote you on that?

DR. ALLEN: Well, I don't mean credible, I mean to be considered.

Well, Ardyth, do you have any comments on it? After all, you are spending the money.

DR. SIMMONS: Yes. Ardyth Simmons, DOE.

I think we have to remember that even though we may be able to convince the scientists, the geologists and also the engineers may be able to design for a facility that would accommodate a magnitude such as 7.5, we also have the burden of proof to the public as well, and, we have an obligation to complete site characterization. That is not to say that as we complete the surface faulting studies at Midway Valley and we can use that information to evaluate and re-assess the seismic hazard that we might not make some modifications and maybe adjustments to what the hazard and what the vulnerabilities would be.

But, we still have the obligation to be able to demonstrate it to the public.

DR. ALLEN: Okay. I certainly agree that that is a terribly important point. I would also say that if not the public was convinced, at least the NRC was convinced at Diablo Canyon would withstand 7.2.

MR. SULLIVAN: This is Tim Sullivan. I also work for the DOE.

With regard to the alternative tectonic models, I think it is important to keep in mind that those models serve not only to address seismic issues, but other issues as well. They will provide information on the three dimensional distribution of rock units from the water table to the ground surface. And while the principal driver may not be seismic issues, there is information contained in those alternative tectonic models that may influence performance assessment.

And secondly, in regard to the network, I used to work with seismologists, and my recollection is that they would look enthusiastically at the opportunity to monitor the occurrence of a moderate magnitude earthquake within their network.

DR. ALLEN: Oh, they would love it. My question is, what does that have to do with the safety of this facility?

MR. SULLIVAN: Well, I think, my concern would be if we shut off the network and such an event occurred, we would not have gathered available pertinent information.

MR. ALLEN: Well, as a seismologist I agree with part of what you say, but I would also point out that seismologists in general are always in favor of bigger and better networks and not always because they might help a particular facility to be more safe or less safe. And after all, the DOE does have a research program. This is not it. This is a Yucca Mountain program. And I think those differences have to be kept in mind.

Bert, you had something to say.

MR. SWAN: Yes. Bert Swan.

The regulatory guideline we are working to now is expressed in terms of probability of release of radionuclides to the environment. That is what we have to ultimately satisfy.

We could say in a deterministic sense we could design for and accept a magnitude of 7.5, but when we do the analysis, the complete risk assessment analysis, we don't want them to stake a conservative assumption because we can live with it, a magnitude of 7.5, for what is likely to occur. We run this analysis, the exposure is most likely not going to be coming from magnitude of 7.5 events; it will be coming from the more frequent moderate size events. Postulating a magnitude 7.5, if we postulate it and use it in analysis enough times, pretty soon that is going to become reality. We can have 7.5's out there.

My guess is, we haven't run the analysis, but my guess is if we postulate 7.5 earthquakes out there we'll lower the probability of release to the environment because we will soak up that moment rate in very, rare, large, magnitude events, decreasing the number of smaller magnitude events which are the ones that are really going to occur and the ones that could cause damage.

Although it sounds very conservative to postulate these big events because we think we can live with them, in reality it may not be conservative. It may lead us to lower numerical values of risk than would be if we took a more realistic approach as to what will happen in the probabilistic approach. And we aren't going to get away from the probabilistic approach from the standpoint that is where the regulatory guide is couched in probabilistic terms, probability of release to the environment. So, we are going to have to run these analyses. And we don't want to lose sight intuitively appears to be conservative. It may not be conservative at all.

DR. ALLEN: David, do you have something you want to say?

MR. TILLSON: This is David Tillson.

Let me give you a couple of examples of why you should not even think about doing no regional tectonic studies or developing tectonic models.

The first one that comes to mind which I was directly involved in and many of the people in this room were involved in when they were with Woodward-Clyde, is the Satsup Plant in western Washington.

In 1973 when we licensed that plant for construction, the concept of plate tectonics was more of a theory than a fact and we licensed it based on models that we thought existed at that time. We added levels of conservatism, the maximum earthquake was 7.1 magnitude. We kicked it up to 7.5 magnitude. Ten years later we go back in for licensing for operation and we are hit with the question, what is going to be the magnitude of the earthquake for the subduction zone that is directly under your site.

Now, the other case was over at Hanford where there were four reactors that had been licensed to at that time an intensity 8, which was .25g. Along came somebody that discovered that a large earthquake prior to the time of recording may have occurred over in the Columbia Plateau and we were faced with a concept of having to spend billions of dollars to go back and retrofit that design.

So, I think it behooves you to know as much about the tectonic model as possible before there is major expenditures on the design of that facility.

DR. ALLEN: Well, I think these are very good points. Please remember I was asking that as a devil's advocate.

