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ANN RILEY & ASSOCIATES, LTD.
1612 K St. N.W., Suite 300
Washington, D.C. 20006
(202) 293-3950

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING ON APACHE LEAP TUFF

SITE FIELD HEATER EXPERIMENT

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U.S. Nuclear Regulatory Commission

11555 Rockville Pike

Conference Room 6B11

Rockville, Maryland

Thursday, June 20, 1991

The above-entitled meeting convened at 2:47 p.m., pursuant to notice, Tom Nicholson, Chairman, presiding.

PRESENT:

Mr. Nicholson
Mr. Rasmussen
Mr. Russell
Mr. Wescott
Mr. Neuman
Mr. Dodge
Mr. Green
Mr. Chowdhury
Mr. Ford
Mr. Silberberg
Mr. Nataraja
Mr. Wallace
Mr. Daemen
Ms. Lehman
Mr. Voss
Mr. Patrick
Mr. Cady
Mr. Philip
Mr. Tanious

P R O C E E D I N G S

[2:47 p.m.]

MR. NICHOLSON: Let's start with Todd Rasmussen.

MR. RASMUSSEN: This is just a list I went through and inventoried and tried to summarize what people had said.

One of the important comments was -- or a series of comments were related to our objectives and one of the critical ones by EPRI was that we should relate how this program interfaces with the vadose zone program, especially with regard to critical paths potential release.

I couldn't find any direct reference in the characterization plan which is what I had to deal with and most of the work regarding field tests were with regard to the engineered barrier system itself rather than critical path potential release. Maybe I'm just not aware of this component of the program. So I focused, instead, on those issues and the motivation for the DOE characterization program.

The initially proposed objectives are too broad was another statement. Generally one starts with a broad objective and then narrows it down to specific -- more specific subobjectives and particular a hypothesis. That's the vein I chose to take. Another comment was that the effort should focus on transport mechanisms alone and that hydrologic and pneumatic and thermal processes are irrelevant to transport mechanisms. And so -- but I think that -- I think in order to understand transport you have to look at the hydrologic, pneumatic and mechanical. They all have some bearing upon --

MR. SILBERBERG: We won't even ask -- we won't even ask about that comment. Okay.

VOICE: Would you also tell us who?

MR. RASMUSSEN: No. No.

MR. SILBERBERG: You will protect the innocent. I don't know, but you whisper in my ear later.

MR. RASMUSSEN: They said that we should not be concerned with computer models, that we should only emphasize the need to validate mechanisms. The objective should focus on whether all relevant processes have been incorporated into models in an appropriate manner. I would agree with that more.

An important subobjective should be the review and characterization of existing data. The data should be used to identify additional field research needs. So I think that's a legitimate comment, I think that should be an important and integral part of the experimental plan.

Subobjectives should be the integration of many technical disciplines into a single experimental undertaking. And I take that for granted that we really do have to work in an integrated fashion to look at some of these processes.

Another subobjective should include validation of thermal-mechanical modeling which is along the lines the previous statement. Now there were some just general statements not related to the objectives themselves.

It is advised that specific processes be identified and the computer models used to implement these models be determined. The calibration and validation needed by the models should then be specified.

I think that's a very good point and in part of the implementation part of this, one would want to be more specific in terms of what -- not just computer models but conceptual models, I guess I would say. Identify hypothesis prior to conducting the test, including the heat pipe signatures, the capillary flow and fractures and wetting diffusivity. Those are important processes or I guess hypotheses.

The resolution and accuracy of data needed for model validation needs to be determined. Criteria for determining acceptance will have to be identified along with parameters, sensitivities, and data and uncertainties.

At this point I'm not addressing model validation, I mean I don't really understand it myself to any degree. I think that what we can do is try and compare model results with field results to see if there is some reason for a discrepancy or if they do a good job of reproducing the actual field data. What we're looking for are processes that we had missed and unanticipated events and consequences that have been overlooked.

Can a thermal response both under baseline and test condition be used as an indicator fluid movement, by convection or conduction. If so, can other independent tests using tracers or water content variation be used to validate the thermal response. I think that's an excellent idea. I think that is the direction we're leaning at this point. The primary emphasis should be on the thermal transport. It is readily modeled and can be used as a very sensitive indicator of fluid flow processes.

It is important to incorporate field characterization data prior to determining the optimal heater experiments. So this is what I'm saying, somebody asks for it, we're going to put the heater as I have to say I don't know yet. We have to do some field characterization before we determine where and how we put in the heater. The spatial and temporal resolution of data needs to be resolved prior to conducting the characterization test. That's a very good point. And we incorporated that into one of phases.

The appropriateness and justification for the planned activities should be addressed. And I tried to do that based upon our laboratory -- previous laboratory experiments and the fact that we would like to scale up slightly certainly not to the repository level or the canister level or a field of canister level, but something larger than the current laboratory scale. The scale of the experiment may be to small for inferring the effects of repository scales. It's hard to answer that one. It is true that it may be difficult to extrapolate these effects to repository scale components, but for us to undertake technologically tests of repository skills would be difficult at this point.

Prior to conducting the field scale tests a large block experiment should be performed with better control on boundary condition and mass balances. And that was part of the motivation for going to this unfractured block experiment. I think that may be a good idea. The problem with the fracture flow experiment currently is that tremendous fluid may leak out through the fracture. Perhaps it would be better to look at a more confined environment. Homogeneous as well, where it's easier to control the boundary conditions and the balances.

Comments related to material properties are that it's important to measure the rock wetting and drying diffusivity both before and after the test and that's something we're currently doing in the laboratory for both the fracture and the rock matrix. And a similar comment by someone else that the wetting diffusivity of a fracture is an important characterization parameter. And part of what I skipped over today is our experiments related to that.

Interaction between temperature and rock deformation with and without fractures needs to be determined. Rockwater mechanical properties also need to be determined. I think that's something that's overlooked to a large degree in some of the thermal hydrologic experiments, is this interaction between the fracture geometry and the temperature regime and they are critically interrelated I think.

Significant characterization will be required. We acknowledge that. From our previous characterization at the covered bore hole we took a large number of core data as well as field measurements and characterized them for a whole suite of different parameters under a wide variety of water contents and matrix potentials. So I think we acknowledged that that's true.

Substantial geologic variability and hydraulic conductivity is observed in correlation with other properties is minimal. That's a very correct statement. So we have to measure the hydraulic conductivity. One would be in the laboratory using air and water as well as current field tests of using air as a surrogate has proven to be successful so far. An understanding of the residual stress patterns will be required. I guess that gets back to our emphasis on rock mechanical incorporating some of those effects, that's true.

And the process and importance of heat transport across fractures needs to be determined. I would acknowledge that to be true as well.

Boundary conditions. The boundary conditions for the experiment need to be firmly established. That's an issue. For underground work a bulkhead should be installed near the heater site to prevent ventilation. For near-surface work a cover should be placed over the site. But in response to that we had a comment of a different nature that said that rather than try to control boundary conditions it would be better to monitor them. An important model comparison may be obtained by examining a response to a transient boundary condition. I'm not sure what is the better philosophy, one would be try and isolate this block as much as possible from the environment. The other philosophy is go ahead and allow the boundary conditions to fluctuate, but monitor extremely well the response to it. And at this point I don't know how to resolve those two comments.

Due to differences in in-situ stress regimes it would be better to work underground as well as the following comments. Processes relevant at the repository may not exist in near-surface field location. Also, near-surface processes may overwhelm important repository depth processes. I guess in an ideal "ivory tower world" it would be nice to go down and instrument very carefully at tremendous cost and material complexity in an underground environment.

The question is, could a study performed at near the surface still have some utility in terms of developing an instrumentation techniques as well as being able to monitor a response to a thermal source. And I would argue that if there is going to be a response it may be amplified near the surface, that's true. And it may not be entirely analogous to what's going on at depth, but the process itself would certainly be able to be monitored.

I guess we just -- in this case we're limited to the real world environment where financially we're not capable of performing this kind of work at depth and the accessibility of the site may make it easier to gather more data that would allow us to make up for the fact that there will be a noisier environment near the surface.

MR. PATRICK: I don't think the point of this comment is just the noisiness, Todd. I don't know how many dollars will, you know, be dumped into this project per year over what number of years, but you're talking about a multi-million dollar project.

MR. SILBERBERG: It's not.

MR. PATRICK: If it runs for -- you're talking three years.

MR. SILBERBERG: It's not multi-million. It's definitely not multi-million. You might have that impression, but it could certainly get that but it --

MR. PATRICK: Well, I got that impression from taking --

MR. SILBERBERG: Previously yeah. This is known as doing it on the cheap, okay.

MR. PATRICK: But the -- I think the salient point here is that if there are -- for instance near-surface fractures whose characteristics are so different, for instance, because of calcite coatings and things of that nature that not even the same phenomena are occurring in the near-surface than would occur at depth in the "fresh rock". Then we could tilt at a windmill scientifically here and miss the phenomena where it shows a greater importance. So you do --

MR. SILBERBERG: That would be a concern.

MR. PATRICK: So you do something on the cheap, but you study the wrong problem.

MR. SILBERBERG: That would be a concern.

MR. PATRICK: Mathematicians can get away with that. They solve problems that exist, they don't --

MR. SILBERBERG: Yeah. Good. That would be my point, your point is that one would have to be sure that in doing it near the surface and in taking whatever deficiencies you're going to take that in fact gets some assurance that those deficiencies don't wipe you out so bad -- so totally that in fact it's useless.

MR. PATRICK: Todd's talked a lot about processes and I think as long as everyone's at peace that the processes are relevant.

MR. SILBERBERG: That's the issue.

MR. PATRICK: Subtlties and boundary conditions and fracture characteristics and something go away, but we have to have, I think, some confidences of the processes.

MR. NEUMAN: In fact, if I may --

MR. SILBERBERG: Yeah, go ahead, please.

MR. NEUMAN: We found, for example, through our experiments at Oracle in saturated granite which were very close to the surface that we were criticized for the same reason that they had a tremendous advantage in that certain experiments, certain observations could have been done relatively cheaply and rapidly. This is called in engineering, sometimes, scaled up in time experiments or accelerated experiments.

There are certain things which will take such a long time, certain processes, which will take so long to evolve under the ambient conditions of interest that you simply will not be able to observe them. If an experiment or at least not sufficiently, if an experiment is done near the surface where the fractures are larger, things happen faster, there is more air flow than there would be down there and it would appear as more water flow and so then perhaps there's an advantage, actually in running it near the surface.

MR. SILBERBERG: But in any case, advantage or disadvantage, what one would consciously want to do and I think I submit that's what you're going to do is to look at what you've got and say okay, these are the phenomenon I'm trying to deal with in some models. I mean, and again, if you -- you may not want to do too many. You may want to say well let's look at what tractable, go back and say, given the processes of what may happen because of the location, it may affect these processes.

Well, maybe the effect isn't so bad or it still puts you in the right domain or as Shlomo says maybe he gets some information that actually helps you. So it's understanding how to deal with the phenomenon that you're looking at. So, I think you have to make a conscious attempt to see how bad or good the situation is for whatever -- for the particular, for key for now. Now, maybe if you say, well, I'm looking at 15 phenomena and I got 10 out of 15 you know, then I think that's another judgment.

MR. RASMUSSEN: Another comment was air flow near the surface seems to be neglected.

We did neglect it in our report. We'd like to put it in. I mean, we have a thesis that has look at that at the site.

Air flow near the surface will be quite a bit greater than at depth, which may mean that it would be easier to detect thermally-induced flow, with speeding up the process, definitely, if you can have a greater magnitude signal.

And the question of the signal to noise, near the surface it may be much noisier, but if you could use that noise in terms of the signal, I mean, take advantage of some of those shallow, noisy boundary conditions as part of the signal, or the response of the system, it may make it easier to monitor.

Trying to monitor a very slow air convection at depth in a very right fracture system may be beyond our instrumentational capabilities. But, near the surface, where it is much greater, you may be able to monitor it, where you wouldn't have been able to in depth. I don't know. I mean, I'm just speculating.

In terms of the heater source itself, our previous experiment had just been a, let's say a naked heater, with no canister surrounding it. A comment was that it actually should be placed inside of a canister.

We looked at the Lawrence Livermore canister, and it seems fairly easy to produce their configuration. The question of what diameter, and what length, and what power, is another interesting question. Should it be backfilled, should it be open --

MR. NATARAJA: What's behind this comment? As long as the heat is generated, what difference does it make whether it is inside or not?

MR. RASMUSSEN: Whether it's conduction or radiant? I don't know.

MR. NATARAJA: How does it make a difference from the point of view of studying the rock?

MR. RASMUSSEN: If you wanted to reproduce the waste canister itself, I guess.

MR. NICHOLSON: One at a time.

MR. NATARAJA: Mine was just a side comment. I was trying to find out what was behind the comment.

MR. NICHOLSON: Oh, yes. I understand.

MR. NATARAJA: It doesn't look like they want to study the canisters, or if that's not the case, the comment doesn't seem to make much sense. Your purpose is not to study the canisters?

MR. SILBERBERG: It's operational.

MR. PATRICK: Somebody has done a detailed design, and they found they had trouble there.

MR. FORD: I think I heard Tom Buschek talk, and I thought it had something to do with convection.

MR. SILBERBERG: Oh. Internal?

MR. FORD: Yes.

MR. SILBERBERG: Secondary?

MR. FORD: Yes.

MR. SILBERBERG: Oh. Okay. Then if you didn't account for it, you could have a problem.

MR. RASMUSSEN: Another comment is that coupons, or just different types of metals made of various proposed canister materials should be placed near the heater source to examine corrosion processes. This is sort of a piggyback, a cheap and easy experiment, to use this as a platform for evaluating alternate canister materials.

Initially, the heat source will behave as a cylindrical source, and later as a spherical source.

I think this came from the Lawrence Livermore people who started modeling it as a cylindrical heat source, but after a very short time, it appeared as though you could reproduce it as just a point source. And it may be better to use a point source of heat as a canister, rather than a cylindrical source. In terms of modeling the system, it may be to make a very compact heat source, and numerically, it might be easier to evaluate it.

A vertical heater test is recommended. No justification there.

I think that most of the heater tests have been vertical, except for the G-tunnel and our previous Apache Leap experiment.

The advantages of vertical versus horizontal heating are, you induce completely different flow regimes. Perhaps a point source would avoid this problem entirely in that a point would be neither vertical nor horizontal, so it can get around both of those. The access hole, it's true, would still have a particular orientation.

The test should incrementally increase system complexity. I think this was directed towards the strength of the heater source itself, in that as one becomes hotter and hotter, the types of coupled phenomena in terms of two-phase flow become more complex, as well as the material properties may change significantly, at very high temperatures.

So the concept here would be, perform a low-energy heater experiment where materials may not behave too non-linearly, and then slowly bring up the heater strength to increase the complexity.

One of the heater tests should be located in unfractured rock; another test should intersect a single fracture, just to be able to incorporate the difference between those two. And the heater tests should begin with low temperatures, to minimize coupled effects, by a different source.

Another comment I should mention here, though, is that Tom Buschek has mentioned that low heater tests, low-temperature tests will take a very long time to establish any heat-pipe phenomena, if at all. And I think this could best be answered by some computer simulation modeling to see whether any phenomena are observable at all, at a sub-boiling experiment.

MR. DODGE: With respect to heat-pipe effects, the air flow in and out of your system, don't you think that could short-circuit any heat pipes? If the air flow takes the vapor out and brings it back in, you'll never get a heat pipe.

MR. RASMUSSEN: Yes.

MR. DODGE: So that would be one of the disadvantages of having a lot of air flow near the surface.

MR. RASMUSSEN: Through the fractures, I would agree. In the matrix from our simulations, we got up to 15, 16 bars pressure in the matrix.

MR. DODGE: It would have to be in the fracture, but that's where you would find the air, isn't it?

MR. RASMUSSEN: Air flow is in the fracture, right. Yes.I

MR. DODGE: In the boreholes that intersect the fractures.

MR. RASMUSSEN: That's right. So there may be quite a bit more vapor flow, that's true, through the fractures, in an open system like this. That's true.

MR. NATARAJA: Excuse me. In the last comment, wasn't your idea to study the coupled effects? Why would you try to minimize it?

MR. RASMUSSEN: It's not as complicated. It would not be as difficult to model and to reproduce the experimental effects.

MR. SILBERBERG: It's a reference. If you can't understand it without the coupled effects, forget it.

MR. NATARAJA: So, it's a basic thing --

MR. SILBERBERG: It's a basic, I think.

MR. NICHOLSON: There's quite a few comments that could be related to the last one about minimizing coupled effects.

One of the issues was, there's two sides. One is, you hit it as hard as you can with the greatest thermal pulse, and then you see effects, especially on rock mechanical.

If you keep it low, then you're going to see basically the thermal hydrologic, and you're not going to get the rock mechanical or geochemistry.

So the question is, how complex do you want to make it; how important are the heterogeneities.

So, you know, it's a philosophical question you have to answer when you design the experiment. Because if you ramp up and ramp down, it's quite different than if you hit it hard, you won't see, obviously, the initial conditions.

So the question is, do you want to run the experiment in a very simplistic fashion, in a very methodical fashion; or do you want to just go for the big effects?

MR. CHOWDHURY: I think this comment has been made to say, start with a simple process, study the individual effects, and then go for coupled effects.

MR. SILBERBERG: Then add things.

MR. CHOWDHURY: Yes. Add things, one by one, instead of going for coupled effects directly, to understand the phenomena more closely. I think that is the idea behind this comment.

MR. RASMUSSEN: Now, the response of the system to the heater source, I mean, we've specified the source now, presumably, and some of the responses here are, an equivalent porous media model is insufficient to model the thermal response. It would be better to use the discrete fracture network model.

MR. NEUMAN: These are new responses, now?

MR. RASMUSSEN: No. These are the --

MR. NEUMAN: Oh, these are still some of the comments.

MR. RASMUSSEN: Right.

MR. NEUMAN: Okay.

MR. RASMUSSEN: I'm not sure a discrete fracture network model would be appropriate. Only if we have just an individual fracture, I think in that sense. But that would not be a network model; it would just superimpose a discrete fracture on a permeable matrix. It would be almost a dual porosity, perhaps.

But I'm not quite sure we really need to use either an equivalent porous media or a discrete fracture to interpret the test.

During the cooling phrase, the primary rewetting will be due to vapor condensation, rather than liquid imbibition.

The concept here is that there was the one side with the circular condensate region around the center heater. How will that rewet the dry rock near the heater once the heat source has been removed? And the statement here was that instead of it being a liquid imbibition, that it will rewet in the vapor phase, rather than liquid phase.

This is speculation based upon computer modeling. It might be interesting to monitor this.

The tests should incorporate multiple ions with different charges in various cation exchange environments.

Looking at geochemical effects of chromatographic separation of ions. That could be done.

Some destructive sampling following the test could be performed to look at the distribution of these different ions, or tracers, whatever.

Place monitoring equipment perpendicular to the discrete fracture intersected by the heater.

The question that arose here is whether you would want to put equipment along the fracture, or perpendicular away from the fracture -- and I think this was made by the Lawrence-Berkeley group -- that really, what you're interested in is the interaction between the fracture and the matrix. And following a fracture along may be, one, disruptive to the flow regime; and two, not provide significant information.

I guess the next one corresponds to the previous one, in that tracers should be employed to determine fluid movement.. Rock should be sampled after the test to determine the final distribution and the disposition of the tracers, where the fractures are located, and the configuration of the flow field.

Emphasis should be placed on the need to evaluate new technology.

Again, this is a platform. If we were to put in this system, it would be nice if people had resources of their own. We would be quite amenable to them testing their equipment at our field site.

It may be better to put sensors in sealed and insulated boreholes, rather than in packed off intervals.

This needs some clarification, that we were thinking, now, rather than packed-off intervals, we'll use packers that are continuous the entire length of the borehole, and then we'll just place the sensors between the packer and the rock.

Determine what measurements can be used to monitor water and air mass balances during the test. And we're certainly working on that.

If all of the measured response is deferred, then follow up tests should be performed. I think the whole basis of this is to look for those, compare those two different responses.

Geographical aspects of a liquid vapor environment will complicate the near-field measurements. In light of this hydrologic effects in the far-field may be easier to measure. And I'm going to have to get back to the source on this because I didn't really understand it. Liquid vapor environment will complicate the near-field measurements. And then my final slide.

Electrical resistivity can be used to monitor water chemistry not just resistivity, I think, but just many tomographic -- inverse tomographic techniques. Mineral chemistry should be employed to examine rock changes before and after the tests. The mineralogic changes I presume. So I think that that is certainly possible.

Calcite precipitation due to heating of water and volatilization of dissolved CO_2 may occur. And that's an intriguing concept that a tremendous CO_2 may be generated in the environment and so that would change the geochemical regime as well.

Monitoring of air pressure near the heater should be included and that was proposed. Solute transport monitoring suffers from technological constraints. Research needs include the determination of appropriate tracers and monitoring techniques. I mean the whole question of geochemical or transport, some transport monitoring is critical I would imagine and but yet our capabilities for monitoring vadose chemistry are so limited at this point. But perhaps we can emphasize that in the experimental plan that additional research needs to be performed in this region.

Can neutron logging adequately monitor water contents. And actually it can in the matrix, but not necessarily in fractures. Displacement and strain monitoring should be included in the test program. That's a very good point. Build redundancy into the entire system, do not rely upon individual sensors or heater elements. Thermistors are more useful for measuring temperatures and thermocouples and finally, maintaining thermal contact between the thermistors and the rockfall is extremely critical. Existence of air gaps can substantially affect temperatures as well as induce the heat pipe effect.

MR. NICHOLSON: Okay, and thank you very much, Todd. I would like to now call on Charlie Voss and have him make his comments.

MR. VOSS: Before I get started, just for those of you who don't know why I'm here, I'm with Golder Associates, but I serve as the DOE civilian radioactive waste management member at INTRAVAL, and we have a -- we being the OCRWM program have a real interest in this experiment. Because we're not able to obtain permits to get on to the Yucca Mountain site even though this has nothing to do with Yucca Mountain, it is tuff. We can't really do any of the experiments of our own out there, so we're always interested in any other experiments of similar nature that we will eventually be doing out there to characterize this site.

We also have had, as has already been mentioned a lot of experience in performing heater experiments, both in G-tunnel and other locations and we would like to pass on any kind of lessons learned that we can to make this more successful and avoid a lot of the pitfalls that we certainly encountered.

As a side note, I guess, and Wes has already brought it up, but you know my experience has been the temperature field is really easy. Beyond that we've always been fraught with a lot of problems. Especially in the rock mechanics stuff, the displacements, the stresses. Just finding instruments that can survive the high temperature environments, it's major -- I don't think it's been resolved and I don't think much progress has been made. So just keep that in mind.

I guess our primary activity in the INTRAVAL is going to be on the G-Tunnel heater experiment. We're not going to be actively modeling the core experiment and probably the pneumatic testing that's also being done up there. A big part of our effort, as part of the heater experiment modeling will be to pull together this thermal mechanical hydrologic data base of information and data that we collected in G-Tunnel over the years and Alan Flint did a wet versus dry drilling experiment during the phase, that was the phase one test case in INTRAVAL that's being reported on right now and he's taking that data and trying to develop some new characteristic curves in that to use in some of the modeling that we're going to be doing.

And also we have the Livermore heater experiment that Todd talked about and although some of the data is going to be reported on and I think it's been finalized and it should be released soon if it hasn't already in a report. There are a lot of data that have not yet been reported on and so a lot of our effort this year, this fiscal year is going to be putting together that data into a report.

So, let's see, Tom Buschek is working on the Livermore data and as I mentioned, Alan Flint is working on some of the other G-Tunnel data.

In order to provide comments here today and just in general I asked Mark Cunnane who is at Golder Associates in my office to put together a fairly simplistic model of a heater experiment so we could get some ideas of the spatial scales that we were likely to observe, any kind of changes over different heat laws. And so I wanted to just show you some of the results of those analyses today and draw some conclusions and make a few recommendations.

We did four simulations. We assumed an infinite line source so we could use an axisymmetric model. We used the TOUGH Code and we did these runs on an RS-6000, for those of you who run this. And primarily what we are looking at is what -- how the bearing heat inputs would affect what we would observe. Some of these things that you saw earlier when Todd was going through some of the questions and also the other variable, I guess, was the absolute permeability of the top.

And we simulated 100 days of heating followed by an 80-day cool down period. We looked at two heat rates, one was 0.5 kilowatts per meter and the second one was one kilowatt per meter and then we looked at three different permeabilities, ten to the minus 15, 0.5 times ten to the minus 15 meters squared, and then went way up to ten to the minus 12.

Sorry about the quality of my viewgraphs. And we assumed initial conditions of a 67 percent saturation in 20 degrees C. So this is supposed to show you what the models -- what models we did. We did just one at this low heat rate and then the rest are all done at this higher heating rate.

MR. SILBERBERG: Charlie, could you please calibrate the -- help me, that heating, that linear heating law, how does that compare with what time in the cooling cycle of a spent fuel rock? Where is it? Is it the same?

MR. PATRICK: The low heating rate for a standard spent fuel assembly is two and a half years out of core for a single spent fuel assembly. Yeah, about 1.5 kilowatts for a three meter long light water reactor fuel assembly. And the other one, Mel, would be three kilowatts per meter. I don't know that may be one of the DOE several spent fuel assemblies disaggregated and stuffed back into a container. I don't know what the SCP would be.

I'm not sure about it and 10 years out of core it's about 550 watts per canister so at three kilowatts you're talking about stuffing the equivalent of five of six spent fuel assemblies into a single can and I think that's about the SCP design.

MR. VOSS: Those are ballpark.

MR. PATRICK: They're ballpark.

MR. VOSS: But that's not the reason that we chose these heat laws.

MR. SILBERBERG: Sure.

MR. PATRICK: The reason is their relevance to the Livermore experiment.

MR. SILBERBERG: Okay.

MR. PATRICK: But they probably played the same.

MR. SILBERBERG: Exactly. Thank you.

MR. VOSS: I'm going to show you the results at 87-days, so we're almost at the end of the heating cycle. This heat right here is the 0.5 kilowatts per meter model and -- let's see here -- what I wanted to show is under these conditions, oh, and this is also the ten to the minus 15 permeability meter squared permeability.

As we see a condensation zone very close to the heater and the -- almost all the heat transfer is done by conduction. The liquid saturation over time -- and these types of curves are interesting if you're thinking about where you want to locate instruments. The only place where -- well, I shouldn't say the only place, but you had to get pretty close to the heat source before you would see any kind of rewetting after cool down or during cool down.

MR. PATRICK: In the model, what was the conductivity and what were your -- how did you treat water thermally for this volatilization and recondensation? Did you treat that explicitly?

MR. VOSS: Yes.

MR. PATRICK: Or did you look strictly at the hydraulics?

MR. VOSS: Yeah, we did do phase transfers.

I should mention that we are putting together a report. It will be ready in another week or two and I can send anybody who is interested a copy. I've got a couple of draft copies with me, but there are some errors about who is funding this experiment.

[Laughter.]

MR. VOSS: Let's see, the next curve is the same conditions except that the higher heat rate, the one kilowatt per meter and so all it's done is shift everything over. Your wetting fronts moved over, further away, it's now about one and a half meters distance away. But still the heat transfers conduction dominated.

Now, when -- oh, and the other thing is you don't see the rewetting on those, you know, during this 80-day cool down period, you have to wait quite a bit longer. And the reason we were interested in these types of things is -- and again, our objectives are a little bit different. We're looking at this as far as an INTRAVAL experiment and INTRAVAL Phase II has a three year period. So obviously if you've got these time constraints about how long you actually have to do -- look at the data and interpret it, you have to get your data in a hurry.

Now, this is with a somewhat higher permeability and here we see this heat pipe or convection cell developing here and you know it's near isothermal conditions over about a meter length. So, what it's telling us is if this heat pipe is something that we're really interested in you want to be very careful about where you run the experiment because if the absolute permeabilities have that small change in -- the absolute permeabilities have this big of an effect you could easily miss phenomena that you wanted to observe.

And now, this is -- again, this is with the one kilowatt per meter heating rate. We do see rewetting with a higher permeability. So whereas in the previous one where we had lower permeability we didn't see rewetting during the first 180 days with this higher permeability we do. And finally, this is again one kilowatt per meter but it has a significantly higher permeability, this time ten to the minus 12. And you see the heat pipe extends all the way to the heater. It's convection dominated, the heat transferred there. And very little change in the moisture content as a function of the radial distance away and everything that happens, happens over all the changes that you can monitor happen over this very small region.

So based on that here are some of our --

MR. NEUMAN: Before you conclude, can I ask you a question?

MR. VOSS: Sure.

MR. NEUMAN: What was the largest change, predicted change in saturation that you have predicted say a distance, a meter away or a half a meter away, the largest?

MR. VOSS: I'll tell you what, while the next speaker --

MR. NEUMAN: You have it right there on your viewgraphs. If you go back to some of those --

MR. VOSS: Well, you're right. Okay. This

MR. RASMUSSEN: The large change what?

MR. NEUMAN: The largest change in saturation -- liquid saturation.

MR. VOSS: Obviously this one, not by very much. This is again, the high permeability.

MR. NEUMAN: About five percent.

MR. VOSS: Yeah.

MR. NEUMAN: And that happened within a few centimeters next to -- the X's.

MR. VOSS: Right.

MR. DODGE: I think it went all the way down to zero.

MR. VOSS: Well, yeah, it depends on the conditions. Now here's 70 percent over about --

MR. NEUMAN: And that is about a meter or two away.

MR. VOSS: About two meters. This one goes up to 90.

MR. DODGE: Then it goes down to zero doesn't it?

MR. VOSS: Yeah, he's saying the change.

MR. NEUMAN: Yeah, it goes down to zero from whatever it was.

MR. VOSS: Right. And it started out at 67 -- 65 something like that, 67.

MR. NEUMAN: Okay, thank you.

MR. RASMUSSEN: Wouldn't the X-R's come in G or --

MR. VOSS: I'm sorry? I didn't hear you.

MR. RASMUSSEN: Well, gas pressure is down at the bottom, isn't it? Or --

MR. VOSS: I'm sorry, Todd?

MR. RASMUSSEN: You have four topics on there, what are the four curves?

MR. VOSS: The top one is air, liquid, temperature and gas pressure.

MR. WESCOTT: Is that -- fraction or is that what it's supposed to be?

MR. DODGE: Is gas supposed to be as vapor, water vapor?

MR. VOSS: Yes.

MR. DODGE: A ratio of water vapors?

MR. VOSS: No.

MR. DODGE: Partial pressure of the water vapor, that's all it is.

MR. WESCOTT: Oh, okay.

MR. VOSS: I apologize, I just got back from vacation --

[Everyone speaking at once.]

MR. WESCOTT: I see, that's the area of gas, it was hidden there.

MR. VOSS: I'm sorry, I haven't been looking at this.

MR. DODGE: Okay. All right.

MR. VOSS: So, based on these very simple models and the relatively small range of conditions that we looked at it appears that the response is limited to approximately the first couple of meters under these conditions.

MR. NATARAJA: Which ones have a --

MR. VOSS: Well, all the ones I had, I'll add a saturation, but again we were talking about 100-day heating period. It's just that most of what happens occurs fairly rapidly in the first 50 days, after that the changes continue but the rate of change drops down substantially.

MR. NATARAJA: Is this a finite source or an infinite source?

MR. VOSS: This is an infinite line source.

MR. NATARAJA: An infinite line source so you're not talking about it vertically.

MR. VOSS: No, just radially. I'm sorry, all of this is just a radial.

The response rate is obviously very proportional to the heat rate and -- but one thing we did and I don't -- I didn't show you the results, I don't have them, but we also looked at like a two and a half kilowatts per meter heat rate and when we did that we ran into some problems and so one of our conclusions, I guess or very high heat rates may cause modeling difficulties.

You go -- what tuff does is it looks at the steam table and once you get above 374 degrees C, you start getting errors. So you become unstable and it just wouldn't run. Again, that's just a modeling limitation. But I don't know if --

MR. RUSSELL: Is that because the table is cut off there or because the numerics -- maybe?

MR. VOSS: I don't know. I don't know. We haven't investigated it enough to know the exact reason.

MR. WESCOTT: Yeah, kind of along that -- I don't know -- I guess you could have told from your table, applying air pressure; were you getting into an area where you had superheated steam there?

MR. VOSS: Yeah.

MR. WESCOTT: And I don't know, will TOUGH handle a super heated environment?

MR. VOSS: No, I think that's -- that's the problem. I don't think it was designed to.

I shouldn't talk for Karsten, but at least --

MR. WESCOTT: I suspect that that's true, yeah.

MR. NEUMAN: Just if I may come back for a second to your two meter range conclusion, I think is affected when in fact that this is a regular one and we're running it in the three-dimensional model, because in three dimensions the dissipation is going to occur in all directions and therefore actually you should expect less than two meter change. Under conductive conditions the rate and dissipation is 3-D in proportion to one over R whereas in 2-D it's proportional to log of four.

[Everyone speaking at once.]

MR. VOSS: Well, it would still affect it, but --

MR. NEUMAN: Oh, yeah. Instead of two meters maybe you would have to come down to one meter or half a meter or something like that. And the magnitude would be less.

MR. VOSS: And one of these really, this whole question of how far are you going to observe these changes becomes very important when you're going to law -- if your interested in the fracture effects, you would want to decide where to put the heater. And it really becomes critical depending on how much time you want to take to see your observation or observe any changes.

And as I mentioned before, the absolute permeability strongly influences the physics which occur. If you happen to be down in that ten to the minus 12 type you know, you're going to have this nice big convection and a lot of flux going on, but there is no way to really measure it. And the things that we can measure aren't going to change much.

Oh, I didn't finish that one because --

[Laughter.]

MR. VOSS: So, I guess that's about it.

MR. NEUMAN: That's because you went on vacation.

[Laughter.]

MR. NICHOLSON: Charlie, could you answer three questions for us?

MR. VOSS: Sure. I just want to make one other statement, I guess. We kicked this around in the group that I work in, in Golder quite a bit, and came up with the same recommendation, I guess, that Shlomo mentioned earlier and that's -- well, this is actually something that we were telling the people out at the Yucca Mountain project office that maybe they should really place an emphasis on monitoring temperatures now. I mean or when they finally get to the point where they're able to drill bore holes and things, because we think that's probably our best opportunity to really make observations about changes that are occurring, especially prior to the excavation of the exploratory shaft facility and that sort of thing. Because if they wait until they put that in to start monitoring, it's too late because you've already returned to the system, but I would also go along with this idea that you should certainly put a lot of sensors in to monitor temperature changes.

MR. NICHOLSON: The question I was going to ask you, as Mark has already done some of these very preliminary modeling, as Todd gets more information available, will you guys be able to follow it also in modeling, or is this just a one time modeling activity?

MR. VOSS: We've pretty well depleted our funding available for that for this year, although next year -- well, let's say after October 1, yes, sir.

MR. NICHOLSON: Okay. As Todd collects -- let's say he goes out and puts in some bore holes and starts doing the information collection will you be able to follow that and use your TOUGH Code and simulate ?

MR. VOSS: Right. I should mention now, all the data for the model, the input data is primarily based on those 105 core experiments that Todd reported earlier.

MR. NICHOLSON: Sure. The other thing is, for those of you who weren't at the meeting in Seattle, Tom Buscheck came and gave us a very good presentation for a good part of the afternoon. Will Tom be able to again follow up on this work and take the data sets that were developed at G-Tunnel and provide those to us through INTRAVAL? When well we get that information, do you think?

MR. VOSS: Tom and I and Alan are getting together in about two weeks to plan the rest of this year's and then next year's work. And I would really -- I don't want to talk for him right now.

