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SCIENTIFIC AND TECHNICAL ISSUES

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P R O C E E D I N G S

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(8:00 a.m.)

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

9 This summarizes the results of the drift scale
10 test, THC modeling and analysis, fracture matrix interactions
11 very weak in the condensation drainage zone, water-rock
12 interactions strongest for calcite and silica polymorphs.
13 Then, you pick up aluminosilicate, K-feldspar in particular.
14 I've showed before a comparison of our models to gas-phase
15 CO₂ concentrations and how, in general, we do a pretty good
16 job of predicting the evolution of CO₂ in the gas-phase and
17 also pH which is obviously directly related.

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

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

4 ³⁶Cl validation, I don't think I have to say
5 probably too much about the purpose. We're validating.
6 We're trying to validate the occurrence of bomb-pulse at two
7 locations in the ESF, the Sundance Fault down by Alcove 6 and
8 the Drillhole Wash Fault which is just before the ECRB
9 intersect. If you go back to the Parumph meeting, I believe
10 that was--it's been a while back now. Livermore and Los
11 Alamos PIs presented results on validation core on the core
12 that was taken from these locations and the Los Alamos
13 results in ³⁶Cl, the chloride ratios were consistent with what
14 June Fabricca Martin and collected previously from the area.
15 Didn't find bomb-pulse, but the background is like 800 to
16 1000 10^{-15} , whereas Mark Kappy from Livermore was coming up
17 with numbers more like 50 to 150 times 10^{-15} . So, we had
18 splits of the same core and we were getting different
19 numbers. So, went about to try to figure out why we were
20 getting those differences.

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

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

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

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

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

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

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

1 in the ESF. We're looking at flow and seepage processes at
2 the scale of tens of meters. This is just a schematic
3 diagram showing the layout of that. Again, this is about 18
4 meters. Infiltration plot in the floor of Alcove 8 and Niche
5 3 down in the ESF where we're monitoring the movement of the
6 water, the tracer front both from downlooking and uplooking
7 boreholes and also collection trays within Niche 3. To cut
8 to the chase, I'll talk about this. We have seen seepage
9 into Niche 3 at this point. So, we'll talk some about that.

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

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

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

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

25 Infiltration in Alcove 8 in liters as a function of

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

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

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

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

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

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

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

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

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

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

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

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

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

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

20 Some pictures, three pictures to be specific.
21 Remember there's drip cloths hanging in part of this
22 bulkheaded section back towards the second bulkhead at about
23 2500 meters from the intersection of the ECRB. Here's a drip
24 cloth here. You've heard about the mottling or the blue
25 streaking. This is an example here. One of the drip cloths

1 showing the evidence of what we're attributing to
2 condensation. Here's an area on the conveyor where water
3 collected. You can see it's pretty dirty. That's
4 appropriate to the next slide. Here's another set of drip
5 cloths showing the same mottling and here, as you all have
6 heard before, in some cases we have some rock debris that has
7 fallen on the drip cloths; small, relatively small, smaller
8 than fist size type rock that can make it through the mesh.
9 Small print here, but it's probably clearer in your
10 hard copies. We've taken water samples. What do we mean by
11 dark and clear water? Some of the water was laying on top of
12 the conveyor. The conveyor has rock, dust, it's been sprayed
13 with J-13, it's for dust control. And so, we've done
14 analysis of that water, but it clearly contaminated; very
15 high ionic strength. It makes no sense, whatsoever, in terms
16 of even being seepage water. There is some containers along
17 the conveyor where we've actually captured some water. When
18 we analyze that water very dilute, it looks like condensate.
19 Preliminary measurements, now this is the kind of
20 information in my opinion that we can get some good chemical
21 information and also isotopic information that could tell us
22 a lot about whether it's condensation or seepage. We're
23 looking at improving our water collection capability, as
24 well, possibly interacting them in some way with the drip
25 cloth.

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

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

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

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

1 give us the information on the Calico.

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

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

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

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

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

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

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

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

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

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

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

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

1 We did a series of four isolated interval using
2 different screens, pump tests. We've also done a series of
3 three single-well tracer tests where we inject tracer
4 sometimes with no rest, meaning we pump it right back, and in
5 other cases, we shut in the well and then pump back after a
6 certain period of time. The next plot will show some results
7 from that last--the last of those three, but the preliminary
8 results from those tests indicate insignificant diffusion
9 from the groundwater into the stagnant water. So, we've got
10 an invection dominated system. There is some dispersion
11 along the flow path, however, and we continue to try to
12 quantify effective porosity from the test results.

