UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING MEETING

May 14, 2003

The Watergate Hotel
2650 Virginia Avenue, N.W.
Washington, DC 20037

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen
Dr. Thure Cerling
Dr. Norman Christensen
Dr. Michael Corradini, Chairman, NWTRB
Dr. Paul P. Craig
Dr. David Duquette
Dr. Ronald Latanision, Session Chair
Dr. Priscilla P. Nelson, Session Chair
Dr. Richard R. Parizek

SENIOR PROFESSIONAL STAFF

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

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Linda Hiatt, Management Analyst
Linda Coultry, Management Assistant
Davonya Barnes, Staff Assistant

CONSULTANT

Dr. William Melson

I N D E X

PAGE NO.

P R O C E E D I N G S

8:00 a.m.

LATANISION: I'd like to welcome you all back to this second day and final day of our Nuclear Waste Technical Review Board Spring Meeting. I'm Ron Latanision, and I'm going to chair the first technical session this morning.

I want to point out to you that three of the Board members, Chairman Corradini, Dan Bullen, and Mark Abkowitz are off site at the moment tending to some other Board business. They will rejoin us a bit later in the morning, hopefully around 10 o'clock. This is something we don't often do to have Board members off the site during a Board meeting, but they do have some important business, and they will rejoin us shortly.

In this session, Mark Peters from Los Alamos is going to lead off, and he will present the status of ongoing testing in the project. Bob Budnitz will speak next. Bob is from Lawrence Livermore National Lab. He's a scientist who is on leave, and as you know, an advisor to Margaret Chu on the Science and Technology Program. He will give an update of the S&T Program's activity and plans.

Each presentation will be followed by, as usual, a brief question and answer period, and after Bob's presentation, we'll have a break and we'll then return for the final session of the morning at about 10:15.

Likewise, later this morning, there will be some discussion of the igneous consequences study. One of the consultants who is active in assisting the Board in this matter, Bill Melson from the Smithsonian Institution, is here, in fact, and Bill is sitting right behind the Board table. So, we're happy to have Bill with us, and I'm sure he will contribute to the discussion.

And, finally, as a reminder, there will be a public comment period at the end of the day, as always. Anyone who is interested in speaking should register with Linda Coultry, who is out in the lobby area, or Linda Hiatt, both out in the lobby area. As always, you're welcome to submit your comments in writing for the record, and of course if you have questions that you'd like to have the Board ask presenters directly, please give those questions in writing to Linda or Linda, both Lindas, out in the lobby.

Mark, we're ready, and I welcome you back to the second day of our meeting. Thank you.

PETERS: Thanks for having me back. I hope people aren't getting too tired of seeing my happy face up here. I slept right behind there, actually. (Laughter.) I look tired. I've had plenty of coffee, though, so I'm in good shape.

I'm up here today to talk about the status of the ongoing testing program. I want to make a couple points up front to put this into context of my presentation and Bob's presentation, also some of the discussion yesterday about the ongoing science program. I'm going to be talking about the status of ongoing work on the project. This is work, some of which will transition over to the Performance Confirmation Program, other work that will continue just as ongoing model validation. But one of the things I'd like you to take away is there is an ongoing testing program on the project.

In addition, Bob will then talk about the S&T program, and what we've started in '03 and what we plan to do in the out years. So, my point is is my take on this is that, you know, we still do have a science program on this project, focused on the near term and also the further out looking S&T Program. So, hopefully, through the course of the two presentations, we can talk about what's ongoing and what we have planned.

I'm up here talking again about a lot of other people's work, and I will try as I go through to sprinkle those names and organizations in. I'm sometimes successful, sometimes not. But, I want to say up front this is work that's been done on project by primarily the Performance Assessment Organization, BSC, U.S. Geological Survey, Livermore, Los Alamos, Berkeley, Sandia. I'm not going to be talking about much in the way of work that's gone on at Argonne and PNNL today, but they also have a role on the project.

So, with no further ado, this is structured in a similar way to what you all have seen from me before in terms of these ongoing status reports.

Again, I just want to provide a status on the data collection and testing program as we move forward to the update, updating the models and the design for the license application. As I walk through, I'm actually mapping the front part to the flow that we followed through yesterday. I'm going to start with a little bit of discussion of the drift scale test, here focused on the measurements and the status of the field. Bo talked a lot about the validation aspects.

I'm going to touch on pore water geochemistry again, although I hesitate to do that too much based on how much we talked about it yesterday. This is more the basic data that the Survey is collecting in that area.

I'll talk about the field and laboratory investigations in thermal-mechanical properties, a little bit more on the data on dust investigations that was the basis for a lot of what we talked about yesterday when we were talking about dust and deliquescence, et cetera, dust-leachate interactions, and metal degradation investigations.

Here, I have some very nice slides on some work that's being done looking at laser peening to look at how one can mitigate stress corrosion cracking in the weld zones.

I've also got updates on saturated zone, a couple slides on what the site-scale model is looking like in terms of the update for the license application, some data collected by the USGS in the area of litho and hydrochemistry, an update on the Chlorine 36 validation project, also a series of slides on our work at Pena Blanca, and then, finally, a few slides on the igneous consequences studies. I don't want to steal the thunder of Dr. Rubin, who's going to be talking later in the morning. So, there's only a few slides here, and that probably will generate quite a bit of discussion either here or during Dr. Rubin's presentation. And, finally, wrap up.

So, the first part kind of walks through the way we walked through the near field environment yesterday. The second part is more of a miscellaneous set of things that the Board asked to hear about in terms of an update.

So, starting with just a reminder of our underground test facility at Yucca Mountain, the exploratory studies facility, the cross-drift, the various niches and alcoves where we've done our testing in the past, and continue to do testing in some of the areas. The proposed repository block would be in this region here, so north is in this direction. You've got the Solitario Canyon Fault bounding it on the west, and there here the Ghost Dance to the east of the ESF.

A more detailed diagram, I don't want to dwell on this. You all have seen this before. But this is a more detailed layout of the cross-drift as it's been mined in the underground. Again, the Solitario Canyon Fault north is in this direction here, moving to my left. What's also shown on here is the contacts between the different sub-units of the Topopah Spring, the upper lithophysal, the middle non-lithophysal, the lower lithophysal, as exposed in the cross-drift, which makes up about 70, 75 per cent of the proposed repository horizon, and then finally the lower non-lithophysal before you get to the Solitario Canyon Fault.

The alcoves that are shown in blue Italicized with approximate station numbers are those that are currently planned, but not yet constructed. And ongoing testing, or testing that's been complete has been done in the areas that are shown in the regular type.

First, the drift scale test. Bo talked about this test quite extensively yesterday. We were heating the rock with nine canister heaters that were inside the heated drift, as well as 50 wing heaters, rod heaters that are in boreholes in the rock, 25 on each side of the drift. This test heated for a little over four years, and we turned off the heaters in January of '02. So, we're close to a year and a half into the cooling phase at this point. It's a natural cooling phase, meaning that we had the heaters running up until January of '02, flipped the switch and are watching the rocks cool at this point.

Just some representative data. This happens to be temperature data, temperature in celsius versus distance from the centerline of the heated drift. What this is is this is two boreholes that are drilled out to the side of the heated drift, parallel to the wing heaters, and it's just showing how the rock is cooling, what stage we're at. This is at the beginning of the cooling phase, up to near present. Actually, the drift wall, if you go out there today, the drift wall is just below the boiling point. But, this shows the double hump profile is due to the fact that the rod, the wing heaters, the rod heaters actually had two separate heater segments, the outer heater segment being at a higher power than the lower, than the inner heater segment. It just shows the progress of the cooling phase in these two boreholes. These are located about halfway down the heated drift.

Also, at the last meeting, we discussed the presence of at the time were called red spots. We had found deposits on some of the canisters, as well as on the floor in the heated drift when we ran our camera in and out. They were noticed last August. They were first noticed on Canister 7. So, what this is is this is a plan view looking from above of the heated drift, the nine heater canisters. Again, it's about a 50 meter long heated drift. You've got the bulkhead here. Here's the back end. We're supporting it with rock bolts and mesh, and recall we also have this case in place concrete liner at the back end where we're looking at testing variety of ground supports. This is back to the A times when we were actually designing this test.

But, this just shows where we've noticed these deposits on the canisters in the floor. We've since gone in and done some collection of those, and how we did that, we didn't send a person inside, we actually put together a sampling assembly on the camera itself, and went in and took scape samples of those deposits, the XRD analyses, and those are, in fact, iron oxide deposits in the heated drift.

Based on the camera runs that we do periodically, they were deposited between April and August of last year, shortly after we turned off the heaters, and the current hypothesis is that it's believed to originate from the Swellex bolts that are in place in the crown down the spring line throughout the heated drift.

We will be able to characterize these further once the cooling phase is over and we go back in for post-test characterization. We have not developed the detailed plans for that, but one of the things that we may very well do is also over core some of those rock bolts to look at rock bolt, grout rock interaction, also how the rock bolts are involved as you heat it. We did that in the single heater test, so I would expect we would at least entertain that here in the drift scale test.

I won't dwell on these bullets. This just reiterates really what Bo said yesterday, how important the drift scale test has been to our model, building confidence in our coupled process models through the blind predictions, comparison to measurements, and iterative process that we've gone through throughout the test.

Moving now to geochemistry of pore water, again, this is work that's gone on, Zell Peterman and his folks at the Survey in Denver. A couple key points of why we're worried about pore water, and I don't think we need to dwell on that. We spent quite a bit of time on that yesterday. It's important in terms of how the water evolves. It could eventually enter the drift. It's important in terms of what kind of salts it may leave behind as it evaporates in the rock, and how that might impact dust load and also chemistry during the cooling phase. And, of course, it's a key starting point for understanding how the water evolves through the system.

How do we get the water out? From the welded tuffs, the Survey uses ultracentrifuge, spins the samples and extracts the water that way, and then does chemical analyses, isotopic analyses in the laboratory.

I showed in the last meeting some of the chemistry data. There's a plot in the backup that I used in January. I went ahead and put that in the backup for this meeting.

One of the things that I did want to point out is this is work that Brian Marshall at the Survey out of Zell's group, where they've extracted the pore water. Recall, they had done quite a bit of work on leaching rock, and then measuring the pore salts for isotopic compositions, and one of the isotope systems that they looked at is strontium. So, what's shown here is the strontium 87/86 ratio of the pore water salts from work that had been done in the past versus the measurements of the actual pore water that they extracted. I find this actually very encouraging. You're getting very similar numbers for the pore water salts versus the extracted pore water itself. That's I think a very positive observation.

You can take the strontium isotope compositions and using one dimensional advection diffusion reaction models, the literature Johnson and DePaulo and others have presented models like that in literature, you can look at and get interesting constraints on the fluxes that go through these systems. Now, they're one dimensional calculations, but just the same, when you go through and look at the strontium isotope ratios in the pore water versus what you have in the non-welded tuff above, and do these one dimensional calculations, you get reactions that require low velocities of a few centimeters per year. That's on the order of what we would expect for the kind of fluxes that we had for percolation flux that comes out of the calibrated UZ flow model. Let's call that a multiple line of evidence.

Thermal properties investigations. Recall, over the past year and a half to two years, we really went out and spent a lot of time in the field in the cross-drift in particular in the lower lithophysal unit collecting various sized samples, also doing field tests to look at thermal properties, thermal conductivity, as well as thermal-mechanical properties.

I've talked a lot about the thermal conductivity aspects of this in past meetings, so I'm not going to spend a lot of time on that. There are some slides in the backup which are somewhat repetitive from previous meetings, but just to remind you all where we're at. This is just a very brief reminder that we did three field tests in the lower lithophysal unit. These were all in the cross-drift where we heated the rock, different configurations for the three tests, but we heated the rock and measured temperature profiles as a function of time, and through that, were able to get constraints on the thermal conductivity of the lower lithophysal.

Of course, the lithophysae themselves are important considerations, the porosity of the rock is an important consideration when you look at thermal and mechanical properties. This has allowed us to get a good handle on the scaling of those properties, and also when you compare the field to the lab, it puts together I think quite a nice story as to what the effect of the porosity is on these properties.

This is a compilation of a lot of the data that we've collected. It's thermal conductivity in watts per meter K versus porosity. This is primarily showing the laboratory data. This particular dataset is at 70 degrees celsius. You're seeing both wet and dry values, meaning saturated and then heated samples.

Also plotted on here is previously quoted values from work that had been done at Sandia and other places earlier in the Nineties. Also plotted on here are the field results. What we mean here by arbitrary porosity, recall the field tests that we've done out there, David Bush and co-workers have gone in and looked at the lithophysal porosity in detail to test. So, arbitrary might not have been the best choice of words. The field results span a range. You could actually plot those up as a function of porosity and they make a lot of sense in relation.

What I'm trying to show here is that the field results in fact overlap with the observations that we're making in the laboratory.

Bringing the mechanical piece in, in the backup, there's a reminder that we've done three field tests in both the non-lithophysal and lithophysal rocks, in addition to previous plate loading tests in Alcove 5. Those were an important part of this program. We've also done a series, a lot of coring in the underground, and taken fairly large scale samples back to the laboratory for measurements of a variety of different mechanical properties at both ambient and elevated temperatures. And those lab measurements continue. The in situ field tests are now complete.

These are some examples of some of the work that continues at Sandia National Laboratory. Showing here is two sets of data, ultimate strength and log scale in megapascals versus volume of the sample in cubic meters. These are both log scales. Showing 1986 data, which is data on middle non-lithophysal samples, and 2003 data. This data happens to be on lower non-lithophysal samples. They're both non-lithophysal samples, different sub-units of the Topopah, just to give you an example of the kind of data that we're collecting in terms of strength versus volume of sample. The relationships make sense, what you'd expect when you go and do these kinds of experiments.

Time dependent mechanical behavior. We're also looking at the effect on strength as a function of strain rate, and also doing some static fatigue, doing some fatigue type experiments. There's also experiments being planned to gather the appropriate data so that we can look at the stress versus time to failure data.

The next plot shows some of the data on rate dependent strength. This is ultimate strength versus strain rate. Here, this is a linear scale versus log of strain rate, in inverse seconds, shows the data, all the data points, and then the mean is in the brighter red circles, shows an overturn as you go to higher strain rates due to poor pressure effects.

This particular one I believe is in non-lithophysal, but I'll have to get that confirmed for you. It's in non-lithophysal, I'm almost positive. It's got similar strength.

Now, let's move to dust. We talked through this in some great detail yesterday. Why is it important? It's important to the near field environment inside the drift. USGS has done quite a bit of work on sampling dust in the tunnel, and Zell helped me out quite a bit yesterday in talking through what we've done in that part of the program.

The sources of dust, construction activities, rock dust, dust brought in, anthropogenic dust, dust brought in during the work, and then of course dust that could be brought in from the outside atmosphere through the ventilation system.

Again, some of what we talked about yesterday. There is organic carbon present. Zell also pointed out they've done some microbial analyses and there is penicillin and other things like that growing in the dust. They do soluble analyses of the water soluble anions and cations. There's a plot in your backup that I also showed in January that shows some of the compositional data.

Chloride and bromide, and I want to show a little bit more about chloride and bromide and how it tells us about the mix of salts in terms of what the influence of use of construction water in the underground and how that's influencing the dust composition. I thought that was pretty interesting data.

LATANISION: Latanision, Board.

Could we go back? You mean you found elemental iron? What is this?

PETERS: Zell? I'd have to ask Zell to clarify that one, Ron. He's walking up. Do you want to handle it now? Let's just go ahead and handle it now.

PETERMAN: Zell Peterman, USGS.

What we see is a pretty substantial increase in ferrous iron. And, so, this is an interpretation. There's no other place to get ferrous iron in the underground except metallic iron, and of course in the analyses, the metallic iron comes out as ferrous iron, just by the technique that's used. It's the titration method. So, this is my interpretation that there has to be iron particulates, which to me isn't surprising. I mean, there's a lot of metallic iron in the tunnel, the trains running back and forth all the time, steel wheels, steel rims. I just can't think of any place else to get this increase in so-called ferrous iron in the analyses other than metallic iron.

LATANISION: Just again a point of clarification. You're finding, however, iron in some form other than zero valence; right?

PETERS: Right. I think what he's saying is is it's occurring as ferrous iron, and he's hypothesizing that it's coming from--

LATANISION: No, I understand. Don't you think that's a little bit misleading?

PETERS: Fair enough. Fair enough. Good comment.

LATANISION: Okay. Thank you.

PETERS: I lost my backup there on the phone call.

Here's some of the analysis of some of the dust, and I mentioned the chloride/bromide ratios. This is a standard way of looking at mixing in a geochemical system. You plot ratio versus the concentration of the denominator, and this is a chloride/bromide ratio of the dust versus the bromide concentration.

On these kinds of diagrams, in simple mixing, you'd expect a curve just like you see here, a binary mixing curve. It shows the dust. The dust compositions actually plot very nicely along a binary mixing code, suggesting that the salts, you're basically getting a mixture of native pore water salts and salts derived from evaporation of construction water, and that's explained in the chloride/bromide ratio.

Chloride to nitrate, we've talked a lot about that yesterday. Here's the dust analyses at different mesh sizes, and also the pore water analyses that were talked about extensively yesterday, nitrate versus chloride. This particular line happens to be a nitrate to chloride ratio of .2. We focused a lot on nitrate/chloride ratio of .1 yesterday. That line, of course would draw just about right here. But this, I think, brings home the point that the nitrate concentrations in this dust is actually quite high.

A lot of what I've already said, so I don't need to dwell on this. The importance of nitrate, that was I think self-evident through yesterday's discussions.

Moving now to material degradation investigations. Joe talked extensively yesterday about localized corrosion, so I'm not going to go into that. I want to talk a little bit about some work that's being done at Livermore for YMP in cooperation with UC Davis, as well as a private company in the Livermore area, looking at laser peening.

Recall that we, of course, have to weld our packages. When you weld, you put the area into a tensile stress field, and that makes it susceptible to stress corrosion cracking. So, you can look at various ways of mitigating that stress and putting at least the near surface of the weld into a compressive stress field, and that mitigates the--that helps us with any possible deleterious effects from stress corrosion cracking.

Through the design evolution process, we've actually, our stress mitigation techniques as we move forward to LA are going to focus on laser peening as of right now. But I want to talk a little bit about some of the experimental data that we've collected on metals, and how well laser peening appears to be working in terms of mitigating stress corrosion cracking.

Two samples, both 316 stainless steel. This is what I'd call boiling green death, 40 per cent mag chloride at very high temperatures. The one on the left, the welds are shown here. The one on the left has been laser peened. The one on the right has not. I think it's pretty obvious to the eye how laser peening and putting it into a compressive stress state, the near surface of the weld is helped in terms of mitigating cracking of the welded area.

They've also used a contour method to measure the residual stress. So, here's a sample. This is shown, it's a color scale, but it shows the stress field within a coupon that's a little over 3 centimeters thick. The unpeened sample showing the tensile field towards the surface, really throughout the coupon near the weld. After peening, you can see that we've set up a nice compressive stress state near the surface within a pretty significant thickness of the coupon, and that's the sort of phenomena that helps mitigate the cracking of the metal even in very aggressive environments.

A little bit more on the depth of the effect of the peening in terms of putting it into a residual state. The plot on the left is the thickness of a variety of coupons that have been looked at in terms of peening versus how deep the stress becomes compressive after you peen the sample. You can see they're actually getting to fairly significant depths relative to the total thickness of the coupon. I'm not a metallurgist, but I'm told that this is actually quite a break-through in terms of the laser peening technology.

Finally, this is just a different way of looking at it. The residual stress is a function of depth from the surface, showing compressive near the top of the sample, and as you go deeper, transition back into the tensile field.

So, now moving to the more miscellaneous pieces at the back end of the presentation, updates on other pieces of our program on the project. Let's start with the saturated zone. This is a reminder that Nye County's program continues. Also, Inyo County is going to stand up and talk a little later today about their program that's just been started in cooperation with DOE.

One thing I would want to point out is that we have worked very cooperatively with Nye County to collect a lot of very important information that's been used for calibration and validation of our models, our site scale model. And I think there's a similar set of bullets that one could put together for the Inyo program. As they start to collect information, that will be very valuable to all of us for understanding more to the regional scale hydrogeologic framework.

This is a slightly outdated diagram, but it shows the boreholes that were drilled for Phases 1, 2 and 3 by Nye County, and the kinds of data that we've collected. Another diagram showing the location of the Phase 4 drill holes that were just completed this fiscal year.

A little bit about the site-scale model and how it's being updated for license application. The key aspects, calibration aspects of it using parameter optimization techniques, using the water level and head measurements from the project boreholes, as well as the Nye County boreholes.

That results in predicted flow paths. I don't have the flow paths plotted out here, but they are very similar to what you've seen in the SSPA EIS calculations. They start from the proposed repository, move to the south-southeast, and then trend down Fortymile Wash.

We've done a variety of confidence building or validations. Prediction and comparison of Nye County water level data that wasn't used in the calibrations. There's actually a table in the backup that tabulates some predicted versus measured water level data from the Nye County wells that weren't used in the calibration numbers.

We're also comparing permeability data that wasn't used to calibrate the model. And then, finally, looking at hydrochemistry data and temperature data as well as ways of building confidence in the model, and also looking at alternative conceptual models, and include an updated geologic framework from more recent Nye County data, as well as the effect of faults in terms of how they control the flow paths from the repository, the proposed repository.

A couple of specific examples of the on-going data collection in the saturated zone in cooperation with the Nye County program in the lithostratigraphy, hydrostratigraphy area. This is work that Rick Spangler is doing at the Survey in Denver. He's looking at geophysical logs and putting together resistivity models to help bolster our confidence in the lithostratigraphic and hydrostratigraphic variability down gradient of Yucca Mountain, particularly focused on down where the alluvium gets quite thick.

And then you can also use these kinds of resistivity models to help guide locations for potential future drilling, in cooperation with Nye County, or through other options.

The next slide just shows the existing geophysical sounding locations, as well as the locations of the boreholes, just to give a sense for the data coverage that Rick has when he's doing these resistivity models.

This is just an example of the apparent profiling across the wash, the resistivity profiles, as well as lithologic logs, and how he's going about matching the known lithologic logs with the resistivity data, and then putting those together into an overall resistivity model. This is work in progress. He continues to put this model together.

Dave, you also asked for a thickness of alluvium. That's in the backup. There's one in the backup which we may want to discuss at a later time.

Moving to hydrochemistry, I talked last meeting about work that Gary Patterson has been doing looking at the inorganic geochemistry, and this is a real important aspect to the validation of SE site-scale model. In particular, improving our understanding of the variability in the third dimension. But, it's a very valuable validation technique that DOE is using currently.