MR. WESNOUSKY: I just want to ask Bert a question on that last point. I am Steve Wesnousky, University of Nevada, Reno.

You were just mentioning a possibility of magnitude 7.5 and that assumes that there is a fault there that can produce it. There is a fundamental tenant basically or a concept called the concept of elastic rebound. And that basically says that these earthquakes are the result of the release of slowing accumulating tectonic strain on a fault. And if we know the last time that fault released a strain, we can say something about the next time it will occur.

But, without that information, how can you do anything about really estimating the probability of a magnitude of 7.5 after assuming that it will occur, without that piece of information?

In other words, if we don't know the last time that one of these larger earthquakes occurred, how can you start to clothe your estimate of occurrence in a probability?

MR. SWAN: That was the point I tried to emphasize actually during my ASCE presentation.

Quite frankly, I think to do a rigorous probabilistic analysis, it requires more data than to do the old traditional deterministic analysis. You do want to try to get time and slip rate, the last time since the most recent event, and what has been the distribution.

Typically, what we do is assume uniform distribution through time because we don't have data to the contrary and it is mathematically appealing to do so. We really want to base our assessment on what the likelihood of future events is based on and what is the history of past events. How have they been distributed in the past? And, we can't gather those data for every single fault out there, but there are unique opportunities where we can gather it for select faults and those should be well documented.

Historically, working with the NRC one or two well documented cases where you build a high degree of confidence in what your data base is, goes a lot farther than expert judgment and supposition and tectonic models. And, to build the kind of confidence we need to license a repository, I think it behooves us to gather what data are within our current abilities, technologies, methodologies. The position of the ASCE working group is these methodologies, technologies are state of the practice and we just have to apply state of the practice and we can solve many of these problems quite easily.

MR. WESNOUSKY: I have some other thoughts just on some things that happened today and I know that David is leaving here shortly, so I would like to make--

DR. ALLEN: Yes. David is on his way out. Yes, please go ahead.

MR. WESNOUSKY: Some comments more in terms of our perception of how the earth works. My career in earthquakes isn't as long as most people's career here, but my emphasis has been on how faults behave and how we can use that understanding for seismic hazard analysis.

With respect to David's observation that earthquakes occur on faults, I would like to say I agree wholeheartedly in general.

DR. ALLEN: However--

MR. WESNOUSKY: But, I would just like to iterate a little history here in terms of how scientists like to embrace and even more consulting firms like to embrace an idea of how the earth works and apply it. There is this general once it comes out, this is how it works and we've got a number.

But, you can go back to the 1880's and everyone knows the famous geologist G. K. Gilbert, and he made the observation along the Wasatch that earthquakes occur on faults. And David showed that particular fault that he did in his study. In 1906 the Great San Francisco earthquake occurred and Harry Fielding Reed looked at some geodetic data and came up with a model of the concept of elastic rebound.

Okay, so then we have this information, earthquakes occur on faults and we have a simple model that says Earthquakes, we can infer occur periodically as a function of how often or how fast the strain accumulates.

In the 1920's Bailey Willis says, really all we need to do, to do seismic hazard is identify where all the active faults are.

A little bit later a guy by the name of Clarence Allen comes along reiterates that in the 1970's with a little more embellishment and observation. This is getting very accepted and used. And then David Schwartz and Kevin Coppersmith and Bert Swan start looking in trenches and there is this idea that earthquakes occur periodically at about the same size. Then there is this other guy named Wesnousky, that's me, I take these ideas and say we can synthesize this. And I am so bold as to take all the data and create seismic hazard maps for all of California. At about that time I think everybody is getting very confident about how the earth works and this is really a simple business.

But, simultaneously with that we get a couple of earthquakes. One, the Coalinga earthquake which I think is quite famous now, which wasn't associated with a fault that we could see at the surface, and the Loma Preita even more recently, the same thing. Then the Superstition Hills Earthquake as well produces a left lateral fault on a strike in an area that we never really considered as producing active faults. So three earthquakes occurred.

Now, I know what my geological colleagues are saying, we can recognize those now because we had seen them. But the point I want to make is that we are looking at California, a region with rates of strain accumulation on the order of many centimeters per year. We've been around for about 80 years studying earthquakes and we think we know how things are going and boom, there are exceptions.

Now we are going over to the Basin & Range where the recording history is probably even less, or maybe on the same order but the rates of strain accumulation are even less. So to think that with all our models and observations that we can really predict the style of earthquakes that might occur in the Basin & Range with the assumption that all of them occurred, is wrought with some uncertainty. And, I just want to convey that information to the panels and boards that be. I think that should be considered.

Another point related to that is that if we look at California we have these simple ideas of repeated earthquakes, but I would like to be a little bit cautious when I see these probability trees and the idea that we can produce a probability and give it to you and say that this is the number and it synthesizes all our uncertainties. But, what I would like to make sure that there is some effort to look at the probabilities that the probabilities are correct in a sense.

