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NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

NATURAL AND ARCHEOLOGICAL ANALOGUES

April 17, 1991

Peppermill Hotel
2707 South Virginia Avenue
Reno, Nevada

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I N D E X

SPEAKERS: PAGE NO.

P R O C E E D I N G S

(8:30 a.m.)

DR. DEERE: Good morning, ladies and gentlemen. We are ready to begin today's session. As you would imagine, there are a great number of the board members who are on panels that are vitally interested in the information that you presented yesterday and the information that you will be presenting today. These include our panels on the Engineered Barrier System, on Environment and Public Health, and Groundwater and Geochemistry, amongst others.

The chairman of this morning's session will be the chairman of one of those particular panels, Dr. Warner North, who is chairman of the panel on Risk and Performance Analysis. Warner?

DR. NORTH: Thank you, Dr. Deere.

On the second day of our meeting, we're going to continue to hear from the Department of Energy. Russell Dyer is going to give us an introduction to the Yucca Mountain Project use of natural analogues and introduce the remaining Department of Energy speakers.

DR. DYER: Thank you, Dr. North.

Again, what I have in mind this morning is a brief introduction to a distinguished line of speakers here. There perhaps is a perception on some people's part that analogues play little or no part in the Yucca Mountain Project program and I would like to put that fear or perception to rest and categorically state this is our philosophy for the use of natural analogues. They're an integral part of the business of doing earth sciences. What we would like to do is to show you a smattering, certainly not a full sampling, of some of the places of which analogues are an intimate part of our program.

This is a slide I put up yesterday and I'll just reiterate it again. Earth science, in general, has a triad of fields of endeavor that make it up; theoretical, experimental, and observational. And, the things you will hear about today are primarily concerned with this part of this triad: passive observation in order to identify processes that may be important in a repository setting; efforts to bound process rates; and finally, if we involve modeling also, the observation of the response of a stressed system.

Just to remind you, our working definition of an analogue is a very broad definition. Our working definition is a very broad definition and it primarily is based around a process oriented definition and it's those processes analogous to those that may exist at a site being characterized as a potential repository and/or induced by the storage of radioactive waste. This would include things like thermal effects, radiolysis, hydrothermal effects.

And, another thing that we touched on yesterday was in this natural analogue, how natural must an analogue be to convey useful information? And, we will contend that some anthropogenic analogues provide important and possibly unique information on processes that might operate a potential repository. You'll hear Everett Springer and some other speakers today speak about this issue.

There's no single, exact natural analogue for a site being characterized, any site being characterized, for its suitability for geological disposal of radioactive waste. Because there is no unique natural analogue site for Yucca Mountain, we're concentrating on identifying the processes and the process rates which may be operative at Yucca Mountain now and in the future in the undisturbed system and in a perturbed or disturbed system. These studies are being conducted at numerous sites, including Yucca Mountain, and you'll hear Dave Bish give what, I think, is a very excellent talk on using Yucca Mountain as an analogue for Yucca Mountain.

Process-oriented analogue studies carried out by the Yucca Mountain Site Characterization Project include these topical categories: climate change; geochemistry and transport; hydrology and flow; tectonics; material behavior; et cetera, et cetera. And, we will try to give you a representative sampling of these categories. In general, these are components of studies outlined in the site characterization plan, the implementing study plans, or scientific investigation plans.

This is a repeat of a slide from yesterday. There are three major outcomes or uses of information gained from analogue studies: quantitative in nature in which we can use the results from analogue study to assist in the validation process of process models perhaps that are rolled up into performance assessment models; qualitative, certainly, just the recognition of operative processes, the scoping phase, recognition of signposts; communicating technical information to those not technically trained, we talked about that quite a bit yesterday; and finally, communications within the scientific and technical community.

This is our lineup for this morning. The first speaker will be Julie Canepa of Los Alamos who will talk about the role of analogues in the radionuclide transport program and the point we're going to try to make here is that in order for an analogue study to be useful, it must be focused, it must have a discrete question that you want to answer. Dave Curtis yesterday talked about one of the results of going in and getting more answers than you had questions for in the initial phases. And then, we're going to have a series of individual talks on specific topics. Hydrothermal analogues will be presented by Carol Bruton of Lawrence Livermore. Dwight Hoxie of the USGS will talk about paleoclimate and hydrological analogues. Burt Johnson of Pacific Northwest

Labs will give a talk on ancient materials as analogues for repository materials. I think you'll find this every bit as interesting as Ike Winograd's talk yesterday. Bill Bourcier of Lawrence Livermore will talk about natural analogues for nuclear waste glass. Everett Springer of Los Alamos will talk about anthropogenic analogues, some of the studies of radionuclide transport at Department of Energy sites, not Yucca Mountain, but some of the weapons related programs. Dave Bish of Los Alamos will talk about Yucca Mountain as a natural analogue to repository-induced alteration.