MR. NICHOLSON: Sure.

MR. VOSS: I know that he is going to attempt to put together a report by the end of this fiscal year on that data. Now whether or not he's successful or not, I don't really know.

MR. NICHOLSON: The real question was, will Todd have access to the G-Tunnel data? You know one of the comments we received earlier and one that was explored later during the discussion period of that available in reports from previous experiments and do you think DOE can make some commitment with regard to us, meaning the University of Arizona and INTRAVAL people getting that data set so we can use that to test models and conceptual models?

MR. VOSS: Although I am representing DOE today I certainly can't speak for them, but it's certainly my intent and I think the other people that are participating in and INTRAVAL's intent that we would certainly share this data as soon as we pull it together.

MR. NICHOLSON: Okay.

MR. VOSS: Again, there is a tremendous amount of data that has been collected and there's always a hesitancy to release things too early and then have to come back and say well, I'm sorry but you know I didn't look at this piece of information, it just looks a bit sloppy. So they want to be through.

MR. NICHOLSON: Okay. Are there any other questions of Charlie.

MR. DODGE: Yeah, are we supposed to infer from this -- taking a high-level look at this that you're saying that there is no need for a big scale experiment. That all you need is like a meter or two experiment?

MR. VOSS: Again, for the purposes of INTRAVAL, that's probably would be accurate.

MR. DODGE: Well, if I believed your number there I wouldn't need to be monitoring something five meters away from the heater.

MR. VOSS: Not unless you had a lot of money to spare.

MR. NEUMAN: The one difference, of course, is that you assumed a homogenous regime?

MR. VOSS: That's right, with fracture -- now, that was somebody interesting too. We really wanted to do a -- we planned to do a model with a fracture in it, but you know, TOUGH does not handle anisotropic hydrologic relationships for fractures. In other words, all your properties are assumed to be the same whether your talking about down the length of a fracture. You can put a fracture in there, but --

MR. NEUMAN: You could build in a high permeability porous zone?

MR. VOSS: Right. But you wouldn't see trickling, for example, down the fracture.

MR. NEUMAN: You would see something similar --

MR. VOSS: Yeah, but see you have to get it near matrix properties across the fracture because you have all these contact zones.

MR. NEUMAN: You see the fracture also has a lot of contact.

MR. VOSS: That's right.

MR. NEUMAN: So I view a fracture, a natural fracture, unless it's really open as a porous medium as long as they're porous.

MR. VOSS: Well, I have a little bit different concept and I think of -- you know, I would hate to throw out channeling, but I do, based on my --

MR. NEUMAN: You can never use that in a porous medium, it depends on how you distribute its properties.

MR. VOSS: Okay. Well, in that case then we agree, but anyway, TOUGH, the way it's set up right now can adequately handle these anisotropic properties.

MR. WESCOTT: Fractures, I'll agree, influence the flow of water, as such, but I don't believe will change your inclusions on where the temperature takes place --

MR. NEUMAN: Unless you create conductive conditions.

MR. WESCOTT: Unless you have a heat pipe going down to take the heat down there somehow.

MR. NICHOLSON: Okay. I thank you very much, Charlie. Jaak, would you like to give us some comments on your feelings on the experiment? We'll put it that way.

MR. DAEMEN: So what I'm going to talk about is almost only the mechanical end. What I saw here was an opportunity to expand the relatively modest cost let's say, typical in a research budget, as the universities get it, to do some additional work that could greatly increase I think the potential benefits of the program. And I'm going to look at a couple of different phases in the proposed heaten test from design of the test to initial site characterization to conducting the test finally interpretation of the result. Most of my comments were based on the reading of Todd's draft of January and some of them I see have already been superceded by comments that he made today.

One of the intriguing possibilities I see at the site from a mechanical order, total mechanical characterization and multi evaluation is that because you are right on the surface, you have no confinement. So that's a highly unusual situation compared to most other tests in the waste program, Stripa, Finsjon, Climax, all those tests were done deep in the ground with a confinement. It seems to me that it's a very intriguing question. You have very simple boundary conditions because the surfaces feel stress and it is a highly unusual condition. So you could consider it as an extremely unusual condition.

You can argue the opposite, it's not

representative, but I think from a point of view where you're evaluating the validity of a model, that does not necessarily mater too much. I can see some very simple modeling and obviously the kind of steam hydrological modeling that Charlie was describing.

I don't know, did you do any mechanical, or -- it would be very easy to do something similar mechanical where you have the axisymmetric, it could be done simple continuum analysis. My question of course is the main reason for suggesting this is to try to determine whether you're going to be able to measure the deformations or not. That is a real concern in my mind, a very quick, back of the envelope calculation suggests that it's marginal. It probably can be done, but I don't know how much displacement actually will be and that's a problem going back again to stripa and climax and, oh, WIPP even worse. But I did this placement has traditionally been very difficult, so I think maybe it could be done at relatively low cost.

My basic suggestion would be an axisymmetric continuum model and a two dimensional continuum model and yet here will be two dimensional continuum model would be to look, for example, at string regions and string field surrounded and see whether there are ten size string regions which presumable would suggest opening up of fractures.

You could, of course, go from the continuum model to a slightly more complicated -- still, I would suggest initially 2-D discontinuum model and look whether any of these locks may be lifting up or whether you make a separation along the -- in the 10 size drain fields, whether there really is a need for a 3-D analysis or not, it would be very nice to do a 3-D analysis whether or not SEA would be able to pay for it is another question, I guess.

MR. NICHOLSON: One quick question, Jaak. Because of the limitations, let's say, on doing experiments you need obviously to collect lots of background information before you do the experiment. You go on with a preconceived idea of your analysis for the rock mechanical response. Would you think in your best knowledge that you should plan for a 3-D analysis of the perturbation due to the heat source and put in your monitoring program in that fashion or plain on very simplistic modeling and so therefore you should be able to use a minimal amount of monitoring points to see what effect the heat source would have?

MR. DAEMEN: At this point I, for test design purposes would only look at axisymmetric and 2-D.

MR. NICHOLSON: Okay.

MR. DAEMEN: And from that evaluate first whether you can measure anything or not and all the other way around. How much -- where do you put the heater and how -- but I presume all that will be determined hydrologically. I am only looking at what are you guys going to do and then can read right on the back of that and see what we can measure and get out of it.

So, at this point 3-D I think would be overkill.

MR. NICHOLSON: Okay.

MR. DAEMEN: So the question is, or at least my purposes here would be to see whether the displacements can be monitored and where I would like to monitor them to get presumably discriminatory results.

I would like to see, from a mechanical point of view, some pre-test monitoring baseline characterization as much as possible right now and if you go possible to install a number of strain gauges whitmore gauges or things like that across a number of different cracks, ideally of course look at them in the summer and in the winter you have variation, I guess 30 degrees C. You show 25 degrees C variation, that seemed kind of low to me for that side, but anyway a 30 degrees C and with a fracture spacing of about three four meters, my perception is that you might, and I say might be able to measure the contraction and/or expansion of the block or the fracture something like that. Again, for the low -- very low cost budget it would be nice to do it on the surface, obviously people do it at greater depth or closer to the potential heater location would be nice, but it would be quite a bit more expensive.

MR. NICHOLSON: Jaak, quick question. How do you determine if you're going to do this baseline characterization -- how do you determine which fractures you would want to measure?

MR. DAEMEN: Presumably right now, purely by visual inspections. Do we have any indication that any cracks are opening up or might have opened up in the past or are more readily accessible or something like that. It's a tough decision. Can they or can you judge based on air out flow or on water in flow where something is deeply connected or not. It's not an easy decision. Are you willing, you know, I don't know, let's say for $500 a shot to put in 10 or 20 of them. I don't know. It's somewhat of a judgment call. But I think we still could do an awful lot of that for a few thousand dollars.

Now here, of course, you would be more expensive, but it still could be done.

MR. PATRICK: Part of the analysis that never got done on climax and I don't believe ever got done on the stripa was --

MR. DAEMEN: On stripa not on climax, at least in the initial Berkeley test nothing got analyzed.

MR. PATRICK: There were some -- everyone worried about the fractures to begin with, but then as we began to look further at it, the fracture is such a small part of the whole test area that the elastic compression of tens of meters of matrix rock is swamped anything that happened across, we thought, although the analysis never got done; swamped what may have happened across a couple of ten micro meter --

MR. DAEMEN: That's my understanding --

MR. PATRICK: -- wide fractures.

MR. DAEMEN: That's my understanding from Stripa also and I agree with that yes, but here the question is you know if the rock expansion is absorbed in the fracture that means that the fracture aperture is changing and I do not know enough about these joints and not enough about this kind of flow patterns, but my hopeful thinking here is that the hydrologist might be interested from the point of view of determining how much the aperture is changing from evaluating rock mechanics model, it might be -- I think it would be interesting to find out. I recognize, I am well aware that it has been done at Stripa and at Climax. At Stripa I know for a fact that it was never analyzed, at least, none of the initial data, the initial Berkeley program and at Climax I was told fairly recently that it never --

MR. PATRICK: I know we never -- unless somebody did something in the last year or so.

MR. DAEMEN: I don't believe so. But I agree that from an overall deformation point of view it may all be taken up by the rock, but if the rock expansion is taken up in the joints presumably that must lead -- well, I did a very crude calculation. In taking the blocks the flux spacing of four meters 30 degree centigrade change for the initial monitoring and things like that, and I don't remember what the results were --

[Laughter.]

MR. DAEMEN: I thought that it may be --

MR. PATRICK: Micrometer.

MR. DAEMEN: -- of the order of -- well no, of the order of multiple tenths of a millimeter.

MR. PATRICK: Tenths of a millimeter?

MR. DAEMEN: Yeah. Now whether that is significant in these fractures, if you measure it on the surface almost certainly not. At 20 meters depth it may well be and it depends which fracture you're going to monitor in those welded types.

So during the meeting phase whatever else you would want to do, I again think that at relatively modest costs you could monitor the displacements of that now with the high quality -- again, that would need to be checked, somebody -- but my suspicion is that with a good modern surveying type instrument you could probably pick up a lot of deformations. I would obviously again like to do that over a one summer/winter year cycle before you get going. But then during the test I think that's a very low cost item. And it may be worth while from a thermal mechanical point of view to see how that mountain deforms.

The same with fractures, you know, are you willing or are some of you willing to put on a bunch of gauges to measure where the deformations are taking place across the fractures and it's quite a problematic question here because of being so right at the surface. You know are these fractures representative. And again it would be preferable probably to do it underground, but then you are paying much more for it.

MR. NATARAJA: Jaak, are you exaggerating the surface --

MR. DAEMEN: Yes.

MR. NATARAJA: -- or is that the way it is?

MR. DAEMEN: It's exaggerated quite a bit, you know, but I wanted to illustrate the idea that you have no confinement or very little confinement, anyway. You have a free surface. Well, it's not that flat. It's noticeably curved. Quite noticeably curved. It's not flat by any means. It's not as if you would have something that is flat enough that your horizontal stresses could build up significantly. I find it very --

MR. NATARAJA: Do you have a picture?

MR. RASMUSSEN: I have a topographic.

MR. NATARAJA: That does not answer my question.

MR. DAEMEN: So I think it's very fair to argue that depending on how deep they go, but they will be stress free. So that's my -- that is my basic -- I think that's one interesting part. It's not representative of deep underground facility, but it's the other extreme of a very simple --

MR. VOSS: But that could still have a positive impact on -- well, just on examining the processes.

MR. DAEMEN: On validating the --

MR. VOSS: Because under higher stresses, for example, the fractures more than likely would be much more closed.

MR. DAEMEN: Correct.

MR. VOSS: Their influence on the --

[Everyone speaking at once.]

MR. WALLACE: Jaak, what would you be validating?

MR. DAEMEN: I have not validated anything at this stage.

MR. NEUMAN: I just heard validating the model? I understand that you could answer the following question with such measurements. You could ask yourself the question, given some information about stress distribution in this mode before the heater experiments started, what changes would I be measuring after the heater was activated if you actually had strain gauges over specific fractures you could perhaps say something about aperture variations in those fractures?

I can understand that, and that from a mechanical standpoint I can understand that that would be interesting.

I have difficulty seeing the next step and that is the connection to hydrology. Where would you and how would you link this to hydrology if the question that you have in mind, tell me if I read you correctly, is that you would expect this to change to permeability field because the fractures may be opening and closing and you would want to read this in the hydrology data somehow, than I would submit that they will probably not see it? That virtually any hydrologic monitoring system that I can think of at the present time, except on a laboratory scale, yes I think there you would see it. On this scale, I don't see it. If anybody else in here dares about how you could go about measuring hydrologic response to the kind of changes in stress and strain that you're talking about then this is the time to talk about it.

MR. DAEMEN: Well, I should -- you know I do not know really what kind of stress changes I considered in all probability negligible. The strain and displacement changes I'm putting a question mark up. I think they may be mechanically measurable, even there I'm -- you know, that's not a very strong statement obviously.

What I am suggesting is that at least the people who run the experiments should be aware that in my perception you have a fairly high probability of having significant zones around the heater of significant -- of potentially significant strains. Those strains are most likely to concentrate in joints and to open up joints. I do not know. Honestly I have no feeling whether the change in aperture is significant or not. But I think it would be worthwhile. You see the first types of analysis can be done by a graduate student in a week. I mean it's a relatively trivial thing to do.

MR. NEUMAN: Well, the reason that I'm suggesting that hydrology will not cede is not because the effect will not occur, actually I'm sure the effect will occur. But the effect will occur in a very interminant way because the graduate student's work would entail an idealized fracture in an idealized rock mass. And indeed if you have a single idealized fracture think of the world as being two dimensional and you have this single fracture here, okay, you've got fracture closes at one point, it's going to affect everything because it's a two dimensional world. In a three-dimensional world the fracture closes here. It may not close here, it may not close here. And this is what we're seeing in hydrology over and over again; two-dimensional, certainly one-dimensional models are a gross over simplification of nature.

So, given my understanding of stress rate, I am predicting that hydrology, except on a laboratory scale, if a single fracture where you really have a very good control over exactly what is happening, even there I have some doubts about it, but otherwise in the field I don't think that you'll be able to read anything into the hydrology bit. That's my prediction.

Can somebody come up with a model that would prove me wrong because if so, then it's worthwhile coupling the two. Otherwise you have the mechanical measurements standing on their own and they may be absolutely valuable and I wouldn't argue with that, but I don't see their connection to the hydrology.

MR. NATARAJA: That is a useful finding, isn't it?

MR. NEUMAN: No, because I can predict it right now. I know it's going to happen.

MR. NATARAJA: You can pick up the -- no, that's a fact, I've made some measurements.

[Everyone speaking at once.]

MR. NEUMAN: I would say that to run an experiment of this magnitude in order to verify something which is quite obvious to most hydrologist, I believe, is probably not worth it. If you had a hypothesis, a reasonable hypothesis which says, yes, there would be coupling, you could test it this way. But I'm suggesting that hydrology will not be able to see it, that's what I'm saying. It's not that they do not occur, but it will occur on a scale in such a way that the hydrology -- you have the measurements that we are currently able to do. I'm not going to be able to see it. I've thrown this at the table, you know, it's open to discussion. I may be absolutely wrong, but that's my feeling.

MR. NICHOLSON: I would think that if you got into the transport question; Todd raised a question earlier the people at EPRI were very worried that -- it's funny, a lot of people talk about this being some sort of analog to repository and they embraced the research objective of simply looking at what happens when you put a thermal source in a you know heterogeneous geologic framework with the understanding that this has to have some bearing on performance assessment vis-a-vis transport. If you get into solute and vapor movement of a tracer, then I think somebody might be able to address your question. I agree from a hydrologic standpoint it would be very difficult.

MR. NEUMAN: The more things happen, the more ambiguous are the interpretations and one of the best examples are the INTRAVAL projects. Every single one of

INTRAVAL Phase I projects, one of the best projects in INTRAVAL, in my view, is the Finsjon site, character is one of the best characterized fractured rock sites I have ever seen. And I've got to talk about some of the data from there tomorrow.

Two types of tracer tests conducted between the same set of wells, quite a bit of information about hydraulic conductivity, fractures, geophysics, geology, and so on and look at the ambiguity in the interpretation. You have 11 project teams, or maybe I'm exaggerating, but it was a good number of project teams, each one being able to fit the data to one extent of another. It was the following models a random fracture network model, porous medium models with one fracture, a wealth of different concepts, channels, three different channels; magically two of those passed exactly through two of the monitoring wells out of four. Out of three monitoring wells, out of a total of four. Every single one of these models is able to reproduce the data, they all come up with parameters, *** numbers, porosities, dispersivities, and so on and I claim they mean zero.

Actually what I claimed of these that INTRAVAL I has shown with respect to Finnjan is is precisely that this wealth of data is insufficient to validate any of these models. The only thing that we have learned is that the system is complex, that a simple porous medium model constant properties and so on doesn't work. That you have to go around it, you have to build some type of heterogeneity into it, but how you build this heterogeneity has not come out of INTRAVAL I and will not come out of INTRAVAL II in my view.

And I what I would like to be able to do to avoid --

[Everyone speaking at once again.]

MR. PATRICK: I would have to, for what it's worth, have to side with what Shlomo was saying. I think the thing, one of the things that you mentioned, one of my pet peeves earlier, but the other thing I think we fail to do is put enough effort into design calculations and we end up having to have too broad of objectives, having -- biting off too much, and not one, as you pointed out Jaak, not one of the rock mechanical tests that have been run yet has been calculable even as a retrospective.

MR. DAEMEN: Beyond that, I would put it --

MR. PATRICK: Now we're talking about --

MR. DAEMEN: Nobody has been willing to pay to do the analysis.

MR. PATRICK: No. Lots of money has been spent. I would argue the opposite that the resources became so diffuse because of so many competing goals. Now, I see here a project that probably originally started out to be a hydrology project, given that a bunch of hydrologists dreamed the thing up and I mean I've been among the people who said, hey, if we're really going to put this thing into the field, let's look at putting other piggyback experiments on it, draining all of the information we possibly can for the money expended, but if there are things like this where we can do calculations that we have some reasonable expectation will be accurate, you know, maybe it's Sot and Simon need to go back and do some UDEC or 3DEC calculations coupled with the hydraulics and say hey, are these measurable. And I think that's really the question.

He's hypothesizing they are not measurable. We have some tools that are pretty good that would tell us whether these phenomenon are measurable. And if they're not, maybe our goal ought to be to say okay, because they are relatively unimportant we will design a test which is either insensitive to those things and/or develop some boundary conditions which do not allow those things to vary so that we can home in on the two or three or four parameters that are most important to us at this stage. And then if Phase X says well, now we understand all of that, let's come in and see if we can also throw in a third coupling factor, namely the mechanical side and see if our original hypothesis with our simple fracture thermal mechanical models were correct. And if we find out they're not, then perhaps we pick up the next step.

But you know we're -- I think we may be getting caught up with getting too many objectives here.

MR. NATARAJA: Have you been able to measure the impacts of -- thermal impacts on the hydrology?

MR. NEUMAN: Let's go back to Stripa. We know that the Stripa experiment has never been fully analyzed as Jaak has pointed out.

MR. DAEMEN: If you talk to Neville Cook about that and I talked recently to that at Climax and those people will not agree that they have ever had even remotely to support -- to analyze the data.

MR. NEUMAN: That's the point. They're supposed to analyze the data, but do you mean financial support?

MR. DAEMEN: Yes.

MR. NEUMAN: I mean technical data. I mean -- I don't think -- now I haven't really looked at it in detail, because I was never too interested in this, but from my -- I did read that heater experiment report quite a few years ago and if I remember correctly, they did not have enough hydraulic data, permeabilities --

MR. DAEMEN: I do not know about hydrology.

MR. NEUMAN: -- no, but that's my point. I don't think that they had enough permeability data, and head data and spaced on to relate any changes in thermal stress to hydrology. And that's why from a hydrologic standpoint I don't think that there was anything to analyze in that test.

MR. DAEMEN: I think it's quite different from a rock mechanics point of view.

MR. NEUMAN: Yes, absolutely.

MR. DAEMEN: The temperature distributions were very easy and that's why they were done. Because at close form solution gave good answers on the temperature. The number was that that could never be connected to the strains and the displacement. There was plenty of strain and displacement monitoring to try to analyze why they predicted this displacement, it depends on how you look at it. In those days people said were significantly different from what had been calculated with simple models and I think this is the dilemma where I come with validation and where I would like to avoid the term because I'm not convinced that being off by a few millimeters has any impact on waste isolation. That's a different question.

[Laughter.]

MR. DAEMEN: But that's the way Climax was written up, isn't it?

MR. PATRICK: Not by project people, but by others, we lost our data yeah.

MR. DAEMEN: Well, all I can say is that I think there was enough data there to evaluate all kinds of discrete, discontent or whatever rock mechanics models at least to check how often do we have an opportunity like Stripa where they measured during a heater test for two years in extreme detail all the displacements and then a simple analytical model did not fit and that was the end of the project quite literally. And my understanding was that that was true at Climax there were plenty of people ready to do all kinds of model evaluations and they were never allowed to.

MR. PATRICK: Yeah, there were a little over 15 million data points that were collected on Climax and I don't know what Stripa was.

MR. DAEMEN: Yeah, the other comment I wanted to make here which I kind of forgot to make, was that obviously from a mechanical point of view I am correct on certain -- when I hear that you are going to generate 15 bars there, because I do not know where that top is going to end up if you try to do that, but --

[Laughter.]

MR. DAEMEN: I'll be out -- so I think displacement monitoring would be quite valuable. One question I had in terms of this being of no use to hydrology, let's suppose that we now know that in Stripa, for example, the flow was in a very small number of extremely --

MR. NEUMAN: I don't think it's not useful. What I'm saying is I don't think that you can measure unambiguously any hydrologic response to that. That's what I'm saying. I'm not saying it's not going to affect the hydrology. It's going to affect the hydrology.

MR. DAEMEN: You know, I suspect, I don't know how justified this is, but based on observations, admittedly this is involved, but for example in the old highway tunnel, that probably flow occurred in a very small number of these fractures. You know, so now I do not know how to identify and advance which ones, but if we could -- if we could monitor the displacements and I don't know, and if the displacements are significant enough to affect the aperture of the fracture, is that something that you can use -- from my point of view, just from a rock mechanic's point of view I think I can justify doing the experiment.

MR. NEUMAN: The only way that you will be able to isolate the effect of a single fracture in the field is if that fracture truly dominates flow in that area. If that truly dominates flow in that area, it is a large fracture. If it is a large fracture, you would need a heck of a lot of displacement to truly affect the permeability -- its ability to still continue acting as the main --

MR. DAEMEN: What do you mean by large fracture?

MR. NEUMAN: Well, on the order of more than 100 micro meters many many --

MR. DAEMEN: There are many of those. I've seen one of them that you are going -- okay. Obviously, then in terms of data analysis and interpretation I would like to see if possible to do some deformation calculations. I -- again, this was kind of in response to Todd's original draft. I think calibrations are very worthwhile and I wish they could do them for Stripa and for any other large scale field test. I think playing with the results can give a lot of insight even though it is backfitting. And I would like to avoid to work -- validate for the time being because predicting has never worked very well in any of these rock mechanics programs for the time being.

Obviously you would need some support to get going. You would have to know what the rock properties are and thermal mechanical properties and all that, but from my point of view, from the rock mechanic's point of view I can see some very interesting games we could play in contesting.

MR. NICHOLSON: Does anyone have any question for Jaak?

MR. VOSS: Could I make a comment?

MR. NICHOLSON: Sure.

MR. VOSS: It just so happens, and this isn't a plug, but when I was at PNL a couple of years ago we had funding to do some discrete rock modeling and we used UDEC and to model the region around an inplacement hole, so we modeled the thermal mechanical response that we thought would occur and we came up with a bunch of conceptual models for this structure. And you know, we did some where we had a lot of fractures to simulate an excavation blast effect type zone, all sorts of things like that. And we had laboratory data, we had mechanical hydrologic data for tuff taken from Apache Leap, that I went out there with my little core drill took out which had fractures which we stuck in the triaxial cell and we monitored changes in aperture under saturated conditions and then the changes in the hydrawlic conductivity of those fractures. And we didn't -- as part of this analysis we didn't really look at -- we didn't you know, put some sort of flow field loss or anything else, we just looked at displacements which is getting at what you're talking about. We compared these results and again these have the fracture characteristics data as far as the stiffness and that sort of thing goes from our laboratory testing. We compared that data against a continuum model using the ANSI Code and we really couldn't see the effects of the fractures except in a few cases under really severe geometries, of you know, the way the fractures intersect it.

Now, for a small diameter heater experiment which is I think what we're talking about, I think those conditions are fairly unlikely to occur. So I guess, you know, just listening to the debate that went on, I would have to throw my hat on the side where I don't think you're going to see much effect.

MR. NICHOLSON: Todd.

MR. RASMUSSEN: My only comment would be that because of the cubic law, the permeability that comes from the aperture tube, if you have a -- even a minor 10 percent change in the aperture you're going to have a 30 percent change in the permeability. If there was a -- if you're doing any kind of air permeability test, and you see a decrease in the air permeability that could be attributed to one fracture closure or two -- perhaps an increased saturation of the fracture. So to resolve which of those -- if the fracture aperture is actually increasing by monitoring at the same time the air permeability is decreasing, I would argue that it's probably additional information that you could use to look at --

MR. VOSS: You have to keep in mind that kinematics that are involved in there. I mean when we started looking at what was happening in these UDEC models where these blocks are free to rotate and slide and everything, I mean you get some regions that are open a little bit, but down over here and see closed.

MR. NEUMAN: This is not an ideal fracture. All of these are not ideal. In an ideal fracture, you're absolutely right. Cubic law holds when you increase the stress, you reduce the aperture by -- and the effect on the flow, not on the permeability of the aperture is cubed. The effect of the flow -- on the velocity is square.

Okay, but the real world looks more like this, you have these openings and closings, opening and closing and therefore the same stress would create much less of an effect because it is going to be distributed also through these points of contact. And if it is true, which I think is true and we have quite a bit of evidence from John Gale's work on Stripa cores and the work at Berkeley and by others, that in fact apertures of fractures tend to form large and small channels, then these larger channels are going to have very little affecte because of that. Because they have these walls which are going to resist movement.

And so you are really talking about, an effect which on the scale of a laboratory core, yes, I think there you would be able to see some of this perhaps, but on the field scale, unless you measure there's a tremendous amount of detail that happens right next to your heater. I frankly don't think it will be able to see this.

MR. DAEMEN: The other thing that we saw on those experiments which is kind of interesting, we had a nice planar fracture running down the axis of the core so we could see some effects. But when we sheared it at all, if we did it under stresses it just completely plugged the thing up. I mean, you know, everybody says well you're going to get dilation, you're going to see all of these big water magnitude increases. Well, if we took the fracture apart and offset it and put it back together and stuck it in there, sure enough, we saw huge orders of magnitude increase in its ability to transmit flow, but if we did it under any kind of law the gouge material or whatever plugged up all those nice little apertures or whatever.

MR. CADY: Did that --

MR. DAEMEN: Yeah, but it was probably only a meter -- the core samples were taken about a meter below the surface and I don't think -- it was probably partially welded.

MR. CADY: Is that because the samples the center have here --

[Everyone speaking at once.]

THE REPORTER: One at a time, I can't get that.

MR. DAEMEN: You have collected some samples from the joint -- so far we made only three tests, we were doing much more than that, but our results show that there is significant deformation --

MR. VOSS: But are you at the same time looking at the hydrologic --

MR. DAEMEN: No. No hydrologic, it's only mechanical.

MR. NEUMAN: We know that shear has a tremendous effect on mechanical and hydraulic properties from some experiments done in Norway, for example, by Makurat on natural fractures in some metamorphic rockes. Just like you did at Stripa. He showed -- there's a paper in the 1985 Tucson proceedings and I'm sure there must be much additional since then. He showed tiny shear displacements in a fracture under normal stress of a given magnitude had a tremendous effect on the permeability and when he plotted permeability as a function of the nominal aperture, something that you would measure externally with a strain gauge if you calculate it based on cubic law, you have a very nice curve. If you look at the actual measurements of flow so the single fracture in a single core as a function of --

MR. VOSS: Lateral displacement.

MR. DAEMEN: -- well, yes. What you saw is a tremendous chaotic hysteretic phenomenon, absolutely chaotic. Showing the tiny tiny displacement completely -- essentially showing the cubic law doesn't hold at all under those circumstances because there are so many contacts between the asperities on the two sides that this notion of flow between parallel plates is just totally inapplicable.

You have to open the fracture quite a bit so that its average opening is large compared to the average asperity amplitude in order for that to start holding and that has been shown Gale and by others.

So that's why I was saying, if you have a fracture which is relatively large, yes, then this cubic law effect will show itself, but the fracture will be large. And so you will not really see its effect so much.

MR. NICHOLSON: Jaak, you were going to make a point?

MR. DAEMEN: Well, not really. I'm still somewhat confused with your -- you know, the same people from Stripa, particularly that a whole set of papers that Cook and his students -- depending on what you call large, but if you have contacts and they show that the combined contraction of the contacts and the deformation of the gaps has quite a significant -- when the significant effect of the shear -- the very small shear displacements, you know, there are some additional papers by Barton and all that, is what I would suspect, I am not 100 percent sure, but looking at the Apache Leap samples, they are very rough samples and they are very hard samples. I would suspect that a very small shear displacement is going to give orders of magnitude increase --

MR. NEUMAN: That's exactly what I'm saying, all the scale of a single core. Oh, absolutely. All I'm saying is that on a field scale you will not be able to see hydrologic effects. I cannot conceive -- now, I'm throwing this at the table --

[Everyone speaking at once.]

MR. DAEMEN: I'm not sure either, but suppose you have a wedge on top of the knoll that as a result of thermal effects shifts a little bit up -- I don't -- you know, that's why I'm going to see some design analysis like that. That is the type of mechanism I am thinking of. And when Tom asked the question, how do you identify which fracture to monitor, unfortunately I cannot answer that and that's the real dilemma because if I would know -- when you look at the set of joints at that knoll you have a whole bunch of parallel joint sets -- two sets, and my guess would be that there may be, and I don't know, but there may be if the thermal analysis suggests that, there may be one of those joints along which slip may occur a little bit.

Now, if somehow we could find out which one, and then monitor that one, then you know -- I'm just thinking in terms of how do you analyze the test design. I first would like to do a very simple analysis and identify is it really possible that the block may slip? Because I'm not sure of that. Although it's very encouraging when they say there is going to be 15 pounds of pressure there, but if that happens, then I think we might be able to have --

MR. NEUMAN: Should I try again, one more time, or should we leave this?

MR. NICHOLSON: No. We'll continue this, but I think because Todd stepped out of the room, I think some people want to take a short break and I have to get this thing downstairs in the next five minutes. Let's take a five minute break --

MR. NATARAJA: Can I ask one question before we --

MR. NICHOLSON: Ask the question, but we won't answer it. Ask the question.

[Laughter.]

MR. NATARAJA: My question is now, you said that you can't measure the impacts of mechanical changes on hydrology. Can we measure the impacts of the -- the thermal impacts on the hydrology on the field scale?

MR. NEUMAN: Well, it depends what you mean by the thermal impact.

MR. NATARAJA: The second that -- as far as transport is concerned.

MR. NEUMAN: Well, let's go back then to this issue of the shear stresses and how they are going to affect permeability, okay? Let's take the one experiment that I have clearly in front of my eyes and that is the experiment of Makurat. We chose counting on those random changes in permeability up and down in response to shear. You cannot predict which way it's going to be because of the complexity of a fracture -- single fracture on the scale. What this suggests to me, and I have no way to prove it, is that if you apply shear on a field scale to a fracture, and let's say there is a single fracture there, because there are some contacts in some places, the permeability is going to luckily increase the aperture is going to increase in other places. It's going to luckily decrease. That's what this single experiment which I am fully aware of suggests to me.

MR. NICHOLSON: Okay. Well, let's take a quick break and then you get to talk after the five minute break Shlomo, so you can begin then.

[Brief recess.]

MR. NICHOLSON: Shlomo, why don't you start then with your comments?

MR. NEUMAN: Okay. This is quite informal so I assume I can sit here? I don't have any viewgraphs to show.

MR. NICHOLSON: Yes.

MR. NEUMAN: Can you hear me?

MR. NICHOLSON: Linda wants to say something why don't you bring up that one point?

MR. LEHMAN: I just wanted to make a comment about this ongoing thing about Jaak's proposal and I agree that I think you may not be able to see it during -- at least during the experiment, but to me, I still think it's valuable, perhaps more qualitatively rather than quantitatively because if you know that you are getting some strain or dilatation along some certain fault set and you have some idea that you have fracture flow occurring, then maybe as a worst case you can say okay, in my scenario modeling I can assume that perhaps we're having an increase in flow. So from more or less a probability perspective or scenario perspective, that knowledge would be useful. But coupling it to get an exact flux, I don't see how that would be possible, but I still think it would be useful and I wouldn't want to rule it out just because you can't see it specifically in the hydrologic point.

MR. NEUMAN: I want to repeat what I said before and that is I do not see anything wrong in doing what Jaak is suggesting and I'm sure that it can provide you not only qualitative it may be even quantitative information that is relevant. What I'm suggesting is that from the standpoint of the hydrology, we are not going to see -- now, let me maybe go through some of my comments here and then we can come back to the details, because this is a good example of what my major concern with the heater experiment the way it was explained in the draft document of January which is the one I'm going to refer to what concern I had with that.

I think that most of you probably have received today a copy of my comments which are dated February 12, so --

MR. NICHOLSON: If you turn to your agenda, it's attached to the back of your agenda, Shlomo's comments to us.

MR. NEUMAN: That was written by me in response to a suggestion by Todd that I provide him with comments which he gave me, the document toward the end of January and I must say that even though my comments are dated February 12, after today I consider them still to be essentially valid in their -- almost in their entirety.

One aspect that is going to change is the scale. In here I was suggesting that the scale of 30 meters may not be a scale on which we can see things, and again we're coming to this ability to see things that we want to see. And it is now my understanding that Todd is considering to go down to either 10 meters or three meters. Three first and then 10. So I think that's a change.

What I would like to do since it all, still stands, is just give you a flavor of my comments here. If I had to put it in a nut shell in one sentence I would say that what I did not see in the original experiment of design though I realize that Todd is aware of the need to do so, but it certainly did not appear in the original document were well spelled out specific hypotheses or technical questions that the experiment would try to address. In terms of hypotheses, hypotheses that the experiment would try to refute or validate with respect to specific technical questions, questions that the experiment would promise to actually be able to answer, okay.

Under the heading of specific objectives in the original document there were some extremely broad questions, valid, but very broad questions asked. So I don't think that they are specific enough to be able to really judge the value of the experiment. In other words, I am finding myself in a situation where I don't know whether this experiment is going to be valuable or not valuable experiment, simply because there is not enough in the document to judge it.

Here is what I said. I am quoting from the document. The experiment is designed to evaluate the relative significance associated with excluding various processes and to evaluate scale dependent procedures used to estimate material properties. Very worthwhile, but extremely general. Modeling of the experiment or results is an important validation aspect and everything was still concentrated at a time when validation and is the principal reason for conducting the test. Okay. So something is to be validated as a result of this test.

And then what I continue saying is that these statements of purpose, objective, design goal and principal motivation behind the heater experiment are broad and ambitious. They are also very general and therefore open to multiple, perhaps conflicting interpretations. In other words, what I am saying is there are too general really, much too general to be able to judge their value and our ability to address them in the context of the experiment.

