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NUCLEAR REGULATORY COMMISSION

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135TH MEETING

ADVISORY COMMITTEE ON NUCLEAR WASTE

(ACNW)

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TUESDAY

JUNE 18, 2002

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ROCKVILLE, MARYLAND

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The Advisory Committee met at 12:30 p.m. at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Dr. George M. Hornberger, Chairman, presiding.

SUBCOMMITTEE MEMBERS:

George M. Hornberger, Chairman
Raymond G. Wymer, Vice Chairman
B. John Garrick, Member
William J. Hinze, Consultant
Milton N. Levenson, Member
Bruce Marsh, Consultant

ACNW STAFF PRESENT:

Howard Larson
Sher Bahadur
Andrew C. Campbell
Lynn Deering
Latif Hamdan
Timothy Kobetz
Michael Lee
Richard K. Major
Richard P. Savio

ALSO PRESENT:

Derek Elsworth
William Melson
Meghan Morrissey
Ken B. Sorenson
Jeremy Sprung

A-G-E-N-D-A

P-R-O-C-E-E-D-I-N-G-S

12:30 p.m.

CHAIRMAN HORNBERGER: The meeting will come to order. This is the first day of the 135th meeting of the Advisory Committee on Nuclear Waste. My name is George Hornberger, Chairman of the ACNW. The other members of the committee present are Raymond Wymer, Vice Chairman, John Garrick, and Milt Levenson. Drs. William Hinze and Bruce Marsh, ACNW invited experts are also participating in today's session.

During today's meeting the committee will (1) hear presentations by several nuclear waste technical review board consultants on their perceptions on igneous activity efforts. (2) Hear an update by representatives of the Spent Fuel Project Office in Sandia National Laboratories on the current and future transportation safety studies and potential confirmatory testing. (3) Discuss preparation of ACNW reports.

John Larkins is the designated -- John Larkins is not the designated federal official. Strike that. Howard Larson is the designated federal official for today's initial session.

This meeting is being conducted in accordance with the provisions of the Federal Advisory

Committee Act. We have received no request for time to make oral statements from members of the public regarding today's sessions. Should anyone wish to address the committee, please make your wishes known to one of the committee staff.

It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so that they can be readily heard.

Before proceeding I would like to cover some brief items of current interest. Dr. Andy Campbell, Senior Staff Scientist, has returned to the committee staff. Dr. Latif Hamdan completed his rotational assignment and has returned to NMSS.

Phil Justus has returned from a stint in Nevada has the Yucca Mountain site representative and has relieved Dave Brooks as ACNW liaison. We thank Dave for his yeoman work and welcome Phil back.

Alabama, Florida, Tennessee, Virginia, and the Southeast Compact Commission filed suit June 3rd in the U.S. Supreme Court accusing North Carolina for failing to follow through on commitments to host the disposal facility. They seek $90 million in penalties. President Bush as reappointed Commissioner Merrifield to the NRC.

With that boiler plate out of the way, we will proceed to the important part of our meeting. We are very pleased to have with us today Drs. Melson, Elsworth, and Morrissey. We, the ACNW has been interested for some time in the issue of igneous activity with regard to Yucca Mountain.

In particular, the Commission had asked us to look into the consequence analysis, some of the preliminary consequence analysis that was done under NRC sponsorship. We knew that the Nuclear Waste Technical Review Board had several experts look at it for them and we appreciated reading the reports.

We are very happy that the same experts agreed to come and give us the benefit of their wisdom. We have three presentations and we'll go through those in order and then we will have ample time for questions and discussion. Dr. Bill Melson is going to go first.

DR. MELSON: Thank you, Dr. Hornberger. Can you all hear me? Can you hear me in the back?

CHAIRMAN HORNBERGER: Mike.

MR. LEE: We would just like to remind everyone listening that the views that are being expressed are those of the consultants and not necessarily reflect the views or positions of the TRB.

DR. MELSON: Okay. Thank you. I'm going to look at the CNWRA inputs into the program, but I'm also going to integrate that with what DOE has done and other groups have done in moving forward the volcanic consequence analysis. I'll also look at near the end what more can we expect and what more do we need to reach some kind of closure on the volcanic disruption issues.

In 1968 Arenal Volcano in Costa Rica had a large explosion. Actually, a series of explosions which destroyed about seven square kilometers. I'll show you how this connects to this in just a second.

About seven years later -- I had been studying the volcano at that time. About seven years later the power company in Costa Rica decided to build a large earth-filled dam in that region. The power company hired a board of consultants that included volcanologists and they went in and gave a report.

When the Inter-American Bank visited the site because they were going to fund it, they found out, in fact, that this large volcano was sitting within seven kilometers of the earth-filled dam site.

I was asked to serve on the board of consultants for the Inter-American Bank because they didn't believe the previous report was sufficiently objective. On that panel also was a man named Bon Deere who became one of the first chairmen of the TRB.

I got involved there because of a similar incident. Around 1992 -- I believe it was around 1992, a man named John Trapp, who is sitting here, stopped on Yucca Mountain, or something to that effect, and looked out and saw these cinder cones all around the site and became alarmed.

Thus began an intensification of the volcanic hazards studies and Don Deere asked me to start working with the board on the interpretation of DOE and other work done on volcanism.

This is a really large shockwave it involved and the volcano is still active. As a matter of fact, Leon Ryder could probably give you an update. He was down there recently. It does produce shock waves still. So-called flashing arcs.

Now, the kind of volcano we have in Central America is a subduction zone volcano. Very large and repeatedly active in the same place. If you look here, we are talking about scattered volcanos in the southwest.

Just incidentally, this is a wonderful program available on the web which allows you to call up any part of the regions of the earth and look at the earthquake and volcanic picture since 1960.

What we are dealing with, of course, at Yucca Mountain is not the kind of thing that happens at large. Water rich magnetic explosion eruptions like Mt. St. Helens. It's more like we do see in some subductions on volcanos. This is the Cerro Negro in Nicaragua where Chuck Connor of the CNWRA expressed some concerns that this was somewhat similar to some of the Yucca Mountain volcanos but much smaller activity, much lower volume.

This is an interesting one that we have a big gas magnum coming out the base whereas we are getting pyroclastic eruptions simultaneously from the summit cone.

Here is our picture at Lathrop Wells and the Crater Flat field. Here is a very small volcanic field. Very rare eruptions and very small. There is a more active one to the north of Lunar Crater which I'll come back to, a volcanic field.

Up here we have the trace of the Yellow Stone hot spot. Very large. The Snake River Plains are here. Very large and a lot of volcanic potential up here with much less in this particular region.

Here is the Yucca Mountain volcanic field. Here we have the duration, a million years of activity. The activity there is about 4.5. The volume is about as small as one can get. Here the lunar crater is larger. If we look at some of these other fields like the fields up here, we see tremendous amounts of activity compared to what we see in Yucca Mountain.

Let's go into what more is needed concerning the probability of disruption and the consequence of intrusion and disruption. This is a picture that many of you have seen before. Here is the footprint of the repository. Here is the Lathrop Wells cone. Here we see the Pliocene and a Quaternary volcano unit here in Crater Flat field. Some varied anomalies here.

This doesn't show topography but this activity here is mostly within rift-valley sequence. We have small volume basaltic eruptions and they are mostly monogenic cones. That is, a single episode eruption produces a cone and it's dead. It's gone.

Again, so far the activity is restricted to the rift-valley just west of Yucca Mountain. There are some cones scattered and far away from it but this is by far the most of them.

The probability of dike intersection were estimated many years ago by Bruce Crowe and his co-workers at about 10^{-8} per annum. Recent estimates are close to that with the NRC estimate slightly higher, about 10^{-7}. Anyway, regardless of what we think about Yucca Mountain, the intersection of the repository by a dike, the probability is very low.

If we look at the activity through time, we have a thirsty mesa large volume activity and then dropping, dropping on down to the Lathrop Wells cone. Recurrence rates are very low, 10^{-5} to 10^{-6} per year.

In the simplest sense the recurrence rate in the region of interest, and this can be defined differently, and has been defined differently by different people. In other words, they drew different boundaries around it.

Plus the possibility that recurrence will, in fact, intersect the repository. That depends on dike length and dike abundance, a whole bunch of factors. There are a lot of possibilities here. The results normally range between 10^{-6} and 10^{-9} per year.

Real quickly some benchmarks in the probability of disruption. In 1980-1990 I mentioned this was DOE's work with Bruce Crowe. In 1995 the first higher estimates began to show up. Conner with the CNWRA and Ho and Smith came up with some higher values.

Chuck Conner's work was very important in that he introduced the concept of working with curl statistics where you have decrease in probability away from the center instead of uniform probability over the broad area.

The mid-90's there was a rather hot controversy was resolved between most of the USGS scientists and the Los Alamos group. The Los Alamos group believed that Lathrop Wells cone had formed by repeated eruptions, a polygenetic cone.

This was an unlikely hypothesis because normally we think of those things as monogenetic. They base it on topography and the very uneroded nature of the cone and other features suggested it had in fact been polygenetic. That was resolved, I think, to most people's satisfaction as being a monogenetic cone about 75,000 years old.

Because of lots of controversy and lots of spread, DOE convened this Probability of Volcanic Hazard Analysis in 1996. I'll go over that briefly. Now what still needs completion is the idea of buried magnetic anomalies because these can change the probability of interception, albeit I think quite small. A very small amount.

This was the PVHA expert panel. This was a new approach to me at the time. Always it was one person had an idea and another person had an idea. What Coppersmith did in his company, they pulled together all these different folks, interviewed them independently after having them do the work and came up with a study of their statistics as well as the statistics they came up with.

They came up with an estimate that I'll come to in a minute but here is how they -- just to give you some idea of diversity, Dick Fisher defined his volcanic zone of interest by this one line, McBirney another line, and so on.

Each one had a different feeling or sense of how they wanted to do this work. No one drew their circle say from Lathrop Wells over Yucca Mountain as some of the other studies not done by these folks had done.

Anyway, this is the final analysis that came out in 1996. Here is the mean at about 10^{-8} in here. These are some of the other values. The thing to notice is most of them are falling within the same cluster.

These things, most active fields and the Cima and Lunar Crater fields, they project what would have been true, a very high possibly intersection in these very active fields compared to the very low probabilities in the Yucca Mountain area.

Work that needs to be done. There's a lot of newly recognized magnetic anomalies. Here, by the way, is the repository. Here is the scale down here. You can probably see that better than I can. Anyway, this is a broad regional scale. That work is underway and will be completed.

Recently we had a little excitement come out because of this article by Gene Smith in GSA Today which indicated a tremendously higher risk to the site. What he noticed and drew a line between Yucca mountain and he drew this line here all the way up to the very active lunar crater. Because of this he felt this would be a more active region that we had anticipated. In fact, there would even be some coordination between these activities.

The problem with that is this line is, I think, a very artificial line. There are no major young volcanoes along that line. Furthermore, the chemistry of this system, particularly the neodymium isotopes and this system are totally different. There is, in fact, we feel -- at least, I feel, very little relationship between the low activity here and the high activity here.

Another thing that Gene came up with that was interesting was kind of a correlation between the level of activity through time. This is the age of the activity and he has some peaks and he puts them together and comes up with a total activity of this red. It shows a similar peak. Whether that is fortuitous or what that means is uncertain but it certainly doesn't mean that they are tightly connected in the future.

Let's move on very quickly to the consequences of disruption. In the early 1890s there was some looking at lithic contents of eruptives. The reason that is done is, of course, is to see.

This work was done under contract with the DOE by Bruce Crowe, Link, and others. In 1990 the release-based requirements were put into effect. DOE began examining factors governing dike and sill formation. They again looked at lithic contents of analog volcanoes, and they assumed back-filled drifts in their thinking.

They terminated this work about 1/3 complete due to low probability and other programmatic factors. Mainly, I think, funding. They are proposing now, and this will be talked about more later, to resume those studies based on the fact.

1995 the transition to dose-based requirements in regard to assessing hazards. In 1998 there is a new design, large packages and back-filled drifts. This is critical in back-filled drifts.

Volcanism was then recognized -- relied mainly on literature and idealized calculations. In 2001 it was understood that DOE had to redo their work. The CNWRA had a real big role in pushing this and they came out with looking at shock processes that would be caused by a dike interrupting the drifts.

They came out with various papers, one of which may have been published by now by Wood and others. They used steady state, pseudo-fluid flow into and through drifts. Meghan Morrissey will talk more about that in pursuing it.

I would like to just say the NRWRA had a real and critical role as a catalyst in doing studies that probably would had to have been done anyway. DOE is now taking that ball and running with it.

DOE is a peer review process that started this year. They looked over all the work done and the plan work on consequences. What we are also seeing now is the ongoing resolution of DOE and NRC issues. So as I stand before you alot of work is going on that will be reported on. I think January of February the peer review group for the DOE will be reporting. Hopefully NRC and DOE will continue to resolve some issues they have. In a way we are on the court now as I talk to you. Things are happening and those things I think are good things.

This is the paper which included Doubik and Brit Hill looking at magmatic and hydromagmatic conduit development during the Tolbachik eruption in Kamchatka.

In this particular eruption what they showed was that the conduit can be cord at different points in the eruption. That process is important in Yucca Mountain. If this does happen, it would result in much greater emissions of potential radioactive material.

This is their stratigraphic sequence at Tolbachik. The earliest thing were fire fountains as we often see in these cinder cone and fissure eruptions. Then there are outbursts of lithic rich material from the conduit. They attribute this to the drying out of the conduit, water coming in, and having magmatic explosions eventually ending with another return to fire fountain activity.

This is simply their little cartoon showing the widening of the event and how deep it went. They knew the totigraphy here so they could reconstruct the depth of the coring of the conduit. Here we see a scale of five kilometers, this is some of the lithic rich action. An air pocket has been found, although one wonders how good it is, in an Iceland borehole. In 1977 an eruption came up the borehole. It was a very small eruption. There was an initial explosion and then within 15 or 20 minutes there was no activity. Then a series of closely spaced explosions.

The total volume or deposit was 26 m^3. There is a question in my mind how analogous this is to anything that might happen at Yucca Mountain.

The problem with all analog studies of a complex process such as a consequence of intrusion into Yucca Mountain is that one analog is just not enough in a complex process. It would be -- if I make it the worse case, it would be like one medical case proving something about a major disease.

Instead we have a real problem in finding enough statistics to give a meaningful result. We're talking about anecdotal evidence. Therefore, not to put that evidence down but we must not think that one analog is going to give us the answers to Yucca Mountain, or even two.

The peer review panel, which was mentioned, is now working. I think the report will be January or February of next year. In regard to the volcano things of my interest, these two are particularly important as is Larry Mastin. Allan Rubin is interested in dike placements. Frank Spera is an expert on magnetic properties.

In terms of some of the work done in the last few years on the intrusion, volatiles is a very important part of that work in that they control the explosivity of the magma.

I've got just a little cartoon here for those of you not familiar with how the water content affects eruptions. What we are going to look at is a cartoon of the system albite and aluminum silicate as a function of water.

Here is a phased diagram of albitic melt and this is the water content up to 8, almost 9 percent water. This is the pressure in kilobars. That can be converted to an equivalent depth. Four kilobars we are down to about 12 kilometers.

In this region the load pressure is sufficient to stop any vapor forming. Now let's go into the cartoon part. Imagine we have a rising plume of water-rich magma. This is about 7 percent water.

As long as this magma is rising within the zone of undersaturation water not much happens.

As it approaches the water absolution curve things start happening, the warning curve. Vapor now exceeds the low pressure. The ground starts expanding. This is about the time, for example, that Mt. St. Helens when the alarms start because at the surface there is a lot of activity.

I want to just insert here as someone who studied active volcanos, it is always -- I have always been the minority and say why doesn't DOE -- if we take volcanism seriously, why not simply keep a seismic net operating. I don't know why that isn't but there will be seismicity in the region before anything happens.

Then you have major deformation and eventually, of course, those figuring out what happens realize they were watching the rise of water-rich magma. Cerro Negro indicates more what happens when the degassed magmas reach the surface. You have low plumes and, of course, that lava fountain and lava flow field I showed you.

Typical Aa-flow. This is very much like the ones you see in the Crater Flat area in your Yucca Mountain, quietly moving Aa-flows. If something did happen at Yucca Mountain, this is the picture it might follow. You have a dike intersecting repository possibly reaching the surface.

You have fissure eruptions at the service. You may have activity going on in the conduits if it doesn't. You will have some. Eventually after the fissure eruptions you have a central volcano forming and this is pretty much a universal pattern for the monogenetic volcanos.

Often as at Pericci Tin you'll have some days of quiet, but then you'll have a large eruption. You'll have violent Strombolian activity. This is the kind of thing that could disburse should it disrupt the repository and, in the worse cases, some of the high-level waste.

These kind of explosive volcanic activity alternates with effusive lava flow activity and Strombolian activity, the kind I showed you at Cerro Negro. Often this sort of cone-building phase is the longest phase of monogenetic volcanos. It involves small pyroclatic eruptions and lava flows.

The work by CNWRA on intrusive consequence has been helpful, including shock wave consequences, and will be commented on by Meghan. Again, I would like to repeat myself and say that the work done by the center has impelled studies. It's been helpful and constructive. I dare say we would be a little further behind at what we have to look at if it hadn't gone on.

There are three magnetic parameters, however, that didn't go, I feel, sufficiently into the early models done by both DOE and CNWRA on the consequence. One is the process of when you have a magma water-rich expanding you have adiabatic cooling. That causes a rapid fluidification in many cases.

The other thing is if you read some of these papers its almost as if magma can melt its way through anything. This is not true. Magma has a limited capacity to melt other materials without forming a solid glass or crystallizing themselves. The final point is the pressure that is likely to be generated is uncertain but this very high pressure of two kilobars would be at the very upper end. The lower one is far more likely.

Just a bit of data from Greg Valentine's work on the water contents. Just to show you the importance, here is a 4 percent magma. The saturation pressure here is very low and, of course, very high as you get into higher contents. Note also lots of other things happen. Water has a very powerful effect on the viscosity of magmas and lowers the density of liquidus temperature.

I wish we could get a magic number that would predict what happened or what the magma would be like that might rise beneath the Yucca Mountains in the future. There is a diverse spread of values and we have to, I think, deal with this kind of a spread.

Here are some features of supposedly erupted pumices that had high-water contents. You'll see even though they have the foam texture, they are solid. This is partly due to the adiabatic expansion. The same thing happens when you let air out of a tire. The cold you hear happens when you degass a magma violently.

The lack of excess heat in magmas. Most magmas have mixtures of solids and a liquid. They are below the liquidus. What that simply means is if you take heat out of them by any kind of interaction, you are going to cause more crystallization. They don't have a lot of capacity to do other things.

This is just one of the cartoons from the DOE analysis showing bombs plastered to the canisters, the dike intersecting and a shockwave moving out and a whole series of processes.

DOE tried to define different zones of

interaction. In Zone 1 all the canisters would be destroyed. In Zone 2 they would be highly compromised. In Zone 3 they may escape unaltered.

One of the things that comes up again and again is what happens when the magma hits the canister. That is not yet subject to sufficient analysis. I think we are hoping that it will be in the future.

Summary: The probability estimates I don't believe are going to be greatly changed by the additional work. The magnetic anomalies may change it but it's also true that they enlarge the area we are considering. If we change the footprint of the repository, that will increase the probability. Remember we are talking about very small numbers.

The main missing analyses I think concern the consequences of intrusion. Past work by DOE and CNWRA has been helpful in moving process along but must now be extended by a broader approach to take into account parameters using long-tested code and will be done by Gaffney as proposed by DOE.

For example, he does include expansion cooling and a whole bunch of other parameters which in the initial studies were not able to handle by their approach.

DOE is proposing to study the Lathrop cone a bit more closely in regard to the lithics in it. They propose this work. The code for ASHPLUME I, which was introduced by the center, is going to also be looked at to see if that can be improved in any way. It will simply be examined. At least, that is the proposal.

Another thing that is going on, of course, is the DOE peer review. That is very important. The DOE and NRC exchanges, which I have been lucky enough to attend, are very useful in moving toward resolution of certain items and they are still underway.

This is my perception. The work on volcanic hazards that needs to be done is either proposed or underway. We all see surprises in this program. We don't know what the next alarm will be. My perception -- and I've been involved with the program ever since John gazed across from the Yucca Mountain to the Cinder cones -- is that we've made tremendous progress. I'm excited about what's coming down the road.

That's about all I had to say.

CHAIRMAN HORNBERGER: Thanks very much, Bill. We'll take some questions for Bill as we go and he'll be available later as well.

Bruce.

DR. MARSH: Bruce Marsh from Johns Hopkins. I think Bill touched on very many good pertinent points. For example, a dike flowing along can only flow so far because the flow of magma is normal to the conduction of the walls so it can never erode back the walls thermally. In other words, they are thermal moving out from the cold wall rock continually going in and trying to chunk off the magma at all times.

What this does it reseals the system up so after you get magma flowing in these systems, it tends to make a chilled margin on the edges. Everything it touches it chills. I could bring in a piece of solid just like this, for example, that has a piece of crustal rock in it and you would see chilled glassy material around the outside of it, that piece of foreign rock.

Even mantle zenless that come from quite deep below the crust, they also have chilled magma around them. In other words, when Bill is mentioning about the interaction with the canisters it is an extremely pertinent point that anytime a magma touches anything like that, it will actually chill out around it and form a glass container basically around it.

If the waste container can expand and explode, for example, it will break but will chill again right around it so it will basically make a glass container. This is very important to take into account the whole solidification process. I'll say a few things and I'll show a little bit about this later maybe.

The other thing I would like to say a few words about is the whole essence of the nature of the systems, as Bill also mentioned, about the size of the systems that we are using for analog models. Almost all the systems that are mentioned in all of these things as analogs are large ongoing volcanic systems that have a lot of mass behind them, especially in Iceland.

We have a system that's been going for 20 million years, for example. They have a lot of magma behind it and there are lots of things that can go on. For example, you can have volatiles concentrating in various parts of the system. In other words, a small amount of magma can have an inordinately larger amount of volatiles with it that may collect in the system.

This is very important to get down. In other words, the small volume systems that we see here in Yucca Mountain area means that the amount of magma available, the amount of thermal inertia in the system is really rather small. These examples that we see from Cerro Negro and Stromboli are so much more massive and ongoing.

There is also a compositional effect. In other words, the whole idea -- I think Meghan will probably talk about it later but the whole idea of shockwaves coming out of systems.

They are mostly large silicic systems and the conditions you need to set up the right kind of initial conditions for a shockwave to come out is much better developed in a long-term system, a system that is capped up and bottled up. These systems I'll talk about a little bit later. I'll show you some pictures.

They are leaky systems when you have systems that are propagating dikes out and systems like this that are just trying to reestablish themselves. The systems are very leaky and they tend to dissipate themselves rapidly. These are basically somewhat of an implication. Not a question of what Bill is saying but I thought it would be pertinent at this point.

DR. HINZE: Bill Hinze, Purdue University. Bill, you alluded to the need to have more than one analog in our considerations. As geoscientists and engineers we like analogs, don't we? I assume that you are referring to the Icelandic drillhole explosions.

I can't think of anyone that has had more worldwide experience than you have with volcanos. Can we expect to find analogs on this topic? We've heard discussions about the Karst topography in China and so forth. I think it's important to put on the table your thoughts on the possibility of having analogs to support and compliment the modeling that is currently underway.

DR. MELSON: Well, we have talked about this. I work with a lot of folks interested in volcanoes. Dick Fisk, for example, we went after him about the possibility of lava tubes in Hawaii, that maybe we had a lava tube or something went into. His first comment was, "It will be degassed by the time it gets to the lava tube." He doesn't know, nor has he ever seen a lava tube.

As far as the caves are concerned, I don't know. I don't know of any analogy. The nature of the process fills up and disguises perhaps some of the tubes that may have been there. DOE is looking out in the southwest.