Let me turn the speakership here over to Julie Canepa of Los Alamos. Julie has been with Los Alamos for five years. In 1987, she became the technical coordinator for site characterization activities and, in 1988, was appointed project leader for site and regulatory investigations, overseeing site characterization, performance assessment, and regulatory interaction activities. She will speak to us on the role of analogues in the radionuclide transport program.

DR. NORTH: I think before she proceeds, we might have a comment on the agenda. There is a typed agenda being prepared, but we're having some computer difficulty. In addition to the speakers that have just been listed for the Department of Energy, we are also going to have Rod Ewing from the University of New Mexico speaking on natural glass systems. Now, according to the agenda I have before me, we may get all of this in before lunch and then we'll have the general round table after lunch, extending about two hours.

Please, go ahead?

DR. CANEPA: Well, from my introduction, you can tell that I'm a manager at Los Alamos, but one that is very interested in how we're going to integrate a natural analogue program within the radionuclide transport program. It's my responsibility to make that integration and make natural analogue studies work, make sure that they are clear and concise in their objectives. I think it's a big task. We've heard some really very interesting talks on natural analogues, but in my mind, after the day, it's made my job even more confusing in some instances. But, in other instances, I think we're well on the road to having very good analogues for some of the processes we're interested in.

I'd like to get off to an auspicious start by starting with the fourth viewgraph. As you will hear in my talk, you've heard a lot of these concepts before, but we're going to repeat them this morning. Analogues may provide evidence that we understand radionuclide transport. And, I took this information from bullets from this International Atomic Energy Agency report, 1989 report. Basically, that report says that analogues can be used to define situations where models or concepts can be applied. Conversely, analogues can be used to identify concepts/processes that maybe your conceptual models did not include and I think Mike Shea showed a good example or several good examples of that, how natural analogues helped in that area. Analogues can be used to validate assumptions. Analogues may not be seen as selective evidence, but analogues may strengthen your scientific credibility.

So now, we'll go back to the first viewgraph. Now, how would analogues support our radionuclide transport program. When I was thinking about that, I had to think about where are the areas within our program where analogues would be useful. From our perspective, natural analogues came to use to validate our understanding of radionuclide transport processes, solubility, sorption, diffusion, dispersion. Natural analogues can be used to validate our conceptual models for radionuclide transport and I'd like to focus specifically to transport at Yucca Mountain. Natural analogues will be useful in developing a conceptual model and then possibly testing that conceptual model.

Other ways analogues can be used and the way we're hoping to use them within the radionuclide transport program would be to validate our computational models. Basically, our computational model should embody our conceptual models; therefore, natural analogues can be used to help construct our computational codes. But, basically, I think that with a fairly good data base, we may be able to test our code predictive capability. And, we're not heavily involved in the performance assessment area, but the reality is that natural analogues are used or could be used to validate the applica- tion of performance modeling to long-term transport processes.

Analogues may also provide a piece of the puzzle. Key parameters that are missing from within our laboratory or control field tests, parameters that we can't obtain in our laboratory and field tests. Transport processes, the possibility that some transport processes we may be able to understand more about them through analogues than we can through laboratory or field tests. It certainly provides you the time-scale that we're interested in and we're interested in the size-scale. Controlled field tests probably won't be done any larger than 10 meter by 10 meter blocks and what's the application to a repository scale? And, all our computational codes will probably be repository scale type calculations. So, size-scale is an important parameter that we may be able to get a handle on through the use of natural analogues.

A little history here, natural analogues were discussed in the site characterization plan. It was discussed in Chapter 4 of the site characterization plan. Chapter 4 was the geochemistry section and Chapter 4 was also the data chapter. It was basically data that we knew about the 1987 time frame. Arend Meijer wrote this particular section in the

Chapter 4. He discussed warm and hot springs and in the Section 4 he has a fairly lengthy discussion of hydrothermal alteration looking at Yellowstone, specifically at Yellowstone. And, he was interested in looking at the mineral alteration, looking at the water chemistry. Basically, it presented a system if there was a fair amount of data, could we predict the alteration that we saw there or could we predict the water chemistry that we saw. We also felt that possibly Yellowstone would be a good analogue to test the capability of EQ-3/6.