13 Implications for TSPA, this is the bullet that I used last
14 time. That continues to be borne out. What we're using in
15 TSPA in terms of single porosity continuum is acceptable for
16 alluvium, at least in the ATC area. I mentioned the multi-
17 well test. Now, that we're finished with the single-well
18 tests, we'll move into Nye County and we'll drill some
19 additional injection monitoring wells and those will be
20 installed, we're hoping, later this calendar year. Then,
21 we'll start a series of cross-hole hydraulic and tracer tests
22 again later this calendar year. We'll look at a whole host
23 of parameters for the model.

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

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

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

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

23 Okay. Now, switching gears to engineered barrier.
24 The ventilation test at Atlas doing a 1/4-scale test for
25 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

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

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

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

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

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

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

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

9 What have we done with some of these pore waters?
10 This is just a set of bullets that give some highlights of
11 what we continue to do to look at the environment on the drip
12 shield and the waste package. We continue to do work. This
13 is done at Livermore. Greg Gdowski is the PI for this work
14 continuing to look at evaporative concentration of pore
15 waters. We're looking at pore waters. There pore waters
16 happen to be similar to waters that we collected from Alcove
17 5 when we did some ultracentrifuge work to try to get a
18 handle on the Topopah Spring pore water compositions. We're
19 looking at again trace element concentration in brines and
20 continue to look at the effect of PCO_2 on the evolution of
21 those brines.

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

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

11 Some highlights of results. Here's some results
12 from Alloy 22 and calcium chloride, calcium nitrate water at
13 120 degrees Celsius. This is cyclic polarization data. I
14 don't have plots here, but we could compare this to Alloy 22
15 performance and cyclic polarization experiments under other
16 water chemistries and it would be very similar. We not
17 seeing--we're seeing passive film development, but no
18 evidence of passive film breakdown. So, Alloy 22, similar
19 again to Alloy 22 tested in other environments. It continues
20 to look very promising in terms of passive film development
21 and very robust passive film. But, as we heard yesterday,
22 we're continuing to look at that including a peer review.

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

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

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

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

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

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

5 PETERS: I saw that.

6 WONG: Questions from the Board? Dr. Runnells?

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

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

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

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

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

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

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

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

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

25 RUNNELLS: Right.

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

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

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

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

24 PETERS: Right.

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

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

8 PETERS: Well, I can't go into the holes and look at
9 them specifically right now to tell for absolute sure that
10 it--but the magnitude of precipitation and dissolution that
11 we're seeing in the field from the limited samples we've
12 taken during heating and also from the modeling that we've
13 done would suggest you couldn't produce those kind of shifts-
14 -

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

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

22 RUNNELLS: Right.

23 PETERS: So, we think that it's primarily driven by the-
24 -

25 RUNNELLS: Very good, thank you.

1 WONG: Dr. Parizek?

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

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

9 PARIZEK: Okay.

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

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

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

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

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

1 PARIZEK: Okay, that's where it is.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

14 PETERS: Okay.

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

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

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

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

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

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

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

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

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

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

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

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

18 KNOPMAN: Yeah.

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

22 KNOPMAN: Right.

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

1 KNOPMAN: But, these would have different chemical
2 signatures?

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

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

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

16 KNOPMAN: Okay.

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

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

1 Gdowski is doing from Alcove 5?

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

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

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

12 GDOWSKI: --years.

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

15 KNOPMAN: And, there are results that we have?

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

18 KNOPMAN: Okay.

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

22 KNOPMAN: Okay.

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

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

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

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

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

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

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

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

25 GORDON: Yeah. Gerry Gordon, Yucca Mountain Project.

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

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

17 GORDON: Right.

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

21 GORDON: That's true. Actually, there are two slopes to
22 the $1/T$ curve. The activation energy varies from 20 to
23 60 kilocalories per mole. I didn't do the fitting, but I
24 think they did an average fit.