Then I go and kind of in one paragraph overview what we do know or what we know less and we don't know at all about the processes which will be taking place during the proposed experiment and I end up that paragraph by saying that aspects about which we know extremely little if anything include heat conduction and convection coupled with multi-phase flow and fractured porous media which is what we want to investigate. Multi-phase water transport through nonuniform porous and/or fractured rocks. Which is what we are going to encounter.

At temperatures above the boiling point which we may have, gas flow under similar conditions and solute transport under all but the conditions listed in connection with the earlier simpler isothermal case which I haven't discussed with you now here. In other words, what I'm saying is we know virtually nothing about solute transport under those conditions.

Now let me read to what I say next, the proposed heater experiment involves many aspects of foreign transport about which relatively little or nothing is presently known. With respect to these aspects the ability of the experiment to confirm and validate must be quite limited. One can only confirm or validate what one knows, or can reasonably hypothesize. You can validate a hypothesis, you can invalidate a hypothesis. You can't invalidate something that you know very little about. And then observe and measure. If you cannot observe and measure then your hypothesis cannot be validated or invalidated.

To date little has been done to validate our ability or lack of it to measure and describe, not to speak of predicting the space time distribution of water in fractured toughs under static isothermal conditions. Not to mention isothermal dynamic flow regimes. At a space time resolution that could clearly distinguish between the roles of matrix blocks and fractures. Not to think of finer channels.

In storing and conducting fluids on field scales of up to 30 meters. At that time it was a 30 meter experiment. So essentially what I am saying is that we are attempting to do -- to build into the heater experiment things which none of us have seen done with any degree of success on much smaller scales under much simpler conditions.

Our present understanding of isothermal floral transport in unsaturated fractured Tuffs and our current ability to define and measure element rock properties, and state variables are at best rudimentary. Such conceptual understanding and ability to define and measure are better, in my view, developed and validated under the relatively simple conditions of isothermal flow. So, what I'm suggesting is that if we want to do -- if one of the things that we want to do as stated in the original suggested experimental design was to compare models such as discrete fracture model versus porous media model versus dual porosity model versus stochastic porous media models then this experiment is probably going to be too complicated for us to be able to do this particular thing.

But that was one of the things, one of the objectives listed. The more complex conditions created by a heater test render it less -- and therefore make it much more difficult to relate effects to closes in an unambiguous manner than a well thought out and executed isothermal experiment. We don't have such isothermal experiments to date. So here we are planning a much more complex experiment.

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Given that model validation is considered to be the principal reason for conducting a heater experiment, I am asking what can we expect to be validate by such an experiment, specifically and I think that unless and until we have spelled it out in detail and spelled out in detail on how we are going to go about such a validation we really don't have an experiment which knows what it is going to validate and how.

How can such a validation be accomplished? Then there is a discussion in the original design about calibration versus validation there is a suggestion that the model will in fact not be calibrated or be only calibrated partially and I think Jaak made a very good comment on that in his written comments and I fully agree with what you said today, Jaak.

Given the length of the experiment, given the complexity, I don't see that we can rule out -- actually I don't think that we can avoid calibrating our model against data. Once you use a good amount of your data to calibrate your model then the question of validation become extremely difficult to answer. You will have very few data independent data against which you will be able to validate the model. So these are the kind of things that I think we need to think about.

I believe, just like Jaak does that a combination of these two approaches is needed if we are to have a successful experiment. I proposed here in my comments that one first discuss in some detail what models will be calibrated against what data, at what stage of the experiment and how and I also proposed that this be followed by a relatively detailed discussion in the outline -- in the design of the experiment of what aspects of the models or underlying theory or theories will be validated, against what data at what stage of the experiment and how. And only on the basis of such discussion need in my view become possible to evaluate the potential benefits of the proposed experiment. So I'm not saying the experiment is not valuable, I'm saying I really don't have anything to go by at the present time. To say that it is going to or is not going to yield valuable results.

Okay, let's go into some details. It is not entirely clear to me from the proposal how a heater experiment on the proposed scales of what used to be up to 30 meters and is now three or two, it seems to become a little bit clearer with a smaller scale, could under the given budget and time constraints -- now, you said, before Mel, that the budget is not going to be multimillion. I'm going to suggest that if we want to run a really good long-term large-scale experiment it will have to be multimillion or you will have to scale down both the scope and the scale of the experiment.

MR. SILBERBERG: No question about it.

MR. NEUMAN: Quite a bit.

MR. SILBERBERG: Yeah, but what can you do?

MR. NEUMAN: You know, the objectives spelled out in the original plan, in my view, is all achievable.

MR. SILBERBERG: We'll have to pick a program.

MR. NEUMAN: Would require tremendous amounts --

[Everyone speaking at once.]

MR. SILBERBERG: When I first --

MR. NEUMAN: -- of time and money.

MR. SILBERBERG: When I first read it, that was my impression too. I said there seems to be an awful lot here for the -- what I knew was the level -- the resource level of the work as I said -- it might add up, I said it would be nice if we could do it, but what do they say -- happy endings only happen in movies, you know.

[Laughter.]

MR. SILBERBERG: Something like that, right, you know.

[Laughter.]

MR. NEUMAN: So my suggestion would be, you know, I just said in my comments that I feel much more comfortable about a heater experiment if its purpose and objective were more focused and perhaps based on what you just said now also scaled down, scaled down in spatial scale, in time scale and in objectives as well. And then I made some specific proposals. I proposed that such a focus might be provided by attempting to answer specific questions and I go through a list of specific questions on pages 3, 4 and 5 of my comments.

Now, I'm not saying that all of these questions are important questions, but they are much more specific than the kind of questions that the original design document spells out. So let me just give you an example of what I mean by specific questions.

For example, number one. How accurately and with what space time resolution can one measure and describe the distribution of temperatures on a scale of up to 30 meters in unsaturated fractured tuff at the Apache Leap site? Okay. So one would have to say something about the density of measurements and the accuracy of measurements before one could say anything about the resolution with which he will be able to describe the temperature field. And it is my feeling that the temperature field can be measured much more accurately and with a much better resolution than anything else.

So if we decided that the temperature field can only be measured with a resolution of half a meter to a meter then we will know that certain fractures in between may be missed or there effect may be missed. And this is going to be doubly true and triply true about water contents, saturation and pressure. Pressures may be less though, but certainly water contents and saturations.

Now, here's a specific question. What accuracy and resolution are required to detect anomalies? Due to convective air and vapor currents through major fractures, and/or channels or on causes. If we are not interested in these anomalies and we have essentially a homogenous porous medium and Charlie's modeling results from today are valid, in fact we have done similar modeling of conductive heat with respect to the Oracle site using data from the Oracle site, and we saw exactly what you saw. Heat conduction affects a very small volume out of the heater we did it with a finite heater. It was much smaller than yours.

So if you really want to see the heat pipe effect and the heat pipe effect is -- involves movement of fluid, involves convection then you have to say something, first of all, am I interested in seeing it in fractures or in the porous. If I am interested in seeing it in fractures well then with what resolution can I see it. Will I see it in the temperature field for example. Okay.

So I'm making some specific comments and I will not go through those, but essentially what I am suggesting and I also suggested a change in the time schedule and the phases, the proposed phases of the experiments. I don't want to bore you with that, it late in the day, but essentially what I'm saying again, to summarize the key point that bothers me, I think what we need is to spell out in technical details what is it that we want to answer, what are the questions that we asking ourselves, not in generalities, but specifically what are the hypothesis that we would like to test, do we want to observe the heat pipe effect, is that all we want, qualitatively. Do we want to do more? Do we want to quantify it? On what scale do we expect it to occur? We can say quite a bit about it before we are on the experiment and then see whether or not we can address these issues with the expert. These are my comments.

MR. NATARAJA: Also, those questions and how they relate to the disposal --

MR. NEUMAN: Of course.

MR. NATARAJA: Because some of those questions may be addressed but not be -- of particular interest here.

MR. FORD: I have a comment here. This is basically my comment, that I feel that we need to get specific, we can't achieve all the goals probably that you've seen thrown up on the viewgraphs. Even if you had a large amount of money. When I was in Tucson last January I listened to all the comments. I was thinking, well, geez, even if you had a large amount of money you probably couldn't design an experiment to satisfy all these desires. So you are going to have to get specific on what specific desires you're trying to achieve with the experiment and design your experiment as best you can to satisfy those desires and then see what you can add on. So that you can at least achieve some of your objectives.

MR. NEUMAN: So, for that reason, for example I would side with the suggestion by randy Bassett that there is a lot that can be done in the laboratory on blocks before we go out to the field and/or if we go out to the field then we limit the scope, really not to more than three by three by three meters at most to start with. Unless somebody comes up with a very specific design which is addressing very specific questions that this larger scale experiment can address, I see here a white elephant in that we will be pouring not millions, but hundreds of thousands and a lot of hope which is really much more bothering to me, into an experiment which the entire Scientific community is going to concentrate on through INTRAVAL. They are all going to hope to see something come out of it.

First of all we saw today in terms of the timeframe nothing may come out of it within the timeframe of Phase II of INTRAVAL, but even after post-phase II of INTRAVAL people will be expecting to see results out of this and unless we know apriori that there is a high probability you never know in experiments what you will get. But there is a reasonably high probability that certain specific questions will be addressable in a quite unambiguous way, we will, I think stand where we stand with most of the other experiments that INTRAVAL I has addressed. And that is a lot of data, a lot of money a lot of time and a lot of modeling effort with very little conclusive conclusions that can be drawn out of them.

MR. FORD: On of my thoughts, Tom, was that one of the things they done in the past and we've had -- I've had projects like this where I didn't know quite how to approach it, was my first step -- I approached it in a step-wise fashion. And your first step might be to look at other experiments and see what you feel you need to focus on. You know, put some money into that.

MR. NEUMAN: Wy not, for example, for the DOE and the NRC jointly, put -- I know this is -- I shouldn't even say that, but maybe it something to consider, jointly put resources into evaluating existing experimental data for the G-Tunnel for example. What can we learn from the G-Tunnel experiment. Is there anything to be learned there, or did we not add there is nothing to be learned. I don't know I haven't heard enough about it so I have no idea.

If we cannot exclude the possibility that there is a lot still to learn for the G-Tunnel experiment, from the Stripa experiment, maybe from some other experiment, then maybe that's the first thing to do. Actually put resources into analyzing those data; put resources in to analyzing the Finsjon experiment, other experiments that have or have not crossed the INTRAVAL agenda. There is a lot out there. And at the same time, maybe you put resources into doing a block experiment in the laboratory, maybe a small scale field experiment. I would suggest, however, some monitoring of ambient temperatures. That's easy to do and I think that can be done at a relatively minor cost at shallow depths. Not at depths of more than a few meters.

What can we learn about ambient distribution under ambient conditions, can one detect any anomalies in the temperature from the temperature data about convection and so on?

MR. NICHOLSON: Than you very much Shlomo. Are there any questions of Shlomo, any -- Ron, you said you wanted to make a comment.

MR. GREEN: Yeah. This concerns an internal research project that we're conducting -- a geophysical investigation and it's been on the books for some time but approximately a month or two ago we got out to -- I wasn't personally involved with it, but we got out to Apache Leap site and did a geophysical crosshole study and we did both a lot still to learn for the G-Tunnel experiment, from the Stripa experiment, maybe from some other experiment, then maybe that's the first thing to do. Actually put resources into analyzing those data; put resources in to analyzing the Finnjan experiment, other experiments that have or have not crossed the INTRAVAL agenda. There is a lot out there. And at the same time, maybe you put resources into doing a block experiment in the laboratory, maybe a small scale field experiment. I would suggest, however, some monitoring of ambient temperatures. That's easy to do and I think that can be done at a relatively minor cost at shallow depths. Not at depths of more than a few meters.

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Then they also conducted some tomographic measurements at either three or six meters with a time domain, so there should be some tomographic results out by the end of July and there may be some problems, I just wanted to mention this for Todd because in Phase II you mentioned that you're going to identify measurement techniques and that's one possible non-intrusive measurement technique for water moisture measurements.

MR. FORD: I would say though that probably you wouldn't want to make your objective of this research to develop new techniques. You may have to, but probably not make that the objective like some of the earlier research because the Yucca Mountain project is kind of moving into a different phase and the length of this test that may not be one of your prior objectives.

MR. NEUMAN: You see, if you don't have instruments such as geophysical instruments that you can push through a relatively higher resolution then you will not get information from experiments of this kind whether they are conducted here or by DOE at Yucca Mountain. Then we'll be able to address specific and technical questions that come to my mind and I haven't seen to many others actually asked in the document. But I have quite a few specific questions that I'm suggesting that one try to ask. I don't see how without being able to get a high resolution of measurements across temperatures and water contents that you will be able to come up with an unambiguous answer to those questions.

MR. FORD: Yeah, I wasn't saying that you wouldn't have to develop some new gear. I was just thinking in terms of justifying the research.

MR. NEUMAN: You need techniques to make measurements. You need to be able to make measurements in any experiment.

MR. NICHOLSON: Okay, Jaak?

MR. DAEMEN: Not being a hydrologist I can ask this stupid question, right?

[Laughter.]

MR. DAEMEN: When I listen it sounds, honestly, so negative that I wonder if the state-of-the-art is that rudimentary for what is going to happen around the repository. And at what stage are we and what kind of a timeframe are you thinking in terms of demonstrating that the Yucca Mountain site might be an acceptable site.

MR. NEUMAN: I'm not sure I want to answer that question. Let me just refer to your saying that I sound very negative. Let me stress again what I am saying. What I am saying --

MR. DAEMEN: Well, it's a long list -- well, if I understand it a rather important concept when you say we know almost nothing about or very little about --

MR. NEUMAN: Maybe -- maybe what I should do in answer to that is go over with you the list, but I don't want to bore the company here.

MR. DAEMEN: Oh, no, the company is loving it all and listening to you --

[Laughter.]

MR. SILBERBERG: We have nothing to do tonight anyway.

MR. NEUMAN: Can one literally use the space, time, temperature distribution on a scale of up to 30 meters observed prior to activating the heater under ambient or pretest conditions by means of a simple model which accounts only for heat conduction and treats the rocks as a uniform continuum. The properties of which heat conductivity and capacity as functions of water content, for example, are based laboratory measurements on porous and blocks. It's a technical question. You may have an answer to it already. You may want to validate it, verify it on a scale of 30 meters. Okay. That doesn't mean that I am going to say that you cannot do this. All I'm suggesting is that there is a technical question here which needs to be addressed in order for you to be able to predict temperature distributions around the heater around the repository in Yucca Mountain.

Existing data suggests that heat conductivity and capacity vary much less than hydraulic conductivity and dispersitivity, we have seen it today. Can this reproduction be improved through a calibration process in which one varies the distribution of water contents in three dimensional space? Okay, now, to what extent is it important for you to know what the water content distribution is in order to predict temperatures? That's a technical question. You need to be able to address -- to give an answer to it, that doesn't mean that if you don't have this answer you must come to the pessimistic conclusion that Yucca Mountain is a bad site. But it's a valid technical question that must be addressed before you say something with a tremendous amount of confidence.

Can the calibrated water contents be verified, validated, independently by means of neutron probes? Tomography, auto measurements, if the latter is not possible, can such measurements be used to help you prove the calibration? In other words I am -- these are questions related to calibration, what can you learn from temperatures with respect to water contents and independently how can you measure water contents and so on.

Let me skip to something else. What can be learned from observations of temperature and water content under present conditions? With or without the above model about ambient heat fluxes through the rock on a scale of up to 30 meters? These are the kinds of questions I'm suggesting to address.

MR. DAEMEN: I was thinking more in terms of -- you made a quite strong and convincing argument that you don't even know enough about isothermal conditions.

MR. NEUMAN: That is correct.

MR. DAEMEN: Okay, I was thinking more about that.

MR. NEUMAN: Okay. Let me go back to that then.

MR. DAEMEN: And in a much broader complex that is the one of course that is fairly troublesome.

MR. NEUMAN: Okay. Let me first address the technical aspects of your question and then let me see if I want to say anything about Yucca Mountain.

VOICE: Or any repository site for that matter.

MR. NEUMAN: Or any repository site. Our current understanding of flow and transport in unsaturated fracture tuffs under field conditions is limited and speculative. I'm saying -- and I stand by this. Hence subject to uncertainty and controversy. Controversy we know, we have it right in this room. There are certain aspects of coupled heat, here I'm quoting from the document, liquid gas and solute transport, about which we know more and other aspects about which we know less or very little.

Just let me spell out some details. Aspects about which we know quite a lot include heat conduction in both saturated and unsaturated porous matrix. Liquid flow in saturated and unsaturated porous rock matrix under isothermal conditions. Conservative isothermal solute transport at tracer concentrations in saturated porous medium. Aspects about which we know less include, heat conduction in saturated and unsaturated fractured rocks. Although I would argue that we probably do know quite a bit about it, okay -- so --

MR. NATARAJA: I have a question about these questions.

[Laughter.]

MR. NATARAJA: How many -- how important is it to answer these questions to show compliance with --

MR. NEUMAN: Well, let's go back to that later. Let me just very quickly go through some of these. Liquid flows through saturated fractured rocks under isothermal conditions, gas flow and so on. And then I go through a list and let's go back to about which we know virtually nothing and let's see if we agree that we know virtually nothing.

MR. DAEMEN: I have no idea. I'm asking.

MR. NEUMAN: Okay asterisks about which we know extremely little if anything, about which I know extremely little or if anything include heat conduction and convection coupled with multi-phase fluid flow infracture porous medium. Multi-phase water transport through non-uniform porous and/or fractured rocks. These are highly non-uniform at temperatures above the boiling point, primarily.

Gas flow under similar conditions, and I say I know virtually nothing about sodium transfer under all but the simplest of these conditions. Okay. How important it is, that's a completely separate question.

MR. DAEMEN: Well, I'm saying, is it simply an academic type of inquiry or is it later to be raised a disposal problem, that is mine.

MR. PATRICK: Well, I would sure say those last three aren't terribly academic. Those are right at the heart.

MR. SILBERBERG: Yeah, at heart of the matter. Yeah, I mean -- I assume that's the --

MR. PATRICK: Well and transport under repository conditions.

[Everyone speaking at once.]

MR. DAEMEN: How accurately do you have to understand --

MR. SILBERBERG: That's another question.

MR. NEUMAN: That's the question.

MR. PATRICK: He's just saying that --

[Everyone speaking at once.]

[Laughter.]

MR. NEUMAN: If you ask me if I was a decision maker with sufficient power to make decisions, then I would make my decisions without having the knowledge of all of this. I would have to make a decision with respect to waste disposal of nuclear waste. There is a problem of nuclear waste, there is an issue of energy used in the future. I have certain political convictions of my own and I have certain understanding of my own -- my subjective understanding of decisions, so I would make a decision based on that -- without having answers to all of this. I would opt for underground nuclear waste disposal. I would perhaps weigh again what is the best place, NTS, not necessarily, but then if I was the decision maker without constraints then I would -- I would be able to move outside of NTS. You live in a political world.

MR. NATARAJA: No, but the point --

MR. NEUMAN: And so the question is political.

MR. NATARAJA: No. No. I'm talking from 10 CFR 60.

MR. NEUMAN: 10 CFR 60 --

MR. NICHOLSON: If you look at 10 CFR Part 60 the reason for the task and we put up at the very beginning of this meeting, the three tasks in their project that related to what they're doing is because Part 60 does address, especially with regard to the unsaturated zone criteria, this issue of vapor phase transport. It also addresses the complexity of a site with regard to these various coupled processes. And so we have to think what is a logical series of experiments that Research wants to do independent of DOE to get insights into how this may occur. We've already done, in the previous experiments with the simple two bore hole set up in Queen Creek road tunnel, very simple.. A qualitative experiment. Bill Davies did the core experiment. Now, the questions is how do we then move on to this whole issue of coupled processes.

What we didn't discuss today was that we had a project at one time, with Lawrence Berkeley Laboratory's Chin-Fu Tsang who did a whole project on coupled processes, he's published a book on that. All Right, We've also been involved in INTRAVAL and we are very much aware, obviously, of what you've talked about with regard to the Stripa and the G-Tunnel and the Climax. And we understand the importance of that and we want to build upon what we have learned from those experiments to design and you're quite correct Shlomo, a well designed, focused experiment to look at coupled processes.

Now, either we can do something, or we are going to do nothing at all, unless we get a large amount of money and do it so-called "right." I don't know what "right" is, just like I don't know what conservative is, or we can take what limited resources we have been given and do the best job we can to understand in a beginning sense what coupled processes are all about in a natural environment. We can do it in the lab for ever and ever but people always argue that's under such strict controls, it has no bearing on the real world. So we want to do both. We want to slowly work in the field and when we get the field plan we backtrack and say we'll do it on a matrix block without a fracture or the fractures hopefully will have a minimum effect. But we also want to do it on a large block, 10 meters on a side, in which we have a single fracture and which we can try to understand the effect of that fracture on this coupled processes.

And, you know, I have no problem with the questions you're raising. I have no problems with that whatsoever.

MR. NEUMAN: My problem, Tom, is very simple. I am not against any experiment of any sort. I would be the first one to support experiments. I believe in experiments.

I believe in the science needing to make experiments. Chin-Fu's book is based primarily on models.

MR. NICHOLSON: Exactly.

MR. NEUMAN: And Chin-Fu's book is a collection of papers which are modeling studies. You have my name in there because I wrote a little thing up in back of the book so I am quite familiar with the book.

There is very little experimentation that has been done. Okay. So, the pessimism that is being attributed to me is not -- I don't want anybody to think that I am saying don't make experiments. On the contrary. Make as many of them as possible, but why today during a full day of discussions dedicated to the experiment we have not asked one technical question and come up with one set of technical answers as to how we are going to address this technical question. This is all general.

Yes, we want to make experiments. Yes, we have to do this, we have to do that -- but what is the specific question we want to address with this experiment and are we going to get an answer? What is needed? What kind of measurements? What kind of instruments? We haven't touched that. That's my criticism.

MR. NICHOLSON: If this were to go on for two days, which I wish we could have, then I would recommend that Todd provide to you people with what he and I were luckily able to obtain from Abe Ramirez and the gentleman at Lawrence-Livermore where they went through to the best of their ability an autopsy of the G-Tunnel experiment telling us lessons learned, "surprises," I think is what Abe Ramirez referred to them.

We do have in them in the comments Todd has written up as a trip report and we do have some of the documentation that he has provided. We want to go through that work in detail. And from that information, as well as the Climax, as well as Stripa, then begin to frame the specific questions you want to address using, I think, hypothesis testing with regard to it not just thermohydrologic or thermohydrologic mechanical. I don't know and that's why Jaak is involved to try to give us some insights into the rock mechanical aspects.

MR. NEUMAN: That's what I'm suggesting, that's all.

MR. NICHOLSON: And we could do that, we could easily do that. We may want to reserve at a later time after Todd has gone back and revised the report. We have said that. I think Mel has said this earlier this morning, a commitment to do the project and specifically to write a NUREG/CR report that spells out the detail that you've asked him for. Especially with regard to bringing in lessons learned from previous field heater and laboratory experiments.

And perhaps that would be wise at a future date to go through the detailed specifics you want, especially after Todd and of course the Golder Associates people and the Center (CNWRA) also do some modeling using the TOUGH code, looking at this from an INTRAVAL standpoint. And I think that would be very wise.

MR. NEUMAN: And here is an example of a question you should look at based on today's discussion. Can you measure changes in the stress, possibly strain field, at the knoll as a result of heaters of given geometry given output and can you relate this to some hydrologic parameters? Spell out what they are, spell out how you would go about answering this question. Those are the kind of things that I think we need to discuss and that's all my criticism essentially centers about it, it hasn't been done, and I think it needs to be done. That's all I'm saying. That's all I'm saying.

MR. PATRICK: Well, you had another point as well, and it's one that I made in our comments -- as one of the commenters from the Center stated and that is that there is a lot of data out there. A lot of data that we don't understand and I think our comment may even have been should there be a zero phase to this study that doesn't just go in and do what you alluded to in your closing remarks there, Tom, it goes in and examines surprises and stuff from the test, but that actually goes in and uses data that was acquired at a cost of millions and millions of dollars and which no one has really -- even the people who collected the data do not feel that it has been appropriately analyzed. And I mean those folks usually have a pretty big ego and they say, well, yeah, I just really rung that stuff out.

But every one of the people that you've heard in my case reporting directly and Charlie reporting on behalf of others and Todd doing the same, no one feels that they've analyzed the data that's at hand.

MR. SILBERBERG: I understand what you're saying, but why shouldn't this agency make a request that there is data out there and the department do more work with it and we would be happy to do something too, and you know, check it. Any -- I'm not asking for an answer but I would say that I would recommend to my colleagues from the -- I would recommend this to my colleagues from the NMSS that -- look, if there's you know, if you say there's good data out there and a lot more can be done with it, sound like -- not only should we be able to do something with it in our little -- with our small program, but perhaps -- we perhaps should encourage the department to do the same thing.

MR. VOSS: The problem is politics because --

MR. SILBERBERG: Well, I don't --

MR. VOSS: -- because we've been told and --

MR. PATRICK: Problemmatic sensitivities.

[Laughter.]

MR. PATRICK: Is that politically correct.

[Everyone speaking at once.]

MR. SILBERBERG: You don't have to answer.

MR. VOSS: This wasn't quality assurance. It wasn't done under proper quality assurance programs, so -- the data is not defensible --

MR. SILBERBERG: That's the answer, we don't want to look at it because it didn't have the quality assurance.

MR. VOSS: No. That's an illegitimate answer except as a licensing issue. Except as a licensing issue, I would say that may not be able to do --

MR. SILBERBERG: That may not be legitimate, okay.

MR. PATRICK: The data acquired at WIPP, I mean we're talking about a diverse set of experiments.

MR. SILBERBERG: Sure.

MR. PATRICK: The data acquired there, the data acquired at Table Rock the New Lines experiment, the spent mule test, all of those programs had quality assurance programs in place.

MR. SILBERBERG: Okay.

MR. PATRICK: I mean even from a licensing standpoint you have a NUREG that covers how you deal with preexisting data.

MR. SILBERBERG: Right.

MR. PATRICK: We're talking about a scientific endeavor here.

MR. SILBERBERG: Yes.

MR. PATRICK: If the observations are documented and the instrumentation was calibrated, which I think without exception the studies we're talking about, that is the case. We have reasonable expectation that those data accurately represent what went on at those sites.

MR. SILBERBERG: But I -- okay. Actually, I have now missed the scope of this discussion and I've gotten outside -- I've violated my own boundary conditions by going outside and getting into -- getting beyond the NRC which is inappropriate. I mean still after this meeting is over I can choose do what I like, but -- but, no, for this discussion I -- really I didn't want to put you on the spot, certainly not, by all means. But you know, that question could be asked.

MR. NICHOLSON: I think Wes has a very legitimate point --

[Everyone speaking at once.]

MR. NICHOLSON: The dilemma is --

MR. DODGE: I don't think you want to hold up your research program for DOE.

MR. SILBERBERG: No. But you know us, we haven't done that yet.

MR. NICHOLSON: What we've done is, Todd has begun the communication process, we have gone and visited the national labs, Charlie Voss and the DOE people have done, as I understand, to the best of their ability tried to look at the G-Tunnel data and if possible analyze it and process all of that data into something that would be of value to INTRAVAL.

We're at the mercy of their good wishes and their abilities, as well as the national labs. This, to me, reinforces the question of why NRC has to have an independent research capability. I would prefer, if possible to do what you said, and I think in a practical sense we can't rely solely upon getting their data and analyzing it and understanding it.

MR. SILBERBERG: He's not suggesting you do that.

MR. NICHOLSON: No. No. No.

MR. SILBERBERG: He's say to start with.

MR. NICHOLSON: To start with and we are committed to do that to a certain extent. The question of how much, I don't know.

MR. PATRICK: Well, we're mixing two things here. You're mixing getting their data and analyzing it --

MR. SILBERBERG: Yeah.

MR. PATRICK: -- with waiting for them to analyze it.

MR. SILBERBERG: Yeah, and those are two different things, sure.

MR. NICHOLSON: Well, okay. What I would recommend is what Charlie said. We're going to rely upon Charlie to get the G-Tunnel data and to process it and to provide it to us. In the meantime Todd will communicate, again, with the Lawrence-Livermore Laboratory people who have done the work in G-Tunnel and Climax and of course that will be part of the INTRAVAL lessons including the Stripa data. But those can only give him some broad lessons learned and some practical aspects of instrumentation of design of the experiment or surprises. It's still his responsibility at the University of Arizona to take that wealth of information and to focus it in on the experiment they propose to do for us.

MR. DODGE: The modelers that tried to model and analyze that data?

MR. NEUMAN: Let me maybe explain, you know, since University of Arizona is being mentioned.

MR. NICHOLSON: Right.

MR. NEUMAN: There are four people, I think, on the project. Dan Evans, Todd, Mike Sully and Randy. Now, we have to recognize that Randy and Mike are actually concentrating on other aspects of the project and Mike told me that he essentially concurs with my comments. So you can almost see my comments as coming from me and him. You have separate comments from Randy Bassett who is suggesting essentially to scale everything down to the laboratory to a block. I am not sure that that is absolutely necessary, but I definitely think that given what I have seen today, that may not be a bad idea to think about scaling things down.

So, right now what we have is Dan and Todd coming up with this particular proposal and we essentially saying now wait a second, this is too general for us. We don't know how to go about it this way. And we are not sure that we want to commit the University of Arizona to going in that direction because right now to us, it's not clear where it leads. That's where the University of Arizona stands on this.

MR. NICHOLSON: And we're in the midst right now of going out and getting comments both internal and external. Unfortunately there are other gentlemen who we've talked with and ladies who weren't here to provide their comments and you have they comments. You know, I won't go into who all theose people were, but I think it is obvious that the onous is on you people, and when I say you people, I mean the University of Arizona to go through the comments and to revise the statement of -- excuse me, the experimental plan and then to submit that to us. And we will review it as --

MR. NEUMAN: Maybe that's something that you should discuss as well with the department chair.

MR. NICHOLSON: Sure.

MR. SILBERBERG: Well, I'm just saying that -- I'm just telling you what the office policy is. The office and the agency policy is that the quality and the correctness or the appropriateness of a piece of work in the final analysis rests with the performing group. Be it the university, be it the Center, be it whatever.

MR. NEUMAN: That is why you have seen --

MR. SILBERBERG: Because --

[Everyone speaking at once.]

MR. NICHOLSON: We want that.

MR. SILBERBERG: I think that's a very proper statement. If the group -- if the performing group organization has questions, what are you doing, the organization with the University of Arizona or whatever it is, any other laboratory, then that's a message to us that you know, if they're performing -- if the organization responsible for doing the work has a contract to do a piece of work, if they're saying wait a minute I want to think about this, then we stop right there.

MR. NICHOLSON: Okay. Are there any other comments? Let's go around the room and we'll finish off by allowing people who haven't had a chance to say anything to make a comment on the experimental design as presented today or what they reviewed?

Rex, do you have any comments?

MR. WESCOTT: Yeah, you know we sent in some comments and I think from the -- they're on the last page -- and I think from you know what we've heard, I think we would probably want to modify some of ours. I think our concern about the electrical resistivity maybe has gone away a little bit, but I think you mentioned tomography and certainly Ron you talked about some stuff you did out there. I would be interested in seeing the results, but it appears that you may have a procedure for getting some pretty good real time moisture data which we think is very important for this experiment to work.

I think we're probably still concerned about scale. You know, I don't really how much you've got planned on the three meters -- that three meter block experiment that you're going to be doing in the laboratory, I don't know how --

MR. SILBERBERG: In the field.

MR. WESCOTT: In the field, okay. I don't know, you know, how Bill feels, maybe Bill would want to say something, but I think we would probably like to see a lot more done on that where you can control your contributions much better before you -- I mean I don't really if enough has been gained yet to know how to design a large field experiment. I think I would probably like to see more block stuff, but at the same time we realize that if you're going to do a field experiment you've at least got to get into the monitoring phase on it, you know very shortly in order to have a good baseline. So, yeah, I think we modified some of our comments and maybe some others we might even make a little bit stronger, I think I would like to go through a look at the transcripts of this whole -- and read all the comments that are attached before I say too much more.

MR. NICHOLSON: Okay, Frank?

MR. DODGE: One of the questions that will be asked is what would be the questions that would be answered by this particular project. This obviously what we call a tight find demonstration strategy, you may not know what that -- a strategy in which we will have to do some independent research and modeling in order to be able to review the findings. So I mean that's one of the things we would be interested in finding out. What questions are we answering of regulatory questions.

MR. SILBERBERG: You'll also come back and tell us what you think you might need to do that too. To see if we can meet.

MR. DODGE: I mean that's where the --

MR. SILBERBERG: I don't know. What do you think you need? And then you know, we'll think what we think you think you need --

[Laughter.]

MR. SILBERBERG: -- and maybe the two will come together. You know you have to start over in your -- okay, in your organization.

MR. DODGE: I'll think about that. I think that's using the letter is supposed be, that's what it is supposed to accomplish. We write a letter to you saying this is what we need and you're supposed to provide us those answers, so hopefully --

MR. NICHOLSON: I think they just respond, they don't always provide.

[Laughter.]

MR. TANIOUS: I just have one comment about the connection between the displacement and the hydrology and if it's worthwhile then I would say this, that some of these instrumentation that you know things like laser surveying instruments can be used to detect quite a bit of small displacements at the surface. Somebody several years ago had using a vibrating rod instrument to measure across a joint. I don't know what they did with that, but --

MR. VOSS: Hopefully they threw it away.

[Laughter.]

MR. TANIOUS: So this is to the standard --

[Everyone speaking at once.]

MR. TANIOUS: Nevertheless, the idea is to find instrumentation to make a displacement and to -- the other side of that of course is the flow -- is predominantly in fractures as given by some of your slides where you had some plastic bags over some fractures showing that there is air movement in these fractures. So you can make the -- between the two and get, if not quantitative, at least for a creative connection between rock mechanic at law, I believe you can do it. You just have to do a little bit of detective work and planning with your field work already that you probably can do a good a job there.

MR. NICHOLSON: Ralph, do you have any comments?

MR. CADY: No, not really.

MR. NICHOLSON: Wes?

MR. PATRICK: Nothing further?

MR. NICHOLSON: Jake?

MR. PHILIP: Well, just to -- a little bit on what Jaak was talking about, he talked about putting in a heater and it's going to be pretty in a very shallow depth almost and he's talked about a wedge, you have a wedge, you know, the wedge could displace, it is very possible that there would be no flow over there because flow takes place only in a very very small percentage of fractures. So even if that block moved from a hydrological standpoint you may not see anything because the flow never occurred there.

If you look at some of the experiments that Todd has done in his previous site, he tried to get a hydraulic connect between two bore holes and he thought he had terribly characterized that, but he could not get a connect between two bore holes it's a very short -- very close to each other. Sure, the complexity of the problem like Shlomo says and we don't know where it's going. It could be going through a fracture, which they would never encounter in just putting those two bore holes so close to each other.

So, surely deformations do matter, particularly when you are right up on top of the mountain where you get -- with an absence of stress you get most of your deformation. And if you just look at the hydrology without looking at rock behavior, I don't know how you analyze any results that you get if you get any. So, that is the problem that I see.

MR. NICHOLSON: Linda, comments?

[No response.]

MR. NICHOLSON: Charlie? Anything you want to add? [No response.]

MR. NICHOLSON: No.

MR. NATARAJA: You have the last word.

MR. VOSS: I'm just a little bit nervous from the INTRAVAL point of view. We're heavily -- I can say that DOE is more committed in this particular instance than I've ever seen them ever committed.

MR. NICHOLSON: Great.

MR. SILBERBERG: That's what I want to hear.

MR. VOSS: I just don't want to go back next year and so oh, by the way, that experiment is not going to be done any more or it's going to be done three years from now when a lot of issues -- and valid issues are resolved. So that's my concern.