I'm going to talk today about a dataset that's been collected again at the Survey, looking at Carbon 14 systematics in both dissolved organic carbon and dissolved inorganic carbon in water samples down gradient from Yucca Mountain. There's actually a map in the backup that shows the locations of these samples.

What's being plotted here is percent modern carbon for the inorganic carbon component. This is really total carbon versus the percent modern carbon for the organic carbon. This may be useful to explain this plot a little bit more. What's shown here on the line with the tic marks is actually the I'll call it the isochron, the geochron-- isochron, excuse me. It's the evolution. So, samples that plot up here, if they were concordant, these up here would be 20,000 years old. Remember, the half life of Carbon 14 is 5,700 years. So, we're looking at trying to date groundwater samples using the carbon system to give us an idea of how that fits in with the kinds of travel times that we have in our SC model.

Interesting systematics. Again, the DIC is really a measure of total carbons. So, as you're flowing through the system, you're picking up some amount of dead carbon. So, you'd expect those ages to be greater, the inorganic carbon ages to be greater than the organic carbon ages, and that's consistent with the systematics, with the exception of these two sitting up here that are showing some reverse discordance, and that's under evaluation.

But, the bottom line is the dissolved organic carbon ages were between 8 and 16,000 years. The DIC ages tend to be greater than 12,000 years. But, I'm encouraged by the fact that these kinds of ages are broadly consistent with the kind of flow times that we would expect in our system within the hydrogeologic basin that we're dealing with here.

There's uncertainties in the significance of these ages. If you look at the stabilized values of the DOC component, dissolved organic carbon component, the have very light values. That suggests some contamination of the samples. There's some complicating factors in here. Also, when the Survey does analyses of blind standards, they are slightly outside of tolerance. And there's also complications about local recharge and how that might affect the systematics.

Moving now to Chlorine 36 validation, the Survey and Livermore and Los Alamos have a report being drafted. It's being worked on very actively. A lot of time is being spent. Jim Paces at the Survey is spending a lot of time putting together the meat of this report. There are several drafts that are being passed back and forth between all the parties. That's currently planned to be delivered in late summer in terms of a final report.

In addition, DOE is in receipt of a proposed study to do this independent study that I alluded to in the last meeting. It's still a proposed study. There has not been any funding put to the study. It's being evaluated through the formal merit review process, and at that point, the decisions will be made by management whether to perform the work and whether to devote budget to it. But, it would involve a background investigation.

The particular proposal that we've received really is focusing on wanting to see the report, even just in draft form, so that can help them develop the kinds of experiments that they would do in the independent study. It would include, in all likelihood, new sampling in the underground.

And then, finally, hopefully we can come to a final conclusion about understanding what's going on with Chlorine 36. I emphasize hopefully, at least from my perspective.

Why study Pena Blanca? I'm shifting gears now. We're moving over to Pena Blanca. Everybody is aware I think Pena Blanca is a uranium deposit in Chihuahua Province of Northern Mexico, not too far from El Paso. There's actually a group going down there next week. It's a similar geologic setting. It's a uranium deposit in a welded, non-welded ask flow sequence above the water table in a very similar climate setting to what we have at Yucca Mountain today. It's about a little under 100 meters above the water table, and it's a UO2 deposit, so it's similar to the kind of waste forms that will be introduced into the repository. So, there's a lot of positive aspects about this as Pena Blanca really being a true analog of Yucca Mountain. Talk about analogues, this is about as close as you're going to get.

There's been a lot of work done at Pena Blanca. The Center has been down there doing a lot of work. DOE has done some work. And there's also an ongoing program that DOE is conducting down there that I'll talk about. The studies that have gone on before have been to map the deposit, characterize the mineralogy of the ore body. There's been a lot of work on the uranium series isotopes in the fractures, in particular, and how that constrains transport times, and finally that data has been used as the basis to do some radionuclide migration modeling of the deposit and the altered zone.

This is a plan view map of the ore body itself. It's a topo map, but it's reference to local mine level, if that makes any sense. So, the zero point is referenced to the surface at the zero zero level of the mine. So, this is really a topo map looking at difference in meters. We're not 50 meters above sea level here. We're a little higher than that, is the point.

But, what's shown here in the darker gray is the ore body itself. It's kind of a stock plug type ore body. Also shown in the lighter gray is the altered zone around the ore body. The dark lines that are labelled with letters are actually fracture sets, not faults, but fracture sets that were sampled by the Center, and also DOE, where they did a lot of U-series measurements.

The three red dots are where DOE is in the process of drilling three boreholes at Pena Blanca. I say DOE, we're working with the University of Chihuahua. We have a drilling contract with them from Mexico, who's doing the drilling. We have folks on site overseeing the drilling as well and, again, working closely with the University of Chihuahua. But, water table, again, is about 70, 80 meters, less than 100 meters below the deposit. Water flows in general in this direction. So, from my left to my right, maybe more from my upper left to my upper right. But, the point is these two holes here are up gradient and down gradient boreholes that were not cored. They were punched down below the water table to collect water samples.

We've also cored through the ore body itself. The core rig is actually still working. I'm not sure if it will still be working next week when we get there. But, we've gotten stuck a couple times, and so we've had to pull out and move close by to continuing coring. So, we may very well see something turning to the right when we're down there next week. We'll have to see.

But, it's an important program. We're collecting and analyzing rock and water samples, and that will allow us to update our conceptual model for flow and transport at the Pena Blanca site.

A lot of what I've already said, just some pretty pictures. Some of us will see this next week.

Again, we're collecting core and cuttings and also water samples. Those two holes up and down gradient will allow us to go in and collect water samples as a function of time as well.

Where we're at, we want to complete the drilling of the boreholes that we've set out to drill, do a lot of lithologic description of the samples on site, do some mineralogic and petrologic type analyses, do some addition U-series analyses of the samples, and also look at leachate from the rocks to look at sorbed radionuclides that might have moved away from the ore deposit, and how far they've travelled, et cetera, et cetera.

Again, continue to sample and analyze water from the wells that have been drilled, update our stratigraphic and hydrologic framework understanding for the deposit, develop a conceptual model, and finally, do process of the models to simulate the uranium migration in the unsaturated zone at Pena Blanca.

Bob will talk about potential follow-on work that could go on at Pena Blanca in addition to this, and so I won't say anymore than just to set him up and let him talk more about that.

Moving now to the igneous consequences studies, igneous consequence peer review. Dr. Rubin I believe is here. He's going to give a presentation on the results of that DOE sponsored peer review on igneous consequences, so, I don't want to dwell on this. He's going to really talk about the charge to the peer review committee to look at the adequacy of the models, the ability of the models to quantify uncertainties, and finally, the level of analysis necessary given what we have, how can we adequately address the issues given the limitations of the science that we currently have. And he'll talk a lot more about that.

Again, he's going to talk about the report that was issued in late February. It was a thorough and complete review. It was an excellent peer review. Those are my words. But I think it was outstanding. It really helped us a lot in terms of really focusing in on what we really needed to do moving forward to license application.

Many of the comments we're already addressing. We already had ongoing work, or we've started work that addresses a lot of the comments that were made. Some of the planned work will be confirmatory in terms of the license application timing. By process, we owe a formal response. We do not yet have that formal response. It's in preparation and will be available at the end of June. And also, of course, the Nuclear Waste Technical Review Board had a set of consultants that commented on the report, and those were on the TRB website. Those will be considered as we lay out our path forward.

What sorts of ongoing and planned work are we talking about? There was a lot of discussion of magma discharge rate into the drifts and how that affects the magma pressure within the drifts, which is key to how many packages you might disrupt in a disruptive scenario involving dikes intersecting a repository drift. That's a big focus of our work. A lot of discussion about the effect of a propagating dike tip. That's also being accounted for in our ongoing and planned work. And, finally, quite a bit of effort to try to model how the magma, the dynamics of magma flow inside that drift if a dike would intersect a drift.

Additional field studies. This might be somewhat confusing. What we're really alluding to here is recall the probability, I talked about probability aspects at the last meeting. Recall there's some potential aeromagnetic anomalies out there in the area that we're looking at doing some additional work. It's in the baseline, but it's in the out years as confirmatory work to look at potentially additional geophysics as well as potentially drilling some of those anomalies to address the probability aspects of the problem.

So, to wrap up, I didn't cover all the data collection and testing program that's going on, but I've tried to capture what I hadn't talked about in previous meetings and what I think the Board wanted to hear about more at this meeting. We've got an ongoing program. We feel it's addressing the uncertainties in our system and provides that additional confidence that's necessary to support our license application.

So, with that, I'll stop.

LATANISION: Thanks, Mark. Questions? David?

DUQUETTE: Duquette, Board.

Could you go to Slide Number 27, please? Mark, as you may know, a number of us visited the laser testing facility near Livermore a few months ago. It's a very impressive operation, as a matter of fact. However, if you look at this diagram, it looks like your compressive stresses from the laser peening probably are about a millimeter to maybe as much as 3 millimeters before you get to the tensile part of it.

PETERS: Right.

DUQUETTE: Just using an approximation from this. And, of course, what that doesn't show, and I'm a little bit surprised at it, is I would have expected higher tensile stresses below the compressive stresses. I realize that's a model that you have up there now, but the fact of the matter is the point that I'd like to make is if you get a localized corrosion process that eliminates that upper 1 to 3 millimeters of material, you're getting into a fairly strong tensile field below that. And if you do have a stress corrosion cracking problem, it doesn't go away if you have a localized corrosion process that gets you into the tensile area.

PETERS: Right. Understood. You could corrode this layer away, and then also you've exposed yourself to the SEC.

LATANISION: Correct.

PETERS: I see your point. I think that gets--good point--the only thing I would say is is this whole business about continuing to push technology to try to drive that compressive state deeper and deeper into the weld I think is an important part of the puzzle, and I think you probably saw it, they're trying real hard to see if they can drive that compressive state even deeper and deeper into the sample, and I think that's important technology that we need to go do.

LATANISION: Yes, no question.

PETERS: That would be my only comment.

LATANISION: Just to follow up on that, and on the same subject, in your backup material, Number 64, if we could go to that? That's the corrosion rate?

PETERS: Right.

LATANISION: I would expect laser peening to affect phenomena like stress corrosion cracking because you're building in a compressive residual stress. But does this refer to electrochemical polarization as has been suggested that this is uniform corrosion rates; is that the case?

PETERS: I believe this is--I don't know if Joe made it this morning, but I believe it's localized corrosion measurements.

LATANISION: Localized meaning stress corrosion?

PETERS: Yes. Joe can elaborate.

LATANISION: Okay. I'd like to clarify that, because--and I appreciate that maybe--

PETERS: He's on his way up.

LATANISION: Oh, okay.

FARMER: Yes, Ron, I think the measurements, these I think were done by Frank Wong, and I think that these are standard electrochemical measurements, and he's probably taking current density and converting it into penetration. I don't think these are stress corrosion cracking propagation rates, if that's what you're asking.

LATANISION: It just surprises me because, I mean, I would have expected residual compressive stresses to affect stress corrosion cracking, but I wouldn't have expected to see an effect on the uniform corrosion rate. This is interesting data.

FARMER: Well, the French, you know, they have made some measurements and they've published it at the Electrochemical Society meeting and some investigators there have seen an impact of laser peening on the, you know, like the potentials we discussed yesterday, you know, the corrosion potential, the repassivation potential. I don't recall, you could look at the data, as I recall, because this was a couple of years ago in San Francisco they presented this, and it seems to me I recall that the current density, just looking at their raw data, was affected, but I don't think they really summarized that data. So, I think what Frank is observing is probably consistent with that.

LATANISION: Interesting. Okay, thank you.

NELSON: Thanks, Mark. Nelson, Board.

I've just been looking at your backup slide, and just a couple questions. On Backup Slide 59, you have some summary data on strength versus perhaps some sort of a calculated or otherwise estimated visual porosity as opposed to measured. This is for the lithophysal rock?

PETERS: Yes.

NELSON: And this is dry?

PETERS: I think it's probably mixing data. I couldn't go through the data points and tell you exactly what the saturation state is throughout.

NELSON: Because there is a moisture content.

PETERS: Right.

NELSON: Influence on strength.

PETERS: And, unfortunately, I didn't put that in backup. I had another one that showed the strength as a function of saturation. I didn't put that in.

NELSON: All right. And the large diameter cores are 12 inch?

PETERS: Yes.

NELSON: What does this tell you about the behavior of the rock at the stress conditions in the drifts?

PETERS: You mean in terms of how the drifts will degrade?

NELSON: Yes. Is this what you expected, or is this high enough to have non-thermally driven stress redistribution causing deterioration?

PETERS: Yes is the answer that I would give. Is it what we expected? I think my resources for those kinds of questions, I talked to Mark Board. When I asked Mark Board that question, he said yes, this is what we would expect.

NELSON: All right. And thermally, you would expect additional?

PETERS: Yes, there would be an effect.

NELSON: Although it's a mix of dryer rock at that point.

PETERS: Right.

NELSON: Which is stronger rock.

PETERS: Right.

NELSON: What's the understanding that you have now about what's going to happen? What are the adits going to look like in the lithophysal rock at the end of the thermal pulse?

PETERS: There's simulations that show drift degradation as a function of time that I have seen. They are still very preliminary, and they make certain assumptions, and the key is what the--I'm going to pretty quick here go into your area. But the cohesion aspect is key, of course, and what you assume for that. So, what you assume, depending upon what you assume, you could go anywhere from a completely collapsed drift to a fairly intact drift. When you look at the kind of cohesions that we expect, there is some degradation, particularly in the lithophysal units. That's being looked at. I mean, that's being modelled as we speak. It's also being looked at in the context of how that influences seepage and other aspects of the process.

NELSON: Nelson, Board.

At the panel meeting last month, was it, Mark Board made some pretty interesting at least preliminary presentations of directions that he was going, which looked pretty interesting. But, if some of this deterioration is strength, is non-thermally driven, do you see significant deterioration in the ESF? I know that there's been some in cored holes, but in the ESF at that larger scale, or in the ECRB, have you seen deterioration in the tunnel molds?

PETERS: A little bit of what you would call, I think you would call raveling or air raveling on the side, probably from the drying. But significant degradation? No.

NELSON: And that air raveling separating what is the tunnel drape muck from what is actually air raveling because of drying, is part of the dust question I was asking yesterday.

PETERS: Okay, fair enough.

NELSON: Okay, this should be really interesting. I think it's important to continue to observe any deterioration that's occurring in the ECRB, even though you might not be doing very much active experimentation in there.

PETERS: Right.

NELSON: I'd like to ask you on 61, Slide 61, you've got some discussion there about reactive transport experiments with seepage. Can you explain this?

PETERS: What we've done in the laboratory is we've put various kinds of grout mixtures--I talked about this in the January meeting, but it was added in because we thought it might come up in the context of the near field environment, and I recall yesterday when you asked me are we using grout in our drifts, right now our design basis is no grout. But just the same, we are looking at these are lab experiments. This is a conceptual model that kind of lays out the problem. There's lab experiments in autoclaves, closed system autoclaves, looking at reaction of grouts with solutions and how the CO2 and the pH evolves, how the grout carbonates and how that evolves over time, too. Those are being done by Carl Steiffel and folks like that at Livermore.

NELSON: Okay. And when I asked you about rock bolts, this is Nelson, Board again, yesterday, you said split sets and not Swellex. Is this sort of a reaction to questions about what's happening in the drift scale test with Swellex and the iron?

PETERS: It's more, and, Priscilla, I might have--the take home point was no grout. Grout is an important driver to the in-drift chemistry that was, let's say, a parameter that was adding uncertainty to the system that we currently did not want to deal with. So, we're going with the ground support that doesn't involve grout in the drifts.

NELSON: So, Swellex are still--

PETERS: Yeah, I used maybe split sets in the example.

NELSON: Okay. And just finally a question on the ages of the groundwater that you gave. Are we to interpret those as real ages of the groundwater?

PETERS: No, no, I'm calling them apparent ages. And, also, when you look at the kind of travel times, if you look at travel times in the SZ that come out of our model, you know, they range over quite a broad range. This is just me looking at the data. I'm very encouraged at the kinds of apparent ages that you get are in the thousands of years. That's consistent with my general idea of the way the saturated zone operates.

NELSON: Nelson, Board.

When I look at the map, I'm trying to figure out some of these appear to be bedrock saturated zone, some may be alluvial samples. Is that true, that there was a mix in the data that you--

PETERS: Some would have come from the volcanic aquifer.

MELSON: Item Number 68, the last one I think that you have.

PETERS: Yes, this is color coded to the plot that's shown in the main part of the presentation. And the answer is they're samples from the saturated zone, so, yes, that by definition, some would be from the volcanic aquifer, and some would be from the alluvial aquifer.

NELSON: All right. Okay, thanks.

CERLING: Cerling, Board.

Just returning to the question of the radiocarbon dating on your Slide 36.

PETERS: Right.

CERLING: On the page after that, you say that particularly light LDC 13 values suggest organic contamination, and I was just wondering what you thought that might be, or if you have any insight on that?

PETERS: I don't. Zell, do you have any insight on what, other than what's said in the bullet, is there anything to add? Maybe for my benefit, and perhaps Dr. Cerling's benefit, how light are they? He might appreciate that.

PETERMAN: Zell Peterman, USGS, and I don't remember.

PETERS: I stumped you.

PETERMAN: Too many numbers to carry around. The light values we're seeing in some of the Nye County waters, and I guess it's maybe just suggestive that, you know, for a freshly drilled well that's not a production well, it's probably a little difficult to clean out to the extent that we need to be able to use this technique.

PARIZEK: Parizek, Board. On Figure 8, you show the red spots, and they seem to be maybe I guess positioned with regard to rock bolt holes. The time of red spot formation is given. How does that scaling this off to statements that Bo made yesterday about maybe dryout zone for 2500 years? Here, we're getting obviously drips back through the dryout zone in the X number of days from the time the heaters are shut down.

Is that consistent or inconsistent with this vapor barrier discussion yesterday?

PETERS: Let me be clear on the current hypothesis that it's not discrete fracture flow back into the drift that's bringing the deposits. It has to do with rapid cooling of the rock bolts and the iron oxides flaking off the bolts.

PARIZEK: But the repository will have rock bolts. So, we're putting it back in the context of what the repository probably will look like?

PETERS: Yes, and I would take it back more to my piece of the story, where we talked about committed drift materials. That iron oxide is a part of the in-drift chemistry that one has to deal with to understand how it evolves. But what I want to clarify and make sure is real clear is this, we currently do not think that this is evidence of discrete fracture flow back into the drift along the rock bolts.

PARIZEK: But the water, what's the source of the water? You had to get the water to the rock bolts.

PETERS: It's actually--the rock bolts are actually swelling and contracting.

PARIZEK: They're just falling out as dry material?

PETERS: Yes.

PARIZEK: Okay. So, it's not a drip. Thank you.

PETERS: Yes, that's what I was trying to say.

PARIZEK: Then is there any new information on debris on the heaters in the heater experiment? I mean, any rockfall debris that's accumulating?

PETERS: There's been some. Recall a few meetings back, we talked about there had been some, and I don't recall exactly, I believe it was in this area in the roof, there were some rocks that--there were some slabs, thinner slabs that had fallen onto the mesh.

PARIZEK: Right.

PETERS: You see some pebbles and things along spring line on the floor all up and down.

PARIZEK: That got through the mesh?

PETERS: Yes. Big chunks? No, nowhere have we seen that.

PARIZEK: But the mesh, there's some slabs, but it's not changed much? We're looking really at the role of heat on rock degradation, and yesterday this was not discussed, I was cut off, but if we have a hot repository, there's other aspects to yesterday's discussion that were brought up in terms of just the stability of the rock through time, and then that creates an environment of its own.

PETERS: Agreed. And Priscilla was also bringing that part of the picture in. When you look at this kind of picture, you know, the mechanical piece needs to be a part of the story. That's going to be brought in.

PARIZEK: Right. And then on Page 31, you had some discussion about faults and their effect on flow paths. Can you be a little more specific as to what's happening there in terms of either what new drill holes may have hit the faults, or are planned to hit faults in future drilling?

PETERS: Go to--

PARIZEK: It was Slide 31 that you had that.

PETERS: Yes, but I want to see the map. Go to 29. I won't be able to take this into the kind of detail that Ettibar or somebody could, but what they're doing is looking at alternative conceptual models, and I'll tie it to some of the observations that Linda Lehman made in the past as well about the importance of some of these north-south trending faults and how that may control flow. That's the kind of thinking that they're doing in the alternative conceptual model.

PARIZEK: Because everything for the moment is south, southeastward, south, which buys time. On the other hand, if it is straight south, that has to be understood whether it is straight south or not.

PETERS: Right. There's, of course, a series of flow paths, the ones that you're referring to, the one is the primary flow path that kind of does that.

PARIZEK: Right. One other question, and it doesn't have to be answered necessarily now, but in terms of Pena

Blanca, you've got basically this uranium oxide in joints, in a matrix of tight rock. Yucca Mountain, on the other hand, is going to be waste packages in a kind of well aerated, not open space, and so the connection as an analog needs to be thought about here, and I don't have an answer for that. But I'm just going to be thinking about that next week when we go down and look at this place.

PETERS: Okay.

PARIZEK: Here, you have rooms and you've got waste packages. It's a little bit different than maybe having a little tiny crack with some uranium, you know, jammed in that little space, and as a result, the availability of leaching it out of there may be a little bit different.

PETERS: Good point.

PARIZEK: So, how to make the connection, and I'm going to be watching to see how to do that, and maybe you even know how you're going to do that.

PETERS: Well, that will be a good discussion. That will be a good one to sit when we're kicking the rock.

REITER: Leon Reiter, Staff.

Two questions, Mark. On Pena Blanca, I notice you did not mention the waste form dissolution. You talked about the flow and transport. There's some stuff NRC showed at the last waste meeting, they get a very large reduction in dose due to assuming Pena Blanca model. Has that work been finished? Or do you plan to do anything about that?

PETERS: That's not as much of a component of work that's being done on the project, but that's being looked at, and maybe, Bob, you touch on that. The S&T program has currently a group of people putting together a very detailed proposal or plan that S&T is going to consider for '04 funding, and that's being brought into that program for serious consideration.

REITER: So, waste form dissolution, that will be post-licensing application?

PETERS: Yes, that aspect of Pena Blanca would come into the S&T program if it's funded next year.

REITER: Okay. Because I thought you had done some--okay, second question about the igneous consequences, I notice you mentioned the word mathematical modeling, that you're doing some. The panel had some pretty extensive recommendations using things, making sure using compressible flow in both the dike tip propagation and dike drift interaction. Is the modeling that you're doing now, do those take those recommendations into account?