If we look at California, USGS, let's put together a number of scientists who have been willing enough to actually put probabilities on the forecast of earthquakes along the San Andres and San Jacinto Fault systems. Perhaps along these fault systems there is more data available for paleo earthquake studies just due to the geologic and physiographic environment than we will ever have in the Basin & Range, and they have put out these probabilities. But now there are members of the scientific community that start to look at these, and if you look at the uncertainties and the probabilities, you have to have some question that probabilities are of significance. That is a recent paper published by Jim Savage, I believe. So, I would like that sort of thought to come into being.

Moreover, I think the more data you collect on these faults, the more we realize they are complicated. And that comes out most recently on the San Andres Fault with the work of Kerry Sieh, where a decade ago as a result of his paleo earthquake studies, these earthquakes were occurring periodically. But with the advances in dating methodologies and more looks at the sections, these faults don't produce earthquakes necessarily periodically.

So, even though our models are simple, the more data you get it seems to appear that the earth is more complex than we would like to think. Although we embrace these ideas anytime that we can try and simplify in our mind how the earth works, we can't really be certain it is as simple as our models.

Another point that has been ignored today is that there is evidence of recent vulcanism out in the Basin & Range. That has been ignored. And we know less about the relationship of faulting to volcanic processes than what we do to say the San Andres and the slope tectonic accumulation of strain.

This is just an observation I want to bring up. I was recently in Japan and thinking about this idea of new tectonic faults. And the one well-documented case of a new tectonic fault occurring in bedrock without any evidence of preexisting motion is in Japan. It is a result of the 1966 Matsushiro earthquake swarm. Now these earthquakes weren't really large. The largest of them was magnitude 5, but there is some aspect here that new faults can occur and they have been documented.   It isn't a volcanic region, but I would like to emphasize that it wasn't as a result of an actual, there was no volcanic activity at the time, nor has there been historically. Nothing more than the fact that there are volcanic cones much like out in the greater flat.

So those are the sorts of things that have come up in my mind today and thank you for listening.

DR. ALLEN: Thanks, Steve.

Dave, do you have any particular comments you want to make?

MR. SCHWARTZ: Well, I guess I am going to miss the first shuttle, so we'll try and push it down to the second. No, I think I could probably sit here for two hours and point by point discuss things with Steve. I think he has raised some very valid issues and there really is variability in the way the earth behaves. We have got to take that into account.

DR. ALLEN: I guess Dave and I and maybe someone else here are on these Probability Committees for the Bay Area and Southern California. One of the interesting experiences we had, we were discussing the San Jacinto Fault one afternoon, a meeting in Menlo Park of several segments of San Jacinto Fault. When we got down to the south end of the Superstition Hills Fault, we argued and argued about what the slip rate was, what magnitude it might be, and finally after some heated and rather very differences of opinion we voted. The last thing we did was to vote that we just didn't have enough information to assign any probability to it.

So, I got on the airplane and while I was on the airplane to come back to L.A. the Superstitions Hills Fault broke, which--

MR. SCHWARTZ: True story.

DR. ALLEN: Which one thing points out that ignorance is by no means a sign for comfort. You shouldn't equate ignorance with safety in the case of studying faults and so forth.

Robin McGuire had something he wanted to say.

Thank you, Steve.

MR. MCGUIRE: Robin McGuire with Risk Engineering.

Let me get back to your devil's advocate question, Clarence.

I would respond by saying the reason we don't want to assume some arbitrarily large magnitude at a close distance at Yucca Mountain for seismic issues is the same reason that we don't want to do it and don't do it for nuclear plants in the Eastern U.S., for example. The reason is that you can't achieve optimum levels, meaning lowest levels of overall risk to the public from a facility by concentrating on one aspect of the design and assuming a very conservative level for that aspect. Because, as a result you'll probably incur a much larger risk in other aspects as a result of that decision.

A simple example comes to mind that was published by Harold Lewis. If you are designing a house in Pasadena you might be worried about snow loads, so you might say to be very conservative and design the house for snow load on the roof that is two feet of thick, heavy, wet snow. So you build a concrete roof for your house and you are very safe with respect to snow loads. The problem is, your earthquake risk has gone to hell.

That is an example of concentrating on piece of the problem and making decisions there in earthquake design would have negative influences and probably produce much larger risks overall to the public, than if you try to do the best job, get the best models and say we are not that dumb, we know something about the tectonics, let's make the best decision we can on the tectonics.

DR. ALLEN: Well, I certainly agree. That is one reason I pointed out in my introduction yesterday the need for a systems approach to this whole affair. Seismicity cannot be considered independent of many other concerns, transportation, engineered barriers, thermal loading, all these other things. As a matter of fact, the Board has felt that the DOE has not been addressing in that area as they should have been.

