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SCIENTIFIC AND TECHNICAL ISSUES

Wednesday, May 9, 2001

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P R O C E E D I N G S

(8:00 a.m.)

COHON: Good morning. May I ask you to take your seats, please? We're about to get started.

It's my pleasure to welcome you back to the second day of our meeting. Jeff Wong, Board Member, will be the Chair. Jeff?

WONG: Thanks, Jerry. Good morning, everyone. It's my pleasure today to be the Chair. Again, as Jerry said, I'm a member of the Nuclear Waste Technical Review Board and again I'll be serving as Chair for today's session. I expect the session to be full of information, but unlike yesterday, it won't be as long.

Today, you will be hearing first from Mark Peters who will update the Board as to what happened since January in the scientific and technical investigations by the DOE. And, later on, we will be hearing from Narasi Sridhar from the Center for Nuclear Waste Regulatory Analysis and he will be describing the waste package studies sponsored at his organization by the NRC.

So, I'd like to have Mark Peters come up. I need to get off before Dr. Bullen starts asking me questions.

PETERS: Good morning. Everybody hear me okay? I made up my own question that I'm here to answer. It is can I actually make it through this big pile of paper? I think the

answer is yes. A lot of pictures, there is a lot of stuff; believe it or not, it could have been even longer than this. We have so much going on, I pared it back which is kind of a scary thought.

Similar presentations to what I've given you all over the past several meetings now; providing a status on the overall scientific and engineering testing program. I'll march through it in a very similar order; unsaturated zone--yes, that study is first--cross drift studies next, some things like ^{36}Cl and bulkhead investigations that I know are of a lot of interest to the Board, a short update on what's going on in the field at Busted Butte, and then some discussion of saturated zone work in cooperation with the Nye County drilling program and some early data from some of the single hole tracer testing at the alluvial testing complex.

Then, moving into the engineered barrier, some results from Phase 1 of the ventilation tests that you heard about last meeting and also a status on the Phase 2, that test which was started now at the Atlas facility. Something the Board has not heard about much in the past from me, we're starting a program to look at thermal properties, thermal conductivity, in particular, and there's a field program that's in the process of being fielded and I'll talk about that briefly. Then, several slides on materials testing focusing there on highlights of what we found. Obviously, I

can't do justice to all the work that's being on in materials in the short time that I have. And then, wrap up with a summary.

Starting with the unsaturated zone and the exploratory studies facility, a diagram that the Board has seen before, the ESF with the potential repository block here to the west, north in this direction. Again, I will talk about several slides on the drift scale test in Alcove 5 and then discuss the progress on ^{36}Cl validation and then move into the cross drift and I won't dwell on the locations in the cross drift. I have another slide later that I can point out what areas I'm going to discuss.

Starting with the drift scale test, everybody is familiar with the layout of the drift scale test and observation drift with the connecting drift and the heated drift here; approximately, a 50 yard long heated drift with nine canister heaters and 25 wing heaters on each side. Boreholes drilled both above and below the drift, as well as within the drift.

Total power and temperature, we've turned down the power according to this plot three times to maintain the drift wall at approximately 200 degrees Celsius. We've since just last week turned down the power one more time. So, we've now turned it down at five percent increments four times or at 80 percent of where we started. The drift wall

had crept slightly above 200. So, the thermal testing decided to adjust it down one more time to maintain that 200. Still intending, the plan is to heat until December of this calendar year.

Just a plot to give you a feel for how the volume of rock above boiling has evolved through time as we've heated in the drift scale test. This is volume in thousands of cubic meters versus time. You can see we've elevated a significant percentage of volume of rock over 20,000 cubic meters above the boiling point of water.

What about TH uncertainty? And, yesterday, I believe Dr. Bullen mentioned something about the fact that we're losing heat from the drift scale test. A lot of that heat that we're losing from the test happens to be involved with the bulkhead boundary. It's an open boundary and we're losing heat through that bulkhead. We've done a lot of work in modeling space to try to understand the uncertainties that are involved with that heat loss and that--mass and heat loss both and looked at a lot of conceptualizations of the bulkhead to get a feel for what kind of uncertainties that's causing in the interpretation of the test. I won't go into a lot of detail. This would take a lot longer than I have, but we've maintained massive energy conservation at every grid block as you do in all your numerical models. We've looked at a lot of alternative conceptualizations of the bulkhead.

One conceptualization, we have a barometric pumping across that boundary. We looked at different ways of conceptualizing the wing heater holes. The wing heater holes are open, they're not sealed, they're not grouted, they're also open to the drift. The bottom line is when we look at all these different alternative conceptualizations, we feel we can constrain fairly well the heat loss through that boundary, and overall when we account for that boundary, we do real well in terms of comparing simulated and measured temperatures. I could make similar statements about the overall saturation changes that we see in the test block, as well.

We continue to do air permeability measurements. This is a whole series of boreholes. All these boreholes happen to be from the observation drift drilled an in up-angle at different distances from the heated drift. There's a lot of data here. Bottom line is you've got time versus permeability at baseline versus some time during the heating phase. So, if you were at 1, basically the air permeability would be the same as it was prior to the start of heating. Anything below 1 would suggest that with time we're getting increased saturation within the fractures. Anything above would be opposite. You can see that in a lot of cases, we're getting increases in fracture saturation. In some cases, we're even getting things going from the condensation zone

into the dryout zone. So, we're getting wetting of the fractures and then progressive drying of the fractures. Bottom line here is we continue to collect this sort of data and we're comparing it to the simulations from our 3-D TH models, and in general, they are very consistent with the simulations for TH simulations.

What about chemistry? We have thermal hydrologic chemical models for the drift scale test. We did predictions prior to test and we continue to update those models as we go. As you've heard before, we continue to collect gas and water samples to compare to those model predictions. This is just two examples of model simulations using the THC model for the drift scale test. This is a cutaway through the heated drift about halfway down showing one of the arrays of boreholes from the observation drift and shows the change in calcite percentage and amorphous silica percentage at three years within the fractures showing that we would expect dissolution of calcite in the condensation and drainage zones and precipitation above the heaters, as well as precipitation of amorphous silica above the heaters. We've gone in and taken some samples. We had taken samples for mineralogy prior to the test. Everybody is aware of that. We've since gone in and done some sidewall sampling in some of the holes to try to get a feel for any changes that have taken place within the two and half or three years since we started.

Preliminary results from some of those cores shows evidence that we're getting precipitation of amorphous silica. That's consistent, and we're seeing that in the holes that are above the heaters, that's consistent broadly with the predictions. We're planning on going in and doing some more sampling, sidewall type sampling, in some of these holes in the next couple of months to continue to try to build confidence in this model.

This summarizes the results of the drift scale test, THC modeling and analysis, fracture matrix interactions very weak in the condensation drainage zone, water-rock interactions strongest for calcite and silica polymorphs. Then, you pick up aluminosilicate, K-feldspar in particular. I've showed before a comparison of our models to gas-phase CO_2 concentrations and how, in general, we do a pretty good job of predicting the evolution of CO_2 in the gas-phase and also pH which is obviously directly related.

Again, we do capture trends in pH relatively well. There's some things going on with the feldspar reactions, in particular, that shift the pH. So, we continue to have to refine our kinetics and thermodynamic databases. We're learning as we go about those databases for these important minerals. I've already talked about this, but the calcite in amorphous silica precipitation is predicted and we've got some preliminary observations that suggest that we're doing a

pretty good job there. Important to point, we talk about the changes in fracture porosity due to the chemistry. They're very small in the drift scale test.

^{36}Cl validation, I don't think I have to say probably too much about the purpose. We're validating. We're trying to validate the occurrence of bomb-pulse at two locations in the ESF, the Sundance Fault down by Alcove 6 and the Drillhole Wash Fault which is just before the ECRB intersect. If you go back to the Parumph meeting, I believe that was--it's been a while back now. Livermore and Los Alamos PIs presented results on validation core on the core that was taken from these locations and the Los Alamos results in ^{36}Cl, the chloride ratios were consistent with what June Fabricka Martin and collected previously from the area. Didn't find bomb-pulse, but the background is like 800 to 1000 10^{-15}, whereas Mark Kappy from Livermore was coming up with numbers more like 50 to 150 times 10^{-15}. So, we had splits of the same core and we were getting different numbers. So, went about to try to figure out why we were getting those differences.

I've talked before, we collected a reference sample that both laboratories used. We've since completed the work on that reference sample. I should back up. They always do work, of course, through the course of all their work that they do for a lot of other programs on primary and secondary

standards. We're convinced that the differences are not attributable to any measurements within the accelerator where you do the ^{36}Cl measurements.

Again, the reference sample work is complete, nearly complete. We've done a lot of sensitivities on leaching. You crush the rock, you put it in deionized water. You can leach it either passively meaning I leave it sitting in a beaker for hours to days or I can actively leach it where I rotate it in a drum or I shake it on a shaking table. How you leach the rock does affect the $^{36}Cl/Cl$ results. That's why we started down this path because we thought that would be one of the things that could be driving the differences.

Just last week, we met and decided amongst the principal investigators, the USGS, Livermore, and Los Alamos, to come up with a common crushing method. We're going to use one laboratory to crush all the samples, all the future validation samples, and arrive at a common passive leaching technique. We're going to stick it in deionized water in a beaker basically for a day. That will be used by both laboratories for all the validation samples from here on out. So, we're about to embark on looking at another set of validation samples from the Sundance and probably also look at something in the Drillhole Wash structure. That will continue through the summer with these common processing

methods and we still intend to come up with a final report later this calendar year.

Moving to the cross drift, it shows a detailed layout of the cross drift. The alcoves that are in italics and blue are those that are in the long-range plan, not yet excavated. Those in the regular font are the ones where there's ongoing testing. So, I'm going to talk today about some interesting results from the crossover alcove, the drift-to-drift test, some results from the seepage testing at Niche 5 and the lower lithophysal, and also results from the bulkhead experiment reminding you there's three bulkheads in the ECRB; one here about halfway down, one just before the Solitario Canyon Fault, and one just before the TBMs at the back of the cross drift. Also shown on here, in case anybody wants to be reminded, are the contacts for the different subunits of the Topopah Spring, the upper lith exposed here up in this section, middle non here, and lower lith and the majority of the cross drift. So, we're focusing on the lower lith for our characterization. I'm also going to mention some results from systematic seepage, air permeability measurements that Berkeley is doing, primarily in this part of the lower lithophysal right now.

First, Alcove 8, Niche 3, crossover alcove, I'll use both--excuse me for jumping back and forth. Again, here, we've got an alcove in the ECRB about 18 meters above Niche 3

in the ESF. We're looking at flow and seepage processes at the scale of tens of meters. This is just a schematic diagram showing the layout of that. Again, this is about 18 meters. Infiltration plot in the floor of Alcove 8 and Niche 3 down in the ESF where we're monitoring the movement of the water, the tracer front both from downlooking and uplooking boreholes and also collection trays within Niche 3. To cut to the chase, I'll talk about this. We have seen seepage into Niche 3 at this point. So, we'll talk some about that.

To remind you, we started back last calendar year with a very small plot along the fault at the back of Alcove 8. It was not taking up much water. We were seeing no seepage into the niche underneath in the ESF. So, we excavated a trench along the fault to try to expose more surface area and have since started the next phase of infiltration experiments.

We've got four sections of fault that we're infiltrating along. We started in early March and we saw seepage in 35 days into Niche 3 underneath. We saw the wetting front by those uplooking boreholes about a day before we saw it in the niche and we actually have a camera system down there, a wet-based camera system, that actually captured the drift development on the ceiling before which it had started dripping or the actual wet spot before it started dripping. We're currently collecting the water, quantifying

how much is dripping in and also collecting it for chemical analysis.

This is just some pictures. I won't dwell on these. Here's the trench along the fault in Alcove 8, the four sections of fault where we're infiltration water. This is just the permeameter set up that we're using to infiltrate the water, a constant head experiment. This is a picture of Niche 3 in the ESF underneath with the collection trays in the ceiling of Niche 3 and then the automated water collection system inside the niche. There's multiple trays, different sets of trays inside the niche. So, we're quantifying different sections of the niche, how much is dripping in as a function of time. Here's a picture outside the Niche 3 bulkhead where you can kind of pick up where we're starting to see the wetting just outside the bulkhead. The fault comes down and is just behind the Niche 3 bulkhead and cuts through and outside the bulkhead. So, the seepage right now is focused within Niche 3 along the fault. We're not seeing much in the way of spreading yet beyond the fault within Niche 3. This is some seepage on what would be the right rib of Niche 3. If you were facing the niche and walked inside on the right side, here's the seepage on the north wall, again concentrated along the fault. It tends to be concentrated along the fault.

Infiltration in Alcove 8 in liters as a function of

time, average rate about 140 liters/day is what the fault is taking up. As of early May, we put in over 8,000 liters of water into the fault and you can see this is just cumulative infiltration as a function of time. The first seepage was observed here on April 10.

I mentioned there's multiple trays in Niche 3 and we're quantifying each unique set. This is just another plot showing volume in liters as a function of time for the three trays that are seeing seepage. As you can see, we've put in over 8,000 liters and we're collecting on the order of 20 liters, 25 liters inside the niche at this stage. It continues to increase.

I should mention the predictions. We did predictions prior to the test and there is some uncertainty with the properties of the fault, the hydrologic properties of the fault. So, we did a range of sensitivities, and within the range of the sensitivities, we predicted the breakthrough within the time frame that you saw. But, again, we did quite a bit of sensitivities on the fault properties because we weren't real certain on those fault properties.

Moving to Niche 5, seepage tests in the lower lithophysal supports the drift scale seepage model and the UZ model. We've all seen this before, the access drift and then the actual test niche at the back of Niche 5. We have boreholes that were drilled prior to excavation. That's

where we do our air permeability to characterize it in pre-excavation. We then excavate the niche, do some post-excavation air permeability to see any changes due to the excavation, and then do the liquid release tests above the niche from these same boreholes. We've done our first phase of liquid release tests in Niche 5 and that's what the next set of bullets will discuss.

Test 1 was done in February. It demonstrates the capacity for the lower lith to store or divert water. We performed it in one borehole about a meter and a half above the niche. It ran for almost 40 days, a constant pumping rate of about 8 liters/day. We released almost 300 liters of water into the rock. There was some return flow implying that the amount of water we were pumping in exceeded how much the rock could take up. We saw no seepage or wetting at the ceiling. So, the water is being stored in the matrix or diverted around the niche.

One always has to ask the question, in our previous experiments, even if you see wetting and you're not getting all the water going into the niche, where is all the water going? So, you're got a question of mass balance. This is a very busy figure. What I want to point out is that we're reviewing right now in the field, as we speak, we're excavating what we're calling Bat Wings which are slot cuts on each side of the niche to try to collect more water. If

the water does flow around the niche, try to collect that water to get closer to mass balance constraint on the tests as we move forward. That was something that has been discussed with peer review panels, as well as the NRC, and we committed to do that. So, that again is ongoing. Once that slot cuts are finished, we will then go in and continue the liquid release tests in the niche.

Systematic hydrological characterization, we talked about this before. We've got a series of boreholes in the crown of the cross drift, regularly spaced locations. Looking for statistical distribution of hydrological characteristics. Permeability, looking at air permeability and doing seepage measurements in the borehole and also doing gas tracer measurements to get a handle on other fracture properties.

A schematic of how this setup works. Berkeley is the principal investigator for these experiments. This is just a schematic of one of those low-angle boreholes, 30 meters long, different zones packed off by inflatable packers, and we do with Air-K within each zone and then also do seepage experiments, and then collect the water in the crown of the drift.

Similar things to what we're seeing in Niche 5 in terms of storage. I'll remind you--it's probably obvious--but these are transient tests at this point. We have not run

these to steady state. So, we're seeing storage that could be a transient effect. But, one of the interesting things is they've done a lot of work with looking at storage in this transient environment. When you start out with a dry formation, in this particular test it took five days to start to see wetting on the crown and you see seepage within 16 days. If you pause that during the test and if you wait, say, five days, you get seepage to start much faster. The same thing we saw in Alcove 1, but it underscores the importance of fast capacity, but also initial tests emphasize the storage component of the lower lith under transient conditions. Now, the niche test, I should go back. Our intent is to run those to steady state if, at all, possible. When we look at the mass balance with the Bat Wings, etcetera, we intend to try to get those to run to steady state.

Bulkhead investigations, I'll provide an update on what we're seen in the bulkhead at the back half of the cross drift. The primary update will be some preliminary water chemistry on some of the waters in the sample. Our working hypothesis continues to be that the absorbed moisture is attributed to condensation. The heat from the TBM, part of that hypothesis is that the heat from the TBM is causing the temperature gradient behind the bulkheads and producing the phenomena that we're observing. Ongoing work and path

forward, we're collecting water and analyzing that chemically. I'll take some about preliminary results from that. We're getting some data to more closely monitor the TBM power, how much power is going to the TBM to help with understanding the test and modeling space. We're talking a lot, we haven't gotten down to actually buying instruments. We're doing a lot of discussion about how we might go about trying to monitor behind the bulkheads to try to distinguish condensation versus seepage. As you can imagine, that's a challenge. So, we're working through that right now. I don't have any answers for you right now, but we're working through that with the hope of doing something later this summer to try to better monitor the conditions behind the bulkhead and probably improve some of our measurements of atmospheric conditions in there, as well. Bob MacKinnon yesterday already mentioned about the fact that EBS modeling folks are going to be looking at this test, as well as the UZ folks, to try to understand what it's telling us about indrift processes.

Some pictures, three pictures to be specific. Remember there's drip cloths hanging in part of this bulkheaded section back towards the second bulkhead at about 2500 meters from the intersection of the ECRB. Here's a drip cloth here. You've heard about the mottling or the blue streaking. This is an example here. One of the drip cloths

showing the evidence of what we're attributing to condensation. Here's an area on the conveyor where water collected. You can see it's pretty dirty. That's appropriate to the next slide. Here's another set of drip cloths showing the same mottling and here, as you all have heard before, in some cases we have some rock debris that has fallen on the drip cloths; small, relatively small, smaller than fist size type rock that can make it through the mesh.

Small print here, but it's probably clearer in your hard copies. We've taken water samples. What do we mean by dark and clear water? Some of the water was laying on top of the conveyor. The conveyor has rock, dust, it's been sprayed with J-13, it's for dust control. And so, we've done analysis of that water, but it clearly contaminated; very high ionic strength. It makes no sense, whatsoever, in terms of even being seepage water. There is some containers along the conveyor where we've actually captured some water. When we analyze that water very dilute, it looks like condensate. Preliminary measurements, now this is the kind of information in my opinion that we can get some good chemical information and also isotopic information that could tell us a lot about whether it's condensation or seepage. We're looking at improving our water collection capability, as well, possibly interacting them in some way with the drip cloth.

Move out of the Topopah Spring and down to Busted Butt, the objectives at Busted Butte--here, we're again southeast of the ESF about seven miles or so, looking at the bottom of the Topopah Spring and the top of the Calico Hills formation. Looking at several objectives at Busted Butte, this test has been going on, you've heard about it many times on the order of three years. We're looking at the influence of heterogeneities on flow and transport below the potential repository horizon; at least, as an analog to below the potential repository horizon. Looking at other aspects like fracture matrix interaction, permeability contrasts. Colloids, as you've heard, we had some mixed results on colloids in the field, but we're going a lot of lab-based experiments with the same rock and I've got a couple of slides on that later. We've got a wealth of laboratory batch sorption data on radionuclides and here we're looking at scaling of that sorption data. And, ultimately, calibrating and validating the flow and transport model and I've already mentioned the scaling issues.

A more detailed layout of the test; the main adit, the test alcove. You're seen this before. We have two planes of injection boreholes and then planes of collection boreholes off the main adit below those. The large Phase II test block, we've stopped injection at this point and we're in post-test characterization phase right now. Also, notice

there's some faults in the block. That will become important in the slide after the next one.

Part of the post-test characterization was to do a series of overcores of some of the injection holes. This is a plan view of the injection face in the test adit there showing the two planes of injection boreholes and five overcores that we've done of those injection holes to chase the tracer front. What we were after here is the movement of the reactive tracers. We saw a breakthrough of the conservative in the collection holes. We saw a breakthrough of lithium in a lot of the collection holes, but we did not see the other reactors, the more highly sorbing reactive radionuclides. So, here, they probably haven't traveled too far from the boreholes. So, we were using overcores to try to understand the travel distance. WE did predictions for how far we thought they would have flown at the end of the test and we're now going to analyze the rock. We're analyzing the rock as we speak to see how well we did with our prediction. We overcored up at the top of the block. Remember, we're in the Topopah Spring in the vitrophere. So, it's fractured, nonfractured, but relatively coherent. It holds together real well when you core it. When we went down to the Calico Hills which was the bedded tuff, we weren't really able to keep the core intact. So, this core down here wasn't as useful, but we're now doing a mineback which will

give us the information on the Calico.

A very busy diagram, but one to point out. Again, here's the main adit, the test alcove. Here's the injection holes running off the test alcove. This is all to scale. We're doing a mineback where we're excavating an access drift back to this point and we're now working our way down across the injection plane to get a handle and we're taking several slices out of that mineback, much like we did with the smaller Phase I-A mineback that you've heard about before. Imaging where the phlorhizin has gone and also taking auger samples to analyze in the laboratory to again compare to our predictions. The reason why there's this little nubbin off here at the bottom, there is a fault in the back of the test area and we wanted to try to access that fault, in particular, and run an injection hole across it. So, that's why we're where we are. So, that's ongoing. We've got about--we're about to here at this point.

What about the colloids? We're doing, I mentioned, colloid experiments in the laboratory. We're looking at colloid stability as a function of ionic strength and cation concentration. We've talked before about we have not seen--we've not gotten much out of the colloid experiments at Busted Butte. We think that's a primary function of the ionic strength of the water that we're using. So, we've gone to laboratory-based experiments with intact core and crushed

rock from Busted Butte. We're doing measurements to look at pore size distribution and connectivity of pores. Again, I mentioned this, but we've got block experiments that we've taken, blocks that we're taken from Busted Butte on the order of 8"x8"x8", and we're looking at the effect of interfaces and unsaturated flow on colloid transport. And, this is all being incorporated in the colloid model for use in the UZ model.

Just an example of what we're seeing in some of those Calico Hills samples. This is normalized concentration versus the cumulative volume of a fluid being put into the core showing the breakthrough of 190 nm latex microspheres as a function of ionic strength of the solution. So, this gets at my point about the fact that we're seeing a strong function of the colloid response. It's being influenced strongly by the ionic strength. So, at 0 ppm lithium bromide, you've got this breakthrough. You can see that at higher concentrations it's not only delay, but it's also less volume overall.

AECL, the Canadian program, is also doing some work with rocks from Busted Butte. We've collected blocks from Busted Butte from the Calico Hills primarily and they're doing laboratory-based transport experiments. At Busted Butte, remember, we're using analogues for the radioactive material. In the laboratory, they're able to use actual

radionuclides. So, they're using technetium iodine, neptunium, and others and they're doing transport experiments both under unsaturated and saturated conditions. This is a little misleading. We collected cubic meter blocks. Those tests are ongoing. These results here are from a smaller block that we took. We took a 30 cubic centimeter block--30 centimeters by 30 centimeters by 30 centimeters--excuse me, so more than that--and we've done an unsaturated experiment to scope out the larger blocks. The results from that smaller experiment are shown in the second two bullets, preliminary results. We've got good agreement with the batch sorption data that we've collected in the laboratory at Los Alamos primarily for both Np and Tc. The transport behavior of Np seems to agree pretty well with that that we've seen for nickel at Busted Butte. We're using nickel at Busted Butte as a geochemical analog, so to speak, under oxidizing conditions for Np.

Moving to the saturated zone, our work is focused on cooperative work with Nye County, early warning drilling program. The objectives of our work in cooperation with Nye County and how that feeds into the SZ flow and transport model, you've all seen this before. It's shown down here on the right. I'm going to talk and give an update on the lithologic work that's being done at the U.S. Geological Survey and also a short update on what's going on at the U.S.