PETERS: I'm going to punt. The question was the modeling techniques that we're using to look at dike tip propagation and also magma dynamics in the drift, the peer review panel had quite a few recommendations on how one goes about modeling those problems, and we've alluded to the fact we're modeling, and the question is are we taking those--

CLINE: Yes, Mike Cline, BSC.

I can say yes, we are seriously looking at the comments. They had some 30-some comments related to the dike propagation and the pressurization. They're looking at that very seriously in their models.

REITER: And this stuff might be pre-licensing?

CLINE: Yes.

MELSON: Melson, consultant to the Board.

Just real quick on the magnetic anomaly work. There's been a lot of aeromagnetic anomalies that were done, and as you know, the closer you get to the ground, the more sensitive a magnetic survey is. And, so, I'm wondering what you would say about the possibility of missing small dikes. They still may have important implications if we did--what is the level the planes are flying for this work, and what might we see more clearly by someone on the ground magnetic measurements?

PETERS: The more detailed surveys, some of those are going to be ground based; correct? Or no?

CLINE: Mike Cline, BSC again.

The airborne aeromag will be by helicopter. So, it will low elevation.

PETERS: Okay.

CLINE: I don't know the exact elevation they're going to fly, but it will be low elevation.

PYE: Pye, Board Staff.

Mark, could you turn to Slide 15? I recognize this dataset from a paper at the recent High-Level Waste Conference, so a couple of points. The dry thermal K on matrix samples, I think the paper reported about a 1.7 plus or minus point watt meter K thermal conductivity. They did an interesting experiment. They put two transverse on orthognal 5 millimeter holes in one or two of the packs. They actually introduced about a 10 per cent artificial porosity into the matrix. Interesting conclusion. It increased--or reduced the thermal K by about .5 to .65. So, that gives you an indication of what additional porosity can do to at least matrix thermal conductivity.

PETERS: So, just let me follow you. So, it was here, and it came down to here?

PYE: Right. Correct. Again, what wasn't clear in the paper, and maybe you can clarify it here, there are two outliers, again showing thermal Ks around .75, two inch diameter specimens. Okay?

PETERS: These two?

PYE: Right. Again, if you look at Dave Bush's work, and you look at the work he's done with lithophysae porosity, potentially you could see, oh, another 20 per cent porosity introduced on a rock mass scale to the composite. So, using the artificial porosity as an indication of what it might affect, is if you take a simple volumetric averaging concept for a rock mass thermal K dry, you could be potentially looking at thermal Ks, oh, a mean thermal K around 1.

In the same paper, Livermore did three tests. They backed out of thermal diffusion tests, thermal Ks of .5 to 1.1. So, my point is this. If you look at the chart we used yesterday, how would that affect peak thermal temperatures and the duration of peak pulse, and is the project looking at the implications of lower thermal K on thermal management and repository design?

PETERS: Well, yes, it will affect it. That's the easy part. You're aware I know of the SSPA calculations where they did sensitivities on that parameter, and how that might affect that curve.

PYE: Well, the thermal K in SSPA related to the uncertainty. And my main concern is the mean is going to shift significantly without even accounting for uncertainty.

PETERS: Well, yes, I guess, John, I think part of what you're--let me maybe take another way of what you're getting at. What you're getting at is you can draw relationship between thermal K and porosity that might be, say, a line like this.

PYE: Yes.

PETERS: And when you get to the kind of porosities that

David is measuring in the field, you could be way out here.

PYE: When you account for matrix and lithophysae.

PETERS: Right.

PYE: Total porosity, the constitutive properties at a rock mass tunnel scale would drive it down to closer to 1.

PETERS: So, now I'm going to talk out the other side of my mouth when they say they're consistent, but I'm going to say that where David does those measurements is the same place that we're getting these kind of values in the field.

PYE: Well, I understand that, but let's look at Slide 16. A number of issues. Again, we've talked about dry thermal K, and there's also a set of wet thermal K, and clearly the saturation has an effect on the thermal K value. Okay, the test configuration shown in 14 would place both the heater and the thermal couples at about the dryout zone.

PETERS: Right.

PYE: So, there's some question as to what is the saturation. It's not totally saturated. It's partially saturated.

PETERS: Right.

PYE: The other issue, the uncertainty associated with the in situ lithophysal porosity. Again, there's some uncertainty there.

PETERS: Okay. Well, I guess--well, clearly, we've got a set of field data and a set of laboratory data, and we think they make a consistent story. But, there's going to have to be uncertainty analyses done in support of the license application so that we can nail down what exactly this temperature time history is. I mean, there's a lot of discussion, and Nancy Brodsky and Jeff Robertson, people like that, need to be up here to talk about the difference between flash methods versus field versus lab.

PYE: Right.

PETERS: With regard to heated plate type stuff.

PYE: And, again, you've made a point in a number of presentations when you reported to the Board that temperature and thermal gradients affect the results, too. The results shown here are again for 70 degrees, and again you're talking about operating a repository up in the 160, maybe 180 range. So, again, it looks like it could be very significant on the project's thermal management approach.

PETERS: Fair comment. Setting aside your what I'll take as concerns on the field tests, as you know, the big one, we heated up--we actually dried out quite a bit of rock, and we're looking at both dry thermal properties inside that dryout zone. So, we have elevated rock above boiling and looked at the properties above boiling. We're trying to get at that problem.

PYE: Also, on a practical level, coupling is a problem. What processes go on actually in the boreholes? Are they limited to purely conduction, or is there a convection component, too? Again, they're difficult measurements to make, and I'm pleased that you're doing them.

PETERS: Thank you.

LATANISION: Thank you, John. Mark, thank you very much. I think we will transition to our next speaker. Thank you.

We're going to hear next from Bob Budnitz about the Science and Technology Program. Bob, thanks for joining us this morning.

BUDNITZ: My name is Robert Budnitz. I'm from the Lawrence Livermore National Laboratory, and as the slide shows, I'm on detail for two years to the OCRWM office. It's been about six month, and so I'm just learning my way around. I'm on a two year assignment, along with Tom Kiess, who's here, and Mark Peters, with whom you just interacted. The three of us are putting together the new Science and Technology Program.

And this says update because there was a presentation two Board meetings ago in September by Steve Brocoum. At that time, there had been a six month task force that Steve chaired. Perhaps a half a dozen people in the project who had done some scoping work, very important scoping work, to put together what was the foundation for the Science and Technology Program.

Then, in the fall, it was really November 1st that I showed up, and Tom Kiess came on board just shortly before then, the task force was disbanded, and this became an office or a program on the organization chart with NRW. So, it's a little less than a half a year, and we're just getting started. And to give you a preview, this year, fiscal year '03, the budget for this activity, which by the way, we didn't even start until, you know, the continue resolution wasn't until March, and we didn't really start until April, and the first money is just being sent out now, this year the budget is $1.7 million, as Margaret Chu I guess said yesterday.

But next year's President's budget is $25 million, and if the Congress appropriates it and we get that, we have a real program, and that's a preview of the slides that I'm going to come to.

So, the first slide. And this is a crucial point, and Margaret said this a year ago. In fact, Undersecretary Card was here just about a year ago at this time talking about this, too. The idea here is to take the longer view. What the longer view means is that programs that mature in three years or five or even ten are not only part of the program, but that's the thrust of the Science and Technology Program. We're not going to undertake projects with a six or twelve time frame. We are going to do things that have a longer view, because we want to make sure that when we get to 2010 or 2015 and look back, that some long-term things that require the sort of diligent long-term approach have been undertaken and bear their fruit.

Furthermore, as it says, we're explicitly distinct from the mainline activity of the license application. What that means is they're doing their thing and they've got 99 per cent of all the effort. And this is distinct in the sense that we're not interacting with them to try to produce a deliverable that's going to affect the license application, which is only a year and a half away, and our program is explicitly longer. But, also, as you know perfectly well, the license application is going to be followed by a couple of years of staff review, and then a year or more of hearings, and who knows what else, during which all sorts of probing of the license application and its technical basis are going to take place. This is explicitly distinct from that.

Now, from time to time we may uncover something in this program that's relevant to that, and if that's true, we'll bring it forth. And if it's something negative, we'll publish it the very next day, and if it's something positive that can help, we're going to decide what to do with it as it impacts the license application. But, when I say explicitly distinct, it's that not only are the activities going to be separate, although everything is related, but we've made a pledge at least this year not to draw important people away to do new projects in this area that are vital to making sure that the mainline work takes place.

Furthermore, the scope is very broad, as it says, to support all of the activities of the office. You know, the principal thing that's going on is Yucca Mountain. But, the office has responsibilities, if you go read the chart here, that go to managing the nation's radioactive waste that go beyond Yucca Mountain. Right now, for example, there's more waste than will fit into the 70,000 metric ton limit, both on the government side and on the commercial side, and what's to happen then, I mean, God only knows. None of us know. But, with certain projects that could impact that are explicitly part of OCRWM's scope and, therefore, part of our scope. How much of that we do is going to be decided as we go through the years, because we're just starting. But, it's certainly part of the scope, and proposals and ideas along those areas are not only welcome, but are going to be given, you know, due consideration.

And as Margaret said yesterday, Margaret Chu, the Director, and as Undersecretary Card has been saying right along, the goal here is to institutionalize this program so it's a permanent activity, so that in 26 and 27 and 2017 and 2027, there will be a program which is doing for the office what needs to be done in looking at the long term. Now, that's a very important philosophy, and it's something that I believe will have tremendous benefit looking back.

Now, there are two quite different objectives, although they very much interact, and I'll say what they are. These words are in testimony, and so on, and so these exact words are what they are, but the idea I'll try to explain because it's important to explain it. Everybody should understand.

First, the objective is to improve existing technology and develop new technologies that could achieve savings and efficiencies in the system, the broad system. And, secondly, it's to improve understanding of the repository performance, or by the way, it might be understanding of the transportation system, or generally of the activity.

Now, I've got to explain this because it's important that you understand my perspective, because, of course, understanding leads to improved technologies, too. The way I like to explain this, and I've been doing this recently, and you'll have to bear with me for a minute, is use the analogy of commercial aircraft. I was in college and the first jets came out, and I remember flying on that 707 to Florida from New York, and it was marvelous. It was faster and it was safer and it was cheaper. Everybody that's old enough remember that? It was the best there was.

But, today, we're not flying 707s. We're flying 757s and 767s and 777s and modern airbuses, and everybody knows they're better. Now, they're better because--and it's only 30 years--because of a lot of little improvements and a few break-throughs that happened in the industry, and some that happened outside the industry to which the industry took advantage. And it isn't only, and this is a very important point, it isn't only that there are better engines and better metal and better computers and better control system, that's all true, and everything in the planes is different, but even what you think are little things are better.

For example, the seats are fireproof, saves lives. The galleys are better. The exits are better. But, you know, a person walks on a plane and sits down in the seat, and there's wings and a pilot, they don't necessarily understand that. But let me explain to you something.

The repository application which we're going to put in at the end of next year will discuss and describe in detail that a certain waste package is going into the mountain in 2010 for the first time, and there will be certain robots and certain instruments and there's certain surface facilities and there will be cranes and there will be, you know, everything. And it also says that the last one is going in in 2034 and it looks just like the first one.

Why? What else could it say. So, it says that. And approval is going to be sought for that, and if approval is granted, you could do that. But, we're not flying 707s anymore. And I don't believe the last one is going in and the same robots and the same instruments and the same metal is going to go in in 2034. Do you?

But, of course, you have to have a program that develops technologies, takes advantage of them, and learns about the system and understands the margins, so that new engineering is enabled by the understanding of the margins. Sometimes you have more margin than you really need, and sometimes you have not enough and you have to do things. So, this program has as its objective understanding and engineering technology on its own. But, of course, the understanding leads to technological improvements of various kinds, some of which are, you know, metallurgical and some of which are in the earth sciences and some of which are in the surface facilities, and so on.

Now, let's just get back to the bottom line. The 757 is more than an order of magnitude safer than the 707. Everybody knows that. And that's one of the bottom lines. If you're flying an airplane, you want it to be safe, you want it to be efficient. It has to, you know, has to cost something, so that it doesn't $100 million to fly from here someplace. It's cheaper. The fuel efficiency is one-third better. And that came about because of a program that took little things and put them in, and big things when they came along.

Now, I can't promise you anything about what we're going to do here, but the program has as its philosophy to develop technologies, to achieve savings and efficiencies, and increase understanding, so that when we get to 2015, we look back and say, gee, we're glad we did that. So, it will be going in. And when we get to 2034, we're going to say for sure, because without such a program, the same technologies will continue to be used, or at least their penetration into the system will be very sort of catch as catch can, and not systematic. So, that's the idea.

This idea, of course, depends on follow-through, which is just starting. This year, we have $1.7 million. It's just starting up. Next year, $25 if the President's budgets becomes reality. And as Margaret said yesterday just right here, she's hoping that the following year, it will be 30 or 35. We're not sure. They're still planning. But the idea is that it should be several percent of the budget going into things that will help make the system more efficient, more cost effective, and increase our understanding.

Now, as I said, this year, there are few initial projects that we just started now. By the way, the funding for them is just going out now, a few last week, and some the next couple weeks. Tom Kiess is handling that and probably you can ask him the details if you wish.

Some of them are scoping, and I'll tell you about them. Scoping means, you know, a three or a nine month thing that's going to help us understand an issue so that we can plan a real program. And some of them are actual things that we're going to start now that are a few years long that we already know about, and that we hope to use as a springboard.

Next year, we're launching a major program, which I'll tell you about in the next couple slides. We're planning for $25 million. We have a plan that demonstrates why $25 million makes sense, and part of it is because people that are skeptical of this will ask the question, and it's a fair question, why $25 million, and why this year? Well, of course you can't really argue about why 25 if somebody says 22. But, I can explain why $3 million isn't the right number. And why this year is an easier thing to explain, and I'll try to explain it to you.

You see, all through this time, and it's been 15 years, the project has been trying to develop a final license application design, a design for the surface facilities, a design for the repository, a design for the invert, which metal to use, which it's not grout anymore, that sort of thing. Finally, that design is just now being frozen for the purposes of submitting a license application, and that frozen design is going to be subject to review by everybody, the public, you, NRC, ourselves. And for the next two, three, four years, the people who would otherwise be working on improvements, for example, three years ago, something, and now it's better, aren't going to be doing as much of that because they're going to be doing what you think they're doing. They're going to be trying to defend why we think this design should be licensed to go ahead.

This is the perfect time for a program like this to now take off and say, okay, while that's going on, with 90-odd per cent of all their effort, we're going to launch this thing, which is then going to take the next step and provide the basis for improvements as they come along. Is any of our stuff going to get in the license application? No way, it's only 18, 20 months away. Might some of it come along in two years or five? You bet.

What will happen to it? Well, I can't answer that question, but obviously if we come up with something that's better, we're going to contemplate putting it into the Nuclear Regulatory Commission as an amendment or an exemption, or whatever it is. There's a change. And perhaps they will improve it, and maybe they won't. I mean, that's the way nuclear power reactors have worked all this time. An older reactor, take a reactor, for example, like Diablo Canyon in California where I've been the last 35 years, it's been there for 15 or 20 years, and the design is 30 years old. There are amendments every week, every month, that say we want to do this a little better, and they're approved because they're efficient or they're better or they're safer, or whatever. I'm sure that's going to go on here, too, but we're not contemplating any specifics because who knows what's going to come.

Now, in order to launch this $25 million program right, we've done two things, and I'll explain them to you. First is we've done a lot of work ourselves. We have a small staff, and some help, which Ii will tell you about. And we've been trying to figure out just where the most promising opportunities are, and as you can imagine, and this is easy to imagine, as soon as there's new money out there, people interested in it come to you. They call you up. They want to have meetings. They send e-mails, they send brochures. And this is wonderful, because you have no idea how many neat ideas there are that people have been stewing on that might make an improvement.

And, so, we've been hearing them. A lot of it has come from our national labs. We actually, and when Brocoum's task force was in existence, they actually solicited the labs, went out to each lab and said tell us about your ideas. And some of those are the basis for the early program. But, of course, the people there know, because some of them have been designing something and they know full well that what's in the license application, some proven technology, and they've got an idea that they know about that's three years away, but it isn't in there because it's not proven. That's our role.

So, we have a whole lot from the labs and the U.S. Geological Survey and within the project. We're getting ideas from outside, from companies, from universities, from institutes, and the like. In order to sort this out, we're planning a broad solicitation, which is what this says, a request for proposals, which we expect will be out sort of the end of the summer and the fall. We're not sure what the schedule is now because we're working on it. But, the idea is to get wider input, including both existing technologies that some company has or university has been developed or there's an institute or the labs, that could be applicable, and also out of the box ideas. We're really looking for out of the box ideas, something that could be quite different that would take a long time to develop, but over the years could make a major improvement.

I mean, just one example, the waste packages are metal. Well, non-metallic waste packages aren't in the plan now, but who knows whether that will be the best thing in 2026, just to pick a year that's so far away that I can't contemplate it. I mean, that sort of out of the box project idea is something we're looking for. So, then we'll put together a solicitation, and anybody can bid on it, including foreign, except the labs, because there's a rule against them. So, we're going to have to go to the labs and do that separate. But anybody else can bid, and we hope, and this is sort of one way of getting the word out, we hope that if you know anybody that has an idea, that they should know about this and they should send in proposals, and we're going to do a competitive evaluation, and we're going to fund.

The whole $25 million isn't going to go into this solicitation because some of it we're going to direct to the labs, and some of it is going to ongoing work that we're starting this year. But, a lot of it will be, and we hope that we're going to get a whole lot of interesting technology ideas, analysis ideas, and so on, that will help us launch this thing in a very strong and technical way.

How that's going to come about, I don't know. We are not even sure what's going to come in until it does come in. Now, turn to the, not the next slide, but turn to two over, because I put these in out of order, and let me tell you how I realize it, putting them in out of order, because exactly a week ago, I gave this same talk to the National Academy's Board on Radioactive Waste Management. That was my dry run, and I now know what questions they ask, and I realize the order should have been like this.

So, let's look ahead to next year, and I'll come back to the previous slide and talk about the $1.7 million for this year after that. Next year, we put together, and this was a request from Margaret Chu, what we call principal program thrust areas, our themes, about which our program is going to be centered. Now, this isn't everything we're going to do. Somebody with an idea elsewhere that isn't in one of these themes should certainly propose it, and if we're interested and it looks like it's--or whatever, we're going to go with it. But the purpose of these is first to explain to somebody outside just what it's about. And most everything of these things, by the way, not everything, but a whole lot, and, secondly, to help us focus the write-up so we can tell people what we're looking for, not in detail, because we're looking for lots of ideas, but sort of explain the issues.

To help us do this, Margaret Chu appointed a half a year ago, a review panel of experts, of people that a lot of you in the room know, and they have met three times, I guess, with us and have done a whole lot of work on their own to help us develop what technical ideas there are out there that would be worthy. It's chaired by Dave Moeller, retired from Harvard, Joe Payer who was in the room yesterday from Case Western Reserve, and an expert on corrosion, Chris Whipple from California who most of you know, Charles Fairhurst, University of Minnesota, and (inaudible), who's retired from the Sandia from the WIPP project, and the five of them have been reviewing our work and trying to help us understand the issues and launch this. And with their help, we've developed these--well, I'll just explain them briefly because it's sort of more than just the words, but it isn't a lot more, because we don't have too much explicit thinking of ours into this. We just want to hear from the world about it.

But advances in materials is one of them. We're looking, as you know perfectly well, much of what limits the technology that's in that repository or on the surface is limited by materials, and we're looking for all sorts of ideas for advances in materials.

Next is sensors and robotics. There's a tremendous advance each year, fast moving field of robotics, and there's a whole lot of robotics in the surface facility design and the underground operations, and we're looking for advances there and in sensors that could be deployed that will do what we need to do better and less expensively, more efficiently over the years.

Drift engineering. This is an important one. As you probably know, and we talked earlier about the drifts, the drift design uses existing technology, but there has been a rapid advance in drift engineering in the last--it just continues a pace, like in many other fields, and there are novel ideas in drift engineering that we could take advantage of, and which we hope to develop, that could easily change the way the drifts are put together, so that, you know, we're not going to dig all those drifts in the first five years, they're going to be dug, and so on, as needed over the life of the repository, out to the 2030s.

So, if there's a better way to do that in 2015, we'll start using it. And that's again, a long-term thing that we think could have tremendous potential for us.

The next area is source term, and that's the whole area of understanding what happens in the repository once water finally contacts the waste form, as it will, many many millennia hence. For sure it will many millennias, although we hope that the analysis we'll show doesn't do it real early, and we believe that. But to understand that better requires some research in many different disciplines, and some program that can help us really feel as if we have a more realistic understanding. And that realistic understanding, this isn't something that's going to be in a year, but if it's done in three years or five years or eight years, it could not only improve our understanding and, therefore, the modeling, but it could enable some engineering changes in the out years as it's developed. What they are, we don't know. Why? Haven't done the work, having even conceptualized some of this work.

Next is the natural system, both the unsaturated zone and the saturated zone, their flow and transport are areas where we'd like to explore to understand better. Again, understanding for its own sake and also understanding to enable us to perhaps improve the design. And it isn't only there, but there we're going to do some analogues. You'll see, and we had some discussion just now about Pena Blanca, but some others that we're contemplating, in order to see if we can take advantage of that to validate or help us build better and more detailed models.

And, finally, the area of operations. Transportation is a complex transportation problem out there. There are sites in 30 states that have waste and spent fuel. It's got to come to Yucca Mountain. It has to pass through three dozen states, and there's a whole lot of technology involved, design of the transportation casks and the design of the--the logistics of the system, systems engineering. And there we're waiting on the transportation side for the main plan, and then we're going to react to that and build on that because that's the idea. There's the base program, and then we're going to do beyond it.

But, we're looking right now for ideas in this RFP, have new operations, present a surface facility. You know, the surface facility is a few billion dollars. It's one of the most expensive facilities that the Department will have ever built. And then it's going to cost billions to run over the years. So, efficiencies that could be developed there that are adequately safe, or safer, and that make things more efficient and less expensive are being sought, and we're just open for ideas.

Operations on the surface, operations underground, the transportation system, how all that fits together as a system is another one of our program themes. But, as I said, we're also open to any idea that's within the scope--when we say scope of the RFP, it's really the scope of the office, and we don't know what we're going to get. I don't know whether we're going to get 26 proposals in response or 2600 or 26 million. We really don't know. We're being bombarded by all sorts of things, so we think there's going to be a lot. But until the first round comes in, we're sort of eagerly awaiting that, and that isn't going to be until the fall.