Bob Kennedy.

DR. KENNEDY: Bob Kennedy.

I have been involved for over 20 years on commercial nuclear power plants. I certainly support that you need to collect the seismological and geotechnical information and that this is extremely important in the design process.

I am also sure that in the past sometimes this information has been used to drive down the design earthquakes for which facilities were designed and that that was a serious mistake that plants have come to regret much more slowly as more and more information becomes available and people start questioning the size of their design earthquakes.

I do think you do need to collect this information, but I also think you need to collect information as to what is it really going to cost you if you raise the design criteria. There is some place where it is going to start really costing you. But, if you are below that place, don't sharpen your pencil to even one significant figure, I guess. Collect the information but don't use it to drive down the seismic design criteria.

DR. ALLEN: Let me ask another question. A number of people at the meeting today and yesterday have implied that if and when we get underground and let's say in a 100 foot length of a tunnel with good stratigraphy exposed in the walls, we can document that there is no visible fault displacement within that 100 foot segment, that they would be willing to say that it is sufficiently conservative now and protective of public safety and the environment, know the rocks are 13 million years old, to assume that there will be no further displacement during the next 10,000 years. Many people sort of implied, that yes, that would be a reasonable conclusion.

Let me ask, are there some people who disagree with that?

Kevin, in your presentation you were sort of, and you may want to explain this, you were sort of assuming that there could be distributive breaks, but maybe that was without the knowledge that it hadn't been broken if you could see that in the tunnel.

MR. COPPERSMITH: I think it has to do with your pretext.

DR. ALLEN: Yes.

MR. COPPERSMITH: Basically, to me you deal with how you are going to model the problem of fault displacement. We can go from a model that says that we basically don't know very much about where faulting will occur, which would be a very dumb model to one that says well we know where the primary faults, the big guys are, but we are not so sure about other faults and their deformation to one that yes, we are able to identify faults and this is where your basic pretext is that you have that information in a very site specific way within the excavation, let's say. If you have that, then I think you are able to exercise the smarter models that actually say that's okay, we have intact rock here, we have faults here and can spatially locate those. Given that pretext, then you basically can use those types of models.

The issue that I was addressing was where are we now in that state of knowledge? I think we are back a step in our detailed knowledge even in terms of the mapping of the mountain itself and the nature of the stratigraphy that we can see with the present level of site characterization it has got on it.

DR. ALLEN: Let me ask Dave Tillson, if that circumstance would arise and we are not sure that it ever will, but if we could get underground and find a 100 foot long segment, good stratigraphy with no visible offset, would you be willing to buy that in the next 10,000 years we could safely assume there is going to be no further offset?

MR. TILLSON: As a geologist or as one who would be responsible for trying to license it? It's two different issues.

DR. ALLEN: Okay.

MR. TILLSON: Now, I admit that the chances for licensing would improve substantially if you went underground and found no significant faults. I don't believe that you will find absolutely no faults, but I believe there is a possibility that you would find no significant faults.

But, this goes back to your first question. If you have not developed a very well structured and believable tectonic model, you are going to have trouble convincing people that new faulting could not occur. So, if you do have such a model and you do have a good understanding of the geology, I think your chances are very good.

DR. ALLEN: Any further comment on that question?

MR. TILLSON: I do have another comment.

You still have the problem of performance. We keep talking about design during the pre-closure of the operational phase, and I don't think that that is going to be the difficulty. The difficulty is going to be demonstrating performance of that repository once it has been closed up and that you know enough about the system to reasonably assure the regulators that you are going to meet whatever comes out of 40 CFR 191 or revised 40, or 10 CFR 60. It is a difficult problem.

DR. ALLEN: Yes.

MR. ROSEBOOM: Gene Roseboom.

Yes, I would like to consider the post-closure part of the performance of the repository. And, it seems to me what we haven't looked at here is really where the rubber meets the road or where the rock meets the canister.

We have two kinds of hazards, apparently, the seismic shaking and then also possible fault displacement. With regard to the seismic shaking, you are going to shake the canisters, maybe scratch them up, pull out some rocks loose, scar them and maybe increase the rate of which you get dissolution of the canister. On the other hand we can only take credit for 300 to 1,000 years for a canister anyway unless we make a special case for a longer lived canister.

What is the worst situation in terms of new breakage and suppose we share a dozen canisters. I think we really need the performance assessment people to tell us how serious is that if you do share a dozen canisters. You have only got about three millimeters a year of flux going through the repository and that is what would be actually meeting a canister and those are the basic barriers we are dealing with, the very low flux and then of course the other barriers once you dissolve some waste. We have the Calico Hills below it; we have a number of barriers.