25 SAGÜÉS: And, how about the actual values of the

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

3 GORDON: Just the slope.

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

8 GORDON: That's correct.

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

11 GORDON: Right.

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

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

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

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

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

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

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

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

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

7 SAGÜÉS: Okay, thank you.

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

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

24 NELSON: Well, actually, could it seal fractures?

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

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

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

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

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

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

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

1 lower lith. We look forward to the information.

2 PETERS: You're welcome.

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

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

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

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

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

9 PETERS: That's--

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

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

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

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

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

1 temperature and saturation distributions observed in the
2 experiments.

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

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

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

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

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

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

1 WONG: Dr. Bullen?

2 BULLEN: How much time do I have?

3 WONG: 32 seconds.

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

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

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

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

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

11 BULLEN: Oh, you've got pictures.

12 PETERS: Yeah.

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

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

18 BULLEN: Okay.

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

20 BULLEN: Okay.

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

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

10 BULLEN: Okay, thank you.

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

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

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

18 BULLEN: Raveling or--

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

20 BULLEN: Okay, okay.

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

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

1 down the Solitario Canyon Fault or--

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

9 BULLEN: Okay. Do any of them leak?

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

11 BULLEN: Okay. Thank you.

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

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

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

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

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

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

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

23 PETERS: Right.

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

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

8 PETERS: The Crest Alcove.

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

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

1 CRAIG: Okay. Well, I--

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

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

6 PETERS: Right.

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

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

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

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

24 CRAIG: Right. What I'm--

25 PETERS: It's not just the measurement technique.

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

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

14 CRAIG: They don't exist now?

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

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

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

3 PETERS: Thanks.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

1 corrosion is not a credible process.

2 Now, we have evaluated the localized corrosion as a
3 function of several environmental factors, chloride being one
4 of the most important deleterious element in the environment
5 for localized corrosion and these are several of the alloys
6 that DOE has considered over the years for the container
7 materials. And, you can see that this is the repassivation
8 potential plotted as a function of chloride concentration.
9 It's a highly nonlinear behavior. For example, if you
10 consider that the environmental potential is somewhere at
11 zero, then 316L stainless steel would stop corroding at a
12 very low chloride concentration, 10^{-3} molar; 825 will start
13 corroding at a slightly higher chloride concentration; 625
14 will start corroding at even a higher chloride; and the Alloy
15 22 which is the current material will not suffer localized
16 corrosion unless you come very close to saturation with
17 respect to chloride concentration. So, using this concept,
18 by incorporating this model in our performance assessment
19 goal, we can also evaluate the idea of DOE designs on the
20 performance of the container and that is one of the things
21 we're doing.

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

24 SRIDHAR: These pHs are natural pHs. We have also
25 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

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

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

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

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

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

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

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

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

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

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

5 One of the experiments that we are doing--as I
6 mentioned before, to measure these vacancy movements in these
7 thin films is very difficult. So, what we are trying to do
8 is to get a handle on stoichiometry of the dissolution. Is
9 there a selective leaching of chromium or nickel or
10 molybdenum in the alloy? To do that, we have an Alloy 22
11 plate that has a very small cavity that is machined into
12 this. So, we placed the cell on top of this and we have
13 controlled electrode chemistry and then we can extract
14 solutions from here and this is, of course, is maintained at
15 95 degrees Centigrade or whatever is the temperature of
16 interest and we extract solution from here and using
17 capillary electrophoresis measure very sensitively the
18 concentration of various cationic species to get indirectly
19 at the rate of dissolution. So, these are ongoing
20 experiments and I don't have results at this point to talk
21 about.

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

1 stresses. One of the things we have considered is is there a
2 critical potential for stress corrosion cracking? Can we say
3 that if the corrosion potential of the material does not
4 exceed a certain value that stress corrosion cracking will
5 not occur? So, what we have examined here is a crack growth
6 rate on a pre-cracked sample measured as a function of
7 applied potential. This is the repassivation potential that
8 I talked about before on a non-stress specimen. So, this is
9 repassivation potential generated for localized corrosion and
10 what we see is that you have very high crack growth rates,
11 10^{-9} meters per second, and below the repassivation potential,
12 the crack growth rate reduces quite a bit. The arrows
13 indicate that this is our measurement limit, and if we wait
14 longer, we can measure lower crack growth rate provided there
15 is no crack growth. So, we have done these tests for up to a
16 year and--this is the kind of crack growth rate we observe--
17 show that below the repassivation potential, the stress
18 corrosion cracking susceptibility decreases quite
19 significantly.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