MR. NICHOLSON: I don't think our airing of both NRC contractors and staff comments says that the NRC is no longer committed to do this work. I think we still are. The question is how is it to be done and what is the logical process by which it should be done.

MR. VOSS: I was more concerned about timing.

MR. NICHOLSON: Oh, well, timing is always an issue at the NRC. And because of budget and other factors we can't make any promises but we'll do our best. I guess we really should give Todd the last word and the I'll let Mel thank everybody.

Todd, do you have any comments?

MR. RASMUSSEN: Well, I mean, we've been treating this more as a generic study with no obligation to actually do the experiment at this point. It's more of a study exercise and how would one go about performing such a test and I want to reemphasize that. And that perhaps this is not something that would ever happen at the Apache Leap, but hopefully DOE may benefit by having a group of people like us go through this exercise and think it through. What are the issues? And what technological capabilities do we have?

Hopefully we can pursue this and actually demonstrate it at the Apache Leap site and actually evaluate some of the technologies that are available. And the other comment I wanted to make was that time to get the theoretical and modeling interest together at an early stage and have them help and determine what the precise objectives or hypotheses to be tested are, I think, is quite important.

Perhaps I look at it more from an experimentalist point of view of you know, how do I actually monitor these variables rather than the hypothesis testing end of things. So I welcome any comments regarding possible hypotheses to be tested.

So I think it makes the exercise more relevant to the modelers, I think, if they have an integral say in how the experiment is designed and in particular what you want to focus on in terms of data sets.

MR. NICHOLSON: Thank you. Mel, the last word.

MR. SILBERBERG: Thank you. Well, first I'm sorry that I wasn't here for the -- for as much of the meeting as I would have like to have been, but I think for the time that I was here during the afternoon, at the end, I think I got a good -- very good sense of discussion and comments.

I want to first thank Todd for all of the work and putting together what he's done and what he's done so far and talk to a large number of people and try to put together a lot of information. That obviously is a difficult -- difficult undertaking and I think a lot has been done and it's clear from the comments that a lot more needs to be done, but that's why -- that's why we called this meeting. And in the spirit of, you know, what -- why are you calling a peer review, as I started at the beginning of the meeting that I indicated why we wanted a peer review and what we would hope to get out of it, it's very clear to me that we've achieved that objective and we have a very good collegial airing from a broad cross section of expertise around the country. All involved in some different interests on the program, but on the work, but -- representing different interests, but the important thing was it also demonstrates that at least on a technical and scientific level we can communicate. We should be communicating and in fact we can.

So I appreciate people that were within the NRC community, and outside of the NRC community being here. Taking the trouble -- we do value your comments and we're certainly going to take them quite seriously.

I would hope that maybe with this meeting, maybe we've set some kind of a model, I guess, or template on you know, how we might want to do things in the future on different programs at different times depending on the subject and you know, where we are. There is no substitute for taking that time and having these kinds of discussions.

So I want to thank you very much. I feel that our objectives for the meeting were fully fulfilled and as promised at the beginning, we will certainly obviously come back to you with what we think is the next phase of this venture, after of course letting everyone factor in all of the comments that were made. And I'm not sure what that next step is. That is something I will have to hear from Todd and from Tom and the rest of the University certainly. So we'll certainly keep you informed, but again I must thank you very much for all your help.

MR. NICHOLSON: Thank you.

[Whereupon at 5:40 p.m. the meeting was concluded.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

in the matter of:

NAME OF PROCEEDING: Public Meeting on Apache Leap Tuff Site Field Heater Experiment

DOCKET NUMBER:

PLACE OF PROCEEDING: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

[signature]

Official Reporter
Ann Riley & Associates, Ltd.

AGENDA FOR

PUBLIC MEETING ON APACHE LEAP TUFF SITE FIELD HEATER EXPERIMENT

U.S. Nuclear Regulatory Commission Headquarters

June 21, 1991

9:30	Welcoming Remarks and Introductions - Mel Silberberg, RES
9:45	Research Objectives and Strategy - Tom Nicholson, RES
10:00	Executive Overview - Todd Rasmussen, UAz
10:30	Break
10:45	Design Strategies - Todd Rasmussen, UAz
12:00	Lunch
1:00	Implementation Strategies - Todd Rasmussen, UAz
3:00	Break
3:15	Open Discussion - All Comments will be transcribed by stenographer for consideration by research investigators. Written comments can be submitted up to thirty days following meeting.
5:00	Adjourn

Topics relevant to HLW performance assessment:

- Model validation aspects
- Relevant coupled nonisothermal processes
- Alternate conceptual models
- Site characterization (e.g., hydraulic, pneumatic, thermal, rock mechanical and transport properties)
- Field instrumentation and monitoring design issues

Key Technical Issues:

1. The appropriateness of the field scale;
2. The ability of the investigators to monitor and isolate specific dynamic space-time domain responses; and
3. Possible difficulties in identifying ambient and boundary conditions for the proposed site.

Validation Studies for Assessing Unsaturated Flow and Transport Through Fractured Rock (FIN: L1282)

Investigators:

Daniel D. Evans
Todd C. Rasmussen
Michael J. Sully
Randy L. Bassett

TASKS:

1: Laboratory Heater, Hydraulic, Pneumatic and Tracer Tests

2: Field Heater Tests

5: INTRAVAL Test Case 1

RESEARCH OBJECTIVE

Examine individual and coupled processes

as they relate to the effects of a thermal source

in a heterogeneous hydrogeologic system

ALTS Proposed Experiment

- Integral component of RES's HLW research plan
- Creates a logical lead-in to analyzing future coupled processes experiments on a variety of spatial and temporal scales
- Builds upon previous laboratory and field nonisothermal experiments (e.g., NRC UAz and CNWRA, DOE G-tunnel, Climax, and WIPP)
- Provides the opportunity to integrate multidisciplinary research issues for assessing complex nonisothermal systems
- Enables testing of conceptual models for performance assessment of HLW.

DRAFT

NONISOTHERMAL EXPERIMENTAL PLAN IN UNSATURATED, FRACTURED TUFFACEOUS ROCK

COORDINATED BY:

DANIEL D. EVANS AND TODD C. RASMUSSEN
DEPARTMENT OF HYDROLOGY & WATER RESOURCES
UNIVERSITY OF ARIZONA, TUCSON, AZ 85721

PREPARED FOR:

OFFICE OF NUCLEAR REGULATORY RESEARCH
DIVISION OF ENGINEERING
U.S. NUCLEAR REGULATORY COMMISSION
WASHINGTON, DC 20555

JUNE 15, 1991

OUTLINE - JUNE 20, 1991

9:30 INTRODUCTION

THOMAS J. NICHOLSON, U.S. NRC
OFFICE OF NUCLEAR REGULATORY RESEARCH

9:45 OVERVIEW

SYSTEMS APPROACH
PROCESSES
DEFINITIONS
COMPLEXITIES
I.T. VS. R.W.
UNANTICIPATED EVENTS
FIELD SITE DESCRIPTION

10:30 BREAK

10:45 DESIGN

MOTIVATION
OBJECTIVE
SUB-OBJECTIVES
PHASES
PROCEDURES

12:00 LUNCH

1:00 IMPLEMENTATION

ANALYSIS OF PREVIOUS EXPERIMENTS
(UAz, CNWRA, WIPP, CLIMAX, G-TUNNEL, FOREIGN)

BASELINE MONITORING

SITE CHARACTERIZATION
CORE, BLOCK, AND FIELD TESTS

IDENTIFY TECHNOLOGICAL CONSTRAINTS

SIMULATION MODELING

FIELD HEATER EXPERIMENTS
SITE PREPARATION
HEATER STRENGTH, ORIENTATION AND DIMENSIONS
HEATER COMPOSITION AND CONSTRUCTION

2:40 BREAK

3:00 DISCUSSION OF COMMENTS

3:30 ROUND-ROBIN DISCUSSION

5:00 ADJOURN

Guiding Philosophy

- U.S. DOE will conduct nonisothermal tests at Yucca Mountain to demonstrate near-field engineered barrier systems.
- Previous prototype experiments have yielded new insights into nonisothermal processes and been used as a platform to test new technologies.
- In order to focus attention and prepare U.S. NRC staff with a knowledge base upon which to evaluate U.S. DOE experimental plans and procedures,
- As well as to confirm existing conceptual models and technological capabilities,
- In conjunction with the need to identify technological and conceptual inadequacies:
- A generic "Nonisothermal Experimental Plan" is proposed with specific application to the Apache Leap Tuff Site.

Proposed DOE Site Characterization Studies

- 8.3.1.3.3 Studies to provided information required on stability of minerals and glasses

  o Natural analog of hydrothermal systems in tuff

  o Kinetics and thermodynamics of mineral evolution

  o Conceptual model of mineral evolution

- 8.3.1.15.1.6 In situ thermomechanical properties

- o Heater experiment in unit TSw1
- o Canister-scale heater experiment
- o Yucca Mountain heated block
- o Thermal stress measurements
- o Heated room experiment

8.3.4.2 ISSUE 1.10: HAVE THE CHARACTERISTICS AND CONFIGURATIONS OF THE WASTE PACKAGES BEEN ADEQUATELY ESTABLISHED TO:

(A) SHOW COMPLIANCE WITH THE POSTCLOSURE DESIGN CRITERIA OF 10 CFR 60.135, AND

(B) PROVIDE INFORMATION TO SUPPORT RESOLUTION OF THE PERFORMANCE ISSUES?

- 8.3.4.2.4.1 CHARACTERIZE CHEMICAL AND MINERALOGICAL CHANGES IN THE POSTEMPLACEMENT ENVIRONMENT.

- o ROCK-WATER INTERACTIONS AT ELEVATED TEMPERATURES
- o DISSOLUTION OF PHASES IN THE WASTE PACKAGE ENVIRONMENT
- o NUMERICAL ANALYSIS AND MODELING OF ROCK-WATER INTERACTION

- 8.3.4.2.4.2 HYDROLOGIC PROPERTIES OF WASTE PACKAGE ENVIRONMENT

- o SINGLE-PHASE FLUID SYSTEM PROPERTIES
- o TWO-PHASE FLUID SYSTEM PROPERTIES
- o NUMERICAL ANALYSIS OF FLOW AND TRANSPORT IN LABORATORY SYSTEMS

- 8.3.4.2.4.3 MECHANICAL ATTRIBUTES OF THE WASTE PACKAGE ENVIRONMENT

o WASTE PACKAGE ENVIRONMENT STRESS FIELD ANALYSIS

- 8.3.4.2.4.4 ENGINEERED BARRIER SYSTEM FIELD TESTS

  - o REPOSITORY HORIZON NEAR-FIELD HYDROLOGIC PROPERTIES
  - o REPOSITORY HORIZON ROCK-WATER INTERACTION
  - o NUMERICAL ANALYSES OF FLUID FLOW AND TRANSPORT IN THE REPOSITORY HORIZON NEAR-FIELD ENVIRONMENT

- THE NONISOTHERMAL EXPERIMENTAL PLAN CAN BE USED TO:
  - o GUIDE THE EXPERIMENTAL DESIGN PROCESS SO THAT ALL RELEVANT PROCESSES ARE INCORPORATED;
  - o DOCUMENT THE CURRENT CONCEPTUAL MODEL OF SYSTEM RESPONSES TO NONISOTHERMAL CONDITIONS;
  - o REVIEW EXISTING DATA FOR THEIR APPLICABILITY TO FUTURE NONISOTHERMAL EXPERIMENTS; AND
  - o IDENTIFY AND ANTICIPATE CRITICAL RESEARCH NEEDS.

MOTIVATION

- IDENTIFY RESEARCH NEEDS.
- CONFIRM EXISTING UNDERSTANDING AND METHODOLOGIES.
- ANTICIPATE PROBLEMS AND DEFICIENCIES.

OBJECTIVE

- MONITOR RESPONSE OF GEOLOGIC SYSTEM TO A THERMAL SOURCE, AND TO ADEQUATELY EXPLAIN THE OBSERVED BEHAVIOR USING NONLINEAR, COUPLED, HETEROGENEOUS CONSTITUTIVE RELATIONSHIPS.

SUB-OBJECTIVES

- EVALUATE ABILITY OF EXISTING TECHNOLOGIES TO OBTAIN MEANINGFUL AND RELEVANT DATA.
- EVALUATE RELATIVE IMPORTANCE OF VARIOUS PROCESSES RELATED TO WASTE MOVEMENT IN UNSATURATED FRACTURED TUFFACEOUS ROCK.
- EVALUATE CAPABILITY OF EXISTING CONSTITUTIVE RELATIONSHIPS TO REPRODUCE THE OBSERVED BEHAVIOR.

COMPLEXITIES

- NONLINEAR CONSTITUTIVE RELATIONSHIPS

  PARAMETERS ARE A FUNCTION OF THE STATE VARIABLE.

- COUPLED PROCESSES

  STATE VARIABLES AND PARAMETERS ARE A FUNCTION OF STATE VARIABLES FOR OTHER PROCESSES.

- HETEROGENEOUS ENVIRONMENT

  PARAMETERS VARY SPATIALLY AND AS A FUNCTION OF SCALE.

  THE MEDIUM HAS VARIABLE DIMENSIONALITY AND CONSISTS OF MULTIPLE POROSITIES.

IVORY TOWER VS. REAL WORLD

- REDUCE COMPLEXITY

  START WITH FIRST ORDER ANALYSIS.

  MINIMIZE INTERACTIONS BY ISOLATING PROCESSES.

  PERFORM ON COMPUTER OR IN LABORATORY.

  LEARN FROM PREVIOUS EXPERIMENTS.

  MINIMIZE PRECONCEPTIONS.

- INCORPORATE UNCERTAINTY

  ACCEPT LARGE GEOLOGIC VARIABILITY.

  REPEAT EXPERIMENTS UNDER WIDE VARIETY OF CONDITIONS.

SYSTEMS APPROACH

- TOP-DOWN DESIGN
  BOTTOM-UP IMPLEMENTATION
- FOCUS ON GEOLOGIC PROCESSES
- GUIDED BY REGULATORY FRAMEWORK AND
  EXISTING TECHNOLOGICAL CONSTRAINTS
- MAINTAIN I.T. PERSPECTIVE WHILE
  ACKNOWLEDGING R.W. CONDITIONS

DEFINITIONS

- **PROCESS:**

  A NATURAL PHENOMENON WHICH INCORPORATES ACTIONS.

- **CONSTITUTIVE RELATIONSHIP:**

  A CONCISE MATHEMATICAL DESCRIPTION OF A PROCESS WHICH INCORPORATES INPUTS, OUTPUTS, CHANGES IN STATE, PARAMETERS AND INITIAL CONDITIONS.

- **STATE VARIABLE:**

  A THEORETICALLY MEASURABLE QUANTITY WHICH UNAMBIGUOUSLY DETERMINES THE STATE OF THE SYSTEM.

- **INPUT AND OUTPUT:**

  THE MOVEMENT OF MASS OR ENERGY INTO OR OUT OF A SYSTEM.

- **INITIAL CONDITION:**

  THE INITIAL STATE OF THE SYSTEM.

- **PARAMETER:**

  A MATERIAL PROPERTY OF THE SYSTEM.

Processes

- Thermal
- Hydrologic
- Chemical
- Pneumatic
- Mechanical
- Biological

Processes, State Variables, and Parameters

Process	State Variable	Parameters
Thermal	Temperature	Thermal Conductivity Heat Capacity
Hydraulic	Pressure Head	Hydraulic Conductivity Characteristic Curve
Pneumatic	Gas Pressure	Air Permeability Compressibility
Vapor	Vapor Pressure	Vapor Diffusivity
Solute	Concentration	Solute Diffusivity

Related Disciplines

- Hydrogeology	Saturated environment
- Soil Physics	Soil environment
- Rock Mechanics	Mine environment
- Geochemistry	Saturated environment
- Atmospheric Physics	Above surface environment
- Chemical Engineers	Laboratory environment

Uncoupled Processes

$$\nabla \cdot q_I = \nabla \cdot (K_I \nabla \phi_I) = C_I \, \partial\phi_I / \partial T + Q_I$$

WHERE

- ∇ DIVERGENCE OPERATOR;
- q FLUX RATE;
- K CONDUCTANCE TERM;
- C CAPACITANCE TERM
- ϕ POTENTIAL TERM;
- Q SOURCE OR SINK TERM; AND
- T TIME.

Coupled Processes

$$q_I = - \Sigma K_{IJ} \nabla \phi_J$$

$$C_I = F(\phi_J)$$

$$K_I = F(\phi_J)$$

$$\nabla \cdot [\Sigma (K_{IJ}(\phi_K) \nabla \phi_J)]_I = C_I(\phi_K) \, \partial\phi_I / \partial T + Q_I$$

PARAMETERS:

K HYDRAULIC CONDUCTIVITY, M/S

T FRACTURE TRANSMISSIVITY, M^2/S

H_M PRESSURE HEAD AT MATRIX WETTING FRONT, M

H_F PRESSURE HEAD AT FRACTURE WETTING FRONT, M

B FRACTURE APERTURE, M

θ MATRIX VOLUMETRIC POROSITY, DIMENSIONLESS

α MATRIX SORPTIVITY, $M/S^{1/2}$

β FRACTURE SORPTIVITY, $M/S^{1/2}$

$$\alpha = [2K(H_I - H_M)/\theta]^{1/2} \qquad \beta = [2T(H_O - H_F)/B]^{1/2}$$

INPUTS AND OUTPUTS

Q_M FLUX INTO MATRIX, M/S

Q_F FLUX INTO FRACTURE, M^2/S

STATE VARIABLES

H_I HEAD IN FRACTURE DRIVING MATRIX FLOW, M

H_O HEAD AT THE FRACTURE ENTRANCE, M

EXAMPLES OF COUPLED EFFECTS

- ROCK DEFORMATION BY HEATING:
  - o AFFECTS FRACTURE APERTURES
- OSMOTIC POTENTIAL:
  - o FLUID PRESSURE IS AFFECTED BY SOLUTE CONCENTRATION
- CHEMICAL SOLUBILITY AFFECTED BY HEAT
  - o CHEMICAL PRECIPITATION AND DISSOLUTION CHANGES
- HYDROTHERMAL EFFECTS:
  - o FLUID DENSITY AND VISCOSITY EFFECTS
- CHEMICAL-MECHANICAL EFFECTS:
  - o FRACTURE HEALING AFFECTS ROCK DEFORMATION

UNANTICIPATED EVENTS

- EVEN WITH A COMPLETE DESCRIPTION OF A SYSTEM, VARIOUS COMBINATIONS OF CONDITIONS CAN RESULT IN UNIQUE AND UNIMAGINED EVENTS.

- IT IS NOT THAT WE BELIEVE THEM TO BE UNLIKELY, RATHER, WE HAVE NOT IMAGINED THEIR EXISTENCE. RISK ASSESSMENTS, THEREFORE, INHERENTLY UNDERESTIMATE THE TOTAL RISK.

- EXTENSIVE TESTING WILL PROVIDE THE OPPORTUNITY TO EXPERIENCE UNUSUAL EVENTS.

NONISOTHERMAL HYDROLOGIC TRANSPORT STUDY

PHASE 1	PERFORM SIMULATIONS USING EXISTING CHARACTERIZATION DATA FROM THE NEARBY APACHE LEAP INJECTION SITE FOR THE PURPOSE OF OBTAINING A PRELIMINARY EXPERIMENTAL DESIGN. USE EXISTING DATA SETS TO GUIDE DESIGN PROCESSES. EVALUATE ALTERNATE CHARACTERIZATION AND MONITORING TECHNIQUES AT THE INJECTION SITE. IDENTIFY TECHNOLOGICAL CONSTRAINTS AND RESEARCH NEEDS. ISSUES SUCH AS MONITORING BOREHOLE LOCATIONS, ORIENTATIONS AND DRILLING METHODS WILL BE RESOLVED DURING THIS PHASE.

PHASE 2	INSTALL MONITORING BOREHOLES AND FURTHER CHARACTERIZE THE HEATER SITE USING IN SITU AND LABORATORY CORE MEASUREMENTS. OBTAIN IN SITU CONDITIONS FOR WATER CONTENTS AND TEMPERATURES. SELECT AND CALIBRATE MEASUREMENT DEVICES.

PHASE 3	REFINE THE EXPERIMENTAL DESIGN BASED ON DATA COLLECTED IN PHASE 2 AND ADDITIONAL SIMULATION STUDIES. USING THIS INFORMATION, INSTALL BASELINE SENSORS FOR REAL-TIME MECHANICAL, PRESSURE AND TEMPERATURE CHANGES. ALSO, INSTALL ADDITIONAL BOREHOLES IF NEEDED.

PHASE 4	COLLECT BASELINE DATA FROM SENSORS INSTALLED IN PHASE 3, AND CONTINUE MONITORING WATER CONTENTS WHICH WERE STARTED IN PHASE 2. USE DATA SETS IN CONJUNCTION WITH COMPUTER MODELING ACTIVITIES TO REFINE HEATING SCHEDULE.

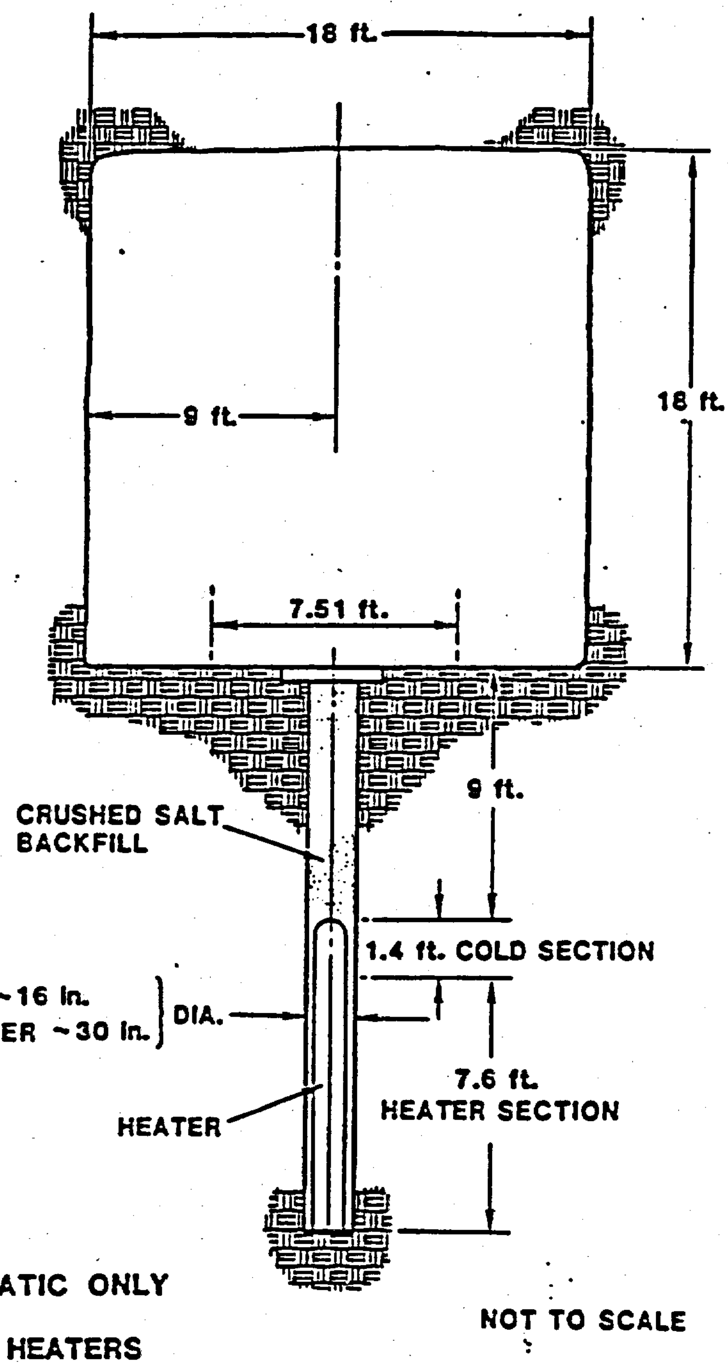
PHASE 5	PERFORM PRELIMINARY HEATER TEST AND MEASURE RESPONSES. CONCURRENTLY AND INDEPENDENTLY SIMULATE RESPONSES USING BASELINE AND CHARACTERIZATION DATA, AS WELL AS OBSERVED INITIAL AND BOUNDARY CONDITIONS.

PHASE 6	COMPARE EXPERIMENTAL AND SIMULATION RESULTS. DETERMINE WHETHER THE OBSERVED RESPONSE LIES WITHIN FORECASTED CONFIDENCE INTERVALS. OBTAIN AND TEST CORE SAMPLES TO CONFIRM FINAL CONCLUSIONS. REPEAT TESTS INCORPORATING INCREASED POWER AND COMPLEXITY.

PREVIOUS THERMAL STUDIES

- UNIVERSITY OF ARIZONA (TUFF)
  - o ROAD TUNNEL T - H
  - o LABORATORY CORES T - H - S
- CNWRA (GLASS BEADS)
  - o LABORATORY BOXES T - H - S
- G-TUNNEL (TUFF)
  - o HEATER TESTS T - H - S
- NTS CLIMAX MINE (GRANITE)
  - o AGED REACTOR WASTE T - M
- WIPP SITE (SALT)
  - o 18-W/M^2 MOCKUP T - M
  - o WASTE PACKAGE T - M - C

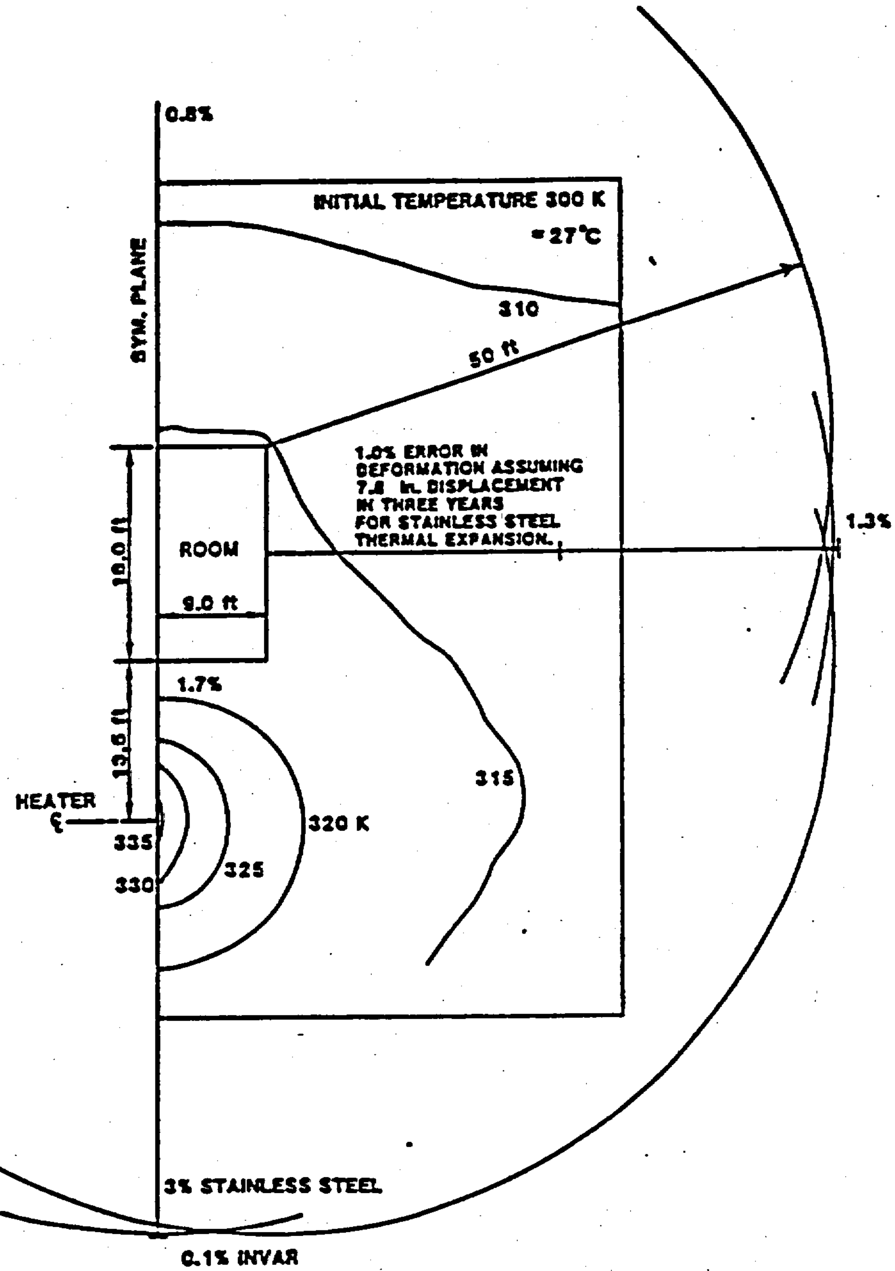
T: THERMAL
H: HYDROLOGIC
S: SOLUTE TRACER
M: MECHANICAL
C: CHEMICAL (CORROSION)



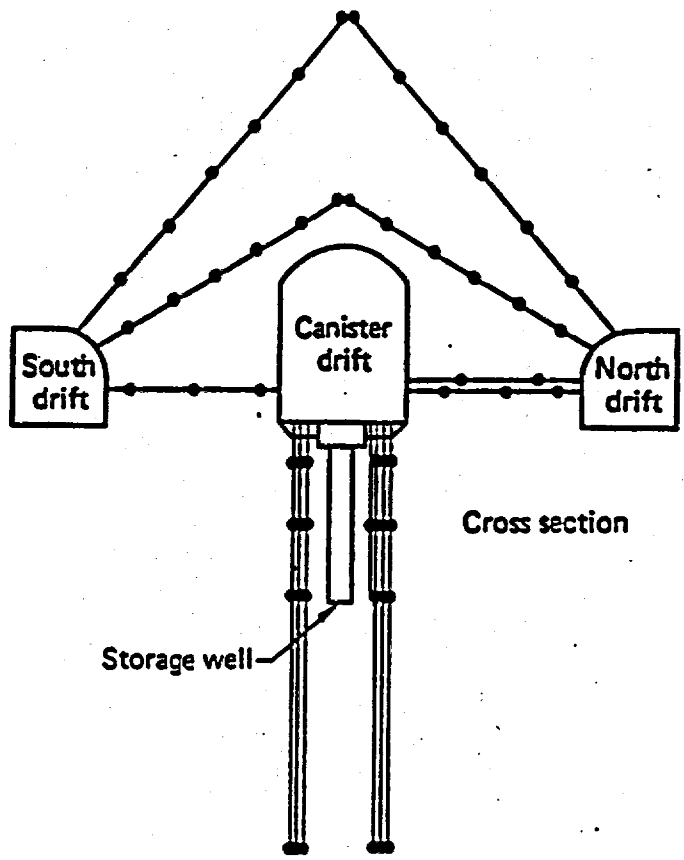
NOTE:

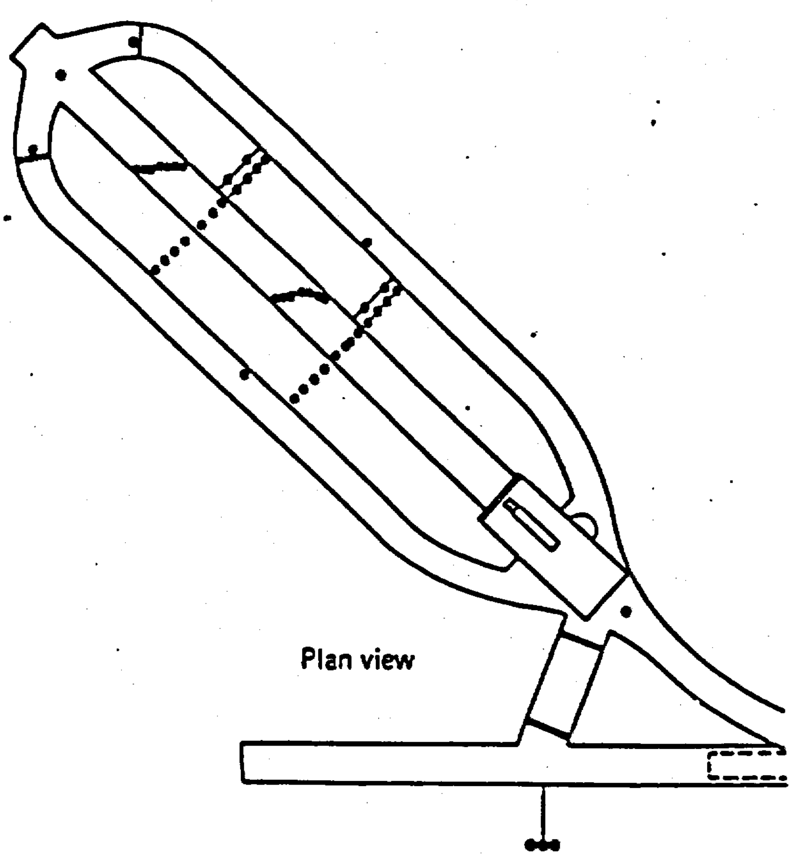
SCHEMATIC ONLY

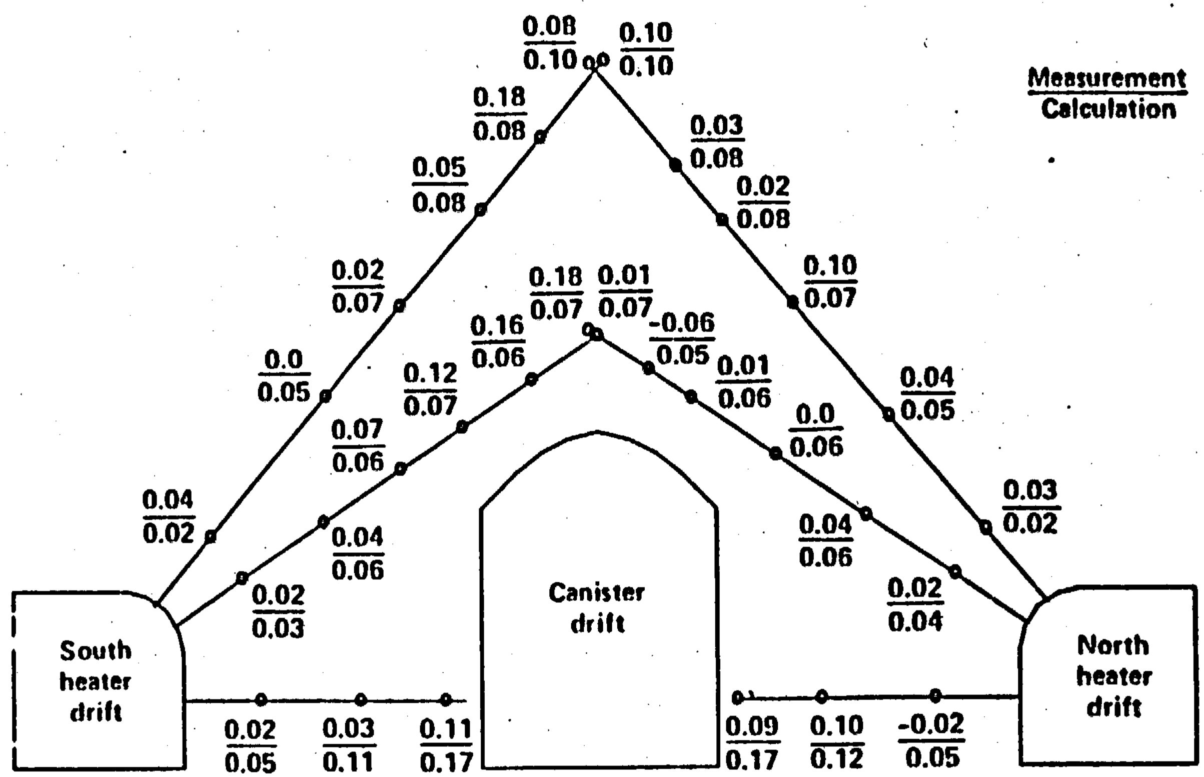
GUARD HEATERS CENTERED IN ROOM, BUT CANISTER HEATERS IN DOUBLE ROW



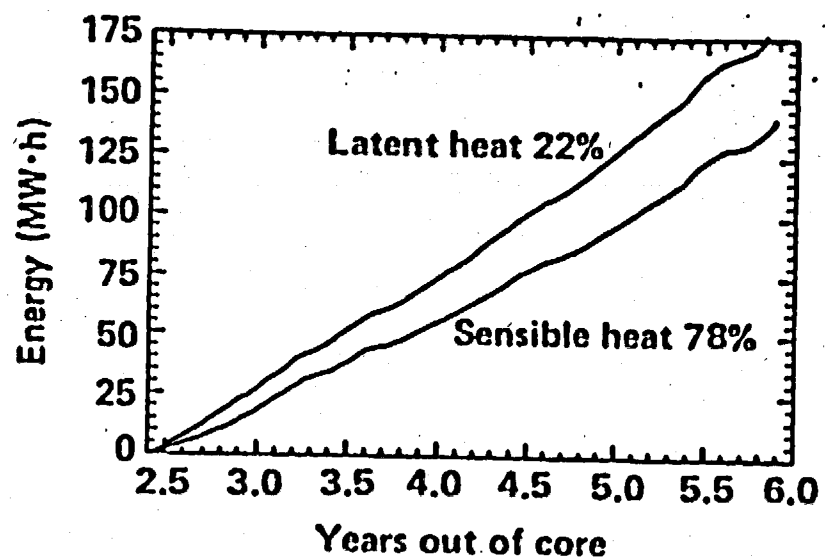
0.8%
INITIAL TEMPERATURE 300 K
≈27°C
SYM. PLANE
310
50 ft
1.0% ERROR IN
DEFORMATION ASSUMING
7.8 in. DISPLACEMENT
IN THREE YEARS
FOR STAINLESS STEEL
THERMAL EXPANSION.
1.3%
ROOM
10.0 ft
9.0 ft
1.7%
13.5 ft
HEATER
℄
335
330
325
320 K
315
3% STAINLESS STEEL
0.1% INVAR