Now, go back one because this is my last slide. We started a few projects just now, and this is the 2003. And this is $1.7 million. A few of these, and I'll talk about them briefly, but I don't have any idea how much time I have left.

LATANISION: Well, I think you have about five or ten minutes.

BUDNITZ: Okay, no sweat, thank you. And then there will be questions.

About half of these are projects that are a few months off, six months, and they're scoping in nature. We're going to try to sort out using the project what to do next. And the other half are things that are two or three or four years long, where we've launched them now. And I'll tell you about them briefly, although I don't have time to go into them all in detail. And some of them I really can't get into the detail because I'm not an expert. But, besides these, I want to explain another activity that's important that we do.

We've had a series of interactions with DOE's Office of Science. Ray Orbach is the director, and he and Margaret Chu have met and discussed, and I've been part of those discussions, because the Office of Science is interested in supporting our program, as it is other missions of the Department, but over the years, hasn't known as much about our technical problems as they want to know. I'm just explaining. The Office of Science has a budget of $3.3 billion. Of course, a lot of that is running facilities, accelerators, reactors, and a lot of it is running experiments at those facilities that people are doing. But nearly half of it is individual investigators at the laboratories and universities and institutes who are either working individually or in collaboration doing experiments and all sorts of things. And the Office of Science's mission is fundamental research and some applied research, and part of it is to support the rest of the agency, the office, its energy mission and its defense missions.

So, in that spirit, we've been meeting with people in the Office of Science to see if we can find areas where we have an idea and they have investigators already working who could apply their talent to our technical issues. We've had a significant set of interactions already on the corrosion area, and we're going to have a meeting soon with a few of their experts, people they've been funding for years who do fundamental work in surface science and corrosion, and explore whether they can apply their expertise to our issues. That's only one example of where the Office of Science could really apply expertise, it's been there for years, to help us do fundamental work that might mature in a few years, or maybe even 15, and lead to a fundamental understanding.

Also, interacting, we have been for a while with the EM, that's the Energy Management--Environmental Management, the EM Science Program. The Office of Environmental Management used to have its own science program, but it's now been transferred to the Office of Science, Teresa Pryberger runs it, and we've interacted with Teresa Pryberger and her colleagues to see if there's some work there, and there is, that they're doing that's looking at their problems, you know, waste that's transported in the unsaturated zone, for example, at Hanford, where some technology that they've been developing or some techniques or data that they've used in developing could be applied to our questions usefully by their investigators, or using some of their other--the Office of Science also has tremendous analytical capability in everything, from electron microscopes to accelerators. You will probably understand it's one of the great centers for funding that stuff in the world, and taking advantage of that is something we're trying to do.

Now, just to go down these briefly, because there are nine of them, I guess, and I won't even given them a minute each, but just to explain, the first one is a collaborative effort that we've just launched with DARPA. That's the Defense Advanced Research Project Agency, which does advanced research for the Department of Defense.

As you probably can guess, the Department of Defense has issues with corrosion. One of their services works in the sea, which is a corrosive environment. You can probably guess which one it is. And--but it isn't only them. They've had a program for years to try to work on protective coatings that could allay or eliminate or reduce corrosion in certain applications that are interesting to them, and applying some of that to our problem, particularly the waste package and the weld on the waste package is an idea that we just initiated with them. It's going to be a three year thing. Lawrence Livermore is involved on our side, and Oak Ridge and some others and DARPA, and we're hoping that over the next two or three years, something real may come out of that that could be a technological advance in the waste package area.

The next one, advanced welding method. The current method for welding up the packages is arc welding. We know how it works. But there are advanced welding methods. The particular one that we're about to fund, we're just launching, is to explore electron beam welding. The nice thing about it is today, you have to make seven, eight or nine passes around a weld, and the affected zone is broad. The electron beam welding, you do it in one pass, and occasionally a second one. It's easier, more efficient, more easily inspected. The affected zone is smaller. But to prove that that welding technique works on our alloy is a project that we're going to undertake. We hope that if it does prove out, we can use it rather than the current method, and you'll have the same package, although we think better, but in particular less expensively.

Now, you should know that the packages are 500,000 each and there are 12,000 of them, or whatever. If that's the right number, the multiplication is $6 billion dollars if you bought them all today, and maybe it's more or less, I can't remember exactly. But a few percent saving, that's big money. It's just the sort of thing where we are seeking the technology advance that's out there that we could apply to our problem and maybe it will mature. We don't know. We're just starting.

The next two are analogue, so you'll see a mix here. Some of them are advanced technologies, but there's a couple of analogues. There are some that are improving our understanding, we hope, of the repository, and that should lead hopefully, perhaps to some engineering changes. We don't know.

The next is analogues at Pena Blanca. I won't say anything about this because for want of time, but Mark Peters described the project's Pena Blanca work. We think there's a possible much more expanded scope there that could take a few years to complete, and that could produce a whole lot more. We don't know yet. So, this is a scoping analysis, study, in which we're going to spend the next few months with a few tens of thousands seeing if we can develop what the really most important ideas there that we could support on our end that would jump off on the work that the project is already doing when that scope is done.

The next one, Nevada Test Site. As you probably know, the Nevada Test Site has a whole lot of radioactive material out there in various places, most of which is still in the cavities, and some of which is in the environment around the cavities of the tests, and most of it is in geologic media that aren't of interest to us, but some of it is in media that are, tuff and alluvium, and so on. And there's been some data collected over the years, but it hasn't been looked at carefully to see whether those measurement sets could be used by our people to do a better job, as analogues, a better job of understanding transport. And, so, this is again a scoping study to look at the data and see whether there's something there that's work a more extended look later.

The next two are studies to try to improve our understanding. They're both experimental, one at Livermore, the in-drift environment at Livermore, the in-package environment studies at Argonne, and both of them are scoping studies, small experimental studies to see if we can understand better how, for example, water and hot metal interact to see if there's some advantage we can take of improved understanding of those phenomena. And, if that's true, perhaps a year or two or three from now, will add to everybody's understanding.

The next one is seismic. As you probably know, the seismic hazard at the site is really quite large. And using the current seismic hazard analysis that was done several years ago, the ground motions at the site are unreasonably large, at least that's Bob Budnitz's view, and although I'm not a seismologist I've been hanging around that community for many years, and the view of many of the people in the community. This is again a small scoping study. This is Lamont Doherty and ITASCA that's going to try to do some simulation of fault slippage, dynamic slip modeling to see whether or not by taking a cut of heterogeneities along the fault structure, you can have a more detailed understanding of fault slippage in the near field.

Faults of interest to Yucca Mountain are right near the repository. They're not 40 miles away. So, it's the near field environment that produces near field motion that is of interest, and to see if we can do a better job of understanding that through simulations would be an advance, and this is just a scoping study to see if there's something there. And if there is, we're probably going to do something more extensive that would be a few years long.

The next one is a technetium "getter." The word getter is in parentheses. Ethlene dye means layers that are low to the "getters" in the laboratory for pertechnate. You know, it just grabs it and holds it. We have no idea whether you can deploy something like that in a repository environment as an engineering--that could grab pertechnate so it wouldn't be available in the environment. This is again an exploration to see whether or not something like that could be chemically designed and, by the way, the hard part is to figure out how to deploy it, so that it could provide advantage to us over the long, long haul if and when technetium becomes a problem to analyze and transport, and maybe this could even make the analysis easier. Don't know. Again, speculative.

The final one is modeling in the drifts. This has to do with not just the modeling in the drifts, it's in the near surface around the drift, the first meter or so, or even less, where very near field modeling of discrete fractures is an issue, and this is a project that, again, is just starting, and perhaps in six or twelve months, we'll find that it shows real promise, we don't know, maybe we'll find that it doesn't, to see if we can explore better ways to do that modeling with specific discrete fracture input. Now, of course, because it's a stochastic process, the 14th drift, I don't know what it's going to look like until you're in there, you're going to have to try to do something discrete that then becomes modelled in a more probabilistic way in the end. And, so, how that all is going to get deployed, if ever, from the knowledge is something that is again speculative.

So, let me just end by saying one or two summary things. By the way, we have evaluated 100 ideas for this first set of few projects, and these choices were made by the

Director herself, Margaret Chu, came in with a whole lot of things and she said this one and this one and this one. but you can see it's a mix. Advanced technologies, analogues, understanding, seismic, something far out in the "getter," a whole mix of things that are trying to have a--to show that the flavor of the S&T program is going to be mixed like that.

Of course, we don't really know what we're going to get until we see the proposals. But we have enough already in the door, proposals I mean, you know, to do the $25 million, and it looks great. But, of course, a lot of what we are going to fund isn't going yet, because people haven't, you know--so, just what I have in my in-box, you know, the e-mails and informally, because nothing is really formal here, look very exciting, and what we're really going to get we'll only know sort of in the fall.

If you'll then go back to Slide Number 2, I just want to summarize by being sure that you'll notice this. We have two goals here. We want to try to do the very fine work that the office deserves. We want to do it in a way that assures that this program is institutionalized, so that three years from now, eleven years from now, 22 years from now, it's there as a part of the program, just as the Nuclear Regulatory Commission has an office of research, which by the way is in the statute. I was its director once, and I can tell you it's in the statute, and if it wasn't in the statute, it wouldn't be nearly as strong. Well, this isn't in the statute, it makes it harder, but nevertheless, the idea of institutionalizing it so that a quarter century later there's an office of research, that's what this is, seems to me a worthy goal.

I'm done and I'm happy to answer your questions.

LATANISION: Thanks, Bob. Priscilla?

NELSON: Priscilla Nelson from the Board.

The relationship between your organization and performance confirmation is going to be important. I know that there's a defined difference that I've had explained to me between the two, but in fact I don't think it's so clear, and I would expect performance confirmation to be back into you and for you to want to take advantage of performance confirmation to actually provide venues for very interesting complex testing and interpretation.

BUDNITZ: Yes, ma'am.

NELSON: So, when do you start working together?

BUDNITZ: Well, that's a very profound question, and I understand its significance. There is a performance confirmation program that is just now being put together, and in fact I thought it was going to be on the agenda for this meeting, but I guess you'll have it at the next one, and it involves certain tests, certain experiments, certain analysis and certain instruments. That's the baseline. Our job would be then to think about ways to improve that. Now, some of it is instruments and some of it is experiments, and so on. But some of it would be advanced thinking that produces an idea of, you know, you really would like to test Parameter 56, but you haven't got any decent way of doing that, that would be an early warning of problems if they arose. And developing that advanced thinking is certainly within our scope. So, we just have to react to that program and build on it.

NELSON: Nelson, Board.

More than reaction. I think it's actually there's a real win/win situation. As experiments are framed, monitoring situations are framed, during the framing, to be able to ask bigger questions so that you may choose some of your budget to develop in that context--

BUDNITZ: Thank you. Thank you. Perfectly correct. They may have an instrument that's a year away from being in final development in order to do that, but there may be something that's five years away, and that's for us.

NELSON: So, make your own definitions, but don't let it separate you. Finally, I'm a fed and you're a fed now; right?

BUDNITZ: No, no, actually I'm Livermore on loan. But, go ahead.

NELSON: Well, you're a fed, believe me.

BUDNITZ: No, I'm Livermore on loan.

NELSON: In any event, federal agencies to federal agencies, I think there's a partnership that extends beyond DOE, and people tend to think of National Science Foundation many times as a partner, but many of these areas that you've identified, we get proposals all the time at National Science Foundation, and I think that if you went over and had a conversation with the engineering directorate, you might actually open doors where some good ideas could be co-supported or--

BUDNITZ: That's an excellent suggestion, just to say we've been talking to DARPA already as a project, we've been talking--you know, the Geological Survey has been part of the project all along, and they're going to certainly be part of this. That's an excellent suggestion which I'm sure we'll jump right on.

NELSON: Good.

LATANISION: Okay, we have Paul, Richard and Dan.

CRAIG: Paul Craig, Board.

I'm going to make a suggestion which I know won't fit in with your mode of operating, but it's okay. I've got to do it. And you will recall that several decades ago, you and I spent some time in this very building doing CONAES. Unknown terminology--

BUDNITZ: And the National Academy of Sciences Committee on Nuclear and Alternate Energy Systems, 1975 to '78.

CRAIG: Very good. Thank you. And in the course of that, it was brought up by a number of people that there were social issues which are important, too. And the nuclear area, especially the nuclear waste area, is one which is fairly polarized, and there are indeed social issues, and the way in which the Department of Energy responds is typically not so wonderful from the point of view of many, which includes folks on both the pro and the anti nuke side of the fence.

But, you explicitly didn't go in that direction, and I can understand why. My suggestion is that maybe you should rethink that decision.

BUDNITZ: Thank you. Actually, there's a story to tell here which I think you'll find positive. I made a presentation like this to the Academy last week, but three or four months ago at the Academy's Board on Radioactive Waste Management, three or four months ago, I made an earlier presentation which was very preliminary because I was only given a few weeks about science and technology and the philosophy, and I got three or four questions from them, social scientists, Jean Rosa, Howard Kernreuther, that shouldn't this be science and technology and social science. And the answer to that is this is science and technology. The office should be doing some of that stuff. But it's not necessarily under our science and technology rubric.

Now, there's been a followup. We've had discussions with a half a dozen social scientists, some of whom have drawn from the Academy's group, and some others, about what a program might look like if a social science program were put together that the office would support, and whether it will come under us, I don't know. I mean, that's an organizational thing. The first question is there something to do and is there something--and, yes, there are some lessons that are important that the social science community can offer, and yes, there's a need and it's recognized I think it's fair to say by the Director, Margaret Chu herself has endorsed the idea that there should be something there. But we have one particular problem I need to say, because it makes it difficult.

Many social scientists run in the door and say I want to do this thing or this thing, that we can't do, because the project will be or will appear to be as if we're doing it to manipulate public opinion, and we're not doing that. For example, a survey about how to communicate better could be seen as how to propagandize. A survey about how to change the Department's structure so as to make it--I don't mean a survey--a study so as to make a higher probability that it will be here for 100 years, you know, the long-term institutional question about whether there's going to be something here, you know, for 300 years is a question that social scientists worry about.

They're so desperate they're reorganizing the federal government. We have to be careful that when we do social science research, we avoid the pitfalls of either manipulating or appearing to, because that we're not going to do. It's not only not in our scope, but it's just--we're not going to do it. So, we have to find social science projects that pass that test, and a whole lot of them don't, and some of them do. So, we're in discussions now, I mean, it's only in the early stages, about doing something there, and I can't say how it's going to come out because we're just in the early discussions.

But, I can tell you there's a tremendous amount of need there, and I'm just looking over at the social scientists in the room here, Dan Metlay on your staff. Ten or eleven years ago, he was the staffer on a study that was done under the Department--the Secretary of Energy Advisory Board that Todd Laport chaired, which looked at the problems of trust and communication in EM, which is the Environmental Management Office, and in our office, and questions about, as you know, the department doesn't have a lot of trust in some quarters, and it's lost a lot, and how to regain it and what one should do to try to look at that. Well, that thing, I got it a couple months ago from Dan, I hadn't even known of its existence. Margaret Chu has seen it.

The lessons in 1992 are still as valid. All of you ought to go get that and read it. It's interesting. The lessons are just as valid today. The reason I didn't--so, there are recommendations there, Paul, that could be acted on today for work to do. And what we're going to do there I don't know.

CRAIG: And that report went nowhere, and I prefaced my remark by saying that I understood why this suggestion was not going to go anyplace, but I was going to make it anyway.

BUDNITZ: But I don't think your pessimism is necessarily warranted. We may, I can't speak because I don't know from the Director, we may do some stuff.

LATANISION: Again, with that lead in, let me take your comment next.

METLAY: Dan Metlay, Board Staff.

I'm not going to talk about social science. I will hazard a guess that even if you receive your $25 million for FY '04, the demand for money will be greater than the supply.

BUDNITZ: Yes.

METLAY: That's just a hazard guess.

BUDNITZ: That's for sure.

METLAY: So, then the question is, particularly if you're talking about institutionalizing this office within RW, what kinds of considerations do you anticipate being used to allocate this money, given that the demand will be greater than the supply?

BUDNITZ: Well, there are two things, really three, two at first. We're going to review the project proposals for their technical merit and for their relevance.

BUDNITZ: Okay. That's easy to say, not as easy to implement, but easy to say. And the third is we're going to have a mix as we started with. That is, even if we could fund all $25 million in robots, we're not going to do it. We're going to have a mix. And the reason we want to have a mix is we're going to start with a mix because we want to stimulate a whole community of people in Area Number 15 to say, gee, three of them got funded. Next year I want to be in there, too. So, we have a philosophy of starting that way in order to generate a community of participants who want to propose to us, get funded by us, and become a community supporting Yucca Mountain, or OCRWM generally.

So, the criteria are really three. Technical merit, relevance, and in the first round, we're going to have a mix, and that means that we're going to have to do some judgments on our side that are ultimately going to have to be--that's what the federal departments are there to do, is to make those calls.

PARIZEK: Parizek, Board.

Bob Budnitz in the candy shop. No, this is the enthusiasm--

BUDNITZ: You know, I'm from New England, and the expression is I haven't had so much fun since the pigs ate my baby brother. (Laughter.) And unless you're from the Brookshires, you don't know that expression.

PARIZEK: Since you brought it up, you didn't notice perhaps, but earlier there was a duck on the window sill. It was a Mallard duck, but it reminds me of the AFLAC insurance idea, and the program really does have potential for insuring or adding insurance to the whole DOE/Yucca Mountain project the way you visualize it, the way you're talking about, because in the time frame of finally getting a license, and so on, a lot of the points you raise here ought to strengthen this whole effort. So, the duck, although it was a Mallard, it serves the same purpose.

BUDNITZ: By the way, I would have used the word assurance rather than insurance, but go ahead.

PARIZEK: The question about the international, I think you mentioned that you're also going to encourage international effort.

BUDNITZ: Yes.

PARIZEK: But your one international person, although all of you six are pretty international people, but you had another person on who is not now on. How are you going to deal with the international part of this, or how to involve this? I mean, obviously all the topics you mentioned may or may not be relevant to other nations, and so on. But, how do you find--

BUDNITZ: Well, there are two ways. There are a dozen or so important programs in other countries that are like RW, and some of them not as mature, some of them quite mature, the Swedes and the Fins, for example. The directors of those programs know about us. We've made that point. And, so, if there's work there that they're doing or that their investigators are doing, then we hope that they will know about it and submit. How to reach a university professor in some funny place, I don't know, except just through the societies. We hope that we're going to announce this in all the usual trade press, as well as, you know, in the Federal Register. So, it will be in Physics Today and in C&E News and, you know, monthly, and so on. We don't know quite how to reach that wonderful idea in an institute in someplace that wouldn't be in the mainstream. But that's a challenge for us. But we are in contact with the main international groups that are in the repository business, and I think that's a nice start, and perhaps it will spread. You know, for somebody at some other place say, gee, I can get U.S. money to do something interesting, well, that's great.

PARIZEK: Do you visualize getting together maybe discussion groups to facilitate new idea development where you say, really, we're going to put up a few bucks to have a meeting and open up, and whoever wants to come to look at natural ventilation, or other analog examples--

BUDNITZ: Well, yes.

PARIZEK: Is that something you might do?

BUDNITZ: Yes. Actually, we were going to do one of those in the saturated zone, and we still may in another few months. We're having a meeting specifically with the Office of Science people about corrosion. But, yes, we've thought about having several of those, and we're not quite sure--and, by the way, they cost a few tens of thousands, and they could be of tremendous benefit. And we're not quite sure how many of those we'll do or what, but we have certainly thought about that, and a couple of them we're explicitly finding.

PARIZEK: I wish you luck, and thank you.

LATANISION: We'll take questions from David, and then we will take a break.

DIODATO: Thanks. Diodato, Staff.

Bob, thanks. You made one statement that I just couldn't let pass. You said license application is "proven technology." And from my perspective, I could make take issue with that, because I look around and I see many different aspects that are really at the cutting edge of scientific research, or engineering technology. For example, seismology.

BUDNITZ: That's fair.

DIODATO: Volcanism, hydrology, fractured unsaturated rocks, issues related to the engineered barrier system we've been talking about yesterday and today and will continue to talk about. So, I don't think you can necessary say that license application is based on proven technology at this time.

BUDNITZ: That's a fair comment. Without saying that that was overstated, proven means that it's sufficient for us to use it in the license application, and then it's for somebody else to decide whether or not that's okay, and you know who that somebody else is, it's the Nuclear Regulatory Commission and their staff and, you know, contractors. But, in some cases, it's proven enough for a license application, but it's not proven enough to use, and the project itself is going to develop that in the next two years or three. But, in some cases, what proven means is that the person doing the work knows himself or herself that there's something advanced that isn't in there, because it's just beyond what could be used. Maybe it's only a year beyond, in some cases, of course, it's twelve years beyond. But I think it's a fair comment.

LATANISION: Priscilla, how about if we take a ten minute break?

NELSON: Ten minutes.

LATANISION: Thank you.

(Whereupon, a brief recess was taken.)

NELSON: Okay, please take your seats. We're going to start the session. I want to thank you for coming back to the final technical session of this meeting of the Board. I'm Priscilla Nelson and I'm Chair of this session.

To begin, we'll have Gustavo Cragnolino, who will present corrosion research from the Center for Nuclear Waste Regulatory Analyses. Gustavo is a corrosion scientist at the CNWRA.

And, next we will have Andrew Remus, Yucca Mountain Project Coordinator for Inyo County, California, and he will introduce the hydrologic investigation program that has been begun by that group.

Mike King from the Hydrodynamics Group will describe the geophysical and hydrogeological investigations in more detail, including findings about potential groundwater flow through the Funeral Mountains into Death Valley.

And, next, Allan Rubin from Princeton University will present the final report of the Igneous Consequences Peer Review Panel, and we'll invite the Board consultants to make comments and ask questions, and leading into the discussion following that presentation.

And, regardless of what happens with the schedule, and I'll hold everybody to it, please be brief and to the point, questioners and presenters, because we will stop at 12:30 for the public comment time, as promised.

And those of you who want to make comment, please register with Linda Coultry or Linda Hiatt at the table outside the door in the back of the room back there on the left. And, as always, you're also welcome to submit your comments in writing for the record.

If you have questions that you'd like to have the Board pose to the presenters, please give them to one of the Lindas or directly to me, and we'll ask them if possible.

At the moment, we do not have too many speakers registered so that we have to consider rationing the time, but if there are many additional ones, we may have to do so.

So, without any further ado, I invite Gustavo to begin his presentation. Thank you.