What is the real consequences of seismic shaking and new fault displacement? Of course, Clarence at the beginning you suggested maybe we could emplace canisters simply loosely in a tunnel or maybe engineering solutions like that.

DR. ALLEN: Not loosely. We've got to tie them down.

MR. ROSEBOOM: Well it sounds like if a new fault breakage will go around a bank vault, maybe we need the canisters in a trench in the middle of a drift or something where there is flexibility for the new fractures to pass around them. If the canister is stronger than the immediately surrounding material for some distance, maybe that will take care of the hazard.

DR. ALLEN: Does anyone care to respond or comment?

Russ McFarland.

MR. MCFARLAND: It has been interesting, Bob Kennedy is brand new to this community, and yet he has asked some questions today that I have wondered about for the three years I have been in it. Now, Gene comes up with an issue.

I have been wondering for the two days watching, if everyone was trying to solve the same problem or if everyone had a different problem they were trying to solve.

In your introduction you raised a question of systems; you raised a question of different emplacement. Bob of course, raised the comment a couple of centimeters of clearance on the canisters is ridiculous.

We have nothing but a conceptual definition of a repository system. It has never gone beyond concept which is nothing more than a vague, general definition. And, yet I don't hear but very recently anyone in the scientific community questioning that configuration. We started to do it in our thermal meeting. As I said I am very sensitive to the thermal issues, since that was my burden. I listened to the hot cell issue in the design of a receiving facility. When is someone questioning the location, the need of, or the number of hot cells? Do we need a hot cell? Do we need three of them? Do we need them in Midway Valley? And, my perennial question, why aren't they underground if there is a potential for such risk.

And I ask you, presently, why aren't we questioning more of the basic premise? Why do we take as given gospel the reference configuration which is a conceptual configuration. And I follow that with a statement that has been made repeatedly by DOE that we cannot advance that conceptual design, and it really isn't a design, that conceptual repository until we have a great amount of site characterization data. And I am at a loss to know what site characterization data is needed to advance a conceptual design.

Do I have any reactions to that?

DR. ALLEN: Perhaps someone from the DOE would have a reaction?

DR. SIMMONS: Well I am not an engineer, but it seems logical to me that before you would advance the conceptual design, you would want to try to pin down a bit more what some of your various thresholds would be for fault offset and things like that. We can do some modeling studies given different scenarios and indeed disruptive scenarios are going to be a part of total performance assessment. There are scenarios that deal with faulting and ground motion as hazards. But, in order to have some confidence in that modeling, we have to be able to put in as much real data as possible.

MR. MCFARLAND: But in changing a configuration, for example, as Clarence mentioned, I could go to drift emplacement of very robust, large, multi-purpose casks, allow me to have much smaller, more stable openings. I could do away with my receiving facility, perhaps, depending on what the rest of the system requires of it. And if I need it, if I need a hot cell, I will go underground. I can change my needs such that a good percentage of the data that has been sought and obtained is no longer of value. Shouldn't we be looking or carrying along multiple conceptual configurations in trying to understand which particular configuration of features of a system best meets our need of building a repository to contain waste.

We call it a fly-off in the Air Force. Up until almost cutting metal, you may have two or three alternative configurations. Usually at the conceptual phase, we are not smart enough as engineers to determine what configuration is the most optimum.

DR. SIMMONS: Well, I might add just one more point on that with regards to doing system analyses. There is a study going on at the present time. It is in its rather incipient stages right now, being done in Washington that will assess various configurations of the repository and various conceptual designs in terms of both the configuration of emplacement of the canisters and the robustness of them. I don't know the details of exactly how many different concepts are being considered at this time, but I think that we will have advanced a little bit in that regard within the next year.

DR. ALLEN: Bob Kennedy.

DR. KENNEDY: I want to go on record as completely supporting you, of course I am an engineer, and there may be an issue between engineering. But it seems to me both should go forward concurrently. There should be more work than I have heard about here. And, is there ways to make this facility more forgiving of both shaking and differential displacement.

I am concerned that certain aspects don't sound as forgiving as I think they ought to be. Drift emplacement rather than borehole, at least conceptually to me sounds like it would be more forgiving. But, that needs a lot more study.

The idea of a universal canister and not having to have waste cells, sounds like it would make it much more forgiving as far as I am concerned.

The other area that I am really concerned about and I guess the Board has expressed those concerns previously, you really need to get underground with drifts not holes to find the faults that are down underground. That needs to happen very early in this program. They don't have to be that large of drifts. Now if you had a design that could work in smaller drifts they might be useful in the design. Later on you could broaden it if you had a design that you need bigger. But, you need to get underground.

DR. ALLEN: Don Deere is on an airplane somewhere, but I suspect he is hearing you and applauding.