Geological Survey with hydrochemistry data from Nye County.

Lithostratigraphy, our last meeting, I showed you some preliminary cross sections that Rick Spangler from the GS has put together. He continues to work on that. At this point, he's got the Phase I and Phase II data from the drilling program, as well as results from the surface geophysics that's been done, aeromag and gravity surveys, and those are being incorporated into a set of cross sections. There's a north-south cross section that runs--there's a north-south cross section that runs up Fortymile Wash. It runs basically up Fortymile Wash. And, an east-west one that runs basically along US-95. These are nearing completion and they'll be used to update the geologic framework for the site-scale SZ model. Preliminary interpretations, remember that Nye County penetrated the carbonates in 2DB. Rick's interpretations at this point are that they penetrated Silurian to Ordovician dolomitic limestones which is pertinent when you talk about the details of what you're seeing in the carbonate aquifer. And, he continues to look in great detail at the tertiary tuff section with the Fortymile Wash section and how that transitions into alluvium.

This is work led by Gary Patterson of the USGS. The USGS continues to collect a lot of water samples to do hydrochemistry and isotope analyses. Just a list of the Nye

County holes and the Nevada Test Site holes where they've collected data. That's all being entered into a large database and we're using it in the SZ model to help calibrate the flow field. That will all be released in the USGS OFR later this summer.

What about the alluvium? Remember, we're also starting up an alluvial testing complex. Yucca Mountain, here, US-95 running along here. 19D is the current single hole that is planned to be the cornerstone of the multi-well test that we're planning--will likely start later this calendar year. Here is one potential flow path coming from the repository down the wash and down gradient to the southeast and then to the south. That's one potential flow path. You can see 19D is located right along that potential flow path.

Again, 19D penetrated into tertiary sediments, the water table located here at about over 300 feet. You have a fixed sequence of valley fill or alluvial deposits, a tertiary tuff section, then a tertiary sedimentary section. This was drilled by Nye County and we're now conducting--we've conducted hydraulic and single hole tracer tests in this hole. We've got a series of screens below the water table where we'll be doing isolated interval hydraulic testing. Again, we're focusing here on these four screens within the alluvium.

We did a series of four isolated interval using different screens, pump tests. We've also done a series of three single-well tracer tests where we inject tracer sometimes with no rest, meaning we pump it right back, and in other cases, we shut in the well and then pump back after a certain period of time. The next plot will show some results from that last--the last of those three, but the preliminary results from those tests indicate insignificant diffusion from the groundwater into the stagnant water. So, we've got an invection dominated system. There is some dispersion along the flow path, however, and we continue to try to quantify effective porosity from the test results. Implications for TSPA, this is the bullet that I used last time. That continues to be borne out. What we're using in TSPA in terms of single porosity continuum is acceptable for alluvium, at least in the ATC area. I mentioned the multi-well test. Now, that we're finished with the single-well tests, we'll move into Nye County and we'll drill some additional injection monitoring wells and those will be installed, we're hoping, later this calendar year. Then, we'll start a series of cross-hole hydraulic and tracer tests again later this calendar year. We'll look at a whole host of parameters for the model.

Some results from the last of the three tests. This is a complicated plot. The blue or the two shades of

purple focus on this axis and elapsed days. So, what we did is we injected the tracer, shut in the well for 30 days, and then pumped back for 57 days. Okay? So, this shows the response of fluorobenzoic, that's poly-fluorobenzoic, acid, and bromide as a function of days since we started pumping. You can see the response is very similar. Different diffusion coefficients. That gets back to the conclusion that we're making about little diffusion, invection dominating.

A different way of looking at it. If you concentrate on these axes here, this is just the number of days that we pumped since February 27 as a function of concentration for just the fluorobenzoic. What we're showing here is periodically twice during the pump back, we stopped pumping to look for the effects of matrix diffusion. So, you'd expect to see some rise in concentration. There is a rise in concentration, but notice the scale here. It isn't a significant rise. So, there's an effect of matrix diffusion, but relatively small.

I won't dwell on this. This simply shows the mass recovered for the two tracers for that last test as a function of time since we started pumping.

Okay. Now, switching gears to engineered barrier. The ventilation test at Atlas doing a 1/4-scale test for validation of preclosure ventilation models. Again, we have

1 a simulated drift with a set of simulated waste packages, 25
2 inside that concrete culvert pipe. We've got a crushed tuff
3 invert and we're measuring temperature, humidity, temperature
4 in the air, temperature on the surfaces of the waste package,
5 the walls, the insulation and also heater power input, and
6 then constructing temperature and velocity profiles as a
7 function of--we're doing basically an engineering matrix,
8 varying velocity and looking at the effects/the efficiency of
9 removal of heat by the ventilation. That was Phase I. Next,
10 just some pictures showing the scale of the concrete pipe and
11 then the simulated waste package and then looking down this
12 mocked up drift at Atlas.
13 Phase I, we were taking air in from the room and it
14 was a flow-through system. We weren't recirculating the air.
15 I've talked about Phase I last time. Here, we've calculated
16 some heat transfer efficiencies from the test results. You
17 can see they're on the order of 80 to 90 percent, in general,
18 for the different flow rates and different temperature powers
19 coming out of the waste package. This is real important data
20 for use for the validation of the preclosure ventilation
21 model and what they assume in design for the ventilation.
22 We've now reconfigured the test where we're
23 recirculating the air. So, it's no longer just a flow-
24 through. We're recirculating the air. We're going to do
25 three different temperatures, the air at different times to

represent remote sections of the drift; meaning in a real repository, when you get halfway down a drift, the temperature is going to be a lot higher than when it started, obviously, because you're removing heat. So, we're trying to use three temperatures to try to understand how that works. What we've done at this point, we've injected and we've brought in air at 25 and we're doing 35 degrees C, as we speak, and 45 would be next. That just started--well, it says here it started in late April. So, that's ongoing.

What about thermal properties? Thermal conductivity in the lower lithophysal, in particular, is an area that we're starting to focus on with our testing program. Two components; there's a field part and a laboratory component. You've been down in the lower lithophysal. Remember, the lithophysal cavities tend to be quite large. So, when you think about taking a core and measuring thermal conductivity, what's that telling you? It's telling you matrix property, but is it telling your rock mass, thermal conductivity? So, we're setting up a field program where we're using borehole arrays to try to get at the effects of scaling on the thermal properties. This is an example of one of those arrays that was drilled. You have a heater with a thermocouple array underneath, and through inverse modeling, we can back out the thermal properties. We'll do predictions with the models that we're using for

thermal conductivity in the models to see how well we do with the lower lithophysal. This is the first array that we've drilled. We're drilling the second array as we speak. But, this test should be starting any day.

Laboratory program will be done in conjunction with the field program. This is probably obvious to most, but remember that the conductivity is a function of a lot of different properties, the porosity, the saturation, the lithophysal porosity, temperature, the gradient. And, we're working on defining that laboratory program, what techniques we'll use to measure thermal conductivity, and also looking at some geostatistics to try to understand the variability of uncertainty in the measurement so we constrain how many samples we'll analyze in the laboratory.

Okay. Moving to materials testing, waste package materials testing, you've seen these bullets before. Again, we're doing a lot of materials testing in support of the waste package and drip degradation models and the design of the waste package and drip shield. There's long-term tests at the corrosion facility at Livermore that have been underway for quite a while and we're looking at a range of conditions; range of different geometries, coupons, flat coupons just looking at weight loss, U-bend specimens. Looking at crevice corrosion and crevice specimens, looking at welds, a whole bunch of different kinds of scenarios. The

test conditions in the vessels range in temperature, ionic strength, and pH to bound the problem in terms of corrosion.

I was asked to discuss briefly where we're at with water chemistry in the indrift environment. We're investigating a range of water chemistries to bound the conditions at the surface of the package and the drip shield. J-13 water is representative of a sodium-bicarbonate water. It's similar to perched water and, I should say, some waters sampled from the field thermal tests. Not all the water is just like J-13. Our other representative water is a calcium-magnesium-chloride-sulfate. Sodium-calcium-magnesium-chloride-sulfate water, typical of pore water in the Paintbrush, the nonwelded Paintbrush, above the repository. These are waters that have characteristics that are important to focus on, to look at for waste package and drip shield degradation. We vary the ionic strength and pH of these representative waters to bound the expected water chemistries. We're including minor constituents, lead and arsenic, that are important to understand for corrosion processes. Again, the focus is on the characteristics of the water, ph, ionic strength that are important to the degradation models. A bullet that's somewhat related that I wanted to point out. We've recently initiated a formal internal self-assessment of J-13 and J-12 water chemistry. There was some concerns about how J-13 and J-12 are sampled.

So, we did a self-assessment and what we found was that the consistency and the data for cation and anion analyses, in particular, which are important for the models that I mentioned above, they were collected appropriately and are representative of each borehole. So, the bottom line on that assessment was there's no impact to testing analysis and documentation. I can talk more about that in the questions, if you'd like.

What have we done with some of these pore waters? This is just a set of bullets that give some highlights of what we continue to do to look at the environment on the drip shield and the waste package. We continue to do work. This is done at Livermore. Greg Gdowski is the PI for this work continuing to look at evaporative concentration of pore waters. We're looking at pore waters. There pore waters happen to be similar to waters that we collected from Alcove 5 when we did some ultracentrifuge work to try to get a handle on the Topopah Spring pore water compositions. We're looking at again trace element concentration in brines and continue to look at the effect of PCO_2 on the evolution of those brines.

USGS is also conducting some analyses of dust. Dust is important. We talked about potential development of brines on the surface of the waste package. So, we've gone through and the GS has collected 28 samples and done a whole

suite of major and trace element analyses. Here's some results of interest I should emphasize. These are for the dust. Okay? This is the dust analyses and these are trace elements in the dust. We're stating to do some measurements in soluble chloride, nitrate, sulfate, and other components. These are being compared to the rock values, in general. The dust looks like the rock, no terrible surprise We're proposing to take some additional samples to continue this program to better constrain the composition of the dust that might contact the waste package.

Some highlights of results. Here's some results from Alloy 22 and calcium chloride, calcium nitrate water at 120 degrees Celsius. This is cyclic polarization data. I don't have plots here, but we could compare this to Alloy 22 performance and cyclic polarization experiments under other water chemistries and it would be very similar. We not seeing--we're seeing passive film development, but no evidence of passive film breakdown. So, Alloy 22, similar again to Alloy 22 tested in other environments. It continues to look very promising in terms of passive film development and very robust passive film. But, as we heard yesterday, we're continuing to look at that including a peer review.

What about stress corrosion cracking? Here is a diagram showing--it fell off of here--but, this is stress versus strain. So, at constant strain rate in a variety of

different environments, we've taken Alloy 22 subjected it to slow strain rates and taken it up to where you see cracks developing or until some cracks develop. We've done it in a variety of different environments. This particular example shows adding trace elements like lead to the environment and shows that there's really no impact, at least in this particular specimen in terms of introduction of trace elements and the influences on stress corrosion cracking. Just an example of the continuing experiments that we're doing in this area at Livermore.

Laser peening, that was discussed yesterday. I'm not the guy to sit up here and talk about the engineering of laser peening, but what we've done here--and it's hard to pick up--but what we did is took stainless and we peened the weld here and not here, stuck it inside of $MgCl_2$ at boiling, and a crack initiated along the weld, and then once it got near the peened region, it changed direction. So, we think we're starting to see evidence that, in fact, peening puts this particular area under compression and will inhibit stress corrosion cracking. This is just an example. We're continuing work like that.

So, to wrap up, hopefully not too fast, we continue testing in the ESF, the cross drift, Atlas facility, and laboratories to address the key processes and related uncertainties, and we continue to collect date, analyze the

data, and this will be incorporated into the site recommendation documentation as appropriate.

WONG: Thank you, Mark. Mark, you're a good boy. You finished ahead of schedule.

PETERS: I saw that.

WONG: Questions from the Board? Dr. Runnells?

RUNNELLS: Runnells, Board. Mark, on your Slide 39 which is a presentation of the effects of the tests on colloids, it seems like the project is having a tough time understanding colloids. As I understand it, the field tests at Busted Butte, the colloids sort of disappeared. They didn't disappear; they didn't come through. I guess, we attribute that to effects of ionic strength and rock sorption and things like that. This set of tests using lithium bromide as a matrix, it's a particular size of colloid, 190 nm. It seems to me that there's so many variables in this study of colloids that I wonder if there's really any hope of understanding the colloids. For example, why not use synthetic pore water in these experiments as a way, at least, to eliminate one variable, lithium bromide? Could you just comment on sort of the direction the colloidal study is going and what the investigators think are the odds of really understanding this? There's so many variables.

PETERS: Yeah. First, Busted Butte, you're right. What happened, we think, is they were flocculating because of the

ionic strength and they never probably made it out of the injection hole. So, you were right on there. This was a synthetic pore water--it started as a synthetic pore water and we just used different concentration of lithium bromide. So, it does look somewhat like a pore water, ignore the lithium bromide for now. The other cation concentrations are similar to a pore water.

RUNNELLS: I see. So, the lithium bromide was put in just to control the ionic strength?

PETERS: Yeah, right--well, and we saw that the bromide-lithium content in Busted Butte, we thought, were the drivers for why we were getting the flocculation.

RUNNELLS: What about the size, the 190 nm? What's the basis for that?

PETERS: Well, we're looking at different sizes. The basis for that was that was similar to what we used at Busted Butte, similar to what we used at the C-wells. It's similar to what we're thinking about using for the ATC.

RUNNELLS: But, in terms of nature, in terms of colloids in the field, what's the basis for choosing 190 nm, for example?

PETERS: I don't know the answer to that specific question, Don. I mean, I think--I'm not doing justice to the overall program just by showing one example.

RUNNELLS: Right.

PETERS: We're doing different sizes, we're doing different chemistries, we're characterizing. But, we have had a problem in the unsaturated zone doing it in the field. Now, we're talking about--we're proposing to do some colloid stuff at Alcove 8, as well. Acknowledging that we've had these concentration problems, we'll try to do that in a smarter way to try to make sure that we see breakthrough in Alcove 8. We're focusing the Calico Hills experiments on the laboratory where we control things better.

RUNNELLS: It's a difficult problem and yet people are focusing on colloidal transport.

PETERS: We are, but I won't--I agree with you there's a lot of variables. We're doing it in a systematic way and again I'm not giving it justice here and I can't--the PI, Maureen McGraw at Los Alamos could stand up here and probably tell you a lot more and give you more confidence. But, I feel like we're working through the problem systematically, but it's a difficult path.

RUNNELLS: Very good. Jeff, may I have one quick additional question? On your Slide #9, that's the one that shows the air permeability changes, and down at the bottom, you show two or three that go down and then start back up. And, you attribute these to changes in saturation.

PETERS: Right.

RUNNELLS: One question would be how do you know that,

but another question would be could they be due to changes in degree of plugging all the pore spaces of the fractures by mineral precipitates, such as silica? Plugging and then dissolution; in other words, two chemical processes going on there to decrease it and then increase it? How do you know it's water saturation and not mineral precipitation and dissolution?

PETERS: Well, I can't go into the holes and look at them specifically right now to tell for absolute sure that it--but the magnitude of precipitation and dissolution that we're seeing in the field from the limited samples we've taken during heating and also from the modeling that we've done would suggest you couldn't produce those kind of shifts--

RUNNELLS: Good answer. The magnitude is too great. Something that could be checked later.

PETERS: Right. Now, this conclusion was thought through very carefully by the PIs, both--because you can think of chemical--I'll give you more than you even asked. What about mechanical and we make similar arguments for the mechanical effect.

RUNNELLS: Right.

PETERS: So, we think that it's primarily driven by the--

RUNNELLS: Very good, thank you.

WONG: Dr. Parizek?

PARIZEK: Parizek, Board. Mark, that same slide, could you help us with the positioning of the boreholes that are represented? You said it, I think, but I missed whether above or below or side of.

PETERS: There the three out boreholes there. So, there are three inclined up above the heated drift about halfway down the observation drift.

PARIZEK: Okay.

PETERS: And, the different intervals, 1, 2, 3, and 4, are the different packed off intervals in the hole. So, 1 is closest to the collar. 1 would be here, 2, 3, 4. Okay?

PARIZEK: Yeah. Similar question as to what Don was raising about silica precipitation versus water content change. So, you won't really know the overcore, I guess. Finally, there will be overcoring done, but there's not been overcoring yet in that region?

PETERS: Not in these holes, but remember there's--go back to the drift scale test, you know, the pickup sticks diagram with all the boreholes all over it.

PARIZEK: Not that one; that's too easy.

PETERS: Yeah, that one, John. One more, I'm sorry. What we were looking at there, Dick, was this array here. There's a chemistry array right next to it and we've pulled the liners out of those and done some sidewall sampling.

PARIZEK: Okay, that's where it is.

PETERS: And, that's where we're getting some preliminary mineralogic information that suggesting that we may be seeing more silica precipitation.

PARIZEK: And, that's less than 1 percent change in porosity number came out of that observation or--

PETERS: That was a model conclusion, but what we're seeing in the field corroborates very small changes that you'd see.

PARIZEK: Well, when is the coreback experiments going to be done after the heater is turned off? Is that after cooling sometime?

PETERS: The current plan would be to--yes, it would be after cooling.

PARIZEK: Okay. So, that would be a little while in the future before we know the outcome?

PETERS: Yeah, it would be about five years from now, four and a half years from now, according to the current schedule. That's not to say that we don't need to go--if we don't go in earlier and, say, we cool it for less and go in and collect information sooner.

PARIZEK: Page 14, you added water to induce leakage to try to get a cross-connection effect between the--you gave the amount of water you had to add. It was 140 liters/day. My question is, I guess, over what area? So, what sort of

rainfall is that equivalent to or is--you obviously put 140 liters into some area, surface area.

PETERS: Yeah, I'm not sure I'm going to do that off the top of my head.

PARIZEK: Yeah. I just wanted to get Dr. Sharp's reaction. Is that in her models?

PETERS: Well, it's 140 on the--the trench is about 3 meters long by a foot deep. So--

PARIZEK: We can figure out what that area is--

PETERS: Yeah, I'm not sure I'm going to do that sitting up here.

PARIZEK: Roughly, what kind of rainfall is that, yeah. But, that's something that would be worth knowing.

PETERS: Okay.

PARIZEK: Because that's induced connection which is--

PETERS: Well, it's a--we're doing a constant head there. We're putting water into the trench and just keeping a constant head on it. It's not trying to simulate a--but we can do that calculation very easily, I think, if I wasn't standing up here--

PARIZEK: Yeah. Then, you've got a wetting front and now you have some drips. So, you're capturing some water. Is that--you said that water would be tested chemically. Are you going to look for colloids in it? Obviously, everything is dusty to start with, but if you run water through there

for a while, will you be able to look for colloids or will you be adding anything up above that--again, microspheres or something else to see if you can get particles to pass through that same interval?

PETERS: The next phase, we're looking at possibly adding microspheres to that experiment.

PARIZEK: That would be a useful experiment. And then, as far as the TBM air flow storage, it's kind of interesting. You probably have experiments there that could understand better how air and water may move once you have emplacement drifts filled with waste as you approach bulkheads, as you approach variations in temperature in the roof. That's the thing. I think, you said you were thinking about it or the group is thinking about what you can do to understand the processes that operate because of these heat differences.

PETERS: I've been talking a lot to Bob MacKinnon and Jim Blinken (phonetic), EBS folks, about that.

PARIZEK: So, I mean, there's obviously data there. The question is what can you do with it and how does it help you confirm models?

PETERS: Right. Personally, I think it's telling us something about indrift processes that I think we need to--

KNOPMAN: On that same subject, Mark, of the cross drift bulkheads. If we could look at Slide 32, I'm a bit puzzled as to why it's so hard to tell the difference between

condensation and pore water seepage. It seems to me the picture in Picture 1 may have a lot of the story there. I don't know how much you're actually analyzing on the drip cloth any of the residue that's the precipitate on the drip cloth itself that would tell you something about some signature of condensate or dripping. I'm just puzzled as to what's so hard here to figure out origin. Then, just a related question on Slide 33 looking at those two pictures of some of the water collected. The one sitting on the conveyor belt, I mean, it looks rusty from here. I mean, I would assume it's just localized dust and particles coming in there. Again, why is this hard to figure out?

PETERS: Well, the conveyor--okay. I think if we can get chemistry analysis, that's going to continue to help us. But, what I was saying is hard to figure out--if you go back, John. I'm not sure. I'll try to answer your question. These look like drip marks to me.

KNOPMAN: Yeah.

PETERS: But, the question is did that drip from the rock or did it condense on the rock and then drip down? That's what I'm trying to get at.

KNOPMAN: Right.

PETERS: Because if it condensed on the rock or it condensed on the steel or the mesh and then dripped down, then that's not seepage in--

KNOPMAN: But, these would have different chemical signatures?

PETERS: Yes. And, that's--and we are looking at that. It's just that I don't have a lot of data yet to tell for sure, but give me a couple months and I can probably give you a much better feeling about what the chemistry is telling us and whether it's condensation or seepage.

KNOPMAN: That leads to my next question which is what is the timing of the analyses and the conveyance of results here on this?

PETERS: The chemistry analyses are ongoing. So, they'll continue through the summer on what we collected and then right now we're intended to enter again in the summertime. We would collect additional samples at that point and also improve the way we collect the water, as well.

KNOPMAN: Okay.

PETERS: Now, the conveyor--one final point. The conveyor, it's very dirty. We didn't go in and clean the conveyor. It had rock dust all over it. And so, that's why that water looks like that. It lays in there and it's just rusty looking, dirty looking. We collected it and analyzed it, but it's not--we need that clean water that we collected to really understand.

KNOPMAN: How much is the work here, the chemistry analysis of the pore waters, connected to the work that Greg

Gdowski is doing from Alcove 5?

PETERS: Right now, we don't think we're collecting pore water. We think it's condensation. But, the results--anything we collect in terms of water chemistry and what we say will be made. They'll be made fully aware of it to make sure that they understand what we're seeing and make sure there's still within their bounds.

KNOPMAN: Okay. And, the schedule of his work for--

PETERS: It continues. That's being going on for--Greg, how long have you been working on that; two years, three years?

GDOWSKI: --years.

PETERS: Yeah, it's been going on for two years and that work continues.

KNOPMAN: And, there are results that we have?

PETERS: Yeah, I didn't do it justice. I mean, there's a lot of results.

KNOPMAN: Okay.

PETERS: And, that will be presented at the peer review. Greg is going to do an overview on what he's done at the waste package peer review that Joe Payer discussed yesterday.

KNOPMAN: Okay.

PETERS: That's all in the AMRs, as well.

SAGÜÉS: I wanted to start by making--general comment. You're presentation is entitled scientific and engineering

testing. And, I think this is a good idea to remind ourselves what the meaning of the word "testing" is. You can have testing for two general purposes. One of them is testing in which you do experiments to obtain parameters for well-established models. For example, you may do an STM kind of test to determine the strength of a given alloy and that is a pretty well-understood kind of testing and you get a number that you can use for design purposes and so on. And, there are a number of tests conducted along those lines in here.

Now, then there is totally a different kind of test which is testing for establishing the validity of a theory or a model or assumptions. And, there are some of those in here, as well. And, somehow, the distinction between the two kinds of tests is not clear certainly for the general public and for many of us, as well, now and then because we have this complex of assumptions to go before a particular kind of activity and then how the results are being used. I think it would be interesting to keep in mind differentiating between the two types of tests. We ourselves use the terminology testing sometimes a little bit in a hazy manner and the problem with not making that differentiation is that sometimes we end up making assumptions in the varied standard of the result of the test.

I guess that a good example of that is what I'm

leading to. Is when we obtain, say, corrosion rates and indeed the long-term experiments with a test stance are aimed to obtain corrosion rates. But, now, a corrosion rate is an obstruction, it's an assumption that--it's like saying the speed of a car and is it going to be the same all the way or is it going to increase, is it going to decrease, and so on? What is the meaning as to when you do it? So, long preface to the question that I wanted to ask.