CRAGNOLINO: Thank you very much. Good morning.

I would like to thank the Board for the opportunity to present part of our work on corrosion research by the Center for Nuclear Waste Regulatory Analyses.

I'd also like to acknowledge the work and contribution of my co-workers, D.S. Dunn, Y.M. Pan, O. Pensado, L. Yang, and V. Jain. And this, as you know, is work performed for the Nuclear Regulatory Commission. And I'm not going to read the disclaimer.

This work, as you know, is conducted in support of the Nuclear Regulatory Commission for the purpose of conducting independent research and providing technical assistance in the process of pre-licensing and license review for the application for Yucca Mountain Repository. And, for that, we use this overall approach. We try to identify risk significance of different corrosion processes, to provide input to performance assessment models and codes, to increase the confidence in conceptual and abstracted models for evaluating classes of materials, nickel, chromium, molybdenum alloys, through experimental research and modeling, and try to play very clearly the interplay that exists between environmental conditions and metallurgical condition of the materials that are important aspects related to corrosion modes and corrosion rates.

We evaluate natural, archeological, and industrial metal analogues to support the technical basis for these performance assessment models. And in many cases, also to provide a more complete understanding or support the mechanistic understanding of the processes.

And, finally, it's important for us to assess the adequacy of the DOE models, data and analyses for the predominant corrosion processes.

In this presentation today, I am going to focus only on experiments and modeling on the corrosion behavior of Alloy 22, even though that we have considered other materials that are part of the engineering barrier system and even corrosion of waste form or cladding.

This is an outline of my presentation. Briefly, I am going to try to describe for you our experimental results and mechanistic modeling of passive corrosion, localized corrosion, and I'm going to pay attention to the effects of welding and manufacturing processes, trying to make it clear for you the connection in between microstructural alteration and localized corrosion susceptibility, to end up with a brief description of our result on the stress corrosion cracking.

The foregoing slide is very important to show you the uniform passive corrosion behavior of Alloy 22. Each data point in this plot is an independent experiment in which at a given potential, we measure the current until the current density becomes stable with time after a period of approximately 48 hours, and we have this value of the current density over this range of potential. Current densities lower than 10^{-7} ampere per square centimeter up to this voltage here of 400 millivolts, indicate passive behavior.

One thing that you can realize from the data that is plotted here only for a one temperature up to 95 degrees C. is the fact that the passive current densities are almost independent of potential, chloride and pH. You see that we have a wide variation of concentration of chloride over a wide range of pH, and the current densities remain below 10^{-7}.

Only at potentials that are very high potentials, 400 to 600 millivolt in the Calomel scale, you have this process of transpassive dissolution that corresponds to the dissolution of the chromium oxide rich film to chromate, and corresponding increasing corrosion rate, these potentials are not usually attained under the conditions of the repository. And one important thing to emphasize in these types of alloys, you have no pitting corrosion that can be observed.

The effect of temperature is important, is important variable on the passive current density, and we have evaluated this by going up in temperature from 25 degrees C. to 95 and returning to that temperature as a function of time here at the very specific applied potential that is in the middle of the passive range that I showed before, conducting very careful experiments in nitrogen-deaelated solution to avoid interference of the cathodic reactions related with the presence of oxygen in the system that will remove impurities to avoid interference and have a true anodic current density measured in this type of test.

And you see here that the behavior of this material under passive conditions exhibited an arrhenius dependence on temperature, and this is the expression we can infer from that data and this apparent activation energy is relatively low, and is typical of ion-transfer processes through the electrochemical layer, double layer, in the surface of the metal.

How we go from here to what is useful parameters for assessing the behavior of the material in the long-term, the long-term extrapolation of passive corrosion? There are several assumptions. The dissolution is stoichiometrical and planar. The corrosion rate does not change with time if variables, such as the temperature, remain constant. But if the temperature decays with time, a fact that would happen in the repository, we can account for this by knowing the valuations of the dependence that we have presented before.

It's very important to be able to model this behavior, the passive behavior, and this is done with an approach that is at the frontier of corrosion science by using the Point Defect Model, and adapting this model for ternary alloys. The idea is that this passive film is based on the chromium oxide rich film with nickel chromium molybdenum as an interstitial cation are predominant charge carriers.

And the process of dissolution of the metal through this passive film leads to the formation of vacancies that are created by alloy dissolution and accumulated at the metal-film interface as a result of the fact that they have very low diffusivities in the metal lattice.

However, there are processes to consider that could impair in the long term the stability of the film, and these are listed here. Periodic spalling of the passive film, roughening of the corroding surface, and enhancement of corrosion rates by transient transpassivity. But this process, as I mentioned before, only takes place at very high potential, that in principle are not attainable.

The conclusion of this process of modeling can be shown here. Here, we have the comparison of the experimental data in this system with the potential in the middle of the passive range, at 95 degrees is the solution that simulates groundwater, with a content of low concentration of anionic species and this is the 95 percentile of the current density and just shows a lot of transience because this is a process of breakdown and repassivation of the passive film. The passive film is not a static structure. It's a sort of film that is desolving and forming, desolving and forming in a constantly repeating process. But the modeling indicates that our approach to modeling this process can be done, and we computed a decrease in terms of vacancy accumulation at the interface.

The passive current density decreases with time. You can measure this passive current density in potentiodynamic polarization tests. You need to wait until you get a steady state condition that corresponds to reaching this critical value of vacancy accumulation. And this is a very important consequence. From using Faraday's law, a fundamental law of electrochemistry and electrochemical corrosion, you can infer from the passive current density, using the equivalent weight of the alloy, Faraday constant and the density, a corrosion rate.

And to give you an idea of what corrosion rate we're talking about, one times ten to the minus eighth ampere per square centimeter is roughly 0.1 micrometers per year. This is the next slide, and my first back-up slide, Number 21, you have a more complete example of this.

We have a picture of what is called passive corrosion, and this is the behavior that is desirable for Alloy 22. However, this alloy is susceptible to localized corrosion. It's far more resistant than other alloys of the same family due to the addition of chromium that forms the passive film, and in particular, of molybdenum and tungsten.

And what is the approach that we use to measure this effect of the alloying elements on the behavior of the material in terms of localized corrosion, is to use a parameter that is called crevice corrosion repassivation potential. It's measured in short-term tests. However, we can consider, and we have demonstrated this in the paper that was published in Corrosion Journal in January of 2000 applied to a different alloy of the same system, Alloy 825, that this is really the lowest threshold potential for the long-term initiation of localized corrosion. And this is a powerful approach that you have by using this potential as a minimum potential for the occurrence of localized corrosion.

In order to do the localized corrosion testing of Alloy 22, we need to measure this potential, and we need to compare this potential with the corrosion potential. And here is an important concept. Localized corrosion can only occur if the corrosion potential is higher than the crevice corrosion repassivation potential.

You can think of this difference in between these parameters as the driving force. It's the driving force for localized corrosion. But, you have to be very careful. These are not thermodynamic quantities by any means. It's the driving force, and it's not comparable for, say, change of free energy, for example. These are kinetically controlled parameters that you measure, that you try to measure under a steady state conditions that are not equilibrium conditions.

It's a powerful approach, but has to be clearly considered for the way you measure this parameter is very important. And not thermodynamic quality depending upon the way that you get there.

This is measurement of corrosion potential. Corrosion potentials are measured in separate experiments, in air saturated solutions, because this really is a mixed potential. It's not at electrode potential, account for cathodic and anodic reactions taking place in the metal. The anodic reaction is the dissolution of the metal to form the passive film. The cathodic reaction in this case is the reaction of oxygen. And there's a significant difference depending upon the metal and the conditions. These are done with smooth specimens without crevice.

If you have an acidic system, and I have to emphasize that these data points reflect three specimen that are exposed simultaneously to the same solution in the same electrochemical site, and it gives you a range of variation. In acidic conditions, the variation, the variability, is much more narrow. But in alkaline conditions, it's much more broad. There are some data, which I don't have with me now, but it's in the paper that we recently published, that it's more relevant at pH of around 8 to 9, and with the variations in between this -150 and almost 100 millivolts in the Calomel scale for the thermally oxidized material. The material that was oxidized first in air and later on with post-dissolution.

And one important conclusion is the following. The corrosion potential is strongly dependent on solution pH, as you can see here, but it is slightly dependent on chloride concentration over a wide range of chloride concentrations.

This is done only over 60 days, but we have data for almost two or three years showing the evolution of this, and with the aging of the passive film, the corrosion potential didn't increase. However, as an example, for 4 molar chloride solution at 95 degrees C., after two years, the pH is 7, the corrosion potential reaches a value in this particular sample of -150. But, you have to consider always that you have a range of variation. Under the active dissolution, the metal shows a very well-defined corrosion potential. But where you have passive film, the phenomena are much more complex, and there's a lot of variability--intrinsic variability on the surface of the specimen.

Now we go to the next slide, in which what we have are localized corrosion of mill-annealed Alloy 22. This is a complicated slide in some ways, because we tried to bring the example of other alloys that have been considered by the project previously, Alloy 825, for example. And, we have here this parameter that I mentioned to you, the repassivation potential that we measured in separate experiments. Each data point corresponds to a separate experiment as a function of chloride concentration.

And this is a typical behavior of many methods. You have a region of practical independence with the chloride concentration until you renew a critical potential about which there is linear dependence in between the repassivation potential and the log of the chloride concentration.

And this plot shows that Alloy 22 in the mill annealed condition is quite resistant to localized corrosion, and obviously is a very good choice of material for the containers. What's not a good choice is 825. Even the attempt to use 625 didn't have too much margin. But, the case is completely different for Alloy 22.

I will have to tell you that here, we have two data points that are missing for the alloy and that display this dependence very well. These are in my Slide Number 22. You can compare them later on. These data correspond to a saturated solution of lithium chloride, a situation that probably is not attainable by any medium in the repository. These are close to the saturation of sodium chloride solution, and this is the strength that we are interested in. The two data points that I mentioned that are missing are here and there and, so, this common dependence that I mentioned before.

These are, by the way, data taken in autoclave and compared with data is a glass cell. This is the behavior of mill annealed material compared with the range of corrosion potential that I mentioned before, and with the range of corrosion potential in this case for a more acidic condition that probably is not prevailing in the repository, but it's interesting for you to have here.

In this region, obviously, 316 cannot be used, 825 cannot be used. 625 has very limited advantage with respect to 825. But Alloy 22 becomes pretty resistant, and you have only to get to very high chloride concentrations to produce localized corrosion.

But what happens in the next slide where we consider the effect of welding and fabrication processes? And this is very, very important, because this is a real condition that the materials are going to confront. Topological close--TCP--phases precipitate at grain boundaries in a few minutes at 800 to 900 degrees C.

Also, in the welds, you have what are called interdendritic regions that become rich in molybdenum and tungsten and depleted in nickel. Therefore, as a welded material, there are these TCP phases in the interdendritic regions, and these precipitates have high concentrations of molybdenum and tungsten. This is a contributing factor that we didn't discuss and analyze well, but it's relatively well known that cold work prior to forming and machine operation may increase the precipitation kinetics.

In Slide Number 25, you can check later on, I give additional information about the relevance of this type of problem, and the role that they play in the metallurgy of the material that is an important part.

To illustrate my point, let's go to the next slide, in which you see the grain boundary microstructure and the chemistry of this material only after five minutes at 870 degree C. We don't pretend that this material is going to be isothermally treated at 870 degrees C. for five minutes, but this is a process that naturally occurs when you're cooling from what is called the solution of annealing temperature, that is 1,100 weld. And dependent upon the section and the cooling rate, you can have even more than five minutes in a temperature response that goes from 900 to 800, in which this precipitation is very fast.

This is the probe scan, and this is a precipitate crossing through in a grain boundary, and you see the profile. Nickel is slightly depleted there, but this corresponds to a clear enrichment of molybdenum and a slight enrichment in tungsten. While iron is a completely minor element in this case, but more important, chrome maintains practically constant.

Aging at 870 degrees C. only for five minutes produces this type of thin film precipitate at grain boundaries that are molybdenum and tungsten rich.

We didn't detect any depletion of molybdenum, tungsten or chromium across the grain boundaries, but this is dependent upon the sensitivity of the technique. It's possible to have some depletion of molybdenum close to this enrichment in the precipitate, and this could be extremely detrimental from the point of view of the corrosion process.

We don't know yet if the process is associated with the precipitate per se or to this region that we cannot clearly detect here, and we need more sensitivity to find out.

In Slide Number 26, you have what's happened when you go for 30 minutes in order to demonstrate the importance of the phenomena, not because we believe that this is a potential situation, and in Slide Number 27, you have the composition of this phase. This particular phase is what is called P phase, very rich in molybdenum and in tungsten.

In the next slide, the important conclusion is shown in terms of the repassivation potential versus chloride concentrations representation, in which we have 31 points. I didn't want to put too many things in this slide, but by comparison with the life of the mill annealed material, for the mill annealed material, you will have a linear plot going in this region. And you can see, it's very obvious, that at 95 degrees C, the same testing temperature, the aged material has a significant decrease in the repassivation potential.

How do you interpret this? You interpret it in two ways. If we have a very low chloride concentration, let's say .1 molar, and we have the corrosion potential I showed you before, the material in the welded--in the aged condition--could be marginally resistant, but it's not resistant in the chloride concentration increase just above .1 molar.

Obviously, we have another problem. At even temperatures as low as 60 degrees, we can have a marginal resistance to localized corrosion of the aged material, and in less proportions with respect to the welded material. And we have more updated information of this type of results, but not in the conditions presented in these results here, because it has not been finally approved by the Nuclear Regulatory Commission.

In Slide Number 23, you can look for a comparison of the parameters that describe this linear relationship between the repassivation potential and the log of the chloride concentration. And I want to emphasize this dependence on the log of the chloride concentration, because this is a very well known fact in corrosion research, and there is theory and models to interpret this aspect of dependence. This is not something unique to Alloy 22 The only thing is that Alloy 22 shows this behavior displaced to higher chloride concentrations, and for that reason the material is more resistant than others.

Definitely, we can conclude that welding and short-term aging--and this is thermal annealing in our case, but this could result also from slow cooling, increases the localized corrosion susceptibility, and localized corrosion is observed at lower chloride and lower temperatures compared to the mill annealed condition.

What about the propagation rate? What about propagation rate of those localized corrosion processes? I'm going to introduce problem here. I'm going to introduce it because yesterday, I was thinking that people were talking about brief periods of hundreds of years. In this process, hundreds of years is not a brief period. The rates that we are talking about of this type of process are rates on the order of millimeter per years, 20 millimeters 20 years. For that reason, what we have to decide is if it is possible to have occurrence of localized corrosion or not.

Well, to illustrate this and make this thing less boring, let me show you a photo and a slide. This is the appearance of the attack. This is thermally treated material, very low concentrations, 95 degrees C. with a creviced sample, and in three of the 25 crevice sites, you have this type of intergranular attack, very deep intergranular attack. If you increase the concentration, you will see the attack, and the attack is obviously related to the precipitation of this phase that I mentioned.

I tried to paint until now a very blurry picture, but not bad news. There is good news. The good news is the effect of nitrate that was discussed at length yesterday. But, we have a different approach to discuss this. We tried to isolate variables, not to have all the variables bunched together. We isolate variables, and these are variables that

I isolate--nitrate, and what happens is that nitrate is a very efficient inhibitor of localized corrosion induced by chloride.

And, here we have a plot of the repassivation potential as a function of the nitrate to chloride concentration ratio. Here is a mistake. It's one order of magnitude lower, the value, as you can compare here in my plot, is .12 for the mill annealed material. I have repassivation potential in this range that compared to the corrosion potential of the material in the mill annealed condition, we consider this marginal, because it's a very concentrated sodium chloride solution.

However, the nitrate at this point, .12, we have two tests. One, we observe crevice corrosion, but at a very high repassivation potential, and another one in which there was no crevice corrosion at all, like in this case.

Now, go to the welded material that I showed you before that is more sensitive to localized corrosion. Obviously, we use a lower chloride concentration to be in a borderline type of situation, and we need to increase the concentration to .2. But, nevertheless, it's a very good nitrate to chloride ratio, and in this plot, there are two thoughts. One is the lower nitrate to chloride ratio that shows the repassivation potential you have seen before. Notice that I have a different scale here and a different scale here. You have to pull the two here to compare in a much more rapid way.

But, if we increase the ratio we have here, we don't observe localized corrosion. With an additional point that goes to the question that Dave Duquette asked yesterday. What's happening is you have the initiation of localized corrosion, and you have nitrate. Well, nitrate added after the corrosion initiation process takes place slows down the process, gives higher repassivation potential, and we can consider this as a pretty safe region.

There are fundamental reasons for the role of nitrate. There is ample literature on the issue related to competitive transport other--but the important thing from the point of view of the project is that the critical molar concentration of nitrate to chloride is very low. However, the question is this. Are we going to preserve for all the conditions this ratio? Well, depending upon the material and depending upon the environmental conditions.

Finally, very briefly, I'll go over stress corrosion cracking. We have to report that we didn't observe stress corrosion cracking in very severe types of tests using precracked compact tension specimens. And this is described for different conditions above and below the repassivation potential, because for 316 nuclear grade we demonstrate that the critical potential for stress corrosion cracking is related to the repassivation, crevice corrosion repassivation potential. That means that this is a very powerful tool for performance assessment goals. And we don't observe crack growth, even in the thermal aged condition, you can look later on in more detail in this plot, these are the conditions of the tests. We're monitoring in situ the crack growth using complex measurement, and in the last slide, I'm going to show you what happened with the thermally treated alloy in concentrated sodium chloride solutions.

We initiate the test, and the current increased associated with the grain boundary attack that we observed after the test. However, we don't have increase in the crack opening displacement that is an indication of crack growth. The experiment is interrupted here. We removed the sample to examine, then put it in again. The current increased again. This jump that you see here is an artifact, but the COD doesn't increase, and this is very clearly demonstrated by the constant value of what is called the compliance ratio. That means that even though that we have inter-granular effect in grain boundaries, we cannot propagate the crack in the form of stress corrosion cracks. And this is good news, but we need to do more experiments to confirm this type of preliminary observation.

In summary, I can say that we measured passive corrosion rates, and with the support of mechanistic modeling, we came to the conclusion that we can estimate container life well beyond the 10,000 year regulatory period.

The Alloy 22 is very resistant to pitting corrosion, but is susceptible to crevice corrosion in this chloride solution at temperatures above 60 degrees when this condition is fulfilled. But, it all depends upon the interplay in between these three important factors that are environmental factors. Therefore, it's very important that all these types of calculations can be available to evaluate how it's going to evolve, the environment in contact with the waste package.

And nitrate to chloride ratio is very favorable as an important factor to control the localized corrosion resistance, but it will depend upon the chloride concentration and temperature.

My main point of emphasis was this is because this is an engineering structure at the end. It has to be fabricated, and this problem has to be dealt with, and for the stress corrosion cracking, this is a main conclusion.

Thank you for your attention.

NELSON: Thank you very much, Gustavo.

We have an abbreviated period for questions. David and Ron.

DUQUETTE: Duquette, Board.

Gustavo, I presume you haven't done any corrosion tests on the TCP phases per se.

CRAGNOLINO: Not yet.

DUQUETTE: Because you've assumed the classical model of a depletion process adjacent to them, but there are lots of alloys where second phases which might appear to be corrosion resistant are not very corrosion resistant, and, so, it's quite possible that you're actually dissolving the TCP phases and not having an appreciable depletion of the grain boundaries.

CRAGNOLINO: Let me respond very briefly. It's a very good point. We'll try to do this eventually, you know, within the scope of what will become acceptable in our program. That is a very important fundamental point, because this could lead to an improvement in the condition of the material later on. You are right. We are exploring more than depletion around the particle that we include in defining depletion, thinking that this was an important factor, that molybdenum will decay enough not to play the same role for the bulk alloy.

DUQUETTE: I mean, you're well aware that molybdenum alloys can be corroded at very high rates.

CRAGNOLINO: Sure.

DUQUETTE: As an alloying element, it's very important. But as a primary phase, it may or may not be resistant to corrosion.

CRAGNOLINO: Sure. We've got to define better the range of potential. I agree with you.

NELSON: Ron?

LATANISION: Latanision, Board.

First, thank you for a very comprehensive summary of the work of the Center.

Let's turn to Slide 6. This slide shows the temperature dependence of the corrosion rate in aqueous solutions. And, obviously, when you exceed 95 degrees Centigrade, you're boiling and, therefore, you're not dealing with the same environmental, not a condensed phase.

But, what would be your sense of the question of what one might expect if the temperature were to exceed 95 degrees Centigrade? I mean, we're talking about temperatures that may approach 160, let's say, or in that range. How would you evaluate, or is it important from your perspective to evaluate the behavior of the package, the C-22, at temperatures that exceed 95 degrees Centigrade?

CRAGNOLINO: Yes. I want to correct something that was mentioned yesterday. The boiling point for solutions of this type is much higher. It's very well known, for instance, in the literature that the boiling point of concentrated magnesium chloride solutions is 150 degrees C.

LATANISION: Right.

CRAGNOLINO: That means you can have a liquid phase.

It's not that you have a salt that has been deposited and humidified. As soon as this forms a saturated solution and there's enough humidity to keep this saturated solution there, you have a liquid environment.

LATANISION: Right.

CRAGNOLINO: It's a localized liquid environment. We are reporting precisely at this present time, and continue to review for NRC, and reporting this information.

The only thing that I can tell you roughly, because I don't see any problem, is that the activation energy is the same. It's the same at temperatures up to 120 degrees, 125 degrees.

LATANISION: So, then you could calculate a range--

CRAGNOLINO: Yes. But we have to explore better the condition of the value for high Cl.

LATANISION: I see. Good, I'm glad to hear you're approaching that.

NELSON: Dan?

BULLEN: Bullen, Board.

Could we go to Slide 16, I think it is? And this is just a followup on a question that Dr. Duquette alluded to yesterday, that being you said you added the nitrate on the right side where the closed triangle is?

CRAGNOLINO: Yes.

BULLEN: So, you added nitrate after the initiation of crevice corrosion?

CRAGNOLINO: Yes.

BULLEN: Did you measure the conditions in the crevice itself, or is that just a bulk addition of nitrate? And, so, could you tell that the nitrate had an effect, I mean, obviously you had a change of potential, but could you tell that the nitrate had an effect in the crack itself, or in the localized corrosion area itself?

CRAGNOLINO: No, there's an inference in experimenting, and we add the nitrate after, and in the system we can stir very fast in order to make sure that we have the right homogenation of the solution. But, this is the bulk solution.

BULLEN: Okay.