He also discussed Oklo, he discussed Alligator Rivers, and he had a--I don't know if it was a real lengthy discussion, but he had key information presented on anthropogenic analogues, the Cambric test, and the subsequent well pumping information, and the area near Los Alamos, Technical Area 21. We call this Technical Area 21, Area T. You'll hear it called the DP site. There's a lot of terms used to describe this area in Los Alamos. Basically, in the late 40's and early 50's, the lab was processing plutonium and they built these waste beds. They engineered them with cobble and clay and, basically, it captured the outfall from the plutonium process waste facility. That shut down in the early 50's, but then there has been a history to those waste beds later on through the 50's and early 60's. And, there has been evidence of radionuclide migration into the Bandelier tuff which makes up the Pajarito Plateau. The Bandelier tuff is a non-welded to moderately welded tuff that Los Alamos sits on. And, Everett Springer will be talking in detail on this particular waste site and he will also be presenting information on Cambric. Some of that information is similar to what Larry Ramspott presented yesterday. So, those were the discussions that were in Chapter 4.

Then, in Chapter 8, as you're aware, we were writing our plans and what we were going to continue to do as part of the site characterization program. And, so I outlined the sections in Chapter 8 where there's some discussion of natural analogues. Basically, for your interest if you wanted to look it up, for mineral alteration and water chemistry analogues, basically we wrote a section in Chapter 8 that described hydrothermal alteration. We didn't write about Yellowstone. Arend made the decision that in order to really gain a more useful information from Yellowstone, we would have to get back at the site, we would have to convince the Park Service to drill a hole, things like that. And, I think that's maybe a bigger brick wall than the permitting problem in Nevada. But, the water chemistry and mineral alteration information can be gained if we look at a hydrothermal alteration site. Livermore has taken the lead in that area and it's more appropriate that they do from their near-field studies and they have done a selection. They've looked at Yellowstone, they've looked at other hydrothermal areas and Carol Bruton will be presenting their, basically, selection criteria and their choice of New Zealand hydrothermal system. She'll have a more detailed discussion of how they went about making that choice and what they plan to do.

In terms of tracer migration analogues, you've heard about chloride-36, you've heard about carbon-14 tritium. These are the SCP sections. They represent the water movement test. They represent hydrochemistry tests in the saturated zone and unsaturated zone. I won't get into these because you certainly know about them. Noble gases is another possibility to use as a tracer analogue. Bill Steinkampt from the U.S. Geological Survey is interested in it. It's a little bit in a formative stage, but he is working with Jane Poths from Los Alamos. I think Jane is working with Dave Curtis in the isotope nuclear chemistry group. And, there's a possibility of collaboration there in use of noble gases. The other possibility may be technetium-99. Certainly, Dave Curtis' lab has the capability, but we're a little uncertain of whether we should pursue technetium-99. I think there needs to probably be some significant work. We may not need to, but that's a possibility.

The natural analogues from a transport perspective, the Oklo type analogues, the uranium mine deposit type analogues, are part of our overall strategy, albeit our strategy in some instances is not very well defined, in other instances it is. Our validation strategy consists of laboratory experiments and field tests. Our laboratory experiments are described in these SCP sections. You have heard Bob Runberg discuss the dynamic transport column experiments. Some members of your committee and others in the audience have heard Ines Triay at our absorption workshop and our Nuclear Regulatory Commission technical exchange discuss our dynamic transport column experiments. We also hope to scale up these experiments to large block experiments. Right now, the only tests or interaction we have is with the Canadians and this is an interaction with the international programs which Bob Levich is working on and establishing. And, we're going to be working with Chuck Vandergraf and it will be Bob Runberg working with Chuck Vandergraf. And, initially, they're going to be looking at some large granite blocks, fractured granite blocks. But, we hope to extend the studies to, obviously, tuff fracture block. That will demand that we get a sample of tuff and we--a large block sample and, hopefully, a discrete fracture. We will plan on probably doing--I believe that the agreement suggests that we will be doing a tuff block experiment at White Shelf and we will also be developing the capability to handle tuff blocks in the laboratory in Los Alamos.

This particular section here, field validation tests, is kind of a little undefined right now, but this section contains the concept that natural analogues fit in a field validation strategy, fits with our controlled field tests. Right now, we're focusing more on controlled field tests. We have intermediate scale tests and large scale tests. We think large scale, in terms of physically being able to do a test, is about a 10 meter by 10 meter block. The intermediate scale test, we're planning a caisson experiment at Los Alamos and that was part of the performance assessment group. We're going to be designing and instrumenting and filling the caisson this fiscal year. We'll be conducting--it has to settle over the wintertime and we will be conducting caisson experiments next spring and it will be part of validation experience--I don't know--testing, if you want to define--I don't know how you define validation. That's something that's important to me. I have to define that. But, we'll be using that for the performance assessment group. So, all the people participating in performance assessment will probably be modeling this particular experiment.

Natural analogues falls in here, as does anthropogenic analogues. And, right now, admittedly, we do not have explicit plans of what analogues we're really going to be focusing in on right now. We are definitely involved in the international interactions, but these particular studies, we're looking to Dave Curtis and June Fabrica-Martin to help us assist in developing what explicit studies we should be looking at and we probably will be working with the environmental restoration program at Los Alamos and looking specifically at Technical Area 21, our plutonium process outfall area.