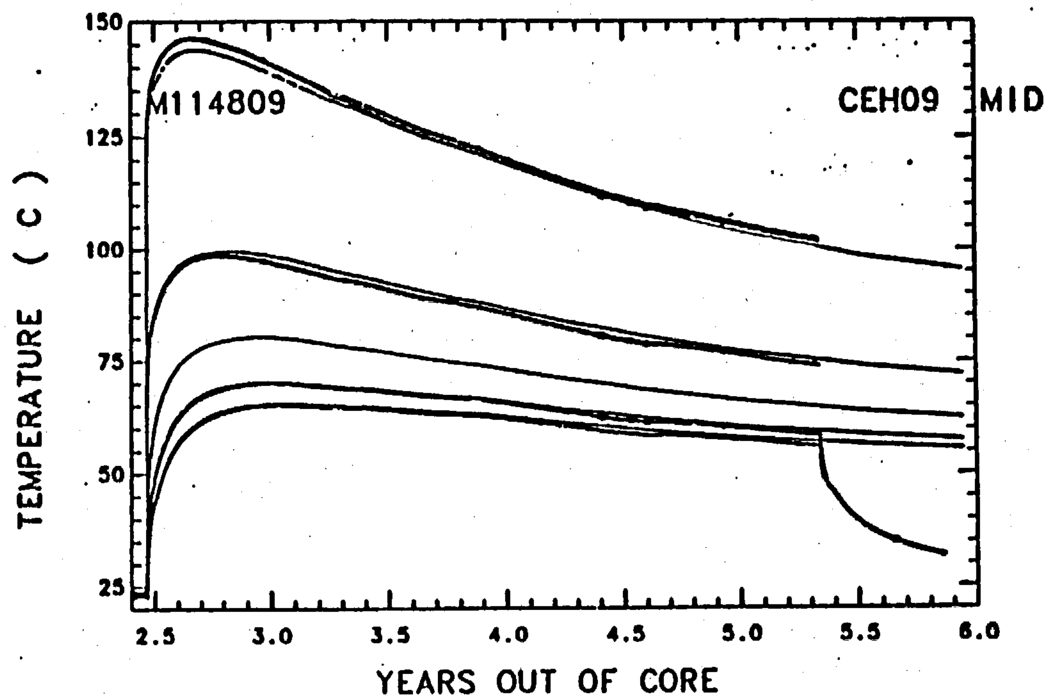

Canister drift
South drift
North drift
Cross section
Storage well


Plan view

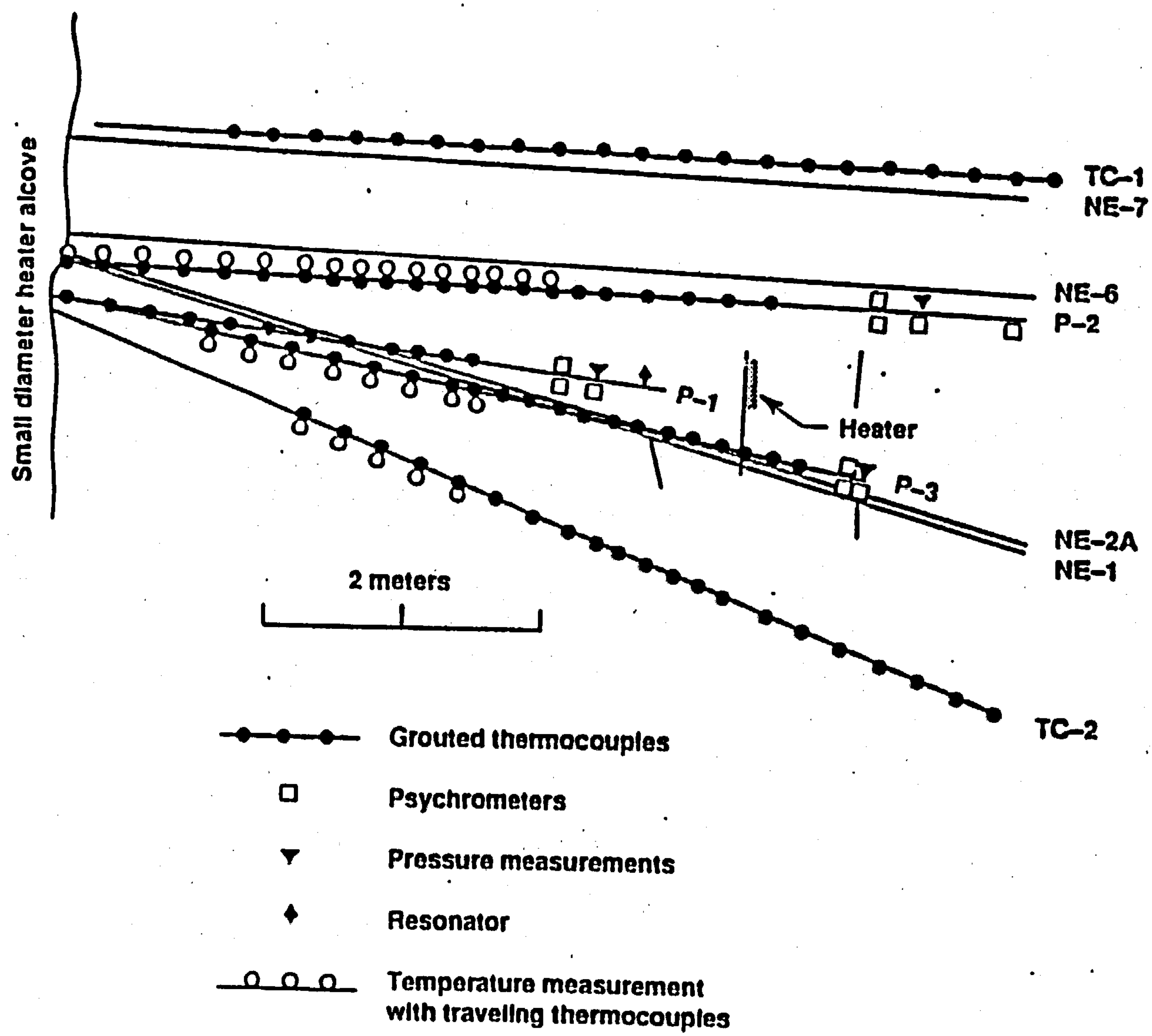


Measurement
Calculation
0.08
0.10
0.10
0.10
0.18
0.08
0.03
0.08
0.05
0.08
0.02
0.08
0.02
0.07
0.10
0.07
0.18
0.07
0.01
0.07
-0.06
0.05
0.16
0.06
0.01
0.06
0.0
0.05
0.12
0.07
0.04
0.05
0.0
0.06
0.07
0.06
0.03
0.02
0.04
0.02
0.04
0.06
0.04
0.06
0.02
0.03
0.02
0.04
Canister
drift
South
heater
drift
North
heater
drift
0.02
0.05
0.03
0.11
0.11
0.17
0.09
0.17
0.10
0.12
-0.02
0.05

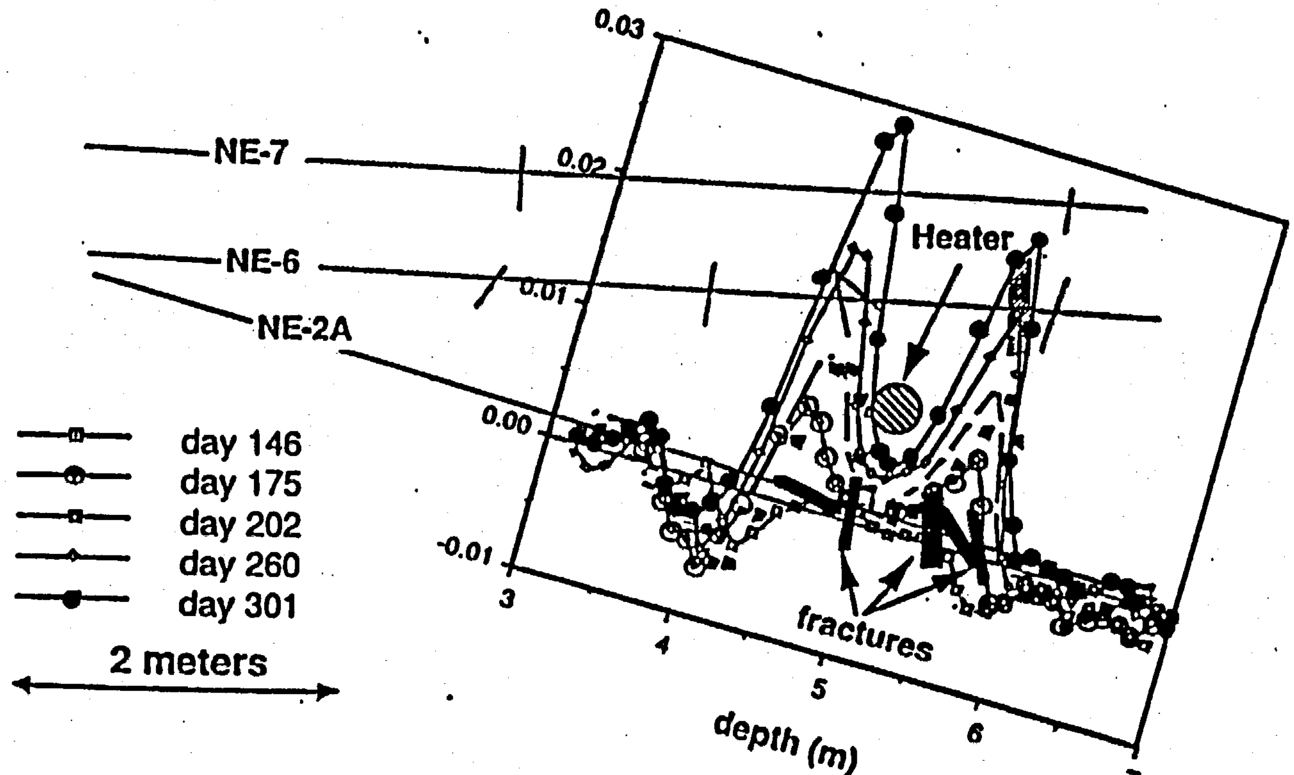

175
150
125
100
75
50
25
0
Energy (MW•h)
Latent heat 22%
Sensible heat 78%
2.5
3.0
3.5
4.0
4.5
5.0
5.5
6.0
Years out of core



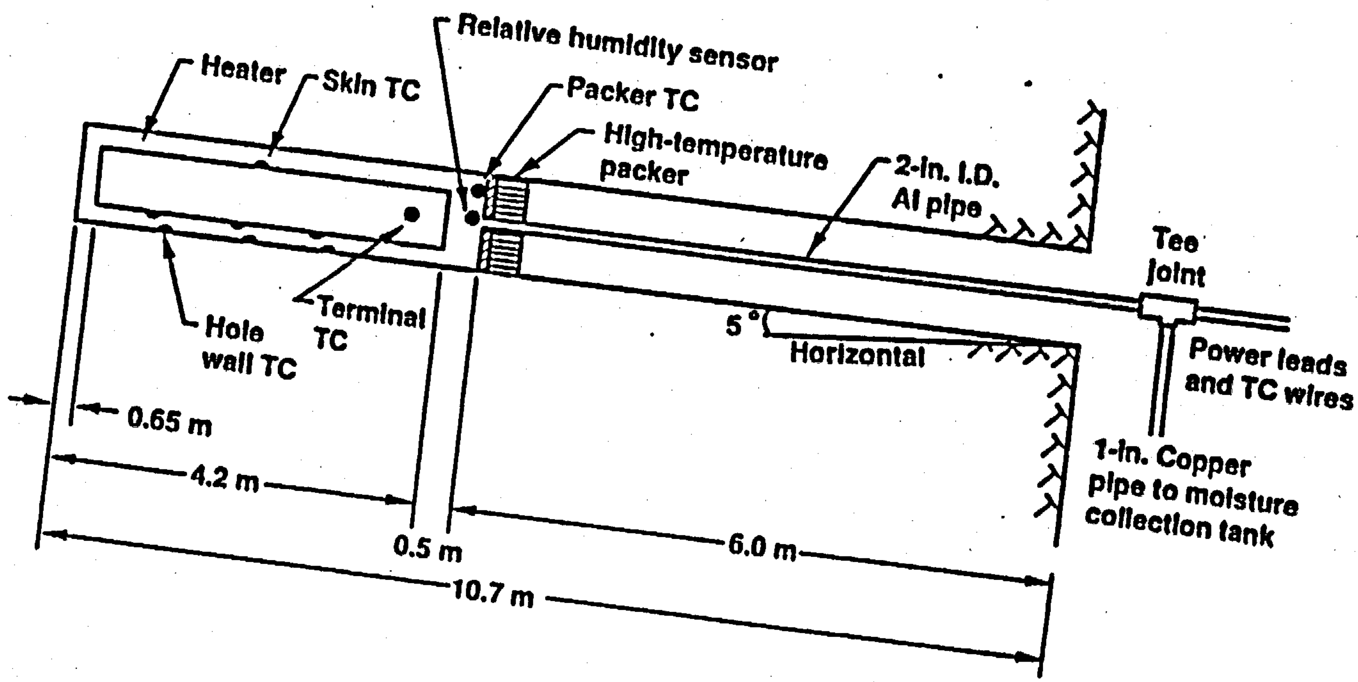
M114809
CEH09 MID
TEMPERATURE (C)
150
125
100
75
50
25
2.5
3.0
3.5
4.0
4.5
5.0
5.5
6.0
YEARS OUT OF CORE



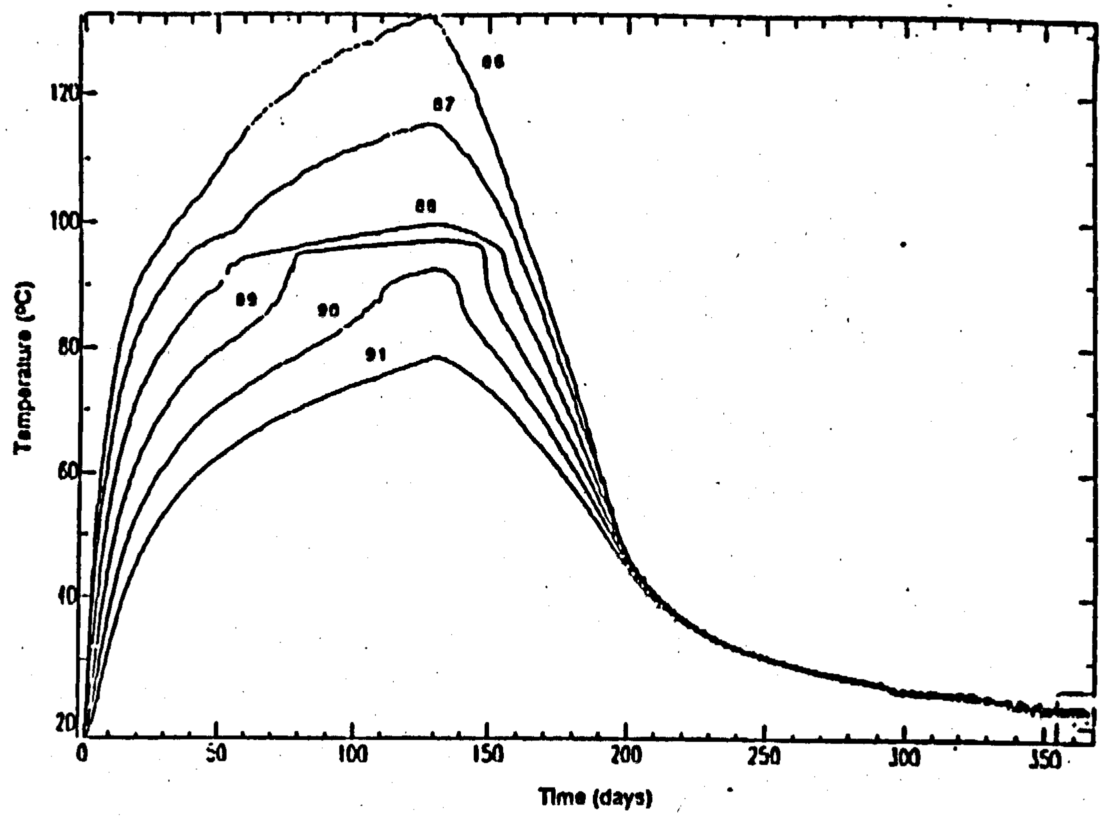
Small diameter heater alcove
TC-1
NE-7
NE-6
P-2
P-1
Heater
P-3
NE-2A
NE-1
TC-2
2 meters
Grouted thermocouples
Psychrometers
Pressure measurements
Resonator
Temperature measurement
with traveling thermocouples



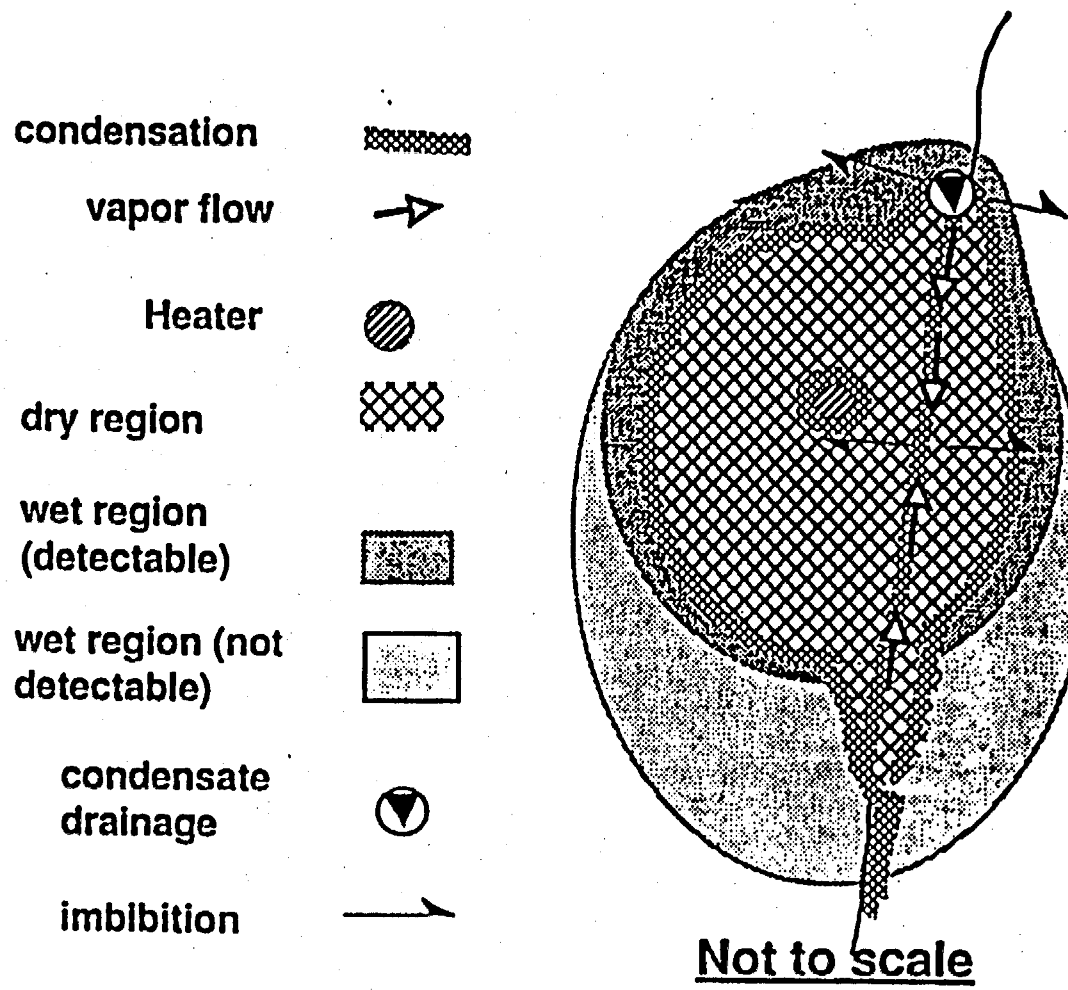
NE-7
NE-6
NE-2A
Heater
fractures
day 146
day 175
day 202
day 260
day 301
2 meters
0.03
0.02
0.01
0.00
-0.01
3
4
5
6
7
depth (m)



Relative humidity sensor
Heater
Skin TC
Packer TC
High-temperature packer
2-in. I.D. Al pipe
Tee joint
Hole wall TC
Terminal TC
5°
Horizontal
Power leads and TC wires
1-in. Copper pipe to moisture collection tank
0.65 m
4.2 m
0.5 m
6.0 m
10.7 m



Temperature (°C)
Time (days)
86
87
88
89
90
91
20
40
60
80
100
120
0
50
100
150
200
250
300
350


condensation
vapor flow
Heater
dry region
wet region
(detectable)
wet region (not
detectable)
condensate
drainage
imbibition
Not to scale

AVAILABLE LABORATORY DATA

- ISOTHERMAL CORE EXPERIMENTS

  PHYSICAL, HYDRAULIC, PNEUMATIC, THERMAL AND ELECTRICAL CONDUCTIVITY PROPERTIES

- ISOTHERMAL BLOCK EXPERIMENTS

  FRACTURE FLOW AS FUNCTION OF MATRIC POTENTIAL

  FRACTURE-MATRIX INTERACTIONS

  CHEMICAL TRANSPORT THROUGH FRACTURE

  FRACTURE SURFACE ROUGHNESS AND APERTURE PROFILES

- NONISOTHERMAL CORE EXPERIMENTS

  SOLUTE AND LIQUID TRANSPORT IN CORE DUE TO THERMAL GRADIENT.

Available Field Data

- Thermal

  Annual heat cycle and geothermal gradient.

- Pneumatic

  Borehole flow rates showing barometric, topographic, gas composition and wind direction effects.

- Hydrologic

  Rock water content measured using neutron probe. Surface infiltration from watershed studies. Mine inflow rates resulting from rainfall-runoff.

- Structural

  Fracture orientations and extents.

- Chemical

  Sulfate distribution near fracture. Stable isotope distributions near fracture. Fracture filling mineralogy.

- Electrical

  Resistivity surveys of proposed heater site.

Numerical Studies

- Near field nonisothermal conditions

  Cullinan and Shaikh

- Unsaturated fracture flow

  Rasmussen

BASELINE MONITORING

- TEMPERATURES

  THERMISTORS
  THERMOCOUPLES

- WATER CONTENT

  NEUTRON PROBE
  RESISTIVITY
  TDR

- MATRIC POTENTIAL

  PSYCHROMETERS
  TENSIOMETERS

- ROCK/FRACTURE DEFORMATION

  STRAIN/DISPLACEMENT GAGES

- AIR PRESSURE

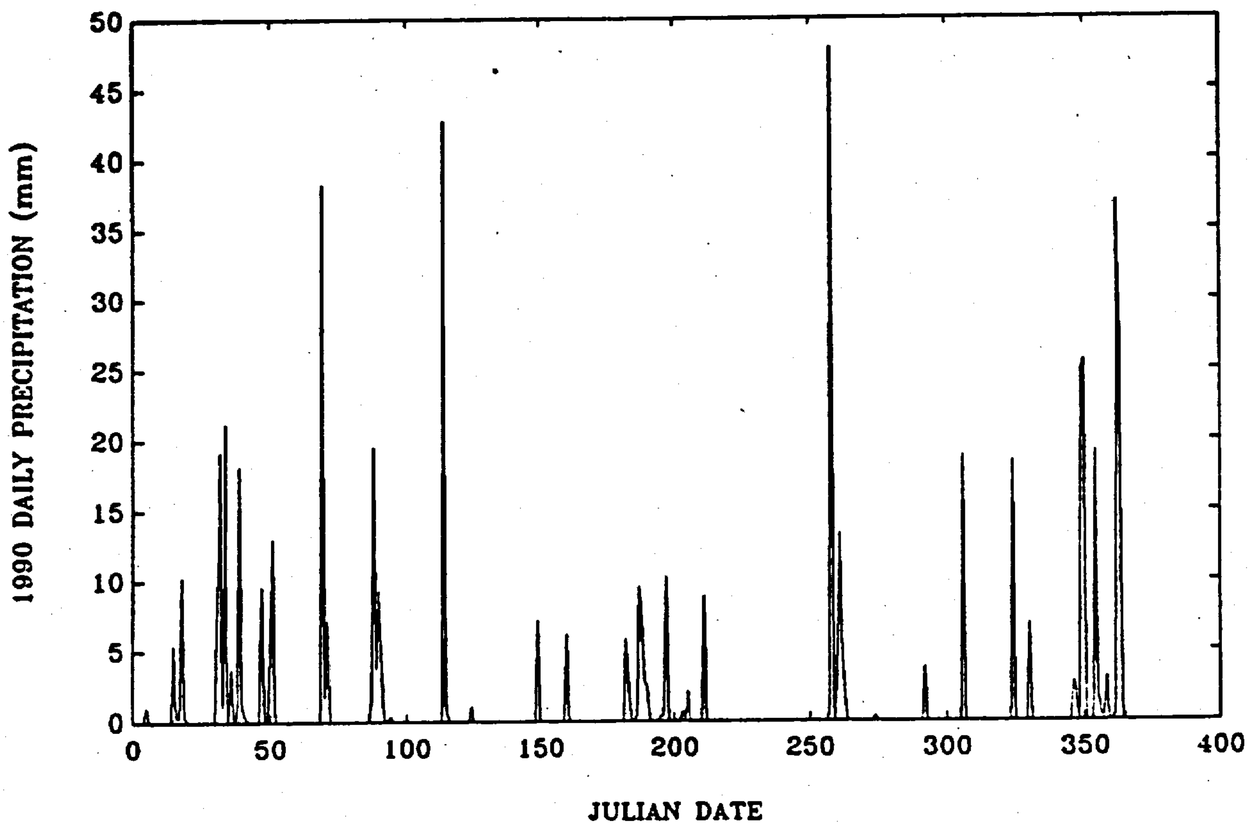
  TRANSDUCERS

- GAS COMPOSITION

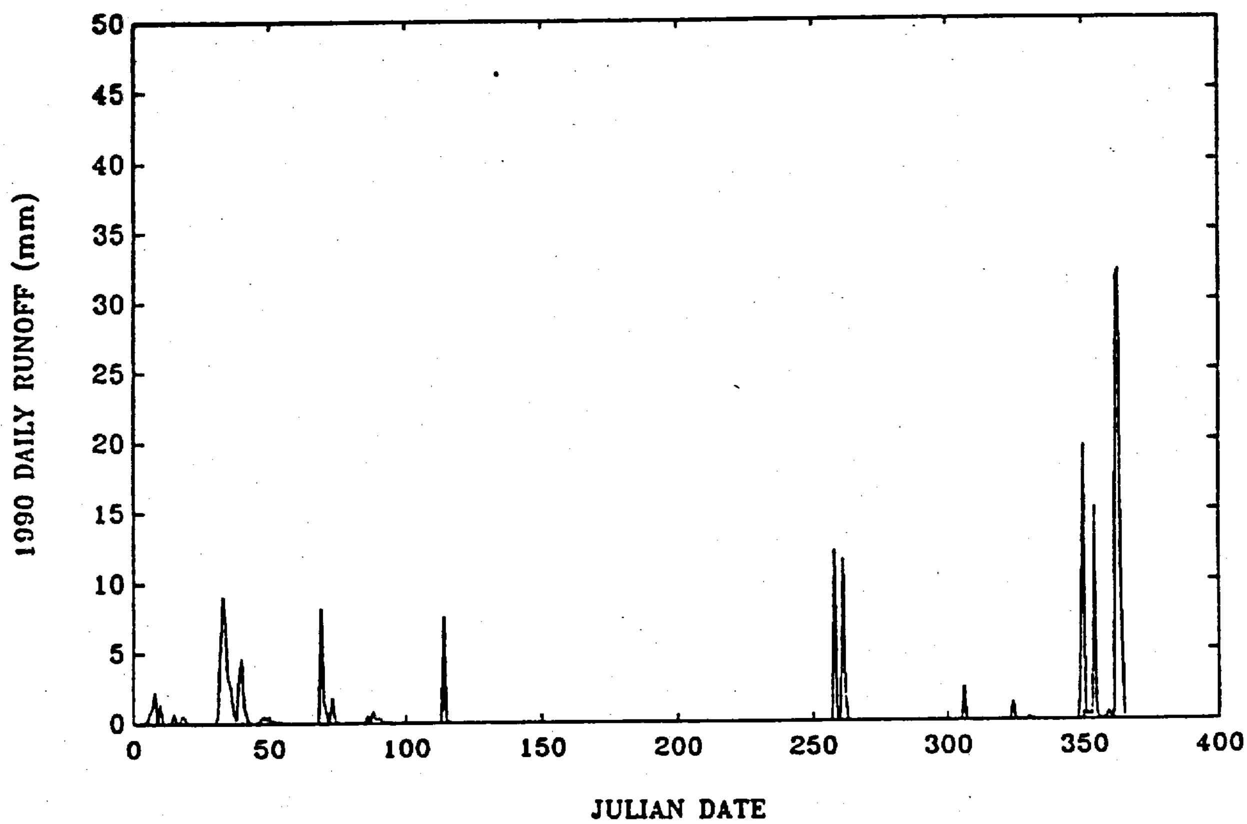
  OXYGEN, HUMIDITY SENSORS

- WATER COMPOSITION

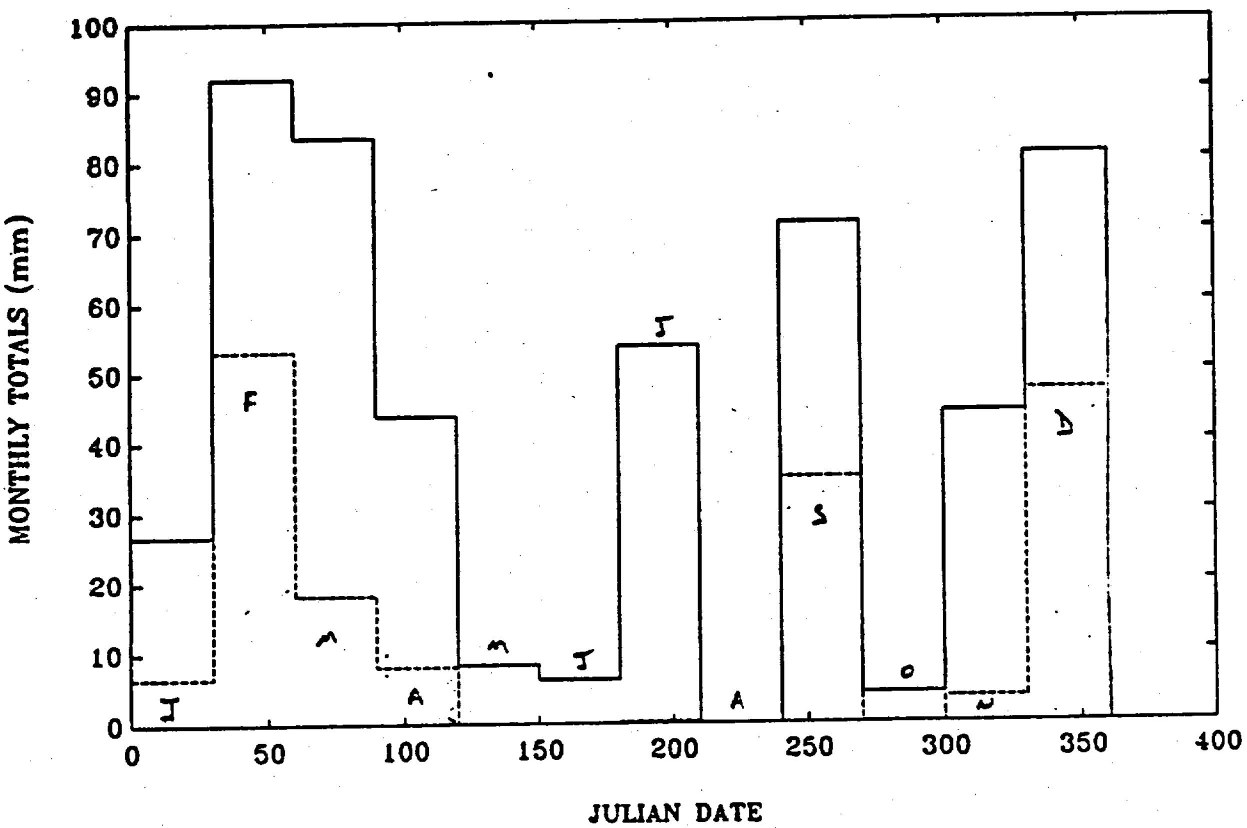
  RESISTIVITY
  SUCTION LYSIMETERS



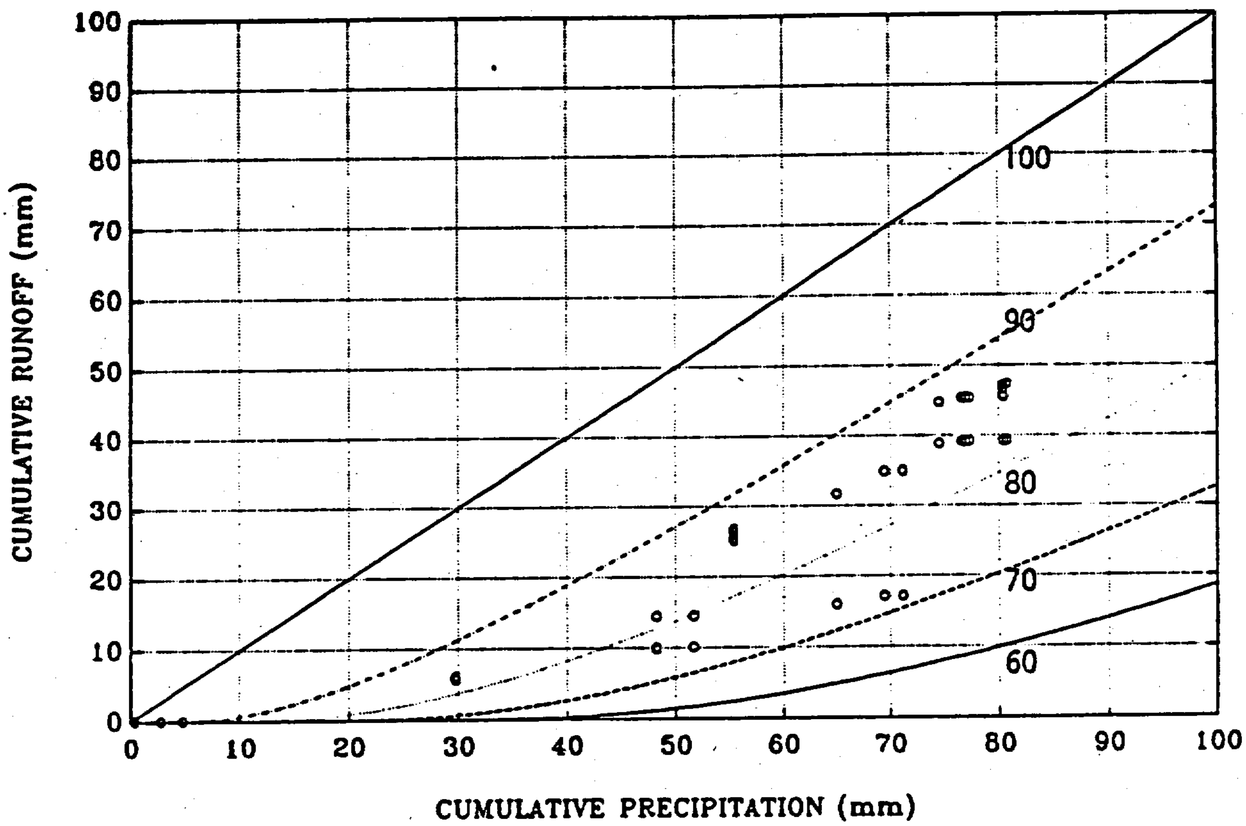
1990 DAILY PRECIPITATION (mm)
50
45
40
35
30
25
20
15
10
5
0
0
50
100
150
200
250
300
350
400
JULIAN DATE