The project is getting numbers right now from the time tests. And, those numbers in the past were being used in things like TSPA and so on. Now, yesterday, we had, at least for me, what was the first indication that maybe either different sources of corrosion rates are being used, such as the University of Virginia results, or maybe parts of those experiments are being used to establish, say, the temperature dependents of corrosion rates. What is the status of that? If the project now shifting to use results from short-term chemical tests at the University of Virginia instead of the long-term numbers from the time tests?

PETERS: I'm going to have to get Greg or Tammy to speak to that because they're--Alberto, one thing I--or Gerry, either way. Your point at the beginning about distinction is well-taken. That can be certainly made clear, I would agree. Gerry, if you wouldn't mind trying to tackle that one?

GORDON: Yeah. Gerry Gordon, Yucca Mountain Project.

Let's see, first the weight loss tests, the corrosion rates are very low and the temperature range, 60 to 90 degrees C, is very short. So, it's very difficult to establish a slope of corrosion rate versus temperature of 1 over T. The University of Virginia used potentiostatic tests over a broad range of temperature and we were able to better establish the Arrhenius relationship. It's an estimate. We're using it that way currently. We're using potentiostatic tests in J-13 type environments over a broad range of temperatures and we hope to establish a better temperature dependency in the not too distant future. I don't know if that answers your question.

SAGÜÉS: Okay. Yeah, I have actually the numbers. What is intriguing me a little bit, yesterday, I think, that they were talking about fairly large activation energy like 60 kilocalories or more, in that order.

GORDON: Right.

SAGÜÉS: Which would imply in the 60 to 90 degree region a variation of a couple of orders of magnitude, maybe? Is that--

GORDON: That's true. Actually, there are two slopes to the 1 over T curve. The activation energy varies from 20 to 60 kilocalories per mole. I didn't do the fitting, but I think they did an average fit.

SAGÜÉS: And, how about the actual values of the

University of Virginia? Are you using the values that they obtain or just--

GORDON: Just the slope.

SAGÜÉS: Just the slope. So then, the idea of this moment or, at least, the way in which this is headed is to perhaps use the absolute numbers from the gravimetric tests, but the temperature depends from the potentiostatic tests?

GORDON: That's correct.

SAGÜÉS: That's the general idea what is being attempted?

GORDON: Right.

SAGÜÉS: Okay, okay. I wanted to clarify that. Okay. Thank you very much.

I have one more question and this is just simply seen in one of the transparencies of what you showed, these slow strain rate measurements. What was that, 59? Yeah. What was the temperature of those tests?

PETERS: I believe it was--I don't know exactly. Tammy or Greg, you guys remember the temperature for that?

GDOWSKI: Greg Gdowski, Livermore. It was actually 75 degrees C.

SAGÜÉS: At this moment, actually most of the experiments we've laid actually gave better longation to fractures than the ones without.

GDOWSKI: Greg Gdowski, Livermore. I don't believe that

the measurements are able to distinguish--the resolution of the experiments is able to distinguish that one is more or less susceptible than the other ones.

SAGÜÉS: All the data that you have right now is just a selection of the data?

GDOWSKI: No, those are all the data we have right now.

SAGÜÉS: Okay, thank you.

NELSON: Thanks. Mark, I want to ask a couple of questions. First, this is Nelson, Board. On the precipitation of amorphous silica, I'm trying to get a handle on you indicated that there didn't seem to be a change in matrix porosity of any significance. But, I would expect it to be more important as an anticipation as a surface modifying effect along fractures really reducing the interaction between the matrix and the fracture. Where are you finding the silica precipitation and will you look for it?

PETERS: It's on top in most cases of what is already probably a sequence of fractures, minerals that you see. You know, late stage calcite, opal, you know, the typical sequence that you see, but it's growth on top of that, I think the answer is. Could it produce roughness in the fractures? Yes.

NELSON: Well, actually, could it seal fractures?

PETERS: The apertures, the volumes that we're seeing

and that we predict and even in the THC models for PA don't seem to close up fractures.

NELSON: Okay. There's the semantics. I'm not talking about sealing the fracture, but as filling and possibly sealing the surface.

PETERS: Inhibiting the interact with the matrix, yes. I think it could have an influence. And, you saw one of the bullets, we talked about limited fracture matrix interaction. One of the conclusions from the THC modeling is there is limited fracture matrix interaction. I would think that if you were precipitating additional minerals, it could inhibit interaction.

NELSON: Yeah, and it would seem to me that that would be, if anything, more important than the actual sealing of a fracture is that nature of interaction. And, I'm wondering if when you do overcoring or whatever postmortem you might do if you would plan that level of looking at where the chemistry is, where the precipitations are?

PETERS: Yes. I mean, the single-heater test, we overcored some of the chemistry holes to look for exactly that and I would suspect it's a long time away, but I would guess there would be an overcoring program and it will focus heavily on chemistry.

NELSON: Okay. Let me ask one question about thermal conductivity. Thanks very much for sorting the tests in the

lower lith. We look forward to the information.

PETERS: You're welcome.

NELSON: But, I'm wondering, given the importance of water content, how are you going to evaluate the water content at the point as you make the bulk measurement?

PETERS: As we're making it, that's difficult. We're going to try to do some saturation measurements of core before and we're going to log it, neutron log it, to try to get an understanding on water content. But, during, right now, it's simply a set of thermocouples and a heater and we're heating it up and cooling it down.

NELSON: Okay. Well, this sort of leads into a question that's in general that I think the Board has asked about and it has to do with the numerical analysis of the thermal hydrologic experiments in THC experiments as they get increasingly complex. I'm aware that there's been some problems with some of the modeling of the complex situations, I think, and maybe some different results from different models that have been used to analyze or to predict. I may be misinformed, but I recall talking with someone about some of the tests about the trend model of the drip shield and the effect there and having some model instabilities develop at tremendous durations for the modeling process itself when NOUGH was used.

So, I guess this is a general comment in terms of

the tool that the project is going to use to analyze or to understand the results of experiments that are run. What is your--or the projects thinking about the models that have been used, the codes, the analytical codes, that have been used in terms of are they the right ones, are they performing well for more than one source, more than one kind of an analysis, are they giving results that make sense? You might not be the right person to ask this of.

PETERS: That's--

NELSON: But, I think it's a question that keeps coming up in terms of thermohydrologic modeling and THC modeling about many of the experiments that have been run. I sort of use the word "experiment" instead of test for something like this because it really is more of an experiment. But, such a model could be used actually to predict what was going to happen with water content in that lower lith test, for example.

PETERS: Well, specifically, we are going to do that with this test. I'll speak for myself. There's people in the audience who can defend the models and the codes much better than me. That's not my bailiwick. But, yeah, there's some instances where we've had some instances where different codes looking at a similar problem, we've had a hard time converging them. We're looking a lot at ANSYS and NOUGH, for example. That may have been the example that you were

alluding to. But, we're working through those issues. I personally have pretty good confidence in what we're doing now.

The other point I'd make is--and I lost my other point. Oh, yeah, simple calculations. There's been--I think Dr. Craig has made this point several times. Can you do this on the back of a napkin as opposed to using a massively paralleled coupled simulation. I've talked specifically with thermal conductivity. We've been talking to the folks about that. Can you just pull out--and do a local solution instead of using TOUGH II. They're thinking about that to try to build confidence in those kind of analyses, but that would be all I'd really be able to say specific to that and broader. Jim may want to comment some more.

BLINK: Jim Blink, Livermore. For the thermal test, the drift scale test, the single-heater test, and the large block tests, we used NOUGH and TOUGH II to analyze those tests implementing the models in a number of different fashions. Some in a full 3-D, some with 2-D slices stitched together. The models ran in reasonable amounts of time. Those tests did not have a drip shield in them. So, I'm not sure if that was the experiment that you were talking about. But, the results of the tests were comparable to each other. So, in a sense, the codes benchmarked each other and they were reasonably accurate in measuring or replicating the

temperature and saturation distributions observed in the experiments.

Maybe you could amply on the drip shield question? I'm not sure what experiment you were talking about.

NELSON Well, what I'm mostly interested in is that we have heard that there are several models that are being used and they have some different assumptions embedded in them, some different capabilities, and there are some models that are out there that are not being used that are out there finding codes that have been developed by others. And, is the project happy with the models that it has chosen? Is there a search or a plan to develop models more to develop maybe better confidence or greater speed or more stability in the models or is the project happy with modeling capability for thermohydrologic and THC modeling that it has right now?

BLINK: For the porous medium modeling codes, THC, and TH models, I think we're fairly happy. The TOUGH II family of codes is, I think, well-respected and used by many other programs besides Yucca Mountain. NOUGH is an outgrowth of that family of codes. Within the drift, the heat and mass transfer within the drift, in some cases the porous medium approach is not the best approach when air movement and boundary layers dominate the process and so we're using some computational fluid dynamics codes to investigate that; codes such as FLUENT and FIDAF. Those codes solve the full Navy or

Stokes equations. They're fairly easy to implement in two dimensions or in a steady state kind of problem. If you have a 3-D implementation of such a code, it typically gives run times that are comparable to real time. So, it's hard to be long-term situations. So, just as in the porous medium codes, we've had to get clever in the way that we use them in order to simulate thousands of year problems in hours today of CPU time.

We have to do the same thing with the CFD codes. And, we are doing that. Bob MacKinnon showed you yesterday some results using a CFD code and we have other results, as well. The ANSYS code which does a good job in a conduction dominated problem in the rock, but can't handle the movement of water and the phase changes, also has a CFD module and we've used that CFD module in the drift and got a reasonable simulation of the natural convective processes. And, ANSYS, of course, is the commercial code that's well-respected by the NRC.

NELSON: Let me just close by saying that I think the story of the codes and how they fit together is a good one to tell in a way that the public and interested people can try to understand this because it's--what's trying to be modeled is just incredibly important and the credibility of those codes and how they fit together and how they're used by the project is a story that needs to be told well.

WONG: Dr. Bullen?

BULLEN: How much time do I have?

WONG: 32 seconds.

BULLEN: Got it. Let me follow up on Dr. Nelson's question. Basically, we saw the update on the Atlas facility and I'm asking my questions in inverse order because of that. I doing the dimensional analysis or did you do a dimensional analysis or just a CFD calculation on the flow and transport from the Atlas facility? The reason I'm asking that is because yesterday we saw a couple of models of how the drip shield and how convective cells might be set up. Are you using the Atlas facility to benchmark those kinds of calculations on a full drift setup of convective cells that may be set up in the drift, itself? Like maybe turn the heater off, stop the flow, and see if the convective cells are set up? Have you thought about that?

PETERS: They're thinking about that and they're looking at the Phase II that was ongoing and we're also planning a followon phase, Phase III, that we're looking at involving moisture in the invert to see how well it moves moisture and those family of codes that Jim was alluding to would be used to try to--as a validation exercise.

BULLEN: Okay. Two more quick questions in my 32 seconds. I had the opportunity to take a Yucca Mountain tour for either the sixth or seventh time last week and I want to

compliment the Department on a great tour. That was outstanding. We took about 120 or 130 people down in the mountain and one of my students got to go along. During the tour, I want to know if I heard the information right. In the drift scale heater test, did you have a little bit of rock fall from the top and is that a surprise or would you have expected to see that? I guess, since you're still in the heatup phase, I guess I wouldn't have expected rockfall. So, do you want to comment on that?

PETERS: Do you want to see a picture of it?

BULLEN: Oh, you've got pictures.

PETERS: Yeah.

BULLEN: Sure. Show me the rockfall. That would be great. And, could you talk about was it expected or a surprise?

PETERS: Yeah. About three or four meters into the-- yeah, I'll get to that.

BULLEN: Okay.

PETERS: The answer is no, I don't think.

BULLEN: Okay.

PETERS: Three to four meters into the heated drift beyond the bulkhead in the crown, there was a slab about like yea that had pushed down the mesh. We noticed it when we were looking into the window. We've done a camera run and looked at the whole drift. It's pretty much localized to a

couple stations. Similar kind of thing in the crown. Again, almost fish looking type rocks, not real big, but the mesh is holding them up. We think it's because--remember, that was excavated with an Alpine. So, it's got very irregular surfaces and there's a Williams bolt that goes through it not to support the ground, but to hold up the camera rail. So, it's probably related to that, too, but we think it was just due to the irregular nature of the excavation, we got small slabs. It's on a key block type fault.

BULLEN: Okay, thank you.

PETERS: That's the story on that. We're continuing to look at it compared to MPBX data and other things, yeah.

BULLEN: That was the first rockfall you'd seen in the drift scale test?

PETERS: If you look down on the left when you look in the window and look down on the left down about the third canister, you see a little bit of--

BULLEN: Raveling or--

PETERS: A little raveling, but it's dust almost.

BULLEN: Okay, okay.

PETERS: But, it wasn't a fall. It got caught by the mesh.

BULLEN: Right. Last question. In the bulk of the experiments in the cross drift, is the TBM a source of the water or is the water all coming from condensate air flow

down the Solitario Canyon Fault or--

PETERS: We're making absolutely sure the TBM doesn't-- we're making absolutely sure that there's not a reservoir sitting back in the TBM where you're basically boiling-- horizon water, but I'm pretty convinced that it's not coming from the TBM itself. There's not a reservoir back there that's open to the atmosphere. All the lines are closed off, but we're continuing to absolutely confirm that 100 percent.

BULLEN: Okay. Do any of them leak?

PETERS: That's what we're confirming 100 percent.

BULLEN: Okay. Thank you.

CRAIG: You covered a lot of ground, Mark. You always do. There are two areas which you talked about which seem to be becoming increasingly symbolic for understanding of the UZ. One of these has to do with the cross drift and the other has to do with ^{36}Cl. Both of them are areas where there's ambiguity about what's actually going on which makes them particularly nice areas for testing one's understanding because you can make predictions and then see what happens.

In the cross drift, let me talk about that one first. You talked about doing analysis. Where do you stand with respect to actual models or do there exist models that make predictions for how much water might have been moved around? You know the heat source term, you know you've got TH measurements, you've got wind speed measurements in there.

It ought to be possible to do halfway decent modeling even now.

PETERS: And, they're just starting with water, that part probably. The UZ model has been looking at it from an ambient problem for two years now. Bo is just starting, those folks are starting to put the heat source at the back end to start looking at those kind of effects, but it's very preliminary.

CRAIG: Okay. So, at the present time, there is no modeling and it's--

PETERS: Well, it's ongoing, but I can't tell you results.

CRAIG: But, no results. Secondly, if it turns out that when you actually do the modeling there is a significant chance that this is seepage, then that becomes quite important because there are many predictions for what the seepage ought to be in this particular experiment, inadvertent experiment perhaps, but nevertheless, an experiment is under the Solitario and it's over in the section which is most likely to have seep. So, it's in the right place in terms of running good tests on the model or tests of the understanding.

PETERS: Right.

CRAIG: And, the question here is this might be a place to actually do some more experiments since there is the

possibility that these are real seeps. We don't know at this point, but they might be real seeps. In anticipation that that might be the case, it might be worthwhile to think about doing some experiments which are explicitly designed to test that and there have been such experiments proposed; the Ridge experiment, I think, is the name that's sometimes used for some of those.

PETERS: The Crest Alcove.

CRAIG: The Crest Alcove, yeah, where you put in a special alcove. Is there thought about going in that direction?

PETERS: As we go into '02, as Steve mentioned yesterday, the guidance will come over and we're going to consider--the Crest Alcove, so you understand, is an alcove, oh, around--just beyond the first bulkhead, but under the high infiltration area. Okay? And, it's an ambient--it's an experiment where we excavate an alcove and bulkhead it off. It's a planned alcove. There, you're presumably not having the influence of the heat source. That's something that we're going to have to consider in the '02 plan as to the timing of that whether we continue--because the complication is the bulkhead is in the way. So, we either continue the bulkhead experiment or you go do the Crest Alcove or you move the bulkhead down. So, we're working through that in the planning process.

CRAIG: Okay. Well, I--

PETERS: So, yeah, we're considering all that.

CRAIG: Yeah. I'm expressing my view that these two experiments are really important in establishing the credibility of the overall understanding of the UZ.

PETERS: Right.

CRAIG: Coming to the ^{36}Cl, you talked about a number of experiments which are underway, but one of the things that you didn't tell us and I hope you will now is what criteria will be used in order to decide whether this is or is not bomb-pulse chlorine coming through fast paths?

PETERS: The criteria, as you probably recall, that was used in the previous investigations was, what, 1200×10^{-15} was the bomb-pulse threshold. We're going to continue to use that as the criteria.

CRAIG: But, that is critically dependant upon the measurement technique so you need criteria that--you need criteria that will take into account the particular measurement technique that you use.

PETERS: That's actually dependent on a whole host of things that includes the systematic variations of function of field strength and a lot of other things that you're aware of, I'm sure.

CRAIG: Right. What I'm--

PETERS: It's not just the measurement technique.

CRAIG: What I'm getting at is you now have a very complicated situation where the measurement technique is heavily involved and I remain quite uncertain as to how. When you give the next presentation, I will decide whether you do or do not believe there is bomb--or whether I should believe or not believe whether there is fast path ^{36}Cl. I'm looking for some sharp criteria which I can use the next time you show here to find out whether it is or is not there.

PETERS: Okay, that's fair. Maybe next time, I won't have to do it. That was a joke. All right. No, I will--that's a good point and that's something that we need to make very clear as this thing closes out. That's a very good point.

CRAIG: They don't exist now?

PETERS: Well, I still maintain that a lot of that thresh--the threshold that we use was based on pack rat midden data and a lot of other data that isn't complicated by the leaching process out of a crystalline rock that we're dealing with. So, I can't go into much more detail that than that, but I'm not convinced that it's that difficult to provide that criteria. I'm not going to do it off the top of my head, but I don't think it's as complicated as you're thinking in terms of the threshold.

WONG: Thank you, Mark. I have a big long list of people who want to ask more questions, but I have to be a

nasty time cop and put them aside and rescue you so you can sit down.

PETERS: Thanks.

WONG: Our next speaker will be Narasi Sridhar from the Center for Nuclear Waste Regulatory Analysis and he will talk about corrosion research that's sponsored by the U.S. Nuclear Regulatory Commission. Narasi?

SRIDHAR: First, I want to thank TRB for inviting me to talk about the NRC and Center program on container corrosion-related issues. Then, I want to acknowledge all the people who have contributed to the program over the years and also put our standard disclaimer that this is a Center viewpoint. I'm not presenting necessarily the NRC's regulatory viewpoint.

Okay. The overall approach for our program is to identify risk significance of the various processes from two perspectives. One is a programmatic perspective. From NRC's regulatory goal, we are not necessarily generating all the data to make the safety case, but we are here to do enough work for us to intelligently ask the right questions and to analyze--assess the DOE's analysis and data from a point of view of their significance. The second thing, of course, is a practical thing. Our budget is lower than DOE's. So, we cannot do everything that DOE is doing. The second asterisk is to provide input to performance assessment, NRC's

performance assessment code, TPA. The third is to increase confidence in the models that we have used or abstracted models that go into the performance assessment program. And, finally, to assess the adequacy of DOE's data or analyses by evaluating classes of materials. By classes, I mean that over the years this shows the number of materials versus years in the program, the various years since the materials were designed for Yucca Mountain Project. You can see that the number of materials and the type of design area varied over the years. So, our program cannot generate data on each and every one of them in terms of long-term data, but what we want to do is focus on classes of materials. For example, the class of nickel-chromium moly alloys, and if we generate data to get confidence in the models, then we can assess the adequacy of the safety case.

The corrosion-related experimental program I'm going to talk about today have several components and I'm not going to talk about all of them today. We are looking at the evolution of the waste package environment. This is, of course, one of the most important issues for predicting the corrosion lifetime. We are looking at the container studies, both related to the corrosion, as well as the mechanical integrity. We're looking at the cladding issues, the drip shield performance, as well as looking at in a preliminary sort of way the performance confirmation tools. Today, I'm

going to talk only about these two aspects not because those are unimportant, because I have only a short period of time.

But, nevertheless, to set the stage for talking about the corrosion related experiments, I'm just going to show one slide on what we are doing in terms of the near-field environment. This may take the whole day if I want to talk about all the things they're doing, but just to give you a brief idea. One of the important issues is, of course, the deliquescence humidity of salt mixtures. We feel that using the pure salt--for example, sodium nitrate--may give an non-conservative idea of when water condensation occurs on a container. So, we feel that a mixture of pure salt is a more conservative approach and we are doing some confirmatory studies or at least planning to do some experimental studies to look at the effect of deliquescence humidity in salt mixtures.

The other is analysis we are doing of evaporative concentration of water. What is the chemistry of the evaporated water on the container? We are using software designed by OLI Systems mainly because this software can go up to high concentration solutions. We are also using MULTIFLO or reactive transport code to look at the temperature and relative humidity and chemistry of the drift surface. But, this code cannot adequately predict what happens on the waste package surface. So, we are using a

combination of these to get an idea of what the chemistry of the water is on the waste package surface.

And, of course, we realize that the presence of drip shield may influence the deliquescence humidity and the chemistry of condensed water.

In terms of an overall approach to predicting localized corrosion, I want to show you this cartoon to give you an idea of how we are approaching this. There are two potentials that are of importance in predicting when localized corrosion is going to occur. Localized corrosion, of course, is important because the rate of localized corrosion is many, many orders of magnitude higher than the rate of uniform corrosion. The dry period, of course, there is no aqueous corrosion. So, it's essentially oxidation and for the kind of container materials that are being considered right now, the rate of oxidation is very low. But, once water condenses, then you can have a good idea of corrosion modes. Let's say that the corrosion potential evolves like this because it's just a schematic. This is not an actual calculated result. But, let's say that initially there is very low corrosion potential and slowly as the temperature decreases, oxygen ingress, of course, and the corrosion potential increases. Maybe there is some radiolysis effects, you know, and other kinds of effects to increase the corrosion potential. If this corrosion potential goes above

this potential called the repassivation potential, then localized corrosion is triggered. Then, during this period where the corrosion potential is higher than the repassivation potential, you get growth of this localized corrosion pits. So, if you really want to predict the penetration depth as a function of time, initially it's a very low penetration rate because you have just uniform corrosion rate or a dry oxidation. Then, once the growth starts, the corrosion rate may be very high. So, in the sense of performance assessment what one needs to insure is that this penetration depth during the performance period doesn't exceed some critical depth related to the wall thickness. So, this is the approach that we are using to model the corrosion performance of the waste package.

What are the issues in approaching the corrosion performance of the waste package? Well, of course, localized corrosion initiation and growth is one of the most important issues in our opinion. Effect of near-field is something we are looking at. Effect of fabrication, I mentioned a little bit about that. This is not something we have examined in detail up to this point and we are going to look at this more closely in the future. There was some mention about the fact of minor impurities in the environment, such as lead and mercury. After the state's presentation last year, we are taking a look at this a little bit more closely.

Of course, the containers that are very highly corrosion-resistant may not suffer localized corrosion in which case the life of the container is determined by the uniform dissolution in the electrochemical parlance that is called passive dissolution because the materials are protected by an oxide film that make it inactive, the corrosion process. And so, what we want to do is measure this corrosion rate because these corrosion rates are extremely low. So, the conventional weight-loss techniques may not be sensitive enough to measure the corrosion rate and we also want to understand through modeling what the long-term behavior would be because most of these measurements are pretty short term.

The third aspect is the stress corrosion cracking. The question we are asking ourselves is is that a critical potential below which stress corrosion cracking is extremely slow or non-existent? We want to look at the effect of cyclic fluid superimposed on a static loading because this is something that the DOE is doing and we had not done it in the past and this is--we wanted to verify that this concept of a critical potential is still valid even if you have a cyclic loading imposed on a static loading. And, of course, we want to look at the effect on minor impurities.