4 That's all. Thank you.

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

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

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

16 SRIDHAR: This one?

17 SAGÜÉS: Yes. I think that that's a revealing
18 transparency, and for the benefit of my colleagues and those
19 in the audience, I think that we want to look at the time
20 scale of that event. That is, of course, you create an upset
21 on the system and then you observe the amount of time that it
22 takes to do something. And, I've got a pointer here. You
23 know, over there, that's 1,000 days. So, that's about a year
24 over there. So, if you go in the same time scale, the 10,000
25 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

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

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

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

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

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

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

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

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

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

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

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

13 WONG: Thank you very much. Dr. Bullen?

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

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

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

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

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

5 SRIDHAR: Yeah.

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

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

12 BULLEN: Oh, okay.

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

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

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

10 SRIDHAR: Right.

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

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

15 BULLEN: Yes.

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

22 BULLEN: Okay.

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

25 BULLEN: Do you have a model that's temperature

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

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

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

9 SRIDHAR: Yeah.

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

12 SRIDHAR: My figures are getting scrambled.

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

15 SRIDHAR: Okay.

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

20 SRIDHAR: Peter Andreson.

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

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

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

14 BULLEN: Thank you.

15 WONG: Dr. Runnells?

16 RUNNELLS: Runnells, Board. We seem to be sort of
17 working our way backward through your slides. Could we have
18 31 in your backups, please? This concerns something you said
19 right at the front end of your talk, mainly--it's that slide
20 that shows the deliquescence humidities. You mentioned right
21 at the start of your talk that the particular mixtures of
22 salts are important in terms of the temperatures at which a
23 brine will form on the surface of a material. I agree 100
24 percent. I wonder if you could just explain the significance
25 of this particular slide in the context of your concern about

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

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

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

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

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

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

20 RUNNELLS: Okay. Thank you.

21 WONG: Any further questions from Board members?

22 (No audible response.)

23 WONG: Board staff?

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

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

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

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

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

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

7 (Whereupon, a brief recess was taken.)

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

20 The next set of speakers will be representing a
21 joint study by the University of Nevada at Las Vegas, the
22 USGS, and the State of Nevada, and I believe that much of
23 this work is again sponsored by the DOE. I need to express
24 special thanks to two of our speakers, Jean Cline and Bob
25 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

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

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

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

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

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

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

1 about carbon and oxygen isotope data, as well.

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

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

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

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

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

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

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

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

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

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

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

1 at different sample sites.

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

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

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

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

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

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

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

25 Okay. A real key question is where are these fluid

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

15 Thank you.

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

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

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

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

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

1 KNOPMAN: Thank you.

2 WONG: Any other questions from the Board?

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

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

1 speculation on my part at this time.

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

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

7 WILSON: 1.9.

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

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

22 CLINE: To me, that's low salinity.

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

25 CLINE: Magmatic systems commonly have salinities that

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

5 RUNNELLS: Okay. Thank you.

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

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

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

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

1 Mountain?

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

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

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

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

1 through the tuff and the results of interaction of this
2 rainwater with the tuff? So, we tried to answer this
3 question and we did some preliminary thermodynamic modeling
4 on it. How would rainwater, Yucca Mountain area average
5 rainwater, how would it interact with tuffs and what minerals
6 would be compatible with this system? So, this is just a
7 small part of the work and it's very preliminary. We are
8 acquiring that. So, on this axis, we have temperatures 25,
9 50, 75, and 100 degrees kind of model temperatures. And,
10 here is a ratio of rock to water. In other words, it's how
11 old this water is or how far was the reaction between tuffs
12 and the water and when? And, this preliminary analysis shows
13 either we can basically--those arrows indicate our estimation
14 of the rock to water ratio for Yucca Mountain based on two
15 different approaches and this is the rock scale also. So, we
16 can see at some elevated temperatures, we can have zeolites
17 deposited from these waters, but to form fluorite, even if
18 you raise the temperature up to 100 degree, fluorite is not
19 supposed to be deposited from this water unless you have
20 extremely, extremely high rock to water ratio which you can
21 hardly expect from the fluid which percolated through the
22 welded tuff over like 50, 60 meters like near the north
23 portal. The conclusion is its complex mineralogy indicates
24 complex and varying in time and space chemistry of water.
25 Minerals that are observed in ESF, for instance fluorites,