We always interact with the radionuclide migration project as part of the NTS. Bob Runberg and others are partially funded by that particular working group, as well as Yucca Mountain. So, the reason why we are involved in radionuclide transport for Yucca Mountain Project is because of that initial work at the test site. So, we've been able to take that technology and take what we've learned from the NTS and apply it to Yucca Mountain radionuclide transport investigations.

You've seen this before, you've seen a lot of this before. But, basically, now we have to get down to if we are focusing in on natural analogues, what do we want to focus in on? What processes are we interested in? And, all I've listed here are processes and possible analogue sites. Mineral alteration, water chemistry, we looked at Yellowstone, probably won't go back there. Carol Bruton is going to be discussing the New Zealand geothermal system. And then, Dave Bish will be discussing Yucca Mountain as an analogue to the possible hydrothermal alteration that may occur at Yucca Mountain.

Solubility, speciation of radionuclides, we could gain information from uranium deposits, mill tailings, certainly from the Cambric test, and possibly the anthropogenic analogue at TA-21. Radionuclide sorption, I think that the work that the U.S. Geological Survey is going to be doing on carbon-14, albeit it's difficult to interpret, but I think we may be able to get some information regarding radionuclide sorption from that work. Uranium deposits, specifically Cigar Lake that has the clay halo, we may be able to learn something about radionuclide sorption working in that international program looking at sorption in the clay halo. Cambric test, Pajarito Plateau also provide radionuclide sorption information. Matrix diffusion, if we're interested, uranium deposits once again. June Fabrica-Martin provided basically this outline and I've basically stolen that from her and made these viewgraphs. But, chlorine in tuff invaded by sea water is also another possibility. I asked her where that might be and she said that the information and literature where she got that information didn't say that there was any explicit sites where that's occurred.

Fracture transport, not exactly a process in and of itself. Obviously, a couple processes are involved here. But, Yucca Mountain itself will provide information, as well as these other areas, looking at tracer migration, using these tracers to get a handle on fracture transport. Rainier Mesa is a fractured area that basically has a higher recharge than Yucca Mountain and could be used--I think Dwight is going to be talking a little bit about that--but may represent an analogue to a higher recharge environment. This might be a scenario that we would have to deal with in evaluating Yucca Mountain. Apache Leap tuff is fractured. Pajarito Plateau, Bandelier tuff is also fractured.

Colloid transport, this is an area that I'm very interested in right now, so interested in it that I cut the funding at Los Alamos with regards to the colloid area. But, the problem was, we had lots of good scientific research going on in colloid transport, but it wasn't focused to help really get at some of the questions on source of colloids. Colloid transport, I guess I'm a believer that, yes, that transport mechanism exists, but I don't know how significant it is. And, we just couldn't get a handle on how to focus our research to get a handle on the significance of colloid transport. Maybe, we can use natural analogues. Maybe, that will be a way to have a good use of natural analogues to get a handle on the significance of colloid transport whether that mechanism will be significant at Yucca Mountain. And, I invite any comments people have with helping us to find the source and define the significance of transport to help us with that.

Chesire, there is an indication that at the Chesire event that there was colloid transport. That was a 200 kiloton event. I don't know how much that helped. So, you've got some processes that need to be decoupled there. Uranium deposits, it possibly depends what the matrix was, I suppose. Pajarito Plateau, as Everett will present, he will discuss some of the results from this migration pattern that we've seen. A colloid transport type model fits the data, okay, but so do two other models. So, that's a possibility.

This just repeats the kinds of analogue sites we have. This would be natural analogue sites for migration in tuff which you're familiar with. These areas, of course, are not tuff, are nontuff areas. So, if we are involved in this area, we need to be very explicit what we hope to get out of some of these international interactions.

These are the anthropogenic analogues which you have seen. We're interacting with the hydrology radionuclide project only from a sense that we are aware of their data, but we are not explicitly involved in any specific testing activities. I think we'll be involved--this particular work for the environmental restoration program at Los Alamos is getting underway. It's part of everybody's five year plan. We hope to take advantage of a lot of the work that's going on here and Everett will be discussing that. Rainier Mesa, it's a natural analogue, but it also is an anthropogenic. There's been a lot of testing in Rainier Mesa. And then, I don't know anything really about the Hanford or Idaho National Engineering Laboratory, but other DOE sites around have their environmental problems and we may be able to learn or gain information about radionuclide transport by staying in touch with those involved with environmental restoration.

The next set of slides are pure management. These are my concerns, these are my concerns about use of natural analogues, and how we might develop increased--basically, develop a program to study natural analogues. It's very important to me these objectives must be clear. From my perspective, we have to carefully consider the allocation of the resources. And then, this is important, this is reality, this is the life we live in as participants in the Yucca Mountain Project, validation in the strictest sense must be done within a quality assurance program.