CRAGNOLINO: It's striking, but we don't know at this point in time. I can find information to explore what is the depth of the attack at this particular point. But exploring is something that should be the subject of a separate type of investigation, and we use the geometry of the peak lead type of electrode to analyze these.

The theory behind the effect of nitrate is--there are two theories. One is the competitive transport. However, and this is a very intriguing thing, stainless steel, a less corrosion resistant material, needs much more higher nitrate to chloride ratio to become inhibitors--to our surprise a very low value. Now, it may be there is an interplay with electrochemical reaction taking place. We don't know.

BULLEN: Okay, thank you.

NELSON: Last question, Mike?

CORRADINI: Corradini. Can you go back two slides. I just want to get something clarified. You said it, and I listened to it as you were saying it, so I'm going to use colors since I'm not so clear. We've got the 60 degree age, which is the diamonds, green, and then when you went through the welded, the squished up diamonds, half filled, they move to the left and down, which is your, as I understand it, indication as to grading and its corrosion resistance. Am I correct in understanding that?

CRAGNOLINO: Yes.

CORRADINI: Okay. Then my question is then I've got the red, 95 aged, and it moves up to the 95 welded. I don't understand.

CRAGNOLINO: I don't understand either.

CORRADINI: So, that means when I welded it, it got better?

CRAGNOLINO: It's a good point. It's a good point.

CORRADINI: Am I misinterpreting?

CRAGNOLINO: No.

CORRADINI: Okay.

CRAGNOLINO: You are not misinterpreting. We pay much more attention to the trend here, apparently there is some improvement, I don't know how to call it, it's not so bad in welded material compared to a thermal aged material. But, in welded material, contrary to the thermal aged, there is much more variability in the measurement. This is a result that we're in the process of confirming. The trend in this direction looks okay. The trend in this other direction is something that worries us, because we don't have a good explanation. But, you have to realize that this is close to a marginal condition. When you have a marginal condition, there's repassivation, crevice corrosion repassivation measurements, have much more variability. This can be seen very clearly, for instance, in the Slide Number--for the mill annealed.

CORRADINI: It was back a few.

CRAGNOLINO: Back a few, yes. I'll have to get you a number.

CORRADINI: Ten.

CRAGNOLINO: Good, 10. 11. You see, in this range, there's a lot of variability. And this is because it's reaching a marginal condition for localized corrosion versus non-localized corrosion. We observe localized corrosion, but at very high potential.

CORRADINI: So, let me go back to--so, the reason that you have that cliff is why?

CRAGNOLINO: You're transitioning here from high potential in which the predominant process for the alloy, as I show in the plot of passive dissolution, is transpassive dissolution. That means that your oxide film that is originally chromium oxide, rich oxide film, becomes transformed, and Chromium-3 in the film becomes progressively converted in Chromium 6. I mean that this film changes properties. Therefore, the localized corrosion process that has to be initiated is initiated in a different type of surface that tends to propagate the attack much more shallow and much more extended regions.

And, in this condition, it's very difficult to define from a scientific a good repassivation potential.

CORRADINI: Okay.

CRAGNOLINO: So, it's not what you call really localized corrosion. It's a mixed process in which you have for one side, transpassive dissolution that is related to some form of localized corrosion.

CORRADINI: So, I have two follow-on questions, because I have to admit, since I'm not a corrosion expert, I see this data and I always want to think of a mechanism, and I'm not catching the physical mechanism. So, if I have the cliff, and I see it, and the presence of nitrate actually moves the cliff to the right--

CRAGNOLINO: Yes.

CORRADINI: --in other words, you have more of the upper shelf can exist at higher molar concentrations, or put it differently, can exist at higher temperatures. What does the nitrate do to stave off this behavior?

CRAGNOLINO: Displace this here.

CORRADINI: Why?

CRAGNOLINO: Because compete with chloride, to attack in localized spots the passive film, or the passive film is initially having this process of breakdown, and we have an embryo of a pit complete for the propagation of the attack.

CORRADINI: Okay.

CRAGNOLINO: And decrease chloride concentration, it's like you move to a situation that instead of having this real chloride concentration, is like having this chloride concentration.

CORRADINI: What is the length scale we're talking about? A micron, 10 microns, 100 microns?

CRAGNOLINO: The length is about--in a micropit. We are talking about far less than 1 micron.

CORRADINI: Okay.

CRAGNOLINO: When the pit is developed, it could be in the order of a few microns to 10 microns.

CORRADINI: What if I were able to lay down then a micron of nitrogen right where I want it, would that help?

Or is it the nitrate?

CRAGNOLINO: The nitrate. Well, the process from studies of localized corrosion, is more than a competitive transport between nitrate and chloride. And in this case, there is a competition for the arrival of more chloride to the localized site at the bottom of the pit.

CORRADINI: But if I were--so, one last thing, and then I'll stop. So, since we're thinking that there's a difficulty here and we want to, I hope we'd like to solve the difficulty, if I can lay down nitrogen at the location, would that hurt, help, or be indifferent?

CRAGNOLINO: Obviously, you can help, according to the results that have been shown.

CORRADINI: Fine. Thanks.

CRAGNOLINO: The point is this. What you call the location, the accepting plate, we have to provide a supply in the above condition, an excessive concentration of nitrate, that probably is going beyond what I mentioned in here, and this is a different story. For that reason, it's very important when I see, for example, this chloride to nitrate ratio, and they are telling me that the ratio is barely .1, I said I know something is--with my experiment, .1, .2 is great, but is it true in the real system? I'm not sure.

CORRADINI: Okay.

NELSON: Dan?

BULLEN: Bullen, Board. Just one last quick question.
Could you go to Slide 22? And just give us a brief explanation, I know it's--
NELSON: Five seconds.
BULLEN: --a backup slide, but you basically give us a nice temperature dependence for localized corrosion.
CRAGNOLINO: Right. In an attempt to go beyond the 100 degrees that is the boiling temperature of the dilute aqueous solution, we did this test in an autoclave system. It's not a real situation, but we want to know what was happening with our repassivation potential of Alloy 22 at temperatures above 100 degrees, and we did this in an autoclave system. And what we show very clearly here is there is a significant decrease in the repassivation potential from temperatures that go from 80 degrees to 105 to 120 degrees C. and then they tend to level off, and 4 molar produce localized corrosion of the mill annealed material at even 80 degrees, without any doubt, and the same for one molar.
But for .5 molar, we don't produce localized corrosion here, or the 95 degrees. This is the limit. But observing this regime, localized corrosion will take place at this low potential. Are they obtainable? Well, one thing that is missing in my presentation is that we didn't explore yet the effect of temperature and corrosion potential, as you realize. We have it all done at 95 degrees. Probably, we

are going to do this, not using an autoclave system in which there is a lot of data that has been used in the nuclear industry to evaluate that the corrosion potential decreases with temperature, with decreasing temperature, but with a system that uses saturated salt.

BULLEN: Thank you.

NELSON: Thank you, Gustavo. We'll have to move on to the next presentation, and I invite Andrew Remus from Inyo County. Is Dr. Bredehoeft going to talk as well?

REMUS: No, just Mike king.

NELSON: Okay. And Michael King from Hydrodynamics.

REMUS: Good afternoon. I'm Andrew Remus. I'm the project coordinator for the Inyo County, California Yucca Mountain Assessment Office. I'm here today with Mike King of the Hydrodynamics Group. Mike is the County's primary contractor for the Yucca Mountain Hydrology Program, and is in charge of both field operations and data analysis for this program.

Inyo County wants to express its great appreciation for the role that this Board plays in providing thorough and balanced oversight of the Yucca Mountain program, and we're very thankful for today's opportunity to speak.

I'm going to give a very brief sketch of the County's history with regards to its efforts to explore potential hydrologic connections between the Yucca Mountain project site and groundwater resources important to our county. Then I'll hand the presentation over to Mike, who will update you on our latest drilling project and our current thinking on regional groundwater.

Inyo County was designated a unit of local government by the Nuclear Waste Policy Act, and we became an effective unit in 1991. Beginning in 1996, we began to investigate spring discharge in the Death Valley region, finding that some of the spring waters in Death Valley National Park bore a strong resemblance to lower carbonate aquifer water, the lower part of that aquifer being a geologic formation extending below the Yucca Mountain site.

In 1998, Nye County included Inyo County in its hydrologic research program, and we were involved in a joint funding agreement for the years 1998, '99 and 2000. Under that agreement, we conducted further spring characterizations, geophysical research, and evapotranspiration measurements that provided further evidence that there could be geological continuity between the water supply to the national park and the saturated zone beneath Yucca Mountain. This three year study also provided inputs into the USGS regional groundwater model.

In 2001, the county applied to DOE for research funding, and in the spring of last year, DOE awarded the county a $5 million three year grant to construct five deep research and monitoring wells designed to locate the lower carbonate aquifer with respect to the Funeral Mountain Range, and with respect to the park's primary spring complex.

Through that project, we hope to characterize lower carbonate waters and provide inputs into the California side of the USGS regional groundwater model.

In coordination with the National Park Service, the county worked through the California Environmental Quality Act process that allowed the siting of a deep research well within the national park. And we then contracted with the Hydrodynamics Group and the U.S. Geological Survey to drill the first well, which has been completed within the last month.

The funding for this project is a combination of DOE grant funds, effective unit oversight funds, and National Park Service research funding. The current plans call for the construction of the next two wells before the end of the current federal fiscal year.

And, with that, I'll hand it over to Mike.

KING: Inyo County has two important factors or concerns that they're looking at. Obviously, the radionuclide transport through the lower carbonate aquifer, LCA, and the Death Valley spring system. In association with that concern, we're worried about the degradation of the upper gradient in the lower carbonate aquifer and how it may affect the spring flows in this situation in terms of this--the potential for inducing radioactive nuclide transport because of reduction in that head is an important factor. This is an update from the top we've done before the Board, and will present our new research.

We presented this slide before, which shows the proposed site for the nuclear waste disposal, and some potential groundwater flow paths which show water potentially getting into the Death Valley spring system. This is our projection of possibly through the lower carbonate system. Some of the other modeling by Zarnaki and others several years ago showed in the welded tuffs, or the tuff modeling, discharge into the Franklin Lake Playa area. So, there's some mixing of the waters coming between the two systems.

This is the geological framework for the area. The pink in here is the paleocarbonates, somewhat equivalent to the lower carbonated aquifer system, and these are the exposed rocks in the southern Funeral Mountain Range. To give you an idea, here's the Furnace Creek Ranch area. To locate yourself, I think of the Longstreet Casino is someplace over here. So, it will kind of give you an idea where we're at.

We're talking about three different areas here, and how we characterize those areas to develop our program, so we'll talk about A, which is the east side of the Funerals, B, which is in the Travertine Spring area where we just completed a well, and then our plans for studying Area C, which is the discharge, we want to determine the under flow from these springs into the Death Valley system.

We've conducted some 23 different geophysical survey lines on the east side of the Funeral Mountain Range. This zone through here is actually the exposure of the lower carbonate system in the southern Funeral Mountain Range. This is going out into the Pahrump/Amargosa Valley area. What we found are areas that we'd like to drill, which would be penetrating to the tertiary rocks into the lower carbonate. We have a site here, here, here, and then right at state line is another site where we find a high point. These high points are on the order of a couple thousand feet below ground surface, 2,000 to 3,000 feet. So, we had to find areas that we could penetrate. If we go out in these areas, we'd be drilling 6,000 feet wells. So, by characterizing flow through this system, we might find out how groundwater moves through this mountain range.

Our current plan is to drill two of these wells, I'm thinking this well and the one at state line, starting in August of this year.

This is the other map and a plan view. Again, our drilling locations are more or less along this area through here and here, and then this higher area along state line.

The state line fault system runs approximately through here. So, we're finally getting a pretty good characterization of what that carbonate surface looks like before we even drill.

Area B is in the Travertine Spring area. This map was taken from Machette. I'm sure he took from other people. But this is the Furnace Creek Mountain Range where the lower carbonates are exposed, the Furnace Creek Fault System. What's interesting are the Travertine and Texas Spring, and then up in this area would be the Nevada Springs, which is the major discharge of lower carbonate waters into the Death Valley system.

So, what we're trying to figure out in this area is how water moves from this mountain range through this system, and discharges into the spring system. In terms of Area C, we know that there's quite a bit of under flow under the springs, and so we're going to be evaluating the discharge on the alluvial fan areas to try and determine the total discharge through the mountain system. Then we can model it.

We drilled a single well here at the Travertine Spring well, and we'll look at that next. Again, a geological map of the area in a little more detail. Again, this is the Travertine Spring, which are discharging along a force fault system, which we don't know much about. There's an existing 250 foot USGS well, and we just completed this well to a depth of 1,300 feet. So, let's look at the profiles through that system.

Mike Machette did some seismic reflection surveys through the system, and he identified alluvial materials and then the Funeral formation, and then the Furnace Creek formation in this area. So, this was what he came up with the geophysics, and then we drill our well down to a depth of 1,300 feet. This well we matched up pretty well with his system. So, what we have are these conglomerate gravels, which are incredibly porous, very high transmisivities. We went through a stiff clay system, and then at the very top of the Furnace Creek formation, we had a gravel zone, which is in here. So, part of our interest is is how waters move through the Furnace Creek faults into these materials, and then discharging out here on the Travertine Spring system.

What we don't know is what's going on here, and one of our plans then would be to drill another well in this area so we have a complete profile through the system.

Hard to read, but this was the geophysical log on the well we just completed. That's that upper gravel zone we talked about, these lacustrine clays, and so we have an unconfined aquifer system up here with a hydrostatic head of 89 feet below ground surface, where the hydrostatic head in this confined bed was 84. So, we have a higher head, an upper gradient, which we've seen through a lot of the lower carbonate systems. So, this is basically the formations that we've run into. Below that depth, down to 1,300 feet, was basically clay with some minor ones of sand and gravel.

Our third area of interest is trying to figure out what's the under flow from the springs. These springs are high instrumented and we know what they're discharging. But that water system comes out and then discharges at this alluvial fan, and what you have is an exposure of a number of mesquite growths in this area. And, so, what we're trying to figure out is how to characterize that and do a water balance.

So, this was our first shot at it where we did a number of gravity lines through the system to try and figure out what the bedrock system looks like.

And like we showed on the east side, we have again this deep aquifer system. So, in this area, the depth to the lower carbonates are on the order of 6,000 feet, which again we suspected. What we don't see here is some of the fault range design.

This has a graben structure within that alluvial fan area, and that's going to be interesting because there's water coming into the system that hits that graben structure, and there's water being discharged into this deep basin. So, there might be quite a bit of fresh water out there that we don't even know about running into that system and supporting it.

This was the original conceptual hydraulic model framework that Chris Fridrich at the USGS put together, and the key here is he shows this somewhat of a dam or barrier to groundwater movement, and then he shows a number of different pathways that water can move through the system, through the mountain range. So, we were interested in the flow path to the north.

The wells that we were looking at drilling, we're looking at one here, we're looking at one here, and we just finished the Travertine well, which is in here, and here's the Furnace Creek fault system. So, you can kind of see we're trying to characterize movement from this side as well as the west side of the mountain range.

John Bredehoeft did a model of this which did present to the Board showing that it certainly is possible for a flow path through the system. What's real interesting is through these gaps, the head difference across there is only between 40 and 100 feet. So, if you drop the head on the east side of the Funerals some 40 or 100 feet, these springs may dry up.

Now, that's being reflected in the water level declines in response to pumping that we do over at Devil's Fault. So, we're looking at some dramatic changes if particularly Nye County or the Las Vegas Water District wanted to mine water out of the lower carbonate, it may have an impact.

Chris Fridrich just last week gave me--this is his new hydraulic model, framework model, for the area. In this case, he's not showing that dam system as being as prominent. There's obviously a disconnect between Naval Spring and the Travertine Spring in this area, but he's showing that maybe the flow path is a little more direct through the system as he's finishing up his field modeling.

We also see some modern seeps coming out through the system in here, and there's a feeling that there might be a clear boundary in the lower carbonate system in here which would bind the system, flowing the water into this direction. John Bredehoeft is modeling that material and he's going to present that next week at the Devil's Hole workshop.

So, what are the main issues of Inyo County? Well, again, we've talked about the lower carbonate and its flow path through the Southern Funeral. We see a path, we think it's real, we're going to characterize it with drilling on both the east side and the west side of the system, and then try and see how those heads work out and if the system works.

More important to us, though, in the near term is the maintenance of this upward gradient in the lower carbonate. This is certainly a barrier to radionuclide transport at Yucca Mountain, but what we also see in regard to that, this is a very fragile hydraulic system. Again, minor changes in head through that mountain range, and we've lost the primary water supply to Death Valley National Park and the tourist elements of that system. So, we're going to concentrate on that.

Here's the rest of our program. We're going to construct three more monitoring wells into the lower carbonate on the east side of the Funerals. Right now, we have funding for this year to complete two of those wells. And the target for that is to start drilling in probably August.

We're going to drill another lower carbonate well right along the Furnace Creek Fault. We want to see how those alluvial materials, how they hydraulically connect through that fault system. So, we're going to drill a 3,000 to 4,000 foot well in that system to find out. We think we need to drill another well at the Travertine Spring down gradient of it, or right at the spring so, again, we can get a better profile through the system.

And then the final element is we want to do this water balance analysis of the Furnace Creek area. That's going to involve some ET, evaporation studies. We're going to drill some monitoring wells and further geophysics.

Thank you.

NELSON: Thank you very much. It's great to hear about the progress in your program.

Questions from the Board? Dan?

BULLEN: Bullen, Board.

This is a question from a non-hydrologist, so you have to bear with me. If you could go to your I think it's your third slide, the one that has the map, that would be great. Yes, that one. You mentioned that a slight change in the hydraulic gradient sort of upstream could have a significant impact, and you mentioned pumping at Devil's Hole?

KING: No, there's no pumping--

BULLEN: Level changes at Devil's Hole?

KING: There was a period when there was excessive pumping, and there was a significant decline in water levels measured at Devil's Hole.

BULLEN: Right.

KING: Lawsuit comes in, they mandate a certain water level be maintained. They stop the pumping and the water levels rise, but not completely. So, what that indicates is that the system is very sensitive to any over drafting of the system in the Amargosa Farms area.

BULLEN: Bullen, Board.

I agree with that, but did you see effects of discharge at the springs in Death Valley from pumping at Devil's Hole?

KING: We don't have the data on that.

BULLEN: Okay. So, I guess I misinterpreted the fact that this is an awful long way away, and if you saw pumping at, you know, changes at Devil's Hole that had an impact on the discharge in Death Valley, I would have been real surprised.

KING: Our model shows, we have a regional model of the area, and that model shows about a 25 year lag between recharge in the Amargosa Valley and when we see that recharge in the Furnace Creek springs.

BULLEN: Okay, thank you.

NELSON: Do you plan on doing any age dating on the water?

KING: Yes, we are. We're going to run, again with the DOE program, and as a matter of fact, the whole program is YMP QAd, so we're going to do the major anion, cations, isotope series, including carbon dating. We have to be careful because of the carbonate waters and making sure they impact on the dating system.

NELSON: That was Nelson, Board. Richard?

PARIZEK: Parizek, Board.

On the alluvial fan discussion, you think that there could be some deep leakage that's not appearing in the springs and re-infiltrating as that water moves down onto the fan, because obviously there's a lot of recycling of some of that spring water, or at least there's a potential for that, I guess in the field.

KING: Well, they're using some of that water on the golf course and in those areas. So, that's part of the analysis. But we do see the mesquite growths which are being supported by that spring flow that's coming underneath the alluvial fan.

PARIZEK: This is way down by the Native reservation?

KING: It's in the tribal areas as well. But, also, that area has been historically an oasis type spring discharge area in the area of the ranch. So, you know, we think that's where the pot of gold is where the water is coming through the system. Obviously, part of their water balance is to find out what the infiltration from the golf course is going to be.

PARIZEK: It will be all the spring discharges, what you do know, plus the golf course re-used then additional water--

KING: Right, and then whatever the evapotranspiration accumulation there is, and then with the monitoring wells, we can see how much water might be passing through the system. So, with all of that data, then we might have a better handle on the total discharge from the spring, which then goes back into John's model to see how much water is flowing through the southern Funeral mountain range. I mean, right now, it's a black hole, so we need to figure it out, and so that's kind of what we've earmarked as maybe next year's studies.

NELSON: Dave Diodato?

DIODATO: Thanks, Mike. Diodato, Staff.

I appreciate your presentation today and you coming out. You made one remark that I wanted to address, in that you said you had a regional model and it indicated there's like a 25 year response time between recharge up around Amargosa Farms, and then response in the springs out in the Furnace Creek area. And as I recall last year, I just wanted to clarify that that's not a travel time number necessarily. With the discussions with John Bredehoeft last year about this time, he kind of indicated that's a pressure response that gets--

KING: I think you're right about that.

DIODATO: So, I just wanted to make sure there's no confusion about some incredibly rapid travel times.

KING: No, we don't know the time frame, and that's going to be part of our analysis is to figure travel time.

DIODATO: And then to follow up, I mean, you had this conclusion stated rather dramatically here about the 50 foot change in hydraulic head would impact Furnace Creek Springs and that's based on your understanding of the conceptual model, and some of the analyses you're doing. There's still some uncertainty in terms of the exact response and what the flow paths are at this time. Which parts of your analysis, you know, would you try to describe, and I guess the question would be describe the assumptions in that statement and then how your analysis is going to--it seems to me it's really designed to address that to firm up that conclusion a little bit; right?

KING: Well, if we look at the--I don't know the slide number--this was the one with the flow path and work model. Okay, what I'm talking about is the 50 foot head change across this area here. You may need a larger response out here to get a 50 foot head change across this path way. But if we do that, since there's only about 50 feet across here, then the water level on this side could drop below whatever our dam level was here in terms of elevation. At that point, then eventually the system will deplete itself.

DIODATO: I guess the point would be that this is kind of a conceptual model that you have right now, and it is somewhat interpretive; right?

KING: Right. That's why we're going--you know, what I do is we get some data, we model it, and then based on the results of that, we start seeing where best to put, for example, that told me we need to put a well up here. And then when we get that heading, we'll put it back into the model and then we'll revise that again.

DIODATO: Excellent. All right, thanks.

KING: So, we just keep getting closer.

DIODATO: Thanks.

NELSON: All right, thank you very much. We're in a time crisis and when you come back with results, you'll have ten extra minutes. It's a deal. Thank you very much.

Our final presentation today is going to be a report produced by the igneous consequences peer review panel.