I know Frank Spera has been on the phone. Everybody who has ever done anything field work or worked on volcanoes just about trying to find an analogy. Whether he has found one, I don't know. I think the possibilities are slim. If one is found, we have to ask the question like the Icelandic case, how relevant it is.

DR. HINZE: Are the explosions at the Icelandic situation impacted at all by the presence of the water table in proximity to the surface?

DR. MELSON: To my knowledge, no. It was a hot zone and there was no water table involvement. It was mostly hot and dry with some water but not a water table interaction.

DR. HINZE: On another topic, you mentioned just fleetingly the impact of earthquakes. We know that earthquakes do occur associated with magnetic intrusion. Are you satisfied that there is sufficient work being done to consider the impact of earthquakes on the repository prior to its intersection by a dike?

DR. MELSON: I think there's enough work being done. My comment was somewhat different because when I go to a volcano, if I haven't done it and nobody has done it, the first thing we do is start tilt measurements and put in seismographs when we're concerned. That's just an inexpensive way of beginning to monitor.

Now, I think Leon said or someone has told me the USGA has a tilt network. Otherwise, a ground deformation network at Yucca Mountain. A seismic monitor is low cost and it's an automated system so I was speaking more of that just to listen to it very closely. The regional seismic nets often don't pick up the high frequency signals of moving magma.

DR. HINZE: Do you think that these high frequency, low magnitude events could have an impact on the repository that might have a further impact upon the propagation of shockwaves or within the repository itself?

DR. MELSON: What do you think, Meghan?

DR. MORRISSEY: I'm sorry. My train of thought was somewhere else.

DR. HINZE: Presumably the seismic activity that accompanies a magmatic intrusion is going to be felt within the repository, the drifts themselves. I' just wondering if anyone is considering what is being done and what is a potential effect of these earthquakes on the repository causing mock faults, causing changes within the repository itself.

DR. MORRISSEY: I'm not aware.

DR. MELSON: Well, they are not doing that kind of study but these are low magnitude events, one, two, and three magnitude. I would not expect to have any negative effect.

DR. HINZE: Except that they may be in very close proximity.

DR. MELSON: Well, my interest is that, okay, we have this repository. It's full of nuclear waste and it's just prudent to listen to see if, in fact, something bothersome does happen in the region we'll have some ability to -- well, we'll know that and can act accordingly according to what those signals are.

I mean, we're talking about a tremendously small probability of intersection and it would probably be a waste of seismic system if it's a high frequency one. It's a very low cost thing. I'm talking more about monitoring, not the impact on the depositor.

DR. HINZE: But it isn't within that 10^{-8} envelope so it doesn't fall within the unlikely event. Finally, Bill, I was very pleased to hear you be somewhat laudatory, if I may put it in those terms, of what the NRC has done in their primitive, if I may, modeling which has led to this recent work by Ed Gaffney and George Barr and so forth.

Can you compare for us the technical basis for the destruction of the canisters, three on either side of the dike in Zone 1, the technical basis for that compared to the modeling work that was done by Woods, et al.?

DR. MELSON: I really can't. I mean, I was not sure how they did that. I think it is overly conservative. I think for a change the DOE overdid the cinder in terms of the worse case scenario. I mean, I don't know. Maybe somebody else can comment. Meghan maybe.

The attempt I saw by DOE has been to really look at the worse case on purpose. If that comes out as acceptable and the risk is still very low of that, then the work is finished. It seems to me that approach was part of what was going on. It was not a poor approach.

DR. HINZE: You could assume it was worse case but it is also the simplest one. If you model, that's the place to start. That's the back of the envelope type of modeling.

I, too, don't really understand the

technical basis for the destruction of the canisters in Zone 1. I heard a few comments on that by the person that did those calculations, one of the tech exchanges that we were both at. It was pretty back of the envelope and that is being very kind, I think. Thanks very much.

DR. MELSON: You're welcome.

CHAIRMAN HORNBERGER: Other questions?

DR. LEVENSON: I have one. Were those actual analysis and calculations or were those not just assumptions as to how many canisters were involved at the stride?

DR. HINZE: Here we are going to have to rely on memory because I've not been able to find the hard copy on it. My memory tells me that this was based upon calculation of a series of canisters being pushed by the magma, colliding against each other sequentially much like as would happen after being hit by a train.

That was the reference that was made. I assume there were some calculations made in this. As I think both Bill and I are recalling, and perhaps others know better, is that they were based on some very back-of-the-envelope calculations.

DR. LEVENSON: But collision calculations doesn't result in a finally dispersed and dissolved material.

DR. HINZE: No it was a matter of destruction of the canister. That may have been overly conservative.

CHAIRMAN HORNBERGER: Raymond, do you have anything?

VICE CHAIRMAN WYMER: Nothing.

CHAIRMAN HORNBERGER: John.

DR. GARRICK: I just wanted to ask one question. You mentioned the importance of design and noted the importance of back fill. Are there any other design concepts that would have a material impact on the intersection?

DR. MELSON: The whole idea of engineered barriers has come up. As far as a specific proposal except back fill being removed to deal with this issue, I know of none. Maybe, Leon, you do. Maybe somebody does know of a specific design intended to ameliorate volcanic consequences besides the back fill.

DR. MORRISSEY: What I'm going to talk about is kind of some ideas to consider. I don't think there is a set plan how to design it but I think people are discussing it. There is still a long way to go to the final design.

DR. ELSWORTH: It's not to ameliorate igneous consequences but the changes from circa 1997 when the drift spacing was of the order of 20 meters. Since it now is 80, spreading the load out would have some lessening in terms of number of canisters you could possibly access.

DR. GARRICK: Okay. Thank you.

CHAIRMAN HORNBERGER: Thanks very much, Bill. Derek Elsworth is going to be our next presenter.

DR. ELSWORTH: I have handouts as well.

CHAIRMAN HORNBERGER: Oh, excellent.

DR. ELSWORTH: Okay. This is a representation with some additions of a presentation that was done for the Technical Review Board in November, specifically asking or asked to address issues regarding rock mechanics aspects of consequences of igneous intrusion with repository. And the first part deals primarily with an analysis or a review, basically a day and a half's review of some of the work that had been done to that stage, both by the Center and by DOE, in a variety of reports and some thoughts, conjectural thoughts about that, and the second part which we'll talk about is some additional thoughts coming out of the beginning of the peer review meeting in May, May 21 and 22, just a few weeks ago.

What I'll do is I'll split my comments into looking at the mechanisms or the influence of mechanical behavior as a dike or conduit, primarily a dike, would rise through the system, would contact the repository horizon, would work its way through the repository horizon and then ultimately egress to the external biosphere. And we'll deal with each of those components in turn.

First, a brief review about dike mechanics and the points that were raised earlier about dikes not melting their ways through systems. They typically don't. Bruce Marsh's comments and Bill's before about the cooling. The minimum thickness of dikes is the reason that dikes are not often found below order of meter is because they'll get chilled. They get frozen and they can't propagate any further.

So typically, one of the controls on diking is that, first of all, they have to be large enough volume, enough energy in the system, to be able to get around the removal of conductive thermal energy from the margins. To propagate, the pressure in the dike has to be larger than the minimum horizontal stress in situ, and the size of the dike, the thickness, if you like, of the dike, the length, width, and thickness of the dike, is controlled basically by elastic mechanics. It's controlled by positive ratio, shear modulus, the over pressure in the system, and the width of the system, and this is basically the equation for a penetrate crack. So in other words, if you know rough ideas of geometry, you can figure out what the thicknesses would be.

But the reality is that these cracks are relatively unstable because they generate large stresses at the tips and if that stress is large enough to overcome the stress intensity factor, they'll split. They'll fracture either sideways or vertically. And we can calculate the critical stress intensity factors which are generated, compare those with likely magnitudes in situ, and figure out whether these will actually form.

And it turns out -- I'll skip right to this diagram in the bottom which is using this here-- is that if you get dikes of the order of 30 kilometers which is crustal depths in this location, five kilometers or even one kilometer, the magnitude of over pressure that you have to have to overcome a typical fracture toughness of the rock is trivial. So the basic conclusion is that dikes, when they form, will propagate if they truly do exceed the in situ stress and will propagate perpendicular to that direction.

The reason for the invitation to present in November at th TRB was to comment on the Woods, et al. work that was sponsored by the Center and the one comment that comes out of their work is that their assumptions for the dike moving up through the system were based on, predicated on assumptions of magnitudes of density in the magma and also a change in density from surface depths to crustal depths of these order magnitudes. If you change these relatively slightly, the force that's driving the dike, the excess pressure which is given by the density contrast, is actually roughly ameliorated. So it is something that's relatively sensitive to the magnitude of the density contrast, both of the curst and of the magma that's rising.

I mentioned before the behaviors of the tip process zone. Almost exclusively, fracture toughness magnitudes are trivial compared to the pressures that you develop otherwise and they're really not a concern in the propagation of these, so you can basically evaluate directions and abilities of dikes to propagate primarily based on the local or modified fuel stresses.

Yucca Mountain changed a lot. The last time I looked at Yucca Mountain before this was circa 97-98 when the drift spacings were of the order of 20 - 22 meters apart, and I was surprised to revisit again and find out that they changed so dramatically. But one effect which can condition, if you like, the propagation of dikes are the potential changes in thermal effects that will occur around a repository.

And if you look on this rough time line of the 100,000 year duration of the repository, moisture in place, there'll be some ventilation period -- I'm not sure exactly how long that period will be, but of the order of 65 years -- the repository will reach a thermal maximum, I think, under the current design for around the first 2,000 years and then begin to cool down and this has some effect, perhaps not so much effect as it did previously with the close drift spacing, but it has some effect to actually re-rotate the stresses, the stresses at Yucca Mountain of the order of the over-burden stress is lithostatic which is of the order of seven magaPascals at 300 meters depth. The horizontal stress is about half of that from in situ stress measurements completed by Zorbeck and others and, as it reaches the thermal maximum, you'll actually generate increases in horizontal stress and there'll be a reversal. There'll be a switch so that the maximum principal stress potentially becomes horizontal or certainly becomes increased. Whether it'll become horizontal or not as the maximum stress depends really on the magnitude of the thermal loading which is also conditioned by the density of the canisters and their spacing.

If we believe that dikes' propagation are controlled by the stress field, hardly at all by the properties of the material in terms of strength, then we can calculate stress fields relatively straightforwardly for a variety of repository geometries. And we can do a couple of things. There are two supplementary effects.

One is that within this band of the repository horizon, you'll generate relatively large thermal oriels which will coalesce between drifts and this will create a band if the drifts are close enough, of the heated zone in which you might expect thermal stresses to be of the order of five to 10, perhaps more, megaPascals increase and, in addition to that, superimposed on that is the very local effect of the radial conduction away from the drifts, the more local thermal oriel effect will also generate stresses in the drift walls typically of the same order of magnitude. And if you look at just a ball park estimate in terms of the typical mechanical properties of these materials, the change in stresses account to something of the order of a tenth of a megaPascal per degree centigrade. Just ball park.

So those are the two mechanisms by which you can look at changes in stresses in a broad repository zone, radially outward from the drift with a maximum pressure or stress increase in the hoop stress of the drift and also conforming to the Woods and Bokhove paper for this pressure pulse was originally suggested might reach of the order of the 40 megaPascals driving down the drift. You can calculate what the changes in hoop and radial stresses would be as a result of that. So these are all very calculable issues.

If you go through the calculations and look at these magnitudes, this is what the in situ stresses might look like within the site. Vertical stresses are defined by seven megaPascals at about just by the litostat. The initial minimum horizontal stress would be perturbed at a maximum peak thermal regime to get a little blip but this blip might be of the order of 10 or 15 megaPascals depending on the density of the canisters, etcetera.

So the point is that mountain scale changes in temperature will cause changes in stresses. These stresses, for instance, could, depending on their very nature, deflect or perhaps encourage a dike to propagate towards the repository and that the intersection pressure, the magma, if it were to hit the repository, is in some case conditioned by the magnitudes of these locally determined in situ stresses or developing in situ stresses.

This comes from a previous DOE disruptive events report. Some of the issues. The over pressures are limited by failure of the host rock. By that I really mean failure due to the stress regime rather than actually the strength of the rock, as we've already mentioned. Depending on the thermal loading of the repository, these thermal stresses might be quite large. They'll be largest within the heat up period after the ventilation is turned off and before decay actually reduces the temperatures by conduction. The barriers are pretty thin so if you look at on a crustal scale of about 20 kilometers or 30 kilometers, this zone is probably of the order of 40 meters. So the question is whether that would actually result to deflect or to impede this propagation in any way at all.

If you expand this zone within the horizon, potentially you'll develop an extensional zone below the repository where it's basically reacting against this by pulling this apart, and that might work to effect the motion of the dike in the general vicinity and also as you heat the repository and the minimum and intermediate stresses so the horizontal stresses, principal horizontal stress, maximum horizontal stress and the minimum horizontal stress become closer, then you'd expect perhaps structural controls to have more effect on the repository than they had in the past because now structural control migration pathways.

And also the effect that on the repository scale topography, if you look at the topography with most of the cinder cones located in Crater Flats, there's a question as to whether the topography controls some of these things. So that's kind of an overview on some of the processes affecting ascent as it moves toward the repository.

As it moves toward the drifts, then a couple of issues of interest. One is how the local stress state around the drifts will control what's going on and also how this might affect the magma over pressures as it intersects within the drift and how this then potentially conditions the shock wave or potential development of the shock wave.

As we saw previously, the current stress state at Yucca Mountain is of the order of seven megaPascals vertically, three megaPascals horizontally, two to one stress state. If you heat up this repository zone, what you potentially do is you rotate this by increasing the horizontal stresses. Vertical stress doesn't increase and, as a result of that, we can calculate what the magnitudes of the local drift wall stresses might be and -- that is that these local magnitudes and this ring -- I'm reluctant to use the word hardened but this compressive ring that might develop around the drift could act to deflect dike propagating into the drift perhaps, but it would also act to control the magnitudes of the stresses that can be sustained by any over pressure by pyroclastic cloud or shock wave before it breaks out and basically fractures the rock. So we can use some relatively straightforward precepts to be able to figure out exactly what those stresses are.

This is a cartoon to represent the fact that as time goes on, we'll go up from this initial pool period, thermal maximum and then cool down towards 1,000 years and briefly look at two different conditions. One where we have a static condition with no over pressure in the drift for each of these stress regimes as we go through here and the second one to look at what happens when we get a dynamic over pressure or magma of, as suggested in some of the earlier versions of the Woods et al. paper where the shock wave was amplified to the order of 40 megaPascals.

Again cartoon-wise, we can calculate what these stresses are, both due to the effects of excavation and also due to the effects of the thermal stresses that result, thermal stresses due to this band of heated material that's the oriels coalesce and also due to the local thermal regime where we have this conductive signature away from each drift with a radial stress which increases slightly as we get away from the drift and more dramatically with the hoop stress which increases to relatively large magnitudes as we get close to the drift wall and acts as a way, we mentioned before, to basically harden these conditions.

So the two conditions we looked at are without internal pressure and with internal pressure and we look at two different states. One is when it's warming up and cooling down, relatively low thermal effects, and if you look at the stresses that you will generate around this assumed drift geometry, then you can calculate their magnitudes and from the previous slide -- I won't go through that again -- but merely to note that we can calculate what the magnitudes are.

In the initial state where we have the principal stresses acting vertically, seven megaPascals and four horizontally, then what we get is the maximum hoop stress develops in the crown or in the invert -- springline -- sorry -- in springline, a smaller magnitude develops in the crown and also a longitudinal stress of the order of two megaPascals.

DR. HINZE: I think it would be helpful if you defined springline.

DR. ELSWORTHY: Springline is this line here along the side of the tunnel. This would be the crown, this would be the invert. I'll de-jargonize it. And the magnitudes for the cold opening, the stresses that develop, the minimum stresses longitudinally along the access to the tunnel in the crown and also by symmetry in the base suggesting that you have to overcome this stress to be able to get into the system.

So the question is when you get break into this, you would expect that the magnitudes of the magma stresses, the fluid stresses in the magma, should be of the order of either two or the field stress, principal field stress, of the order of four as it breaks in. So the assumptions in some of the Woods work that these would break in at 10 megaPascals is probably overly conservative. So you can use this to condition the magnitudes of the magma pressures as you move into the system, as you move into the drifts.

As drift wall warms, additional compressive hoop stresses build at about .1 of a megaPascal per degree Centigrade. Dike would ingress at invert if it's coming from up. It would egress at crown potentially and you can suggest that based on the geometry of the drifts and the magnitudes of stresses that you expect and, as it would move out, it would again move to be perpendicular to the minimal principal stresses.

If you developed, as a result of an intersection with this drift, very large gas pressures of the order of 40 megaPascals, again you can see this stress stays of the order of two megaPascals. So whether you can sustain that is a real issue. You'd expect basically that the drifts would unzip. They're release those pressures, depending on how quickly they could bleed them as they fracture if you were to get this large over pressure developing as a result of the shock wave as we'll perhaps examine later.

If you have the over pressure of 40 megaPascals, again you can calculate what the revised stresses would be. Basically, it changes the stress here from being zero megaPascals to being 40 megaPascals compressive. It changes this stress from being four megaPascals to being four minus 40 which is 36. So it's all done by super position and obviously you can't sustain this with the typical strengths of Yucca Mountain rocks.

If you heat up the system, you change it slightly be developing both stresses in this overlapping thermal oriel, this ribbon, if you like, that goes through the repository and also the drift local stresses. So here you see magnitudes of the average stress due to a heated coalescing strip of oriels if you like plus the local drift wall stresses and they get relatively large stress magnitudes. And this is only to illustrate the fact that as you get larger thermal loadings, you actually do in some way harden the drifts to incursion. Whether this is enough to resist it or not is perhaps an open question.

Likewise again, if you over pressure it by large amounts, then again you have relatively low sustainable pressures of the order of -- well, in this case, it would unzip at the springline and you'd off-gas those pressures as they develop, depending on your capacity to get the relatively large volume of gas pressure here out of the system.

Again, the stresses that we might be able to sustain are controlled by really the mechanical properties around the drift and whether the drifts would be able to survive this, I would suggest that on a site where perhaps hundreds of detonations have been done underground, perhaps there should be some interesting data and very local data available to address that.

This is really a summary of perhaps what we've talked about for the last four or five slides and that is that depending on th thermal stresses that exist around the system, the influent magma pressure as you intersect the drift is in some way controlled by the stresses around the drift and the in situ stress conditions. And for a cold repository, those magma pressures would be relatively low. As you heat it, depending on how much you heat it and the initial magnitudes or the properties, the mechanical properties of the rock, that incursion stress would change. We can calculate that relatively -- within reasonable bounds compared to some of the other unknowns in the system.

What happens within the drifts? Well, we've talked about stress magnitudes and how they affect perhaps ingress location, how they affect perhaps magma over pressure. Megan will talk more about the magnitude of the pressure wave and how that might be conditioned by the incursion pressure and what happens on some of these things as we go through there.

What might we expect within the drift? Well, waste package and drip shields. If you generate a pyroclastic flow down these things, perhaps the least of your worries are roof-falls if you've ripped off all the drip shields due to the movement of a high velocity pyroclastic pulse moving down it.

I guess open questions are whether the pressure wave moving down the system is large enough to be able to rip off and rupture drip shields, whether it's able to rupture the casks themselves as they're banged against each other. Questions about how much of the length of the drift would be affected or whether the wave would -- the gas pulse would migrate out of the drift or run the full length of the drift and whether this thing running down the drift would have some dynamic effect on adjacent drifts but perhaps if you have a major incursion into the repository, perhaps this isn't really any particular worry compared to the other effects at all.

If the cross section is partially filled, then you get some benefit from that. People have raised the issue of backfilling drifts. Obviously, you decrease the volume that's available for expansion. You might get a surface of the backfill eroded if you have only a partial backfill. The question is whether when this dike coming into a drift would actually bulldoze a portion of the backfill down still and certainly providing backfill would provide prevention of roof-fall, would save perhaps the cost of adding the $6 billion worth of titanium drift shields, etcetera. Some economies perhaps are available.

And some other alternatives exist in terms of whether you can actually use bulkheads to separate up portions of drifts and how you might separate those canisters with individual bulkheads. Perhaps using just the TSw2 material, crushed material removed from the tunnel-boring machine to basically as stemming to stop the expansion wave or pressurized magma going down the drift. I guess to be able to ensure that what you need to do is make sure that, first of all, any kind of stemming would be adequate to be able to stop it moving down the drift.

This is very quick back of the envelope calculations to figure out what kind of length of stemming you'd need to be able to stop a pressure wave moving down, whether it be magma or whether it be a gas wave, and basically looking at perhaps TSw2 stemming within the place to fill a bulkhead or back-fill filling the complete section of the drift. You can size the plug based on either elastic analysis or a plastic analysis of the stemming material where this is a fraction angle and this is -- ratio to get a rough order of magnitude of what kind of size length of stemming you'd need to be able to resist a large longitudinal force. It turns out to be something of the order of one to one. And of course, if backfill was used, then perhaps this is a calculation you'd need because you'd be able to take care of -- you'd be doing this locally everywhere along the tunnel. So it wouldn't be an issue of local --

And finally, the issue of where you get

egress again is controlled by this understanding of mechanical processes and if you're able to pin down stress states as they might evolve within a system with any kind of reasonable certainty and for a cold repository you can make some conjectures how these might go out and these would actually be again controlled by the stresses that would develop, both globally within this heated zone and also locally around the drift due to the drift local stresses and you can make some inferences about how that might occur. At DOE we are currently moving in that direction.

So again, this is a summary for the TRB talk of last March. The main conclusions are that because of the low fracture toughness of these materials, these fractured rocks, strength is not a large consideration in looking at the propagation of a dike. Really it's controlled by stresses and over pressures. Strength, actually you'd expect to give perhaps less than one megaPascal over the in situ stresses in resisting the propagation of a dike.

The cold repository exists for the largest potential of time or the cool repository, perhaps we should say, and the magma pressures are controlled, if they go into the repository, are controlled by the minimum in situ stresses of the order of say two to five megaPascals. If you over pressure the drifts anything larger than that, you also expect them to unzip within the cold repository because of stress effects. Just like a hydrofract coming out of a drift rather than out of a bore hole.

Hot repositories for somewhat less of the time. Entry pressures are increased. Perhaps the drifts act to deflect propagation of dikes as they come close. The jury it out on that, I think. It's doubtful, I think, whether you could heat them up so much as a mechanism to actually keep dikes from intruding. It's an interesting idea but I think that would probably be a not very reliable way to deal with it. And a relatively straightforward way of dealing with it, of course, is to provide bulkheads or back-fill which I understand has some negative effects on reduction of cladding. Survivability, I guess.

And so I guess the issue is whether degrading the routine performance of the repository by allowing the cladding to fail is worse or better than the potential incursion of a dike into the repository.

The final comments really revolve around the presentations from the May 21 - 22 peer review committee meeting in Las Vegas and just a comment. I think Bill has already talked in some detail about some of the proposed studies. But my own feeling is that I don't think that the issue of dike intrusion and its consequences perhaps can be as tightly constrained as, for instance, the routine performance of the repository. And that's based on the observation that the routine performance of the repository has in, I think, a very logical way been based on increasingly larger and longer duration field tests. The large block test, single heater test, the drift scale test, have all been progressively larger tests, working for larger periods of time, accessing a progressively larger volume of rock and subjecting it to real processes that will go on within the life time of the repository.

The only feasibility of doing that in this case is using the analog geological studies locally perhaps within the Crater Flats region and the Yucca Mountain general region.

The studies that are proposed by DOE cover three main areas. The analog studies, if you like, magma/gas-drift interaction studies, and also rock mechanic studies. My own feel for these is that they are well posed by DOE. My hope is that they go to studying processes so that when the potential geometry of the repository changes as perhaps it will from now into the future, then they can apply those studies based on process understanding to say, well, what happens when you put the drifts close together? Stresses go up. What happens when stresses go up and how does that affect the propagation of dikes and the likelihood of incursion, etcetera.