MR. LUGO: Mike Lugo, SAIC.

I just wanted to respond to Russ's question and add something to what Ardyth said. The studies that you are talking about, these individual trade studies are planned and they will be done during the advance conceptual design which is sometime further down the road.

As you know, the focus of the program right now is on site suitability issues and the testing program. We heard all about that a couple of weeks ago in Arlington. So, it is not, I don't think that we are ignoring it, but it is that it will be done, it is just not on the time frame that we are talking about here.

DR. ALLEN: Robin McGuire.

MR. MCGUIRE: Robin McGuire with Risk Engineering.

Let me give a couple of words in reaction here both to Russ's comments and to Bob Kennedy's question earlier about why look at one centimeter or ten centimeters of displacement. And that is from the perspective of the performance studies that have been sponsored by EPRI.

The reason that we looked at those kinds of displacements and made some assumptions given the conceptual container design, the conceptual repository design, is that we wanted to make some simple assumptions to see whether or not earthquakes and in particular fault displacements underground have an effect on performance. The result that has come back, at least in this preliminary first cut analysis using one expert's model is that they don't make a damn bit of difference. The rates of occurrence of earthquakes and displacements that occur just don't amount to any more releases. And anymore I mean, not much more release than you would get from other sources of releases. So, you don't have to spend a lot of time justifying those models and designing a borehole liner to take 20 centimeters instead of 10 centimeters or making other decisions. And that is the value of those kinds of simple models, making simple assumptions with respect to conceptual designs and seeing whether or not that issue matters. In this case, at least in this preliminary cut, it doesn't matter.

Now, if that holds up with a much broader set of inputs, then we would say that it is not worth spending a lot of time making decisions on final conceptual design based on fault displacement issues, because, they just don't matter much with respect to performance assessment.

DR. ALLEN: Yes, please.

MR. FERNANDEZ: Joe Fernandez from Sandia National Laboratories.

I just want to mention kind of a similar analysis that I had performed about five years ago. I think in response to Gene's concern, I think we have to put it in the perspective of what really is the consequence from all this. The analysis that I had done five years ago where I assumed waste package failure for all of the waste packages in considering the amount of infiltration that you have, it didn't really make any difference. You could have a considerable amount of water, a highly unlikely amount of water coming into the underground facility and still you would be within, and my analysis was based on the 10 CFR 60 criteria, so with that very unlikely amount of water coming into the underground facility, we were still able to achieve the 10 CFR 60 criteria. I did not at that time apply my analysis to the 40 CFR 191 criteria. So, I just wanted to make that point in clarification.

I do think we have to keep this conversation focused and some of the criteria and some of the things that we are talking about here, ten centimeters, an interception of a fault through waste canister, I don't think the waste package people ever guaranteed that they would ever have complete containment. In fact, I think the words that are used now to the best of my recollection are substantially complete in containment. So there is some waste package failure assumed in the performance assessment calculations.

DR. ALLEN: Although, I think you would fully agree that other people have emphasized here that one of our challenges is to convince the public and politicians that perhaps these consequences and risks aren't as great as we think they are. That's a real challenge; a very great challenge.

Leon Reiter, when I faced you on various NRC hearings, you were always able to ask very provocative questions. Could you perhaps ask a question here?

DR. REITER: As Russ said, thanks, Clarence.

I want to give a provocative answer before I do that, to a question raised before about the need to do tectonic models.

Back in the late '70s when I was at the NRC, they went to build a nuclear power plant on the southern coast of Rhode Island. And, usually the kind of loading that was assumed there, pending on what you assumed the sources were or the kind of problems were like .15 or .12 or .18g, the utility decided to bring in a duplicate of the Seabrook Plant, which was at that point was .25g, the largest design used in the Eastern United States. Under the advice of their advisor, their consulting firm, which will go unnamed at this point, they wanted to come and argue that the site, even if it were built at .25g, the site would only require a .15g. And we in the NRC essentially told them to go stuff it because we were only concerned essentially with what .25g and whether that would envelope the family of models that we were considering and we considered a waste of our time trying to determine if that was really a .15g site.

So, I think the real questions are, I think the question that Bert raised, which I am not quite sure I understand, is whether or not from a probabilistic point if small displacements or small motions could be more important than the large motions. And, in fact, I really want to support what Gene said. The thing that I felt was missing at this meeting was some better feeling of modes of failure and consequences.

For instance, and this was raised by an initial presentation that we saw by Terry, was that we have multiple events. A lot of the experience that we heard here was based upon examination of tunnels or structures after one event. I still don't know and I would like perhaps Jay or Bob or those people to tell us, can we take that information and can we extrapolate that and draw conclusions about what would happen to this essentially sealed repository under the occurrence of multiple events. I sort of put that in all of the modes of failure kinds of issues.