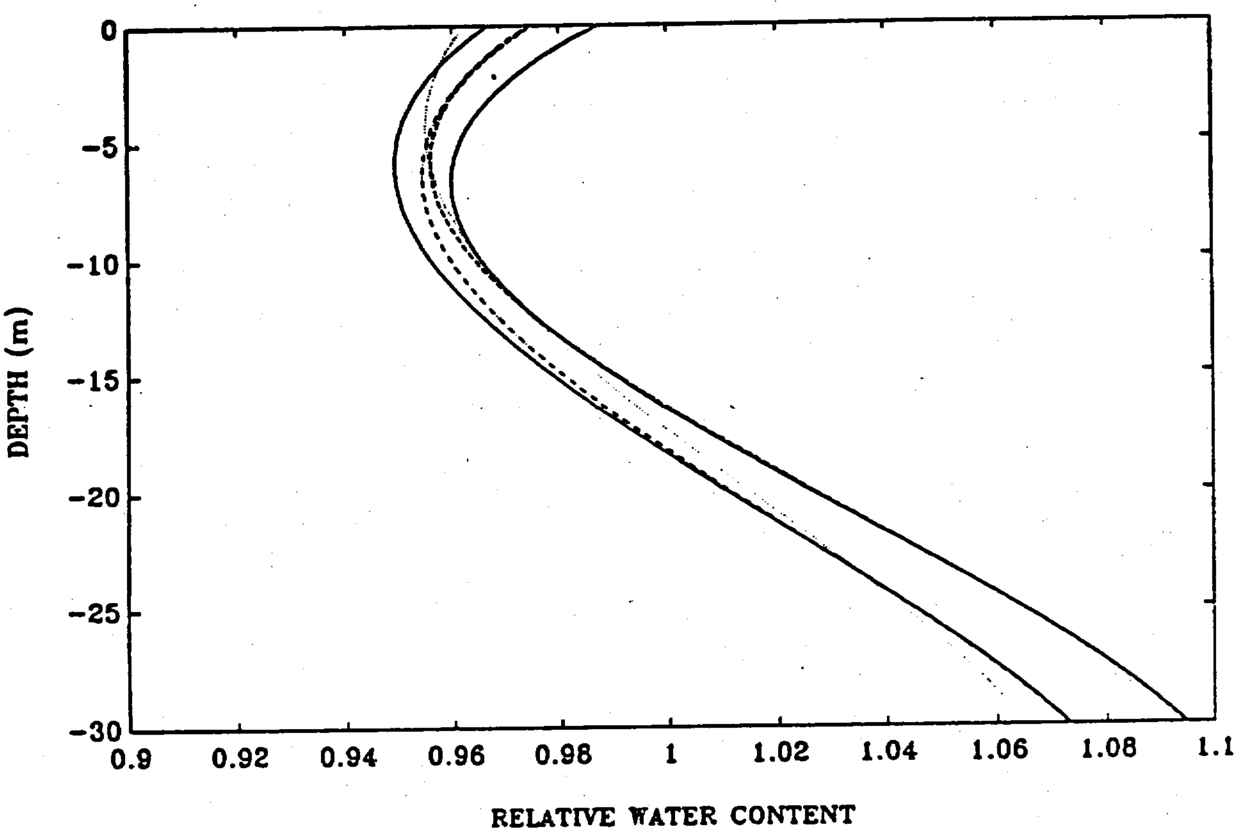


1990 DAILY RUNOFF (mm)
50
45
40
35
30
25
20
15
10
5
0
0
50
100
150
200
250
300
350
400
JULIAN DATE



MONTHLY TOTALS (mm)
100
90
80
70
60
50
40
30
20
10
0
0
50
100
150
200
250
300
350
400
JULIAN DATE
J
F
M
A
M
J
J
A
S
O
N
D


CUMULATIVE RUNOFF (mm)
CUMULATIVE PRECIPITATION (mm)
100
90
80
70
60
0
10
20
30
40
50
60
70
80
90
100



1990

May 3
May 30
July 9
Aug 1
Sept 2

MONITORING TECHNOLOGY

- HIGH ACCURACY, RELIABILITY, RESOLUTION
  - o TEMPERATURE
  - o GAS PRESSURE
  - o GAS COMPOSITION
- HIGH ACCURACY, RELIABILITY / LOW RESOLUTION
  - o NEUTRON COUNTS
  - o MECHANICAL DISPLACEMENTS
  - o RESISTIVITY
- LOW ACCURACY, RELIABILITY, RESOLUTION
  - o LIQUID WATER POTENTIAL
  - o WATER CHEMISTRY
  - o WATER PERMEABILITY

Examples of Technological Constraints

- Monitoring of fluid chemical composition in unsaturated rock
- Monitoring of fracture water content and permeability
- Monitoring of matric potential between 60 and 1000 kPa (0.6 and 10 bars).
- Emplacement of monitoring devices without affecting environment or other monitoring devices.
- Ability to simultaneously simulate coupled thermal, liquid, vapor, tracer, geochemical, and geomechanical processes under unsaturated conditions in heterogeneous materials in three dimensions.

SITE CHARACTERIZATION ACTIVITIES

CORE (UNFRACTURED)

EXISTING DATA SETS PLUS ADDITIONAL CORE ANALYSES

SMALL BLOCK (FRACTURED)

UNCOUPLED THERMAL, MECHANICAL, FLUID, AND GEOCHEMICAL ANALYSES

LARGE BLOCK (UNFRACTURED)

COUPLED ANALYSES IN HOMOGENEOUS MATERIAL

LARGE BLOCK (FRACTURED)

COUPLED ANALYSES IN HETEROGENEOUS MATERIAL

FIELD HEATER TESTS

SINGLE LARGE BLOCK, APPROX 3 X 3 X 3 M, WITH NO OBSERVABLE FRACTURES. FOLLOWED BY TEST IN FRACTURED BLOCK, APPROX 5 X 5 X 5 M, WITH A SINGLE VERTICAL OR HORIZONTAL FRACTURE.

MONITOR RESPONSE TO [VERTICAL LINE SOURCE, SINGLE POINT SOURCE, OR DUAL POINT/LINE SOURCES AT DIFFERENT TEMPERATURES], INCLUDING: TEMPERATURES, WATER CONTENTS, MATRIC POTENTIALS, GAS COMPOSITION AND PRESSURE, AND MECHANICAL, RESISTIVITY, AND WATER CHEMISTRY CHANGES.

OPTIMAL SAMPLING DENSITIES AND LOCATIONS, HEATER STRENGTH AND DURATION, PREVIOUSLY DETERMINED USING CALIBRATED COMPUTER MODEL.

AFTERWARDS, REMOVE BLOCKS AND EXAMINE FOR TRACER MOVEMENTS, MINERALOGIC AND GEOCHEMICAL CHANGES.

SUMMARY OF REVIEWER COMMENTS

- OBJECTIVES:

RELATE HOW THIS PROGRAM INTERFACES WITH THE VADOSE ZONE PROGRAM, ESPECIALLY WITH REGARD TO CRITICAL PATHS OF POTENTIAL RELEASE.

THE INITIALLY PROPOSED OBJECTIVES ARE TOO BROAD.

THE EFFORT SHOULD FOCUS ON TRANSPORT MECHANISMS ALONE.

EMPHASIZE THE NEED TO VALIDATE MECHANISMS, RATHER THAN COMPUTER MODELS.

THE OBJECTIVES SHOULD FOCUS ON WHETHER ALL RELEVANT PROCESSES HAVE BEEN INCORPORATED INTO MODELS IN AN APPROPRIATE MANNER.

AN IMPORTANT SUB-OBJECTIVE SHOULD BE THE REVIEW AND CHARACTERIZATION OF EXISTING DATA. THE DATA SHOULD BE USED TO IDENTIFY ADDITIONAL FIELD RESEARCH NEEDS.

A SUB-OBJECTIVE SHOULD BE THE INTEGRATION OF MANY TECHNICAL DISCIPLINES INTO A SINGLE EXPERIMENTAL UNDERTAKING.

ANOTHER SUB-OBJECTIVE SHOULD INCLUDE VALIDATION OF THERMOMECHANICAL MODELING.

- GENERAL:

IT IS ADVISED THAT SPECIFIC PROCESSES BE IDENTIFIED, AND THE COMPUTER MODELS USED TO IMPLEMENT THESE MODELS BE DETERMINED. THE CALIBRATION AND VALIDATION DATA SETS NEEDED BY MODELS SHOULD THEN BE SPECIFIED.

IDENTIFY HYPOTHESES PRIOR TO CONDUCTING TEST, INCLUDING HEAT PIPE SIGNATURES, CAPILLARY FLOW IN FRACTURES, AND WETTING DIFFUSIVITY.

THE RESOLUTION AND ACCURACY OF DATA NEEDED FOR MODEL VALIDATION NEEDS TO BE DETERMINED. CRITERIA FOR DETERMINING ACCEPTANCE WILL HAVE TO BE IDENTIFIED, ALONG WITH PARAMETER SENSITIVITIES AND DATA UNCERTAINTIES.

CAN A THERMAL RESPONSE (BOTH UNDER BASELINE AND TEST CONDITIONS) BE USED AS AN INDICATOR OF FLUID MOVEMENT (AIR AND WATER) BY CONVECTION OR CONDUCTION. IF SO, CAN OTHER INDEPENDENT TESTS USING TRACERS OR WATER CONTENT VARIATION BE USED TO VALIDATE THE THERMAL RESPONSE.

IT IS IMPORTANT TO INCORPORATE FIELD CHARACTERIZATION DATA PRIOR TO DETERMINING THE OPTIMAL HEATER EXPERIMENTS. THE SPATIAL AND TEMPORAL RESOLUTION OF DATA NEEDS TO BE RESOLVED PRIOR TO CONDUCTING THE CHARACTERIZATION TESTS.

THE APPROPRIATENESS AND JUSTIFICATION FOR THE PLANNED SCALE SHOULD BE ADDRESSED.

THE SCALE OF THE EXPERIMENT MAY BE TOO SMALL FOR INFERRING THE EFFECTS AT REPOSITORY SCALES.

PRIOR TO CONDUCTING THE FIELD-SCALE TEST, A LARGE BLOCK EXPERIMENT SHOULD BE PERFORMED WITH BETTER CONTROL ON BOUNDARY CONDITIONS AND MASS BALANCES.

- SOURCE:

THE HEATER SHOULD BE PLACED INSIDE OF A CANISTER.

COUPONS MADE OF VARIOUS PROPOSED CANISTER MATERIALS SHOULD BE PLACED NEAR THE HEATER SOURCE TO EXAMINE CORROSION PROCESSES.

INITIALLY THE HEAT SOURCE WILL BEHAVE AS CYLINDER, LATER AS A SPHERICAL SOURCE.

IT MAY BE BETTER TO USE A POINT SOURCE OF HEAT, RATHER THAN A CYLINDRICAL SOURCE.

A VERTICAL HEATER TEST IS RECOMMENDED.

THE TESTS SHOULD INCREMENTALLY INCREASE SYSTEM COMPLEXITY.

ONE OF THE HEATER TESTS SHOULD BE LOCATED IN UNFRACTURED ROCK. ANOTHER TEST SHOULD INTERSECT A SINGLE FRACTURE.

THE HEATER TEST SHOULD BEGIN WITH LOW TEMPERATURES (< 100°C) TO MINIMIZE COUPLED EFFECTS.

- MATERIAL PROPERTIES:

IT IS IMPORTANT TO MEASURE THE ROCK WETTING AND DRYING DIFFUSIVITY, BOTH BEFORE AND AFTER THE TEST.

THE WETTING DIFFUSIVITY OF A FRACTURE IS AN IMPORTANT CHARACTERIZATION PARAMETER.

THE INTERACTION BETWEEN TEMPERATURE AND ROCK DEFORMATION (WITH AND WITHOUT FRACTURES) NEEDS TO BE DETERMINED. ROCK JOINT MECHANICAL PROPERTIES ALSO NEED TO BE DETERMINED.

SIGNIFICANT CHARACTERIZATION WILL BE REQUIRED.

SUBSTANTIAL GEOLOGIC VARIABILITY IN HYDRAULIC CONDUCTIVITY IS OBSERVED, AND CORRELATION WITH OTHER PROPERTIES IS MINIMAL.

AN UNDERSTANDING OF THE RESIDUAL STRESS PATTERNS WILL BE REQUIRED.

THE PROCESS AND IMPORTANCE OF HEAT TRANSPORT ACROSS FRACTURES NEEDS TO BE DETERMINED.

- BOUNDARY CONDITIONS:

BOUNDARY CONDITIONS FOR THE EXPERIMENT NEED TO BE FIRMLY ESTABLISHED.

FOR UNDERGROUND WORK, A BULKHEAD SHOULD BE INSTALLED NEAR THE HEATER SITE TO PREVENT VENTILATION.

FOR NEAR-SURFACE WORK, A COVER SHOULD BE PLACED OVER THE SITE.

RATHER THAN TRY TO CONTROL BOUNDARY CONDITIONS, IT WOULD BE BETTER TO MONITOR THEM. AN IMPORTANT MODEL COMPARISON MAY BE OBTAINED BY EXAMINING RESPONSE TO TRANSIENT BOUNDARY CONDITIONS.

DUE TO DIFFERENCES IN IN-SITU STRESS REGIMES, IT WOULD BE BETTER TO WORK UNDERGROUND.

PROCESSES RELEVANT AT THE REPOSITORY MAY NOT EXIST AT A NEAR-SURFACE FIELD LOCATION. ALSO, NEAR-SURFACE PROCESSES MAY OVERWHELM IMPORTANT REPOSITORY DEPTH PROCESSES.

AIR FLOW NEAR THE SURFACE SEEMS TO BE NEGLECTED.

- RESPONSE:

AN EQUIVALENT POROUS MEDIA MODEL IS INSUFFICIENT TO MODEL THE THERMAL RESPONSE. IT WOULD BE BETTER TO USE A DISCRETE FRACTURE NETWORK MODEL.

DURING THE COOLING PHASE, THE PRIMARY REWETTING WILL BE DUE TO VAPOR CONDENSATION RATHER THAN LIQUID IMBIBITION.

THE TEST SHOULD INCORPORATE MULTIPLE IONS, WITH DIFFERENT CHARGES IN VARIOUS CATION EXCHANGE ENVIRONMENTS.

PLACE MONITORING EQUIPMENT PERPENDICULAR TO THE DISCRETE FRACTURE INTERSECTED BY THE HEATER.

TRACERS SHOULD BE EMPLOYED TO DETERMINE FLUID MOVEMENT. THE ROCK SHOULD BE SAMPLED AFTER THE TEST TO DETERMINE THE FINAL DISPOSITION OF THE TRACERS, WHERE THE FRACTURES ARE LOCATED, AND THE CONFIGURATION OF THE FLOW FIELD.

EMPHASIS SHOULD BE PLACED ON THE NEED TO EVALUATE NEW TECHNOLOGY.

IT MAY BE BETTER TO PUT SENSORS IN SEALED AND INSULATED BOREHOLES RATHER THAN IN PACKED-OFF INTERVALS.

DETERMINE WHAT MEASUREMENTS CAN BE USED TO MONITOR WATER AND AIR MASS BALANCES DURING THE TEST.

IF MODELED AND MEASURED RESPONSES DIFFER, THEN FOLLOWUP TESTS SHOULD BE PERFORMED.

GEOCHEMICAL ASPECTS OF A LIQUID-VAPOR ENVIRONMENT WILL COMPLICATE THE NEAR-FIELD MEASUREMENTS. IN LIGHT OF THIS, HYDROLOGIC EFFECTS IN THE FAR FIELD MAY BE EASIER TO MEASURE.

ELECTRICAL RESISTIVITY CAN BE USED TO MONITOR WATER CHEMISTRY.

MINERAL CHEMISTRY SHOULD BE EMPLOYED TO EXAMINE ROCK CHANGES BEFORE AND AFTER THE TEST.

CALCITE PRECIPITATION DUE TO HEATING OF WATER AND VOLATILIZATION OF DISSOLVED CO_2 MAY OCCUR.

MONITORING OF AIR PRESSURE NEAR THE HEATER SHOULD BE INCLUDED.

SOLUTE TRANSPORT MONITORING SUFFERS FROM TECHNOLOGICAL CONSTRAINTS. RESEARCH NEEDS INCLUDE THE DETERMINATION OF APPROPRIATE TRACERS AND MONITORING TECHNIQUES.

CAN NEUTRON LOGGING ADEQUATELY MONITOR WATER CONTENTS.

DISPLACEMENT AND STRAIN MONITORING SHOULD BE INCLUDED IN THE TEST PROGRAM.

BUILD REDUNDANCY INTO ENTIRE SYSTEM. DO NOT RELY UPON INDIVIDUAL SENSORS OR HEATER ELEMENTS.

THERMISTORS ARE MORE USEFUL FOR MEASURING TEMPERATURES THAN THERMOCOUPLES.

MAINTAINING THERMAL CONTACT BETWEEN THERMISTORS AND ROCK WALL IS EXTREMELY CRITICAL. THE EXISTENCE OF AIR GAPS CAN SUBSTANTIALL AFFECT TEMPERATURES, AS WELL AS INDUCE A HEAT PIPE EFFECT.

COMMENTS AND SUGGESTIONS CONCERNING JANUARY 1990 VERSION OF "EXPERIMENTAL PLAN, Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site" by T.C. Rasmussen and D.D. Evans

by Shlomo P. Neuman

February 12, 1991

My comments and suggestions concern a field-scale heater experiment proposed in the above document by Drs. Todd C. Rasmussen and Daniel D. Evans. The purpose of this experiment is (p. 1) to "confirm[] important aspects of coupled heat, liquid, gas and solute transport" and/or (p. 34) "to evaluate and confirm existing conceptual and computer simulation models related to fluid flow in a nonisothermal environment." Its specific objectives are are (p. 5)

(1) "To further assess appropriate methods, techniques and technologies for characterizing and monitoring water flow, transport and thermomechanical changes in unsaturated fractured rock, including interaction between the rock matrix and the fracture system;"
(2) "To examine relevant hydraulic, pneumatic, thermal, solute transport and thermomechanical processes and relevant parameters, singularly and coupled, at field scales of from one to thirty meters;"
(3) "To evaluate the thermomechanical effects of a heat source on fracture and matrix pneumatic and hydraulic transport properties;"
(4) "To generate data sets for complex, coupled flow and transport systems for use in the validation of unsaturated flow and transport models;"
(5) "To assess various modeling approaches and their limitations in predicting flow and transport through nonisothermal, unsaturated, fractured rock."

"The experiment is designed" (p. 6) "to evaluate the relative significance associated with excluding various processes, and to evaluate scale dependent procedures used to estimate material properties." "Modeling of the experimental results is an important validation aspect, and is the principal reason for conducting the test[]."

These statements of purpose, objective, design goal and principal motivation behind the heater experiment are broad and ambitious. They are also quite general and therefore open to multiple (perhaps conflicting) interpretations.

Our current understanding of flow and transport in unsaturated fractured tuffs, under field conditions, is limited and speculative,, hence subject to uncertainty and controversy. There are certain "aspects of coupled heat, liquid, gas and solute transport" about which we know more and other aspects about which we know less or very little. Aspects about which we know quite a lot include heat conduction in both saturated and unsaturated porous rock matrix, liquid flow in saturated and unsaturated porous rock matrix under isothermal conditions, and conservative isothermal solute transport at tracer concentrations in saturated porous media. Aspects about which we know less include heat conduction in saturated and unsaturated fractured rocks, liquid flow through saturated fractured rocks under isothermal conditions, gas flow through liquid-free fractured rocks under similar conditions, and isothermal gas flow through partially saturated porous media. Aspects about which we know still less include heat transport by conduction and convection in heterogeneous saturated porous media and in homogeneous partially saturated porous media, liquid flow through partially saturated fractured porous rocks under isothermal conditions, isothermal gas flow through partially saturated fractured porous rocks, and conservative isothermal solute transport at tracer concentrations in saturated fractured rocks. Aspects about which we know relatively little include heat conduction and convection coupled with multiphase fluid flow in nonuniform porous media, nonisothermal liquid flow through partially saturated fractured porous rocks, nonisothermal gas flow through partially saturated fractured porous rocks, and conservative solute transport at tracer concentrations in unsaturated fractured rocks. Aspects about which we know extremely little, if anything, include heat conduction and convection coupled with multiphase fluid flow in fractured porous media, multiphase water transport through nonuniform porous and/or fractured rocks at temperatures above the boiling point, gas flow under similar conditions, and solute transport under all but the conditions listed in connection with this phenomenon earlier.

The proposed heater experiment involves many aspects of flow and transport about which relatively little or nothing is presently known. With respect to these aspects, the ability of the experiment to "confirm" and "validate" must be quite limited: one can only confirm or validate what one knows or can reasonably hypothesize, then observe and measure. To date, little has been done to validate our ability (or lack of it) to measure and describe (not to speak of predicting) the space-time distribution of water in fractured tuffs under static isothermal conditions (not to mention isothermal dynamic flow regimes) at a space-time resolution that could clearly distinguish between the roles of matrix blocks and fractures (not to think of finer channels) in storing and conducting fluids on field scales of up to thirty meters, as stipulated in objective (2). Our present understanding of isothermal flow and transport in unsaturated fractured tuffs, and our current ability to define and measure relevant rock properties (saturated hydraulic conductivity as affected by fractures on various spatial scales, unsaturated hydraulic conductivity as affected further by water content, air permeability as affected by fractures and water saturation, relationship between air and water permeabilities, total and kinematic porosities as affected by fractures, spatial variability and scale-dependence of these parameters) and state variables (humidity, water content, hydrostatic and capillary pressure, osmotic pressure, their distribution within and between fractures) are, at best, rudimentary. Such conceptual understanding, and ability to define and measure, are better developed and validated under the relatively simple conditions of isothermal flow. The proposed heater experiment would create much more complex conditions and would therefore make it much more difficult to relate effects to causes in an unambiguous manner than a well thought out and executed isothermal experiment.

The complex conditions created by a heater test render it less than ideal for the investigation of issues which arise under isothermal conditions, such as (p. 6) "the ability of various modeling strategies (including the equivalent porous medium representation of fracture flow, as opposed to discrete fracture network flow representation within a porous matrix) ... to accurately represent fluid flow and solute transport processes in unsaturated fractured rock;" another modeling strategy to consider is one that represents the geologic medium by means of one or several overlapping stochastic continua in areas where detailed information about discrete features (fractures, fracture zones, channels) is lacking, while embedding discrete features into the stochastic model where such have been clearly delineated (geologically and geophysically) and adequately characterized (hydraulically). To model flow and transport under nonisothermal conditions, the same distinctions between modeling strategies must be made. As the conditions created by a heater experiment are relatively complex, its stated purpose (p. 34) "to evaluate and confirm existing conceptual and computer simulation models related to fluid flow in a nonisothermal environment" may be difficult to achieve. For the same reason, it may be difficult to design the experiment so as (p. 6) "to evaluate the relative significance associated with excluding various processes, and to evaluate scale dependent procedures used to estimate material properties." It may be equally difficult to achieve significant progress toward some of the specific objectives listed above under (1) - (6), especially those relating to process definition and model validation.

Given that model validation is considered to be the principal reason for conducting a heater experiment, what can we expect to be validated by such an experiment? How can such a validation be accomplished? The authors suggest (p. 6) that "ideally, the tests will be designed using calibrated models, with calibration data sets having been obtained from laboratory and field tests. Once calibrated, the model will be validated by proposing a perturbation of the system not related to calibration experiments for the purpose of evaluating the assumptions inherent in the model." This contrasts with a later statement (p. 36) according to which "no calibration against the experimetal results is expected." Rather, "model accuracy" will be determined by (p. 35) "simulat[ing] responses using baseline and characterization data, as well as observed initial and boundary conditions," and then "compar[ing] experimental and simulation results [to] determine whether the observed response lies within forecasted confidence intervals." I believe that a combination of these two approaches is needed. However, I propose that one first discuss in some detail what model(s) will be calibrated, against what data, at what stage of the experiment, and how. I also propose that this be followed by a relatively detailed discussion of what aspect(s) of the model(s) or underlying theory (theories) will be validated, against what data, at what stage of the experiment, and how. Only on the basis of such discussions may it become possible to evaluate the potential benefits of the proposed experiment.

Considering the relatively low level of knowledge and technology we currently possess concerning isothermal flow and transport in unsaturated fractured tuffs, and the complex conditions created by a heater test, it is in my view important that we clearly separate what we apparently know and probably can do, from what we admittedly don't know and may be unable to do, well in advance of planning the heater experimet. On one hand, our present ability to accurately measure, correctly interpret, and interpolate spatially, quantities other than temperature at depth during the proposed experiment are limited. On the other hand, groundwater models under much simpler conditions than those created by the heater test generally require a large amount of data, and a good amount of calibration effort, before they can meaningfully reproduce observed behavior. Even after considerable calibration against a relative wealth of space-time data, such models often perform poorly as predictors outside the calibration range and must be periodically updated (recalibrated) to remain current (one well-known updating technique being the standard or extended Kalman filter). It is not entirely clear from the proposal how a heater experiment on the proposed scale of up to thirty meters could, under the given budget and time constraints, generate data of sufficient quantity and quality to allow resolving validation issues of the kind discussed earlier and highlighted further on pp. 6 and 7 of the document. I would feel more comfortable about the heater experiment if its purpose and objectives were more focused, particularly on issues which cannot be addressed by means of simpler (isothermal) experiments. I propose that such a focus might be provided by attempting to answer the following questions, more or less in the order they are listed below, which also represents their proposed order of priority:

1. How accurately, and with what space-time resolution, can one measure and describe the distribution of temperatures on a scale of up to 30 meters in unsaturated fractured tuff at the Apache Leap site? What accuracy and resolution are required to detect anomalies due to convective air and vapor currents through major fractures and/or channels, or due to other causes? Might it be better to emplace temperature sensors permanently in sealed and insulated boreholes rather than in packed-off intervals as presently envisioned?

2. Can one reproduce the space-time temperature distribution on a scale of up to 30 meters, observed prior to activating the heater (*i.e.*, under ambient or pre-test conditions), by means of a simple model which accounts only for heat conduction and treats the rock as a uniform continuum, the properties of which (heat conductivity

and capacity as functions of water content) are based on laboratory measurements on cores and blocks? Existing data (p. 28) suggest that heat conductivity and capacity vary much less than hydraulic conductivity (and porosity?). Can this reproduction be improved through a calibration process in which one varies the distribution of water contents in three-dimensional space? Can the calibrated water contents be verified (validated) independently by means of neutron probes, tomography, or other measurements? If the latter is not possible, can such measurements be used to help improve the calibration? If not, can the calibration be improved by embedding observed fractures and/or channels in the above model of a continuum? Can the calibrated properties of these discrete fractures and/or channels be verified (validated) independently through direct hydraulic and/or pneumatic tests? Can a satisfactory calibration be achieved, primarily to anomalies if such have been detected, without modifying the model to allow for convection through known (or unknown) fractures and/or channels? If unknown fractures and/or channels are required for calibration, can their existence and properties be validated by independent measurements, and how?

3. What can be learned from observations of temperature and water content under pre-test conditions, with or without the above model, about ambient heat fluxes through the rock on a scale of up to 30 meters? Can one confidently determine an average heat flux vector, and its variation with time? Can one determine the manner in which the directions and magnitudes of local heat fluxes vary from point to point in three-dimensional space and in time? Can any such determinations be validated through independent measurements, and how? Can one detect and quantify anomalous heat fluxes due to convective air and/or vapor flow through known (or unknown) fractures and/or channels, or due to other causes? Can such fluid flow be detected and quantified independently of the temperature data to help confirm (validate) its existence and extent?

4. Assuming that the pre-test distribution of temperatures, water contents and heat fluxes can be adequately characterized and modeled, what modifications in the model are required so that it can be used to design the heater experiment proper? Considering that at the Apache Leap injection site (p. 27) "field estimates of saturated hydraulic conductivity" show a (p.28) "variation of ... approximately 700% and ranges of five orders of magnitude," how meaningful is it to borrow such data for the design of the heater experiment (p. 2), and how can such a transfer of data from one site to another be accomplished? Could these data be averaged in some meaningful way to yield effective continuum values and if so how? If not, would a stochastic representation and model be required to design the heater experiment? In the latter case, are the available data amenable to a statistical analysis that might yield a meaningful stochastic representation? Would it not be more appropriate to design the heater experiment on the basis of hydraulic and/or pneumatic data collected at the site of the proposed experiment? If so, by means of what method(s), on what spatial scale(s), with what degree of spatial resolution, and how accurately can and need one measure and describe the distribution of saturated hydraulic conductivities, unsaturated hydraulic conductivities as functions of water content and temperature, capillary pressure as a function of water content and temperature, and/or air permeabilities as functions of water content and temperature, on a scale of up to 30 meters at the proposed heater test site? With how much confidence can and need one attribute such measurements to this or that fracture, channel or matrix block? How should the answer(s) to these questions be reflected in the selection and/or development of conceptual, mathematical and numerical model(s) for experimental design?

5. After selecting or developing an appropriate nonisothermal multiphase flow model for experimetal design, and collecting appropriate hydraulic and/or pneumatic data to serve as input parameters for this model, what considerations and criteria should one adopt in designing the experiment? What, according to this model when applied under a range of plausible input parameters, is needed to detect the onset of convection in response to various heater power outputs and schedules? Is it possible to design a (single or multiple) heater power output and schedule (constant or transient) such that the onset of convection can be detected solely by observing temperature variations with time at various points in three-dimensional space? As temperatures are the easiest state variable to measure, this seems highly desirable. Should it also be possible to deduce, from temperature measurements alone, what is the spatial extent of convective currents generated in response to heating? What effect do individual fractures and channels have on convection (do they create identifiable temperature anomalies)? Could such deductions be verified (validated) independently by means of tracers (in liquid and/or gas phases) and/or measurements of water content variations in time? Is a scale of up to 30 meters appropriate for the heater test? How can one determine the average heat flux, Rayleigh number (ratio between buoyancy and viscous retardation forces) and Nusselt number (ratio between total and conductive heat flows) on such a scale?

6. If temperature measurements alone are not enough to detect the onset of convection, its spatial extent, and the role of individual fractures or channels, what additional measurements are required (water contents? liquid pressures? vapor pressures? pneumatic pressures?), at what spatial scales and resolution, with what accuracy? Are such measurements feasible considering available technology, time and budget? Are (p. 17) "tomographic estimates of water content ... , especially near the heater borehole due to the failure of neutron probes at elevated temperatures," presently possible at the required accuracy and resolution? What measurement are required to not only ascertain the presence of convective cells, determine their spatial extent, evaluate the role of individual fractures and channels, and estimate the associated heat fluxes, but also to differentiate between the roles of liquid water and vapor, evaporation and condensation, transport of sensible and latent heat, in the convection process? In other words, what measurements are required to validate the heat pipe effect under field conditions, and to quantify this effect? What measurements, if any, could potentially isolate the role of individual fractures in the generation of heat pipes (will there be more than one)? Are such measurements feasible?

7. What measurements, if any, may detect the effect of heat on the porosities and permeabilities of matrix blocks and fractures? What measurements, if any, may detect the effect of heat on unsaturated rock properties? Are such measurements feasible? To what extent and how should this effect be studied in the laboratory prior to the heater experiment?

8. What measurements, if any, can help in the determination of water and air mass balance during the heater experiment? Are such measurements feasible? Would it be desirable, and technically feasible, to fully or partially isolate the tested rock mass from its surrounding by means of insulation and grouting in order to have a better control on mass balance?

Some of these questions can be answered prior to doing any work at the heater site and I suggest that this constitute Phase 1 of the proposed work. Some questions cannot be answered prior to performing intensive site characterization of fractures, hydraulic and/or pneumatic property distributions, water contents and pressures; I propose that this constitute Phase 2a of the planned work. According to such a plan, Phase 2b would be conducted concurrently with Phase 2a, and would involve experimentation with various in-situ temperature measurement techniques, followed by the installation of a three-dimensional network of thermistors (or other temperature sensors) and the compilation of baseline data on ambient temperature distributions in space-time on the proposed scale of the heater experiment (this scale to be determined as part of Phase 2). Phase 3 would comprise an analysis of the characterization and temperature data collected during Phase 2, followed by an attempt to develop and calibrate a three-dimensional model of the ambient temperature and moisture conditions at the site. During Phase 4, this model would be modified (if necessary) to allow the simulation of conditions other than ambient, as expected during the heater experiment, and then used to design this experiment. In response to needs identified during Phases 3 and 4, Phase 5 would then follow with an expansion of the network of thermistors and additional site characterization. Under Phase 6, this additional characterization and temperature data would be used to recalibrated the model used for experimental design, and to modify the design if necessary. The recalibrated model would generate a prediction of system response to heating under the final design, over the entire projected life of the experiment, including a period of monitoring after the heaters are deactivated. The heater experiment itself would constitute Phase 7a. Phase 7b, initiated concurrently with Phase 7a but continuing beyond this latter phase, would be an analysis of the experimental results and a comparison with the predicted response from Phase 6.