Geological studies -- Greg Valentine presented those in Vegas -- will focus on the last 200,000 years worth of activity locally to try and predict the next 10,000 years. I think we'll try and address many of these anecdotally in understanding exactly what's going on at this particular field, volcanic field.

The magma gas drift interactions. We'll use a currently developed code, one that is, I would muse, has been used perhaps in some of the underground detonation tests and I think the biggest issue here, again in applying it to figure out processes rather than any one super geometry of the repository is to apply appropriate boundary initial conditions to give you the results that you like. I think it'll work towards figuring out exactly whether these drifts with all the obstructions in them can act as a shock tube and what kind of over pressures you might expect if you ingress at moderated intersection pressures, say of the order of two to five megaPascals as we've talked about already and define mechanisms by which magma will ultimately move along the system and potentially move out of them.

It would be also useful if they include some evaluation of the effects of barriers or changes in design that might retard incursion into a repository. I think that would be an intriguing study to add to the slate of studies already prescribed.

The magma drift mechanic studies I guess follow along somewhat, I guess are more complex versions of what we talked about today and it is a difficult problem and it's somewhat more difficult, I would say, than the magma gas interaction studies within the tunnel itself, just because there are so many unknowns. The geometries of structure within the rock mass is unknown. Properties are not well constrained. Stress regime is only based on a couple of independent measurements and so there are a lot of unknowns and again, I think it has to look at primarily processes rather than look at specific geometries.

I think it's hampered a little bit in that the code that's used is ANSI's code but it's not really set primarily to look at dike intrusion or hydraulic fracturing. I think the scope of the rock mechanic study is quite large, quite optimistic in terms of what might be able to be derived from the code versus the time potentially available for it.

I think that also our understanding, the community's general understanding of either hydro fracturing in the presence of structures or rock structure is not very good and I think there needs to be some marriage between what'll be done with the proposed ANSI studies in terms of the rock mechanics and also looking at behavior of codes that perhaps were available in the petroleum industry, looking at the effects of barriers, looking at the effects of hydraulic fractures approaching well bores and trying to understand processes that might impact Yucca Mountain and again, to allow processes to be understood so that they might also be applied if the design were to change in the future.

That's all I have. Thank you very much.

CHAIRMAN HORNBERGER: Derek, is there any hope of using physical models for this last point you talked about?

DR. ELSWORTHY: I guess scaling is always an issue. Are you talking about physical laboratory models or Nevada test site models?

CHAIRMAN HORNBERGER: No, no. Sorry. Physical lab scale.

DR. ELSWORTHY: You can but it's tough to be able to recreate stresses. A lot of work has been done on, for instance, gelatin, injecting into gelatin models. I think those are interesting in being able to give insights. I can remember some stuff by Steve Bartell looking at, for instance, in JGR recently, looking at the deviation of dikes as they propagate underneath a static cone, volcanial pile, and being deflected away from it by stress effects. But I think it's tough to be able to put in and be able to quantify the magnitudes of the stresses you're putting in. I think in terms of processes, yes, you can look at general so there is some. It's interesting.

DR. MARSH: Great presentation, Derek. Very interesting. One of the things that you talk about sort of generally is the state of stress in terms of directing the, maybe influencing the propagation direction of the dike, etcetera, and I can remember 10 years ago my suggestion to like this or maybe even further than that. Also, the topographic stress. You see around the world actually the topographic stress evidently has a big effect on where we get these eruptions. You can see, for example, at Kilueaiki, for example, we had a big pit there next to it but the eruption didn't occur in the pit. It actually occurred up on the shelf. Very common in Hawaii to see this. In Antarctica, the dry valleys which you can see, each an area of 30 million years of no erosion basically, and you can see late stage little cinder cones like this, not in the valley floors but just up a little bit onto the shelf, just outside a little bit.

So one of the things I was wondering, if you couldn't actually show something where you actually take the topographic stress and take a projected model for the crust there for what we know for the alluvium fill and where the faults are and actually just show a stress field through the crust with and without the -- or in the upper crust, let's say it goes down three or four kilometers, with and without the repository in various configurations and then there are various people, like you mentioned, that have done this over the years with gelatin models and other ways actually of showing where in fact the dike will go given this field.

DR. ELSWORTHY: I think some of the DOE work will address that.

DR. MARSH: Because these have been done as far back as Nadai. I think Nadai even shows in his books the stresses, topographic stresses. It should be pretty straightforward.

DR. ELSWORTHY: It is straightforward but the fact --

DR. MARSH: Within uncertainty.

DR. ELSWORTHY: Well, I should back up my comments about the propagation path of these being completely controlled by the in situ stresses. That's true but also the fact that you're injecting these things changes that stress around it and, therefore, there's a feedback within the system which is perhaps more difficult to accommodate. So I think the issue of being able to figure out what stress trajectories numerically are or analytically, Bill Savage's work, for instance, in looking at topographic effects on stress distributions, could be used to define potential trajectories. I think that's a great kind of scoping analysis that gives you a good feel for exactly how these things must evolve, might evolve.

But also there's a secondary feedback in that when you put a 10 kilometer blade of magma which is pressurized at some other different pressure, you generate your own stresses regarding that as well.

That's really my main comment regarding the rock mechanics stuff and the DOE work is that the addition of that which is what the attempt will be to do I think is something that has eluded the petroleum industry for a number of years. The comment was made by Manual Detournay at the peer review meeting, who's much more versed in hydraulic fracturing than I, and I think he made the point that the petroleum industry is still struggling with this issue of interaction of fractures, directions and changes in directions of fractures in stress fields and is not by any way resolved.

DR. MARSH: I'd like to just follow up a little bit. There's a separation of this problem, I think. One is in the details which you're talking about in many ways the hydrofract, when you actually have a hole, you're going to start a fracture, there is a significant uncertainty in essentially the mocal material property's granularity in the system that makes a little bit of uncertainty.

At the other extreme, at the regional extreme, we've had lots of studies over the years, principally, let's say for example, by Nakamura and people like this, that we can actually predict in some certainty where dikes and fissures will show up, what direction they'll show up around volcanic systems or even in postmortem systems they follow very much the regional stress field.

So in other words, by knowing the regional stress field you basically kind of know where the dike is going to be. That actually then sets the stage in a kind of an in the background fashion for -- it lessens the probabilities then, for example, in terms of hydrofract. The hydrafract problem is you start initially from a drill hole and go. This says you have a dike propagating that's set up by regional fracture and then you've given that as an initial condition for the more detailed problem.

DR. ELSWORTHY: This would be the seed. This would be saying that the dike propagates from this bore hole and, once it gets away from the bore hold, then it's controlled by this direction from this seed location so you get an azimuth from that at which you would break surface.

DR. MARSH: Are these things being done? I mean these kind of studies being done? These seem to be absolutely critical.

DR. ELSWORTHY: I think they're being covered in two different areas. I think Greg Valentine's kind of paleo studies of these existing volcanic system as it is have a portion of them because I would say that looking at relationships between tectonic structure and the direction of these dikes that link these cones is part of the puzzle and I think the other part of the puzzle which is being approached is to try doing some studies which DOE has proposed. I think George Barr is doing codes to represent the repository, to represent the stress fields and to try and get a dike propagating through that. Yes, those are under way.

Whether they will be realized within the time frame, six month time frame, is an issue. I mean that's my concern I think more than anything is that they -- they have a very ambitious program and that's very good. But I think it's because of the technology and the fact that they're working with developing a code rather than using an off the shelf code that that's going to have some more hurdles than perhaps they think.

DR. MARSH: And when you talk about enhancing the stress locally due to thermal effects, I assume you're just talking about expansion, mainly heating up. I know what's in some of the things --the thermal pulse from just putting the repository there at T equal 0, when is the maximum in the 10,000 year period?

DR. ELSWORTHY: It's somewhere within that 65 to -- it's a moving target from my review, it's somewhere between 65 and 2,000 years. I'm sure there's someone here who probably speaks about that better than I.

DR. MARSH: So what are the residual effects? That's the other interesting thing. Has anybody looked at the fact that you may actually get some kneeling of the rock and changing its strength properties? Would it stress for that long a time?

DR. ELSWORTHY: I don't know.

DR. MARSH: At this temperature? A lot of these things --

DR. ELSWORTHY: If you're getting fluids moving there, you get pressure solution fractures and kneeling, etcetera. Yes.

DR. MELSON: Just to add to what Derek said, I know in Barr's presentation -- you'll probably remember -- he does have a topographic stress term. His program will include the slope of the ground above the repository.

DR. ELSWORTHY: And multiple drifts perhaps. So I think there is a desire to do the whole repository system.

DR. MARSH: And on top of this then is that the history of the volcanism. It's interesting. How well do we know the history of the topography in the area? For example, we know what it is now. We have 75,000 years ago and we know what the faulting is a bit. Do we know -- it's curious to me that all this volcanism actually none of it's up on the mountains. How many dikes are up there on the mountains?

DR. HINZE: One.

DR. MARSH: One. See, it's very interesting that these -- I can see the bounding faults and things. It's very interesting to me to see that the volcanism in mainly bounded, is in the valleys. That's very interesting in terms of there may be stress barriers to actually keeping it in these areas.

DR. ELSWORTHY: I think there's also some underlying structure in that the three center cones that exist that are aligned on a feature are also kind of almost a conjugant paid. There's also interesting structures that you could conjecture that might be there. I don't know whether there is reality in the structures that might be there.

DR. MARSH: So what's the history for the

Yucca Mountain itself? When you're looking at three or four million years, five million years, do we know that?

DR. MELSON: Bruce, the geological survey folks did a wonderful job on geomorphology out in the rift valley on the slopes. Nobody is here to talk about that but they do have good ideas that need to be, I think, probably resurrected.

DR. MARSH: An evolutionary history of that. Yes. That's very important, I think, to get some evaluation of why the magmatism that we see there is the way it is.

DR. MELSON: Absolutely.

DR. MARSH: Then you can use that as a predictor in the future. I believe it's very, very important.

CHAIRMAN HORNBERGER: Bill.

DR. HINZE: In addition to topography, you also mention the structural controls. Could you expand a bit about that and what you mean by structural controls as pertains to the Yucca Mountain region.

DR. ELSWORTHY: It's valley and ridge province. Basin range. So my main comment with that is that these dikes will attempt to exploit easy structures. Depending on the stress regime, they'll try and do that.

DR. HINZE: You're talking about fractures.

DR. ELSWORTHY: Yes. Fractures and faults. I think the effects of fractures on a small scale is that those just give you a very low fracture toughness. Basically there's no tensile strength. But at large scale, the faults will provide potential conduits to direct the propagation of these things.

DR. HINZE: And that's something that Connor in his papers has tried to do. I found it very interesting that you mentioned the analogs of dynamic waves. There certainly has been a lot of work done over the past half century on demolition of underground openings by the Department of Defense and its various facilities that involve shock waves in underground openings and multiple underground openings. Do you have any feeling for how much is available to the program here?

DR. ELSWORTHY: I have no idea. I'll let DOE people address that.

DR. HINZE: It would be interesting to see what Sandia and Los Alamos have on that.

Kind of tangential to your conversation

but it deals with the igneous activity KTI and that is your statement that you have to have this critical energy within the volcanic intrusion to have it propagate and reach through to the surface. That brings to mind the work on the aeromagnetic study of the potentially hidden events, volcanic events, that there must be a minimum size to these events in order to reach the surface. We can approach that problem in dealing with what is the possibility of us missing a hidden event in the aeromagnetic study. Is it possible for us, considering the regional regime, to calculate the minimum magma that one has to have to reach the surface?

DR. ELSWORTHY: That's all intended to be limited by a meter thickness. And that's because it's a balance between how much heat you lose by conduction versus how much you can invest there by moving into the system by moving it up. So it's a balance between how quickly you can supply heat by vection versus how quickly you can move it by conduction. So it depends a lot on a the geometry.

For instance, if you moved the same amount of volume up a circular conduit, a vent, certainly that's much more efficient. So I think it relates to geometry and how much surface area that you would --

but I think you could some ideas from that kind of analysis.

DR. HINEZ: Some bounding calculations of what the minimums are -- it would have to be. And that would relate then to the smallest hidden event that one could be detect.

DR. ELSWORTH: But it would really have to be a discharge that you'd get rather than the --

DR. HINEZ: Right.

DR. MARSH: Bill, those calculations are available. I did those and published those 20 years ago.

DR. HINEZ: Great.

DR. MARSH: I'd like to share some of these, but it's exactly -- for discharge rate and what kind of conduit you want --

DR. HINEZ: Great.

DR. MARSH: One follow up, one question is that if I understand you right and from my understanding of propagation of dike intrusion is the dike actually, the propagation, stress, really atones itself to what it needs in the wall rod to sort of open up a track. In other words, they don't necessarily travel with a huge over pressure.

DR. ELSWORTH: No. Well, they can't.

They can't because they're unstable.

DR. MARSH: Right.

DR. ELSWORTH: Because they're unstable. They'll build sideways or upwards.

DR. MARSH: Right. It'll form a sill or something.

DR. ELSWORTH: Yes.

DR. MARSH: That's another aspect that should be looked at is the whole -- I'll talk about this a little later maybe, but whether these things actually will, as they say, get an over pressure which is very important. I mean, if you get -- 2 percent water and they actually saturate -- they have a significant depth coming up, especially if they have some CO_2 in it. So if they saturate, the over pressure can be large and it can form a sill and just take away all that depth. That's significant. We see this all the time, actually, in systems. It'll keep tuning itself or reducing itself down to where the over pressure is minimized.

DR. HINEZ: So what you're suggesting is that we not only have to worry about a dike, but we have to worry about a sill that hits all of the drips?

DR. MARSH: You're already worrying about that.

CHAIRMAN HORNBERGER: But the probability is long.

Any questions over here?

DR. LEVENSON: I've got a question in ignorance. In the natural cases where you have differences in stress, etcetera, do they tend to be vertical? Are there are any natural cases where you have discreet uniform isolated tubes of stress doing horizontally, which is the case here with the repository tunnels which have been preheated?

DR. ELSWORTH: Naturally occurring?

DR. LEVENSON: Yes. Yes.

DR. ELSWORTH: Not that I can think of. You mean in analog, this kind of behavior?

DR. LEVENSON: Yes. But would you expect that something is rising and there's a couple of hard tubes now fairly far apart that rather than whether it can build up enough energy to break through this crust, would you expect it to just move and go up between them taking the path of least resistance?

DR. ELSWORTH: Yes, maybe. Maybe. Mechanically you would expect that it would take the path of least resistance. These are the hardened -- stress hardened areas that it would try and deflect away from. So, yes, I think you would.

DR. LEVENSON: Wouldn't that take a great deal to divert it from just a small local area?

DR. ELSWORTH: Well, I think you're helped as you spread out the drifts from 22 meters to 80 meters. And as you go in that direction I think you're helping yourself.

DR. LEVENSON: Well, for two reasons, but you now have -- you've increased the soft area between those areas?

DR. ELSWORTH: Yes, for both reasons, yes.

DR. LEVENSON: Yes.

CHAIRMAN HORNBERGER: Raymond?

VICE CHAIRMAN WYMER: No.

CHAIRMAN HORNBERGER: John?

DR. GARRICK: No.

CHAIRMAN HORNBERGER: Thanks very much, Derek.

DR. ELSWORTH: Thank you.

CHAIRMAN HORNBERGER: And next we have Meghan Morrissey.

DR. MORRISSEY: Can everyone hear me fine?

CHAIRMAN HORNBERGER: Sounds good.

DR. MORRISSEY: Yes? All right.

Well, Leon asked me to join the project back in November. So I'm still in the catch up mode, but he asked me to, more or less, consider the shock wave dynamics involved with Bokhove and Woods model. So, today what I'm going to do is give you a little background information about shock waves in volcanic environments and then do a little review about shock tube mechanics and dynamics. And go over, review the Bokhove and Woods model, and then give some comments and recommendations how shock waves will -- their behavior in the tunnel and the drift and what one should do about engineering for it.

So shock waves are recurring volcanic environments where we have a high pressure magma fluid coming into a low atmosphere. And what happens is there's a shock front and a compression wave that moves into the atmosphere, and that's coupled to an expansion wave that moves down into the magma. So you have these two pressure waves; one that's compressing the low pressure air and one that's trying to lower the pressure of the high pressure magmatic gas.

And so what you see is this shock wave that moves out into the atmosphere and that's followed by the magnetic fluid.

Here's some examples of this actually happening. This is a classic one, it's in Ngauruhoe, the eruption in New Zealand in 1975. You can see this is the onset of the eruption and the shock wave and it compresses the atmosphere behind it, and so it develops this cloud that's very apparent.

Down are some other examples of eruptions in New Zealand that have had these shock waves associated with the eruption.

Here are the records of these shock waves actually occurring from many volcanos during the onset of an eruption. And these are records on micro barographs some tens of kilometers away. So most of the energy is dissipated within the first kilometer. So these are small airways that move out, but they are recorded and they do exist. So these are examples of-- one's from Mount St. Helens, Sakurajima, Mount Pinatubo, Ruapehu and Mount Tokachi. And they also are recorded during Strombolian eruptions, but that's -- they've only recently been recorded because people have put microphones very close to the vent. So these little shock waves, pressure waves do occur at lower pressure type, less energy, less energetic on the eruptions.

So the Bokhove and Woods model is essentially trapping saying that once you have this magmatic plug moving through and it intersects the drift, you're going to trap that compression wave and move it through this 200 meter tunnel instead of running it to expand outward.

So it's essentially acting like a shock tube. When a shock tube -- the shock wave that occurs, it's pressure is dependent on the driving force of the piston divided by the area. But in the case of the magma, it's the magmatic pressure that's moving in that dictates what that initial pressure of that compression wave is. And the speed of that shock wave is dependent on the difference between the air pressure and the driving pressure, and also the temperature of the atmosphere in which its propagating through and how much energy -- or it's magnitude, how much energy it's going to pass as it moves through, reflects off the end of the tunnel and the magmatic interface depends on how -- depends on the properties of the magma and the wall at the end. So its boundary conditions play a big factor in it.

So the Bokhove and Woods model, it's a one dimensional shock tube. It takes account for gravity by -- it takes in account -- it's more like a two dimension as it comes in and it intersects the horizonal tunnel, it takes into account that change in direction by gravity.

The magma enters the drift as a foam. In their model the foam is defined by 70 to 90 percent voids or gas space, and it contains 1 to 2.5 wt% water. And the void fraction is less than the fragmentation level, so it's not a fragmented magma, it's a foam.

It neglects the presence of the waste packages, so it's an open system. It's open.

The dike geometry is fixed, it does not change. So once the magma enters the tunnel its geometry stayed the same. It's more like steady magma flowing in.

And the magma in the model enters at 20 megaPascal and 1000 Kelvin.

And the effect of viscosity is a frictional term.

So here I'm just going to explain some of the pressure behavior in their model, and we're going to focus the middle -- it says pressure versus distance, so it's along the drift and dike. So this is the onset of when the magma enters the drift, it sends a compression wave or the shock wave into the drift and that shock wave is raising the pressure inside the tunnel. So you're seeing it just at time increasing to the left -- to the right. And to the left you'll see the rarefaction wave lowering the pressure in the magma in the drift.

Over here in phase 3 it's more or less showing you in a long term how that rarefaction wave is moving down into the magma, lowering the pressure of the magma. And to the right is the compression wave compressing the air and raising the pressure.

Phase 2 is showed -- the dotted line shows the flow front of the magma as it's moving down into the drift filling the drift. Okay. So it's at more like a steady state here, steady velocity.

These lines here show the shock front moving down the drift so it's raising the pressure inside the tunnel of the drift and it's reflecting off the wall, which in this case is a rigid reflector. So there's no energy dissipating. Sol it's taking all that momentum back into the system raising the pressure.

So that first reflection raises the pressure in the air even more. It intercepts the magma flow front and then it reflects back towards the end of the tunnel so these reflections allow the pressure ahead of the magma to build up. And this is where they're getting their ten to -- 15 to 50 percent increase in pressure by these reflections.

So there's a bit of energy being

dissipated into the magma, and this is where their parametric study comes in. They considered four different factors that can dissipate some of the energy from that shock wave as it reflects of the magma-air interface.

And they first considered the initial pressure of the magma as it enters the tunnel. Also how much water it contains, from 1 to 2.5 percent. Friction. And also increase in the void content of it.

So what we see here is the maximum pressure buildup inside the tunnel, and this is the shock amplification.

So as you increase the pressure of the magma coming into the tunnel, of course it's going to create higher magnitude reflections. So more energy more pressure inside the tunnel is going to build up. So there's very little dissipation from the magma.

As you increase the water content of the magma, again it's not going to absorb that much energy when the shock wave intersects it.

But the friction factor does absorb a lot of the energy, so it really reduces the application of that reflected shock.

Foam, again -- I mean, if you get the

increase void content, it increases the overall pressure inside the system but very little and it reflects how much the shock amplification occurs.

So these are four parameters that they've discussed about how you could reduce the pressure build up inside the air tunnel from that shock wave.

So I'm going to go through and discuss the limitations of their assumptions.

The first assumption is a one dimensional shock tube. And if you consider the magma coming in at any angle, it's going to actually create reflections of the sides of the wall. And so it's going to make, probably, a series of oblique shock waves. And whether those are going to resonate and increase, you know, that may occur, that may not occur. So that pressure build up may not occur if you account for, you know, a two dimensional, three dimensional geometry with the dike coming in at a different angle than 90 degrees.

The second assumption is magma enters the drift as a foam containing 1 to 2.5 wt% water and it's below the fragmentation level, and it's steady state behavior.

Well, if the magma comes in in any other way, if it comes in as a fragment of gas mixture, well that's going to be coming in more turbulent, higher speed and it's going to fill up the tunnel totally different conditions for that shock wave to interact with that turbulent mixture. If it comes in and just stops at the plug, well then that shock wave can reflect back and forth and really build up more pressure than if this magma is slowly moving and filling up the tunnel.

They neglected the presence of water packages, but I'll discuss that in a minute.

The fourth one, the dike geometry is fixed or prescribed. Again, if they consider in reality that that dike geometry will change as the magma is entering, that's going to influence the flow behavior of the magma and so it may fill up the tunnel a lot quicker, therefore reducing the amount of time for thou reflections, so the pressure build up from the reflected shock waves. So that's something to consider.

The magma enters as a very high pressure. And as Derek pointed out, it's probably much lower but still it's something to consider.

The rigid wall at the end of the tunnel will probably be fill material which would allow more of the energy to be absorbed from the reflected shock wave.

The air in the tunnel remains clean. But if you consider if there's any amount of sand/silt on the bottom, the leading shock wave is going to pick that up and train it, and that's going to change the sound speed of the air. And I have some calculations I'll show you the effect of that.

And then the temperature inside the tunnel. At 25^{o} C the sound speed is 340 meters per second. If you increase it to the highest temperature that Derek pointed out, 150^{o}C, you're going to raise it up to 415. So it's going to allow the shock wave to -- more reflections and more build up. So it's something to consider, too.

Now here's a discussion about the presence of the packages in the tunnel. Well, the shock wave will propagate around the packages because the spacing is fairly close. The shock waves will pressurize the packages in the tunnel. They might be localized reflections off the walls that would probably produce a hammering effect on the packages. And also considering the abrasion from a dusty atmosphere as this shock wave is passing through collecting and training more dust, there's going to be very abrasive material moving up and down. So that's something that should be considered.

Here I'm just showing the effect of dust and temperature on the shock wave as it propagates through the tunnel. If you use normal shock relations for moving shock waves, these equation, 7.12 from Anderson, the textbook on Modern Compressible Fluid Flow, this equation here gives -- you can calculate the ratio of the leading shock as it moves into the pressure of the tunnel. And it's a function of gamma, which is the ration -- it's a heat capacity ratio, so it's heat capacity at constant pressure over heat capacity of constant volume. And it's also a function of the mach number. And the mach number is the ratio of the speed of the wave over the sound speed of the wave.