RUBIN: I'm Allan Rubin. I'm a member of the igneous consequences peer review panel. The presentation this morning is pretty much as cannibalized from our final presentation that we gave in Las Vegas in May. One of the things this means is that I erred on the side of including too many, so I'm to go through some of these slides fairly quickly and determine to leave time for questions.

There were several chapters in the report and my presentation sort of followed along. I'll start with the introduction presented by our chairman, Anthony Pearson.

So, just by way of background, there were three volcanologist, of whom I am one, at least according to our chairman's classification. Some volcanologists may balk at that. But there are three geologically trained people here, one fluid mechanician, that's our chairman, one geomechanician, and our previous chairman was Bob Budnitz sitting here. He stepped down when he took his current position at DOE.

This is just a summary of the questions that the panel was asked at our first meeting in May just a year ago. I won't read this. These are very reasonable questions. Our initial role, our charge was to act in some sort of advisory capacity to critique and assess DOE's plans for investigating the volcanic hazard.

It quickly became clear to us that it would be more efficient in some cases for us to do our own calculations, so rather than imagining all the possible outcomes of some scenarios, we could do some calculations that would rule some out and seem very unlikely, we could rule those out and move on. So, much of our work involved our own calculations.

Our perception of the problem. The consequences of an igneous event are neither clear-cut nor readily quantifiable. All volcanic eruptions are different. There is no way you can sit down from first principles and compute your way from beginning to end of one of these things. And, obviously, with the TSPA is the crucial outcome of this operation. This is something that Larry Mastin continually reminded those of us who are, when we got too involved in our own calculations, he would pull us back to reality.

Okay, to our path concentrated on reducing uncertainty where possible. Again, this provided the motivation for doing our own calculations. There were five chapters, one is the introduction, five is the summary, but most of the meat are in the intermediate chapters. Chapter 2 sort of goes through the range of magma properties and eruption scenarios we have to think about. Chapter 3, much of the meat of what we did, the numerical modeling of dike propagation and interaction between a dike and the proposed repository. And, finally, trying to relate everything that came before to some sensible package that would be useful for people trying to make a TSPA. So, this is somewhat more holistic view and probably the most difficult of the bunch.

The other part of cannibalizing is that I was unable to paginate all these files without losing the formatting, so there will be a little step. Okay, so Chapter 2 presented by Frank Spera, the volcanology and magma properties.

So what do we expect? These expectations are drawn from either historical eruptions at the proper composition of magma, or geological investigations of the nearby surroundings. What we expect are eruptive volumes of about a hundredth of a cubic kilometer up to one cubic kilometer, and just for a way of thinking about it, even at this very small end, you're talking about something which is several times the total volume of the proposed repository.

Eruptive duration can last from days to months, possibly years. The eruptions can range from very gentle lava flows, to much more violent eruption columns with plume rates perhaps reaching 10 kilometers, certainly several kilometers are possible.

Eruption chronology, again, it's different from case to case, and there's no way of predicting the fraction of gentle flow versus large eruptive columns as a function of time in any one of these.

Here's some sort of illustrative scenario. The magma moves through the crust and cracks that we call dikes. These dikes are typically a couple meters wide and immediately they begin to freeze from eruptions. Most of the dike can be struck down in a period of hours to days. During the course of this eruption, at certain spots, the magma has mechanically eroded its walls, producing something of a more cylindrical conduit which because it's wider, harder stuff fluxes through and it can last without freezing basically as long as magma is available. So, that's the localizing, and you end up with something which is more of a rifle barrel than a crack.

Okay, some outstanding volcanological issues. We know the volumes of what we can see at the surface quite well. They've been dated so we know their ages to within the error bounds. Some questions in cases where there are a few closely spaced cinder cones with ages that overlap within error bounds. We don't really know if that's a single event or several closely spaced events. There are better dating techniques out there, and some program of more high resolution dating could resolve this issue.

There is also recent very aeromagnetic anomalies interpreted to reflect varied volcanics. We don't know their volume or their ages and, again, some program that would actually drill these and then date them could give us some information there. And that's also one of the panel's recommendations.

As far as the TSPA is concerned, probably the most important aspect of the magma itself is the volatile contents. When the magma is at great depth, all these volatiles, mostly water, are dissolved in the magma. As the magma comes up, pressure goes down. These volatiles can come out of solution perhaps often explosively. You can't just, to determine the volatile content, you can't go out and just pick up a piece of lava today, because it's lost most of its volatiles. So, geochemists have sophisticated techniques for trying to determine the water content. I won't go into that.

The end result is that the expectation is that typically the basalts in this region have somewhere between 2 1/2 and 4 weight per cent dissolved volatiles, mostly water, and the good news is that the thermodynamic behavior of water in these basalts is pretty well characterized by experiment, so we have a fairly good idea of how this water should behave.

I won't go through these. This is just to remind me that throughout Chapter 2 are a lot of compilations, or computations that Frank Spera made of relevant properties of this liquid and gas mixture that's relevant to our calculations. And in the report, we're dealing with something about 4 weight per cent dissolved stuff. And I think now we can move to the next chapter.

So, there are two slides that I'm skipping over at the end of that chapter, and when they're relevant to Number 3, I'll mention them.

Okay, so now here we're into the meat of the discussion of the propagating dike, which is how this whole thing starts at depth. This was presented by Emmanuel Detournay.

And here the cartoon supposedly is purporting to describe how a dike, a magma filled crack rises to the earth's crust. But before I talk about this cartoon, I have to make two points. One is that we can't really discuss this divorced from the model that's recently been proposed by Woods and others for the initial interaction of a propagating dike and a drift.

In their model, they start with a dike that's fully formed, it's a meter or two wide, at time zero it's intersected the drift, and the magma pressure is quite large, 10 megapascals more than the confining pressure. And at time zero, they take an imaginary baffle away and watch the phenomena as they unfold. And what they find is you've got shock waves and very large pressures.

It was the unanimous view of the panel that this initial condition is unrealistic for reasons that I'll get to in a second, unrealistic to the point that most of the conclusions of that study were not credible. And the reason is that you don't start with a dike fully formed. A dike grows from a very narrow crack at the tip, to something that ultimately becomes this one or two meter wide body.

The other point I wanted to make is that all these mechanical models of dike propagation are very complicated, rather esoteric, and I think probably justified only if you're interested in the first few seconds or maybe first few minutes of the interaction. If you're interested in longer term processes, you don't need to worry about models at the level of including everything that's happening at the dike tip. But, of course, because of this, Woods, et al. calculation, we are interested in what's happening early on.

So, what is relevant for the interaction with the repository? There is a crack, or an empty, at least not magma filled crack at the tip of this dike, something between the magma front and the crack tip, which we have called a tip cavity or a lag zone. It arises just because of the difficulty in trying to squeeze a viscus fluid into a crack whose thickness goes to zero at some point. You just can't do it. When you're above the water table, like the repository, this cavity is going to be filled by volatiles absolving from the magma, and we would like to know things like the pressure inside that cavity, how long it is. And the relevant point here is that for reasonable conditions, it's quite possible that an instability of the tip will arise, and actually the dike tip could have reached the surface before magma makes it to the repository. That's a reasonable but by no means guaranteed outcome.

Okay, so the tip cavity, again it's important because the first part of the dike to intersect the drifts, you'd like to have some estimate of the pressure there. There are really two independent constraints on the pressure in the cavity. One just comes from the fact that rock is very weak. If the gas pressure here was greater than the confining pressure, the tip would have propagated dynamically to the surface. So, that's one constraint. Basically, the pressure here has to be less than the confining pressure.

The other independent constraint you can just estimate from some sort of mass balance, magma running down the center of the dike is continually supplying these volatiles to the dike tip, but the host rock by the drifts is very porous, so gas is leaking out. And if you try to balance the flux coming in with the flux going out, what you find is that the pressures in the cavity are pretty low, again, because of the porosity of the walls, probably less than 1 megapascal.

If you take this number and you then plug it into the models and ask how long the cavity should be, you get something of the order of, say, 100 meters. If the confining pressure is something like what it is today, about 3 megapascals, 10 MPA is an estimate for the sort of peak thermal loading. In a couple thousand years after the repository opens, there you might be down to lags of a couple meters.

Okay, so what do these volatiles do? I should mention that when the panel started its work, this data be our calculations of dike propagation at incompressible magma inside the crack, and it very quickly became clear that the most important thing to add to these models was volatiles absolving from the magma as the dike rose, coming out of solution in the compressibility.

So, you need basically equations of the sort that it provided in Chapter 2. For example, what we have here is a function of pressure, the volume fraction of vapor going from close to zero when you're 100 megapascals, to 99.9 per cent by volume when you're at atmospheric pressure. So, you can take a curve like this, and the other important thing to point out, there's a match of numbers that volcanologists like to throw around of 70 per cent bubble fraction. This is taken to be the bubble fraction marking the transition between sort of a bubbly flow of liquid on the one side, and then gas flow with suspended particles on the other side at higher gas fractions.

So, could we go back one slide? So, if we try to plug that into a model like this where we have done a fairly good job is to put the absolving volatiles at a compressibility down well below the magma front here. So, in the model, that's handled fairly well. Where we have tried to do something that's very approximate and probably inadequate is what's going on here at the magma front. There's no longer a well defined demarcation between liquid and gas, and in fact we have an incompatibility here between the very low gas pressure, which can be maintained by the porous rock, and the high gas pressure of fragmentation which for these magmas can be 10 or 20 megapascals. So, we have done what we can, but our treating of this magma front here is not very precise. We don't expect it to change these conclusions by much.

So, all of that you can think of as think propagation in the absence of the repository, and now what does the repository do to this. Well, there are three main ways by which the repository can alter the dike propagation. It can alter the stress state seen by the dike so the dike can start to change its path. Once the dike--this will happen before intersection. Once the dike intersects the drifts, the drifts will act as sinks for the magma, so magma which could have been available, instead is now going into the drifts. And, finally, if you can open up another vent down a drift some distance from the parent dike, this could form a corridor for eruption, and this is something that everyone seems to be calling the dog-leg scenario.

In terms of the influence of the repository on dike via these stress perturbations, the most important one to mention--or the mechanical one, just the fact that there are these holes there is not terribly important. The dike is kilometers long and these holes are five meters wide, and they're spaced every 100 meters or so. But the most important interaction we think comes from the thermo-mechanical stresses, and the consequences of these large horizontal stresses may be reaching 10 megapascals or so.

The large confining pressure at the repository level by feeding back into the magma pressure will make the magma pressure near this magma front a little higher, make the initial interaction with the drifts a little bit more explosive, and maybe more important, it could reorient the dike, perhaps forming a sill either below the repository or along a bedding plane that could actually cut through the repository, and this is something that's difficult to quantify, but I think important to think about.

So, let me move to the recommendations. One is to do a better--continue these dike propagation models just to assure that our intuition which comes from partially including compressibility, but really not completely rigorously, is okay. So, actually include gas dynamics in the equations for magma flow inside the dike.

Another important question that we carried out some scoping calculations, but these are rather rudimentary, was to ask the question where is the magma at the time the drifts fill. And the conclusion from the scoping calculations is that the magma in the dike would be significantly above the drifts, but probably not so it's all the way to the surface by the time the drifts fill. This is very sensitive to the dike thickness, and that sort of thing.

The next was mine. It's this dog-leg scenario. Again, magma comes up the dike, runs down one or more drifts for hundreds of meters, and perhaps up a distant fracture, which is part of what TSPA is concerned, is that it's an important thing to think about. So, we started with some very basic mechanical considerations. In order to open up this distant fracture, the pressure of the flow in the drift at that point had to exceed the confining pressure trying to keep that fracture shut. And everything we do is basically aimed at assessing either the pressure of the flow in the drifts or the confining pressure keeping that crack shut.

So, factors contributing to this normal stress variation, you get, you know, potentially keeping distant fractures shut, topography, it's been pointed out that the ends of the drifts are at a shallower depth in the center, so lower confining pressure. Just some inherent variability in the rock, and many stress changes due to the dike, including tensile cracking and normal faulting. It was our judgment that these give rise to stress changes, they are pretty small, a couple megapascals.

Larger stress variations come from the drifts themselves. The biggest one would be the thermal stress, and the recommendation is that people undertake 3-D mechanical and thermoelastic modeling of these stresses. This is fairly straightforward stuff to do here.

A little less straightforward is trying to estimate the pressure in the drift. One general comment is that it can't exceed the pressure of the dike drift intersection. Beyond that, things get a little more complicated. It's going to depend upon whether the flow in the drifts is a rather gentle lava flow or an explosive pyroclastic flow, it depends on whether the dike is actually venting or propagating or blocked. So, when you put these together, you basically have four scenarios you have to walk through.

In the case of lava flows, once the drift fills, probably you're talking about hours. The pressure quickly equilibrates to that at the dike/drift intersection. And given the expected variability of stress along the length of the drift, it's very possible that the pressure at this distant fracture would exceed the normal stress across the fracture and at least give rise to the possibility of a dogleg.

This is a very important point to keep in mind. It is very difficult to start a dike in cold rock. The cracks are narrow, the flow velocity is very small, and I think this is almost an insurmountable difficulty in starting a liquid crack far from the parent dike. And because this is almost insurmountable, I'll skip this one.

Pyroclastic flows. Again, a different set of considerations come into play. I won't read this, but the conclusion is that if the dike is actively venting, you can probably get pressures of a few megapascals in the conduits. If this parent dike is blocked and you imagine that the entire force of this eruption is coming into the drifts, it still looks like the permeability of the host rock is large enough that the pressure doesn't get too high, more than maybe 5 or 6 mpa, before these drifts fill with pyroclastic material.

So, the conclusion with the lava flows is that getting their dogleg to work is very difficult. The conclusion with the pyroclastic flows is that, again in some qualitative sense, it's difficult to get a pyroclastic dogleg to work.

This brings me to this issue of hot versus cold design. I mentioned previously that in the hot design, the high confining pressure increases the magma pressure near the dike tip a little bit, and increases the explosivity of the initial interaction between the dike and the repository.

Given that we now think quite strongly that this initial shock wave is very unlikely, a more important consideration is what happens long after the dike tip has gone by and the drifts are filled. Now, there's no coupling between the magma pressure inside the drifts and the confining pressure in the host rock. What you like is a large confining pressure to try to clamp these potential secondary fractures shut, and in that case, large thermal stresses might actually help.

An additional benefit of the large thermal stress is that you might either deflect the dike, or even if you don't deflect the dike away from the repository, it may be thinner at the repository, and most of the stuff might come up a kilometer or so away. And, again, this potential pitfall is these large horizontal stresses increase the likelihood of magma coming up the dike, intruding on the bedding plane that cuts the repository, and then up to the surface.

Okay, recommendations. I've mentioned these I think. Can we go on to the next one? Something that's certainly beyond the expertise of the panel but we think is important is to now start coupling something like the Woods, et al. calculation, but now couple that to a real dike propagation calculation where you have 2-D or maybe 3-D numerical models of the rapidly degassing magma flowing into the drifts from a dike which grows from a narrow tip in the sense that we think is physically reasonable.

There are also some questions about the gas's ability to diffuse into the host rock, is it possible that pyroclastic material will clog this up, or something.

And, finally, a big picture is that, you know, me and several of the others have a very reductionist view and we've talked about the single bite from beginning to end, but what we really need to do is to work this into a TSPA so that people can consider other engineering options, such as backfill of the drift, orientations of the drift relative to expected orientation of the dike, that sort of thing.

So, now this is the TSPA, and Larry Mastin made his own slides. Again, the conceptual model based on historical analogues and mapping. Again, this is--he expects a few weight per cent volatiles, including a single cinder cone, days to a couple years, and these various eruption styles.

The bottom line on what we felt about the conceptual model underlying the current TSPA is that it's probably okay. It needs to think more about the dogleg scenario. Most of the assumptions that are made are either realistic to slightly conservative, was our view.

Different modes of waste transport to think about. The one that we are most concerned with is atmospheric dispersal. No one on the panel had any expertise in surface transport after the eruption, and had we had a groundwater specialist on the panel, we might have thought more about groundwater transport following an eruption, but that did not occupy us very much, partly because we also had no expertise on the interaction of the magma with the canisters to think about degradation of canisters in the drifts, even if they were not erupted.

These are the parameters. To quantify for the TSPA number of canisters entrained, percentage of waste in each canister that escapes, grain size distribution. This is important for the tephra dispersal.

This is just a summary of what the TSPA currently assumes. Going from the date to the conduit without any explanation of how you make that transition, again, qualitatively we think we understand, but there's no model that will do that. And basically, the number of canisters entrained will depend upon the number of conduits and their diameter and how many canisters they intersect. Number of conduits from analogues will be one to a few along a dike several kilometers long.

Estimated diameters, well, currently they're assuming a median I think of about 50. More work in the region may be able to give you better bounds on the distribution of conduit diameters. This is what we call the cookie cutter model. If you put this conduit diameter around and you ask how many canisters it intersects, plus a few to each side, you get about 16 canisters, and that's what's in the current TSPA.

On the other hand, if you imagine a dogleg scenario at a single drift, it may flow along for hundreds of meters, something like one order of magnitude more canisters for a single dogleg than a single conduit, and if you think you have more than one dogleg, you go up even higher.

So, again, ten times more canisters for a single dogleg. If you think that two doglegs are very unlikely, and even a single dogleg is at least ten times less likely, then we haven't changed the TSPA very much. That's just a basic statement.

Fraction of waste that escapes the canisters. Those canisters in the path of the eruption occurring, assumption is that 100 per cent of that material is vented. It's certainly conservative. It may be realistic. We don't really know. We had no expertise to talk about degradation of canisters. And I won't go through all this, but it's Larry's estimation of where--these are all important factors in the TSPA, and some estimate of our uncertainty. So, 10^{-2} means you could have a two order of magnitude variation in the estimated number of waste packages could go from 10 for a conduit to 1000 if you had ten doglegs. Fraction of waste in each that's entrained could go from 100 per cent down to maybe one. Grain size of entrained waste, again, we have no expertise.

And what appears to be the case is that those numbers in that table that have the largest uncertainty, grain size of the waste, things like that, how much of the waste in each canister is vented, that lies outside of our expertise. And that's what this last point is here.

So, the recommendations, the dogleg is best studied by numerical and theoretical treatment. That's what we've tried to initiate, but not as far as getting to the point of attaching probabilities. Waste escape and disaggregation, you could imagine doing experiments to understand better how these canisters behave in either a lava flow or a pyroclastic flow.

And that's it. Where am I timewise?

NELSON: Three minutes.

RUBIN: Out of 30? Okay, I think I will just stop because the last five slides are the summary slides. I think

I've mentioned them all. You can maybe while people are asking questions, you can read what those final recommendations were.

NELSON: My goodness. Nelson, Board. Thank you very much. I'd like to, before opening it up to the Board, open it up to the Board's consultant, Bill Melson, to pose some questions that he might have.

MELSON: I have questions and comments, Allan. First of all, I think your report, the panel's report, is incredibly comprehensive, and you wrestle with problems far beyond what I might have expected. So, I've been very pleased with it.

Some of the issues I think we could briefly bat back and forth a bit, First of all, going back into history a little bit, this need for magnetic anomaly studies and the clarification certainly revolves to the northeast in great detail, I think your panel recommended, and I would consider it a very high priority. So, clear up that little bit of a PVHA, and we need to decide also who is going to clear that up. In other words, once the data is there, there has to be reiteration of something like the PVHA.

The tip effect that you brought to the floor is an important part of things, and yet I think, as you would admit, there is still the possibility of shock waves under certain conditions, because it's a modeling study. Maybe you don't. But, in any case, Ed Gaffney's modeling of the effects using his knowledge of shock waves and modeling them from a wide variety of experience I think is really important.

RUBIN: Yes, I agree. Even if there are not shock waves, I think it's important to capture, for instance, you could imagine some scenarios where how this thing starts to fill, even if there are not shock waves, it's very important. I mean, how fast the stuff slams into the canisters.

MELSON: Well, from my experience, I've seen shock waves and Strombolian eruptions at the surface, and it's a different situation, but I'm still keeping that open as an issue in my own mind as to shock waves.

RUBIN: There was no one on the panel that had any shock wave expertise.

MELSON: Well, Megan has some, but she couldn't be here today. I could not agree more with you about the importance of the modeling of the actual canisters, and I'd say even the drift walls with the kind of phenomena that you are talking about. I think that is so important. And how we do that I don't know, but workshops of engineers, are the perfect people, and the volcanologists and yourself, I think would be very fruitful.

RUBIN: One of our meetings coincided with the waste canister meeting, and we kept guessing are we going to meet anybody and then the answer was no, we never did.

MELSON: I would just make a general comment that I think I see within the program many areas of unhealthy fragmentation of interests. We have a volcanology group and we have this group and we have that group, and I think the cost may be too high if the right experts don't get to have coffee with each other and bat ideas back and forth. That's a general comment.

I think also magma properties that Frank Spera and you talked about is extraordinary, and of all the issues that I hope we might lay to rest, we might like to believe that we have the that we have the properties of the magma defined well enough to put that one away. Maybe you don't agree. I don't know.

RUBIN: Sounds good to me. I think Frank Spera would probably have a better informed response in mind. But, I guess my sense is that just the inherent variability you can expect is greater than the uncertainty of any particular measurement you can do. And in that sense, I would agree.

MELSON: The other thing is that you mentioned the need for experimental studies. What would you have in mind that might help us understand, for example, dike propagation and drift interaction?

RUBIN: Dike propagation, again, I think natural variability is going to outweigh what you might learn from any similar experiment. Here's something that I learned.

There was one particular experiment was advocated, and that was the transition from the bubbly flow behind this magma front to the fragments that flow ahead. And the recommendation was that there are some labs out there that are already doing experiments somewhat related to this, or you may have different boundary conditions, but they're looking at the same phenomenon, and it may make a lot of sense to just go to those labs and try to interest them in working with a numerical modeler to say what kind of experiments would be useful for the numerical models.

Again, it's difficult in the course of 30 minutes to talk about all the assumptions and the uncertainties, but, for example, the statement that this tip might be unstable to the surface by the time the magma gets to the drift. That's definitely true in elastic grout, but there are faults here and we know that fault slip during intrusion events, and there are lots of things in the real world that may make that statement true, but not very meaningful. So, even more field studies designed to assess this dike/fault interaction could potentially be useful.

MELSON: I'm almost done, so bear with me for two more.

NELSON: You're fine.