Okay. In terms of a repassivation potential, I mentioned in the cartoon that if the corrosion potential does

not exceed the repassivation potential, you won't get localized corrosion. We've been doing the test for more than three years now. This is a little bit of a dated slide. The data extends quite a bit longer now. What we show here are two things. This is the band of repassivation potential that we measured using short term tests, tests that last only one or two days. These are tests that have been running for many, many months to years. If we apply a very high potential--so if your redox potential of the environment is very high, of course, we don't anticipate that the redox potential is going to be this high in the repository, but if the redox potential is high, then the localized corrosion occurs in a very short time period, within 100 seconds. As the redox potential decreases, it takes longer and longer to trigger the localized corrosion process. And, what we find is that if the potential is close to the repassivation potential, then--we, so far, have not observed localized corrosion over four years of testing. So, this increases the confidence in the conceptual approach that to evaluate the localized corrosion resistance of the container, we want to evaluate what the repassivation potential of that material is in a given near-field environment and what the corrosion potential, which is a function of the redox potential, is compared to that potential. And, if the two potentials are separated from each other, then we know that localized

corrosion is not a credible process.

Now, we have evaluated the localized corrosion as a function of several environmental factors, chloride being one of the most important deleterious element in the environment for localized corrosion and these are several of the alloys that DOE has considered over the years for the container materials. And, you can see that this is the repassivation potential plotted as a function of chloride concentration. It's a highly nonlinear behavior. For example, if you consider that the environmental potential is somewhere at zero, then 316L stainless steel would stop corroding at a very low chloride concentration, 10^{-3} molar; 825 will start corroding at a slightly higher chloride concentration; 625 will start corroding at even a higher chloride; and the Alloy 22 which is the current material will not suffer localized corrosion unless you come very close to saturation with respect to chloride concentration. So, using this concept, by incorporating this model in our performance assessment goal, we can also evaluate the idea of DOE designs on the performance of the container and that is one of the things we're doing.

SPEAKER: Could you just tell us what the pH is of this experiment?

SRIDHAR: These pHs are natural pHs. We have also evaluated--in the backup slide, I have shown a variety of pH.

1 The pH range we have looked at, of course, from 3 to 10.
2 The repassivation potential is not very sensitive to pH,
3 unlike the redox potential which is sensitive to pH.
4 Okay. We have also looked at the effect of
5 temperature on repassivation potential, as well as the effect
6 of temperature on uniform dissolution, and I'll mention that
7 later. We know from literature and industrial experience
8 that as you increase the temperature, you increase the
9 susceptibility of a material for localized corrosion. The
10 reason for that, of course, is because the repassivation
11 potential decreases quite a bit as you increase the
12 temperature. So, if the corrosion potential is somewhere
13 here, then you may not get any localized corrosion in this
14 regime, but once the corrosion potential exceeds this value--
15 say, for example, 100 degrees Centigrade--you still start
16 spawning localized corrosion. And, of course, that is a
17 function of chloride concentration. So, we have also
18 incorporated this information in our performance assessment
19 code to evaluate the effect of container temperature on
20 localized corrosion.
21 The other point I want to make here is that the
22 effect of temperature on corrosion processes cannot be
23 modeled just in terms of a single activation energy because
24 in this regime, you have uniform corrosion with a certain
25 activation energy. Once the localized corrosion starts, it

is a completely different mechanism with a different activation energy. So, the effect of temperature depends on what corrosion mode one is discussing.

The other thing of great importance in the materials performance is the fabrication history. We have looked at again the repassivation potential as a measure of the resistance of the material for localized corrosion as a function of temperature for three different conditions. One is just the (audible) annealed material that is not welded; sort of the baseline case. There's a very high repassivation potential at these temperatures, and as I showed before, decreases the temperature. We looked then at the welded material that is welded with the recommendation of the manufacturer. So, it is what you would expect to be a good weld, if you will. The welded material is almost as good as the (inaudible) annealed material. There's not too much of a decrease in localized corrosion resistance into the welded crusts.

These things are intentionally heat treated to ruin the material, basically. But, to evaluate what happens when the material is thermally cycled during various fabrication processes--for example, post-weld annealing cycles or laser peening, if the temperature stays at a very high temperature for a long period of time--and you can see that the repassivation potential comes down quite a bit lower than the

"as-received" material and in the range of the anticipated corrosion potential which means that this temperature, for example, welded and the "as-received" material may perform quite adequately, but if the material is exposed to this temperature for this time period, it may suffer localized corrosion. The point of this slide is not to say that the material is going to be exposed to this temperature, but is to have a warning bell in our minds that we want to make sure that any post-weld fabrication treatment should have some limits on the temperature and time cycles to which the material is exposed in order not to affect the localized corrosion resistance.

Okay. I'm going to switch gears a little bit and talk about uniform dissolution of containers. As I mentioned before, with the new materials that are specified, the localized corrosion resistance is quite high. So, the life of the container is determined by the uniform dissolution rate assuming that there is no localized corrosion that occurs. We measured the dissolution rate in the lab using relatively short-term tests and they lead to very low dissolution rate in the uniform corrosion mode. If you assume that these corrosion rates are valid over years and years, you can get very large lifetimes. But, I have to carry out these two assertions by saying that these are short-term measurements and so they do not consider defect

generation or metastable events. The defect generation is something that we are considering, metastable events is something that John Scully talks about once in a while. So, these are things we have not considered in this assessment and we have also not considered the effect of fabrication processes that I mentioned before. That is, you could bring down the localized corrosion resistance of the material if you do not adequately control the fabrication treatment.

Michael Farraday was one of the greatest experimenters in my opinion. He once remarked that there is nothing more practical than a good theory. So, one of the problems in assessing the uniform dissolution behavior of these alloys is to really have a good theory to say what you've measured over a short time period is valid over a long time. And, the passive dissolution of these types of alloys have been considered for over 200 years. Essentially, there are a couple of different models that are involved at this point. One is called a point defect model that I have sort of cartooned here. Basically, this shows that this is the alloy, this is the outside film. I've sort of exaggerated the size. Typically, the outside film is only a couple of nanometers thick. And, this is the aqueous phase that is in contact with the alloy. For the alloy to dissolve, there are several defects in the oxide film that move about. For example, the metal cation that are present in the oxide move

in this direction, as well as the oxygen vacancies that are positively charged, but as the cation vacancies move in this direction. The idea that we have here is that over the long time period, these cation vacancies can collect in the oxide metal interface, can either collect in sufficient quantity to have a void that would break the oxide film off or can accumulate inside the metal in sufficient concentrations. Now, in shorter modeling, these cation vacancies are ignored because they are very small in concentration. So, over a short time period, we can ignore them as being diluted by the alloy, but over a 10,000 year period, they may be significant. So, that is something that we have modeled by assuming various fluxes of these species. The problem experimentally is it is very difficult to measure these fluxes in a real system because it's difficult to get into that size scale, as well as the concentration scales.

So, one of the things we are doing is that we are examining the stoichiometry of the dissolution to get at this model in an indirect sense. I'm going to talk a little bit about that later. The other approach is to consider this to be a semiconductor. Typically, the outside film on a stainless steel is a P-type semiconductor and so the conductivity of the oxide which determines the rate of the dissolution is affected by various impurity species. That's the other alternate model that we haven't considered, so far,

but that we need to consider. So, we are at an early stage in this. We hope that by considering these models which are more fundamental in nature, we can get a handle on the long-term dissolution rate.

One of the experiments that we are doing--as I mentioned before, to measure these vacancy movements in these thin films is very difficult. So, what we are trying to do is to get a handle on stoichiometry of the dissolution. Is there a selective leaching of chromium or nickel or molybdenum in the alloy? To do that, we have an Alloy 22 plate that has a very small cavity that is machined into this. So, we placed the cell on top of this and we have controlled electrode chemistry and then we can extract solutions from here and this is, of course, is maintained at 95 degrees Centigrade or whatever is the temperature of interest and we extract solution from here and using capillary electrophoresis measure very sensitively the concentration of various cationic species to get indirectly at the rate of dissolution. So, these are ongoing experiments and I don't have results at this point to talk about.

Okay. Switching gears again, the stress corrosion cracking is another aspect of performance of the material. DOE's approach has been that stress corrosion crack growth can be prevented by laser shock peening to create compressive

stresses. One of the things we have considered is is there a critical potential for stress corrosion cracking? Can we say that if the corrosion potential of the material does not exceed a certain value that stress corrosion cracking will not occur? So, what we have examined here is a crack growth rate on a pre-cracked sample measured as a function of applied potential. This is the repassivation potential that I talked about before on a non-stress specimen. So, this is repassivation potential generated for localized corrosion and what we see is that you have very high crack growth rates, 10^{-9} meters per second, and below the repassivation potential, the crack growth rate reduces quite a bit. The arrows indicate that this is our measurement limit, and if we wait longer, we can measure lower crack growth rate provided there is no crack growth. So, we have done these tests for up to a year and--this is the kind of crack growth rate we observe--show that below the repassivation potential, the stress corrosion cracking susceptibility decreases quite significantly.

Now, these are some early experiments we did. Since then, we have done other experiments to improve our confidence in this type of approach. Again, this shows a completely different type of specimen called compact tension specimen and we have superimposed a small cyclic load on top of that at a very low frequency and currently the best time

is about 1,018 hours. We show that there is no stress corrosion cracking that we observed below (inaudible) the repassivation potential. This is on 316L stainless steel. In C-22, we have not gotten stress corrosion cracking because repassivation potential is quite high. So, these data which are ongoing, again help us improve our confidence in the conceptual approach that we are using for performance assessment.

The last thing we are looking at in terms of the stress corrosion cracking and localized corrosion is the effect of minor impurities and I don't have any data to show you because the experiments are still in progress. But, I'm going to show you the conceptual approach and some of the ideas that we have on other people's data.

The state showed some results that are very deleterious, in fact, of lead and mercury on localized corrosion and stress corrosion cracking of Alloy 22. But, the best temperature used by the state is very high, much higher than could be sustained under atmospheric ambient pressures under wet conditions. And, the pH that the state used in their original data presentation was very low. In previous experiments done at Haynes, we observed that stress corrosion cracking occurred at the low pH as even without lead. So, it is very difficult to evaluate that the lead or mercury exacerbated the stress corrosion cracking

susceptibility at these low pHs. The other thing is the range of lead and mercury concentrations that were used was quite high and we need to evaluate them in terms of the possible concentrations that could be present in the water that condenses on the container. So, we are doing some calculation using OLI and other software codes to look at the concentration of lead on the container surface and then carry on some stress corrosion cracking experiments.

We again want to do this in terms of the potential as a controlling parameter for stress corrosion cracking because we want to put all this experience on a map. I'll talk about this concept a little bit later.

Okay. The next aspect is to look at the use of analogues. I know the TRB has been interested in multiple lines of evidence and other ways to look at the same problem. So, this is an area of interest for us, also. There can be different types of analogues. One is, of course, the archeological and natural analogues that have been proposed and studied. Josephenite has been looked at recently. Iron has been looked at quite a bit in the past. Bronze and copper have been looked at by us and by others in terms of what they can tell us about the performance in metallic objects over long time periods. We shouldn't forget that there's also industrial experience with these types of alloys. Alloy 22 may be new, but there are other similar

alloys that have been in existence for quite a long time. So, we can gain some experience with these provided we know how to put all this experiences in a map, in a given map, in a common frame of reference. That is the most difficult thing to do.

Before we do that, I wanted to give you a brief history of Alloy 22 to show that these alloys and similar class of alloys can be looked at as a group. Haynes was the first one who looked at nickel chromium alloys quite early, 1898. In fact, I want to give you a brief side story on Haynes. He was starting on nickel chromium alloys because he was the first inventor or builder of automobiles in the US. He was one of the first ones, anyway. And, he wanted to increase the life of engines. So, he was looking at nickel chromium alloys as a way to do that. He was also building better kitchen gadgets because his wife was complaining that knifes were blunting and localized corrosion was one of the reasons for that. In fact, Haynes was also the first guy who got an automobile speeding ticket in the US. He was the first builder of the car and on the 10th anniversary of the automobile building in this country, he was asked to lead the procession in New York using his first car. And, Haynes had built much more improved versions by then, but he didn't trust the first car he built. So, the previous evening, he was taking it for a trial run. He was going all of 20 miles

an hour, but he got a speeding ticket. So, the lesson I take away from this is you have to design your performance confirmation program carefully.

After Haynes looked at nickel chromium alloys, there have been many, many improvements over the years. Alloy-C was developed by Union Carbide in the '30s; C-26 was developed by the Germans in the '60s; C-4 was developed by Haynes in the '70s and then C-22. Since then, there have been many other improvements in these alloys.

So, the idea that I'm talking about here is not necessarily to look at the metallurgy of all these alloys, but to put the experience gained in all these alloys over a long period of time in a common map, a common frame of reference. We believe that even though many of the corrosion tests done in these various applications have not systematically collected the electrochemical information, we can guess at some of their electrochemical information and put them on a common frame of reference using the repassivation potential concept. That is an idea that we are considering.

The other idea is looking at analogues and one of the problems in looking at analogues is similarity in electrochemical response. If you look at Josephenite which is a nickel ion, essentially nickel ion intermetallic, it doesn't have the same electrochemical response as Alloy 22.

The protective film on a nickel ion alloy is much less protective than Alloy 22. So, in a chloride containing environment, it won't provide the same electrochemical response. So, one of the alternatives that we are thinking about is intermetallic that is present in nature called ferchromide. Of course, it's not present a lot in nature, but apparently it's available in the native state. So, what we want to look at is get some mechanistic information on this kind of an intermetallic mineral analogue in terms of what kind of localized corrosion mode is present on this material, whether the environment that this was exposed to can be characterized adequately and used to confirm conceptual model.

The other information that we can get from these analogues is the localized corrosion. For example, many mineral artifacts have suffered localized corrosion and we can look at the mechanism under which they suffered localized corrosion and compare it to the localized corrosion mechanism that we have in Alloy 22 and other similar newer alloys. And, of course, as I mentioned before, we can improve the confidence in the conceptual model, but putting all this knowledge on a single map.

So, one of those proposed approaches that we are again in a very preliminary stage in this is to investigate the mineral assemblage of this mineral, ferchromide, identify

whether any specific corrosion mode is present, assess the geochemical history associated with the mineral, and then compare it to model predictions. For example, knowing the composition of this, perhaps we can get a repassivation potential, and knowing the geochemical history, perhaps we can get history of the corrosion potential, and now we can compare the two and see that over a period of a long period of time whether the conceptual approach of comparing these potentials to predict localized corrosion are still valid or not. Of course, there is a lot of stumbling blocks along the path. So, for example, the geochemical history is often very poorly known. We may not be able to characterize repassivation potential of this mineral very accurately because it's dependent on impurity content and so on and so forth. But, this is the kind of thinking of how to approach this kind of material like C-22 from a natural analogue point.

The last thing is to talk about performance confirmation. Performance confirmation improves, obviously, the confidence in the models and laboratory data. Performance confirmation can include many approaches that include laboratory tests and this is in the performance confirmation plan that DOE as put out the preliminary plan. It can include field tests and monitoring. Our main concern or consideration in this is really to look at the sensor

performance. We know that different sensors can be used to monitor, but how would the sensors perform over a long period of time? The sensor light becomes an important consideration in long-term monitoring of the waste package. For example, we know that in the early tests at the Climax mines, a lot of the sensors that they used for temperature measurement that the nickel ion alloy corroded very rapidly. So, we know that the sensor performance is an important consideration and need to be included in the plan quite a bit ahead of time, not as an afterthought.

So, to look at sensor performance, we have a pretty simpleminded approach as to how to simulate a drift test. Now, I want to throw in the disclaimer that we are not evaluating the hydrological model. So, I don't want the hydrologists to jump on my case. We are really evaluating the sensors. So, the approach is very simpleminded. We have a mesh that's made of stainless steel that is surrounded by the crushed tuff from Yucca Mountain to simulate the drift space and we have a variety of sensors that is put in here along with a heater that is coming from the back. And then, we have a water equilibrated with tuff to simulate ground water that is at the top of this. So, the heater evaporates the water. You know, you have this evaporation/condensation cycle and you have the drift through the--and then, you are evaluating the sensor concepts.

We had three different sensor concepts. One is called a sensor array cell. That's got different kinds of reference electrodes to make sure of the corrosion potential over a period of time. We have a corrosion coupon that is just a conventional resistance tool to measure the change in resistance as the wall thickness decreases. Of course, a problem with this is for alloys like Alloy 22; this is a very difficult thing to incorporate, to use. And, we have a galvanic couple sensor that has bimetallic couple to detect onset of corrosion process and that's something illustrated in the next viewgraph.

Basically, it's a sensor that's kind of substrate material, that is either Alloy 22 or whatever material that they're interested in. It has an insulating layer and then has a silver or some other conductive layer on top. So, the insulation prevents these materials from electrically contacting each other, but when there is a water droplet, it condenses and generates a current that is measured very sensitively. Now, this current is not related to the conductivity of the water, but it's related to the electrochemical response of the substrate. So, you can measure, for example, the effect of chloride concentration. The current is very sensitive for chloride concentration. At the low chloride, the current is very low; as you increase the chloride concentration, the current increases. It's also

sensitive to relative humidity as you would expect. So, when there is a condensation occurring, the current increases showing that there is a water film that is present. So, this is the kind of sensor that we are evaluating. The reason is because reference electrodes are extremely sensitive in terms of their performance and often degrade very rapidly in this kind of hot and wet and dry environment. So, we feel that perhaps a galvanic sensor with two different metals would be a better approach. The purpose is not really for us to design the performance confirmation program, but for us to really understand what are the issues in a performance confirmation program. This is one of the things that we're evaluating.

Okay. Last is to summarize what I've presented, so far. We feel that over the years that we have shown that this concept of repassivation potential can be used not only to predict localized corrosion, but to predict the onset of stress corrosion cracking. We strongly feel that the fabrication effects, especially the post-weld annealing and laser peening need to be studied in greater detail to really understand and also to develop recommendations on what should be the criteria, the window to be maintained for good performance. We feel that the long-term passive dissolution needs to be better understood. We have some shotgun data, but we need to understand long-term dissolution processes

better. We feel strongly that sufficient thought should be given to understand what kind of tools and the limitations of these tools for performance confirmation.

That's all. Thank you.

WONG: Thank you, Dr. Sridhar. Questions from the Board?

SAGÜÉS: Thanks for your presentation. I'm very glad to see the emphasis that your program is placing on establishing the validity of the modeling assumptions. I think that that's very encouraging and I think that the (inaudible) certainly looks promising to obtain answers to questions that have been concerning many of us and the program, of course.

A specific question. On your transparency, the one where you have the repassivation potential as a function of time--

SRIDHAR: This one?

SAGÜÉS: Yes. I think that that's a revealing transparency, and for the benefit of my colleagues and those in the audience, I think that we want to look at the time scale of that event. That is, of course, you create an upset on the system and then you observe the amount of time that it takes to do something. And, I've got a pointer here. You know, over there, that's 1,000 days. So, that's about a year over there. So, if you go in the same time scale, the 10,000 years will end up being somewhere over there in that

1 particular scale. And, over there, we're talking about just
2 seconds, something of that order. I think that this shows
3 very much the kind of challenge that we are all faced with.
4 We are trying to get here orders of magnitude ahead of normal
5 experience. These tests are like about three years test.
6 Even if you were to make 10 years test, well, you'd just be
7 moving like over there. If you were making 100 years test,
8 you could be just in that part of the system. So, I think
9 it's quite clear that it's hopeless to obtain evidence for
10 the purposes of establishing what's going to happen at the
11 repository solely with (inaudible) evidence. There has to be
12 a modeling approach behind that.
13 And, I guess, since we have this picture in here,
14 how do you think this kind of evidence that you've got here
15 with three years tests, this particular experimental evidence
16 by itself, how relevant is that in trying to predict what
17 would happen in a package that has, say, moisture condensed
18 on itself because of deliquescence and so on over very, very
19 long periods of time?
20 SRIDHAR: Yeah. By itself, this is not sufficient to
21 say that something will last for tens of thousands of years,
22 but the approach we are trying to use is to break the problem
23 down into two prongs. One is the repassivation potential and
24 the other is the corrosion potential. We feel that all this
25 experiment does is increase our confidence. It cannot

validate something. It just says that based on whatever period of testing that are done, we feel that the repassivation potential is a good parameter to predict onset of localized corrosion. Now, you have to take it to the next step to say when is that a repassivation potential? We have done some work to show, for example, that repassivation potential is related to metal chloride salt film formed at the bottom of the pit. Now, that can be assessed in a fundamental way. Then, maybe we can say that over a long period of time, perhaps that metal chloride salt film may or may not form under the conditions and so you may have a repassivation potential at a certain value over a long period of time.

The other thing is the corrosion potential. Perhaps, our geochemist colleagues and ourselves can work together to define the near-field environment over a long period of time and maybe knowing the kinetics of various processes in a fundamental way, calculate the corrosion potential over 10,000 years. So, that's only hope for predicting the long-term is to break the problem down into more fundamental pieces which you can model using a sounder approach.

But, I agree with you that by this empirical evidence alone, I cannot prove in a conventional way. All I can say is I have increased confidence in the approach that

they'll use. I don't know that I answered you, but--

SAGÜÉS: Sure. One followup question that we always have been asking is, all right, let's look at the corrosion potential part of it and you indicated the logical approach which is looking at the near-field, the prediction. How long do you think it would take with the current level of resources and individuals available and luck in getting results or lack of luck, how long do you think it will take to answer that particular question; to try to bound that corrosion potential and say, look, it is virtually impossible it's going to go below or above 400 millivolts? Well, the likelihood of going above 400 millivolts is so small as to be negligible.

SRIDHAR: I don't know whether I can put a time frame, but I think we are taking sort of an iterative approach. We've already done some modeling of corrosion potential. So, we have shown through that modeling that the corrosion potential that we calculate is reasonably close to the corrosion potential we measured over a short period of time. Then, we have done this point defect model which supposedly predicts a steady-state corrosion potential. We haven't gone very far in that, but one of the things we have concluded is that if the point defect model is valid, then the corrosion potential and the passive dissolution rate is dictated by the vacancy movement inside the metal. That is a very slow

process because it's a solid state diffusion and has a very high activation energy. So, we believe that if you would carry that process through, then we can get a handle on the corrosion potential in a steady-state situation. Now, the question is how valid is the point defect model? We need to evaluate. We haven't gone very far in that.

In terms of a time frame, you know, it's--again, I cannot give you a time frame, but it's an evolution. We have done some work and we hope in the next two or three years that we'll continue to make progress in getting a better handle on this. So, I think that's about all I can say.

SAGÜÉS: One last question. You didn't mention transpassive behavior. In the high moly alloys that is a concern granted that's usually observed at relatively high temperatures and high potentials. Have you tried to quantify this a little bit and tried to guess whether transpassivity is or is not something to be seriously concerned about?

SRIDHAR: I don't think transpassive dissolution under the repository condition is reasonable. In the backup slides, I have shown the defect potential on dissolution rate. What we expect under repository condition is in the shaded rectangle. So, within that shaded rectangle, the dissolution rate is 10^{-8} times per centimeter squared which I translate into, you know, roughly about 30 to 100,000 year lifetime. Okay? And, there, you can also see that that

dissolution rate is relatively independent of pH and chloride concentration which one would expect because it's governed by the movement of defects through the oxide film. The transpassive dissolution occurs above this potential where the dissolution rate increases drastically. But, in my opinion, the repository redox potential will never get that high unless you have some radiolysis effect which also is transmitted. So, my opinion is that I don't think it's of interest really for us to map out the whole behavior, but I think we can assume that the repository behavior will be more close to in this region, at least that's the way they're calculating now.

WONG: Thank you very much. Dr. Bullen?

BULLEN: Bullen Board. We, as scientists and engineers, are always very tantalized by data. So, if you'd go to the previous slide in the backup which is #32, you show the uniform corrosion rates that you'd measured. It's that table.

SRIDHAR: Yeah, I've got it. Okay.

BULLEN: And, I guess, I just had a couple of quick questions. These are short-term corrosion tests. How long do these take?

SRIDHAR: These are short-term tests on the order of days and using electrochemical test techniques. So, you measure the current densities--

BULLEN: Right. So, you did measure current densities. And, I guess, the other question that I had dealing with this is that you came up with a number of this 28,000 as your "as-received" with a very low pH and I understand that.