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

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

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

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

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

17 So, to summarize this part of my talk, I have to
18 say that, thus far, no coherent physical model explains the
19 mechanism of crystallization of large euhedral crystals from
20 films of water at Yucca Mountain have been proposed.
21 Examples of growth of large centimeter scale euhedral
22 crystals of calcite and quartz from films of water are not
23 known. We have been searching literature quite a bit on that
24 and my colleagues from the Institute of Mineralogy who have
25 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 millimeters 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

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

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

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

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

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

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

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

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

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

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

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

8 But, now, I want to address a question. What is
9 the significance of all-liquid inclusions which Jean Cline
10 just talked about? So, we know that fluid inclusion methods
11 does not yield information when the temperature drops below
12 55 or 50 degrees and, to date, we cannot exactly point to
13 this temperature. Just the shrinkage bubble stopped
14 nucleating there and we have all-liquid inclusions. So, all
15 we can tell about the fluids which we deposit in such
16 minerals, such as like magnesium-enriched calcite, they
17 perform below approximately 35 to 50 degrees Centigrade,
18 somewhere there.

19 Now, imagine, we have our minerals formed somewhere
20 in this temperature range or even lower and it was trapped,
21 for instance, just--as an example, at 30 degrees Centigrade
22 and it was trapped in the ESF at the depth of 100 meters, say
23 somewhere in the north ramp. And, this gives us heat flow of
24 6.5 heat flow units which is on this map of the distribution
25 of heat flows, it's way off. It's almost three times as high

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

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

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

1 1 millimeter thick and the isotopic properties change in a
2 systematic manner and the range of these changes are dramatic
3 +8, +9, to -8, -9 per mil. On this graph, I also show the
4 area for these particular samples for which I have fluid
5 inclusion data. A view of the summary graph which was
6 produced by USGS researchers, their isotopic measurements, I
7 overlaid this with my data where I have coupled results,
8 stable isotope fluid inclusion data and the conclusion would
9 be that two phase fluid inclusions are present in calcite
10 with a range of isotopic properties including those which are
11 attributed to the youngest members of the paragenesis.

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

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

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

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

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

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

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

9 Thank you very much.

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

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

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

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

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

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

1 were formed.

2 Furthermore, less than 10 percent of the fractures
3 and cavities in the unsaturated zone welded tuffs contain
4 secondary minerals. This sort of distribution is consistent
5 with percolating water moving down from the surface on long
6 connected pathways and is not consistent with general overall
7 flooding of the unsaturated zone either totally or in part.
8 I mean, we find no places where even 100--small zones where
9 100 percent of the available cavities are mineralized.

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

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

1 that hasn't been said already.

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

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

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

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

17 As Jean pointed out, very, very consistent
18 temperatures when plotted as histograms from these types of
19 fluid inclusion assemblages. We measured 70 of the small
20 vapor/liquid ratio inclusions of an average temperature of
21 52.6 and a standard deviation of 1.8. This is really very
22 good fluid inclusion data.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

23 Thank you.

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

1 Thank you.

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

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

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

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

1 would agree on that.

2 Now, the interpretation, I'll just say a few things
3 about this. When we interpret data, what we tend to do as
4 geoscientists is base those interpretations on comparisons of
5 what we're familiar with with the dataset that we have. So,
6 in the case of Yucca Mountain, what we would like to do is
7 compare those data to what we see in other relatively young,
8 shallow silicic volcanic system that host hydrothermal
9 mineralization. And, if we look at hydrothermal systems, we
10 see a series of interconnected mineralized veins. We
11 typically see quartz or SiO_2 as the dominant mineralizing
12 phase and this is because the upwelling fluids are usually
13 warmer than the surrounding rock and quartz solubility is
14 dominately a function of temperature decrease and so we tend
15 to see a lot of quartz precipitated as the fluids move up
16 into the overlying cooler rocks.