So if you rearrange this equation -- for the mach number and then define the mach number as the speed of the wave over the sound speed, the sound speed is a function of gamma as well as temperature. And gamma, if you consider -- well, first you consider the effective dust. If you add ten weight percent dust to the air, for pure air gamma is 1.5. And if you add ten weight percent, you reduce it by a tenth. So this graph shows the velocity of the wave as a function of gamma. So if you add ten weight percent dust, so it'd be 90 percent, 80 percent, 70 percent; the wave speed is going to decrease which will slow down the number of reflections as the magna's entering the tunnel. So it's something to consider.

I didn't put the other equation. But you can calculate what the mach number would be for the reflected wave, the first reflected wave and then you could use this calculation here to calculate what the pressure would be for the reflected wave going back into the air after the first shock.

So this graph here shows you that if you add dust to the system, it's going to reduce the pressure of the reflected wave, you know, relative to a pure clean environment. So adding dust would slow down the wave, slow down the number of reflections, also reduce the pressure build up.

But temperature, if you go to the higher temperature, the reverse effects. You're going to have a faster wave moving through there and it's going to pressurize a lot higher and quicker.

So, how realistic is the model is the model that they propose? It's fairly realistic. If the magma intrudes into the tunnel, it's going to come up with a high pressure. That pressure from that magma is going to send in a shock into the tunnel.

And they demonstrate the behavior of it.

The magnitude of the shock wave depends on the driving force of the magmatic fluid, and the mechanical properties of the magmatic fluid and the wall; so the boundary conditions. And the initial thermodynamic state of the air inside the tunnel, whether it's cold, hot, how much dust, etcetera.

And the uncertainties of the model. The behavior of the ascending magma; it could be rich in volatiles, it could be ready to expand explosively as it reaches the tunnel so it'll just move a dusty high turbulent mixture into the tunnel or it may behave very passively, move slowly as the model suggests, which will allow more shock waves to reflect and really fill up the pressure.

The boundary conditions at the end of the tunnel, they consider the ridge a reflector. If you consider more realistic material, more energy will be absorbed out of it. So they need to consider that in their model.

And then entrainment of sand and silt. As I demonstrated, that's a big factor, too.

So how to engineer the tunnel for shock waves. Enable walls to absorb and transmit some of the energy out of there.

Pressurize or cool tunnel.

Strengthen the packages and the mounts to withstand the pressurization and abrasion from the reflected shock waves.

So that is all I have to say on that topic. So, any questions?

CHAIRMAN HORNBERGER: Meghan, I'm curious. I'm trying to link some of the things up. You didn't mention any possible effects of cooling and solidification of the magma as it enters the drift. Does that have an effect or --

DR. MORRISSEY: Oh, yes. If it cools and solidifies, then that leading shock wave has a lot of room just to keep resonating if there's no area to -- no means of dissipating that energy. So you'll get more pressure build up than they even say in the model.

So their model -- their 15 to 50 times the initial pressure build up is based on the number of reflections of shock waves as that magma fills the tunnel. So if you stop the magma half way through, it still has all that movement, you know, all that area to reflect and keep building up. So, yes, it does --

DR. MARSH: Well, I think maybe George is thinking also though that the magma may be selfsealing. In other words, when it opens up into the cavity, let's say.

DR. MORRISSEY: Right. But once it opens up into the cavity, it's going to produce. Right.

DR. MARSH: Right. But it needs a volume, it needs something to work on. I mean, it will work back and forth, but --

DR. MORRISSEY: Right, it's going to have the volume of the whole tunnel.

DR. MARSH: Right, back and forth.

DR. MORRISSEY: Right. Right.

DR. MARSH: But I have a kind of a more fundamental question that maybe Bill would -- most of all the shock phenomena that we've ever seen on the earth involves, it seems to me, in volcanic situations involves two types of situations. One is that mainly from volcanic conduits which the analogy between the shock tube or the best way to produce a shock wave really is to pressurize the side of a diaphragm and then puncture the diaphragm and let it go. That's the standard way that shocks are produced in ballistics and everything else. And that is a perfectly ripe geometry for a volcano.

The other thing is that these are established, well establish, usually well established volcanic systems where you can actually get a plug kind of in the system. And in many ways what volcanos are, they're basically nature's way of performing Red Adair's work, you know, run away eruption. They cap themselves. They just go up this thing and cap themselves.

DR. MORRISSEY: Right.

DR. MARSH: And -- all the pressures and then you get this perfectly good situation.

To my knowledge, we have never ever seen a shock produced from an initial break in a fissure or a dike hitting a surface. Because they're --

DR. MORRISSEY: I would like to differ. Because in that situation it's going to be a low pressure. You're going to produce a sound wave when that breaks through. But because no one's ever measured it, until now like at Stromboli, they're putting microphones very close and you can measure these. It's not going to be high energy, especially, you know, a kilometer or so away because it dissipates. But if you trap that into a 200 meter long tunnel that's only ten meters wide, you're going to trap that energy.

DR. MARSH: But the issue is a little different in that when a dike actually propagates along, it actually show -- well Derek was showing a dike, and we all show dikes as being these kind of blunt tipped things. But if you actually look at them, the angle at the leading edge of the tip is actually zero. You know, so in other words we actually show the edge of a propagating dike, it's actually a little thin ribbon out there.

DR. MORRISSEY: That's right.

DR. MARSH: It maybe in fact be several hundred meters or a kilometer ahead of the major part of the dike. We see this very commonly in systems. And so, in other words, the initial break is actually something maybe an inch or two inches wide that dissipates the pressure immediately in the system and by material flowing out and then the dike opens up.

DR. MORRISSEY: Right. But so take that entrapment to that tunnel. If you're going to have that little -- you know, that little fracture --

DR. MARSH: Well, but it's a difference in opening up a large conduit of a given width into the conduit.

DR. MORRISSEY: Right. But you're relieving the pressure, though. You got to consider, too.

DR. MARSH: Or taking down a truck tire,

puncturing a truck tire --

DR. MORRISSEY: Right.

DR. MARSH: -- and letting that go in the cavity.

DR. MORRISSEY: Yes.

DR. MARSH: In other words the magnitude of momentum across the shock front significantly different in these cases, just the mass of the driving force. Just the amount of driving force is significantly different.

So that is -- I mean I'm not --

DR. MORRISSEY: That fracture is opening up by the accumulation of the concentration of gas.

DR. MARSH: Well not necessarily. Not necessarily.

DR. MORRISSEY: So that gas is really expanding to a volume. So --

DR. MARSH: What I'm trying to get at here is that these are very, very delicate assumptions --

DR. MORRISSEY: Yes.

DR. MARSH: -- that are built into the model.

DR. MORRISSEY: Yes.

DR. MARSH: And that is the whole track -- with gas.

DR. MORRISSEY: Right.

DR. MARSH: And, you know, we see -- I mean, how many dikes have we all seen and feel. We never see mirolitic cavities in dikes ever. We never see big -- all the -- sills in the world, maybe thousands of them, and at even high -- even to these region, Gettysburg all the way up through Hartford, Connecticut. We never see any gas at all in the roofs of these sills that have been -- propagated.

So, my question is is that these conditions are probably even more delicately prescribed than we can imagine. The geometry is special.

DR. MORRISSEY: Yes.

DR. MARSH: We don't have a ramping up.

DR. MORRISSEY: Right.

DR. MARSH: In other words, when you ramp up, all these things act to actually blunt the effect of it. If you ramp this thing up slowly in terms of opening its width --

DR. MORRISSEY: What do you give to a long period seismicity? That's a whole, you know, process behind understanding the initial idea of long period seismicity is, is it that opening of a fracture allowing that gas to move out, and that's omitting a lot of energy, seismic energy causing that crack to vibrate above it. And it's on an assumption that it's a lot of gas moving through a lot of mass, fast mass, moving through and a lot of energy moving through.

So, if you consider -- you know, the understanding of these processes in terms of opening a fracture and if that little fracture is opening into a tunnel, you know, you're still moving that mass into the tunnel, too. So those initial conditions are going to occur.

DR. MARSH: So it's extremely sensitive?

DR. MORRISSEY: Yes. Yes. Yes.

DR. MARSH: But we've never recorded any shock of a fissure, right?

DR. MORRISSEY: Well, not large scale shock because we don't have a -- because it doesn't have a lot of pressure build up.

DR. MARSH: Right, and that's my point exactly --

DR. MORRISSEY: Right. But if you went really close to it, you're going to see a low pressure wave moving out. You would see that. So all I'm saying is if you consider that and you're trapping it inside the tunnel, okay.

DR. MARSH: But it may not be a shock. It may just be a --

DR. MORRISSEY: A sound wave. A sound wave that will propagate towards --

DR. MARSH: Right. But a sound wave is what we're using right now to communicate with.

DR. MORRISSEY: Right. Exactly. Right.

DR. MARSH: So in other words it may not be detrimental to the physical, just emotional?

DR. MORRISSEY: No. No. The whole thing is if you start reflecting it, okay. And the energy isn't -- in their case, in their scenario, there's no way to dissipate.

DR. MARSH: Right.

DR. MORRISSEY: Okay. They need to consider dissipation. But if you didn't put a sound wave in there, it could propagate to a shock wave and keep building up pressure. So it's something --

DR. MARSH: In a perfect acoustic -- we'd never hear the end of it.

DR. MORRISSEY: Right. Exactly. Exactly.

DR. MELSON: Meghan, can I make a comment on this?

DR. MORRISSEY: Sure.

DR. MELSON: Bill Melson.

I think Meghan and I, I feel you know that

I'm in communication about the shock wave and at the volcanos because there's it's not a dike process initially even. Sometimes it's a dike. As you know, coming in low hitting the water table and it gives this terribly explosive reatic magmatic phase. But then as Meghan was talking about a Strombolian eruption, you'll have these periods of repose, and you've probably seen these, where you'll form a plug. And you do get an overpressure and you do get that shock wave, but these are not related to dike propagation but to accumulation of pressure, you know a plug like pressure beneath the plug. And that's where I think the communication may be talking about slightly different kinds of mechanisms.

DR. MARSH: The thing that a dike is, that a dike since it has such a long aspect ratio, a huge aspect ratio, it has lots of opportunities to vent. In fact, what you see during an eruption usually is when the fissure opens up, it may open up like Hekla or even in Kilueiki, and they open up over a long distance and then it fountains up a bit and then they start localizing somewhere.

DR. MORRISSEY: Right.

DR. MARSH: So in other words, if it starts freezing up locally, in fact it's a runaway process because where it's thinnest, it goes back to Bill's question, where it actually is thinnest and cooling fastest the magma is not viable so it undergoes thermal death. And where it's a little wider, the magma's actually then concentrated there and so it keeps it alive a little longer.

So if you run into an area in one place, which is really intriguing when you put derricks and stuff into it, if you run into an area locally that sort of holds back the magma for any reason, it'll find another area and it'll vent out somewhere else, especially in the geometry of this where you're on the edge of a large topographic expression.

So the dike, unlike a volcano where everything is concentrated more or less, it's going to happen there and everything is focused towards that. With a dike it's dissipative, it's like a crack in your windshield. It's worse and propagates out. So that's a very, very different circumstance in many ways than the volcanic circumstance -- other than the volcano.

DR. MORRISSEY: Right. But if it still intersects the drift and move that magma in --

DR. MARSH: But it's not -- it isn't clear, though, that it'll actually form a shock.

CHAIRMAN HORNBERGER: In terms of dissipation, I mean Yucca Mountain breaths so that at some level it's not going to be trapped forever in a drift.

DR. MORRISSEY: Right. Exactly. Yes.

CHAIRMAN HORNBERGER: But I guess that all depends upon how large a fracture. I mean, clearly, you are going to have a shock if you --

DR. MORRISSEY: Right, yes. There are going to be circumstances when it will occur, it could occur. And then I was, more or less, explaining their model and their limitations to it.

CHAIRMAN HORNBERGER: Yes.

DR. MORRISSEY: They need to consider more of --

CHAIRMAN HORNBERGER: I mean I guess my question is, is it -- does the dissipation have to be taken into account just on the basis of some parametric approach or do we have a decent theoretical way to do it.

DR. MORRISSEY: In their --

CHAIRMAN HORNBERGER: They don't have it, I know.

DR. MORRISSEY: They didn't have it in their model.

CHAIRMAN HORNBERGER: No, I know they don't have it.

DR. MORRISSEY: But I think what's this --

CHAIRMAN HORNBERGER: Gaffney.

DR. MORRISSEY: Gaffney. Thank you. Gaffney, I think their model will account for different material properties along the wall or energy dissipation. So, you know, they're going to probably show that you're not going to get such high pressure build up.

CHAIRMAN HORNBERGER: Okay. Bill?

DR. HINEZ: Well, in the Woods model we see the horizontal transmission.

DR. MORRISSEY: Yes.

DR. HINEZ: The way it appears to me is this temp that's at right angled -- at right angles to the repository is going to produce a hemispherical shock front.

DR. MORRISSEY: Right.

DR. HINEZ: And the net result is that you're going to have reviberations --

DR. MORRISSEY: Absolutely.

DR. HINEZ: -- that go back and forth. Will this lead to an enhancement of the pressure or to a dissipation?

DR. MORRISSEY: I would say more of a dissipation.

DR. HINEZ: Dissipation?

DR. MORRISSEY: Yes. It is something that, again --

CHAIRMAN HORNBERGER: Unless it's just right.

DR. MORRISSEY: Right.

DR. HINEZ: It'll be more than just one thing that's just right.

DR. MORRISSEY: Yes.

DR. HINEZ: You know, an observation in listening to these three presentations, which have been very good I think, is the -- obviously they're all -- each of these speakers has their area of expertise and we're hearing the results of that. But in the -- when we reach the conclusion on this, we're going to have to integrate all of this and all of these different factors, the rock mechanics, the shock, the volcanology, if you will, into a single model. And this worries me greatly in terms of the fact that the ACNW should keep track that this integration is being done and also can be done in a manner that is appropriate to the time frames that the waste program has in front of it.

But we've heard these separate, essentially separate radar screens. We need to have this multi-dimensional radar screen, and there are more than just what we're hearing here, obviously.

CHAIRMAN HORNBERGER: Milt?

DR. LEVENSON: I've got two questions, I guess. One is did the model assume that the end wall was plainer and perpendicular to the tunnel?

DR. MORRISSEY: Yes.

DR. LEVENSON: Well, since it's neither, how big of an effect is going to have on dissipating? I mean, you've got a three dimensional end wall --

DR. MORRISSEY: Right. Right.

DR. LEVENSON: -- which is not perpendicular to the tunnel.

DR. MORRISSEY: Right, with a lot of irregularities, yes.

DR. LEVENSON: How does that reflect in any way that gives you a build up?

DR. MORRISSEY: Well, it's not going to be this perfectly, you know, one dimensional back and forth, no. So it's going to dissipate.

DR. LEVENSON: It's the way you would design a damper, isn't it?

DR. MORRISSEY: Right. Yes, it would

definitely dissipate a lot more than in their model.

DR. LEVENSON: Yes. The second question, you've discussed the matter of whether the wall dissipates or energy. But one of the factors is compressing the gas. The USGS has actually measured what a leaky sieve this mountain is. How important is the fact that the gas buildup, pressure buildup is not going to be -- has anywhere near as great as what it would it be with a solid wall tube?

DR. MORRISSEY: Well, right. That's the point, is the model is -- it's realistic in the sense of the physics, but it's not realistic in the sense of the boundary conditions. So --

DR. LEVENSON: But it's more energy absorption by the wall, it's leakage also.

DR. MORRISSEY: Leakage, right. Yes. Yes. Right.

DR. GARRICK: But do these very short sense of time offset any advantages that you'd have from a leaky model?

DR. MORRISSEY: You mean the time -- the travel time of the reflection --

DR. GARRICK: Right.

DR. MORRISSEY: -- versus how the --

DR. ELSWORTH: Can I say, I think that the speed and the volume that's coming into this would preclude large amounts of Darcian type leak-off. I think you will unzip -- potentially unzip the drift, which might fracture it which might change the permeable from whatever it is, what -- Darcy scale, 10 to the minus 12 -- so much larger values. So I think you get leak-off by other mechanisms, but I think this would happen so fast is my gut feel.

DR. MORRISSEY: It can enhance the -- yes, the ability of bleed-off to the walls.

DR. GARRICK: This brings me back to my question of mechanisms for shock suppression or energy dissipation.

You identify that these ought to be considered.

DR. MORRISSEY: Yes.

DR. GARRICK: Have you thought about what they ought to be?

DR. MORRISSEY: What other --

DR. GARRICK: What mechanisms other than, say, backfill.

DR. MORRISSEY: Backfill?

DR. GARRICK: Yes.

DR. MORRISSEY: You need to consider how the tunnel's going to respond and open up and increase permeability and leak out a lot of the gas or the air, that's a big factor.

How -- yes, it's going to be a big factor, too, on how the temperature inside is going to build up, too, with these pressure waves and all. So, yes, you have to consider the backfill material, the wall properties and this is all, you know, it's very idealistic in their model. And so, yes, when you consider the reality of the whole tunnel and its properties, it becomes a very complex numerical model.

CHAIRMAN HORNBERGER: Derek, if the tunnel does unzip, does the pressure just keep going to dissipate? That is would the crack keep propagating until the pressure dissipated --

DR. ELSWORTH: Yes, I think the crack would be driven by that gas pressure --

CHAIRMAN HORNBERGER: It'll just keep going.

DR. MARSH: And Derek's early point is that the gas pressure is going to be a lot -- if there is a -- pressure, it's going to be much smaller.

DR. MORRISSEY: Correct.

DR. MARSH: If I could kind of summarize a little bit from your interesting presentation, Meghan, is that that the Woods model, the physics for the problem they set up, this idealized problem of basic things going down a cylinder, the homework problem that they did, they did it correctly.

DR. MORRISSEY: Correct.

DR. MARSH: But the problem may not relate very closely at all to the problem at hand.

DR. MORRISSEY: Yes. Yes.

CHAIRMAN HORNBERGER: Raymond?

VICE CHAIRMAN WYMER: Well, there comes a time in each meeting when I have to expose my ignorance about a subject, and the time has come for me to do that.

I have a couple of pictures in my mind of how these things occur, and I'd like to see whether or not they correspond in any way to reality to you people who really understand these things. And for the purposes of discussing, I want to distinguish between tunnel and drift. To me the tunnel is that main passageway that goes through the model and the drift are the things that run off to the side?

DR. MORRISSEY: Yes. I apologize. I was calling it a drift, the tunnel, but it is the drift.

VICE CHAIRMAN WYMER: Okay. So that's what you mean by tunnel?

DR. MORRISSEY: Yes. Yes.

VICE CHAIRMAN WYMER: Now,my simple picture is that when you get a volcanic eruption of some kind, it'll either come up into the tunnel or into some of the drifts. If it goes up to the tunnel, that's open ended so the magma just runs out.

DR. MORRISSEY: Right.

VICE CHAIRMAN WYMER: You don't have any reflection, any pressure, it just runs out.

CHAIRMAN HORNBERGER: No, not once it's closed.

VICE CHAIRMAN WYMER: Well, what closes it.

DR. HINEZ: But they're talking about backfilling the tunnel.

VICE CHAIRMAN WYMER: The entire tunnel?

DR. HINEZ: Yes.

VICE CHAIRMAN WYMER: After you have all the drifts filled you mean, yes.

DR. HINEZ: Yes.

VICE CHAIRMAN WYMER: Okay. So up until that time -- well, what I'm about to say applies up until that time then.

CHAIRMAN HORNBERGER: You want to talk about preclosure volcanism.

VICE CHAIRMAN WYMER: It's what? If it

come up inside the tunnel and the tunnel is not closed off or is not -- or you got ends on it somehow and those ends blow out, then you don't build up much pressure.

DR. MORRISSEY: That's right.

VICE CHAIRMAN WYMER: And so if it's not that, it's just blocked, then the ends blow out. If it comes up under the drifts, then it -- and one end is the end of the drift the other end goes back into the tunnel.

DR. MORRISSEY: Right. So it's going to follow --

VICE CHAIRMAN WYMER: In which case it's going to go toward the tunnel.

DR. MORRISSEY: Yes.

VICE CHAIRMAN WYMER: So it seems to me that somehow or other the modeling has to take into account the fact that you really don't have, at least under some circumstances, a closed drift that you're going down --

DR. MORRISSEY: Well, correct. That's where the boundary conditions really play into it, whether it's open, closed

CHAIRMAN HORNBERGER: You'd still pressure the closed end.

DR. MORRISSEY: Pardon?

CHAIRMAN HORNBERGER: If you had a dike intersect the drift, you'd still pressurize the closed end, even if --

DR. MORRISSEY: If it's closed. If it's closed, you're going to start pressurizing it, yes.

VICE CHAIRMAN WYMER: But you'd probably blow it out. If you have a rifle and you plug up the end of it, the breech blows up.

DR. MORRISSEY: Are you concerned that you could push that wall out that is closed into the tunnel and open it up?

VICE CHAIRMAN WYMER:: Sure, more easily than you could blast out the other end of the drift.

DR. MARSH: The point is, though, is the safety, basically --

DR. MORRISSEY: Right.

DR. MARSH: -- built into the system.

DR. MORRISSEY: Right.

DR. MARSH: It may always just release itself easily.

VICE CHAIRMAN WYMER:: So why isn't that being considered in all of this? Everything I hear doesn't assume that you can blow things out the tunnel. It's all --

DR. MORRISSEY: Because everyone is under the --

DR. MARSH: To get this to work, you really need a closed container that you actually go into and you can make this work.

DR. MORRISSEY: Yes. They're considering the scenario that you're having a closed drift. If it's open, if it is a weak wall --

DR. MARSH: Yes.

DR. MORRISSEY: Yes.

DR. MARSH: That may save the system from being unzipped also, but you can't get any pressure buildup.

VICE CHAIRMAN WYMER:: So why isn't that given more play, more discussion? Everybody discusses these extreme --

CHAIRMAN HORNBERGER: This isn't her model.

DR. MORRISSEY: It's not my model.

(Laughter.)

VICE CHAIRMAN WYMER:: Everybody seems to be saying that.

DR. ELSWORTH: I think the work that DOE is about to do will incorporate that. The Ed Gaffney, McGaffney, Gaffney model, will allow for release of pressure.

VICE CHAIRMAN WYMER:: Will blow down the tunnel.

DR. MORRISSEY: Yes, there's the recommendations to --

DR. ELSWORTH: The Woods model I think is a scoping analysis --

DR. MORRISSEY: Yes.

DR. ELSWORTH: -- which brings up some valid issues, but it is simplified.

VICE CHAIRMAN WYMER:: So they are planning to consider blowing out the tunnel?

DR. ELSWORTH: DOE. Well, I'm not sure whether they're looking at the ends blowing up. I think they are looking at whether it will unzip, and the release due to that effect.

DR. MORRISSEY: Yes, because everyone associated with this model has a feeling that that tunnel is going to be filled, and then the ends of the drift are going to be filled with that same material. So there is really going to be no room --

VICE CHAIRMAN WYMER:: That is not currently the design.

DR. MORRISSEY: Well, then they have to consider the dynamics of, if this scenario does occur, they have to consider that in the engineering process. So that's the whole -- reviewing the Bokhove and Woods model is more or less, you know, if this occurs, you really need to consider the ramifications of it and bring in more realistic boundary conditions, wall conditions, leakage, all that.

VICE CHAIRMAN WYMER:: I talked to Paul Harrington, the lead engineer on the Yucca Mountain design, about a week ago. The current design is just empty everything. There is no backfill.

DR. MORRISSEY: And now it is open?

VICE CHAIRMAN WYMER:: As far as I know, yes.

DR. MORRISSEY: Okay. Last time in November it was closed.