SRIDHAR: Yeah.

BULLEN: 28,000 for extended lifetime. Yet, when you use the aggressive in your TPA calculations, they went all the way to $2x10^{-7}$ for your current density. So, you ended up with a 10,000 year lifetime. Is that pretty aggressive?

SRIDHAR: Well, I will say that the dissolution rates we used in the PPA code were used before we generated the data.

BULLEN: Oh, okay.

SRIDHAR: So, we sort of took a conservative estimate. We said, well, most of the passive alloys dissolve at this rate. So, we put the rate in there. Then, as we were running the TPA code, we were doing experimental work simultaneously and we said, well, if you'd really do the passive dissolution experiments, the dissolution rate comes down to time and so we measured a little--in fact, when we are doing longer term experiment, we are measuring lower dissolution rates than that.

BULLEN: Okay. Along the same lines with these data, last week at the International High Level Waste meeting, I had a hallway discussion with one of the international representatives of the corrosion community, a yet to be

unnamed professor at the University of Western Ontario, if you want to know who David Shoesmith is. Anyway, David mentioned that there was some German data that suggested that in the temperature range of interest for the waste package that there was really no temperature dependence or very little temperature dependence with respect to the bulk dissolution. Yet, your data between 95 and 20 show an order of magnitude difference in the measured corrosion rate based on your--and not on the solution rate.

SRIDHAR: Right.

BULLEN: So, are you familiar with the German data and do you think there is temperature dependence or not?

SRIDHAR: It's my lost data, I think, is what you are referring to.

BULLEN: Yes.

SRIDHAR: I think the Smellows (phonetic) data was a longer term data, first of all. So, we believe that if you do a longer term test at these temperatures, the dissolution rate will come down and perhaps the differences with respect to temperature may not be as great. They have to be verified, but that's my opinion at this point.

BULLEN: Okay.

SRIDHAR: But, we do show an order of magnitude increase in dissolution rate with them.

BULLEN: Do you have a model that's temperature

dependent for the uniform dissolution rates or has that not been developed yet?

SRIDHAR: The point defect model is one thing we have evaluated, but we have not looked at the active temperature on it at this point. We only looked at what had happened in the long-term, to carry the model a longer term.

BULLEN: Right. After the thermal pulse, it doesn't matter?

SRIDHAR: Yeah.

BULLEN: Okay. The last question is actually on your Figure 16 if you can dig that one out.

SRIDHAR: My figures are getting scrambled.

BULLEN: Oh, I know how that goes. Mine are still in order because they're stapled together.

SRIDHAR: Okay.

BULLEN: You noted that basically with the compact tension specimens and cyclic loading that you had essentially no stress corrosion cracking on C-22. But, John Scully did also some cyclic loading tests and found some--

SRIDHAR: Peter Andreson.

BULLEN: Oh, that's right, Peter Andreson did some. What were the differences between your experiment and his and why did he find an effect and you didn't?

SRIDHAR: There could be a couple of differences. Peter does the cyclic loading before using the environment and

slowly moves into the environmental range. We do the pre-cracking outside the environment, put it in the environment, and then do the cyclic. So, Peter explains that that creates more susceptibility. The other thing is Peter has just followed the changes in the inverse--the voltage gradient and uses that to calculate the--inferred crack load. We have looked at after the forced test in an SEM to infer that there is no crack load. So, we--you know, we have some questions that need to be resolved as to whether the--Peter swears up and down that his voltage measurement is good and he can believe it over years of testing and perhaps he's correct. He has done lots more work in that area than I have. But, those could be the differences.

BULLEN: Thank you.

WONG: Dr. Runnells?

RUNNELLS: Runnells, Board. We seem to be sort of working our way backward through your slides. Could we have 31 in your backups, please? This concerns something you said right at the front end of your talk, mainly--it's that slide that shows the deliquescence humidities. You mentioned right at the start of your talk that the particular mixtures of salts are important in terms of the temperatures at which a brine will form on the surface of a material. I agree 100 percent. I wonder if you could just explain the significance of this particular slide in the context of your concern about

what is the composition of the pore water, what is the composition of the water that will be in touch, in contact with the canister?

SRIDHAR: I think, we could go either way in terms of performance. Of course, the first significance is if the deliquescence is lower, then you start forming an aqueous solution at an earlier time period in the history of the container. So, we had to start kicking off the aqueous corrosion process at an earlier time period. The second aspect is that if--in order to get a lower deliquescence point, if you have a concentrated solution of nitrate and chloride, nitrates are typically corrosion inhibitors. That means they would lessen the tendency for localized corrosion. So, I would say that if you had a chloride/nitrate mixture, one would expect that the corrosion tendency would be even lower even though water would form at an earlier time period.

But, having said that, another caveat that we need to evaluate further is the corrosion mode of this kind of alloy depends critically on the chloride to nitrate ratio. That is if the chloride to nitrate ratio is very high, then your susceptibility to localized corrosion is higher as the nitrate concentration increases. So, if the ratio decreases, the corrosion susceptibility decreases because nitrate is an inhibitor. But, if you have a lot of nitrate, then the corrosion susceptibility can increase again. So, it's a

question of what exactly is the value? That could be a minimum. And, we don't know what the minimum point is at this point and that's one of the things the geochemists are evaluating. What will be the concentration? And, be one to come back behind them and say, okay, let's evaluate some different ratios of nitrate and chloride to make sure that we understand this and this process better.

RUNNELLS: So, you're in touch with the people who are actually evaluating the appropriate composition of the fluids?

SRIDHAR: Yeah, right. Yeah, it's an interactive process. We have done in the past experimental work to map out what regions are susceptible to localized corrosion and the Liverpool chemistry folks have come back and calculated to see if the near-field environment would fall into the regions of susceptibility. Now, as they are calculating these other factors, we need to go back and do some experiments to make sure that our regions of susceptibility doesn't increase or shrink.

RUNNELLS: Okay. Thank you.

WONG: Any further questions from Board members?

(No audible response.)

WONG: Board staff?

MELSON: Bill Melson. It's Bill Melson with an M. This is a question I had for the previous presentation also. That

is how do you think biological activity, particularly bacterial activity, could change your results and are you controlling for them?

SRIDHAR: I don't know exactly how, but we are evaluating two different path. One path would be--and, again, in our conceptual framework, you have these two potentials. One path would be, for example, in seawater, biological organisms are known to increase the corrosion potential. So, if you have a given repassivation potential, if your corrosion potential increases beyond that, then you'll stop kicking off localized corrosion. That's one path. So, we have evaluate some to a certain extent. We haven't done nearly the same amount of work that is necessary to complete our evaluation.

The second aspect is some biological organisms--for example, self-introducing bacteria is one--that can produce the repassivation potential. So, your corrosion potential stays the same, but your resistance of the alloy comes down. So, that's another aspect that we need to look at. To handle that, what we have done is we have intentionally added, for example, a few ppm of (inaudible) sulfate to the solution to see how far it comes down. Now, the microbiologists would argue that that's a lousy way of looking at microbial corrosion, and granted, that is correct. But, that's a quick way for us to see if the corrosion

resistance comes down. What we feel is that having done that, the lowering of repassivation potential to us seems to be not sufficient to spawn localized corrosion process on Alloy 22.

WONG: Okay. Thank you, Dr. Sridhar. We are now scheduled for a break and we will begin promptly at 10:50.

(Whereupon, a brief recess was taken.)

WONG: Welcome back. I hope you all had a lot of coffee. The next series of papers are related to fluid inclusions. One of the challenges to repository development is entering. The question is to whether or not the groundwater will rise to the repository horizon. One of the key indicators of this is the study and characterization of fluid inclusions. We have a series of speakers that will go from now until lunch that will speak about this. There have been a number of studies that looked at the accumulated evidence and I think that the next group of speakers again will provide their findings and interpretations as to, again, the growing body of evidence.

The next set of speakers will be representing a joint study by the University of Nevada at Las Vegas, the USGS, and the State of Nevada, and I believe that much of this work is again sponsored by the DOE. I need to express special thanks to two of our speakers, Jean Cline and Bob Bodnar. AS I understand, they just flew in from Europe and

1 their time might be a bit off.

2 So, with that, I'd like to first ask Jean Cline

3 from the University of Nevada at Las Vegas to provide us with

4 her presentation.

5 CLINE: I would like to thank the Board for inviting us

6 here today to present the results from our study. As most of

7 you know, the discovery of two phase fluid inclusions a

8 couple of years ago led to the first evidence that fluids

9 with elevated temperatures could possibly move through the

10 repository site. The Nuclear Waste Technical Review Board

11 reviewed some of this information, and as a result,

12 recommended that DOE consider funding a study to try to

13 constrain the timing of this fluid movement and to confirm

14 the presence of these fluids. That's what I would like to

15 tell you about today.

16 When we began this study, there were four questions

17 that we addressed. These are those questions. When we

18 began the study, we were not sure that we'd be able to answer

19 all these questions, but I'm happy to report that we have

20 been able to.

21 First of all, we wanted to confirm whether or not

22 there was a hot fluid record at Yucca Mountain. Secondly, if

23 this record was present, we wanted to determine what the

24 temperature range was. Third, we wanted to determine how

25 widespread across the repository site this fluid record had

been recorded. And then, finally, the most difficult part, we wanted to constrain the timing of this fluid record.

This is just a brief overview of where I will go today and what I will talk about. I'm actually going to spend a fair bit of time talking about paragenesis which is really putting together the timing of the different events that are recorded in the rocks. The paragenesis is essential. It gives us the geologic constraints for all the other studies that we do. It's been critical in constraining the fluid inclusion information and the geochronology studies that we've also conducted. I will finish up by giving you our conclusions. I will tell you what those conclusions are right now so that you can sort of think about them as we work through the procedures and look at the data that we've collected.

First of all, there is a hot fluid record at Yucca Mountain. We did confirm that. The range of temperatures average about 45 to 60 degrees Centigrade, although temperatures are a bit higher in one area and a bit lower in another area. Third, this fluid, the records of fluids with elevated temperatures is observed across the entire repository site. Then, finally, this record is recorded in the oldest calcite field as secondary minerals. There's some record in somewhat intermediate minerals, but there is no record of the passage of these hot fluids in the youngest

secondary minerals. And, as I talk about the geochronology study, I'll give you some more absolute constraints on those times.

Okay. The first part of the study, the first step that we had to take was to collect samples. And so, we collected 155 samples from throughout the ESF and the ECRB. Our goal was to collect samples at least every 50 meters and we pretty much accomplished this. In a couple areas, samples are further apart than 50 meters and that's because there simply was not any secondary mineralization to collect in those sites. Our goal was to collect all types of calcite that we found and we did that. We collected thick crust and thin crusts from the lithophysal cavities. We collected samples from fracture fillings including some of the very thin fracture fillings and we also collected some breccia samples.

Okay. As I said, I'm really going to focus on the paragenesis study because now that is really key. These were the tools that we use to constrain the paragenesis study and again this involves putting together, constructing a time history or a growth history or a precipitation history for these secondary minerals in these open spaces. The two most important tools, the tools that really told us the most, were the straightforward petrography and then the chemistry as mapped on the electron microprobe. I will say a little bit

about carbon and oxygen isotope data, as well.

Okay. From each of our 155 samples, we had thin sections made and we studied those again to determine the growth history of each of these samples. This is one of the nicer lithophysal cavity samples. And, essentially, what we see in most of the samples, but not all of them is that we have open space mineralization, mineralization that precipitated in open space from the base outwards. Here's the base of the sample, here's some of the tuff. Our oldest layer is here. It's somewhat finer, a more blocky calcite. This layer was then overgrown by these long thin calcite blades. Then, the last layer to precipitate in this sample is this outermost, somewhat more blocky, more equant Sparry calcite.

This is an example of one of the slides that's not as easy to figure out. This is one of our breccia samples. What you see here are several pieces of tuff that are at various angles. They've fallen into a cavity somewhere and then they're cemented by open space mineralization. What we see here is that it's much more to difficult to figure out where the old calcite is, what's young calcite. It's more difficult to figure out the growth history of these samples. We can very clearly see, though, that the outer surface, the upper surface, is not necessarily the youngest mineralization. So, it takes a fair bit of study to actually

put together the paragenesis of the growth history in samples like this. And, this is where the chemistry and capital luminescence and to some degree the isotope information was helpful in doing this. But, that has to be done before you can put your dating and your fluid inclusion information in the appropriate geologic context.

Okay. Here, we're looking at the outer portion of one of the sections. I forgot to mention in the previous picture that I showed you, samples were about three centimeters wide by about two centimeters thick. And, here, we're looking at the outer edge of one of the samples that shows some especially good textures. What we see here are some of these calcite blades very clearly exhibited and then these calcite blades are overgrown by the youngest blocky, in some cases dark and grungy, Sparry calcite. And, inter-grown with this calcite is opal.

Okay. This particular layer ended up giving us some information that was really crucial to allowing us to tie samples from sample site together and that's because this outermost Sparry calcite is chemically distinct. It has a chemical fingerprint that we can trace in samples across the repository site. What this Sparry calcite contains is oscillatory growth zones, some of which are enriched in magnesium and they don't contain a lot of magnesium; only as much as about one weight percent. But, this feature is

consistent. What's really key is that this layer always forms the outermost and youngest layer of calcite that was precipitated across the repository site and again it can be traced because it's chemically distinct.

Here, we're looking at some of the electron microprobe images that allow us to figure this out. On the left, we have a back scanner electron image. What that image shows is atomic weight. So, all of this medium gray material is the same stuff and it's calcite. And, back here are some holes in the section. Then, in the somewhat darker gray, we have opal mineralization. On the right, we're looking at exactly the same bit of calcite in the same section, but we're looking at a magnesium map. This dark area down here, the base of the section, the older part of the section, is free of magnesium, but the outer and youngest part of the section has really beautiful oscillatory growth zoning. Okay? And, what's important is that you really can't see that zoning. You can't pick this up on petrography alone. Again, key, because it always forms the youngest and outermost layer and it can be traced across the site. We have found this outer magnesium enriched layer in approximately 70 percent of our samples from locations across the site where we do not see this layer. That latest layer simply did not precipitate.

To sort of drive this home, I'll show you two more

slides. This first slide again shows that layer. Here, we have the base. The base of the sample would have been down here. Oldest, earliest calcite again somewhat blocky. Then, overgrown by long, thin blades of calcite. Then, the outermost surface is the magnesium-enriched Sparry calcite. You'll notice that we have a somewhat regular surface because of this overgrowth of these more equant Sparry calcite crystals. So, here's a sample in which precipitation of this latest layer was recorded.

You can contrast that with this section from another locality where we have basal calcite, and then overgrowing that, we have these bladed calcite crystals, but we do not have any of the Sparry calcite overgrowths. So, this sample site, this sample did not record precipitation of this latest event. And, it's quite obvious when you look at the samples because, as you can see here, the outer surface is very rough.

Okay. We looked at all 155 of our sections. We made paragenetic determinations for each of those sections. We put together essentially a growth history for each of those sections and then we summarized those individuals paragenesis on this schematic diagram. The way to look at this diagram is to essentially take vertical slices through different parts of the diagram as you move across the diagram and you will see the different paragenesis that we observed

at different sample sites.

Now, the earliest or oldest part of these samples shows a fair bit of heterogeneity across the repository site, but as the samples become younger, as we look at the outermost part of the samples, things become more homogeneous. What we very commonly see in most of the samples, but not all of them, is this nice bladed crystal, the calcite, shown here and then they are usually overgrown by a Sparry magnesium-enriched calcite shown in gray which again is inter-grown with opal. However, that event did not precipitate anywhere. In some instances, we just have the bladed crystals; in some cases, we just have these tiny little tips that began to precipitate.

Okay. Now that we've put together our geologic context, essentially drawn our geologic map, we can begin looking at the fluid inclusions. This is our sample location map again. What we've now done is add in red the locations of all of those samples that contain fluid inclusion assemblages with two phase fluid inclusions. But, a key point is that calcite or secondary minerals from all of the locations, all of the sample locations, do contain fluid inclusions. Many of these inclusions, however, are only one phase inclusions, they're liquid only inclusions, and they do not record these higher temperatures. Some of these inclusion assemblages have, in addition to one phase fluid

inclusions, the two phase fluid inclusions. These are the inclusions that were trapped at somewhat elevated temperatures and then cooled down and nucleated a vapor bubble. So, it's those sample sites that contain fluid inclusion assemblages with two phase fluid inclusions which are indicated in red. You can see that those locations are sporadic, but they do occur across the repository site.

Here's what some of the inclusions look like. Most of them are what we refer to as primary inclusions. They were trapped along growth zones as the minerals formed. Here, we see some nice bladed growth zones. A lot of these inclusions are empty. They look empty, but they actually contain just liquid. But, I'm hoping that you can see that a number of them have tiny black spots which are the vapor bubbles in them.

Here, I'm showing you all of our fluid inclusion data. What we did was to divide out the repository site into six different areas. In general, the geology is similar throughout the repository site, but there are some differences in the paragenesis and the textures and the mineralogy and also some differences in the range of temperatures that we obtain from the fluid inclusions in these different areas. The red line on each of these diagrams marks the 50 degree spot. So, you can see that in some parts of the repository site, such as here, this would

be in the north portal and north ramp area. The temperatures are somewhat higher. They reach approximately 80 degrees C. Here, in the intensely fractured zone, however, the temperatures average around 40 degrees C, less than 50 degrees.

When you look at data from individual samples, the data are really excellent. What we're looking at here is a histogram of homogenization temperatures which essentially give us the temperature of the fluids that were moving through the repository site and the different colors reflect different fluid inclusion assemblages in a single sample. So, what I'm showing here are data from seven different assemblages of inclusions in a single sample. Our heating steps are 2 degrees C and what we see is that the majority of almost 180 fluid inclusions homogenized over a 6 degree range from about 61 to 67 degrees C. This is extremely tight fluid inclusion data. Fluid inclusion records usually have much more scatter than this. The extreme tightness of this data tell us that these are, in fact, good legitimate fluid inclusion assemblages and, more importantly, that they have not been perturbed after they were formed. These temperatures suggest very strongly that the calcite that contains these inclusions was not heated significantly after these inclusions were trapped.

Okay. A real key question is where are these fluid

inclusion assemblages located within the samples? This is where we start putting the fluid inclusion story in a time frame and in the geologic context with the petrography and the paragenesis. Where we observed the far greater majority of these two phase inclusions is in the base of the samples in the older calcite. We also see some of the two phase inclusions in the very cores of some of the earliest bladed calcite crystals. An important factor though is that we never see two phase fluid inclusion assemblages in these bladed crystals and we never see two phase fluid inclusion assemblages in our magnesium-enriched Sparry calcite that often overgrows these bladed crystals. So, in a relative sense, we can now say that the two phase fluid inclusions which again record the passage of fluids with elevated temperatures are constrained to the oldest calcite and the intermediate calcite. So, the next thing you want to do is add some absolute constraints to this story.

Okay. This is really just a summary of what I mentioned, the two phase inclusions are in relatively old and intermediate calcite, but there's no record of the passage of fluids with elevated temperatures in the younger calcite.

Okay. One more figure I threw in. This is not in your handout. This is probably hard to read. I apologize for it. This is to show some of our carbon and oxygen isotope data from these calcite samples. And, what they've

been able to show is that, as we go from older calcite to younger calcite, there is a definite trend from higher carbon isotope numbers and lower oxygen isotope numbers to lower carbon isotope numbers and higher oxygen isotope numbers. So, from old to young, we sort of move from this area to this area.

This observation raised the question can these isotopes be used as a proxy for age? If we can't absolutely date things, can you somehow use these isotopes and translate those signatures to days. What we've shown on here are values that we've obtained for different morphologies of a calcite with analyses on greater calcite on the magnesium-enriched Sparry calcite, on basal calcite, and so on. In general, our data very much correspond with what the USGS found. However, what's really key, the point to take home, is this kind of a diagram right here. The field in blue outlines the values that we obtained for the magnesium-enriched Sparry calcite which we can date because of the opal in it and again which is free of these two phase inclusions. However, this field coincides with data from bladed calcite which is clearly younger because it's paragenetically below the Sparry calcite. It also coincides with some of the analyses which we have obtained for other calcite which does not record the magnesium chemical signature.

So, essentially, what we end up with is a field in

this area right here which is not unique. The magnesium-enriched Sparry calcite has signatures in this area. It is the youngest layer, but there are older calcite layers that also overlap with that field. So, it's not a unique signature and we cannot use the oxygen and carbon isotopes as a proxy for age.

Okay. We're back looking at our map and this time we have added these little yellow arrows here. They indicate the samples which we have obtained age dates for. We did the similar age dating to dating that the Survey has done. They determined that the opal that's inter-grown with some of the samples, in some of the areas of the samples, contain sufficient uranium to do uranium-lead dating and so these are the samples that we have dated. Unfortunately, there are a number of sample sites which simply do not contain that mineralization and we're not able to date those samples.

Okay. Our dating took two approaches. As I pointed out, the magnesium-enriched Sparry calcite shown here is inter-grown with opal and we have opal layers at the base. We have opal in many cases within the Sparry calcite. And then, we have opal overgrowing this. So, we've obtained a number of dates, close to 20 dates, on this magnesium-enriched Sparry calcite from the base to the top. What these dates collectively tell us is that this material began to precipitate between about 1.9 and 2.8 million years ago.

Again, this calcite does not contain a record of the passage of fluids with elevated temperatures. So, these dates constrain the passage of fluids with elevated temperatures to be more than, at least, about two million years ago. Then, keep in mind that this bladed calcite, which we cannot date, also precipitated during a period when these fluids with elevated temperatures were not moving through the site. So, that was our first approach.

But, we wanted to see if we could constrain the ages of these fluid inclusions even more tightly than that. Unfortunately, there's not nearly as much opal in the intermediate part of these samples as we would like to be able to do that, but there is in a few samples.

A summary of what I've just said, magnesium, Sparry calcite, and opal began to precipitate between 1.9 and 2.8 million years ago and fluids with elevated temperatures are older than two million years.

Okay. Here is one of the two important sections that I'm going to show you that give us our best constraints. What we see here is a nice sample again from a lithophysal cavity. We've got the base of the section here. Older calcite, somewhat finer, somewhat grungier-looking, overlaying a layer of opal which is highlighted by this black line. Outboard of that, we have more clear calcite, another layer of opal highlighted in red, and then outboard of that,

additional calcite and more opal. We have dated these layers. A key feature is that these black squares show the location of fluid inclusion assemblages with two phase fluid inclusions. So, these are the fluid inclusions that record the passage of higher temperature fluids. All of these assemblages lie below and are older than this opal layer and we've been able to date this layer at 5.3 million years. So, in this particular sample, fluids with elevated temperatures move through these rocks more than 5.3 million years ago.

Okay. This is the sample in which we have the oldest constraint. Here's the sample where we show you the opposite constraint. Base of the sample down here moving younger outwards. Again, the black squares give us the locations of fluid inclusion assemblages with two phase fluid inclusions. Here, we've plotted some of these data and what this sample shows us is a feature that we see throughout samples from the sight and that is that the hottest temperatures or hours recorded in the oldest calcite, and as you move towards younger calcite, temperatures always get warmer. Here, we have 45 to about 60 degrees C, 57 degrees C. Overlying that, we have chalcedony that gives us an age of 6.24 million years. So, these inclusion assemblages are older than 6.24 million years. As we move outboard, temperatures become less warm or cooler, ranges from about 47 to 43 degrees C. Then, here, we have additional opal layers

which we have dated giving us ages of 5.8 million years. And then, just outboard of those opal layers, the coolest temperatures that we obtained, 35 to 41 degrees C. So, here, we have the opposite constraint. Here, we show that these inclusions, 35 to 41 degrees C, are younger than 5.8 million years.

Okay, our conclusions. Again, referring back to the questions that we asked. First of all, there is a record of hot waters at Yucca Mountain. These temperatures average about 45 to 60 degrees C, but they are as high as about 80 degrees C in the north portal and north ramp and they are lower than this in the intensely fractured zone. This record is found across the repository site.