MEMORANDUM FOR: John J. Linehan, Acting Director
Repository Licensing and Quality Assurance Project Directorate
Division of High-Level Waste Management

FROM: King Stablein, Senior Project Manager
Repository Licensing and Quality Assurance Project Directorate
Division of High-Level Waste Management

SUBJECT: FORTHCOMING PUBLIC MEETING ON APACHE LEAP TUFF SITE FIELD HEATER EXPERIMENT

DATE & TIME: JUNE 20, 1991
9:30 a.m. - 5:00 p.m.

LOCATION: ONE WHITE FLINT NORTH BLDG. ROOM 6-B-11
11555 Rockville Pike, Rockville, MD

PURPOSE: Dr. Rasmussen will present the experimental plan for conducting a field-scale (i.e., up to 10 m) heater experiment at the Apache Leap Tuff Site (ALTS) for investigating coupled hydrologic, thermal and mechanical processes in unsaturated fractured rock related to disposal of high-level waste (HLW) in an underground repository. He will discuss the study objectives and tasks to be pursued during the proposed experimental phases for the three-year project. Peer-review comments received on the draft experimental plan will be discussed and their disposition into the resulting revised plan (to be distributed at the meeting) will be discussed.

SPEAKER: Dr. Todd C. Rasmussen
Department of Hydrology and Water Resources
University of Arizona
Tucson, Arizona

(Original Signed by) Joseph J. Holonich
for
King Stablein, Senior Project Manager
Repository Licensing and Quality Assurance Project Directorate
Division of High-Level Waste Management

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TRIP REPORT, March 21-22, 1991

Thomas J. Nicholson
Division of Engineering
Office of Nuclear Regulatory Research
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

and

Todd C. Rasmussen
Department of Hydrology and Water Resources
College of Engineering and Mines
University of Arizona
Tucson, AZ 85721

Summary

The purpose of the trip was to present, and solicit comments related to, a Draft Apache Leap Tuff Site Heater Experimental Plan, prepared by Todd C. Rasmussen and Daniel D. Evans of the Department of Hydrology and Water Resources at the University of Arizona. Meetings were arranged ahead of time with various groups in the San Francisco Bay Area who have demonstrated interests in the conduct of the proposed heater experiment, as well as individuals who have participated in similar nonisothermal experiments in unsaturated fractured rock at G-Tunnel and the Climax Mine, both located at the Nevada Test Site. Included in the discussions were Lawrence Livermore National Laboratory personnel, Electric Power Research Institute personnel, Lawrence Berkeley Laboratory personnel, and J. Bredehoeft of the Water Resources Division of the U.S. Geological Survey. What follows is a summary of the discussions conducted during the course of the two day visit.

Thursday, March 21, 1991, Morning Meeting
Lawrence Livermore Natl Lab
Attending: Abelardo L. Ramirez
Tom Buschek
Dwayne Chestnut
Dale Wilder

This meeting was arranged to obtain information regarding field heater experiments conducted at G-Tunnel in welded tuff at Rainier Mesa, NTS, as well as a test conducted at the Climax Mine in unsaturated granite, also at NTS. A second objective was to solicit comments on the proposed ALTS heater experiment. The group attending from LLNL have considerable experimental and modeling experience resulting from their participation in the previous field heater experiments.

Comments on G-Tunnel Heater Experiments:

The G-tunnel experiment had a time line of two and a half years, with a budget of $1.5 million. The budget was mostly for LLNL labor, with only a small portion of the budget allocated for operations and capital. Most operations and capital were funded through a separate account.

<u>Characterization</u>

Prior to the heater tests, a series of characterization activities were performed. From fracture mapping observations, fractures were observed every foot to foot and a half. Single and cross-hole pneumatic tests were also performed. Laboratory estimates of flow and transport properties were not obtained from cores. In one of the preliminary tests, a blue tracer was injected with water. The water was tracked using inverse tomography. Subsequent corings of fractures demonstrated the presence of the tracer which matched the tomography results. Precise determination of borehole position was obtained using pretest borehole orientation surveys. For the tomography, borehole separation of approximately one meter was required, and maximum path lengths of one and a half meters were used. Scattering from boreholes and instrument packages was a significant problem associated with the EM tomography method.

CONCLUSION: It is important to obtain as much characterization data as possible at field and laboratory scales prior to conducting the test, as well as following the test in order to evaluate the impacts of the experiment on rock properties. Characterization should focus on rock matrix and fracture parameters including physical, hydraulic, pneumatic, thermal, and mechanical properties.

<u>Construction</u>

The heater borehole was 12" in diameter, and the canister was 8" in diameter. The heater borehole required seven weeks to drill the 30 foot. The EM equipment was less than 2 cm in diameter. To fill the empty volumes in the observation boreholes, grout was used to prevent an air-phase conduit. The grout was 30-40% sand in a lean cement ratio. A thick grout was used initially to prevent fracture penetration, allowed to set for 1 to 2 days, and then followed by the fluid grout. One problem was that the instrument tube tended to float in the grout to the top of the horizontal hole. (Perhaps spacers are needed around the instrument tube.) A closed cell foam - FIRESTOP - was used as a seal in the vicinity of the heater to prevent vapor transport. It was observed that it would be best to grout around displacement instruments with sponge rubber (Wes Patrick has more information on this method). An inflatable rubber heat resistant packer was placed at the end of the heater. After removal of the packer following the completion of the heater experiment, the rubber surface showed a fracture impression and geochemical precipitates. A restraint was placed over each borehole to prevent ejection of packers in the event of a blowout.

CONCLUSION: It is important to minimize voids in the heater and observation boreholes. This can be accomplished by using grout, closed cell foam, or using heat resistant packers.

Instrumentation

No geomechanical data were collected due to time and cost constraints. This aspect was proposed for a followup experiment. To measure temperatures, K-type thermocouples were used in G-Tunnel. They were obtained from Climax Experiment. Some thermocouples were mounted in a rock placed between the heater and the rock wall. Three were placed below, and one above the heater. Other thermocouples were placed in observation boreholes nearby.

A CPN neutron probe was used to measure porosity and water content. The probe was modified to include a gamma detector below the neutron probe, rather than around the probe. Also, to avoid failure in a high temperature environment, a thermistor was used to monitor probe temperature. When the probe temperature exceeded 70°C, the probe was removed and allowed to cool.

A resonance cavity was used to measure the partial pressure of water vapor. There was a problem with condensation in the cavity leading to equipment failure. A nonlinear relationship between resonant frequency and water content was observed. A patent is pending on a device which uses a solid plate under a tuff rock with a mesh above to measure the water content in a tuff rock. The microwave resonator was placed on the end of the heater canister along with the HUMICAP capacitance sensor, which is also used for humidity measurements. The capacitance sensor worked reasonably well, but the microwave resonator failed due to condensation within the resonance chamber.

CONCLUSION: Instrumentation must be used which is compatible with the severe operating conditions expected around the heater borehole. Extreme care must be taken in positioning and selecting the monitoring equipment. Additional information on fracture water content must be obtained using technology which is currently unavailable.

Observations

During the heater tests, greater drying and heating was observed above the borehole than below. The observed temperature difference between the top and the bottom of the heater canister was approximately 40°C, and may have been even greater. More temperature sensors will be required to examine canister temperature. The heat load also was observed to have an edge effect (the ends of the heater were cooler than the center), and the source appeared to go from a line source to a spherical source over time.

Matrix porosity was about thirteen percent, yet the change in water content was approximately 16 percent due to rock heating. The source of the extra three percent is still unknown. The effect of the drift boundary on water content, movement and temperatures may be important.

After the heater experiment, the pneumatic tests were repeated and showed higher permeabilities. Small changes in fracture aperture can result in large changes in permeability, so thermomechanical properties will be required to evaluate the correlation between fracture displacement and permeabilities.

Because the observation boreholes were emplaced along essentially horizontal planes, the spatial coverage was inadequate. Gravitational forces caused movements above and below the heater which could not be monitored.

The chemistry of collected water was not examined. No conclusions regarding geochemical processes can be made.

CONCLUSIONS: Inadequate monitoring of water contents, temperatures, water chemistry, matric potential, mechanical stresses, and the source term resulted in inconsistent and incomplete data. Emphasis should be placed on obtaining data which are as complete and reliable as possible. A major design issue is selecting the types and precision of data required.

Modeling

LLNL modeled the experimental results by assuming radial symmetry and ignored gravity, but the failure to incorporate gravitational forces severely limited their analysis. To evaluate alternate conceptual models, they compared a matrix only model with a single discrete fracture model, and with a equivalent continuum model. The impact on temperature using the various models was minimal, while water content profiles were substantially different between models.

They noted that while rapidly overdrying the system yields immediate results, subboiling conditions requires an extremely long observation period. There was also a question regarding drying rate as a function of the fracture density, and whether a simplified analytic model developed by LLNL might accurately predict the drying rate.

CONCLUSION: Computer modeling activities must incorporate gravitational effects. A simplified analytic model which incorporates fracture density may be appropriate for predicting water drying rates.

Comments on Climax Heater Experiment:

At the Climax Mine, Dale Wilder supervised much of the work, along with Jessie Yow and Wes Patrick (currently at CNWRA). Unlike the G-Tunnel experiments, the Climax experiment did not investigate hydrologic processes, but focused instead on thermomechanical impacts of a subsurface heat source in the unsaturated zone. From knowlege gained during the Climax experiment, it is advised that only limited effort be placed on measuring fracture apertures, and that it would be better to measure changes in rock mass volume and relative fracture motion. It was observed that shear displacement was important across fractures and shear zones, while normal displacements were generally recoverable and crushed rock zones displayed the greatest deviations from theory. A shortcoming of the Climax experiment was the failure to obtain motion measurements in all directions, as well as to record motion out to the undisturbed region. Also, it was observed that mechanical displacement had a significant impact on the hydrology in that free water was observed at a major fault and along associated fractures. Based on this observation it is recommended that mechanical and hydrologic measurements be obtained along fracture-borehole intersections.

To model geomechanical effects, the Adina-T code from MIT is the recommended structural code. (Butkovitch describes code for linear, thermal conditions without fractures.) Thermomechanical codes worked well at Climax for the rock matrix, but did not perform as well for fractures. It may be possible to attribute changes in bulk rock behavior entirely to fracture changes.

For nonisothermal conditions, thermocouples on displacement posts will be required to account for thermal expansion of reference rods. There will also be a problem with assuming perfect attachment of displacement instruments to rock. It would be best to use a J-Latch on a rod, but there is a question concerning creep. There should be mechanical backup in case of electrical failure. May want to investigate the use of a GOODMAN JACK.

CONCLUSION: Hydrologic processes may be extremely sensitive to mechanical changes. Monitoring of thermomechanical responses is critical if a complete understanding of hydrologic transport is to be obtained.

Comments on ALTS study:

- Pg. 5: Comment on modeling as a cylindrical source. Perhaps a model which initially characterizes the source as cylindrical followed later by a spherical source would be more appropriate.
- Pg. 6: The use of an EPM model will not be accurate. A discrete fracture model is recommended.
- Pg. 10: During the cooling phase, the primary rewetting mechanism will probably be due to vapor condensation rather than liquid imbibition.
- Pg. 16: Heater should be placed inside of a canister.
- Coupons made of various proposed canister materials should be placed on the canister surface to examine corrosion processes.
- Vertical heater tests are recommended for future study.

- It is important to measure the rock wetting and drying diffusivity, both before and after the test. The wetting diffusivity along a fracture is another important characterization parameter.
- If further work is to performed underground, a bulkhead in the drift near the heater site should be installed to prevent ventilation. This will more accurately simulate actual repository conditions.

Thursday, March 21, 1991, Afternoon Meeting
Electric Power Res. Inst.
Attending: Robert A. Shaw
R.F. (Bob) Williams

Comments on draft ALTS heater plan:

There was a concern with regard to how the proposed experiment fits into the whole vadose zone program, especially with regard to critical paths of potential release. The hydrology may not be the only concern, in that hydrology is only important with respect to mass transport. An additional concern was related to model validation, in that we may want to evaluate mechanisms, rather than models. Yet if a wrong model is employed, then the correct mechanism may be discounted.

A need was expressed to identify a narrow and limited set of objectives. The current objectives are too broad and the control may be insufficient to resolve the issues. The criteria for determining acceptance will have to be identified, along with parameter sensitivities and data uncertainties.

It will be important to include modelers to define the experiment for the purpose of examining separate effects, integrated effects, and transient effects for both lab and field scales. The entire effort should be focused on transport mechanisms. To that end the heater test should incorporate multiple ions, with charges of +1, +2, ..., +6, in various cation exchange environments to examine a potential chromatographic effect. The concept of satellite sites was mentioned for the ability to perform invasive (i.e., destructive) sampling. Each of the satellite sites would have independent timelines so that exhumation could occur at different times. It was recommended that we may want to talk to Neville Cook and John Kemeny regarding rock mechanical studies.

CONCLUSION: EPRI is extremely pleased with the proposed heater experiment and would like to be kept informed of the progress of the experiment. They will be providing comments to our experimental plan. Their major concern was the need to design the experiment prior to conducting it so that specific issues can be addressed and resolved.

Friday, March 22, 1991, Morning Meeting
Lawrence Berkeley Lab.
Attending: Karsten Pruess
Joe Wang
Yvonne Tsang
Larry Myer

Comments on ALTS study:

It was emphasized that the experimental design phase should be focused on identifying transport mechanisms and that it would be best to identify hypotheses to be tested prior to developing the experimental design. Potential hypotheses include heat pipe signatures, capillary activity in fractures, and wetting diffusivity. Their opinion was that it is not the purpose of this experiment to reproduce Yucca Mountain, but rather to study mechanisms of two-phase flow in unsaturated fractured rock. The proposed experiment should incrementally increase system complexity, from simple to more complicated conditions. To that end, the experiment should try various source strengths, e.g., if the source is too strong, it may kill the heat pipe effect. Various space-time scales should be used to allow a suite of responses to be observed. Finally, because of fracture variability, the experiment may have to be performed over many fractures.

With regard to modeling of simultaneous heat-moisture transport, it would be best to calculate a conductive heat field, then impose a two phase field and see if thermal field is affected. If it is, then correct the thermal field. Should start with a pure conduction background case, and then introduce fractures and see what fracture properties are required to affect the base case. Because there may be many types of canister environments, it may be best if the experiment would test conditions which are most adverse with respect to waste containment. As part of the experimental design, the computer model should be used to evaluate the impacts of surface infiltration above the heater, and then see how the infiltrate and heater interact. Also, it may be interesting to include this in the field experiment. It was their view that it would be difficult to control all of the boundary conditions, and it may be better to just monitor them, i.e., focus on ambient, natural state, behavior, and monitor where there is no influence from the heater. In terms of defining modeling capabilities, it was suggested that the model should use 3-D grid blocks, about 2000 nodes or blocks with modified TOUGH code from New Zealand, or V-TOUGH, or NORIA. (Talk to George Zyvlowski, LANL.) For thermomechanical modeling, it was noted that ADINA-T is the workhorse of thermomechanical codes.

With regard to placement of the heater, it was concluded that it would be best to intersect a single fracture. A horizontal fracture would not demonstrate much mechanical deformation due to loading and it would be best to intersect a vertical fracture, causing the greatest deformation. Another design question is whether the intersected fracture should be perpendicular or parallel to the heater hole. It was decided that if a vertical heater was used, then the vertical fracture would intersect along the plane of the borehole. In this case, it would be best to put sampling locations perpendicular to the fracture, and not along the fracture in the same observation borehole. If a canister configuration is used, then a horizontal heating borehole will be more complicated due to the shedding of water around the borehole from above. The determination of whether to use a horizontal vs. vertical heater should not be determined based upon modeling capabilities. It would be better to use the orientation which induced the greatest fluid flow.

It was suggested that we use tracers (perhaps radioactive) to determine fluid movement. If possible, it is recommended that the rock mass be mined out after the experiment to determine fracture locations, flow field, and the presence of tracers.

There was a discussion of injecting steam into the rock for the purpose of heating the rock instead of using electrical heat. This alternative would add a significant amount of water to the system, however. A question arose regarding what the heater source and waste package will actually look like. It may be better to use a scaled down repository configuration rather than a canister configuration to determine the volume of the disturbed zone.

They emphasized the need of exploratory instrumentation and the need to evaluate technology. The type and location of sensors should be based on what changes in output variables over time need to be monitored. In terms of defining monitoring capabilities, perhaps we should speak with Karl Keller (located in Santa Fe) about use of a membrane sampler. Also, they would recommend the use of BOFEX or capacitance conductors as displacement transducers to measure the thermomechanical effects. (The BOFEX sensors were used at the Grimsyl site.) The displacement transducers should be located within the heated region, at say 0.1, 1 and 2 m from heater.

CONCLUSIONS: LBL is very interested in participating in the proposed heater experiment from conceptual and computational modeling perspective. It was noted, however, that LBL involvement based on current funding is not possible. They are currently being funded through Paul Kaplan (SNL) to investigate GWTT, and their participation using this funding source requires justification.

Friday, March 22, 1991, Afternoon Meeting
USGS, Menlo Park
Attending: J.D. Bredehoeft

Comments on ALTS study:

An initial question arose regarding the ability to extrapolate short term heater test over longer time horizons appropriate to high-level waste storage. It may be inappropriate to perform a short term test when long term behavior is different. Also, it will be difficult to generalize from this experiment to other sites. Therefore, part of the motivation for this project should be to see if relevant processes are incorporated in models in a reasonable way. The project should attempt to see whether all of the physics can be captured. In this manner the project will be appropriate to resolving issues related to waste storage.

He also had a question regarding the impacts on in situ stress regimes at the knoll site. He believes that it would be better to work underground in order to simulate stress conditions as closely as possible. He would prefer using G-Tunnel for any study.

Another concern he had was the complexity of the situation. The flow regime is probably three dimensional and anisotropic, perhaps with a fracture network dominating the site. While we already know how to model flow in homogeneous porous media, the physics are sufficiently complicated that it will be important to minimize geometric complexity examining only an individual fracture. The question then arises as to how to map flow and transport through a network of fracture.

He thought that it may be best to use a point source to minimize end effects of the heater. He believes that it would be best to perform the experiment at lower temperatures first in order to minimize coupling between processes. With regard to coupled processes, he recommended that we speak with David Pollack at USGS/Reston, who wrote his thesis on coupled mass/energy transport.

CONCLUSION: Dr. Bredehoeft was supportive of a field heater experiment and promised to provide written comments on the experimental plan. His major concern was regarding the feasibility of nuclear waste disposal; his preference being monitored retrievable storage at Yucca Mountain.

COMMENTS AND SUGGESTIONS CONCERNING JANUARY 1990 VERSION OF "EXPERIMENTAL PLAN, Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site" by T.C. Rasmussen and D.D. Evans

by Shlomo P. Neuman

February 12, 1991

My comments and suggestions concern a field-scale heater experiment proposed in the above document by Drs. Todd C. Rasmussen and Daniel D. Evans. The purpose of this experiment is (p. 1) to "confirm[] important aspects of coupled heat, liquid, gas and solute transport" and/or (p. 34) "to evaluate and confirm existing conceptual and computer simulation models related to fluid flow in a nonisothermal environment." Its specific objectives are are (p. 5)

(1) "To further assess appropriate methods, techniques and technologies for characterizing and monitoring water flow, transport and thermomechanical changes in unsaturated fractured rock, including interaction between the rock matrix and the fracture system;"
(2) "To examine relevant hydraulic, pneumatic, thermal, solute transport and thermomechanical processes and relevant parameters, singularly and coupled, at field scales of from one to thirty meters;"
(3) "To evaluate the thermomechanical effects of a heat source on fracture and matrix pneumatic and hydraulic transport properties;"
(4) "To generate data sets for complex, coupled flow and transport systems for use in the validation of unsaturated flow and transport models;"
(5) "To assess various modeling approaches and their limitations in predicting flow and transport through nonisothermal, unsaturated, fractured rock."

"The experiment is designed" (p. 6) "to evaluate the relative significance associated with excluding various processes, and to evaluate scale dependent procedures used to estimate material properties." "Modeling of the experimental results is an important validation aspect, and is the principal reason for conducting the test[]."

These statements of purpose, objective, design goal and principal motivation behind the heater experiment are broad and ambitious. They are also quite general and therefore open to multiple (perhaps conflicting) interpretations.

Our current understanding of flow and transport in unsaturated fractured tuffs, under field conditions, is limited and speculative, hence subject to uncertainty and controversy. There are certain "aspects of coupled heat, liquid, gas and solute transport" about which we know more and other aspects about which we know less or very little. Aspects about which we know quite a lot include heat conduction in both saturated and unsaturated porous rock matrix, liquid flow in saturated and unsaturated porous rock matrix under isothermal conditions, and conservative isothermal solute transport at tracer concentrations in saturated porous media. Aspects about which we know less include heat conduction in saturated and unsaturated fractured rocks, liquid flow through saturated fractured rocks under isothermal conditions, gas flow through liquid-free fractured rocks under similar conditions, and isothermal gas flow through partially saturated porous media. Aspects about which we know still less include heat transport by conduction and convection in heterogeneous saturated porous media and in homogeneous partially saturated porous media, liquid flow through partially saturated fractured porous rocks under isothermal conditions, isothermal gas flow through partially saturated fractured porous rocks, and conservative isothermal solute transport at tracer concentrations in saturated fractured rocks. Aspects about which we know relatively little include heat conduction and convection coupled with multiphase fluid flow in nonuniform porous media.

nonisothermal liquid flow through partially saturated fractured porous rocks, nonisothermal gas flow through partially saturated fractured porous rocks, and conservative solute transport at tracer concentrations in unsaturated fractured rocks. Aspects about which we know extremely little, if anything, include heat conduction and convection coupled with multiphase fluid flow in fractured porous media, multiphase water transport through nonuniform porous and/or fractured rocks at temperatures above the boiling point, gas flow under similar conditions, and solute transport under all but the conditions listed in connection with this phenomenon earlier.

The proposed heater experiment involves many aspects of flow and transport about which relatively little or nothing is presently known. With respect to these aspects, the ability of the experiment to "confirm" and "validate" must be quite limited: one can only confirm or validate what one knows or can reasonably hypothesize, then observe and measure. To date, little has been done to validate our ability (or lack of it) to measure and describe (not to speak of predicting) the space-time distribution of water in fractured tuffs under static isothermal conditions (not to mention isothermal dynamic flow regimes) at a space-time resolution that could clearly distinguish between the roles of matrix blocks and fractures (not to think of finer channels) in storing and conducting fluids on field scales of up to thirty meters, as stipulated in objective (2). Our present understanding of isothermal flow and transport in unsaturated fractured tuffs, and our current ability to define and measure relevant rock properties (saturated hydraulic conductivity as affected by fractures on various spatial scales, unsaturated hydraulic conductivity as affected further by water content, air permeability as affected by fractures and water saturation, relationship between air and water permeabilities, total and kinematic porosities as affected by fractures, spatial variability and scale-dependence of these parameters) and state variables (humidity, water content, hydrostatic and capillary pressure, osmotic pressure, their distribution within and between fractures) are, at best, rudimentary. Such conceptual understanding, and ability to define and measure, are better developed and validated under the relatively simple conditions of isothermal flow. The proposed heater experiment would create much more complex conditions and would therefore make it much more difficult to relate effects to causes in an unambiguous manner than a well thought out and executed isothermal experiment.

The complex conditions created by a heater test render it less than ideal for the investigation of issues which arise under isothermal conditions, such as (p. 6) "the ability of various modeling strategies (including the equivalent porous medium representation of fracture flow, as opposed to discrete fracture network flow representation within a porous matrix) ... to accurately represent fluid flow and solute transport processes in unsaturated fractured rock;" another modeling strategy to consider is one that represents the geologic medium by means of one or several overlapping stochastic continua in areas where detailed information about discrete features (fractures, fracture zones, channels) is lacking, while embedding discrete features into the stochastic model where such have been clearly delineated (geologically and geophysically) and adequately characterized (hydraulically). To model flow and transport under nonisothermal conditions, the same distinctions between modeling strategies must be made. As the conditions created by a heater experiment are relatively complex, its stated purpose (p. 34) "to evaluate and confirm existing conceptual and computer simulation models related to fluid flow in a nonisothermal environment" may be difficult to achieve. For the same reason, it may be difficult to design the experiment so as (p. 6) "to evaluate the relative significance associated with excluding various processes, and to evaluate scale dependent procedures used to estimate material properties." It may be equally difficult to achieve significant progress toward some of the specific objectives listed above under (1) - (6), especially those relating to process definition and model validation.

Given that model validation is considered to be the principal reason for conducting a heater experiment, what can we expect to be validated by such an experiment? How can such a validation be accomplished? The authors suggest (p. 6) that "ideally, the tests will be designed using calibrated models, with calibration data sets having been obtained from laboratory and field tests. Once calibrated, the model will be validated by proposing a perturbation of the system not related to calibration experiments for the purpose of evaluating the assumptions inherent in the model." This contrasts with a later statement (p. 36) according to which "no calibration against the experimetal results is expected." Rather, "model accuracy" will be determined by (p. 35) "simulat[ing] responses using baseline and characterization data, as well as observed initial and boundary conditions," and then "compar[ing] experimental and simulation results [to] determine whether the observed response lies within forecasted confidence intervals." I believe that a combination of these two approaches is needed. However, I propose that one first discuss in some detail what model(s) will be calibrated, against what data, at what stage of the experiment, and how. I also propose that this be followed by a relatively detailed discussion of what aspect(s) of the model(s) or underlying theory (theories) will be validated, against what data, at what stage of the experiment, and how. Only on the basis of such discussions may it become possible to evaluate the potential benefits of the proposed experiment.

Considering the relatively low level of knowledge and technology we currently possess concerning isothermal flow and transport in unsaturated fractured tuffs, and the complex conditions created by a heater test, it is in my view important that we clearly separate what we apparently know and probably can do, from what we admittedly don't know and may be unable to do, well in advance of planning the heater experimet. On one hand, our present ability to accurately measure, correctly interpret, and interpolate spatially, quantities other than temperature at depth during the proposed experiment are limited. On the other hand, groundwater models under much simpler conditions than those created by the heater test generally require a large amount of data, and a good amount of calibration effort, before they can meaningfully reproduce observed behavior. Even after considerable calibration against a relative wealth of space-time data, such models often perform poorly as predictors outside the calibration range and must be periodically updated (recalibrated) to remain current (one well-known updating technique being the standard or extended Kalman filter). It is not entirely clear from the proposal how a heater experiment on the proposed scale of up to thirty meters could, under the given budget and time constraints, generate data of sufficient quantity and quality to allow resolving validation issues of the kind discussed earlier and highlighted further on pp. 6 and 7 of the document. I would feel more comfortable about the heater experiment if its purpose and objectives were more focused, particularly on issues which cannot be addressed by means of simpler (isothermal) experiments. I propose that such a focus might be provided by attempting to answer the following questions, more or less in the order they are listed below, which also represents their proposed order of priority:

1. How accurately, and with what space-time resolution, can one measure and describe the distribution of temperatures on a scale of up to 30 meters in unsaturated fractured tuff at the Apache Leap site? What accuracy and resolution are required to detect anomalies due to convective air and vapor currents through major fractures and/or channels, or due to other causes? Might it be better to emplace temperature sensors permanently in sealed and insulated boreholes rather than in packed-off intervals as presently envisioned?

2. Can one reproduce the space-time temperature distribution on a scale of up to 30 meters, observed prior to activating the heater (*i.e.*, under ambient or pre-test conditions), by means of a simple model which accounts only for heat conduction and treats the rock as a uniform continuum, the properties of which (heat conductivity

and capacity as functions of water content) are based on laboratory measurements on cores and blocks? Existing data (p. 28) suggest that heat conductivity and capacity vary much less than hydraulic conductivity (and porosity?). Can this reproduction be improved through a calibration process in which one varies the distribution of water contents in three-dimensional space? Can the calibrated water contents be verified (validated) independently by means of neutron probes, tomography, or other measurements? If the latter is not possible, can such measurements be used to help improve the calibration? If not, can the calibration be improved by embedding observed fractures and/or channels in the above model of a continuum? Can the calibrated properties of these discrete fractures and/or channels be verified (validated) independently through direct hydraulic and/or pneumatic tests? Can a satisfactory calibration be achieved, primarily to anomalies if such have been detected, without modifying the model to allow for convection through known (or unknown) fractures and/or channels? If unknown fractures and/or channels are required for calibration, can their existence and properties be validated by independent measurements, and how?

3. What can be learned from observations of temperature and water content under pre-test conditions, with or without the above model, about ambient heat fluxes through the rock on a scale of up to 30 meters? Can one confidently determine an average heat flux vector, and its variation with time? Can one determine the manner in which the directions and magnitudes of local heat fluxes vary from point to point in three-dimensional space and in time? Can any such determinations be validated through independent measurements, and how? Can one detect and quantify anomalous heat fluxes due to convective air and/or vapor flow through known (or unknown) fractures and/or channels, or due to other causes? Can such fluid flow be detected and quantified independently of the temperature data to help confirm (validate) its existence and extent?

4. Assuming that the pre-test distribution of temperatures, water contents and heat fluxes can be adequately characterized and modeled, what modifications in the model are required so that it can be used to design the heater experiment proper? Considering that at the Apache Leap injection site (p. 27) "field estimates of saturated hydraulic conductivity" show a (p.28) "variation of ... approximately 700% and ranges of five orders of magnitude," how meaningful is it to borrow such data for the design of the heater experiment (p. 2), and how can such a transfer of data from one site to another be accomplished? Could these data be averaged in some meaningful way to yield effective continuum values and if so how? If not, would a stochastic representation and model be required to design the heater experiment? In the latter case, are the available data amenable to a statistical analysis that might yield a meaningful stochastic representation? Would it not be more appropriate to design the heater experiment on the basis of hydraulic and/or pneumatic data collected at the site of the proposed experiment? If so, by means of what method(s), on what spatial scale(s), with what degree of spatial resolution, and how accurately can and need one measure and describe the distribution of saturated hydraulic conductivities, unsaturated hydraulic conductivities as functions of water content and temperature, capillary pressure as a function of water content and temperature, and/or air permeabilities as functions of water content and temperature, on a scale of up to 30 meters at the proposed heater test site? With how much confidence can and need one attribute such measurements to this or that fracture, channel or matrix block? How should the answer(s) to these questions be reflected in the selection and/or development of conceptual, mathematical and numerical model(s) for experimental design?

5. After selecting or developing an appropriate nonisothermal multiphase flow model for experimetal design, and collecting appropriate hydraulic and/or pneumatic data to serve as input parameters for this model, what considerations and criteria should one adopt in designing the experiment? What, according to this model when applied under a range of plausible input parameters, is needed to detect the onset of convection in response to various heater power outputs and schedules? Is it possible to design a (single or multiple) heater power output and schedule (constant or transient) such that the onset of convection can be detected solely by observing temperature variations with time at various points in three-dimensional space? As temperatures are the easiest state variable to measure, this seems highly desirable. Should it also be possible to deduce, from temperature measurements alone, what is the spatial extent of convective currents generated in response to heating? What effect do individual fractures and channels have on convection (do they create identifiable temperature anomalies)? Could such deductions be verified (validated) independently by means of tracers (in liquid and/or gas phases) and/or measurements of water content variations in time? Is a scale of up to 30 meters appropriate for the heater test? How can one determine the average heat flux, Rayleigh number (ratio between buoyancy and viscous retardation forces) and Nusselt number (ratio between total and conductive heat flows) on such a scale?

6. If temperature measurements alone are not enough to detect the onset of convection, its spatial extent, and the role of individual fractures or channels, what additional measurements are required (water contents? liquid pressures? vapor pressures? pneumatic pressures?), at what spatial scales and resolution, with what accuracy? Are such measurements feasible considering available technology, time and budget? Are (p. 17) "tomographic estimates of water content ... , especially near the heater borehole due to the failure of neutron probes at elevated temperatures," presently possible at the required accuracy and resolution? What measurement are required to not only ascertain the presence of convective cells, determine their spatial extent, evaluate the role of individual fractures and channels, and estimate the associated heat fluxes, but also to differentiate between the roles of liquid water and vapor, evaporation and condensation, transport of sensible and latent heat, in the convection process? In other words, what measurements are required to validate the heat pipe effect under field conditions, and to quantify this effect? What measurements, if any, could potentially isolate the role of individual fractures in the generation of heat pipes (will there be more than one)? Are such measurements feasible?

7. What measurements, if any, may detect the effect of heat on the porosities and permeabilities of matrix blocks and fractures? What measurements, if any, may detect the effect of heat on unsaturated rock properties? Are such measurements feasible? To what extent and how should this effect be studied in the laboratory prior to the heater experiment?

8. What measurements, if any, can help in the determination of water and air mass balance during the heater experiment? Are such measurements feasible? Would it be desirable, and technically feasible, to fully or partially isolate the tested rock mass from its surrounding by means of insulation and grouting in order to have a better control on mass balance?

Some of these questions can be answered prior to doing any work at the heater site and I suggest that this constitute Phase 1 of the proposed work. Some questions cannot be answered prior to performing intensive site characterization of fractures, hydraulic and/or pneumatic property distributions, water contents and pressures; I propose that this constitute Phase 2a of the planned work. According to such a plan, Phase 2b would be conducted concurrently with Phase 2a, and would involve experimentation with various in-situ temperature measurement techniques, followed by the installation of a three-dimensional network of thermistors (or other temperature sensors) and the compilation of baseline data on ambient temperature distributions in space-time on the proposed scale of the heater experiment (this scale to be determined as part of Phase 2). Phase 3 would comprise an analysis of the characterization and temperature data collected during Phase 2, followed by an attempt to develop and calibrate a three-dimensional model of the ambient temperature and moisture conditions at the site. During Phase 4, this model would be modified (if necessary) to allow the simulation of conditions other than ambient, as expected during the heater experiment, and then used to design this experiment. In response to needs identified during Phases 3 and 4, Phase 5 would then follow with an expansion of the network of thermistors and additional site characterization. Under Phase 6, this additional characterization and temperature data would be used to recalibrated the model used for experimental design, and to modify the design if necessary. The recalibrated model would generate a prediction of system response to heating under the final design, over the entire projected life of the experiment, including a period of monitoring after the heaters are deactivated. The heater experiment itself would constitute Phase 7a. Phase 7b, initiated concurrently with Phase 7a but continuing beyond this latter phase, would be an analysis of the experimental results and a comparison with the predicted response from Phase 6.

rec'd 4-26-91 in WMB

Silberberg
Randall
Nicholson
Margulies

Center for Nuclear Waste Regulatory Analyses

P.O. DRAWER 28510 • 6220 CULEBRA ROAD • SAN ANTONIO, TEXAS, U.S.A. 78228-0510
(512) 522-5160 • FAX (512) 522-5155

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U. S. NUCLEAR REGULATORY COMMISSION
ATTN: Mr. Mel Silberberg
1 White Flint North (NLS-260)
11555 Rockville Pike
Rockville, MD 20852

Subject: Comments on Experimental Plan - Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site

Dear Mr. Silberberg:

Per your letter of March 15, 1991, the subject document, "Experimental Plan - Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site" has been reviewed by the staff at the CNWRA. A composite set of the technical comments prepared as a result of the review is presented in the attachment.

Please contact me or Dr. Budhi Sagar if you have any questions on this matter. We look forward to further discussions and interactions regarding the proposed experiments, and would welcome the opportunity to meet collegially with the University of Arizona and NRC staffs on this matter.