MR. McCARTIN: You may be talking past one another. I mean, our understanding is the tunnel, the access tunnel, will be backfilled. I think the only thing is, if you're filling up that access tunnel, the drift goes into it and somewhere where it meets it would be, there would be --

DR. MORRISSEY: That is where it is closed off.

MR. McCARTIN: Yes, right, exactly. But at least I'm not aware of any design--

VICE CHAIRMAN WYMER:: Well, I may have misunderstood; he may just have been talking about the drifts.

MR. McCARTIN: Yes, but the access tunnel, current plans have it, as we understand it, to be backfilled.

DR. MELSON: May I ask a question?

CHAIRMAN HORNBERGER: Bill.

DR. MELSON: How long is that drift and how many canisters are in it, the one that you may actually partially close?

If we don't have this open system you are talking about and envisioning, what are the dimensions of a potentially quasi-closed system?

MR. TRAPP: Take a look at your drawing. The drawing that you presented is probably the best scale you have.

DR. MELSON: I don't really have it up here. Do you have the answer, John?

MR. TRAPP: No, the --

CHAIRMAN HORNBERGER: John, if you're going to talk, you have to come to the microphone.

(Laughter.)

MR. TRAPP: This is John Trapp.

All I'm saying is that the drawing that

was presented by Bill when he was showing the three different zones is probably a good scale to give you the distance that you need to consider. The exact distance, I would guess somewhere on the order of 400 or 500 meters, but I would need to take a look at that.

DR. LEVENSON: Yes, I have kind of a generic question for maybe the three presenters. You commented on the model and things that you think might lead to lower consequences. Did any of you, in reviewing that model, find anything of significance that was overlooked that might have led to greater consequences?

DR. MELSON: Bill Melson.

Let's go back to this earlier point of view. I think it was the most conservative. I mean, I tried to think of worse things that could happen, but I hadn't found any, quite frankly. I think your kind of thinking is kind of appropriate in some of the models. It has been an attempt, I think, to make the worst-case scenario. That is not a bad place to start, except it taints the issues.

DR. LEVENSON: No. We want to be sure.

DR. HINZE: But didn't I understand Meghan to say that the increase in temperature would lead to --

DR. MORRISSEY: Yes, yes, but --

DR. HINZE: So that is one place where they were --

DR. MORRISSEY: But they're modeling on it. They also are considering this fairly high pressure, too, in their model.

DR. HINZE: Sure, I understand.

DR. MORRISSEY: Their model is a very worst-case scenario because it is very idealistic in terms of it's trapping all that energy, where in reality a lot of the energy is going to be dissipated. So it is a worst-case scenario.

DR. LEVENSON: They don't need conservation of energy in their model because they don't let any out.

DR. MORRISSEY: That's right.

(Laughter.)

Right, only through the magma, right, right.

CHAIRMAN HORNBERGER: Okay, thanks very much, Meghan.

Bruce, I know you have some overheads over there. Would you like to tell us what is on your mind?

DR. MARSH: Well, I thought, just as a little background, to give a little background into a little magma dynamics and what we see out there in the world and the kinds of things.

We have been working on magma physics for 30 years. We are process-oriented. We do all the fluid mechanics, thermal stuff, and everything, and crystal growth. I will just give you a little bit of background, just to show you kind of an area that might knit this together a little bit and some natural examples.

DR. HINZE: By "little," do you mean an hour or two? Fifteen minutes?

(Laughter.)

CHAIRMAN HORNBERGER: I guess you're going to have to get wired up (referring to microphone).

DR. MARSH: Magma is a weird material. The deeper it goes into the earth, magma is more at home. I am going to just give you a little bit of background in what people have thought over the years for magma.

A little bit of background, in that we touched on the business of Bill was talking about superheat or magma's lacking superheat. That is true. In other words, superheat means that a system is heated beyond its last appearance of a crystal. We never see this on the earth. Every volcanic eruption, every magma we have ever seen is always at or below its liquidus, which means that it can have various molten crystals in it, which is another area that is not at all talked about in that model that Meghan talked about, for example, or other things. We will see crystals are extremely important.

Now from the one time I visited the area, the volcanic area nearby, and I had a student that worked on some of the Dell molten lavas and things, is that the crystallinity is very low out there. It actually is low in most alkaline basalts in general, but, nevertheless, we will talk about that in general.

So we have no superheated -- the only superheated magmas on earth are from meteorite impacts. For example, the Sudbury melt sheet in Canada, the 1.85 billion-year-old melt sheet, 3 kilometers thick, probably was 200 kilometers in diameter, that thing was heated to about 1800 degrees Centigrade. It destroyed everything in the system. So it was superheated. Its liquidus temperature is about 1200.

But other than that, any ontogenetic system that is produced in the earth, it is all at or below liquidus. Systems, when they are propagating -- you can turn this on the side really and look at propagation of dikes or other kinds of systems -- they have a thermal regime. This is in terms of temperature. These are isotherms as a function of temperature. This is nondimensional distance.

Initially, when they are moving, if they are moving very fast, of course, the leading edge will have basically a step function distribution of temperature, but back in the system what happens is, because all the flow is going this way, all the heat is being evected along this way, conduction is this way out of the system. Since these two vectors are normal to each other, they can't influence each other, except for the fact that solidification fronts start going in immediately.

So in terms of worrying about a dike coming up from 30 kilometers into the crust, it is extremely difficult to do that of any dimension. You would have to have a dike that is really, really large; for example, the Great Rhodesian Dike that may be kilometers wide, you could propagate off the base of the crust. But we can actually show, and I will show a little bit here, the tradeoff between the width, the flow of it, and how far it can go.

There's actually been many calculations of this by other folks. Paul Delaney and the Survey have done these kinds of calculations.

So if you are going to have a dike that is a few meters wide or even 10 meters wide, it really can't have come from very far in the system, which says something about what its initial conditions were like in the system where it came from and degassing, et cetera.

Now in terms of how these things act, if you would look at a dike or a sheet of magma of any kind, this is really what you would see at one point. In other words, the edges of it are solid, and they form a chill. So if you go out and look in the earth anywhere around even here in Virginia, up at Gettysburg, all through, you will see that every dike and every sill has a chill margin, an extremely fine-grain chill margin, almost like a ceramic.

In other words, no matter how big this is and how fast it is coming in, we always have a chill margin. It is because you actually can work out the simple temperature of this, 1200 degrees, and the temperature in the upper crust being basically zero, the contact, you can show, is always at the average of those two temperatures. So if you average out 1200 and zero, temperature at the walls can be 600 degrees. It is going to be held there forever, basically. That is the highest it can absolutely get. Six hundred degrees is a long ways, it is 400 or 500 degrees below the solidus. So you are always going to chill out on the edges.

Then you are going to have a solidification front. The thickness of this front depends on the age of the system. In the middle of it there will be very few crystals. I show no crystals at all, but these systems are always laced with nuclei. They are all "dirty" systems in terms of the engineering sense. So they have nuclei, superclusters of crystals and things in them.

So these fronts then, the thickness of these fronts will reflect the age of the system or how long it has been flowing. So in a system like this, you would look at this as being quite a ways from its source down there, and these fronts are moving in on it. The further it moves away, the fronts go in, and they basically choke it off, the system.

What happens, then, is that only the very fluid magma is the stuff that is moving. So the stuff that has the least crystals in it, the lowest viscosity material will actually move. This material has very -- it is a mushy system going from 100 percent solids out here to almost no solids, and it has very interesting properties as a mush. That really determines how the magma moves.

So we want to worry about the crystallinity in the system. This is a crystallinity across that. So this is 100 percent crystals up at the wall. We can look at it sideways to keep it oriented sort of for you, as we had it a minute ago.

So this would be on the wall of a dike, for example, and this would be moving out in it. It would be near the liquidus out here in the middle. What we know actually is that most all these systems, they get an interlocking set of crystals. For example, if you drill into a Hawaiian lava lake, you can drill down until you get to -- it acts just like it is drilling through solid rock.

Even though you can drill down to about 50 percent crystals, beyond that point you can actually push the drill stem in by hand, but all crystallinity is higher than that 50 percent. It is a interlocking mesh; it has strength, in other words. It has great strength. It can't be deformed. These things then freeze out on the walls and it has strength. I will show you a little bit about the strength now.

Now we know actually that under magmatic regimes it looks like this strength goes out even to about 25 percent crystals. In other words, it starts freezing out the magma. The magma is confined to an ever-decreasing region of flow out in here.

This shows a couple of things in here. One is the strength of this crystalline matrix. You can see it is almost like a series of trusses built up between the crystals, depending on what the crystals are. Feldspar, for example, forms the major amount of many of these basaltic and silicic systems. They form a great interlocking meshwork like this.

The interstitial melt has a viscosity I show on here. That is also a fact that the viscosity is the lowest, of course, out here where it has the lowest crystallinity, but overall the viscosity of this material goes up dramatically. It goes from the magmatic point -- I don't show it on here, but the viscosity would go from a very low point out here, where it is very fluid, up as it approaches 50 percent crystals, it actually goes up to about 10 to the 18 or 16. So it becomes extremely immobile.

We know very little about the strength of magma when it is partially molten, but these are some work that will be published this month and some stuff by me and other folks. We have one experiment up here by Mike Ryant at the USGS on glassy Hawaiian basalt. So it had partial crystals, and here is estimates of strength here. This is in bars here. You can divide by 10 for molten magma Pascals.

Down here you can actually take a cube of molten basalt when it has about 25 percent crystals in it. You can actually put it in a furnace and you can drain the melt from it and leave the crystals standing up there as a meshwork, kind of like an artistic thing. It will sit there and drain.

So from that, you can calculate the strengths get very low down in here. Our work shows that in situations where you have 50 percent crystals, 60, 65, 70 percent crystals, it is around a bar, the strength of it is. So this is useful to know how the flow is confined then from using these strengths.

This has big effects, of course, in terms of what happens in the flow of a magma. So in the walls of a system, for example, if you look at even Darcian flow of magma through this meshwork, the permeability, of course, is decreasing dramatically. The viscosity is going up of the melt because the melt is becoming more silicious. This parameter here, which is very important, of course, the permeability is decreasing, and the viscosity is increasing. So it chops down the velocity. So the effective velocity is zero in here.

There's some region out here where you have some, and you have a slight return flow for this. But if it is a dike, it is flowing upward, for example. In this, you will have this flow coupled to the flow out here a little bit, but it will be an interstitial flow. It will be very weak compared to the other flow.

Now that is in detail about magmatic systems. Mostly, we see these systems at the surface of the earth, and we think of these as a dike, as some kind of a conduit, but they are integrated systems. They have great depth to many magmatic systems, and this is a simple working model that you can see in most magmatic systems like Hawaii and Yan May and other places in the world, Reunion Island and other places.

That is, it is an integrated system of all kinds of complex structure, but usually it is a system that is a series of horizontal structures, sill-like structures, interconnected with conduits of all kinds and possibly dikes, all kinds of detail coming off these things, dikes and things. I'm going to show you some field examples here in a minute.

But this is a very common kind of structure. We see this at all levels, even high up in the crust. So we think about an eruption or a dike coming off of a system. It is related really to something at depth that is more integrated through the system.

DR. HINZE: What is the relative timing between the vertical and horizontal?

DR. MARSH: Well, that is interesting. See, in a system like this, there are all kinds of different timescales in this. For example, there are thermal timescales associated with these conduits. So, in other words, if we have a system like most volcanic systems are on and then they are off, and they're on and they're off, these systems can become choked.

So in terms of the development I think you're talking about, Bill, these things will develop maybe from the bottom up, but when they get sufficiently close to the surface, they will send a whole school of dikes to the surface. I mean major schools of dikes come up to the surface, which is one of the things that is curious in this location we are looking at.

We don't see any really dike swarms, which means that there is not much magma depth. There is nothing -- a big body like this at depth, it looks like it's a starved system. There is no real regional dike swarms in the mountains.

DR. HINZE: Supposedly, I think Frank Perry has come up with that there are three dikes feeding Lathrop Wells.

DR. MARSH: Well, the important thing also to look at regionally is what's out there in terms of dikes in the mountains and seeing everything that is out there. It is a real sign, then, of the vigor of the system at depth and how close it is actually if there is more magma.

Now if you are going to keep a system alive, one of the things that is curious about, as Bill showed a figure earlier, if you are going to keep a system alive for millions of years, and assume that the volcanism in the area nearby is interrelated over a period of 4 to 5 million years, it means that the thermal relaxation time of something at depth has to be that long, which translates into a body that is really large, which probably means it is not realistic to think of it that way.

It means to think about a system that has been alive for 4 or 5 million years, it is not one system. It is a system that has had an eruption in this locality over individual, perhaps uncorrelated, thermally-uncorrelated eruptions over a period of 4 or 5 million years. That is a long period of time to have things viable at depth here and no other signs of activity on the surface, except sporadically over that time.

So, for example, I just copied this yesterday. This is a common model. This is out of a book on laccoliths and things. This is a very common Christmas tree -- "laccoliths" they call it. This is a very common kind of system.

You can see in many volcanic systems that have been deeply eroded you will get eruptions at the surface. We see this in Antarctica. I will show you one sort of example, but we see exactly this kind of thing in Antarctica, sills that go out for 150 kilometers and small conduits that interconnect these things almost over top of each other like this.

This is kind of interesting in the point of view of going into the repository because, in terms of thinking that a magma will enter a zone and come down here and then come out here, we don't see that very often actually. We see that where it comes in, it goes out in these systems. It is very symmetrical in many ways of where they come in and they go back out. We can see these interrelated in great detail in terms of, I call them, fir trees and things like this.

One of the most extensive magmatic systems that you can look at is in the dry valleys of Antarctica, for example. We have been working on it for the last 10 years. This is a very unusual, perhaps some of you know about it, part of the earth. The polar ice cap is over here. McMurtle Sound is over here, and these are regions. This region in here, this is the McMurtle dry valley. This has been permanently free of ice and snow forever.

So, in other words, it was put down there, Antarctica was down there maybe 60 million years ago. The ice cap built up 30 million years go. These areas in here have never had ice and snow on them, just maybe little bits of touches of a little Alpine glacier and things, but just like going into the Four Corners Area of Arizona, northern Arizona, looking at buttes and things like this, it is a spectacular region.

What you see in these things like this, these are sills. This is one sill, for example, a basement sill. You can trace it all the way through the system, and we will be able to trace it for like 150 kilometers, and then there's one on top of it called the Peneplain Sill. We've been able to trace that one, and there are more on top of that, all the way up until the Polar Plateau, a whole series of these sills you can see with interconnected conduits.

Now it is very interesting in the system. How they establish is you can actually see small dikes coming up, 1- and 2-meter dikes. It reflects before anything happened. These are kind of the fillers, kind of the scouts and skirmishers come ahead, open up the system a little bit, and then some of these things develop.

As Derek was saying earlier, really any kind of overpressure will actually allow this stuff to go horizontally, especially when it gets near the surface. In other words, when it can actually feel the surface or has an overpressure that's more than basically the pressure of the overburden, some of the relations that Derek was talking about, it will actually go horizontally, and you can see areas, you can see across here that this crust has been elevated up through these sills -- these sills are about 350-meters thick -- for hundreds of kilometers. This is a system, then, that built up that way, but the dikes are very tiny, although the feeding conduits that developed later aren't.

So here's one example, for example, of the peneplain, of the basement sill. We can even see how these develop. We can actually trace the magma and see how they develop. There's no dikes coming off these whatsoever. These things come up as sills. They propagate horizontally. The leading edge of this thing is perfectly free of crystals, by and large, and it goes to form a chill margin all along this thing.

Following behind is a great slug of crystals coming up. These things are in the middle because that is where you can transport them. They can roll towards the middle, just like transporting sewage really. This is what chemical engineers use, civil engineers, the same principle. These things roll towards the middle and roll down. The leading edge is perfectly free.

At any time it can actually go horizontally, whether it is in granite, whether it is in sediments, or whatever. The basement sill here is in a granite. It actually came up and propagated horizontally for this granite, split this granite for 10,000 square kilometers. The one above it, it is in sandstone, and above that you see a whole series of them in sandstones and things.

So it is very interesting to see, they establish themselves very easily. There are no dikes coming off it whatsoever, nothing off it. These are very clean systems.

One of the things that is very interesting is, because the exposure in the dry valleys is so spectacular, you can look out on the propagating tips of these things and see things that we never are able to see. It is very rare for us to ever see a dike, the propagating tip. There is a dike out in Montana called the Headed Dike. It is a dike that actually stopped and was an erosional cut there. You can see it. It is a bulbous tip. It stopped and became a bulbous tip, and a guy by the name of Bue worked on it about 100 years ago.

This thing, the basement sill, I just had this made this morning. It is a helicopter shot. I am sorry it isn't better, but this is the basement sill. When I was mentioning the geometry, over a distance of 7 kilometers it goes from 300-meters thick down to -- you see the leading edges out here. There are actually a series of dikes coming out, little dikes. You follow it out, and the most part, the leading 250 meters or 300 meters is about a 1-

centimeter or 2-centimeter tiny, little fracture, just kind of worming its way along, undulating out in front of it.

As you can see here, it gets wider. This is about just a meter or so wide here, but this is the aspect ratio we're talking about. We are talking about something that is in a system where the system is basically a dissipative system. In other words, there are lots of fractures in the system, lots of places this thing could go, and the leading tip on it tries out all these things. It is going all over. It is dissipating itself. It is moving out. It is taking anything overpressure in this and it is actually dissipating it at the tip. That is primarily probably what stopped this thing; it was dissipating in so many directions.

So the leading edge is not a conduit that is blunted off. It is a really fine tip out there, way out there. So the model that we would really like to do for the shock II model is a ramping-up, a slow opening and a ramping-up in this thing, a very, very tiny crack to begin with.

CHAIRMAN HORNBERGER: Bruce, why doesn't a 1-centimeter thick dike freeze immediately?

DR. MARSH: It does. That is exactly what happens. This magma behind it then keeps coming, breaks that open, keeps coming right behind it. The leading tip just moves out like this and then it fills right in behind it. That is exactly -- they are frozen immediately, yes.

DR. HAMDAN: Why are all these sills, you don't consider them to be as analogs for a drift --

DR. MARSH: As what?

DR. HAMDAN: Analogs, natural analogs.

DR. MARSH: They possibly could be. They're not open to begin with. That is the big difference. The big difference is there is nothing there to begin with, and they actually split the earth apart and fill horizontally.

Bill?

DR. MELSON: Bruce, yes, I mean the point he is making is that you were saying they actually are open for a short amount of time.

DR. MARSH: Well, they are not open as a --

DR. MELSON: They have to be because your magma is clinching almost immediately, and yet you are moving it. So isn't there some time when it is in fact open due to the fracturing process? And then --

DR. MARSH: Probably not. I mean, it

moves along, and it moves along -- it is always filled with a fluid-filled crack and it has no open vacuum in it, for example, unless it has some gas or something at the leading edge.

But that is the other point in it. We see no signs of any gases in these things whatsoever. We see no open -- the term "myoral" cavity is a term where you can actually see there was a cavity open and the crystals have been growing into a free space. We see nothing of this. We see no vesicles. We see nothing whatsoever like this. In fact, the freezing of vesicles is very rare in any kinds of these kinds or even in alkalic intrusions that we see -- the Shonkin Laccolith in Montana, for example, which is an alkaline system, precious little of that kind of thing. So, in other words, it has been degassed somehow in the system.

Well, in terms of how these things move a little bit, this is how we also see these things moving, and that is, these things don't come in -- we don't see them in Antarctica. Because there are so many crystals in them, we can actually track the process of the opening. We can see where it has stopped, crystals have been sorted a little bit, and it is reopened a bit or been reactivated.

In other words, most volcanic systems don't just erupt continuously. They have some kind of a pulse to the system that is built in, shock-absorber kind of system. These dikes, as I mentioned before, will flow through the center region. They will be trapped on the edges, and it depends on, of course, how long they have been down before they start up again.

Now this doesn't mean I am talking about the time when it breaks into the repository. I am talking about the time when it is coming up through the earth's crust or coming from its parent body. It is not just a shot necessarily that brings up really crystal-free magma. This thing is a process that starts and stops.

The most continuous ones we see are ones that are on the surface when we actually get, like in Iceland, when we get a central area that is erupting, and in Hawaii, too, and then it is fed horizontally in sheets, blades, and plains horizontally. Those can travel actually quite fast at times. The magma comes up kind of from the bottom and the fan travels out. But in the ones coming up from depth, it looks like they are much more sluggish and more periodic.

DR. MELSON: Bruce, one quick question.

DR. MARSH: Yes.

DR. MELSON: You never see any brushation within the dikes anywhere?

DR. MARSH: No, no. We occasionally see a little limb off to the -- that is the other thing that is very interesting. It is a good question, Bill. The contacts are remarkably clean. You can see these things for, like I say, thousands of square kilometers. They are absolutely clean, beautiful contacts.

Occasionally, you will see a little feeder, not a feeder, but a little dikelet, sill-let trying to go off the edge. It might go off for 5 meters. It will be frozen off. So they contain themselves. This is exactly what we are talking about in thermal viability. It will actually go off a little bit and be quenched, and the whole system then maintains itself.

Now in our work in Iceland, in terms of we are looking at a major volcanic system, the Torfajokull area, that produced a lot of silicic in Iceland. One of the things we realize is that, as fissures propagate down from the central area, encrapala area, we get explosion craters, we get center cones developed well on the surface. These are fed horizontally in this.

In other words, I was talking about the massive system and what is behind it in terms of the magmatic energy behind this. This is a big system. It has been a system that has been alive for on the order of 25 million years in general in Iceland. They start propagating horizontally. If we propagated these dikes down horizontally and they froze, that would be the end of it. But they're not. They are used over and over again.

What happens in this case, then, when they are used over and over, is that the systems become pro-grade. In other words, they actually start melting the crust. A dike by itself, propagating out by itself, can't melt anything, as we have said. The contacts are at basically 600 degrees, and they just move in on it.

The only way you can do this is by keep flushing the system, by new magma taking out the cold stuff and keep flushing it through all the time. Eventually, you can actually have the whole crust break down, and it reprocesses the crust.

We see calderas forming. We see silicic magmas coming right up in the basaltic material, et cetera. If you drill into the Icelandic crust, what you find really is you find a horizontal structure that is made from sills and lavas that have come beforehand.

This is a drilling section through the Icelandic crust. You see a lot of horizontal structure in it, wherever you see. Lava is in the top. You see intrusives at the bottom. They are sheetlike and sill-like.

Then the other structure you get, of course, are these propagating fissures in the system. They are fed from very strong magmatic systems, and they can reprocess the whole system. In terms of melting the wall rock, that is really the only way to do it, is to have a system where you are actually flowing the magma a lot, and you can propagate them out from the crust.

I just want to touch on, I just had one thing to touch on Bill's question of thermal viability. This is for the wrong kind of geometry, but the curves are very similar, what I will show here.

This is non-dimensional depth. In other words, you could say this is 30 kilometers or 10 kilometers, based on the exact problem. This is the solidification front regime; in other words, solidus to liquidus for a magma. The magma is all solid here. It is all liquid here.

Starting out from this point down in the crust or down somewhere in the earth, this is just a schematic for you, and here is a geotherm geometry, just the geothermal geometry. If the magma comes up very rapidly, adiabatically, it actually could arrive superheated in the earth's surface.

In other words, if it started out at its liquidus, it could actually come out superheated. This is basically -- adiabat means that it loses about 6 or 8 degrees per 10 kilometers, something like this. It doesn't lose that much. So it comes up almost isothermal.

What we see, of course, is the general geometry, and this is a system with 2 percent water in it, by the way. It has a little dog-leg here in terms of the liquidus going down. This depression is due to a little bit of water in the solidus also.

What this means is that, if we see magmas arriving at the surface with crystals in it, it means that they have intersected the liquidus somewhere. So you get a set of curves then, cooling curves, trajectories of magmas coming up under constant velocity. This is a dike here. If we change it to a dike, these velocities all change. They would have to come faster because the surface area to volume for cooling is so different.