The two phase inclusions across the repository site were trapped more than 1.9 million years ago. Again, we never see a record of two phase fluid inclusions in the magnesium-enriched Sparry calcite and it began to precipitate at approximately two million years ago. Some fluid inclusions were trapped more than 5.3 million years ago. Some fluid inclusions with the lower temperatures, 35 to 41 degrees C, were trapped less than 5.7 million years ago.

And then, finally, sort of stepping back a bit looking more at the big picture, looking in general at the characteristics of the secondary minerals in the deposit, we have concluded that the secondary minerals at Yucca Mountain

do not contain those characteristics that are typical of or consistent with hydrothermal mineralization. There are many of those. The fluid inclusion record is sparse. The two phase fluid inclusion is sparse. It is low in temperature. There's no evidence of hydrothermal brecciation at the sites. There's no reversal of the fluid inclusion temperatures. Things are always cooling. They are never cool and then heated up. The vein style is very simple. We don't have repeated fracturing offsetting. The mineralogy is a low temperature assemblage in hydrothermal systems. We typically see intense solidification of silicate mineral assemblages and it's really only the collapse at the very waning stages of hydrothermal systems that give us calcite mineralogy. And, there are other features in additional to these.

Thank you.

WONG: Thank you, Dr. Cline. Questions from the Board?

KNOPMAN: Thank you, Dr. Cline. That was an excellent presentation. Could you imagine at this point, given what you've just done, any other possible interpretation of this data or other data that could support the hydrothermal upwelling in relatively recent geologic history?

CLINE: No. We haven't answered the question of the source of the fluids or what's responsible for the temperatures that we see. You know, we could put together a hypothesis to explain that. What I've sort of given you is

the bare bones, the simplest, most straightforward interpretation of the data that we see. I think that the interpretation that rocks younger than two million years do not record the passage of fluids with elevated temperatures is as conservative and straightforward as we can get. I don't see any other interpretation that can be drawn from that.

That youngest layer, which is chemically distinct and can be clearly traced, can be readily dated because of the opal, simply does not record the passage of fluids with elevated temperatures and I think that's inescapable. We start looking older into the rock, we start seeing a very low temperature record, 40 degrees C, at between five and six million years, and then as you get even older, that temperature heats up. But, the fact that you have very few inclusions that record elevated temperatures, the fact that you have really only--almost only calcite mineralization or some early silica, some early fluorite, and they record early elevated temperatures, you have to go into the much older parts of the rock to see that record. Those are really observations; they're not interpretations. I don't see any way to argue those observations. We can argue about what caused those temperatures, but those temperatures and where they are in the rock is a function of very straightforward observations.

KNOPMAN: Thank you.

WONG: Any other questions from the Board?

RUNNELLS: Runnells, Board. Jean, I know you've thought about this, but you may not have an answer. The source of the magnesium in that youngest material, I mean one percent is, in fact, you know, a significant amount of magnesium and something happened. Do you have any hypotheses you could offer to us?

CLINE: Nothing very concrete. We've shown these data to other people and a suggestion that some people have made is that it's a response to climate in some way. If you conclude that, then you're concluding something about the source of the fluids. That seems a reasonable hypothesis and one that's very much worth testing. Beyond that, I really don't have any good ideas. Some people have suggested that-- obviously, something has to be eroding. I presume something has to be eroding to give us a magnesium source. Some people have suggested that some of the younger volcanic rocks in the area may have begun eroding then to provide that. If the climate was dry enough back then, there may have been some dry lakes and this is where the climate signal would come in that during periods of greater rainfall or greater precipitation that the magnesium would then go into solution and be transported by meteoric fluids, but that would be concluding that these are meteoric fluids. It's really

speculation on my part at this time.

RUNNELLS: Okay. A second question, the solidity of the fluids in the fluid inclusions, do you have any data on the solidities?

CLINE: Yeah. The solidities range from about half a weight percent to--Nick, do you remember the--

WILSON: 1.9.

CLINE: 1.9 weight percent. So, less than two weight percent. Those are low salinities, but there's certainly more saline than what meteoric water would be, pure meteoric water. But, fluids moving through rocks, those are not unusual salinities for fluids moving through rocks. But, those fluids moved through the surface of those were descending meteoric fluids and they had fallen on a surface that was even slightly saline. Those are consistent salinities for that. They're consistent with salinities. They're also consistent with epithermal fluids, upwelling hydrothermal fluids. They're not distinctive, they're not unique.

RUNNELLS: But, they approach 20,000 parts per million salinity, right? I mean, 1.9 percent, that's--

CLINE: To me, that's low salinity.

RUNNELLS: But, that's two-thirds of seawater which, to me, is pretty salty.

CLINE: Magmatic systems commonly have salinities that

reach as high as 30, 40, 50, 60, 70, even 80 weight percent salinity. So, it's not a saline magmatic fluid. We can say that much about it, but it's not an unusual signature for a natural fluid.

RUNNELLS: Okay. Thank you.

WONG: Thank you, Dr. Cline. Again, the clock runs our life. To present his interpretation of fluid inclusion dating work, I am pleased to next introduce our next speaker, Dr. Yuri Dublyansky. Dr. Dublyansky is a member of the Siberian branch of the Russian Academy of Scientists and is currently serving as a scientific expert to the State of Nevada.

DUBLYANSKY: Thank you very much for inviting me to present the findings and the interpretations of the State of Nevada basically on the same subject.

I'm an independent consultant to the Agency of Nuclear Projects of State of Nevada, and I have the permanent position of senior researcher in the Fluid Inclusion Lab, Institute of Mineralogy and Petrography in Novosibirsk in Russia.

I think the major question which we are trying to answer, studying secondary minerals at Yucca Mountain, it's basically not the temperature which we can find in fluid inclusion, not the division of these temperatures. The major question is what is the origin of secondary minerals at Yucca

Mountain?

As you know, there are two hypotheses which have been advanced. So, we have to answer the question were those minerals deposited from rainwater percolating through the hot mountain? We know that it has to be hot to produce this temperature. Or, were the fluids deposited by deep-seated thermal waters injected in the vadose zone?

In my presentation, I will try to cover these four topics; mineralogy and crystal morphology, fluid inclusions, isotopic properties of calcite, and in the end, I will try to present the model which in our opinion explains everything which was observed this far.

So, I'll start with mineralogy and crystal morphology. First of all, the secondary minerals found in the ESF underground, it is calcite, quartz and chalcedony, fluorite, strontianite, apatite, and zeolite. So, as you can see, those fluids were quite mineralized and contained many strange substances like fluorine, like strontium, which are very difficult to imagine to be dissolved in substantial quantities simply in rainwaters. I just want to point out that the chemistry of a system deposited in secondary minerals wasn't that stable. So, it wasn't just calcite.

Well, next question which can be asked, can this complex chemistry of fluids just indicate that the minerals were a result of interaction between rainwaters percolating

through the tuff and the results of interaction of this rainwater with the tuff? So, we tried to answer this question and we did some preliminary thermodynamic modeling on it. How would rainwater, Yucca Mountain area average rainwater, how would it interact with tuffs and what minerals would be compatible with this system? So, this is just a small part of the work and it's very preliminary. We are acquiring that. So, on this axis, we have temperatures 25, 50, 75, and 100 degrees kind of model temperatures. And, here is a ratio of rock to water. In other words, it's how old this water is or how far was the reaction between tuffs and the water and when? And, this preliminary analysis shows either we can basically--those arrows indicate our estimation of the rock to water ratio for Yucca Mountain based on two different approaches and this is the rock scale also. So, we can see at some elevated temperatures, we can have zeolites deposited from these waters, but to form fluorite, even if you raise the temperature up to 100 degree, fluorite is not supposed to be deposited from this water unless you have extremely, extremely high rock to water ratio which you can hardly expect from the fluid which percolated through the welded tuff over like 50, 60 meters like near the north portal. The conclusion is its complex mineralogy indicates complex and varying in time and space chemistry of water. Minerals that are observed in ESF, for instance fluorites,

should not form from rainwater reacting with these tuffs, but those minerals that should form, for instance kaolinite, albite, and K-feldspar, they are not observed in original records, mineral records of Yucca Mountain.

Another question which has to be asked, very generally, can large centimeter scale euhedral crystals of calcite and quartz grow from films of water? So, essentially, rainwater hypotheses display that we have films of water moving down the mountain and these films of water deposit crystals. To give you an idea what we are talking about, this is quartz crystal which come from ESF and this is bladed calcite crystal with scepter overgrowth. So, this is about half a centimeter scale and this is almost two centimeter scale. In our mineralogical study, you can see these arrows here. They indicate that the growth of crystal occurred from top down to the bottom. We can see growth layers just propagating from the top of the crystal downwards. So, in order to do that, we have to supply material, building material, for these minerals to the top of the crystals so they cannot be supplied by some film water moving up to the crystal and then depositing quartz down. So, this characteristic scepter morphology of crystals, the minerology normally interpreted as indication of diffusion control of growth regime. So, this building material has to have access to this part of the crystal, have access more

radially than lower part of the crystal, which suggests that the density of fluids was high probably at that time.

And, also, in this slide, we can see the dramatic change in the morphology of a crystal. This part of a calcite does not have any crystalline shaping. It's almost irregular even though internally it's crystalline. Normally, it is interpreted as deposition at fast rate and in a system where you have either boring or (inaudible). We had a two phase system and basically we do have all gas inclusions which suggests that indeed was the case.

We have a dramatic change in the environment right here and the growth goes slowly and perfect crystal with perfect crystal graphical shape is formed here. This is basically the simple explanation derived from studies of speleothems why large euhedral crystals do not form from films of water which is normal way of forming speleothems.

So, to summarize this part of my talk, I have to say that, thus far, no coherent physical model explains the mechanism of crystallization of large euhedral crystals from films of water at Yucca Mountain have been proposed. Examples of growth of large centimeter scale euhedral crystals of calcite and quartz from films of water are not known. We have been searching literature quite a bit on that and my colleagues from the Institute of Mineralogy who have been searching for me, we have found nothing on that. I

1 think the morphology and growth related features of crystals
2 from Yucca Mountain indicate growth in submerged state and
3 from fluid with evolving properties. We can tell from the
4 change in morphology of crystals.
5 Now, there is a problem of growth rates. Some work
6 done by USGS, such as the growth rates of these crystals,
7 were remarkably uniform and remarkably slow. So, based on
8 uranium-lead dating, they are talking about growth rate of
9 sometimes less than a million millileters per million year.
10 So, from the standpoint of just crystal growth theory, these
11 rates, they do not seem real to us. As Dr. Craig suggested,
12 we did like outside of the napkin calculations. So, we took
13 this growth rate and took the size of a calcite crystal and
14 calculated how far from equilibrium should we keep our fluids
15 to precipitate this calcite with the rate suggested by the
16 dating. So, omega is equilibrium, omega equals--one is the
17 exact equilibrium, nothing precipitates, nothing gets
18 dissolved. So, in order to keep going with this crystal this
19 way, we have to keep our fluids this far from equilibrium.
20 It doesn't seem reasonable to me. And, probably another way
21 of demonstrating that, imagine, we created an (inaudible) and
22 fixed the temperature (inaudible) at this rate, 1.8
23 millimeter per 2 million--for a million years. And, imagine,
24 we change the temperature by only .1 of a degree what's
25 happened? And, the net rate in moles per square meters per

hour jets up 9 orders of magnitude. So, I don't think you can maintain this growth rate in a laboratory, most sophisticated laboratory. But, in order to make this growth rate correct, we had to maintain this growth rate in vadose zone for millions of years. I don't think it's possible.

Okay. The conclusion is that deposition rates appear to be unrealistic from the standpoint of the general physics, as well as from the standpoint of a theory of crystal growth. We expect the nucleation will be inhibited and we'll have just fluid in metastable state. This calls into question the results of the radiometric dating.

Our next topic is fluid inclusion temperatures. We're making quite a bit of progress from 1998 when many people did not believe that fluid inclusions are there and I have to commend the Board for its role which the Board played in resolution of this problem, starting the resolution, at least. So, basically, this cartoon shows some of the historic situation. What did we know about fluid inclusion three years ago before this joint UNLV, USGS, and State of Nevada project started? What is important here, this data was reported by USDOE back in 1993. This is data from borehole calcite and some inclusion temperatures shown right here, they are higher than 100 degree; 104, 108, 7 degrees Centigrade. What this immediately tells us that if this are data correct, we are in a saturated environment. We cannot

keep temperature higher than at the boiling temperature in the unsaturated zone.

Well, now, how are those fluid inclusion temperatures distributed in the ESF? This is not compiled from my data and, as Dr. Cline told us, the temperature higher near the north portal and also near the south portal. So, we have a very remarkable gradient across the site from east to west. We also have to account for the fact that these samples are taken at an elevation higher than those because the tunnel dips westward. So, the easiest way to account for that is to calculate the heat flows. The heat flow is the product of thermal gradient and the thermal conductivity of the rock. This is the normal way of expressing how heat moves through the earth. This is just to give you a sense of perspective. This is a heat flow. It's measured from earth and you can see under oceans they measure--they vary between 0.5 and 2.5 heat flow units. In western United States, again, between 0.5 and 2.5 heat flow units. So, it's a pretty much stable heat flow on earth unless we have some hydrothermal disturbance. For instance, in Wyoming in Yellowstone, the heat flow can be as high as 30 heat flow units. In Nelson, Nevada, where the heat flow units can locally be up to 24 or 26 heat flow units, there are power plants which are using this power, geothermal power. So, this map shows the fluid inclusion temperatures

recalculated in the heat flow. This, I think, is just an amazing plot. We have heat flows from 5 heat flow units up to 17. It's a tremendous amount of energy goes up here and also the gradient is very substantial. We have tremendous east-west gradient here.

On this plot, I'm comparing the modern day distribution of heat flows at Yucca Mountain done based on the borehole measurements and the paleo heat flow based on the fluid inclusion. As you can see, the general structure is, more or less, similar, but the values are completely different. We have 1 to 1.8 heat flow units in the modern state which is reasonable and we have from 5 to 17 in the fluid inclusion records. And, the latter gradient over the ESF look today to have different heat flow units per kilometer and, in the past, we had 39 heat flow units per kilometer. Just a question. Could those gradients be induced by the Timber Mountain Caldera hydrothermal episode which we know occurred 10 to 11 million years ago and temperatures of the water table by that time could have been as high as 100 degrees Centigrade. So, I compiled the same map. I assumed that water table has a temperature of 100 degrees Centigrade. And, again, we have only 1.1 heat flow units gradient and 39 gradients here. Also, the fact that we cannot possibly explain the temperature which we measure in fluid inclusion by this Timber Mountain Caldera temperatures

is shown on this graph. This is model which is published by Whelan just recently. And, essentially, these black lines are the temperatures in the ESF which could have been there if you have water table at 100 degrees Centigrade than if you have water table at 100 degree and 100 meter higher, and if on top of that, we have the overburden of 100 meters. So, it's kind of a model. And, as you can see, the third line shows the real distribution of fluid inclusion in the ESF, almost perfect negative correlation. So, I don't think real distribution of temperatures can be explained by this model.

Our conclusions. Values of paleo heat flow indicated by fluid inclusions are significantly greater than it is possible for the net conductive heat transfer. Neither values nor spatial structure of a paleo heat flow can be accounted for by any known event in thermal history at Yucca Mountain. The structure of paleo heat flow which is steep east-west gradient requires source of heat associated with major block-bounding from Paintbrush Fault. Parameters of the paleo heat flow preclude any substantial role of rainwater in the deposition of secondary minerals at Yucca Mountain. It is important I think, extremely steep lateral heat gradient cannot be maintained for geologically significant periods of time which again calls into the question the results of the radiometric age dating of secondary minerals. This observation, we believe, can only

be explained by assuming short-lived transient character of the heating input or inputs.

Just a visual feature of that. This is a 3D map of the heat flows in the repository block. So, we have north portal here, 75 heat flow units, and the ESF humps like that. I don't think we can maintain this hump for any extended period of time.

But, now, I want to address a question. What is the significance of all-liquid inclusions which Jean Cline just talked about? So, we know that fluid inclusion methods does not yield information when the temperature drops below 55 or 50 degrees and, to date, we cannot exactly point to this temperature. Just the shrinkage bubble stopped nucleating there and we have all-liquid inclusions. So, all we can tell about the fluids which we deposit in such minerals, such as like magnesium-enriched calcite, they perform below approximately 35 to 50 degrees Centigrade, somewhere there.

Now, imagine, we have our minerals formed somewhere in this temperature range or even lower and it was trapped, for instance, just--as an example, at 30 degrees Centigrade and it was trapped in the ESF at the depth of 100 meters, say somewhere in the north ramp. And, this gives us heat flow of 6.5 heat flow units which is on this map of the distribution of heat flows, it's way off. It's almost three times as high

as the normal heat flow. Which means the fluid with this temperature cannot be called cold fluid or ambient temperature fluid. It is thermal fluid. It has energy derived from some sources other than normal conductive heat flow from the earth's crust.

The reason why I emphasize that just because the inferences which were done by UNLV researchers, Jean Cline just presented them to us, because the two phase fluid inclusions are not present in this magnesium-enriched calcite, the passage of fluids related to temperatures did not occur there. I'm willing to say that this statement just cannot be substantiated. At a depth of planned repository horizon, the temperature of less than 35 to 50 degrees Centigrade may indicate either ambient temperature water or thermal water. We just cannot tell what really the origin of this water was. And, therefore, this conclusion is that a non-thermal origin of the magnesium-enriched calcite cannot be substantiated on the basis of the absence of the two phase fluid inclusions.

Well, isotopic properties, I will try to be a little quick on that. A very interesting feature which we have found and a very unusual one is that we have very systematic and many minerals in gradual change in isotopic properties, even if you study them on the basis of individual crystals. For instance, here, we have one small crust about

1 millimeter thick and the isotopic properties change in a systematic manner and the range of these changes are dramatic +8, +9, to -8, -9 per mil. On this graph, I also show the area for these particular samples for which I have fluid inclusion data. A view of the summary graph which was produced by USGS researchers, their isotopic measurements, I overlayed this with my data where I have coupled results, stable isotope fluid inclusion data and the conclusion would be that two phase fluid inclusions are present in calcite with a range of isotopic properties including those which are attributed to the youngest members of the paragenesis.

And, a very important question in interpretation of the geochemical data, is this positive delta C-13 in calcite. This is a summary on what sort of delta C-13 values we expect to see in the near-surface environment. Well, as you can see, all potential sources of carbon in this environment are negative; -10, -20, -30, and most of the positives, they just shifted to the more negative values. So, it's very difficult and very unusual in the near-surface environment to have carbon as isotopically heavy as we have at the early stages of Yucca Mountain.

And, this far, I could find only one explanation how we can get this heavy carbon. To have this heavy carbon in calcite, we have to have partition between carbon and two dissolved species, one of which is reduced species methane

and second one oxidized species CO_2. And, when we have this exchange there, methane takes most of the light isotopes, and therefore, the CO_2 becomes enriched in heavy isotopes. In order to do that, you can only have this process in very low oxygen. So, you have to have anoxic environment; otherwise, you will not produce calcite with positive delta C-13.

Well, the overall conclusion of my talk, the observation presented so far cannot reasonably be explained by a model invoking deposition of secondary minerals at Yucca Mountain from percolating rainwater. But, the model which explains all observations presently known to us is the model of upwelling.

When we develop a model which has to explain any geological situation, we have to take care that our model is consistent with basic sciences, such as physics and chemistry, it's contradiction-free, and coherently explains all observations available. So, in cartoon style presentation, that's what we think is happening at Yucca Mountain.

First, it's an early-stage upward flow initiated by earthquake with a hypocenter at significant depths. So, there is a lot of hot water that's producing an environment which can account for this positive delta C-13 in calcite. The second stage, this transient thermal water mound formed in the vadose zone. There is some lateral flow in the

enhanced permeability zones. And, in the later stage, there is decay of this mound. This mound cannot--at least for a long time it has to decay. So, it just little bit different perched water bodies and you expect to see these downward flows and some interaction with the rainwater. And, more detailed substantiation of this model will be provided to the Board in a subsequent manuscript which we plan to publish later this year.

Thank you very much.

WONG: Thank you. I think in the interest of time, we will save some questions for Dr. Dublyansky for later. We'll move on to the next speaker.

The next speaker will be Dr. Joseph Whelan from the USGS. Joe will talk about his views on the meaning of the fluid dating studies.

WHELAN: Well, I guess I'm going to be presenting the USGS viewpoint which would be for an unsaturated zone that was warmer than modern ambient conditions at some time in the past. The presentation really is a team effort. Zell Peterman is our team chief and Jim Paces, Leonid Neymark, Brian Marshall, and Ed Roedder all contributed to this. I'm going to be talking first a brief review of our geochemical data that we've completed in the past from underground samples. Then, I'll talk about the fluid inclusion data that we've collected in the last few years and the joint study

with UNLV. And, finally, propose evidence, present evidence for alarm and gradual cooling of the unsaturated zone that's consistent with some of the modeling that we've been doing.

We started studying the secondary minerals in the unsaturated zone because they provided the only record of past water movement in the unsaturated zone. This is water movement in a potential repository. The case is very important. We thought they would provide a means of predicting future water movement. The samples that we got since underground construction in 1995 have been far superior in terms of the quality and they've given us a much better impression, the geologic context and distribution of these deposits in the unsaturated zone.

Virtually all the deposits occur in open space in the unsaturated zone welded tuffs. They occur either in lithophysal cavities or in fairly wide aperture fracture systems. They invariably occur on the floors of the cavities, on the footwalls of the fractures. We see no evidence in the cavities for high water marks that would indicate ponding. We don't see mineralization surrounding the cavity that would indicate that the cavity was filled at the time the minerals formed and we don't see any minerals on the hanging walls or the fractures. We take this as very compelling evidence that these spaces were not filled with fluids at the time that the minerals, the secondary minerals,

were formed.

Furthermore, less than 10 percent of the fractures and cavities in the unsaturated zone welded tuffs contain secondary minerals. This sort of distribution is consistent with percolating water moving down from the surface on long connected pathways and is not consistent with general overall flooding of the unsaturated zone either totally or in part. I mean, we find no places where even 100--small zones where 100 percent of the available cavities are mineralized.

This, I don't have time to go into in detail, but it's data that we had collected to the fluid inclusion work, geochemical data and geochronologic data from the minerals in the unsaturated zone. This data had led us to conclude that the secondary minerals formed in an unsaturated zone setting, from downward percolating water of meteoric-infiltration origin, along focused flowpaths that bypassed many potential flowpaths and depositional sites, and over a long depositional period from at least 10 million years ago, possibly since the tuffs cooled to below 100 degrees Centigrade. As a body of data, it's inconsistent with formation of the secondary minerals from the groundwaters that we know of in the region today.

This just recaps kind of the design of the fluid inclusion research which Jean has already done quite ably and I think I'll just pass over it because there's nothing here

that hasn't been said already.

This is a slide--I'm not going to go into the same depth that Jean went into in describing the paragenetic sequence. We had developed a general paragenetic sequence prior to the fluid inclusion work. We looked at it much more carefully during the fluid inclusion work and basically I can say that our paragenetic sequence and UNLV's paragenetic sequence agreed quite well with each other. We didn't have the advantage of a dedicated electron microprobe to map magnesium in the outer calcite. Jean didn't mention that some of those maps they did took as much as a week. So, we kind of did ours on the basis of petrography, but I think still that our observations were quite consistent with theirs.

We distinguished three types of fluid inclusions in the calcite. The most common are all-liquid fluid inclusions. Our vapor/liquid ratio was zero. There's also a significant population of inclusions with highly variable vapor contents including all vapor inclusions. The inclusions they give us, we have temperature estimates with vapor/liquid ratios of around 1 percent. We found these inclusions in about 50 percent of the sample localities, just as UNLV did. But, with respect to the total number of fluid inclusion assemblages observed in the samples, those actually containing these type of inclusions were a small proportion

of that total. The fluid inclusions assemblages with inclusions suitable for temperature measurement are predominately in early-stage calcite. We find a few, as did UNLV, in the earlier part of the intermediate-stage calcite. We found no fluid inclusions suitable for temperature work in the late-calcite.