Sincerely yours,

Wesley C. Patrick
Technical Director

WCP/RG/nyp

cc:
J. Funches
S. Fortuna
B. Stiltenpole
S. Mearse
CNWRA Directors
CNWRA Element Managers
J. Randall
T. Nicholson
T. Margulies
J. Latz
R. Green
S. Rowe, SwRI Contracts

Comments on
Experimental Plan - Nonisothermal Hydrologic Transport Study
at the Apache Leap Tuff Site

1. Insufficient information was provided in the Plan to allow substantive evaluation of the "key technical issues" that were identified in the NRC request for technical review. (Only about 6 of the 43 pages of the document spoke to the experimental approach.)

2. The Plan conveys little assurance that the nuclear waste program is prepared to proceed with another round of extensive and expensive field testing. The approximately 30 pages of background material do not address the successes and failures of previous attempts to model the results of large-scale field studies. There is a plethora of data already available from tuff, basalt, granite, salt, clay, and soils field research sites, as well as numerous laboratory studies. There is little evidence that the modelling community has succeeded in replicating the results that are already available, under either isothermal or nonisothermal conditions.

   It appears that all but one of the five objectives of the experiments (namely, generation of additional data) could be achieved with data that are currently available but not fully analyzed or evaluated in the context of numerical models. An alternative approach to the proposed Experimental Plan would be to conduct the work in a step-wise fashion, with the new initial phase being the evaluation of models in the context of the existing body of data.

3. Based on the information provided in the Plan, it does not appear that the conceptualization and preliminary design of the experiment takes advantage of the work of other researchers. Specifically:

   - With the exception of one reference to the work of Daily and Ramirez (1989), it does not appear that past field studies conducted by researchers outside the University of Arizona have been examined.

   - In addition to other works related to the late 1980's work on horizontal emplacement of heaters in G-Tunnel, the closely related work conducted by Lappin, Johnstone, and others in G-Tunnel in the 1970's and 1980's should also be considered.

   - Numerous other researchers in the U.S. and abroad have designed and conducted pertinent heat and/or hydrologic flow studies in a variety of geological media. Even though the media differ significantly, the experimental design approaches are likely to be pertinent.

   To proceed without the benefit of thorough examination of the design, conduct, results, and problems associated with the above-noted studies would deny the NRC program of the advantage of the knowledge that has been obtained from those studies.

4. Some concerns exist with regard to the concept of the experiment. First,

although it is an objective of the research to "examine relevant ... processes and relevant parameters," there appears to have been little attention given, thus far, to the validity of the simulation of repository conditions. Assuming that "relevancy" is to be judged with respect to processes, conditions, and parameters likely to exist at the proposed repository site, it is essential that the experiments be a valid simulation of that repository. Otherwise, the observations may lead the researchers to study interesting but unimportant phenomena which are artifacts of experimental technique or, conversely, to miss phenomena which are critical to repository performance. (An example might be to focus on the detailed three-dimensional heat flow that is of interest in the very near field, while missing the character of heat flow from a nearly planar repository-scale heat source).

Second, the proposed location of the test bed may be so close to the surface that near-surface phenomena (which have not had the opportunity to be 'damped out' with depth) may swamp the data. Once again, this could lead the researchers to focus their efforts in the wrong areas. (An example might be wind-driven 'convective' processes which, presumably, would be damped at depth.)

5. Weak's recent analysis of the effects of wind at the summit of Yucca Mountain suggests that on a knoll such as the heater site the wind could strongly affect gas flow and desiccation. This subject also seems to be neglected in the proposal.

6. The boundary conditions of the knoll surface need to be established prior to the commencement of a long-term heater test. Use of predictive models to assess the effects of boundary conditions is highly-dependent upon the values of input parameters of the model. Improper selection of the values assigned to these parameters could result in misleading perceptions of the effect of the geometry of the knoll upon the heater-test affected processes within the subsurface. Thus, because of the unreliability of predictive models, boundary conditions of the test site need to be firmly established for the test results to be meaningful.

7. More attention needs to be given to maximizing the benefits of the test program by integrating across technical disciplines. A field study of this type could allow 'piggy-backing' of other experiments (on a non-interfering basis) such as materials testing, geochemical and hydrological environmental assessments, etc.

8. One of the processes to be investigated is rock matrix and fracture deformation due to heating. Detailed information is not provided in the study plan how specifically the fracture deformation will be monitored. In the types of measurements proposed (page 39), only rock extension due to heating is specifically mentioned.

9. Although the proposed project indicates that coupled hydrologic, thermal and mechanical processes are of interest, most of the discussion within the proposal places emphasis on thermal-hydrologic effects rather than thermal-hydrologic-mechanical effects. Consequently, it is not clear whether the selection of a simulation model or models outlined in Task 1 will focus primarily on fully-coupled simulation codes, or whether these codes will explic-

itly model the joint behavior or use some type of continuum model representation. If the model selected is to simulate the jointed rock mass response, mechanical properties of the joints will need to be determined. The current work plan does not appear to include plans for such joint characterizations.

10. The approach being adopted in this project is to conduct the monitoring and simulation independently and then to compare the results to determine the accuracy of the prediction. However, no follow-up tasks are proposed if the prediction is found to substantially deviate from the measured response, a situation which has been common in tests to date.

11. More generally, it should be noted that any field heater test in the unsaturated zone will entail complicated data analysis and interpretation problems. The intent of this project should be to build on the understanding of the phenomena and predictive capability in this area of activity. Next, interactions among the phenomena have to be studied in a controlled lab environment leading up to a lab-scale block (tuff) heater test. In parallel, analytical tools need to be developed and/or verified. The final step should be a field test to provide a credible data base for use in verification of computation capabilities developed to date.

The U of A proposal, while in general a good idea, needs to recognize the schedule and resource constraints that may apply to perform a credible test program.

- The proposed site will require significant characterization.
- In this thermally-driven application, from a thermo-mechanical sense, an understanding of the residual stress patterns will be required to evaluate the constraint effects on fractures.
- The geochemical aspects of a liquid-vapor environment will complicate the near-field measurements. In light of this, hydrologic effects in the far field of the proposed test may be easier to measure.
- Any piggy-back experiments, such as metal coupon testing, will need to be carefully coordinated to avoid areas where extensive instrumentation and monitoring of the environment will be required.

It should be cautioned that because of potentially large uncertainties in the outcome of the interpretation of data generated by a complex heater test, there a strong likelihood of endless debate among researchers on the subject. This will also be true when data from the proposed ESF heater test become available.

12. In the background information, the variation of various transport properties is shown to be up to several orders of magnitude. Will this variance be determined at the heater site, and how will this variance be considered in both the experiment design and in the simulation studies?

13. A number of allusions to geochemical issues are presented, but little

specific focus is given to potential geochemical effects and possible research topics. Some issues mentioned are osmotic effects on the water vapor pressure and tracer studies of transport processes. Perhaps the means of analysis of these effects will be described in greater detail in subsequent work plans. Some additional subjects of interest are listed below.

- Water chemistry. Will efforts be made to examine water chemistry, despite the clear difficulty in extraction of uncontaminated water from unsaturated rock? A possible tractable and interesting study would be to instrument the site to measure electrical resistivity. As empirically defined in Archie's formula, electrical resistivity is a strong function of ionic strength of the water, as well as saturation state. Independent ionic strength of the latter could permit determination of the former. A dominant effect may be concentration of solutes by evaporation of water, which would be monitored by in situ ionic strength measurements. Electrical resistivity measurements taken in conjunction with saturation level measurements could provide geochemical and hydrologic insight using Archie's formula. Previous work at G-Tunnel and at the Oracle site may shed light on this research possibility.

- Mineral chemistry. Petrographic, x-ray diffraction, and chemical analyses of the minerals and glass in the rock before and after testing could possibly identify important chemical reactions resulting from the fluid and heat transfer processes. Previous heater tests in the Climax stock effectively utilized a borehole sampling scheme to make similar comparisons.

- Coupled processes. Osmotic effects on water vapor pressures are mentioned as a coupled phenomenon. Others may be important. Heating water and volatilization of dissolved CO_2 on heating both induce calcite precipitation, which is a relatively fast reaction. This and/or other reactions could affect the details of hydrologic properties such as permeability or porosity. Complete vaporization of water could also produce important precipitates, especially in a heat-pipe regime where the vaporization front is stationary. The waters and/or muds used in drilling will contaminate the site. Some attention may be devoted to the possible effects of reaction of these materials with the rocks or in the heated system to produce hydrologic consequences.

14. Binary diffusion (particularly of air toward the zone of vaporization and high water vapor pressure) was shown in Pruess's models to be important in controlling the gas pressure and composition. Installation of micro-pressure transducers in the field near the heater could provide valuable information regarding heat-pipe type of phenomena. Analysis of this process seems to be neglected in the proposal.

The University of
ARIZONA
Tucson Arizona

Department of
...ology and Water Resources

College of Engineering and Mines
Building 11
Tucson, Arizona 85721
(602) 621-3082
FAX (602) 621-1422

April 26, 1991

MEMO

TO: Dan Evans

FROM: R.L. Bassett RB

RE: Draft Report, "Experimental Plan - Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site," by Rasmussen and Evans.

Here are a few comments regarding the subject report. We can discuss them at our weekly meeting.

Hydrology and Water Resources

From - Bassett

Review comments for the "Draft Experimental Plan: Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site"

I. General Comments

I have two general comments regarding the subject document which are discussed below. First, after reviewing the existing data on field tests conducted to date, and after reviewing the appropriate laboratory data as mentioned in the subject document, I am convinced that we are probably not ready to conduct a large field scale heater experiment. If we were to perform this field scale experiment according to the proposed schedule, I suspect we would obtain ambiguous information as has been obtained in the past, with poor return for our time and monetary investment. I also do not think that progressing through the phases identified in Table 1 of the subject report, even in a careful and deliberate fashion, will satisfactorily prepare us for the technical difficulties. Secondly, I am concerned about the absence of a plan to develop the ability to monitor the movement of solute. The tests are described in terms of theoretical consideration regarding the movement of solute, but the fact remains that at present we have poor capabilities, and little research effort to develop it.

Large Block Experiment

It seems to me that an intermediate smaller scale test must be conducted first. This seems to me to be a certainty if we are to have any hope of obtaining meaningful chemical data, and probably the same is also true for hydraulic data. I would recommend that a heater test be done first with a large block of tuff, preferably in the lab, but possibly in the field, conducted under well constrained conditions. There are too many experimental details yet untested, and too many other factors that must be optimized.

I am suggesting a departure from the ideas presented in the subject report under Phase 2 and Phase 3, in which monitoring devices are selected and calibrated in core studies. A large block study is one step removed from this approach. Core studies tend to isolate the system to the investigation of only a few parameters, which is desireable for the initial phases; the transfer of this data to a large field scale experiment in this case is too large of an information transfer. In a large block experiment, the synergistic relationship between heater output, heater orientation, and location of monitoring holes, in conjunction with the testing for best method of detection, is critical. This is the aspect of the current plan which is bypassed. Too much faith is placed on existing knowledge, and short sighted experiments. Additionally the orientation and location of monitoring devices in relation to the fractures can be crucial. In a big block, horizontal vs. vertical monitoring holes, various monitoring devices and boundary conditions can be investigated simultaneously. Results from such studies may indicate an entirely different approach for the field study, such as relocation to another surface location, or even the discovery that the tests should be conducted in a tunnel with more tightly constrained boundary conditions such as a more constant temperature profile and moisture content, or that a horizontal heater emplacement is optimal. These kind of evaluations are not done easily on the field scale without either degrading or destroying the site for future work, or escalating to prohibitive expense. Clearly a field test of some magnitude needs to eventually be conducted.

The proposed laboratory instrument calibrations, calibrating data sets and core measurements are essential, but are not in my opinion likely to yield data that will allow us to extrapolate to the design of the currently proposed field scale experiment. Additionally I do not believe our poorly calibrated computer models will allow us to bridge the gap from small cores to field experiments without the large block testing phase.

Solute Transport

The second comment is in regard to the choice, detection and monitoring of solute and gases. I know of no lab studies for unsaturated tuff that have successfully monitored solute movement on the scale of our interest. Other than the obvious methods of monitoring such as by the use of radioactive tracers, remote detection of large resistivity changes or by destructive post experiment analysis of the rock, we do not have any viable procedures for detecting solute migration. Since the theorized processes of heat pipes, bridging across fractures for solute transport, regions of varying osmotic pressure, etc. are of major concern, how can we proceed so rapidly without the appropriate methodologies?. If the effects of these processes are to be measured then much work must be done to develop both the appropriate tracers and effective monitoring techniques. Questions such as whether tracers can be added without disturbing the existing flow field, or whether natural tracers exist in the pore water, have not even begun to be addressed. How is any water sample to be collected? Where should the sampling ports be placed? Must the analysis be done remotely, or in situ? What is the actual device? What is the orientation of the monitoring ports? These are all preliminary questions that have not been asked and there are no defined efforts underway to answer them in the time frame of the proposed study.

I appears to me that many of these questions must be addressed immediately, and tested in cores and small blocks of tuff. Subsequently, a large block experiment, designed to be scaled to field size, can be conducted in the lab (e.g. 1 meter, etc.). It would be less desireable in my opinion, but possible to alternatively conduct the block test in the field, with a well controlled vertically oriented heater, perhaps in a tunnel or in a block of tuff around which we could excavate enough material to control and characterize the hydraulic parameters. I would prefer a boulder or large block study in the lab. We have little knowledge of the accuracy of the measuring devices, required orientation, needed spacing, effects of fractures, and performance of sensors, especially newer less proven methods like tomography, or any kind of newly emerging in situ chemical sensors. All of these parameters could be optimized on the block scale heater test setting. I would much prefer to observe this on a smaller scale, under conditions that can be repeated and under which different monitoring devices can be tested, rather than attempt to predict this for the large scale test.

Successful completion of these tests will greatly improve our chances of a meaningful field study. Without such a precaution I would predict the outcome to be one more marginal test with little useful data for model simulation and validation.

II. Document Specific Comments

p.1, para. 1, line 18. add "diffusion and volatilization," also I do not know what mineralization represents and how it is different from chemical precipitation. There is no mention in this list of conditions of master variables such as redox state, pH and ionic strength considerations.

p.5, para. 1&2. No mention of the measurement of any solute parameters.

p.6, para. 2. This paragraph makes no sense to me.

p.6, para. 3. The idea of using calibrated models and calibration data sets may be feasible for unsaturated hydraulic parameters, but no such data or models exist now for solute chemistry nor are they likely to be extant before the proposed heater test.

p.6, para. 4. The validation exercises exclude solute transport. If this is the plan then you should not include solute transport as an objective (p.5).

p.8, para. 2. Please indicate if this is information you have gained from your own experimental work, in which case I would be very interested in the studies under bullets 3 and 4, please document this. It might be expected intuitively that these last two bullets represent effects that might be significant, but I am dubious that either one will occur to the extent that it will be of any major importance. I would like to see some validating data for these two effects. I cannot believe the dissolution and reprecipitation of secondary minerals will have sufficient mass to affect the hydraulic properties. Likewise, if you are referring to the solute naturally present in the pore space of the tuff, could this solute mass be significant enough to affect hydraulic properties, I would doubt it. If this experimental information is derived from the literature it should be cited.

p.9, para. 2&3. If you intend to evaluate the movement of solute and vapor, then you should include this in the validation exercises. One should also do some laboratory studies with aqueous compositions similarot the natural solute, and plan to install the appropriate monitoring devices for detecting the movement of solute and gases.

p. 9 para. 3. Is it now still reasonable to assume that 100 degree C temperatures are reasonable in the tuff, considering the current packaging strategies and spacings? If not what is the current thinking about thermal gradient?

p. 16-17. It appears to me that many lessons can be learned from the experience of the field test conducted by Davies. First among them would be the need to conduct a laboratory heater test on a large block of tuff, under open and well instrumented conditions. It is obvious that most of the parameters we are interested in monitoring have not been optimized. It seems reasonable that we should investigate these factors before we go through the expense of a field test. Such as:

o are we accurately measuring actual temperature distributions of walls, rock and void spaces?

o How does the presence of liquid water in the open borehole impact any of the measurements of moisture content or psychrometer measurements?

o What more can be learned from multiple packer arrangements, and what should the spacing be?

o What is the optimum power rating of the heater, and how can we effect the movement of moisture in a reasonable time frame?

o How will the vertical orientation relate to previous studies compared with a horizontally oriented heater? Will the vertical orientation require vertical boreholes that will interfere with the attainment of meaningful data above the heater and make detection of vapor and liquid water movement difficult?

o We do not even know how the presence of a fracture will impact the vapor movement in a large block. How can we expect to plan appropriately for monitoring complications derived from the effect of possible multiple fracture sets of indeterminate size, aperture, and frequency in the field setting?

o Is it reasonable to expect that tomography would have any where near the resolution needed for an experiment of this small a scale? How close would the instrument have to be? How would it affect the placement of boreholes and the subsequent effect on the flow field?

STANFORD UNIVERSITY, STANFORD, CALIFORNIA 94305

HENRY J. RAMEY, JR.
KELEEN AND CARLTON BEAL PROFESSOR
OF PETROLEUM ENGINEERING

May 13, 1991

Dr. Mel Silberberg, Chief
Waste Management Branch
US Nuclear Regulatory Commission
Washington, DC 20555

Dear Dr. Silberberg:

This is in reply to your letter dated April 8 re evaluation of a preliminary draft of the University of Arizona "Experimental Plan--Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site" by Drs. Todd Rasmussen and Daniel D. Evans. The objectives of the requested review were outlined in the 2nd paragraph of your letter to me dated April 8, 1991.

Since the manuscript was a "preliminary draft", the authors seek help to prepare the "draft". As a result, the text is not specific as to methods, but does present objectives of the study. The primary objective is to evaluate the ability of an engineered site to contain HLW. The specific objectives of the experiment are cited as assessing methods for monotoring water flow and thermomechanical changes in unsaturated fractured rock; to examine hydraulic, pneumatic, solute transport at field scales of one to thirty meters; evaluate effects of a heat source; and assess various modelling approaches in computing modes of transport.

Your first question concerned the appropriateness of the field scale. I think this is perhaps a critical issue. The authors cite a study at "field scales of one to thirty meters". Their experiment is planned on this basis. The heat source will be in a borehole, and thus small in diameter. I have doubts that it will be easy to scale from their "field scale" to that of a full size HLW repository. Flow in a fractured system must be affected by the frequency of the fractures and the scale at which you can inspect the system. On the other hand, I'm not certain anyone could jump directly to a much larger scale. However, the appropriateness or justification for the planned scale should be addressed in the final draft.

Your second question concerned the ability of the investigators to monitor and isolate specific responses to their experiment. There isn't enough information in this draft to answer this question other than by inference. A previous heating experiment had problems with water in fractures and temperature measurements. The nearby injection experiment went well, however. My speculation is that they can do a reasonable job on the planned scale. The staff appears qualified and able. I am concerned that the small scale may lead to complications in interpretation and scaling to a full-scale operation.

Your third question concerned possible problems in identifying ambient and boundary conditions for the proposed site. As much effort as has been put into identifying conditions at the proposed site, it is not likely that an alternate site would be known better.

One item that concerned me was many comments on modelling. One objective is to evaluate various modelling programs. Another is to use modelling to plan the experiment. Yet little information is given on the models which will be used. It is not clear to me how much effort the authors intend to spend on the modelling. This could be a monumental task in itself. This part of the proposed experiment also needs more discussion in the proposal.

In summary, the investigators appear qualified, there is much experience with the site, but this preproposal provides only brief information on the planned experiment. I am concerned that the interpretation of results from the small scale may be confused by detail.

Sincerely,

Henry J. Ramey Jr.

Henry J. Ramey, Jr.
HJR/

SOUTH DAKOTA
SCHOOL OF MINES
AND TECHNOLOGY

RAPID CITY, SOUTH DAKOTA 57701-3995

DEPARTMENT OF MINING ENGINEERING
PHONE (605) 394-2344

May 17, 1991

Dr. T.C. Rasmussen
Department of Hydrology and Water Resources
University of Arizona
Building #12
Tucson, AZ 85721

Dear Dr. Rasmussen:

First, I must express my sincere apologies for not responding to our discussions in early March before this time. After returning to Rapid City from Tucson, I got the 'flu. Even though I was only away from work for a couple of days, its lingering effects took 4-5 weeks before I was able to get back to a full schedule. As I probably told you, I am writing my dissertation for defense this summer. The dissertation and one course that I am teaching had to take precedent over any other activity during this time.

I am writing today for two main reasons; to comment on your proposed heater experiment and to give you some results for the partly welded tuff samples you gave me.

At this stage, some general comments may help your project as no doubt the proposal has been submitted. Some ideas are fairly obvious but I include them for completeness.

1) Perform an approximate numerical model before the emplacement of temperature sensors. This could help determine where the sensors should be placed to validate the model. The effect of location of access drifts and drill holes for the experiment should be considered. In addition, a model would assist in evaluating at what time to turn the heater on and off. For example, some semblance of attainment of steady state would be advantageous before the heater is turned off.

2) Allow for some redundancy in the placement of the temperature sensors so that if some of the them do not work then others will at least give values in that region. Redundancy could be two sensors in nearly the same location or sensors in additional drill holes from different directions. At a mining project, holes are being drilled vertically and at angles from above and then in horizontal directions at different elevations around a working stope.

3) We personally prefer thermistors for temperature sensors as they are more rugged than thermocouples; they give a higher resistance which is easier to accurately measure and the lengths of the leads do not give major errors; and there is no need to maintain a known temperature reference location. We have the capabilities on campus to calibrate numerous thermistors back to a NIST standard thermometer. The two calibration constants for each thermistor give the relationship between 1/T and ln(R). The temperatures can be easily and accurately obtained for a large temperature range.

4) There is always a concern that the temperature sensor is measuring the air temperature and not the "undisturbed" rock temperature. Small pockets of air can create convection and maybe the distance between your packers is large enough for such convection currents. Backfilling of the drill holes is extremely important as they may act as heat pipes and modify the undisturbed rock temperature. The same safeguards for the permeability work are applicable for the temperature work. In our drill hole probes, we have used springs so that the thermistors are in thermal contact with the wall rock.

The enclosed graphs and pages from the dissertation draft give the thermal conductivity data for two specimens which were prepared from samples I brought back from Tucson. Could you confirm whether the data reported by Davies in his M.S. thesis was for the partly welded tuff or the unwelded tuff? Also, is my terminology correct? and are there any problems in including this information in my dissertation? The data is for your use. I am currently measuring some specimens of unwelded tuff and densely welded tuff which came from other people but were originally from the Apache Leap formation. Initial data indicates that an unwelded specimen has a lower thermal conductivity; in the 0.7 - 0.8 W/(m-K) range. Later, I will have specific heat values so that diffusivity can be determined.

I can reached by phone at 605-394-1970, 2344, or 2361 for further discussions. I may be in Tucson in June and/or July, and have a tentative defense date of July 26. I am looking forward to continuing interesting discussions.

Yours sincerely

Eileen Ashworth
Associate Professor

cc: S.P. Neuman

UNIVERSITY OF NEVADA, RENO

Department of Mining Engineering
Mackay School of Mines
University of Nevada, Reno
Reno, Nevada 89557-0139
(702) 784-6961

May 19, 1991

Professor Daniel D. Evans
Hydrology
University of Arizona
Tucson Az 85721

Dear Dan:

Please find enclosed (finally) my review of the Experimental Plan for your Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site.

Please accept my sincerest apologies for this very tardy response.

I note with interest the repeated recognition in your Plan of the extreme variability of tuff, confirmed of course by work at Yucca Mountain and by our own mechanical characterization work. In light of the potential importance of this variability I would like to suggest that you might consider involving Dr. Kumar Kulatilake of Mining and Geological Engineering, given his considerable expertise in modeling statistical variability in geotechnical engineering, and specifically at the Stripa Project.

Thanks very much for having an opportunity to review a most interesting and valuable project, and very best wishes on an early start-up.

Best Regards,

Jaak

Jaak Daemen

cc: J. Philip, NRC

Review of January 1990 Draft of "EXPERIMENTAL PLAN Nonisothermal Hydrologic Transport Study at the Apache Leap Tuff Site" Coordinated By Todd C. Rasmussen and Daniel D. Evans, Department of Hydrology and Water Resources, University of Arizona, Tucson.

Review by Jaak Daemen JD
Date: 5-18-91

The proposed experimental plan presents an excellent study program that will provide an outstanding opportunity to study the numerous complex processes involved in the near-field geological environment of HLW. Given the lack of such test programs, particularly on the scale and in the medium proposed here, it is clear that the proposed test program will provide an opportunity for making significant advances in the-state-of-the-art, notably with regard to conducting the tests, validating models of the processes, identifying shortcomings in the models and in their computer simulation implementations, and hence in further improvement of prediction methodologies. In sum, the proposed test plan will produce substantial results and benefits. These benefits are stated clearly and convincingly in the test plan, and will not be repeated here.

The primary objecive of this review is to identify, from a rock mechanics point of view, areas in which the program could be strengthened. I recognize that all additional work will incur additional cost, time, and complications. Nevertheless, given the overall benefits to the HLW repository effort that might result from a broadening of the presently proposed effort, I believe that the additional cost and effort can be fully justified.

Major recommendations:

- include thermomechanical modeling in the test design modeling
- include extensive displacement/strain monitoring in the test program
  (I recognize that such modeling is mentioned several times. For reasons discussed in more detail below, I am not convinced that the thermomechanical monitoring requirements have received sufficient attention)
- include validation of thermomechanical modeling as part of the project objectives? (I recognize that this may be expanding the scope of the project too much. On the other hand, many of the measurements needed for monitoring such

variables as rock mass deformation and fracture apertures would provide ample data to pursue a thermomechanical validation effort, and should be of interest to flow model validation efforts as well.)

Detailed Comments

It may be helpful to clearly define what is meant by "verifying" and "validating", e.g. is the use of "verifying" in the abstract really appropriate?

Is sufficient attention paid to geochemical effects that may affect thermomechanical and presumably hydrological and pneumatic properties. Although dissolutioning and precipitation of minerals along fractures are mentioned in the text, they receive very little attention.

The experimental plan repeatedly mentions rock stress. Given the location of the proposed test site, i.e. an unconfined knoll, it would seem rather unlikely that measurable rock stresses can develop. Although thermomechanical analyses would be desirable to evaluate this assumption, intuitively it would appear far more likely that strain and displacement monitoring will be more productive than attempting stress measurements.

The lack of attention paid to rock mass deformation, and its potential influence on flow poperties appears to be confirmed by the lack of measurements of (thermo-)mechanical properties (e.g. p. 2, third paragraph)

I am concerned about the emphasis on prediction without calibration. I recognize the technical validity of the logic. It is important that this formulation of a (valid) objective be expressed very carefully. On several in situ HLW-related test programs (notably the Climax tests, some Stripa tests, some WIPP tests), excessive and/or premature claims of predictability have backfired when the predictability turned out to be uncertain, at best. I believe caution is especially important in light of the potential misunderstanding about eventual discrepancies in the non-technical community.

The experimental plan emphasizes measurements in boreholes (e.g. first paragraph on p.5. From a thermomechanical validation point of view, and possibly from the perspective of assessing the influence of rock mass deformation on flow, it may be productive to include numerous surface deformation stations. A thermomechanical analysis would help in determining whether such displacements could be measured reliably. If the indications are positive, including such measurements would be helpful. If the predicted displacements were fairly large, measuring them might be relatively inexpensive. Such measurements may be useful in evaluating whether the rock mass deformation is continuous or discontinuous. In the latter case, displacement monitoring might assist in identifying those discontinuities along which preferential displacement is taking place. I recognize that the interpretation of such measurements will be complicated by displacements induced by natural temperature variations, as well as by injection pressures of permeability tests.

Have any analyses (thermal, flow) been made of the Apache Leap Field Heater Experiment? (None seems to be mentioned on pp. 12-17). Such an analysis would seem to be a wortwhile endeavor on a somewhat smaller scale than the proposed work.

I presume that the site selection may well be final, for all kinds of practical reasons. However I have to point out two differences from a potential Yucca Mountain repository that seem significant: testing in unwelded tuff, and testing in an essentially unconfined test configuration. While an argument can be made that the latter provides for simple boundary conditions (in terms of mechanical constraints), it needs to be recognized that without confinement rock mass defortion, in particular joint deformation is likely to be significantly different from what would be expected under a confined configuration.

NOTES

(Listed and identified by page/paragraph).

1/1 "validating" rather than "verifying"?

1/2 Include geochemical effects?

1/3 "rock deformations/strains/displacements" rather than "rock stress near the heater source"?

1/3 Last sentence seems to imply a (single?) simulation model that incorporates all relevant processes - a tall order

2/3 No thermomechanical or mechanical properties are available?

5/1 Include borehole deformation gages, surface displacement/deformation/strain gages. (May be implied in 5/2)

5/2 It is unclear what is meant by and included under "sampling"? Collecting physical samples to determine properties?

5/3 "... environment to contain and isolate HLW."

6/5 First bullet: from a thermomechanical point of view it may be worthwhile to broaden "equivalent porous medium" to "equivalent continuum".

7/1 Prediction of displacements/strains may be more readily verifiable (measurable) than stresses.

7/3 The "only" seems to denigrate calibration. I am not convinced that such a negative perception of calibration is justified. Although I very much agree with and like your rationale for basing validations on independent predictions, I also believe firmly that there is a place for and considerable benefit in calibration and back-analysis exercises.

/3 "To successfully validate..." rather than "To successfully verify..."?

8/1 Include chemical (geochemical?) in first sentence? (See last bullet of third paragraph).

8/6 The term "ventilation ducts" usually refers to pipes and/or tubes installed in shafts, tunnels, drifts, etc.. The term "ventilation airways" may be more appropriate if excavations in rock are intended - however this seems to be covered sufficiently by the listed "shafts, drifts, etc.."

8/7 First bullet: it is not obvious that the air will be cooler, at least during the summer, for the early decades of repository operation.

9/2 Condensation seems highly likely for the exhaust system, more so than for isolated chambers.

9/5 "rock mechanical deformations" would seem more likely for an unconfined knob than "rock mechanical stresses"

34/1 I question whether stress fields in an unconfined knob could reach a magnitude where they affect permeability, especially matrix permeability.

36/1 Again, I believe calibration exercises would be useful. As a minimum, they could provide considerable insight into scale effects on parameter input values.

37 Task 14: I wish baseline monitoring could be started earlier, e.g. preferably ASAP with regard to thermally induced deformations of the site

MEMORANDUM FOR: Melvin Silberberg, Branch Chief
Waste Management Branch
Division of Engineering
Office of Nuclear Regulatory Research

FROM: David Brooks, Acting Branch Chief
Hydrology and Systems Performance Branch
Division of High Level Waste Management
NMSS

SUBJECT: COMMENTS ON UNIVERSITY OF ARIZONA RESEARCH PROPOSAL FOR FIELD SCALE HEATER TEST (9A1K, L20057)

As requested in your March 15, 1991, memorandum to Ronald Ballard, the Hydrologic Transport Section staff has reviewed the University of Arizona's experimental plan for a field scale heater test. It should be noted that the staff was also involved in the January 1991 discussion of the proposed project at the workshop in Tucson, Arizona.

Over the past two years, NMSS and RES have been in the process of developing "detailed user needs" statements for all NRC research done in support of the NMSS/DHLWM program (see Memorandum, Browning to Shao, 1/19/90). In general, prior to beginning any research project, the NMSS/DHLWM need for such work must be adequately established. The first step in this process is to (jointly) develop a "detailed user need request" from NMSS/DHLWM. Such a user need request establishes the specific licensing staff needs and determines that research being done by other organizations is not sufficient or adequate for NRC needs. This "first" step can be initiated by either NMSS/DHLWM or RES. Once a draft "detailed user need request" has been prepared, it should be discussed with the appropriate NMSS or RES staff and then finalized. Following the development of such a "detailed user need request," RES would prepare a proposal request or work plan that would then be reviewed by NMSS. This proposed "heater test" project appears to be developing independently of this agreed-to process, and thus it is not responding to a "detailed user need request."

In general, the DHLWM staff has the following concerns, which were also discussed in the evening session at the January 1991 Workshop on Flow and Transport Through Fractured Rock at the University of Arizona. The three main issues, about which there was considerable discussion, were (1) characterizing the site, (2) controlling and measuring boundary conditions, and (3) the usefulness of the experiment for validating existing models. There did not appear to be any consensus on the utility of the proposed research because of these major issues. It is noted that because of the issues raised at the discussion session, the reporting on the proposed heater test experimental plan was dropped from the agenda of the NRC/University of Arizona research review meeting held Friday following the Tucson Workshop. To our knowledge, these concerns remain unresolved and would need to be addressed in the user need justification for NRC sponsored research.

In general, the staff thinks that, for NRC, more experimental work should be done at the "large block scale" prior to beginning a field scale thermo-hydrologic experiment. In addition, we suggest that further consideration of such NRC sponsored research be delayed until the data obtained from the prototype drilling test (USGS) and the electric heater test (LLNL) (both from G-Tunnel) are integrated into a single thermohydrologic data set and the proposed evaluations made. This work is part of a proposal to the Office of Geologic Disposal, US DOE, prepared by Charlie Voss, Golder Associates Inc. (letter of August 2, 1990 to Carl Gertz). Once the information from these completed heater experiments have been evaluated, then possibly arguments could be made in the "detailed user need request" for complementary or larger scale NRC sponsored heater test research. Also, the information and findings from the thermohydrologic study, presently being conducted at the Center for Nuclear Waste Regulatory Analyses, needs to be factored into the supporting information for any proposed field "heater test."

With respect to the University of Arizona "Experimental Plan," some specific comments have been made which are enclosed. These comments were prepared by Rex Wescott, Don Chery, and Bill Ford of the Hydrologic Transport Section.

/s/

David Brooks, Acting Branch Chief
Hydrology and Systems Performance Branch
Division of High-Level Waste Management
Office of Nuclear Material Safety
and Safeguards

Enclosure:
As stated

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NMSS/DHLWM HYDROLOGIC TRANSPORT SECTION COMMENTS

on

EXPERIMENTAL PLAN

NONISOTHERMAL HYDROLOGIC TRANSPORT STUDY AT THE APACHE LEAP TUFF SITE

by

UNIVERSITY OF ARIZONA
submitted
JANUARY 1991

GENERAL:

1. The proposal should contain a detailed statement of need for the experiment. This statement of need should include a greater discussion of results from the G-Tunnel Experiment and what the heater experiment would add to our understanding of nonisothermal hydrologic transport.

2. It is not apparent that enough knowledge has been gained from laboratory experiments with blocks to determine the relative sensitivity of various heat transfer and flow parameters to fracture and matrix properties. This knowledge is necessary to successfully design a field heater experiment in fractured rock.

SPECIFIC:

1. Page 27- From the statistical analysis of bulk density and effective porosity of 105 samples from the Apache Leap injection site, the tuff appears to be relatively homogeneous, yet the saturated hydraulic conductivity has a coefficient of variation of 300%. The coefficient of variation in the results of field test methods for determining hydraulic conductivity is about 700% and ranges over 5 orders of magnitude. This variation indicates that accurate hydrologic characterization of the heater site may be extremely difficult and probably can not be aided by correlation with more readily measured rock properties.

2. Page 28- Third paragraph- The discussion indicates relatively good agreement regarding thermal conductivity between measurements in core segments and observations of response to the annual thermal cycle. This would indicate that the influence of fractures on thermal conductivity in tuff is minor. More details should be provided to support a conclusion which greatly influences the feasibility of the proposed experiment.

3. Section 3.2- Resistivity appears to be to temperature dependent to give dependable moisture indications. Will neutron logging be able to effectively replace it ?