And, I'm just going to outline each one of those bullets, nothing new, but we must determine what these objectives are. We must determine are we going to validate a process, are we going to validate assumptions, are we going to go big time and validate our conceptual models? We have to be very clear. Maybe, one of the ways to help us prioritize what kind of analogue system to look at or processes to look at is look at the processes that seem to be significant in terms of a performance assessment. One example is fracture flow. Okay? Performance modeling seems to be sensitive to this particular fracture transport. This is a couple processes of fracture transport. So, maybe that's one way of helping prioritize the processes that we're going to validate. There may be other ways. I'm not informed in the literature. Other people have probably written papers on how to go about defining your selection criteria and how to prioritize, but that's something that I hope we can learn as time goes on here.

Careful allocation of resources. Analogue initial conditions are very difficult to define, not just natural analogue conditions, but the anthropogenic analogues. TA-21, we don't really know the source. We know plutonium was emplaced in these waste fall beds. We're also not sure what else was put in there. Also, there is a couple processes. Do I have to go back and figure out the whole hydrologic history of Oklo for the last two billion years? I mean, basically, you can exhaust all your funds solving those kinds of technical questions and yet I have a responsibility to study and characterize the Yucca Mountain system. These are really hard. I, personally, am not interested in funding a site characterization program for a site other than Yucca Mountain. So, we have to get back to that clear objective. It has to be very clear.

Validation in the strictest sense must be done within a quality assurance program. I use quantitative and qualitative. I'm not quite certain I really still understand what these mean, but to do validation within a quality assurance program, we can do that easily if the studies are conducted under the auspices of the Yucca Mountain Project and under the rigor of the quality assurance program that we have. It's been very painful, but we spent three years developing our quality program. It's there, it's working. I can assure that validation, if we're in control of the actual tests, can be done under the quality assurance program. I cannot guarantee that for programs outside Yucca Mountain. And, if we use data from programs outside Yucca Mountain--and, I think I would have to probably include the International interactions in that area--if we use that data for validation, however that's defined--okay, the jury is still out--I may have to qualify that data and at this stage that may not be cost-effective. We've had experiences in how to qualify core and it was frightening. And, so I haven't basically embarked on a process to qualify data, even though there's a procedure that describes it, but I don't know if I want to be the first one to try to qualify data in the program. But, we've discovered that in some instances, it may not be cost-effective.

If we use analogues qualitatively, whatever that means, maybe the quality program need not apply. That offends a significant number of the principal investigators that are working very well--it's hard--but are working within the quality program. I'm worried that half my investigators will jump ship and go over to the programs that, hey, quality doesn't apply. But, these are management choices and I don't really want to be the one to have to make that decision, but right now, I think I am. That's a concern of mine. It's a real concern of mine.

DR. EWING: Excuse me, could I ask a question?

DR. CANEPA: Sure?

DR. EWING: How do you handle data in the published literature?

DR. CANEPA: For some reason, that is acceptable within the Yucca Mountain quality assurance system; however, I think when the project finally gets to the point where the data that's going to be used for licensing--DOE is going to have to help me here--I think there may be a qualification type step, some sort of analysis to look at that literature data--if it's going to be used for licensing, it's in their data bases--to make some judgment that it's to be used. I don't exactly know the explicit requirements. Maybe, DOE can help me.

MR. DYER: Yeah, let me step in a little bit, Rod. We can use published data of the literature's confirmatory data. There are four or five different ways that we can qualify that other information, one of which is to compare data within the published literature.

DR. CANEPA: We're going to have to address it and we're going to have to--I don't know if we have to address it soon, but a lot of our computational codes, even EQ-3/6, we've got thermodynamic data bases that we use. Okay? You know, and there's been lots of work going on to make sure that the data in there is good. But, I'm not sure it's been done in a process that's acceptable from whatever the--following this particular procedure, but it's going to be something we're going to have to deal with.

Basically, from my perspective, our scientific contributions from Los Alamos, I have the responsibility to provide the best data, the best possible understanding of radionuclide transport, so that DOE can assess the potential for transport. Therefore, the data that we obtain must be done under acceptable quality assurance programs. All the processes and models that we use must be validated as part of that program. So, therein lies the difficulty for me in figuring out how to use natural analogue programs that may not be conducted under the auspices of the Yucca Mountain Project.

But, in general, I think we have a somewhat varied program with regards to natural analogues. I feel very comfortable with the kind of analogue systems that we're studying with regards to mineral evolution and alteration. I think what Livermore is going to be doing and New Zealand and certainly our studies at Yucca Mountain itself are going to be adequate. I believe the water chemistry information that is gained from understanding what the rock water alteration, what those reactions are, will help with our water chemistry understanding both from Yucca Mountain and New Zealand. I'm very comfortable with the tracer migration type work, chlorine-36, the tritium, carbon-14. I think that's probably an area that I don't think we need to be pursuing much more. I think we've got a handle on that.