So I also have all those. I just didn't happen to have these. These are some class notes I just brought today because I didn't know exactly where we would want to fit in.

But what this shows, then, and we can solve these things, and we can give the eruption rates over the geometries and you can see how big things have to be.

Now if you look at dikes, one thing that is very important in this area to look at, how big are the dikes. Bill was saying there was one dike, seeing how big is it, in Yucca Mountain itself.

DR. MELSON: A meter.

DR. MARSH: A meter. So, I mean, this thing is not very robust. That thing has to travel. Under normal speeds, it can't have come very far. Its thermal relaxation time, its thermal death time is very short, maybe only hours, for example.

So if a system, for example, vented, starting venting into the repository, it may seal itself rather quickly, unless you had a larger volume eruption Now the point Bill was making today, most of the systems we look at for analogs are systems that are large-volume systems. We are talking about a system here that is a very tiny-volume system. So to think that you take all the magma in one these eruptions and put it into the repository is worst than a very conservative estimate of what's going on.

(Laughter.)

But these things can be evaluated quite easily, most of these things, using the characteristics at hand.

So all I meant with this is to give a little bit of background and to kind of tie some of these things together, and to show a little bit about how magma really behaves.

Now another thing I didn't show, but I have in operation at Hopkins, and John has seen some of the -- is that we built a mush column, an experimental system of a mush column with horizontal tanks and interconnected conduits and things. We built a system to understand the eruption or the propagation of magma, the transport of magma, in one of these mush columns with a slug of crystals in it to see what the crystal load coming out the top tells us about the geometry down below.

Now the system is interesting. People

have brought up here about building an analog system. There's a lot of realization that comes from when you build one of these systems right off the start. For example, this is a series of plexiglas tanks with conduits that we can turn on and off and change the geometry in the whole system. It is about 6-feet tall.

If you want to look at this, you can go out to my website. We actually show the system with movies and everything in it.

But one of the things that is interesting is that, if you want to keep the system loaded, of course, with fluid in the lab, you have to have a series of check valves in these conduits. Otherwise, all the fluid just drains out all the time.

So magmatic systems are charged, and there is a series of check valves. As you know, any good plumber, any weekend plumber like me would know or you, is that you can have valves that have a flat valve, like in the back of your toilet tank basically, or we could have little ball valves that have a little reed in them that goes up.

But when we set this system up and charged it and started it in the first run, what we found out is that it went into harmonic tremor. The whole room started vibrating. I mean, you could hear this vibrating system through the whole room. Of course, shortly thereafter they had to crack one of the tanks, but that was the first time.

(Laughter.)

But why I mention this is that just to see one of these systems operate gives you a real feeling for the dynamics in the system. So, for example, if you wanted to have a flow like this invading an open reservoir with a series of waste containers set up of the right densities, the right mass and things, scaled dynamically, you can do it. You can't produce in our system a shock wave at all, but you can certainly see what the magma is going to do when it enters this thing under various scaled overpressures, driving pressures, driving heads. In fact, we have a problem keeping the heads low enough because our system has strength, plexiglas and things.

So these are things that possibly can be done, but one of the things that it is apparent from what I can see is that we have a granularity of research going on in this topic, and what you need is a continuum of it. In other words, you need to get these folks and other folks in the same room in real time doing the real problem, not doing a homework problem that we say is applicable here. But you really should do that. You should have a mini, mini-Manhattan Project here, where you actually solve the real problem with people who can actually address it in real time.

CHAIRMAN HORNBERGER: Thanks a lot, Bruce. That was very useful.

Questions for the group? John, you've been quiet.

DR. GARRICK: Oh, yes.

CHAIRMAN HORNBERGER: Uncharacteristically.

(Laughter.)

DR. GARRICK: Yes. Well, as you know, the way the NRC has been looking at this problem is in terms of the two components of risk; namely, the probabilities and the consequences. The more I listen to the experts, the more I am convinced that my original anxiety about that approach is correct. And that is that it seems that when you attempt to analyze what the consequences of these events are, it is very much dependent upon the assumption set that you employ.

If one avenue of putting this issue to rest is to be convinced that the likelihood or the probability is less than some number, then it seems to me that a very efficient approach to this would be to focus on those assumptions having to do with calculating the consequences that have the greatest impact.

As you soon as you start calculating consequences and start talking about assumptions on cooling and solidification, excess heat, and the moderation of pressure and the eruption sequencing, and what have you, you're now talking about the probability of the event in a very direct way.

So I think that there is considerable risk in separating these two issues too much. Maybe the coupling has always been there that I am concerned about, but I would like to hear you comment on this a little bit.

For example, if we were able to pick out two or three of the assumptions and drive them much more to an evidence-based position rather than an assumption-based position, and in the process pick up two or three orders of magnitude of probability one way or another, that might be a very efficient way to put this in context with respect to the kinds of risks that we are working about for Yucca Mountain.

Can somebody talk about that a little bit?

DR. MARSH: Well, I mean, I think that's really the essence of the problem here, is that what is happening really is that we are only able individually to solve certain kinds of problems. I mean, we can set them up, and we hope that we can over time get incrementally more realistic things.

So the situation you are in now is that we have problems, for example, the Woods, et al., which is a very, very nice -- and these guys are very, very competent and great workers and things, but the problem may have very limited relevance to what we are talking about here. But, nevertheless, it is out there as a signpost. There it is out there. They say, well, the word -- you know, Yucca Mountain is used in the paper, et cetera, and things like this. So it is a scenario where you have to kind of react to it.

Well, more realistically, it would be good to actually sit down with these fellows ahead of time and say, if we relax this thing -- this thing isn't very realistic and this one isn't, and that one could be changed a little bit. It changes the entire perspective of the impact of it.

So you're absolutely correct that there are issues that are based in assumption in all of these aspects, and those are the things that have to be revealed. In many of these processes the assumptions that are made aren't even known, even by the person putting the model forward. In other words, there are subconscious assumptions based in these.

In magmatic processes, for example, all people think of normally for 100 years all magmas are injected instantaneously into these sills and big bodies, instantaneously carry no crystals, which means the system is superheated and no system can ever deliver like that. But it is basically a system -- with those assumptions, then, if you want to explain the end product, you have to have the magma go through all kinds of gyrations to get the end product because the initial conditions are all incorrect.

Mostly what you see in magmatic systems is what it starts out to be is what it ends up to be. In other words, it isn't far from its -- you know, humans produce humans; they don't produce caterpillars. This is basically the way it is.

But even to reveal the assumptions and to kind of interrogate yourself, when you are putting these forward, sometimes isn't easy. So you actually need a group of people together coming from different perspectives and saying, "How about this right here?

Is that important?" "Yes, very important."

DR. GARRICK: But if we do that in the context of being deliberate and systematic about expressing the uncertainties, that gives us something to work with.

DR. MARSH: Absolutely. That's really a good way to proceed.

DR. GARRICK: And if those uncertainties involve a range of 10 to the minus 12 to 10 to the minus 9, then chances are we don't need to do anything else because the issue is it may be 10 to the minus 7 being driven by other considerations. That way of thinking, it would seem to me, would give us a benchmark against which to contextualize this whole issue.

DR. MARSH: I agree, and I think that that is really an interesting way to proceed. In other words, if we had enough expertise to say, let's take the shock model, for example, and say, okay, let's relax this assumption. What's that do to the probability? Where is the probability range? Then let's relax this one and look at this more realistically in real time.

What we have now is that we could have actually a whole series of models coming out from people, from any of us, dynamic models, and no probabilities attached to any of it, and then somebody has to go and not only understand what we are doing, but then put some realistic probability on it.

So there is this gulf. I think there is a gulf, there is a time lag here. There is a hysteresis effect between someone doing a piece of work and other people evaluating and ricocheting back and forth and getting down then eventually to a realistic probability. It is a long series, and the series is not converging very rapidly.

What you can do is you can make the series converge rapidly by getting the pertinent people right together and doing a real-time --

DR. GARRICK: And my point is the probability is not a point value. Probability is a distribution.

DR. MARSH: Right.

DR. GARRICK: If we know those distributions, very often we don't need to increase the precision of any particular parameter.

DR. MARSH: That's right. In other words, once you get to a certain level, you would say, "Well, we're not going to know these now, but they are sufficiently boxed in that we don't need to worry about them. That is the issue. The issue is whether or not you want to worry about these things and carry on with it.

CHAIRMAN HORNBERGER: Thank you.

Milt? Raymond?

VICE CHAIRMAN WYMER:: I've said enough.

CHAIRMAN HORNBERGER: Bill?

DR. HINZE: Well, I wanted to ask Bruce if he thought that one should be concerned about this horizontal flow associated with sills in the repository. You've talked about these. Everything we discussed regarding the repository are vertical dike intersections. What about the sills?

DR. MARSH: Yes. Sill formation, interestingly enough, usually takes place at some depth. In other words, the system we are looking at in Antarctica, for example, that is about a 5-kilometer, that was originally about a 5 kilometers deep to begin with, and we are looking at a whole series up through it.

We can actually see the venting in Antarctica. We actually can see these upper sills actually form feeders and they vent out into shallow lakes that look like and form phreatic eruptions. It didn't look particularly violent. You can actually see the material coming right down magma, actually transitioning from solid materials, from liquids into kind of ashy-type material.

But sills normally will form far from or significant differences from the last horizontal surface. In other words, you won't find a sill forming up in a mountain, for example. You will find it forming at depth.

It really comes down to, what I was asking really the questions of Derek in terms of the stress field in the crust, knowing what it is like. This really depends on what is going on out in that valley.

Now we are in the basin range. We always think of these "Horse-and-Gravin-type" structure with alluvium filling up a lot of the material in the valleys and things. But, I mean, we know, I think, seismically what those valleys are like. We know from the aeromags a little bit how much overburden we have.

DR. HINZE: And gravity.

DR. MARSH: And gravity. Great. Super.

So I think this would be a very realistic way to proceed. Then you can actually address some of these things quantitatively.

DR. HINZE: I would like to respond a bit too, John. I think all of us have these concerns. I am somewhat heartened by the work that is being done by the DOE at Los Alamos these days. We have, I suspect, just a small window into what is being done, but one of the things that is encouraging is that the peer review process is taking place not just at the end of the study, but during the progress of the study. So that we have these five experts, five-six experts who are there to tweak the system and to put some realism into the calculation, so that we will be able to understand the uncertainties.

Now at this point that is just a hope. I think we are going to have to see this play out.

CHAIRMAN HORNBERGER: Mike, you had a question you wanted to throw to Leon Reiter?

MR. LEE: Yes.

CHAIRMAN HORNBERGER: I'm warning you, Leon.

(Laughter.)

MR. LEE: Yes, and this is kind of a follow-on to I think a comment that Bill just had. I am focusing a little bit on the TRB report. First of all, many thanks to the TRB consultants for showing up today and the TRB staff for facilitating their appearance.

What has kind of cued my focus of inquiry here is the TRB in its 2002 Annual Report made a statement on page 10 that their concern has lessened regarding the differences in the NRC and the DOE modeling approaches. I am trying to go back to our role in advising the Commission.

I guess the question I have is, am I correct to assume that the DOE has the apparatus in place now to try to improve the maturity of the science for consequence modeling at Yucca Mountain? I guess that is the question I have for Leon or Dan. I see Dan Fehringer here, too. I'm not trying to put them on the spot, but I know you can't speak for the Board, but you could try to help us interpret --

MR. REITER: Yes, I can't speak for the Board. As you know, today the President appointed five new members and a new Chairman of the Board.

But, anyway, I think what the Board has said you have read; namely, in a previous letter we had a meeting last September 10th and 11th -- it was a terrible day to have a meeting -- and our consultants could not make it to the meeting. But at that meeting we felt there was a lot of unresolved issues between the NRC models and DOE models, and we were concerned about this. We raised our concerns about how can we proceed without resolving some of these concerns.

Then we met with our consultants, and the consultants gave the report. I think you got the gist of that.

MR. LEE: Yes.

MR. REITER: Being essentially that the models proposed are really more like end-member models rather than mean kind of models. There are lots of things you could do, look at, that you would probably relax some of these things. As a result, our concerns have lessened. We still think it is an important thing to work on because it is the largest contributor to dose in the first 10,000 years, and work is being continued on this. We are anxious to see the peer review model and we are following that process.

CHAIRMAN HORNBERGER: Thanks, Leon.

MR. LEE: Thank you.

CHAIRMAN HORNBERGER: I want to particularly thank, for the ACNW, Derek and Bill and Meghan for being here, their excellent presentations, and thanks for trying to educate us and answering our questions.

MS. HANLON: Dr. Hornberger, I just wanted to make one point to add a little information to our discussion here. That is, earlier Dr. Melson had mentioned the advantages and the usefulness of having a seismic network. I just wanted to let us all reconsider the fact that we have had a seismic network at Yucca Mountain since 1978, as well as since 1994 we have been updating that and digitizing it.

So we have a strong motion as well as a weak motion, that is, micro-seismic as well as regional monitors. We have more than 25 of the digitized weak motion networked and between 10 and 19, depending on how you count it, of the strong motion. Those are connected with the University of Nevada at Reno. If anyone were interested in a website, they have a website on the Nevada-Reno home page. I will just give that to you. It is www.seismo.unr.edu, E-D-U. That is under Research Projects, and it takes you into all of our seismic monitoring efforts. So I thought that would be useful for the audience to know.

CHAIRMAN HORNBERGER: Yes. Bill, do you want to get her to sign on for a --

DR. MELSON: Thank you very much. Are these broadband instruments that you are using? Will they pick up the higher frequency vibrations as well?

MS. HANLON: Yes. Yes.

CHAIRMAN HORNBERGER: Do you want to get her to sign on for 300 years of monitoring?

(Laughter.)

DR. MELSON: That really is the issue, isn't it?

(Laughter.)

CHAIRMAN HORNBERGER: Again, thanks again to everybody. We are now going to take a 15-minute break.

(Whereupon, the foregoing matter went off the record at 3:37 p.m. and went back on the record at 3:52 p.m.)

CHAIRMAN HORNBERGER: Okay, we will reconvene.

We are going to talk now about the Package Performance Study, and the lead Committee member for this is Milt Levenson. So he will run the meeting.

DR. LEVENSON: Our general topic is spent fuel transportation, and internal to that a fairly important factor is the matter of identifying the nature of the risks that arise from performance of the package. This is independent of whether the truck drivers run over somebody or other types of accidents, and getting information, bringing ourselves up to speed.

Package Performance, of course, has a long history. There have been a lot of tests done going back several decades. The Package Performance Study, as we understand it, is an attempt to bring up-to-date, due to changing conditions and criteria from what we know in the past. So the Committee is quite interested in hearing what the plans are for the Package Performance Study, how it will help us address the question of plans for shipping.

MR. SORENSON: Can you all hear me okay? Okay, thanks, Mr. Levenson.

Good afternoon, everybody. My name is Ken Sorenson. I am the Manager of the Transportation Packaging and Risk Department at Sandia National Laboratories. We are the prime contractor for the NRC to conduct the Package Performance Study.

I would like to introduce my colleague Dr. Jeremy Sprung here. He is the principal lead for the Package Performance Study as well. So I may ask him during the course of the comments to comment on some of the technical matters as they arise.

What I would like to do today is talk, give you a status of the early part of the Package Performance Study and where we are with what we call the test protocols, which are a preliminary snapshot of some testing that is being considered for the Package Performance Study to further help in the understanding of cask and spent fuel behavior in severe accident environments, both mechanical and thermal environments.

This is the talk as I have outlaid it today. Just to give you a little bit of context with the protocols and Package Performance Study, I would like to talk a little bit about the history of some of the more seminal NRC transportation studies that have occurred and then talk about NUREG 6672, Contractor Report 6672, in a little bit more detail, because that was the most recent reexamination of transportation risk assessments that has been done for the NRC. That was published in the spring of 2000.

Then from that, we will talk more in detail on the Package Performance Study: first, the Issues Report, which is really Phase I of the Package Performance Study, and then the test protocols.

So to start just a little bit of history on major transportation studies sponsored by the NRC, the first one was NUREG-0170. That was done in 1977. That was an Environmental Impact Statement on the risks of transporting all types of nuclear materials over all types of conveyances. I think there's like 26 different categories in the nuclear materials that were looked at. Spent fuel was one of those, as for transportation on road, rail, barges, airplanes, and those sorts of things. What that study did is it confirmed the appropriateness of the regulations as they were to provide safe transport of these materials to public health and safety as well as to the environment.

The second report that I show here is NUREG Contractor Report 0743. That is referred to as the Urban Study. That looked at transporting spent fuel through a highly dense urban area. In this case, it was downtown Manhattan. Again, it affirmed the appropriateness of the regulations to provide safety to the public and the environment during transport of spent fuel. This was also the first study that looked at a sabotage-type event on these type of transports as well.

The third report is Contractor Report 4829. It is referred to as the Modal Study. That was done in 1987. That looked at analytically shipping container response to severe mechanical and thermal environments.

That was a big step in the ability to do risk analysis. This is the first case where they actually quantified an event tree that looked at specific scenarios and severity fractions, and then also assigning probabilities to these scenarios of likelihood of occurrence. So that was a big step.

Then, finally, the fourth report shown here is 6672. That was published in the spring of 2000, and that was, again, a further step forward in the ability to better estimate the risks of transporting spent nuclear fuel both by highway and by rail.

What you see here I think is an evolution of assessing and estimating transportation risks over a period of about, right now we've got about 23 years. It is part of the charter of the NRC to continually look at the state of transportation and its operations and the way these materials are shipped, to again assess the safety of these shipments both to the public and to the environment, and also as a way to look at the appropriateness of the regulations.

Let me talk a little more specifically on NUREG 5572. Again, that was published in the spring of 2000. I'm going to give you the conclusions first.

Basically, the conclusions are that the transportation risks to the public in this document are better estimates than either in NUREG 0170 or in the Modal Study or in the Urban Study for three main reasons:

First, there's more advanced analysis techniques in terms of finite element analyses and those sorts of things that we do, get quantitative estimates, both the mechanical response of the cask and thermal response of the cask. There is more detailed evaluation of transportation routes, and I think the third bullet, new and better data, has been significant as well, especially in doing route analyses.

The first two bullets, a lot that is wrapped up in that is computer power. With the advent of high-speed computers, parallel processing, and those sorts of things, we have been able to make quantum leaps in the ability to analyze cask response in these mechanical-thermal environments, but also to do some very detailed route analyses as well, to provide these better quantitative estimates of risk.

Now this last bullet, what we show in terms of results in 6672 is that non-accident and accident transport risks are estimated in 6672 lower than those in 0170. Again, they continue to support the appropriateness of the regulations. Again, this is an evolution that the NRC has been going through periodically looking at analysis techniques and the data, and being able to take advantage of these advances, and to provide better estimates of risk of transporting nuclear materials.

CHAIRMAN HORNBERGER: The non-accident risks are the radiation, using linear no threshold?

MR. SORENSON: Yes.

CHAIRMAN HORNBERGER: I mean, what are they?

MR. SORENSON: Yes, right. Incident-free risk we call that, just if you are driving alongside a cask on the highway and that sort of thing.

The accident risk results, this is just to give you an idea of perspective here. Please don't strain your eyes trying to read that, but for the accident conditions the risks that are estimated are two to three orders of magnitude lower than those estimated in 0170.

For incident-free, the difference is smaller, but it is still lower, because for non-accident sorts of conditions, even back 20 years ago, it was much easier to estimate dose because you had known conditions of transport as opposed to accident conditions. But what this shows is that for quantifying the risks and comparing them to 0170, the estimates are much lower than what were previously estimated.

Given that, we still identify some conservatisms that were in 6672. Part of that is constraints of budget and schedule, also constraints of analytic capabilities and things like that. But, for example, I've got three main bullets here that show what some of the conservatisms are in the 6672 analyses.

For the impact analyses, response of the cask to these severe mechanical loads that we looked at, we assume that all end and corner impacts were on the closure end of the cask, where you get more likelihood of lid deformation and potential failure of the seal area.

We assumed all impact energy goes into cask deformation. So the velocity of the cask was at normal right angles to the impact surface. So all that kinetic energy was absorbed by deformation of the cask. It wasn't transferred into momentum sorts of transfers and those sorts of things.

Thirdly, we did not look at the canistered fuel, which I think we see a lot now of the industry going to canistered fuel as opposed to air fuel shipments. That was not analyzed.

For the thermal analyses, we assumed all fires are optically dense and completely surround the cask for the entire duration of the fire, and we assumed for these analyses that the fire temperature was 1000 degrees C. The regulations state 800 degrees C.

Then the source terms, we assumed a three-year, cooled, high-burnup fuel for source terms. That is really a pretty large conservative in the analysis of the actual dose.

So those are the sorts of conservativisms that were still in 6672, but we still had lower estimates than what we had in 0170.

So let's leave the history and go to the Package Performance Study, which really came right at the heels of 6672. The Package Performance Study, the purpose is to, was to, well, still is to, identify and implement near-term -- this is a five-year timeframe -- R&D transportation work for the NRC.

We really used a lot of the work that went into these previous risk studies, not only 6672, but 0172 and the Modal Study and all those, as a springboard to look at where we needed to go next in terms of advancing the technical abilities and the public confidence and the programmatic goals of the NRC in the Package Performance Study.

So I've listed three goals here for the

PPS. One is to validate the assumptions and methodologies used to assess the appropriateness of the NRC regulations. A lot of this is in the computer code analyses that are used.

A lot of the public comments, we got a lot of comments from people that they didn't really trust the analyses that were presented. So one of the reasons for this is to be able to better demonstrate the ability of these analyses to properly capture cask response.

Secondly, demonstrate the safety of land transport to stakeholders and the public, and, lastly, advance the knowledge base of cask and spent fuel behavior, not just the cask, but also the behavior of the spent fuel in these severe accident environments during transport accidents.

As I said earlier, the PPS uses 6672 and the other earlier risk studies as a springboard to start the work. It is important to note -- and I will probably repeat this several times during the discussion -- in terms of the protocols, these are preliminary analyses and preliminary recommendations. We are presenting these to the ACNW. They will be out for public comment. The NES will have a chance to look at these. So this is a first-cut preliminary look at where we think we need to go for the Package Performance Study to better meet our objectives, but certainly these aren't the final recommendations that are being made. This is a preliminary look at where we are headed.

The first part, Phase I of the Package Performance Study, is what we call the Issues Report. After 6672 was published, we had some technical review on 6672, and we also had public meetings where we went out and we presented the results to the public. During those meetings we got a lot of feedback on the results of 6672.

We used the Issues Report in the third bullet here, what the Issues Report does is translate these stakeholder public inputs from these meetings into proposals for the Package Performance Study. So we had a long, long list of comments of things, maybe shortcomings from 6672 or things that weren't covered that needed to be covered. We simulated these comments into basic categories like mechanical events, fire events, entries, spent fuel behavior, things like that, and then prioritized the comments based on, if we addressed these particular comments and worked on them, how much of an impact would it have in terms of advancing the demonstration of safe transport.

That was begun in 1999, and the report was issued in June 2000. We also put the results of the report on a web page to get further public input. We had a total of eight public meetings, and four were in 1999, I believe, and four in 2000. We had them in Pahrump, Nevada and Las Vegas and here in White Flint, to get as broad a cross-section of comments from the stakeholders as possible. Then also we got direct comments on the website.

This is a list of stakeholders that we would include as people who have interest in this particular area, but certainly nuclear industry groups, transportation groups, DOE, DOT, state and local and tribal governments, public interest groups, and then just other members of the public as well. We got comments from all of them on 6672.

The results of Phase I of the Issues Report really formed a basis for the work scope as identified in the Package Performance Study. It really focused on five main areas that needed to be addressed to better define and fill some of the gaps were in 6672.