DR. ALLEN: Jay Merritt, do you want to comment on that?

DR. MERRITT: The question of multiple events, of course, has been a major concern of the Department of Defense for a number of years and that was one of the reasons--

DR. ALLEN: Excuse me, is that microphone on?

DR. MERRITT: I think it is.

DR. ALLEN: Okay. Go ahead.

DR. MERRITT: There has been a concern for a number of years and that is the reason that in the presentation yesterday I mentioned that there were structures that were subjected to at least two loadings.

A number of people argue with me the fact that we have successfully designed and had survived structures that had seen up to 2400g's. Whether that is appropriate when you start talking about designing an underground facility for let's say 1/2g. I maintain it is relevant because at 2400g's you certainly will have exposed a number of modes of failure that you may not have seen had you subjected it to even just 2000g's.

As a matter of fact, the standard procedure for designing structure experiments at the test site has always been to array a design at different levels than you designed it for. And example, Dr. Deere mentioned the Hardhat Event. There were three structure drifts in the Hardhat Event. I can't go into the details of it because they are still classified, but the mid-range of the structures was the actual level to which those structures were designed. I should modify that. There were 43 structures involved in the

Hardhat Event. Three types of structures which involved six structures were designed for conditions that occurred between the mid-level and the more remote level of stress. But, the remaining 37 structures, they were designed to withstand the intermediate range and then they were arrayed at a range ahead of that and behind that to counter at least three things.

This was the first planned experiment involving "super-hard construction". So, we had to design that on the basis of data acquired in 1948 where high explosives were used to load on-line tunnels, totally on-line tunnels. The high explosives ranged in size from 320 pounds to 320,000 pounds. And we were looking at significantly higher equivalent energies than the 320,000 pounds. Further, as I already indicated, we were looking at "super-hard structures" of digression just to emphasize the fact that we were stepping into a new arena of behavior.

Second of all, devices even for a dedicated experiment were frequently experimental devices back in those days. So, you had to take into account the possibility that although you planned for "X" number of tons of equivalent explosive, you might get .8 of "X". Don't take that as gospel, I was just giving that as an example; you might get 1.2 "X".

Thirdly, there was the uncertainties in just how confident we were in both the design as well as the construction and so forth aspects of the thing.

So that's a long digression to tell you that we intentionally look for behavior as expected significantly higher than behavior to truly expose the modes of failure and less than that in order to give us some information in a statistical sense on behavior.

I belabored yesterday the invariant strain. And an invariant strain is well documented in reinforced concrete. As I pointed out ACI Code 318, latest version 89, implicitly uses .0015 as limiting strain for axial conditions of stress on the structure that uses double that point .003 for flexural behavior where you have got gradients.

That has been demonstrated, although there's excursions in that. In fact, the data actually show mean values of .004 as compared to .003 in the Code. There has been a lot of debate in the ACI 318 on whether it should be .003 or .004. But, I keep harking back to those data for reinforced concrete as a limiting value of strain. Tuff, whether it be welded or non-welded seems to fail at a strain in axial compression more like a half percent strain. Granted, that is up to perhaps 3/4 of a percent strain at failure.

So, if one things in terms of rocks being an analog of concrete, there may be limiting values of strain in which case you can use those data in order to validate a design if you will, whether it is subjected to a 1/2g or to 5000g's.

DR. ALLEN: Thanks. I am inclined to close this essentially on schedule because of the fact that many of you have airplanes to catch. Let me just ask if any of you have any final, very short statement you would like to make.

Some of you have been very patient, like you Burt Slemmons, one of the world's authorities on faulting and earthquakes. Do you have any comments you want to make in conclusion? We haven't heard from you.

MR. SLEMMONS: Burt Slemmons.

I just have two quick comments. I think I agree in general with what Bert said earlier and in particular Dave Schwartz with regard to the unlikelihood of having new faults rupture. Nevertheless, there are several factors at the siting area that may give a higher possibility or a higher potential for new faults than you normally have.

First there has been a major change in stress orientation of about 20 degrees during the last three million years. This has occurred since the east-west extension during the main period of development of the structures at Yucca Mountain at the 8 to 15 million years ago. This involves a major change with shear stress coming off the San Andreas system. This has been shown by both geodetic as well as geologic information. I think Kevin Coppersmith last year showed strike-slip faults generally have a higher dip, 70 degrees to 90 degrees, whereas the Basin & Range faults that originally developed the site typically have about 40 to 70 degrees. So, there should be with the slow release of strain, the slow rate of activity on the faults, a greater likelihood, I think, of new faults rupturing. Particularly on the eastern site.

If you have faults that are dipping mainly toward the west any new ruptures with a more vertical orientation should occur on the eastern side of the siting area. So, I would have some concern about that area. Nevertheless, from my studies for example in Fairview Peak area, there are perhaps only two small new faults that were generated during that earthquake.