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

1 reversals in hydrothermal systems in silicic volcanic rocks.

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

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

18 Now, although we don't see many of the features
19 that we commonly associate with hydrothermal waters in
20 silicic rocks, there's still some questions that need to be
21 answered because there are also many observations that aren't
22 100 percent consistent with just downwelling rainwater. And,
23 of course, several of these have been brought up here
24 already. The source of the salinity, perhaps not a major
25 question, but it's something that should be addressed. If

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

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

20 And, I'll stop there.

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

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

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

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

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

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

19 But, what do these results mean to the Yucca
20 Mountain Project? The entire study was started by a July 24,
21 1998, letter from Chairman Cohon to Lake Barrett. I assume
22 the letter and the press release are still available at the
23 NWTRB's website. Dan Bullen is nodding his head. So, for
24 people who haven't read it, they can go read it.

25 There were some conclusions, if you will. One

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

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

23 As Joe Whelan mentioned in the secondary minerals
24 for other reasons anyway, you know, in terms of understanding
25 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

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

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

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

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

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

6 And, what was the third question?

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

8 BODNAR: It's important to point out for the record that
9 the use of crystal morphology and mineral textures, it
10 represents a real scientific culture difference between the
11 former Soviet Union and eastern Europe and most western
12 countries. The use of crystal morphology has historically
13 been used to infer environmental formation in the former
14 Soviet Union and it was used in western countries through the
15 1800s and perhaps up to about the middle of the 20th Century.
16 It was then recognized that there were perhaps more
17 exceptions to the norm concerning the mineral textures that
18 using them as a diagnostic means of characterizing the
19 environmental formation could be very misleading and then
20 this has subsequently been confirmed by a lot of experimental
21 work. So, I'm not saying that it's wrong. I'm just saying
22 that there are differences of opinion concerning the use of
23 mineral textures. In western countries now, most scientists
24 will not use mineral textures to infer environmental
25 formation because of the many exceptions to the norm.

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

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

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

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

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

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

15 BODNAR: Correct. Correct.

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

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

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

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

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

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

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

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

13 CLINE: We think so.

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

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

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

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

13 WONG: Dr. Knopman?

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

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

19 CLINE: The calcite?

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

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

1 blue mineralization.

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

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

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

19 KNOPMAN: Excuse me for one second.

20 DUBLYANSKY: Uh-huh?

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

23 DUBLYANSKY: Okay.

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

1 hypothesis hold?

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

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

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

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

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

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

16 DUBLYANSKY: Lack of what?

17 KNOPMAN: The first--veins, veins.

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

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

9 KNOPMAN: Okay.

10 DUBLYANSKY: Sorry, I had a comment on the ages. In
11 terms of ages, if we accepted the ages, uranium-lead ages--
12 for instance, we have set the notion that we have hot fluids,
13 50 to 60 degrees Centigrade sitting in the rock over a period
14 of time which we have from ages, say, from 6 million years to
15 5 million years. So, we have to have water in the rock for 5
16 million years and we have to remove heat over this million
17 years. I think this is just thermodynamically impossible.
18 You just have to remove so much heat that you have to pump
19 much, much water there and this water has to be hot. The
20 heat force that will result from this, they just--well, just
21 unreasonable, I guess. Therefore, we have problem with
22 dating and I think now we know the reason why these ages can
23 be probably incorrect.

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

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

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

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

1 Chairman Cohon.

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

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

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

17 (No audible response.)

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

22 MAKHIJANI: (Inaudible) for one minute.

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

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

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

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

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

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

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

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

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

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

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

14 COHON: Thank you.

15 MARSHALL: Thank you.

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

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

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

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

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

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

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

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

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

10 Okay. Dr. Makhijani?

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

12 COHON: Yes, you do. In fact--

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

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

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

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

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

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

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

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

25 Thank you very much for your time.

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

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

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

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

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

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

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

1 COHON: Thank you. Dr. Dublyansky?

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

9 COHON: Okay. Well, here comes USGS.

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

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

1 conservatively warmer than 100 degrees Centigrade, probably
2 were.

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

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

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

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

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

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

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

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

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

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

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

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

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

1 feels like it's being steamrolled in this process.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

1 Our thanks to all. We are adjourned.
2 (Whereupon, the meeting was adjourned.)