The first work scope item there is perform 3-D finite element analyses to capture the cask and fuel behavior in severe mechanical loadings. One of the constraints in 6672 was in the bolt area, in the closure area, because of computer limitations, we had to do a fairly coarse meshing of the bolts. We feel that to properly capture bolt behavior that needs to be much better, the meshing needs to be much better refined. But that has a tradeoff because it really increases geometrically the amount of computer time that you need as well.

One of the big comments from the public is, how does the fuel really behave in these severe mechanical environments? There's not a lot of data out there in terms of how this fuel behaves. So this is one area that we have put in test protocols as one of the main things, principal things that need to be looked at.

The second bullet here is perform 3-D finite element analyses to capture cask and fuel behavior in thermal environments. We did a 1-D finite element analysis in the thermal environments. Because of that, we were not able to very accurately determine the seal performance and temperatures around the seals. We need to do a 3-D finite element analysis to make sure that we properly capture that performance.

Conduct impact tests on fuel elements to characterize rod and fuel behavior in dynamic loading environments. Not only do we need to do the analysis up here, but we also have to have some empirical data, so that we can benchmark the analysis behavior results that we get to actual test results. Again, there is not a lot of data out there on fuel behavior from the pellet to the pellet inside the rod, to the assemblies.

Then, finally, reconstruct the accident event trees and accident speed and fire distributions. We used basically the event trees that were in the Modal Study in 1987. There was a lot of comment during the public meetings that there is a lot of new data out there. We went from 55 miles an hour on the highways to 75 miles an hour on the highways, and we needed to update the accident event trees and associated probabilities in those as well.

So we are working that as part of the Package Performance Study, but you will not see that in the protocol document because that doesn't involve testing. That is strictly looking, evaluating databases and doing the analysis on the databases.

Okay, so let's talk about -- that's the Issues Report, and that's what kind of got us to identify and define the five main areas in the Package Performance Study. Looking at the preliminary analyses and test recommendations is the responsibility of the test protocols.

Again, this is a document that we have done over the past six months or so that includes preliminary structural and thermal finite element analyses on a cask to determine appropriate orientations for a test, appropriate speeds for a test, to again demonstrate the safety of this cask, to be able to demonstrate that we can properly capture the response analytically of these casks during these very severe environments.

So here I define what's in the protocols. Again, a conceptual level for impact fire and fuel tests is defined in the protocols, and for the impact and the fire they are supported by preliminary analyses. These protocols will be published very soon. I think sometime in July they will be available for public distribution and then review and comment.

Then we will use these protocols, along with comments we get from the ACNW, the NAS, and the public, to define the test plans, the actual test plans that will be used to conduct whatever tests that are decided on that need to be conducted. Again, as I said earlier in the last viewgraph, the non-test issues in the Package Performance Study are not handled in the test protocols. They are handled separately, but are still part of the Package Performance Study. That is basically the reconstruction of the event trees, as we have described.

Okay, let's talk a little bit about the testing analyses that have been done and associated proposed tests. We picked a cask to do the initial analyses, and the cask that was picked was the HOLTEC Hi-Star 100. Again, in the public meetings we had a fair number of comments from people who raised their hand and said, "Show us a test. We want to see a cask that is currently certified by the NRC that is going to be rolling down the road at Yucca Mountain, and it needs to be big. It should probably a real cask."

So using that as part of the criteria -- we had other criteria as well, but we chose the HOLTEC Hi-Star cask as the cask to look at for these protocols in our preliminary analysis. Again, I want to stress that no decision has been made on what actual cask will be used for these tests, but this is what is used in the protocol document.

This just gives a axis symmetric view of the cask, and here a bolt detail. Again, the bolt details for these preliminary analyses is the meshing is rather crude. For the final test analyses, we will do a much more detailed job on the bolt and closure area.

For the protocols, we looked at analysis on three different orientations, kind of classic orientations: end-on, CG-over-corner, and the side-on. Again, this is CG-over-corner with the closure end at the down position.

We did those three analyses for two different impact speeds, one at 60 miles an hour, one at 90 miles an hour. This is with impact limiters on an unyielding target.

For a point of comparison, the regulatory environment is a 30-mile-an-hour impact onto an unyielding target. So you can see that this is really a much more severe impact or insult to the cask than what's in the regulatory environment.

I might also point out that in the regulatory environment that 9-meter drop test onto an unyielding surface captures about 99-plus percent of all real accidents. So what we are looking at here in this 60-to-90-mile-an-hour regime is really the tail-end of the accident distributions in terms of severity for mechanical and thermal impacts. It is an important point.

CHAIRMAN HORNBERGER: But it is credible?

MR. SORENSON: We have looked at the databases, and we have seen impacts or, excuse me, we have seen accidents up to 90-100 miles an hour. Now you could argue whether it is done yielding surface or not, probably not. So I guess the point is, what is credible, what is incredible in terms of how far you take this?

DR. LEVENSON: But in translating the vehicle speed, which is where the database is, I think, to this impact, you are ignoring any energy absorption in this thing tearing itself loose from the truck?

MR. SORENSON: You are ignoring that, and you are ignoring soft targets as well.

DR. LEVENSON: Right. Well, you're ignoring most hard targets because the hard target you have is significantly harder, I think, than any -- what is it, 25-foot-thick concrete slab?

MR. SORENSON: Yes.

DR. LEVENSON: Not many roads like that.

MR. SORENSON: Okay. This just gives a couple basic results.

DR. LEVENSON: Not to belabor this, but you say you're out at the tail-end of the distribution. Can you give me some feel how far out? Are you --

MR. SORENSON: Like how many sigma?

DR. LEVENSON: Yes.

MR. SORENSON: No, I really didn't -- Jerry, do you care to comment on that?

MR. SPRUNG: Probably four nines or so.

DR. LEVENSON: Four nines?

MR. SPRUNG: There aren't many accidents up there, and it is probably further out, if you take the accident speed and ask what's the chance of there really being something like an unfractured assault on igneous rock to hit, of all there is out there that is really close to an unusual target.

MR. SORENSON: Thank you.

This shows a finite element analysis result of the 60-mile-an-hour CG-over-corner center gravity over corner onto an unyielding target. You see a lot of damage to the impact limiter, but really no damage at all to the cask. Again, the important thing to look at here is the closure area with the bolts and basically there's no problem here. It maintains its integrity.

This shows accelerations. This is up about 50-55 Gs are so is the load on that. So it is not real huge. But, again, the regulatory accident, hypothetical accident condition is about a 30-mile-per-hour impact.

By the way, just to let you know for this particular cask design, this is a very complicated impact limiter and a very, say, conservative design. There's three different impact crush materials in here, a honeycomb with different compressive strengths. There's internal piping and gussets in here as well to provide additional structural strength in it.

CHAIRMAN HORNBERGER: Am I correct in assuming, if we look at this versus the 30-mile-per-hour, this would be more than a linear extrapolation? This is more than twice as bad as the 30-mile-an-hour test?

MR. SORENSON: Yes. If you look at it in terms of kinetic energy --

CHAIRMAN HORNBERGER: Yes, it's squared, right?

MR. SORENSON: Yes, it's V-squared. That's correct.

This is the 90-mile-per-hour impact. One of the things, I will touch on this in a couple of slides. We had an expert panel review. Some structural experts from around the country looked at the protocols, and also the public said that, you know, we would like -- if you say you can really capture the response of these casks, to do a regulatory drop, the cask remains in a linear elastic regime and you really don't measure anything. What they really said, what we would like to see is some plastic deformation of the cask itself, the cask by itself.

You will see here right around the flange shoulder of the cask body you do get some actual plastic deformation in this area. This is the closure lid, and then this is part of the cask body, where you actually do get some plastic deformation.

CHAIRMAN HORNBERGER: Who is it who wants to see plastic deformation?

MR. SORENSON: Well, some people in the public made that comment, not from the standpoint of, can you really capture cask response, and it's a "No, never mind" to capture cask response if it remains linear elastic, but if you can show plastic deformation and capture that appropriately with your analysis, that is what they want to see. The expert structural panel also mentioned that as well.

This shows a G loading here. Again, this answers your question that we go from about 55 Gs up to 140 Gs, and it's 50 percent higher impact velocity and about three times the G force.

MR. KOBETZ: Do you know what's happening to the bolts up there?

MR. SORENSON: Yes. Boy, you have a great segue. That's the next viewgraph actually.

The bolts, this is at -- let me superimpose this real quick here. The highly-strained bolts are on this part of the impact, the upper part of the impact. You can think of the cask impacting this way and the bolts up here are the ones that are highly strained.

This shows there's 54 bolts; they are an inch and five-eighths diameter bolts around the cask lid, but this shows a strain plot of individual bolts in the highly-strained area and this shows a plot going around the circumference of the enclosure, where the bolts are, in terms of opening. That shows, I think, about a .2-inch opening at the worst highest spot.

What we are looking for here is, these are metallic seals in this cask. So if you take that opening and you subtract out the compliance of that seal, that's how much of a gap you are going to have in that closure area. You can integrate those specific gaps between each bolt and get a total opening around that.

So we have a case for these preliminary analyses, 60-miles-an-hour, where we have no opening of the cask lid, and 90-miles-an-hour, where we have a small opening of the cask lid.

MR. KOBETZ: Ken, can I also ask, how come you didn't look at slapdowns? Because I know I have seen one test at Sandia where that was the worst drop for a cask.

MR. SORENSON: It is very difficult to analyze properly, for one thing. In terms of the objectives of the test, we felt this was a good orientation for doing the actual testing.

It depends really on the cask design, particularly like the LD over R ratio, the length versus the diameter of the cask, which is the worst orientation, the slapdown and CG-over-corner, and so forth.

So the recommendations from the structural part of the protocols are to conduct detailed finite element analyses on the HOLTEC Hi-Star cask with impact limiters for the final procedures. Again, if we decide on the cask orientation of CG-over-corner, we will only do detailed analyses for that particular orientation.

Right now the recommendation is to do the impact speed of somewhere between 60 and 90 miles per hour, and, again, as I said, to have increased attention on the modeling of the closure lid and the bolts and the impact limiters. The other thing I need to put in here that I missed is the recommendation to actually do the test of that cask as well, based on these analyses.

For the thermal analyses, we looked at really three cases. Regulatory cases, let's say it's just 1 meter above. We looked at 1.3 meters. This is a nuance of the meshing that we did in our particular program. It was either 1.3 or less than 1. So we wanted to put it at 1.3.

But we looked at three different locations in the pool fire and what the effect of that fire would have on the cask itself 1.3 meters above the pool, .3 meters above the pool. You think about it, in most accidents the cask is probably on the ground in the fully engulfing fire. So we thought it was important to look at this case in particular.

Then Case 3 was 3.3 meters above the pool. I will show you some pictures of the actual fire envelope. You have a vapor dome underneath the cask that does not have enough oxygen to combust the fuel mixture there. So you have a relatively cool spot right underneath the cask, which is called the vapor dome. So to put this a little bit higher, it gets you above that vapor dome and it gives you a more uniform heat load on the cask itself.

This is the regulatory case. Here you can see the vapor dome, where you have a relatively cool area on the lower surface of that cask. This actually looks at temperatures of the cask, and part of the vapor dome actually extends up the side of the cask and the middle part of the cask as well.

This is the cask on the ground, and you can see relatively, if you remember the last picture, you have the relatively cool bottom area of that cask, as you would expect.

DR. GARRICK: Ken, I remember reading that you are going to, among other things, determine gas flow velocities and heat fluxes, and that the gas flow velocity measurements are going to be partly based on pressure differentials.

Are you actually going to measure the pressure inside the simulated fuel rods?

MR. SORENSON: Oh, in the fuel rods?

DR. GARRICK: Yes.

MR. SORENSON: That's not the plan right now, John, but there is some pretty good data on there on burst rod temperatures. What we are really looking at right now is internal surface cask temperatures and time to reach those temperatures, because burst rod temperatures are about 750 degrees C. So once we get to that point, you can make the leap to say you are vulnerable to burst rod temperatures, but we don't have any plans at this point on measuring internal pressures of the pins, and so on.

DR. GARRICK: Okay, thank you.

MR. SORENSON: Then this is the cask located 3.3 meters above the pool, and here you can see the vapor dome is much less of an effect on the cask itself. Okay, you still get vapor dome issues on the ends here as well, but you get a more uniform temperature gradient over the cask surface.

DR. LEVENSON: Ken, is your thermal model for fuel temperature a fairly sophisticated one, element by element, et cetera, or are you going by the temperature on the inside surface of the cask?

MR. SORENSON: Well, we are doing both, Milt. We're looking at doing traditional fire modeling, which provides heat fluxes to the cask surface and then heat transport from the cask surface to the inside cask wall. We are also using an inverse heat conduction code which takes the response of the cask and backs out what the surface fluxes should be, so that we can tie those two together, so that we are confident that the fluxes that we are getting on the surface and the surface temperatures are right, so that we can properly model how this --

DR. LEVENSON: Because when you are going the other direction, and you've got fuel that is generating heat --

MR. SORENSON: Right.

DR. LEVENSON: -- it's damned hard to get the heat from the inner elements out to the cask.

MR. SORENSON: Right.

DR. LEVENSON: So the reverse process must also be true --

MR. SORENSON: Right.

DR. LEVENSON: -- that elements won't heat very fast.

MR. SORENSON: Right. As you know, we are not going to have the fuel elements --

DR. LEVENSON: You're not going to have it full of elements, we know.

MR. SORENSON: That's right.

DR. LEVENSON: But, I mean, just using the internal cask temperature, it is not really representative of what would be fuel temperature.

MR. SORENSON: I agree. Yes, I agree.

Okay, then recommendations for the thermal analyses, based on these preliminary -- the thermal analyses and testing, based on these preliminary analyses are, again, conduct more detailed modeling analyses. We are recommending in these protocols to do calorimeter tests, so that we can properly get the heat flux on the cask surface as well as initial heat temperatures, so we can make sure we get the proper boundary conditions in these fire environments.

We are recommending doing two full-scale calorimeter tests, one above the vapor dome -- we don't specify an actual height at this point, but somewhere above the vapor dome -- then one on or near the ground, and then conduct detailed modeling analysis for full-scale casks based on these calorimeter tests. Finally, do two full-scale casks full-fire tests. Well, one on the ground is recommended and then one above the vapor dome.

Okay, I am going to talk a little bit about the fuel test program that we have. Again, in 6672 and all the earlier reports they talked about there was lots of assumptions and inference made in terms of how spent fuel is going to behave in these very severe mechanical and thermal environments. As I said earlier, there's just not a lot of data out there.

So when you do finite element analysis, mechanical response and thermal response, and then you take the leap and look at how, if you breach the fuel, how that fuel deposits on the inside of the cask perhaps or how it becomes aerosolized and gets outside into the environment, you have to make a fair amount of inferences and assumptions on how that is done.

So this part of the Package Performance Study is to get some data, so that we can have a much better basis to tire our analysis to in terms of how this fuel behaves during these very extreme environments.

So here the objective: Given a particle release from the failed spent fuel rod, the goal of the rod, pellet, and CRUD impact test is to develop data that can show -- and then think about there's the spent fuel pellet, the centered pellet inside the Zircalloy tubing, and then all those tubes make up the assembly.

Also, on the outside of the assembly there is a CRUD that is formed. This is an acronym that stands for Chalk River Unidentified Deposits. It is basically in the BWR. It is a nickel-iron spinel that that precipitates out of the spent fuel pool and attaches itself to the outside of the zircalloy tubing, and it turns into cobalt-60, radioactive cobalt-60.

So how that CRUD behaves and performs on those assemblies is important to know because right now a lot of people say you need to assume 100 percent of that becomes aerosolized and gets out and contributes to dose. So we are looking at doing some experiments to better quantify what really happens with that CRUD.

So the first objective we show here is to determine whether fuel fines -- and these are the actual centered pellets -- form particle beds inside the spent fuel rods as a result of a mechanical impact. Then if these particle beds form, whether they efficiently filter other particles that pass through them, so that some of these crushed particles then cannot escape out into the environment; whether CRUD particles will spall off spent fuel rod surfaces, and if the rods are subjected to mechanical impacts or thermal stresses, and then what is the size distribution of these released particles? Are they respirable particles or not? Will they settle out quickly? What is the size distribution of these particles that get out?

We have quite a few tests planned for this. This shows one of the testing aerosol chamber. Actually, there's a very good company in Germany that is very good at aerosol physics and testing, to get this kind of data, particle size distribution and those sorts of things. We are actually going to use them to do these types of tests, so that we don't have to replicate them.

This just shows a schematic of one of the test apparatus that will be used to get that data.

MR. KOBETZ: Ken, are you going to use high-burnup fuel or medium-burnup fuel, or does it matter?

MR. SORENSON: Right now we are going to use, the plan is to use regular and high-burnup both and see if there is a difference between those in terms of how they behave.

That, frankly, is one of the issues. In doing the testing, the plan is to do the spent fuel testing at Sandia. One of the ES&H issues is, what happens to that fuel after you do the testing? Can we ship it back to Germany from whence it came? Will they accept it or not? If we keep it here in the United States, who becomes the proprietor of that spent fuel? So those are some of the detail issues that we are working out right now.

Okay, that is the spent fuel testing. I did want to take a little bit of time to cover the expert panel. At NRC's guidance, we formed two expert panels for review of the work that we are doing, and specifically the protocols. One was a structural panel and the other as a thermal panel. We got experts from academia and industry. Well, we had one from the underwriters' company, and we tried to get a very broad cross-section of people. One is the ex-president of ASME International. So we thought we got a very good cross-section of experts. EPRI was another source that we went. We got a very good cross-section of people to look at this and give it a critical review.

The composition: five members on each panel. We vetted this with the NRC. Then on April 10th and 11th, we actually had a review or we actually had some international participation as well. There was a person from Ontario Hydro and then a person from the BAM in Germany who were on the Committee.

We sent the protocols out ahead of time for them to review, and then we had a two-day meeting where they came in and gave us their critical comments on the protocols.

These show the principal results of these reviews. The structural review panel agreed with the basic approach, as was developed in the protocols. One of their recommendations is not only should we conduct an extra-regulatory test, but we should also conduct one regulatory test. This is to tie the testing and analysis and make a hard link from the regulatory regime to the extra-regulatory regimes.

They said the extra-regulatory tests should focus on closure damage, and the drop height should be such that we bottom out the impact limiter. Again, it is specifically for the HOLTEC Hi-Star; it was a very robust limiter. Even for the 60-mile-an-hour impact loading, we did not use the full stroke of that limiter.

So their recommendation was, regardless of what cask you choose, you should configure the design, the test design, such that you bottom out the impact limiter and you achieve closure deformation. What they mean by that is actual plastic deformation of the closure, again, so we can show or demonstrate that we can properly capture cask response under these environments analytically.

DR. LEVENSON: Ken, what was the charge to these panels? Because what kind of recommendations and review, having been on a number of such things over the years, makes a big difference in what kind of recommendations you get, as to what their charge was. How was their charge worded?

MR. SORENSON: Well, let's see --

DR. LEVENSON: Was it to test what really happens or was it to maximize the technical information you would obtain, whether it is directly related or not to safety? I mean, what was the charge to them?

MR. SORENSON: I understand the question. I'm trying to think back, how we worded the actual call letter.

We tied it to the objectives of the Package Performance Study, which was to advance the technology --

DR. LEVENSON: These three that you have here?

MR. SORENSON: Yes, we tied it to that, and I think with special emphasis on the technical aspect, to make sure that the technical aspect was sound, the approach was sound, to do this.

Does that answer your question, Milt? That is basically what we did. Again, the objectives in the Package Performance Study and gave them pretty free reign in terms of -- and we had a lot of discussion in terms of, you know, well, what is the extra-regulatory test? Because the 60-miles-an-hour impact, for example, you get an elastic response onto an unyielding surface? Is that really something that needed to be tested at this point, if you don't show any plastic deformation?

DR. LEVENSON: Well, you know, I could interpret extra-regulatory to say anything from 10 percent more than what's required by the regulations to two orders or three orders of magnitude more than required by the regulations. So I'm not sure I know what this means.

MR. SORENSON: Well, I think if I can bound that a little bit, from the recommendation here, it is that they wanted it to be severe enough that we would actually show closure deformation, plastic deformation.

DR. LEVENSON: That's not what it says. It just says, go beyond what the regulations are.

CHAIRMAN HORNBERGER: But the qualifying

phrase suggests that it is going to have to be five times what the regulatory thing is, because you are not going to get closure deformation unless you go to something like 65- or 70-miles-an-hour.

MR. SORENSON: May I go on?

CHAIRMAN HORNBERGER: Yes, go ahead.

MR. SORENSON: Okay. The fourth bullet is, you know, there was also a lot of discussion on what's a successful test, what's your metric? We haven't completely closed that loop yet, but one of the strong emphases is for the mechanical regime to focus more on deformations as opposed to accelerations and the strains, because of the uncertainties that are involved in some of these measurements for these very severe environments.

For the Thermal Review Panel, again, they agreed on the basic approach. They recommended that we add three additional calorimeter tests because of the wind concerns. You actually go out there and you do the test. If you have a breeze or a wind, it is going to significantly affect the fire environment and, subsequently, the boundary conditions on the cask during these fire tests. So they recommended that we do additional testing under wind conditions with the calorimeters.

Yes?

CHAIRMAN HORNBERGER: Is it likely that having a wind would somehow make the fire worse?

MR. SORENSON: Less worse.

CHAIRMAN HORNBERGER: Yes, that's what I was just saying --

MR. SORENSON: We assume fully engulfing.

CHAIRMAN HORNBERGER: Right.

MR. SORENSON: So it would be less.

CHAIRMAN HORNBERGER: So I am not sure that I see, if you are going to these extreme tests, why do you want to have a wind to make them slightly less extreme?

MR. SORENSON: Well, the point is, we want to demonstrate that we can capture the response analytically. If we have a test where we have a wind condition and we have an analytical condition where it is fully engulfing, they are not going to match. So we want to be able to bound that condition.

Jerry, did you have a comment?

MR. SPRUNG: An engulfing pool of fire is not a well-mixed fire. So the wind may cause more oxygen to enter the flame; it burns hotter, but if it blows the flame away from the cask, then it is less severe. But if it is offset fire and you engulf the cask and it tipped over, wind-driven flame, it could be worse.

MR. SORENSON: Okay. Some of the outstanding issues that are still unresolved: final configuration of the calorimeter and the cask thermal tests; the configuration of the impact tests, but also the cask. Again, as I said at the outset, there's not been a final decision made as to which cask will be used for the test, and with that, what the scale of the cask will be.

There is, I think, some good suggestions that we also do some pre-test predictions on commercially-available codes. To the extent that that is possible, I think that is probably a good idea, and then also possibly do some round-robin analyses as well, to have other analysts analyze these test configurations and see how well we can match the analyses, and give us, again, confidence in the ability to properly capture cask response for these types of environments.

VICE CHAIRMAN WYMER:: Who is qualified to participate in round-robin analyses?

MR. SORENSON: Well, that is something I think we would have to look at and see how we set up that criteria. We haven't done a lot of thinking about that, to be honest with you, but to do this properly we would have to make sure that whatever organization or laboratory did those analyses had the proper experience and educational background to do it properly. We certainly couldn't just give it to anyone out there who volunteered to do it.

CHAIRMAN HORNBERGER: So I noticed on this slide you have scale model tests possible. So are some of the more extreme tests, like 90-mile-an-hour impacts, would you envision doing those with scale models?

MR. SORENSON: No, actually, we envision doing them full-scale. I just put this up here as a caveat, just to make sure that it's stressed that no final decision has been made.

CHAIRMAN HORNBERGER: Wouldn't a scale model test for some of these extreme things make a lot of sense? Wouldn't it save you a lot of money?

MR. SORENSON: Well, yes. We have done a fair amount of scale model testing actually. Again, from public comments we've gotten, there has been a strong indication that people want to see full-scale testing because we really haven't done full-scale testing of rail-sized casks.

CHAIRMAN HORNBERGER: Do you have any

doubts that your scaling laws don't work?