Second feature is that half of the historic examples in the Intermountain Basin & Range Region, roughly five or six involved more than one fault rupturing tangential to each other anywhere up to as many as four or five. So I think that the likelihood of more than one fault at the site rupturing simultaneously, and having to deal with accumulative slip rates, is a much more reasonable feature than I would have thought otherwise.

The distance between the faults in that zone, some seven kilometers is within the range of the width of the distributed fracture patterns in the major Intermountain Basin & Range fault zones. So, certain amount of detachment and branching is likely, I think, in that zone.

The mechanism that Dick Hardyman had up in the area near Gilles Range and near Walker Lane next to Cedar Mountain zone involved a major but localized detachment system from strike-slip faults. This may show up in the distributive rupture pattern that Terry Grant showed yesterday or the Cedar Mountain zone. I think it could explain some of the width of fracturing that is likely to occur, perhaps up to several kilometers or five kilometers in width.

So, I think that the fracturing during the low activity rates of future earthquakes in the siting area are likely to be more complex than simple. We may have as Clarence has said in the past, surprises in the future.

DR. ALLEN: Thank you.

You have given several examples, of course, of how alternative tectonic models can drastically or significantly affect your evaluation of a hazard.

MR. GRANT: I have a response to his comment. This is Terry Grant with SAIC.

Burt, in the first part of your comment there about possible higher probability of seeing new fracturing, given even in your case there where you may have a new regime in the last few million years, presumably events are occurring at intervals frequent enough given that time period, if we are going to worry about them that the site would have seen the lot of those events already. Wouldn't it seem that whatever was going to occur would have occurred already?

MR. SLEMMONS: Only partly so. The change in stress orientation occurred about two to three million years ago and you have had a rather short period of time and it is a period of time during which you have a drop off in that curve that we saw of rates of activity. So, even though there are multiple events, I think you are in a relatively new cycle.

I think if you have looked at the Tsalenko clay-cake model sequence that he had in his publication of 1970, you see in a clay-cake model the strike-slip faulting and you have for close normal slip faulting a long period of evolution of fracture patterns and through a long part of the total period, you have new faults being generated. This is an experimental type of work. It is only in the later stages where you tend to get integrated systems that are well braided and very clearly defined.

The low sun angle photos that I think some of us have seen in previous presentations from John Whitney for example, shows the activity on many of the braided faults in Yucca Mountain involve discontinuities, the tapering off of activity on one trace and then picking up in another area. So, you still have at least nearer the surface a very complex pattern. So, I don't think the process is fully integrated.

I don't want to overstate the case. I agree with the idea that roughly 99 percent of the ruptures are recurrent and repeats of what has happened in the past and we are talking about a relatively small percentage.

DR. ALLEN: I would sort of like to wrap this thing up if we may. And Simon Hsiung had a comment that he apparently wanted to make in response to Leon's question.

MR. PHILIP: Jacob Philip from the Nuclear Regulatory Commission.

I would like to respond, Leon, to your comments about repeated earthquakes. We have this field program being conducted for us by the Center at the Lucky Friday Mine. And Simon did talk about the repeated mine site events and the affects on the opening and on the ground-water and he would like to restate those things again.

MR. HSIUNG: Simon Hsiung, Center for Nuclear Waste Regulatory Analysis.

I hope that yesterday my talk demonstrated a series of repeated seismic events where it actually caused more damage than it was supposed to have. The DOE's concept right now is talking about a maximum credible event as a design basis, and as a rock mechanics person or mining engineer, we all know that rock mass does show the fatigue behavior which means that if you subject a rock mass or rock specimen to a repeated cyclic loading, you will see marked decrease of rock strength. So, this is the reason that we have this Lucky Friday Mine to see how that the repeated seismic loading will actually have more impact than the right now current concept of credible maximum earthquakes.

Another point I would like to make is the seismic event on the impact on the ground-water, I think in the Lucky Friday Mine, that I am showing that at least seismic events will temporarily increase the water pressure that could raise the ground-water table. I don't see anybody here ever talking about that yet. I would like to know if you would have any plan for DOE to do that kind of analysis and what kind of implication that would have. And, how would that affect the performance assessment?

DR. ALLEN: Okay. I think with that I would like to call it closed.   I say that not only for some of you that have airplanes to catch, but for some of us who have to drive in Orange County, we may have already lost the battle.

Let me thank all of you from the various groups and there are so many groups I won't attempt to name who participated here. I have certainly found it valuable; I think others have. We appreciate your attendance and will look forward to seeing you at the next TRB meeting.

Thank you, very much.

(Whereupon, the meeting was concluded at 3:35 p.m., January 23, 1992.)