This is a photograph of a fluid inclusion or part of a fluid inclusion assemblage in one of the calcite samples. It's a map we made from a number of different photographic exposures at different depths in the sample to try to show the distribution of the different kinds of fluid inclusions. There's all-liquid inclusions and then there's the inclusions with small bubbles which I hope you can see and a number of inclusions with large vapor/liquid ratios. This is a fairly typical sort of fluid inclusion assemblage in the early calcite.

As Jean pointed out, very, very consistent temperatures when plotted as histograms from these types of fluid inclusion assemblages. We measured 70 of the small vapor/liquid ratio inclusions of an average temperature of 52.6 and a standard deviation of 1.8. This is really very good fluid inclusion data.

This is a cross-section of the ESF kind of unfolding. North portal would be here at zero. South portal over here. South bend, north bend. Basically, we find the

same sorts of temperatures that UNLV has found, generally ranging between 40 and 65 degrees. The one place where there's a discrepancy, I guess, between our data and UNLV's is at the site in the north ramp. We found temperatures averaging 85 degrees in one sample that we looked at from there.

Switching back to data that we had prior to the fluid inclusion work, this is a plot of delta C-13 of calcite versus delta 18 of calcite. Actually, Yuri showed an earlier version of this slide. The red dots represent samples--these are all micro samples of calcite from the unsaturated zone that we kind of blindly sampled. So, when we sample near the base of an occurrence, we just called it basal calcite. When we sampled from the outside, it was outer calcite. We didn't place this in a paragenetic context. So, basal calcite O-18 values--and basal calcite would in a general sense usually be older and outer calcite in a general sense would be younger--has O-18 values in this range with the few values over here. Then, there's a general trend of increasing delta O-18 values as we go this direction towards the outer parts of the crusts.

If we use the fractionation of O-18 between the mineral and the depositing water which is a function of temperature--that is, if the water O-18 is constant, the calcite delta O-18 increases as temperature decreases--we can

use this data to make some estimates about the temperature conditions at the time of formation. These estimates indicate, one, that there was some noticeably elevated temperatures probably during the formation of the older calcite consistent with temperatures of 50 to 80 degrees Centigrade and there's a long-term increase in the calcite delta O-18 values that's consistent with long-term cooling of the unsaturated zone rock mass.

In a few instances, we had direct comparisons between the calcite calculated delta O-18 temperatures and fluid inclusion assemblage temperatures. For those localities, we have, I think, very good agreement between the two, the O-18 temperature in the calcite and the fluid inclusion temperature. In some cases, it really remarkably good. This agreement suggests that the O-18 value of unsaturated zone fracture water was relatively uniform and that the calcite delta O-18 values provide, at least, an approximation of depositional temperature.

This is a picture of a thin section that we've done some geochronologic work on. It shows early-stage calcite capped by quartz and chalcedony, intermediate-stage braided calcite, and if there's any late-stage calcite in here, it's very, very minor. There's fluid inclusion assemblages with temperatures of 40 to 55 degrees Centigrade in this early-stage calcite which is all older than 6.5 or 6.2 million

years ago. The intermediate-stage calcite and the paragenetic relations in this part of the sample are less clear than they are in the right side. The intermediate-stage calcite has 40 degree Centigrade fluid inclusion assemblages only. It has none of the warmer fluid inclusion assemblages. We don't have time constraints on this yet, but we're working on it. What we think is that this thick band of opal here that we dated is dispersed throughout this intermediate-stage calcite on the west. And, we're going to try to get a few more ages out of this portion of the sample. So far, the minimum age of elevated temperature calcite formation that we've got is greater than 1.9 million years with the exception of one sample that gives an age of 1.1 million years. But, I don't think that's providing a very good constraint and that's one of the supplemental slides that's in the packet that you picked up. I think the paragenetic relations here indicate that we're dating some of that opal that Jean was ascribing to having formed within or on the top of the late-stage calcite.

Well, in addition to the fluid inclusions assemblage temperatures that we've got some age constraints on, we have age constraints on some of the delta O-18 values. Using the same logic that we use to estimate some temperatures and deposition on the earlier plot, we can take the O-18 data and construct a plot of temperature versus age

and millions of years. We can do that by assuming a value of delta O-18 for the water--we don't know that value; we assume a value that's comparable to modern day water--and plot--and that's the red dots here. I plotted curves, best fit curves, for these data points of -12. That's the central--minus 12 per mil, that's the central water, -13 per mil, and -11 per mil. I didn't plot the data points for all three waters; I only plotted the data points for -12 per mil. You can see there's a nice indication of cooling with time from the calcite delta O-18 temperatures. The fluid inclusion assemblage uranium-lead ages also agree. Those are the blue diamonds. And, we have one uranium-thorium-helium age of 8.7 million years. For an apatite from the north ramp area, that's 8.7 million year age with a closure temperature of about 56 degrees in it. It, too, agrees well with the trend of long-term cooling.

Well, this kind of fit pretty well with some preliminary thermal models that Brian Marshall has been working on and this is a plot of rock mass temperature at 250 meters below the surface and the horizontal axis is time before present for millions of years. We know that just a few kilometers, five or six kilometers to the north, is the edge of the Caldera Complex that was responsible for these magmatic eruptions. The magmatic eruption, the erupted history, was basically from 15 to 11 million years ago. So,

there was a long period of time during which this magma body was producing heat and producing lavas. The model shown here is based on a disk shaped, 30 kilometer diameter, 5,000 cubic kilometer magma chamber centered 5 kilometers below Timber Mountain. So, that would be five, six, seven kilometers north of ESF. We assume the 900 degree Centigrade magma temperature from 15 to 11 million years ago and a 500 meter thick unsaturated zone at Yucca Mountain. An intracaldera hydrothermal connection, this is a the caldera itself, until 10 million years ago. All these parameters can be varied and we can get varied types of curves. But, I think the important thing to note from the preliminary modeling is that we can get warm temperatures at 250 meters depth in an unsaturated zone and it can take a long time for those rocks to cool off after that thermal pulse.

So, if we superimpose this curve from the thermal modeling onto our time/temperature curve based on real data from calcite delta O-18 values and fluid inclusion assemblage temperatures, we see that there's reasonably good agreement. We're not getting quite high enough temperatures in the early part of the history and we're kind of overestimating temperatures in the later part of the history, but all in all, I think the preliminary modeling shows a lot of promise for accounting for this long-term cooling of the unsaturated zone rock mass.

So, to conclude, both fluid inclusions and calcite delta O-18 indicate elevated temperatures during the early and intermediate stages of calcite formation. Those temperatures are consistent with a likely thermal history of the unsaturated zone tuffs as indicated by the age constraint temperature data and by thermal modeling. The fluid inclusion assemblages which include inclusions with large and variable vapor/liquid ratios are consistent with vadose zone information. And, I need to talk in a caveat here that they're consistent with vadose zone formation, but that's not the only possible explanation for the large and variable vapor/liquid ratio inclusions. They could also result from (inaudible), from exolution (phonetic) of gases from liquids or from leaking either in nature or in the laboratories in preparation. Furthermore, the extremely sparse and heterogeneous distribution of the deposits is specifically inconsistent with even local flooding of the unsaturated zone to produce the mineralization. And, finally, we think the potential rock repository block has been at or near present day temperatures for at least the past two million years, in agreement with UNLV, and likely the past three or four million years, probably longer.

Thank you.

WONG: Thank you, Joe. Again, I think we'll move on to the next speaker and save our questions until the last.

Thank you.

Our next speaker will be Dr. Robert Bodnar from the Virginia Polytechnic Institute and State University. Bob has served as a consultant to the Board in its analysis of the issue of possible thermal upwelling at Yucca Mountain and he has followed the UNLV work.

BODNAR: Thank you. I don't have any visuals. I was asked to come and comment on the presentations that were made here related to fluid inclusions that you just heard. So, it would have been presumptuous of me to make overheads ahead of time. I know we're running behind schedule here. So, I'll try to keep this very short.

I'll comment mostly on the UNLV dataset because that's the dataset that I'm most familiar with. As was just said, I've been serving in the role as an expert advisor to the UNLV joint project and the facilitator at the quarterly meetings that have been held with representatives from the State, the USGS, and UNLV.

I'd first like to say that the quality of the data that you've seen is unparallel almost in fluid inclusion studies. The care that was taken during sample collection, sample curation, sample preparation, data collection, and data analysis is quite unusual for a fluid inclusion study. So, I don't think there's any question concerning the numbers themselves and I think everybody from all three of the groups

would agree on that.

Now, the interpretation, I'll just say a few things about this. When we interpret data, what we tend to do as geoscientists is base those interpretations on comparisons of what we're familiar with with the dataset that we have. So, in the case of Yucca Mountain, what we would like to do is compare those data to what we see in other relatively young, shallow silicic volcanic system that host hydrothermal mineralization. And, if we look at hydrothermal systems, we see a series of interconnected mineralized veins. We typically see quartz or SIO_2 as the dominant mineralizing phase and this is because the upwelling fluids are usually warmer than the surrounding rock and quartz solubility is dominately a function of temperature decrease and so we tend to see a lot of quartz precipitated as the fluids move up into the overlying cooler rocks.

We also see very common temperature reversals when we have active hydrothermal systems in silicic volcanic rocks. Again, there are episodic introductions of warmer fluids into the rocks. These fluids tend to deposit minerals and trap fluid inclusions, and during this process, they cool. At a later time, there will be an introduction of warmer fluids into the rocks and this process may repeat itself dozens, or in the case of some epithermal gold deposits, hundreds of times. So, we see many temperature

reversals in hydrothermal systems in silicic volcanic rocks.

Now, we see none of these features at Yucca Mountain. There are small mineralized fractures and bugs with the fissile cavities, but we don't see the continuous interconnected continuously mineralized veins that we typically see in hydrothermal systems. Quartz is a very rare--not rare, uncommon--is an uncommon phase in the secondary mineralization at Yucca Mountain and most of the quartz is clearly early. And, we don't see the temperature reversals that are common in hydrothermal systems.

In the model that Dublyansky just presented with the influx of deep fluids from below, we might expect to see several reversals in temperature as a result of precipitating the minerals, and if this is an episodic process, the next batch of fluids that comes in with the warm, it would precipitate fluids and cool, and so we would see a sawtooth pattern in temperature as a function of time.

Now, although we don't see many of the features that we commonly associate with hydrothermal waters in silicic rocks, there's still some questions that need to be answered because there are also many observations that aren't 100 percent consistent with just downwelling rainwater. And, of course, several of these have been brought up here already. The source of the salinity, perhaps not a major question, but it's something that should be addressed. If

the model is downward flowing rainwater, a model needs to be developed to account for the salinity. And, several things have been mentioned; so, perhaps it isn't a significant problem. And, related to this is the source of the magnesium. I think a satisfactory explanation is needed for this.

And, also, there seems to be considerable disagreement between the temperature time plots that have been developed from fluid inclusions and geochronology and some of the model predictions. Although, we've just seen in the USGS presentation that perhaps these disagreements have been resolved. We also have to remember that we shouldn't use unrealistic model conditions to make our model agree with the TH time data. And, please, I'm not suggesting that the USGS did that by any means. I'm just saying that we can always make the model agree with the data, but we need to make sure that, as we modify the models, that we don't use unrealistic conditions to force those models to agree with the real data.

And, I'll stop there.

WONG: Thank you. We have about four minutes. So, if the Board has any burning questions?

SPEAKER: Have you checked with DOE before you ask for questions?

WONG: All right. I take that back. We'll have Bill

Boyle up here. I'm only sort of a Chair.

BOYLE: Thank you. I'm aware of the time constraints. So, I'll be brief. Drew Coleman would have made this presentation for the DOE, he followed this study most closely, but he's busy in Las Vegas and I was going to be here anyway and I had followed it myself, but not as closely as Drew.

You've heard the scientists speak and Professor Bodnar mentioned that it was a cooperative effort and it really was a pleasure to be involved. It was not only the scientists that were involved, but Board staff were involved, I think, at all the meetings. Board members, I know Dr. Runnells has attended meetings. NRC staff attended; NRC, Nuclear Regulatory Commission consultants, the representatives in the local Government, there were representatives from the State of Nevada besides the scientists, Attorney General's Office. So, it was quite large and open and a very interesting study.

But, what do these results mean to the Yucca Mountain Project? The entire study was started by a July 24, 1998, letter from Chairman Cohon to Lake Barrett. I assume the letter and the press release are still available at the NWTRB's website. Dan Bullen is nodding his head. So, for people who haven't read it, they can go read it.

There were some conclusions, if you will. One

conclusion was that the Board's experts had reviewed the recent data and thought that the 1992 National Academy of Sciences' conclusion that it was more likely cold water flowing down, rather than hot water coming up, still stood. The second bullet dealt with, although there might be some other tests to do, you know, DOE should look at the priorities of whether or not to do those tests to gain more insight. Then, the third bullet began with "however" and it said do the fluid inclusion work which has been done.

Now, you're heard the scientists and you hear Professor Bodnar mention "I think all the groups agree on the measurements." There is some disagreement in the interpretation. I think what DOE will do is when we get the report from UNLV, we will also look at reports and papers that have been published and presented at the High Level Waste conference and elsewhere. The DOE will probably respond to the July 24, 1998, letter and make reference to the work that's been done and will probably come to a conclusion that the 1992 NAS results are still the preferred interpretation and that this work is--even though there's some disagreement, the majority opinion seems to be that this work here supports that conclusion, as well.

As Joe Whelan mentioned in the secondary minerals for other reasons anyway, you know, in terms of understanding the seepage and also the long-term history of Yucca Mountain.

1 So, work will continue on secondary minerals for that
2 reason. You also heard Professor Bodnar mention that there
3 were these other items that perhaps bear investigation and
4 I'm sure some of those will be looked into, as well.
5 WONG: Good going, Bill.
6 BOYLE: Thank you.
7 WONG: Okay. Now we will have some questions from the
8 Board for the previous speakers. I know Dr. Parizek was--
9 PARIZEK: Parizek, Board. I had just questions that
10 could be just of general nature. Then, the question of who
11 should answer them may be a choice. But brings up always
12 some observations that I can't tell how common they are.
13 mineral growth requiring tip growth downward, requiring
14 saturation. Did other people in this program see similar
15 things or was that kind of unique? Those fluorite minerals
16 brought up, you know, there is some inconsistencies to the
17 general conclusions that were presented by Jean Cline, for
18 instance. So, how common are those and how much weight
19 should be we put on those special occurrences? Or are they
20 widespread and that others just did not see them? Somebody
21 want to comment on that? And, again, we've seen a line of
22 evidence and again we see a temperature pattern that Yuri
23 gives us and says it's hot to the east. Is that hot to the
24 east only in his dataset? I guess, we heard from Jean Cline
25 it was also hot in the north portal, higher there than

elsewhere, and do we need to know why it was hot as long as it was hot a long time ago? And, from Yuri's data, I get the impression it was hot not so long ago.

DUBLYANSKY: Well, I'll to clarify some things. First, the mineralogical temperatures which we interpret as indicative of the growth in submerged state, they are basically everywhere. We just--the mineralogists which work with me, they just don't see any other textures which could indicate unsaturated environment. We don't see any characteristic patterns, such as (inaudible) textures, or minuscule textures which are normally seen in such an unsaturated environment except for the location of samples, location of cavities. However, it does not necessarily mean that this environment was unsaturated during this time because examples of gravitation control growth of minerals in saturated environment in hydrothermal deposits they are well known in the textbooks.

So, to answer your first question, the (inaudible) graphical features which indicate submersed growth which is--you heat large euhedral crystal scepter morphology and propagation of growth layers from tops of the crystals downwards. They did experiments. They are quite common. They are everywhere.

Temperatures, the distribution of temperatures within the ESF block, as far as I understand, we all obtained

basically very similar results. So, we just plotted them differently, but all three groups, as far as I can tell, have higher temperatures near the north portal and, you know, the south portal and lower temperatures in the north vent and this major north-south, north drift.

And, what was the third question?

PARIZEK: I think you caught most of the ones I raised.

BODNAR: It's important to point out for the record that the use of crystal morphology and mineral textures, it represents a real scientific culture difference between the former Soviet Union and eastern Europe and most western countries. The use of crystal morphology has historically been used to infer environmental formation in the former Soviet Union and it was used in western countries through the 1800s and perhaps up to about the middle of the 20th Century. It was then recognized that there were perhaps more exceptions to the norm concerning the mineral textures that using them as a diagnostic means of characterizing the environmental formation could be very misleading and then this has subsequently been confirmed by a lot of experimental work. So, I'm not saying that it's wrong. I'm just saying that there are differences of opinion concerning the use of mineral textures. In western countries now, most scientists will not use mineral textures to infer environmental formation because of the many exceptions to the norm.

PARIZEK: Would you go further and comment on just the other question about fluorite or other minerals that were present in some of the thin sections? I mean, are they abnormal or do they need explanation?

BODNAR: I don't think I'm the correct person to comment on that. Perhaps, Jean or--

PARIZEK: While you're up though, could I ask you about other ways to trap, say, two phase fluid inclusions? We heard from Bill Arnold here at the meeting last week of all the mechanisms that could trap them which involves a capillary force which then begins to raise question about the usefulness of any of these techniques if you have different trapping conditions and so on.

BODNAR: Bill made that presentation, I think it was at the February meeting in Las Vegas, and we spent a lot of time at the group meeting discussing that. Theoretically, it appears to be a very sound model. And, it may, in fact, be operating at Yucca Mountain, but I think the most important point here is that even if it's operating at Yucca Mountain, it doesn't affect any of the results or the interpretations. And, the reason is that the process that Arnold describes, if it's operating, will result in a very wide range in liquid to vapor ratios in the fluid inclusions and consequently a very wide range in homogenization temperatures within a group of fluid inclusions. The first criteria that's used to

identify fluid inclusions that contain useful information, in other words correct information, is to look at those fluid inclusions and determine if they have uniform liquid to vapor ratios, and therefore, uniform homogenization temperatures. And, Jean showed that very nicely in the histograms that many dozens or hundreds of fluid inclusions with only a few degree, maybe 10 degree at the most temperature variation, if the process that Arnold is describing were operating, the temperatures would vary literally over hundreds of degrees. So, if that was operating at Yucca Mountain, those inclusions were necessarily eliminated before the measurements. So, the data that were obtained represent real temperatures.

PARIZEK: The mechanism may apply, but it doesn't seem to work for Yucca Mountain based on Jean's data?

BODNAR: Correct. Correct.

CLINE: I'd like to clarify one issue. The second slide that I showed that gave the youngest constraint for arid inclusions with elevated temperatures showed some fluid inclusion as to how much it is homogenized around 40 degrees Centigrade. They were outboard of opal that we dated at about 5.8 million years.

I didn't mention this in my talk, but that's the only assemblage of fluid inclusions with two phases that we found outboard of the silica mineralization. So, the record of temperatures hotter than 40 degrees C is always

constrained below that silica mineral assemblage which is definitely an older part. This is the single unique data spot that gives us temperatures of about 40 degrees C at less than 5.8 million years.

The fluorite, if you put the fluorite in paragenetic contacts, it is most frequently adjacent to the host rocks. It looks as though it may be part of or just after the vapor phase mineralization. In a few samples, we see some fluorite that is a little bit outboard of that and in some of it really are calcite. We're actually in the process of doing some dating uranium-thorium-helium dating on some of the fluorites and those studies are yet to be completed. We need to determine the closure temperatures, but we've dated samples that were adjacent to the wall rocks and they give us ages of more than 10 million years and then we see it just inboard and we get ages of about 7 million years. Very consistent with the paragenetic story that we've put together, also consistent with the fluid inclusion temperatures, higher temperatures in the older fluorite against the host rocks, slight lower temperatures inboard of that, temperatures of around 40 to 45 degrees C.

PARIZEK: Do you know of any analogue work that's been done? We're big on analogues. If you went, I guess, to Paiute Ridge where there's a volcanic signature, has anybody done any fluid inclusion work there and, if so, what did it

show or would there be any reason to do that? It seems like your story is so clearcut, why go any further with any of this except why is it blue? Then, we have a climate person, Dr. Sharp. Maybe, she could help us say what was going on in the climate at the time when you needed this and you suggested various people you've talked with about climate change. So, I don't know whether you've gone as far as you can with sources of the blue calcite.

CLINE: Yeah. I can't comment on--

PARIZEK: Do we know why it's blue? It's good stratigraphy. That seems to apply to so many of your thin sections and isn't that good enough?

CLINE: We think so.

PARIZEK: From the program point of view, when would they stop funding you, in other words?

CLINE: Having dealt with QA, soon is okay. I can't really comment on any--I'm not aware of any fluid inclusion data at the other site. Some of the USGS folks may know about that.

WHELAN: Can I get one of my slides back; #8? Well, first of all, to comment on Jean's uranium-helium ages on fluorite, I believe they agree quite well with our time/temperature curve that we had up there. I wanted to get ta slide up that shows one of our sections just to address our model for surface tension deposition of the later

minerals in these occurrences. As you can see, the crust, the base of the crust in this case was quite porous. In fact, some later solutions have come through and dissolved some of the calcite in the base and this crust was essentially loose. We think that the late calcite on the top of crusts like this form from solutions that entered the cavity, move along the base of the cavity which is quite porous. It's heavily altered with the porosity of about 50 percent or more. Then, by surface tension, moved up the walls of these blades to their tips where, through evaporation, calcite and/or opal were precipitated by evaporation of the solutions.

WONG: Dr. Knopman?

CLINE: Could I just add one thing? I'm sorry, I didn't understand your question. The blue that you see in all these sections is epoxy that was used to stabilize the sample.

PARIZEK: Oh, no. I meant the calcite. I meant the calcite.

CLINE: The calcite?

PARIZEK: Well, the coating that you showed us in the slide that--not the background blue, but the--due to the magnesium-calcite rich zone.

CLINE: Yeah, those are not blue. I'm not sure if I made the point confusing or clarified it. But, the blue that you see in all of these sections is epoxy. So, there's no

blue mineralization.

KNOPMAN: I have a question for Yuri Dublyanksy. Dr. Bodnar raised several points and I'd like you to respond to them directly if you could. What's your explanation of the relative lack of quartz?

DUBLYANSKY: Lack of quartz is quite a little bit misleading expression. We do have quartz and we do have systematic change in mineralogy from north portal--well, essentially, it just repeats the change in the temperature, and we have more silica phase, more quartz, more chalcedony near the north portal where we have higher fluid inclusion temperature, you have much less of these minerals in the western part of the repository or--well, the ESF where the temperatures are lower. So, I would not agree with the statement that we don't have quartz or we have not enough quartz.

In terms of interpreting morphology of crystals, well, if you interpret--

KNOPMAN: Excuse me for one second.

DUBLYANSKY: Uh-huh?

KNOPMAN: Before you get to that, if I could just stick with a point that we haven't gone over yet.

DUBLYANSKY: Okay.

KNOPMAN: And, that's the temperature reversals. If they're not seen, how can the hydrothermal upwelling

hypothesis hold?

DUBLYANSKY: Well, temperature reversals, they depend first on the number of upwellings. Just imagine we have one upwelling, we will have no temperature reversal. That's one. Secondly, these upwellings which we hypothesize, we model--it's our conceptual model--they occur along the fault line, but that's not necessarily distributed uniformly along the area. If you can see--well, if you look at the modern temperatures of the water table, you can see increased temperature along faults, but it's not linear increase along faults; it's a hump of hot water. So, if you have this upwelling, we expect it to be localized in space. So, one upwelling can heat along the fault near the repository block, the second one can heat like 10 kilometers north and we will just not see these records.

The third point, in terms of mineralogy, we do see at least two stages of mineral deposition. In terms of fluid inclusion, at least in one sample, I can see two parcels of thermal water which will decrease, but it was two distinct parcels of this water. So, the paragenesis is there.