With regards to radionuclide sorption, we haven't really pursued analogue systems, but now I have to step back. We've done a development of the radionuclide sorption program and I think we've gone through some throes of trying to focus that program and prioritize it and we've made several presentations on how we're prioritizing our sorption program and Dr. Langmuir has been present at most of that painful exercise of going through that process. I think that we've gotten a handle on how to prioritize and how to possibly apply our radionuclide sorption work. And, maybe analogues can help us in that area. We now are defining--maybe doing our work to define what is the most active water chemistry. And, an analogues system may be useful in helping us define what that most water chemistry is and then we can do our radionuclide sorption work.

Another area within the radionuclide sorption program, I won't go through how we prioritized it, but we focused on a couple of nuclides that we may have to go into more theoretical understanding of the sorption processes and these would be technetium and neptunium and, I believe, you know, what we learn from technetium work from some analogue systems and possibly--I don't know quite what to do with neptunium--but, maybe given that prioritized work, analogues can give us a handle on some of these key low-sorping nuclides that maybe we have to invoke a more theoretical understanding of sorption in order to credibly say sorption applies in a transport scenario. Maybe, natural analogues would help in that area. These are things that I was thinking of yesterday as I was listening to the talk.

In terms of solubility speciation, I think the international programs would be very helpful in some of the those areas. I can't speak to anything we can really focus on right now and, hopefully, our principal investigators will spend some time thinking about that. Colloid transport, that was one area where I already mentioned that I think in order to get a handle on the significance of transport, maybe an analogue system would be very, very helpful in that area.

So, these are my thoughts and, in some instances, we have a fairly well-developed program and, in others, they're in the beginning stages. We have had, at least in Los Alamos, a consistent level of funding, consistent but on a downturn. And, basically, any new analogue investigations, I feel, are new work. Okay? And, so I'm not real interested in diverting some of our resources to a couple FTEs to, you know, jump in there and really develop a good analogue program. Those are hard decisions. I would have to--it may not be the same people I already have employed and I have a--it's a hard decision to make to defer those resources, remove that particular part of what I feel is a core program to embark on a natural analogue effort. And, so those are some of the struggles that I have right now. But, I thank you for allowing me to present my thoughts to you.

DR. NORTH: Thank you for presenting your thoughts including some of your concerns and worries and the difficulty of your decisions because I think that gives us an insight of the difficulties that you wrestle with that is much more helpful to the board than simply a presentation of the data and the programs. So, I thank you for the added part of your presentation and hope that others will do likewise.

DR. CANEPA: You're welcome.

DR. NORTH: Are there any other comments or questions by other members of the board?

(No response.)

DR. NORTH: Okay. In the interest of time, let us then push on.

DR. LANGMUIR: Warner?

DR. NORTH: Yes, sure. Sorry.

DR. LANGMUIR: Julie, this may not be the time of the day for questions or suggestion, but has DOE considered looking at waters in the unsat zone in J-13 in waters which would evolve from water-rock interaction at the higher temperatures around a repository and looked at what kinds of radionuclides would naturally occur in those? And, they'll be there. You can analyze uranium, radium, thorium, radon isotopes in those waters. Their distributions between the solutions that you found in the rocks that are there would give you a very good in-situ handle on the Kds. It's ocean properties in those elements which would then be at higher levels given a repository breach. But, still, the processes would be the same. And, that's, I think, a quick cut to the kinds of interactions you might anticipate.

DR. CANEPA: Well, I know that there has been--I think the only work that, so far, has been done has been in the saturated waters and looking at uranium-thorium disequilibrium, I believe. And, actually, I think it was J.C. Low that did some analyses and, at least from looking at that data, confirmed some of our Kds for uranium and it also confirmed actually the thorium information that we gained from that. Also, thorium is extremely insoluble. So, it actually validated our solubility information. But, Steinkampt, I think, is interested--that's going to be the USGS's hydrochemistry responsibility and, from my knowledge of interactions with them, they are planning on looking at some of these natural occurring elements. But, in the unsat zone, getting beyond the extremely difficult laboratory analytical problems that Al Yang, I mean he's working on, I really don't know the program well enough, but the GS maybe--maybe Dwight can--I don't know if you know--if they're going to try to do some analysis on the pore water chemistry. And then, again, Livermore who has got more of the responsibility of the rock water hydrothermal interaction, I don't know if that's something--I guess, we don't quite have the experts here. But, yeah, you brought this topic up a few times.

DR. LANGMUIR: I'll keep mentioning it.

DR. CANEPA: You'll keep mentioning it? Okay, all right.

DR. NORTH: And, it will probably come back a few more times.