MELSON: The rock permeability issue is something that I feel we need to define a term that maybe has not been defined yet that's very important for explosive volcanism. As your dike comes up and gas is being generated, and I'm not sure, and someone can correct me on this, but we need something about a dynamic permeability, an overload permeability. In other words, you mentioned how the magma can, or vapor can deposit things in block and lower the permeability. And I've seen too many violent explosions, craters, where I know pressure has existed very shallow, to personally believe that these things can bleed off fast enough to prevent volcanic explosion in many cases. And, so, your comment, you had that written down as one of your concerns, and I would certainly underscore that as something we need to really look into, is what is the effect of all this material flowing into--into, we have an equation, we can say it's open, it's going to rise at a certain rate. In reality, there may be blockage in the other parameters that do allow for build-up of pressures. Maybe I'm wrong about this.

RUBIN: I would caution about the analogy with the explosion you see at the surface. It's very difficult for the gas to diffuse through magma. So, if these are bubbles that are sort of interior, it's difficult to get from that bubble to the permeable rock, because you have to pass through relative--well, impermeable magma. We're talking about juxtaposition of this fragmented flow against bare rock. So, in my mind, it's the clogging of the pores that might be the thing to learn about.

MELSON: And the final thing is, and this I think all of us would be concerned about, is the terminology we're all using in terms of volcanology, like violent Strombolian, and I have heard, because this thing that Leon put on the web, I wrote about Strombolian eruptions and the terminology, I got a call from Van Hoeken, who is kind of like, you know, Mr. Terminology and many other things in volcanology, he was saying and in quoting me, that I do not know that, because he has a report he's writing for DOE on Strombolian eruptions, which will include lots of systematic data, the kind your panel dealt with, that he says will be far more detailed about some of the nomenclature issues. But, you're also substantive in this, will there be--what's the probability to have a single big event initially, and say ten big other ones within the next two weeks, based on analogies with known Strombolian history. And there isn't a whole lot of information, but he's trying to put that together to carry along. I was glad to hear that. But a small cone normally, you know, like Lathrop Wells, is not going to put up an eruption up to 10 kilometers. I mean, just the fallback alone would go something much more gigantic than Lathrop Wells cone. So, I think there's some tie to reality we all need to use. We're using these terms that can be potentially alarming. Do you have anything to say about that?

RUBIN: No. I'm all in favor of getting rid of terminology. I mean, it's more important to talk about eruption than the word. So, I mean, of what you said, I'll take the importance of the statement that 10 kilometer eruption columns are not consistent with what you see at Lathrop Wells. That's independent of what you call it, that's important.

MELSON: That's all I have.

NELSON: Nelson, Board.

Can we just ask a question about propagation of the cracks and the importance of rock mass modulus, and rock mass toughness, and differential toughness and modulus as its rising? Did you consider that because you can get--well, I don't think it would be a Bernoulli effect, but something as you change those properties, you can cause some things to happen, it seemed to me. Was that taken into account?

RUBIN: No. There was one recommendation amongst many others that I went over quite quickly that addressed the issue of rapidly varying stresses and the need to incorporate some of that into the numerical modeling. All of the numerical modeling we did assumed uniform properties, uniform modulus, uniform toughness, although as a general rule, we say that toughness of the scale doesn't matter because the rock is so weak. What could matter are the potential, you know, the bedding planes that offer alternate pathways, especially if this thing starts to come up one of the faults and then decides to move down a bedding plane. So, in a sense, that's toughness related. It gives you some sort of--

NELSON: Nelson, Board.

Maybe it will stay in the fault. Okay, Dan Bullen?

RUBIN: And there are examples from this area where dikes come up faults and then move along the bedding plane.

BULLEN: Bullen, Board.

Actually, I had a question on Chapter 3, Figure 3. I know that's a tough one to follow through. With respect to the stress state that's induced by the thermal loading, I'm actually intrigued by that, the calculations or the I guess assumptions that you came up with.

RUBIN: There's a 3-A and a 3-B. If it just says 3 at the beginning, then it's 3-A over there.

BULLEN: Okay.

RUBIN: Number 3?

BULLEN: Yeah, that's not the one. It's the one that has the 10 megapascals.

RUBIN: Oh, yes, I know which one it is. It will say 3-A.

NELSON: It's the sketch of the dike?

RUBIN: No, this is 3-B you're in, I think.

NELSON: Is it this one, Dan?

BULLEN: Yes, that's the one. That one. Where you have basically the 1 meter for 10 megapascals and the 100 meters for the 3 megapascal of pressure, and you commented that with the thermal loading, you get compressive stresses in the rock. And I guess the question that I have sort of harkens back to something we saw yesterday when Bo showed us a picture of the repository with 81 meter spacing, and how he can walk over and look at this. This is actually, you know, sort of to scale about what 81 meters looks like with a 5.5 meter diameter.

And, so, I guess the question is how far into the rock does that stress go from the thermal loading? It's only a few meters; right?

RUBIN: No, no. The mechanical stresses, in the absence of the thermal stresses, the mechanical stresses go something on the length scale of the diameter of the conduit. I have not done any of the thermal modeling, but my understanding is that after hundreds of years, it's almost as if you've put in a hot slab, that the thermal diffusion time is such that you can go 100 years, something like that.

BULLEN: Wow. Bullen, Board.

I actually have difficulty with that one, because the thermal pulse, even in the well defined thermal case, the temperature profile or temperature distribution from the center of the hot drift is cool enough to have water shedding, you know, below, you know, half the drift, half the pillar spacing. And, so, I'm trying to figure out what kind of dimensions I have for the stresses that are introduced in the rock, and I can't envision a much more than about a drift diameter, can I?

RUBIN: Again, I haven't done the modeling. I trust the people who trust the modelers. I don't know the modelers, but I know the people who know the modelers. I trust them. I trust the people that true the modelers, and to what you said, you have to fold in the thermal expansion of the rock and the modulus of the rock.

BULLEN: Right.

RUBIN: And you don't have to heat up rock very much in order to get large thermal stresses. So, the statement that you're below the boiling point of water between the drifts is not the same as saying that there's a small stress change there.

BULLEN: Okay. And the other thing that I'd like to show here is the heterogeneity in the system. And, so, if I have fractures or I have lithophysae or I have zones where there's no rock, then don't I induce fracture, don't I break it? Why do I continue to transmit that stress?

RUBIN: Well, the fact is, yeah, the fractures have to be closed or--when all these fractures are closed at some--they're open and they're touching, and when you increase the compression, you sort of increase the contact area. That statement is that we translate it into a repository scale, the effect of elastic modulus, and when I do these thermal calculations, they should be using--well, if you don't want to model every crack, you should be using a repository scale effective rock modulus. And if they're not, they should be.

NELSON: Mark Abkowitz?

ABKOWITZ: Abkowitz, Board.

Clearly, the focus of this work has been to better characterize igneous consequences. But, I was curious to what extent as a sidebar discussion the panel looked at the probabilities of different scenarios and ultimately, the risk being a combination of likelihood and consequence, I was curious if having finished this piece of work, you can say anything about whether you see the risks as being higher or lower than what you had anticipated going into the study, and also whether we have put any better bounds on the uncertainty in that assessment?

RUBIN: I guess--I think the second question is more realistic, and I'm going to answer it, but I honestly didn't know what to expect walking into this. Could you ask the second question again?

ABKOWITZ: Yes.

RUBIN: Personally, I am reasonably pessimistic regarding our ability to attach a probability to the dogleg. And I think you can, at least we believe that you can make the statement that it's the--the dogleg is the only thing we could think of that has the potential for drastically modifying the current TSPA. So, what you really need to do, what you really would like to do is attach a probability that you are comfortable with to the dogleg. And, in a sense, I'm happy that our year ended at the point that it ended. It absolved me of the responsibility of trying to attach that number, because I don't think I could.

I tended to be more open to this being able to happen and that being able to happen than the other members of the panel. I mean, there was sort of a geological training, engineering training, divided in how complicated we viewed the real world. The consensus of the panel, and this is a true statement, is that the probability of one dogleg is low enough that it basically more than counteracts the increased number of canisters. Is that a statement you would be very happy with? I don't know. And that's this 10 per cent number. A single dogleg reasonably produces or impacts, puts ten times the number of canisters in the drift. That's a statement that we're fairly comfortable with.

ABKOWITZ: Abkowitz, Board.

Can I then reach a conclusion based on that statement that the entire igneous activity is really nothing to be concerned about in terms of the safety of the repository operation?

RUBIN: You mean assuming it is true that the probability of this dogleg is less than 10 per cent, so it doesn't affect the current TSPA, assuming that statement is true, was that--

ABKOWITZ: I'm reacting to the comment you made that you think the probability of the dogleg is sufficiently low that with the repository design as it is, you didn't foresee that as being problematic. So, I'm just saying that if that's the consensus of the panel, would it not follow that one could come to the conclusion that there are no scenarios of high enough probability to suggest that we have to be worried about igneous events disturbing the safety of the repository?

RUBIN: I guess I would caution against blindly following the consensus of the panel. Maybe consensus is the wrong word. I mean, I may be the one, I don't know, I'm certainly less likely than the average panel member to be fully comfortable with that comment. Maybe better than where the consensus would be the mean panel judgment. Yes, the likelihood of the dogleg is low enough that it probably doesn't affect the current TSPA much, although the final--I mean, the reason I was happy signing onto the final report is that in the recommendations, the final recommendations, the slides that I didn't cover, was--it's in Chapter 5 and it's Slide Number 4, the ones before the last, yes, neither the probability of a dogleg flowing, nor its nature, has been quantified so far. And there's a recommendation that more work be done to try to resolve this. Not withstanding my pessimism that it's possible to resolve this, there are certainly calculations you could do which would move you in that direction.

And then this is the mean panel estimate, is that once you do this and you come up with an estimate, you attach a probability, it's unlikely that it will change the TSPA, but without this recommendation, I would be completely uncomfortable with this one. I'm speaking for myself now. I'm not confident enough to say that the probability is less than 10 per cent, plus the smaller probabilities that more than one conduit is affected. So, I'm hedging.

But if you accept our number, and if some day people decide that the probability of a dogleg is less than 10 per cent per single one and far less for multiple ones, then yes, there's nothing the panel could imagine, or nothing that the panel did imagine and we tried, that would have a larger effect on the TSPA. That is true.

REITER: Allan, what about the sill?

RUBIN: Well, it's just a longer dogleg.

NELSON: We've only got four minutes left until our promised public comment period.

RUBIN: Okay. So, let me give a quick answer for this one. But the one thing that--the worst possible scenario that I could imagine was that they come up--they didn't dip it to the east, but it comes up to a level above the drifts, goes down a bedding plane and cuts through the drifts, and up some distant fracture, which these things sometimes happen, and that way there is actually a path through essentially all the drifts that you have multiple doglegs.

Now, what probability do you attach? It doesn't work if it comes the other way. It doesn't work if it goes up like this and then goes up, because it only cuts through these things once and it's just like a dike cutting through, so what's the chance that the dike would actually turn to--do it in that direction, go up above, down below, and then up above again, attach a probability to that.

Now, one thing you could do to avoid that is to backfill. You could--I don't know, somebody said you could design the drifts so that they lie totally within a single unit and are not cut by the geologic boundaries. I don't know how feasible that is. But that's always another option, and if you decide that that particular scenario or the dogleg scenario has too large an effect on the TSPA, you could do that next.

NELSON: Okay, Boss, what do you want to do with that? Thure, yourself, and Richard.

CERLING: Yes, if we could go back to the previous 33, I think it was, the one that was on just before this. 3-A-3. That one. I was just wondering, the leakiness of the gas into the rock is because the rock is quite porous. But a lot of the pores are already filled with water, so I was just wondering sort of what water content the pores are leaking, basically.

RUBIN: Yes, again, what you really care about is an effective drift scale permeability. I honestly don't--I mean, I think there have been some experiments on this, but I don't know, and I don't know if they use water or gas, and it could matter, because of this partial filling. We used 10^{-12}. I'm not sure if that isn't darcies. 10^{-12} meters squared per second. The numbers are in here. There's something goes in the square root of that, so, I mean, you could go through--walk through the calculations again. But, yes, what you really want is an effective permeability at the drift scale.

Now, a general comment is that as you move up in scale, the permeability tends to go down because of the importance of the large scale fractures. So, I think--we thought it was conservative to take the numbers we were given, and I think these were meter scale measurements, but I could be wrong about that.

CORRADINI: Can I stay with this one? It's a nice slide. So explain to me why you care about the transition as a function of void fraction? Because there's been a number of tests at Argonne Labs and Sandia Labs in a totally different material system, which is ceramic, so it's calcic, silica and stuff, and they see essentially what I would call a beer foam on to, particularly in seletious concretes. So, wouldn't you expect a beer foam effect here? Because when you get up to about 66 per cent void fraction, you're going to essentially foam up. So, is that not the real physical flow regime as you increase bubble volume?

RUBIN: That's part of it. But then there's also the question of are dynamics important to that, you know, the equations you use in typical dike flow calculations, you assume that the pressure gradients are balanced by viscosity. So, the sum of force equals zero.

CORRADINI: Right.

RUBIN: The conduit flow calculations that volcanologists do when they have an existing conduit, say F equals MA. They worry about acceleration of material at this transition. You have to use F equals MA in your equations, not F equals zero.

CORRADINI: When I uncork a bottle of beer, the foam flow out.

RUBIN: Yes.

CORRADINI: So, it seems you would have both happening simultaneously, a dynamic flow of a leading edge of foam with a water level, liquid level behind it. And that leads me to the question Thure is asking about how the gas gets in. It seems to me where the gas is going to most get in is where its surface area or where the liquid surface area is maximized, which would be in this upper region, rather than in the bottom region where I have liquid. And I have a hard time figuring out how the gas would get into the sides.

RUBIN: The diffusion is only occurring up here where there is no, at least in this cartoon, no magma yet.

CORRADINI: Okay.

RUBIN: The reason that this region is important is that the only thing--yes, all the mass transfer or the differential flow velocities are occurring in this complicated region here, but the only reason that fresh gas gets here is because the magma in the center of the dike is moving faster than the magma.

CORRADINI: Right.

RUBIN: So, another way of saying what we really lack in our modeling is a good constituent of law for this region. That's what we need, is a good constituent of law, whatever you want to call it, we need a better constituent of law.

NELSON: As has been pointed out, Richard got cut off last session. If you would like to take a short period of time, I'm willing to take the grief of our public commenters.

PARIZEK: I offered to be on after the public if anybody had to leave. But, just kind of a quick point now. The work that your panel did has to be, you know, a lot of attention was given here. The question is you do narrow down uncertainties by the work that was done. The TSPA calculations assume different kind of combinations of packages being disrupted, and it's still a pretty hard thing to constrain. If I was a program manager, I'd have to ask myself do we give you more money or encourage your program to go into these research angles to narrow down your uncertainty, or should I just bite the bullet and do an engineering fix, because your panel also recommended opportunities for backfill, as an example. And the minute you do that, particularly if you drill and find a few of these aeromag anomalies are in fact volcanic, worse than that, they're younger, how many can you tolerate being younger and being volcanic before that gets a new trouble for the program.

But then you could to an engineering fix, and if you did that, is that the solution to the problem? You just bite the bullet. And if you do that, then the program has other problems because that creates a whole environment change in terms of what the--you have rock falls, you have a lot of other problems.

RUBIN: Yes, I mean, my response is--I feel like I have to say that if I answer this question, it's really me speaking and not the panel.

PARIZEK: I don't want to put you on the spot.

RUBIN: And I tend to be more pessimistic I think that the panel member. I am pessimistic that we'll be able to put a--I'm not pessimistic from an attached probability, I'm pessimistic that you could attach a probability that you could defend rigorously. And for that reason, am I answering your question? Well, no, it's not really worth throwing a lot more money at this.

On the other hand, I am also open to the possibility that that statement of mine is wrong, and that there's things you can--there are actually things you could do that would settle some of these issues. And just as an example, this whole thermal depth issue of the distant fracture, it's something that just sort of popped up in the middle of our work, and the day before it popped up, it wasn't there, and the day after, I really feel like we've eliminated a large category of scenarios.

So, there's some advantage to continuing with the work, even if you don't really see the clear path to the more reliable TSPA. So, I'm going to have to weasel again.

PARIZEK: I asked in Japan why volcanoes in Japan aren't a problem to their waste isolation program. They said, well, volcanoes--and we'll just stay away from them. Why can't you stay away from volcanoes in this site? Is there something different now geologically going on there that would say, well, look, you've got the ones you've got. You're not going to get any new ones where they're not now present, or you can't say that.

RUBIN: No, I mean, the previous estimate for the probability based on the information they had is probably quite reasonable, and it may be higher, some of these various anomalies are young ones. There's a question of why couldn't you--I see in there that the probability matched. There's a bull's eye which is essentially not too far from Yucca Mountain, and on those maps, if you go a little bit farther, you're down one or maybe two orders of magnitude in probability. And as to why the repository couldn't be there, I think you'll have to ask other people.

PARIZEK: We can't move the repository. I'm just saying if you don't have any hits at the repository now, why would you expect any in the future, that you'd catch any new eruptions at the repository location in the future? You have no volcanic activities at the repository today.

RUBIN: That's right.

PARIZEK: Therefore, why would you expect any in the future?

RUBIN: Well, because these are all one shot deals, and they come up and they're active for a few months, and the next one happens a few hundred thousand years later somewhere else. So, the mantle beneath Nevada and all of the basin and range is hot, and there's stuff down there and it's waiting to come up. It just comes up very, very infrequently, and it produces a little blip, which is here today and gone tomorrow.

NELSON: Okay, we're going to have to conclude the session right now. Thank you very much, Allan.

CORRADINI: Thank you very much. I think some people of the public, or the present public may have questions of you, so don't go far away.

I've been told we have one member of the public, John Kessler from EPRI. John? And another one writing down a question furiously for Professor Rubin.

KESSLER: John Kessler, EPRI, and this doesn't have to do with volcanism. I just want to bring you back to the discussion yesterday. First of all, I want to say thank you to the Board and to the Board Staff for again a good meeting, getting some new issues out from DOE, getting them in front of you, asking a lot of good questions. Certainly, the more these issues are aired, the sooner the better off we'll all be in the long run.

On the hot versus cold issue, I think I'd like to address one of the comments that Professor Latanision made yesterday, which was he was saying, well, what we're all concerned about is long-term dose, and I agree. That is one of the things that we all need to consider, is long-term dose.

However, my concern is it seems to be the only thing the Board is concerned about, is long-term dose. And I want to sort of reiterate one of the things you heard from DOE yesterday, which is that to put the hot versus cold issue in full perspective, I would encourage the Board to have a more holistic view of it. Look at the preclosure implications of hot versus cold, as well as what I think you're doing a great job on now, which is the postclosure implications of hot versus cold.

I think that if the Board were to make any additional recommendations along the lines of hot versus cold, I think they would be stronger if you had the background or a more full consideration of preclosure versus postclosure issues than what I view you having currently now.

That's all I wanted to say. Thanks.

CORRADINI: Thank you very much. Dr. Budnitz I am told has a comment.

BUDNITZ: Actually, it's a question for Allan Rubin. I know just enough about volcanism to be dangerous because I chaired that panel for the first six months through the interim report, and then I had to step down, as he said. So, I read the final report eagerly, and was in on the discussions for the first half, so I can ask an intelligent question, to which I don't have an answer, and maybe you don't either.

I scoured the final report trying to see if I could find an answer to this question, but was unsuccessful, and that is what causes the significant doses in the environment and to the public after a volcanic event goes significantly into the air rather than just on the surface and then goes somewhere where you get doses? And the scenario in the TSPA now is a single cylinder that comes up and intersects, as you said, a dozen or so waste packages, and off it goes. The dogleg would intersect ten times as much you said, roughly, and off it goes.

But, I didn't see you address the probability that if you get the dogleg scenario, then what comes out over there is violent like that rather than just dribbles on the surface, or maybe even stops. Because it's only the violent ones after the dogleg that could produce the doses that are of concern, or the impacts on the public, rather than just, you know, goes a little bit and it dribbles around and becomes something rather small, just because of the features.

You didn't seem to address that, and I was, I won't say disappointed, because I know all the work you did do, and I just wondered if you had a comment about what further work could be done to get our arms around that part of the overall scenario, because that's the one that would lead to the big doses?

RUBIN: Can we go back to a slide? 3-B. And I can tell you in a minute. Yes, Slide Number 6. We did address this at some level, but I'm not surprised you didn't find it because there's a lot of stuff buried in the report.

So, again, we separate lava flows on the one hand and pyroclastic flows on the other. And the lava flows are the gentle ones. So, the comment here that I didn't go into because I said it's difficult to get past this one, the way to get the highest possible pressure inside the drift is really to have a standing column of gas free magma all the way from the drift to the surface.

So, in a sense, if you have a lava flow that's going to come up, there are two possibilities, that it's mostly contiguous magma down there, but there's still enough gas that by the time it gets out, it will be explosive, or it's contiguous magma down there and it's degassed to the point where even after it comes to atmospheric pressure it's still not going to be violent.

And the way to get the largest--probably the highest pressure down below is if in fact this is a gas free column of magma extending all the way to the surface, which implies probably it's gas free in the drift. And, in this case, a lava flow that went up is likely to be effusive with material entering the biosphere. So, then you go to the pyroclastic flow side of things, which I guess is the next slide, and then you just want to go through these two different scenarios, is the main dike open, is the main dike blocked, and you can attach some probability to that. But if you end up attaching some probability to that, this would be the violent one.

And both of these are unlikely. Are they unlikely at the 10 per cent level or the 20 per cent level or the 2 per cent level? I'm not prepared to say.

The other comment I would make, I don't know if this is what you implied, but we were told in this May meeting is that it's not actually the people who are walking along on the ground as it's erupting that are at risk. It is the temper dispersal that's the ultimate source, but it's in the ground, the surface transport in the years following that, and if you're living 10,000 years from now, it's not the eruption that year, but the integrated effect of the prior eruptions that you have to worry about. But, still, it comes up in the lava flow. It's not going to get into the biosphere quickly.

CORRADINI: I think that's the end--oh, I'm sorry. You'll have to identify yourself.

O'DELL: I'm Dick O'Dell from NRC.

I've taken part in the total system performance assessment studies that NRC and the Center have done, and actually I just wanted to correct your last statement, Allan, that most of the risk is from the initial atmospheric dispersal and inhalation, rather than long-term dose further on like through groundwater pathways or ingested in food stuffs.

RUBIN: That's fine.

RUBIN: I don't think it has changed.

CORRADINI: Okay, I think we end with our public comments. I guess I wanted to thank everyone, and I'll start off with the office, the Yucca Mountain Project Office. I think yesterday and today was a good group of individuals. And I'd like to thank also CNWRA and the staff for putting together the program, and thank everybody for being here. This ends our open meeting, and we'll see you later in September.

(Whereupon, the meeting was adjourned.)