KNOPMAN: But, that's not--the question is timing. So, I still haven't heard an explanation of the young ages or the lack of the evidence of the higher temperatures in the most recently deposited calcite.

DUBLYANSKY: Okay, two points. First, I tried to make a

point that the fact that we observed only the inclusions does not necessarily mean that we had any of this--was cold water. Cold water is the water which--atmospheric precipitation which percolated into the earth crust and just acquired the temperature of the bedrock. In the case of--in our case, we cannot just--we just cannot tell water with the temperature of the hot rock produced by normal geothermal gradient or was the temperature twice higher. So, we just cannot tell. Those are not input of thermal energy. Just the method, the fluid inclusion method, does not allow us to make this conclusion. So, we cannot just tell here's--we have only to conclude, therefore, it's cold water. These conclusions just cannot be made.

KNOPMAN: And, what about the veins, the lack of veins that would show that there had been, in fact, an upwelling?

DUBLYANSKY: Lack of what?

KNOPMAN: The first--veins, veins.

DUBLYANSKY: Veins. Well, in my report which was done three years ago, I showed one photograph of a vein which is just normal classical vein. The simplest explanation is the model which we just produced. In the repository zone, we're kind of away from the major zone where the fluid upwells. So, we--in sort of the side of the mushroom of ths hydrologic mound which was transient in nature and just was percolating down. So, we do not expect to have complete saturation

there. To form a vein, we have to have complete saturation for a long time. It's very slow growth within this vein. What we envisage the growth of those mineral in transient perched water bodies which causes isotopic signature from deep below this positive carbon, which we cannot just explain by any other source of carbon. Then, they were interacting with other waters and deposited in the perched water bodies. So, veins should not necessarily form there.

KNOPMAN: Okay.

DUBLYANSKY: Sorry, I had a comment on the ages. In terms of ages, if we accepted the ages, uranium-lead ages-- for instance, we have set the notion that we have hot fluids, 50 to 60 degrees Centigrade sitting in the rock over a period of time which we have from ages, say, from 6 million years to 5 million years. So, we have to have water in the rock for 5 million years and we have to remove heat over this million years. I think this is just thermodynamically impossible. You just have to remove so much heat that you have to pump much, much water there and this water has to be hot. The heat force that will result from this, they just--well, just unreasonable, I guess. Therefore, we have problem with dating and I think now we know the reason why these ages can be probably incorrect.

KNOPMAN: Dr. Cline, do you want to comment on the question?

CLINE: Yes, I just wanted to clarify your question concerning mineralization and the location. When you look at the paragenetic sequence and build the growth history of the secondary mineral story across the site, what you see is that the silica phase is deposited relatively early, the opal and the chalcedony and the quartz. There are more silicate minerals in the north ramp, but that is simply because more of the early part of the history is recorded at those particular sample sites. If you move into the intensely fractured zone, those are mostly fracture fillings and we do not see vapor phase mineralization in those. So, those fractures happened sometime after the lithophysal cavities were formed and they began recording a history of mineral precipitation later than, say, the north ramp did. So, there's very little silica mineralization there simply because that mineralization--that that part of the mineral record wasn't recorded in that part of the site. And, if you keep in mind your paragenetic context, you can account for the distribution of all of the different minerals really across the repository site.

WONG: Okay, thank you. I would like to thank all of the speakers from this morning's session. It was very interesting and thank them for their cooperation as we tried to manage time.

Now, it's my duty to turn the meeting back over to

Chairman Cohon.

COHON: Thank you very much, Jeff. Thank you for an excellent job as Chair and you were the real Chair. And, thanks to all of the speakers this morning.

We turn now to public comment period. I have a sheet in front of me with five names on them. I'm going to read them, and if you're here and you still intend to comment, I'd like some indulgement of that and then we'll see if there's anybody else who wants to comment. This is also that I can do appropriate time allocation.

Jerry Szymanski? I apologize if I'm going to mess this name up because I can read it very well. But, Marvis Alern, Allen? Marvis? Okay. Someone might have just signed up without knowing they were signing a public comment. Brian Marshall, Arjun Makhijani, Kevin Kamps. Okay. Is there anybody else who cares to make a public comment?

(No audible response.)

COHON: Okay. So, we have five people in 25 minutes. I would ask each of you to limit your comments to five minutes. I apologize for that, but if you're very careful, you can get it done.

MAKHIJANI: (Inaudible) for one minute.

COHON: Who said that? Okay, I'll hold you to that.

So, Dr. Szymanski, you get nine minutes and the clock is ticking

SZYMANSKI: My name is Jerry Szymanski for the record. I do consult occasionally as independent consultant to the Attorney of the State of Nevada.

During the completion of the UNLV project, essentially--personally a long, long saga and nearly two decades long to understand how the system behaves in the long-term. We have developed an impressive database. I think UNLV researchers need to be complimented. Their meticulousness, openness, and just sheer size of the database is truly impressive. Well, the problem is interpretation. One would lead to a search for a new site. It's a very painful process for the nation. On the other hand, the other alternative leads us to continue with Yucca Mountain and perhaps introduce a parallel without a precedence for the future generation. There are two choices.

Now, with the completion at this point in time, I would like to terminate my involvement and I will not any more take any more of the Board's time. Now, it will be the question and the choice, it will be for you. Therefore, I have with my colleagues decided to produce to the Board the last document which I think could help to interpret this data and to develop a position as far as the Board is concerned.

In closing, I would to express my gratitude for the Board indulgence in listening to me over this last several years. Thank you very much.

COHON: Thank you, Jerry. It was very gracious of you. It's not necessary to thank us. I'm going to give this to someone more responsible than I. So, there's no question. Are we on? Jerry, it certainly not necessary for you to thank us. I think everybody owes you thanks for your commitment and the creativity and perserverence that you brought to this problem and we will miss you if it's true that we will not see you participating again in our meetings.

Brian Marshall?

MARSHALL: Brian Marshall, U.S. Geological Survey. I just have a comment on Yuri Dublyansky's presentation that I just cannot let go unstated. That is he stated that--or he concluded the values of paleo heat flow cannot be accounted for by any known event in the thermal history of Yucca Mountain.

Now, I have recently been doing some thermal modeling of Yucca Mountain and I'd just like to turn your attention to his presentation, Page 11, the second slide. He has an assumption stated on there which I don't think he mentioned in his presentation, but it is on the slide. It says that the surface topography is assumed to be unchanged. What he has done is calculated a thermal gradient based on a fairly high fluid inclusion of homogenization temperature that is found in the north ramp of the ESF at a fairly shallow level and he's used the modern topography of the

surface, the shallow depth, to calculate a thermal gradient for a fairly high temperature which, as we've seen from the presentations by Jean Cline and Joe Whelan, probably occurred very early in the history of the mountain. And, by doing so, he comes up with a heat flow unit value of 70 which corresponds to a thermal gradient of over 1,000 degrees Centigrade per kilometer. I'd just like to point that out to the Board.

I'd just like to make one other comment and that is preliminary modeling suggests that the thermal history of Yucca Mountain is readily explained by the response of the Yucca Mountain area to magmatic activity within the Timber Mountain caldera complex, as shown by Joe Whelan.

COHON: Thank you.

MARSHALL: Thank you.

COHON: Do you care to--go ahead, Dr. Dublyansky?

DUBLYANSKY: Yeah, probably. Sorry I didn't make it clear. I presented the calculation which was done, indeed as was indicated on the slide, in the assumption that topography did not change. I did the calculation which Brian has suggested. I used the most conservative which is the highest erosion rate which would lead to removal of about 100 meters of rock from top of the mountain. I repeated that calculation. I just did not have time to show them there. Even though, if you use this, it was the highest--well,

unrealistic even by DOE assessment erosion rate, and even if you assume that the old temperatures was there 10 million years ago. So, the most conservative assessment. You have the heat flows--the highest heat flow is 20 or 25 heat flow units. So, it still cannot be explained and I stand by this stuff.

As far as the comment that I implied--this thermal gradient implied--of a geothermal gradient of 1,000 degrees per kilometer, I think it's just absolutely irrational to make those gradients linear. In hydrothermal system, we have very sharp gradient near the surface and then almost an isothermic environment downwards. So, I don't think it's just scientifically a justifiable assumption that this--I implied that the gradient was linear, it was 1,000 degrees per kilometer. I did not that--I didn't mean to do it. Thanks.

COHON: Back to you. Do you want to say anything? Go right ahead?

MARSHALL: This is Brian Marshall again. I'll just say that the equation to calculate the heat flow is the heat flow equals the thermal conductivity times the thermal gradient. It's just a multiplication. So, I'm just saying that the near-surface thermal gradient that would result in a 70 heat flow unit value based on the thermal conductivity that you gave us, which is reasonable, I think, would calculate out to

be 1,000--over 1,000 degrees per kilometer. That's all.

DUBLYANSKY: I agree with that. It's perfectly correct physically, but it's geologically unreasonable to propagate this short--well, this very steep gradient down--well, you can propagate this gradient down to the center of the earth and you will have the temperature of the sun, but I don't think it's reasonable.

COHON: Okay. I don't know if we made progress on that one. Thank you both for your comments.

Okay. Dr. Makhijani?

MAKHIJANI: Okay. Do I get my four minutes back?

COHON: Yes, you do. In fact--

MAKHIJANI: I may not use all of them. I'm Arjun Makhijani from the Institute for Energy and Environment Research. I get lost in all the geology. I stayed away from Jerry for many years and then Yuri showed up at my shop with a bunch of minerals a few years ago. I'm proud to be head of an institute that to date, I think, has sponsored the only research that has wound up in a peer review journal on fluid inclusion. It cost us about $20,000 so far as I know. I could stand to be correct on this.

I would suggest to the Board that the research that has been presented by Dr. Cline, first of all, that all that data be made available to us. So far, it doesn't seem to be in a state to be made public, but I believe we should have it

before we do a final signup. We are fiercely independent and we would like to see this material. And, we would also suggest that there be some publication sponsored by the DOE before there's a rush to judgment. I think that there is still a fair amount of controversy about this.

Listening to all the presentations, you know, I think I know something and I got confused all over again. I think that Yuri has suggested some things and I would like to suggest some things that could make a better resolution to the scientific issues. I do not believe that we have a satisfactory resolution to the scientific issues, given that there are people who are very fine scientists and recognized in their own field who have produced a body of valuable data. There is agreement that there are fluid inclusions with high temperatures. There are two phase inclusions. I didn't hear anything about hydrocarbons. I would like to know whether hydrocarbons were detected in some of your samples.

Certainly, the report that we produced had some mention of this as additional evidence of upwelling. However, this is not an issue that I would like to settle because I'm not qualified to do it, but I am going to suggest that Yuri has said that this repeated upwelling may occur along the various fault lines, though not in the same place. That clearly suggests a research agenda that if we know where these faults are, we should go and see if there's

similar evidence and it may not be a sawtooth in time in the same place, but it may be a sawtooth in time and space and we have not investigated this hypothesis. It's a very important hypothesis that should be investigated before the NWTRB settles this particular question.

The second thing is in the USGS presentations, I did not see--now, I am from the physical sciences, physics and electrical engineering and the nuclear side of things. I did not see a whole lot of thermodynamic discussion. Yuri has been trying to educate me on the thermodynamic side of geology, but I think some conceptual thermodynamic model that is defensible and the physics--in the second of all sense--and the first of all sense should be put on the table as part of this whole rainwater explanation before the NWTRB signs off on it.

I have a request of Dr. Bodnar. There was an unresolved issue on the silica deposits that I think can be cleared up right away. Yuri said that you see a lot of silica where the temperature was hot--that is, at the north portal--and you don't expect to see silica elsewhere. And, perhaps, Dr. Bodnar could comment on this particular thing whether that observation is correct or not. So, one issue, at least, in this technical person's mind can be settled and I'll go away with one less question.

Thank you very much for your time.

COHON: Thank you. Now, Dr. Cline commented on that last issue already. Is Dr. Bodnar here to--here he comes.

BODNAR: I'm not sure exactly what the question is, but, yes, there is more silica, as I understand it, near the north portal. The temperatures there are warmer. That's totally consistent that very early in the system when it was warmer, there was more silica in solution, but you don't see as much of the calcite there, as I understand it.

COHON: Well, just pursue the line. If Dr. Makhijani disclaims any knowledge in this area, I have any less. So, my question may sound particularly stupid. But, I thought I heard you say that one of the important signatures of upwelling systems is the substantial presence of quartz and silica and that we don't see very much of that at Yucca Mountain. The response to that was, well, there is some, but it's in this one spot where it's warm. So, could that be indicative of at least one part of the system which has been subject to upwelling?

BODNAR: Well, I think--this is a personal opinion now--that the early part of the mineralization at Yucca Mountain that most of the people who are working on it would characterize as hydrothermal, but that's the very earliest part of the system when there is silica in the system. Now, by hydrothermal, I'm not implying necessarily upwelling fluids, downwelling fluids, sideward, moving fluids; just

fluids that were heated enough to carry quartz. The implication for the upwelling fluids is that if fluids are coming up from depth, they would necessarily be hotter regardless of when they came up and they would be carrying silica. So, you don't see that silica in the later stages. I don't know if that clarified it or just muddied the waters more.

COHON: So to speak. Well, Dr. Cline is going to clear it all up for us.

CLINE: We have a couple ages on some samples from the north ramp from some mineralization that is above the silica mineralization and they constrain that silica mineralization to being older than 4 million years. We can't get constraints that are older than that. So, they show the silica mineralization being essentially consistent with what we see in the paragenesis which is to say older. I would also comment that this hotter spot is close to the north portal and it's closer to the surface than some of the other mineralization that is cooler. So, if one wanted to propose the hypothesis that there is hotter mineralization closer to the surface and cooler mineralization at depth, that's consistent with what we see. Now, we haven't really tested that, but someone could suggest that the hotter mineralization closer to the surface might be related to the volcanic activity.

COHON: Thank you. Dr. Dublyansky?

DUBLYANSKY: I have to comment on the ages of silica from north portal. We have a report done by USDOE also USGS and some of silica, some of chalcedony from the north portal area have uranium disequilibrium which means that the age of this silica has to be less than half a million years. So, I would not just immediately agree that the silica is always the oldest part for the mineralogical record.

COHON: Okay. Well, here comes USGS.

WHELAN: Whelan, USGS. Just to address what Yuri just said and Jean pointed this out, as well, silica as opal is quite common in the intermediate and late stages of deposition. It occurs throughout. And, frequently, has young ages, as young as less than 10,000 years for very thin outer layers of that opal. So, while there are young ages for opal, that really doesn't provide much of a constraint on the formation of fluid inclusions in early calcite.

The other thing I would like to mention is that there really was another stage, another time when the tuffs were hot and we haven't discussed it. But, at the time that they erupted, they had a cooling period of hundreds or maybe thousands of years when they were quite high. And, there are fumarolic alteration deposits exposed in the tunnel, exposed at the surface that formed during this early period of tuff cooling when temperatures really could have been

conservatively warmer than 100 degrees Centigrade, probably were.

Just stating that as a possibility, the hotter temperatures that we see in the north ramp could conceivably represent calcite deposition in a conduit to one of these fumaroles at the surface, but that is pure speculation. I just thought it was--it's another period in time when the tuffs were hot and I thought maybe it needed to be pointed out.

COHON: Thank you. Kevin Kamps? And, we'll please try to keep it to five minutes. Thank you.

KAMPS: Thank you, Chairman Cohon and members of the Board, for this opportunity to comment.

I just wanted to speak to you about what's happening out in main street USA. I've been traveling a lot since the last time I was able to speak to you in August and I've not been able to attend any other meetings in between. I've missed most of this one because I just got back from Minnesota. I've been meeting with people who are concerned about the Yucca Mountain Project, especially the transportation implications. I just wanted to share with you some of what's happening out there.

I think it's important to say that the public everywhere that I've gone has seen this Board as one of the last lines of defense in a very politically-charged issue.

The public is feeling very steamrolled in the public participation process. Just as an example, on Friday, the Department of Energy initiated its Yucca Mountain site recommendation process by sending a letter to all the governors and legislatures in the country. There's tremendous concerns in these transportation corridor communities about this because during the draft environmental impact statement process, there were over 11,000 comments submitted to the Department of Energy and well over half of those had to do with transportation. And, there has yet been no Department of Energy response. Based on a meeting that we had with the Office of Civilian Radioactive Waste Management just last Wednesday, it appears there will be no comment to those--there will be no response to those public comments before the site recommendation itself is made perhaps later this year.

I should add that even the people who are able to participate in public hearings, many of those communities had to fight tooth and nail to win a public hearing in their community. Just some examples of that being Chicago, Illinois and Lincoln, Nebraska, major transportation hubs for the proposed transportation to Yucca Mountain. Only in the last waning days of the public comment period were these hearing held. The project maps of projected targeted transport routes were only released in the closing last

couple of weeks of the public comment period. So, for all of the public comment, preceding the release of those maps, people did not have an indication of what routes were being talked about. Just having come back from Minnesota, there was a lot of concern among people there because the Department of Energy maps for Minnesota show a route going right through the Twin Cities right on Interstate 94 and they pointed out that there are restrictions for hazardous materials because there are tunnels going under the Twin Cities on that route. So, there's lots of questions about what routes are going to be used that are not being answered.

Now, again, with the release of the science and engineering report and the soon-to-be released preliminary site suitability evaluation report, there's talk of having more hearings and eliciting more public comment. And, the public is just baffled about the request for more public comment when their comments from before have not been responded to. So, there's a real loss of faith in this entire process. The public feels like it's participating in good faith and does not feel that that's being responded to.

On similar lines, we've had recent meetings with the Environmental Protection Agency about the soon-to-be released radiation regulations for Yucca Mountain. It's been communicated very clearly to the Environmental Protection Agency from a coalition of national, regional, and local

Nevada based groups, environmental and public interest groups, that there are tremendous concerns about this standard that's being proposed.

The Safe Drinking Water Act application to the groundwater at Yucca Mountain is probably a foremost because it's already a source of drinking water for people downstream at Armagosa Valley. The cutoff point of 10,000 years continues to be a concern when the worst doses to the public will probably be 100,000 years into the future. Even the definition of the reasonably maximally exposed individual, assuming that that person will drink bottled water as a part of their diet instead of being a subsistence farmer who gets all of their water, drinking and irrigation and livestock water, from the groundwater under Yucca Mountain. And, even that 18 kilometer buffer zone, there's a joke that keeps coming up at your meetings that we're talking about putting wheels on the fence line at Yucca Mountain with the five kilometers at WIPP and now a proposal for 18 kilometers at Yucca Mountain. Wheels on the fence line and the moving of goal posts continually at Yucca Mountain. The public is very concerned that it's going to be a domino effect. As soon as EPA's rule comes out, then the NRC licensing rule will come out, the DOE guidelines rule will come out.

The public has participated in these comment opportunities faithfully and never received responses and

feels like it's being steamrolled in this process.

Just a couple more points. You were probably too busy with the meeting today to hear the Diane Rheme Show today, but there was a Nuclear Energy Institute spokesperson on the Diane Rheme Show and again this assurance that there's been decades, 30 years, of incident-free transportation of high-level nuclear waste in this country. I think the number they used was 3,000 shipments. And, this just flies in the face of information that the public has.

In Minneapolis, there were conversations about a northern state's power shipment that was stranded in Lacrosse, Wisconsin for several days because the train crew walked off the job and refused to deal with it because it was contaminated. There had been an attempt to jury rig extra shielding on that shipment, but the crew refused to deal with that shipment. So, it sat in the train yard in Lacrosse, Wisconsin for several days.

People from Lacrosse, Wisconsin were at this meeting in Minneapolis and they talked about a time when dairy lands power tried to ship fuel to the Morris, Illinois holding ponds and the casks were warped and they could only hold half of the fuel they were supposed to hold, and immediately after the shipments were completed, the NRC and the utility company admitted that the casks were not road-worthy. So, they were retired immediately after the

shipments. But, it was known going in that they weren't road-worthy.

COHON: Mr. Kamps, I'm sorry, time is running short. Could you wrap up?

KAMPS: Okay, yeah. Just to wrap up, the public sees this as a very politically-charged process and really looks to this Board for objective science and technical expertise. Thank you.

COHON: Thank you for comments. Now, someone else raised their hand and I didn't get a name down. Thank you. Please, identify yourself?

PACES: Thank you, Mr. Chairman. My name is Jim Paces. I'm from the USGS. After the last comment, I kind of hate to stir the pot of the secondary minerals up again. But, as a geochronologist that's been involved in this project, I just feel the need that I have to investigate one of the implications that Dr. Dublyansky made during his presentation, specifically on the uranium-lead dating. We know how important that is to establish a temporal framework. So, we really need to make sure we understand what's going on there. UNLV has produced--and I don't know the exact number, but there are several dozens of uranium-lead dates on these various different phases of opal and chalcedony. The Survey has produced probably 150 to 200 analyses. There's been a similar number or larger number of uranium series and

radiocarbon ages. Most all of these data are consistent with the microstratigraphic positions within these mineral coatings, as Dr. Cline and Joe Whelan were showing, and none of them are older than the age of the host rocks. So, it seems to me that it's very convenient for Yuri to now ignore this large body of data that shows a very large degree of internal consistency. I would just like to know his specific problems with either the way that these data have been collected or with the theoretical aspects of uranium decay in mineral systems that makes him want to ignore all of these data and not apply it to his time/temperature.

COHON: Before you walk away from the microphone, could you spell your last name?

PACES: P-A-C-E-S. Ten paces, turn, and fire.

COHON: Dr. Dublyansky, would you like the last word here?

DUBLYANSKY: Well, I have to say that I would more than happy to accept those ages; those ages, just accept it at face value. We are talking about the existence of hot water in this mountain 5 million years ago which is well-within this regulatory concern period. The problem is--I'll try to delineate my problems with these ages.

First, if you just take them at face value, they produce rate of deposition of this crust which I just cannot explain to myself, cannot convince myself that it's possible.

I was showing you the slides which rates degrees of super saturation, very low degrees of super saturation which we have to maintain for millions of years. I just cannot imagine the environment where these rates could be correct.

Secondly, if you just use these ages like 8 million--imagine you started working with the mineral 8 million years ago, 10 million years ago and you know that they lost temperature of 50 degrees towards about 5 million years ago, you have 5 million of years with the elevated temperature sitting just near the surface of the earth. So, essentially, you create a huge machine which remove the energy from earth just through the normal heat flow. I don't think such a system can exist for millions of years. Well, I think it's a violation of first law of thermodynamic.

But, I do recognize that the ages seem to be--well, seem to be very reasonable from the standpoint that they do not--they are not higher than age of the rock and they decrease with paragenetic time, relative time. And, that's if we do have the explanation for that, the uranian-lead age calculation based on the assumption. So, before you calculate the age from measured ratios of lead and uranium, respective parent and daughter, you extract and support common lead correction which is the amount of lead which has been introduced with the fluid (inaudible). For instance, this lead is not produced insitu due to decay of uranium, but

it's introduced into the system. You have to remove this addition. And, all calculations are based on the assumption that this common lead correction is constant in time. So, you remove the equal amount of this radiogenic lead which was introduced into the system and you obtain these ages. But, within the system which we have hypothesized (inaudible), we expect that at the early stages of the (inaudible) we inject the fluid which has significantly offset a significantly higher amount of radiogenic lead injected in the crystallization side. Therefore, we can just--by removing this fixed common lead correction, we just make appearance of the ages to (inaudible); whereby, it's not ages (inaudible) but this common lead correction is changing the style of it.

COHON: Okay. Thank you, Dr. Dublyansky. And, thank you all for your comments. And, thanks to all who participated in this meeting, both speakers, scheduled speakers, people speaking in the public comment period, Board members, those who served as Chairs. This was a meeting from which we got a great deal. My thanks to the staff who planned this meeting, both its content and its logistics.

We meet again September 10 and 11 in Las Vegas. We hope many of you will be able to be with us then. But, don't forget both our Panel meeting that's happening--what are the dates for that? June 20-21. And, our metals/corrosion workshop probably July 19 to be finalized.

Our thanks to all. We are adjourned.

(Whereupon, the meeting was adjourned.)