DR. CANEPA: Bring money next time. You're not allowed to come to Los Alamos without it.

MR. SAUCIER: I hesitate to take any of your time because I know you're under pressure to get finished before noon. But, my name is Gene Saucier. I'm an independent geologist here in Reno. And, for about 20 years, I've looked for uranium and I've made an effort over these 20 years to keep up with descriptions of all the major deposits that have come out. And, something that's striking here as far as using uranium deposits as natural analogues--and I'm really surprised that this hasn't been addressed--the one thing that we've learned in keeping up and examining all these uranium deposits is that they're almost always associated with tuffs or tuffaceous matter in the host rock. And, the thing that is almost always there is organic matter and it's not the bituminous type of organic matter, it's humic type organic matter that has been around for two and a half billion years. And, the French have done a lot of work on this. They've sampled all the major unconformity deposits and all the deposits and, as far as I know, they've concluded that this organic matter is always present. It's present in the northern territories in Australia and all the unconformity things in the Athabasca Basin.

The striking thing about this and the important thing is that we know that organics can transport these elements. And, there's many of us in the industry in the exploration part of it that think that these organics control the deposit. They're responsible for accumulating the deposit. And, not only that there's strong evidence that these organics help protect the deposit, they become a refractory substance encapsulating the element. And, it's striking that Yucca Mountain is--as a matter of fact, all the sites have been picked to be as inorganic as possible. They want to stay away from organics. Yet, the analogues are rich in organics and none of this research is addressing that problem. So, it calls into serious question after all this money is going to be spent so people can get up and say, yes, but you didn't study the organic connection and, you know, you're staying away--it's no organics, essentially, in the waste disposal site, except that if it's breached and groundwater comes in, they will be carrying organics. And, these are colloids and that is an area that I think really should be addressed.

DR. NORTH: Well, thank you for a most provocative comment. I hope, perhaps, we can get some expansion of this during the discussion period this afternoon. Also, if there's some reference material that you can provide us with in this general area, that would be very useful.

Are there any other comments or questions?

(No response.)

DR. NORTH: Well, let's proceed then with Dr. Bruton.

DR. DYER: Our next speaker is Carol Bruton of Lawrence Livermore National Labs. Carol has been at Livermore for four years. She's a geochemist. Her research includes geochemical simulation of reaction among spent fuel and glass waste forms, groundwater and tuff and geochemical correlation of digenetic mineralogy, mineral chemistry, and fluid composition at Yucca Mountain. And, her talk will be on natural hydrothermal analogues.

DR. BRUTON: Thank you.

The work of the geochemistry and mineralogy group at Livermore, one of the goals has been to characterize the chemical and mineralogic properties of the near-field environment after the emplacement of the waste. In view of that, one of the most important things that's going to happen after you emplace the waste are the interactions between the host rock and the fluids in the environment and these fluids are going to be the unsaturated waters and these waters are going to evaporate. So, you're going to get a vapor phase and you're going to get a condensed phase farther out. It's been our goal to study these fluid-rock interactions using, up to this point, a two-pronged attack; an experimental attack doing laboratory experiments to simulate these interactions and then using theoretical geochemical modeling to simulate these fluid-rock interactions, and then coordinating these two areas.

Now, what we want to move in now is the necessary third part of that study. That is selecting the hydrothermal natural analogues. And, it's necessary to look at natural analogues because we must demonstrate our ability to simulate fluid-rock interactions in complex natural systems and systems that have existed over much longer time periods, thousands of years, than are attainable in the laboratory. By doing these natural analogues, we hope to enhance our ability to forecast potential fluid-rock interactions at Yucca Mountain and then increase our confidence in our predictions of system performance.

There are two roles that we hope our analogue will fill. It's funny, I did these viewgraphs independently of Russ and Julie, but the same topics come out. There's a quantitative aim and a qualitative aim. And, for the quantitative, I took some definition or roles from Chapman who was a member of the natural analogue working group. And, as a quantitative analogue, we want our analogue to serve as a natural experiment which replicate a process, or a group of processes, which are being considered in a model. Now, our models that we'll be testing are geochemical models of fluid-rock interaction, like EQ-3/6 and versions of EQ-3/6 which have been enhanced for kinetics and to take into account fluid flow. And, these processes that we're talking about are fluid-rock interactions, mineral precipitation, mineral dissolution, and the rates of these reactions, and processes such as cation exchange and sorption between fluids and rocks. But, we also have a qualitative goal in doing these studies and that's--I think Russ talked about it. He called it as a recognition of the operative processes. Do our models embody the types of processes that are actually going to be occurring at Yucca Mountain?