MR. SORENSON: No, I don't personally.

DR. LEVENSON: Is "full-scale" a truck cask or a railroad cask?

MR. SORENSON: Well, Milt, for this particular protocol, the preliminary analysis, it is the rail cask. It is the Hi-Star 100 cask that we are looking at.

Then the final viewgraph here gives some approximate dates that we are looking at in the near-term for the Package Performance Study. For the field testing, we actually have made some good progress in getting started on that, and we plan on doing these testings in the fall of this year.

Thermal testing, the calorimeter test, that is to be determined yet, but we are looking at the cask fire test in the fall of 2004. Then for the impact test, we are looking for that test in the summer of 2004. So the intent is to do the impact test with the cask, and then take that cask, after that impact test, and do the fire test with it.

VICE CHAIRMAN WYMER:: You talk about surrogate pellet impact test. What are your surrogates?

MR. SORENSON: Well, we will use glass,

Pyrex, and also an irradiated UO2 -- or DU, is it DU, Jerry?

MR. SPRUNG: Yes.

MR. SORENSON: I'm sorry, DU.

VICE CHAIRMAN WYMER:: Spent fuel is sort of, it has been irradiated and it is kind of fragmented, and it is not a whole lot like glass.

MR. SORENSON: Go ahead, Jerry.

MR. SPRUNG: The impact facility doesn't like radiation too well, so the impact test will be done with surrogates and then we will go to a facility that can handle radioactive materials and do so bare pellet impact tests where we look at both some low- and high-burnup pellets and compare them to the way the surrogate behave, so that we get some feeling for the differences, if any, between surrogate brittle materials and pellets that have been degraded by radiation.

The available data suggests that most brittle materials fracture fairly similarly, at least in terms of the size distribution you get for impact at any particular speed.

VICE CHAIRMAN WYMER:: It would be 30,000-40,000-megawatt-a-day-per-ton burnup?

MR. SPRUNG: That and some high-burnup,

too, up at 55 to 60.

VICE CHAIRMAN WYMER:: Oh, that high?

MR. SORENSON: That's the plan.

MR. SPRUNG: That's the plan.

VICE CHAIRMAN WYMER:: You will have a hard time finding it.

DR. LEVENSON: Would those tests be done before you use the surrogates in all of the other tests? The context of the question is, do you validate the surrogates before you use them?

MR. SPRUNG: The surrogates are fairly well-validated in a sense already, in that there is some data on fracturing of DUO2, and it fractures quite similarly to -- the centered DUO2 pellets fracture quite similarly to glass. The German scientists who conduct this think that's already, in terms of the precision of risk assessment, well inside the ball park, but we would like to confirm that with the tests on the real stuff.

The order at the moment is conducting tests with highly radioactive materials is never simple, and particularly when we are trying to get some German support for the funding of those tests.

DR. LEVENSON: Was there any consideration to just using unirradiated UO2 pellets?

MR. SORENSON: Was it just DU we looked at or did we consider --

MR. SPRUNG: Fresh -- no, we will use DUO2 pellets in the German tests. When we go to a radiation facility, we will use DUO2 glass and then actual spent fuel pellets.

MR. SORENSON: But, Milt, your question was fresh pellets?

DR. LEVENSON: No, no. My question was, I thought you were using the glass pellets or irradiated UO2.

MR. SPRUNG: No, glass or depleted uranium dioxide in the facility that doesn't handle high radiation. In hot cell tests we will look at a Plene Hammer that is drop-weight tests for fracturing of glass pellets, depleted uranium pellets and high-burnup spent fuel pellets.

DR. LEVENSON: I guess we follow up on Ray's question. We need to ask the same question about the surrogate CRUD. How does it get qualified?

MR. SPRUNG: Do you want me to try that?

MR. SORENSON: Yes, go ahead. This is his specialty.

MR. SPRUNG: The hard part there is the adherence question. We know the chemical formula or range of formulas of surrogate CRUD. We know what its structure is, and it is an inverse spinel. We know how to synthesize this and deposit it onto a zircalloy surface so we get deposits that look like the scanning electron micrographs of real stuff.

What we don't know about the real stuff is actually, how hard is it in some measured way to scrap it off or make it come off a surface? So we have to assume that, if we make something with the right chemical composition and make it form deposits on the surface of zircalloy that has the right morphology, that it will have something like the right adherence.

DR. GARRICK: Where are you going to get your data for the CRUD synthesis exercise? In other words, that's very much dependent upon burnup and a lot of other things.

DR. LEVENSON: The water chemistry.

DR. GARRICK: Yes.

MR. SPRUNG: There were a number of reports that gave the characteristics of CRUD on real fuel rods that were looked at. So we have -- it is all over the map, of course, the surface coverages, but in order of magnitude the variations in the chemical composition, the average is about a nickel point six iron, 2.404 is sort of the average composition of PWR CRUD, and we have figured out how to synthesize that and make it look like the real surface deposits. Whether that is an exact surrogate is problematic.

DR. GARRICK: So you've got a problem of the precursor information being all over the map and then, after the impact tests, the distribution is going to be all the map.

MR. SPRUNG: But, remember, what we are dealing with is trying to answer, does 100 percent come off, 10 percent, or 1 percent?

VICE CHAIRMAN WYMER:: This is all PWR, no BWR?

MR. SPRUNG: Yes, the BWR is usually softer and not quite as -- sometimes not as radioactive because there's not so much nickel in it.

DR. GARRICK: Let me ask a couple of questions about the data. You said, in going from the Modal Study to 6672, you got better estimates for a number of reasons, among which was newer and better data, and you noted, Ken, that you especially got better data in the area of routes. A couple of questions there.

What did 6672 tell you about the sensitivity of risk to different routes, LNT notwithstanding?

MR. SORENSON: The way we looked at the routes was we looked at about 400 total routes between truck and rail and broke those into rural, suburban, and urban segments, and out of that, came up with the representative routes that we used for the risk assessment.

I think what we can say about that is there is not a lot of sensitivity with respect to the final risk estimate. There may be higher accident rates along one specific route, but in terms of the effect of, if you have an accident and release a source term, there's not much sensitivity in the actual route that anybody could select.

I mean, a lot of people raise their hand and say, "Well, what about going down, you know, Highway 287? You're going over a bridge and a chasm, and it goes off the edge and rolls down the hill?" Well, it may be a relatively dangerous route, but from the standpoint of if you have an accident and you have release of material from the cask, it is still a pretty remote possibility that will happen. So you really don't have much effect on the --

DR. GARRICK: So you think you have substantial evidence that it is pretty much routeinsensitive?

MR. SORENSON: Yes.

DR. GARRICK: Route-independent?

VICE CHAIRMAN WYMER:: As far as accidents are concerned, not as fa as radiation is concerned.

MR. SORENSON: Well, as far as -- now you may have higher accident rates on specific routes, but --

DR. GARRICK: No, I think it is the other way around.

MR. SORENSON: Yes.

VICE CHAIRMAN WYMER:: No, it's not.

MR. SORENSON: But what I am trying to say is that, if you do have an accident, it is a higher accident rate route and you have an accident, chances are you are not going to have a release. As I mentioned earlier, the Modal Study said that, looking at all the accidents that have occurred in what we have records in the database, 99.4 percent of them are captured by the regulatory environment, which says that the cask will maintain its integrity.

VICE CHAIRMAN WYMER:: The does to people is much more likely to be from passing by it than it is from releases.

MR. SORENSON: That's right.

VICE CHAIRMAN WYMER:: That would be route-dependent.

MR. SORENSON: That is correct.

DR. GARRICK: That's the chronic dose, yes.

DR. LEVENSON: Was there any measurable or any significant difference between truck and rail, aggregating all of the routes in two different categories?

MR. SORENSON: Jerry did that work; I'll let him respond to that.

MR. SPRUNG: Let me go back, first, to your question. We looked at actually about almost 800 real routes, point-to-point routes, and aggregated the properties of those routes into distributions, and then sampled the distributions to run 200 representative routes. So that the results of the risk calculations give you 200 complementary cumulative distributions. If you've ever seen what Sandia calls a "horsetail plot," if you plot them all, you get a black band.

Now if you look at the X axis down here, the risks are ranging over eight orders of magnitude. I mean, excuse me, the doses, population doses are ranging over eight orders of magnitude, and the risks are going over something like twelve orders of magnitude. The band is about an order of magnitude thick from bottom to top.

Okay, so that in a sense, I mean, if you say an order of magnitude sounds big, compared to how the variation to everything else, the route is not having a big effect.

DR. GARRICK: That's right.

MR. SPRUNG: Now can I have your question again, Milt?

DR. LEVENSON: Yes. If you split that aggregation and aggregated truck versus rail, would the horsetails fall on top of each other or would there be significant displacement?

MR. SPRUNG: My recollection is that the Y intercepts of the CCDF and the band started about the same place, but, of course, the rail cask has much more fuel in it. Therefore, it comes down further off to the right in the plot. Of course, there's far fewer shipments because there's more stuff per shipment, and I don't right off the top of my head know how those two tradeoffs --

DR. LEVENSON: You didn't specifically look at that?

MR. SPRUNG: No. No.

DR. GARRICK: I wanted to ask another data question because I was always struck by the absence in these documents of much reference to the crash tests of 1975 through 1977, or whatever it was. What can you say about the use of the crash test in, say, 6672 or even the modal analysis, the crash test data?

MR. SORENSON: We can use those I think in a qualitative sense. We didn't do a lot of instrumentation on those tests. We had some basic accelerometer data, photometrics, and those sorts of things, but the main purpose of those tests was to get some global behavior of the cask, to help benchmark some early code work that was being done back in the late seventies, and also to look for just gross behavior of the cask, and particularly if there is going to be any gross failure of the cask.

So from that standpoint, how that relates to the Modal Study and 6672, I think we, as engineers, had a good sense that these packages would perform in a robust and sound way, but we weren't able to really take data that we got from those tests and benchmark them to the analysis that we did in 6672 for those early rail tests and those sorts of things.

Jerry, would you agree with that?

MR. SPRUNG: This is a personal opinion.

I think those tests provide a visual demonstration that the cask is hard and the things that are likely to strike it are soft, and if you remember the rail locomotive test, the locomotive just deforms around the cask. Similarly, with the test, I think it was BNFL ran, where they slammed a train into a cask.

If you were trying to use those as a technical basis for a pre-test prediction, we didn't do it back then, and after the fact there wasn't instrumentation available to try it after the fact. So my sense was they generally gave us a sense that real accident environments that are within the credible range, not often these tiny tails that we address in a risk assessment, suggest that the casks are going to survive pretty much unscathed.

DR. LEVENSON: I guess, Ken, I've got to ask you one of your "stop beating your wife"-type questions.

(Laughter.)

You made the comment that in the area of fuel you have very little data and, therefore, you need the tests, but 6672 uses test data. I'm not vouching for how good that data is. I personally think it probably came from studies of reactor accidents. It may not be directly relevant.

But why do you exclude that data? I mean, do you think, like I do, that maybe it isn't the best data?

MR. SORENSON: Well, no, I wouldn't say we exclude it, but we're adding onto it. I think that is the Lorenz data, is that right, Jerry?

MR. SPRUNG: Yes. The --

DR. LEVENSON: I was talking about your comment that there's essentially no data on fuel.

MR. SORENSON: I should say little data.

MR. SPRUNG: There is a lot of data on how brittle materials fracture, and there's a lot of models on how a particle bed filters. None of it, though, applies to spent fuel or to depleted uranium dioxide.

So the question of whether it is obvious that the particles that are present, the fuel fines that are normally in the rod, and the additional ones produced by impact or maybe thermal loads, whether they will form particle beds that will filter, it seemed to us it would be wise to do some tests that showed that the standard and traditional aerosol mechanics that everyone believes actually is applicable to spent fuel rods with spent fuel pellets in them, subject to severe impact loads. We expect, of course, to find that this quite true.

DR. LEVENSON: I understand that, but isn't the material referenced in 6672 based on experimental work at Oak Ridge with real fuel?

MR. SPRUNG: The Oak Ridge Lorenz experiments are all thermal. There is no impact fracturing. Therefore, it is not clear to what degree the release is inhibited by particle bed formation in filtering. So that we wanted to do something that would produce the amounts of particles you would see under a severe accident and then see whether they did do as much filtering as we claimed it did in 6672.

VICE CHAIRMAN WYMER:: Most of those were through pinholes that they deliberately drilled into the cladding, and many of them --

MR. SPRUNG: Or burst rupture under thermal load.

VICE CHAIRMAN WYMER:: And many of them were in steam environments as well.

DR. GARRICK: Have you taken whatever data you can find on particle fines and particle distribution and performed a parametric analysis of what this means in terms of dose calculations? In other words, how sensitive is a dose calculation going to be to these kinds of changes that are going to affect the source term?

MR. SPRUNG: In 6672, using traditional aerosol physics, we assumed that 99 percent of the particles at 10 microns or just below would be filtered out by passage through the particle beds. So that is a one hundred-fold reduction in the source term. That is significant.

Whether the change in the size distribution of the released particles below 10 microns, you know, the respirable range, would have a significant effect we did look at.

DR. GARRICK: Yes. See, what I am getting at is, so what? If you do get a considerable distribution of fines and particle sizes, what does that really mean in terms of what we are concerned about; namely, the doses?

MR. SPRUNG: The thing we are trying to confirm is that hundred-fold reduction that we assumed, based on bed filtration. We believe that is real, but is it really a hundred-fold? Is it tenfold? Is it a thousand-fold? Without some real data for a spent fuel rod with its shrunken gap and crack network, without knowing something about maybe how real pellets fracture for the real pellet tests, we are really trying to confirm that traditional aerosol physics gets us well in the ball park for the reductions in what gets out of the rod due to the bed formation and filtering, not trying to confirm with great precision the size distribution.

DR. GARRICK: Yes, and I think it is pretty clear, the more I read and study this, that what you are talking about here is not going to contribute much to a better calculation of the risk; that you are going to learn something about the thresholds at which these things fail, and at which you might get a rupture of the cask, and at which you might get some redistribution of material within the fuel elements, but as far as a risk calculation, a transportation risk, I am not very optimistic about this program helping you very much.

MR. SPRUNG: Let me try one more. I think we don't know for sure very cleanly. We have a computational result for the strains at which the rods would fail with small tears. If, for example, we would discover that, due to rod flexing, you know, the ability to bend, that the failure speed for tears and cracks was substantially higher than we assumed, we might actually decrease things quite substantially based on what we would learn from the impact test.

I mean, at the moment right now we really don't know, other than by computation and by judgment, again, the speed at which a rod might fail and what the failure might look like.

DR. GARRICK: Yes, but my point is very simple: that the risk is not dependent upon the kind of events you are testing. That is my point. Because those events are going to be so rare and so much in the 10 to the minus 11, 10 to the minus 12, 10 to the minus 10 category, that they are not even going to be a visible contributor to the risk.

So, as we discussed in Sandia, from the science standpoint you are going to learn something, but from the risk of transportation I don't think you're going to learn much of anything. I suspect that if we did a real comprehensive risk assessment with uncertainties, that you would barely see anything in the distribution curves, if anything. That is my suspicion because you're outside the envelope. You're outside of the risk domain.

MR. SORENSON: Except that one point to add to that, though, is you may get fuel failure at lower speeds. Now you won't necessarily get closure opening and source term release, but --

DR. LEVENSON: But fuel failure does not generate any risk or --

MR. SORENSON: I agree.

DR. LEVENSON: You have to get canister failure, which you're completely ignoring, and you have to get --

MR. SORENSON: If the containment is sound, you won't --

DR. LEVENSON: You have to have an expelling mechanism to distribute it and, unlike reactor accidents, there's not much in the way of stored energy inside a cask for a dispersal mechanism. So the release from the fuel pin per se would not lead to dose.

MR. SORENSON: I agree. It has to have a way to get out.

DR. LEVENSON: Well, the canister, out through the cask, and there has to be a mechanism to disperse it. A hole isn't enough.

CHAIRMAN HORNBERGER: Another way to look at it is, suppose your filtration factor were 10 instead of 100, would it affect your risk? Our suspicion is no. So doing the experiments to learn whether it's 10, 100, or 1,000 doesn't make any difference to risk.

MR. SPRUNG: If I go back to 6672, increase all my source terms by a factor of 10, then my risks will all go up by a factor of 10. If I add a canister in there, then I suspect my risks go to zero because I suspect -- you know, we didn't have a canister in that study, and we don't see a very good way to fail a canister. That's one of the reasons, of course, that the NRC thinks we should be looking at canisterized casks in the Package Performance Study.

CHAIRMAN HORNBERGER: Right.

MR. SPRUNG: Whether it's obvious in advance of doing any testing that you can't fail a canister --

CHAIRMAN HORNBERGER: But suppose your risk does go up by -- suppose it is linear. Suppose it goes up by a factor of 10. Does that tell you that it is unsafe? I mean, you went down, what, two orders of magnitude or three orders of magnitude from your Modal Study, but the Modal Study didn't suggest that it was unsafe. Now you're saying, "Oh, now we'll go back up by a factor," and who cares?

MR. SPRUNG: I think the question of whether you try to show it is that your best current ability to analyze shows that it is still lower, whether that is worth doing is a choice I think NRC has to make, not the technical person.

CHAIRMAN HORNBERGER: But it's not based

on risk.

DR. GARRICK: Yes. See, it's another example of where the risk-informed perspective is not dominating the decisionmaking process.

DR. LEVENSON: Okay, Ray, questions?

VICE CHAIRMAN WYMER:: I wouldn't touch any of this.

(Laughter.)

DR. LEVENSON: John?

DR. GARRICK: No, I think I'm finished.

DR. LEVENSON: John?

DR. GARRICK: No, fine.

DR. LEVENSON: Okay. Well, I want to thank both you guys for coming, and getting from Albuquerque to here is no easier than getting from here to Albuquerque. We've done that.

MR. SORENSON: You did it last week.

(Laughter.)

DR. LEVENSON: So thank you very much.

MR. SORENSON: Thank you for your attention.

MR. SPRUNG: Yes.

DR. LEVENSON: I turn this back over to you, George.

CHAIRMAN HORNBERGER: Yes, my thanks as

well. That was very good.

MR. SORENSON: Thank you.

CHAIRMAN HORNBERGER: Excellent.

MR. MAYFIELD: Mr. Chairman, if I might?

CHAIRMAN HORNBERGER: Oh, I'm sorry.

MR. MAYFIELD: I'm the Director of the Division of Engineering Technology and Research, and the Package Performance Study is being managed out of my Division. There were a couple of points we wanted to make sure didn't get lost with the Committee or in the record.

We wanted to re-stress the point that Ken had made that the protocols are going to be published for public comment. They are going to be out late this month, and will be out through September. It is a 90-day public comment period.

We are looking specifically for input on things like choice of cask to be tested, impact speeds, the fire test parameters. We are then going to hold some public meetings in both Nevada and here to seek public comment on this.

As Ken pointed out, the test protocols are an initial proposal. We are keenly interested in stakeholder input on the nature of the tests and the specifics and the protocols. Once we get that public comment, then we will finalize the test plan and move forward, but we did want to make sure that everyone understood that this is an initial proposal that is going to be put out for public comment.

DR. GARRICK: Yes, one of the concerns that the Committee has, I know, about this is that we don't want to find ourselves engaged in a program that results in a high likelihood of a ratcheting phenomena. In other words, it would be unfortunate if out of this came a requirement for changing the regulations, increasing the tests, and doing something that made transportation risk and outlier from a risk-informed regulatory point of view from other activities that you regulate.

MR. MAYFIELD: I understand.

DR. GARRICK: And you know, when you're dealing with the public and you talk enough about something, there's a tendency to think that that something should be the basis for the rules. I think we have to be very careful about that. I think that if we are going to do that, it has to be appropriately characterized. That's why we made the distinction in our discussion between something for the purpose of better understanding the risk and something for the purpose of better understanding the science.

MR. MAYFIELD: I think that is an important point, and it was the second thing I wanted to emphasize. There was a lot of interesting dialog about the fuel piece of this and the risk considerations. The test we have asked Sandia, the primary focus for the structural interest, the structural test goes to being able to provide some validation for the computer codes that are used in analyzing the casks and to take that beyond the linear elastic regime, where I think everyone is convinced the computer codes work just fine.

There are a lot of us convinced the computer codes will work just fine beyond that, but the fact is we don't have the large-scale demonstration tests using modern-sized casks to demonstrate that fact. That was the driving interest in going into these tests, as opposed to evaluating any specific cask design or any particular beyond-design-basis, or they call it extra-regulatory conditions. It is to get enough velocity, enough energy into the cask so that you do, in fact, take it beyond the linear elastic regime.

DR. LEVENSON: The question I have is, if in the real world no accident, no case is going to take it there, why do we need to increase our understanding of it?

MR. MAYFIELD: Well, I think that Ken pointed out you begin to be out in the tails of the distributions, and I'm not sure you can say with absolute certainty that no accident will take you beyond those --

DR. LEVENSON: No, but we have a basic philosophy that says, somewhere out on the tail there's a cutoff and we'll quit worrying about it.

MR. MAYFIELD: And the issue is to evaluate the computer codes, to make sure that as we're evaluating other designs and other conditions, that those computer codes have an experimental basis, that we can demonstrate our ability to do those calculations.

DR. LEVENSON: Why don't we do a cask test at 250 miles an hour?

CHAIRMAN HORNBERGER: It's too far out on the tail.

(Laughter.)

MR. MAYFIELD: It's too far out on the tail.

DR. LEVENSON: No, but that's my whole point: How far out on the tail? It seems to me that --

CHAIRMAN HORNBERGER: I knew that was your point.

DR. LEVENSON: -- this is kind of an arbitrary --

CHAIRMAN HORNBERGER: I knew that was your point. You made your point very well.

(Laughter.)

MR. MAYFIELD: Well, again, I think that's part of your point, is why we're seeking public comment on the test protocols and stakeholder input, and, obviously, input from this Committee will be of interest to us.

MR. SORENSON: If I may interject real briefly, one example, Milt, to answer your question, is there's been some discussion about looking at accidents that do not involve the limiters, a back-breaker accident, if you will, where the cask is impacted in the middle of the cask, and a non-limiters example is a bridge abutment. If we can go to the point where we do this test and we can demonstrate that we can capture the response in the elastic-plastic regime for one case, we can say, you know, we can analyze that and we're confident that we can get the response of that cask analytically; we don't have to do a test for every scenario that you can think of that might cause plastic deformation of the cask body.

DR. LEVENSON: But I guess, with the matter of how sensitive is this as a public issue, it seems to me that you could do the same thing by making a test vehicle that would be substantially cheaper than a commercial cask and one that you can design specifically to maximize the data you're going to get to validate a code. My guess is that an actual cask is not your first choice for a test vehicle, if primarily what you want to do is validate the code.

MR. MAYFIELD: If I could, coming from a research and large-scale experimental background, I can assure you I could design a test vehicle that would answer the question. However, the public interest is not being addressed by that kind of test vehicle, and that becomes a very important consideration.

DR. LEVENSON: But the public issue is answered by testing up to maximum probable conditions. When you go way beyond, that is not answering the public question.

MR. MAYFIELD: Well, I think the characterization of "way beyond" is what we're looking for some feedback on, and I think there is a range of views about how far is too far.

DR. LEVENSON: Well, you know, some of the tests, preliminary test protocols for fuel go up to 150 miles per hour, and that's pretty far beyond.

MR. MAYFIELD: Again, that is why we are seeking some stakeholder input.

(Laughter.)

CHAIRMAN HORNBERGER: I said, tongue-in-cheek, to my colleagues, I guess over lunch, that I really am glad that you folks weren't in charge of designing the Verrazano-Narrows Bridge.

(Laughter.)

Any other comments?

(No response.)

Okay, we're going to break here. We'll go off. We don't need to be on the record any longer. Let's take a 10-minute break and reconvene. We'll talk about some letters.

(Whereupon, the foregoing matter went off the record at 5:20 p.m.)