While we were looking for natural analogues, as Julie mentioned, we considered ones all over the world. We had a various set of criteria. We analyzed areas in Iceland, Long Valley caldera, western U.S., Yellowstone. We looked at fields in Japan. And, looking at all these different fields, each of them had their own strengths. What we found, that the geothermal area in New Zealand satisfied a lot of our different criteria and had a lot of different pluses associated with it. So, what I'd like to do today is to give you an overview of our criteria for choosing New Zealand as a potential site, give you a quick description of what's there in terms of fluid chemistries and the mineralogy, and then to show you a purely generic approach of what we intend to do to attack this natural analogue problem, what do we intend to do when we get it, and what some of our expected results are.

Okay. Why did we choose the hydrothermal areas in New Zealand? Now, these hydrothermal, it's in the Taupo Hydrothermal or the volcanic area of New Zealand. We found that the rock type and the temperatures are similar to those in the post-emplacement Yucca Mountain environment, dominated by rhyolitic tuffs and the temperatures there go up to 300 degrees C. The hydrothermal system has been active for up to half a million years. So, we've got an element of time that we want to study. It's got extremely well-characterized geology and hydrology and that was a primary consideration in our choosing this site. In view of limited time, limited funds, we've got to choose an area that has as much information about it as we can. So, we have a lot of this groundwork already done. And, because there's been so much work done in this area, we know fluid chemistries very well, water samples, gas samples. And. also we know the secondary mineralogy is known and we can deal with it in our geochemical models; a variety of minerals, zeolites, clays, carbonates, silica polymorphs, and so on.

Now, there are also two other features that drew us to New Zealand versus other areas and the fact that this area has been developed for geothermal utilization as an energy resource. And, so human activity at the site since 1953 has noticeably altered springs and water chemistry. This gives the chance to test our ability to model perturbances in natural systems. What happens when man goes into a natural system and messes around with it? And, we predict what the effects of those interactions are. But, also in each field, they have like 100 wells drilled and they taste these wells with a variety of materials; metals, cements, and so on. And, they've been recording the degradation of these man-emplaced materials and been studying that. And, some of these materials have been subjected to temperatures up to 300 degrees C. So, it gives us an opportunity to study the chemical consequences of emplacement of man-made materials in the site. So, in essence, we have a chance to look at two natural analogues; one for man-made materials and also link that to our study of fluid-rock interaction natural analogue.

Now, the Taupo Volcanic Zone in New Zealand occurs in the North Island and I'll just give you a quick overview of what's there. We can go over details later if you're interested. But, the geothermal field is an expansion around Lake Taupo and New Zealand, occurs the convergence of two plates. So, we have a lot of hydrothermal activity because we have a series of north/northwest trending faults and these geothermal zones or different geothermal areas occur at the intersection of northeast faults and northwest faults. So, you've got high permeability fracture zones and that's what these geothermal will intersect at depth. In each of these different fields--you may have heard of some these; Wairakei and Broadlands, which has been renamed Ohaaki so you'll see this joint name--they're the ones that have been developed for resources and they have 100 wells in some of these fields. So, it's been well sampled throughout the years.

Now, the rock types in this, this is the end of an island arc and, in this area, you've got about almost 2,000 meters of volcanic materials that have been deposited. I'm going to show you an example in the Broadlands system. Into basically Pleiosene, the Quaternary, we've got a series of thousands of meters of rhyolitic vulcanism going on; ignimbrites, ash flow tuffs, rhyolite lavas, and all sorts of things that are relatively flat-lined and it has a relatively good analogy to what we see at Yucca Mountain.

We know the fluid chemistry and I'll just show you some examples of the water chemistry and the gas chemistry. Another thing that drew us to this site is the variety in the fluid chemistry. In this top diagram here, we just simplified the water chemistry in terms of salinity, in terms of parts per million sodium chloride for different geothermal fields. The salinities in all the fields are relatively low, an ion strength of less than .1. So, we won't have any problems with activity--in our geochemical models. But, you see, we have a widely varying fluid chemistry which will give us a chance to test the impact of fluid chemistry and see whether that affects fluid-rock interactions. And, we've also got compositions of gases throughout the fields dominated by CO_2 and H_2S.

Well, it's good you know the fluid chemistry, but you've also got to know how the fluids are moving in the field because these fluid and the rock interactions are occurring in highly dynamic systems. And, there's been a number of articles published on characterizing the hydrologic systems in the areas. And, here's just an example that I took from a recent AJS article by Lonker where he's just taking for a few of these geothermal wells where this is depth in meters and just try to characterize the recharge from surface--waters and then upflow from down below. So, all this data is available for us to use when simulating these fluid-rock interactions.

And, as I mentioned, for a wide variety of alteration minerals, here's just an example from the Broadlands system. On the left here, you see the stratigraphic sequence from just a specific field and there's a wide variety of alteration minerals with depth overprinted here, the temperature 100 to 120 degrees C at depth. And, just to give you an example of what minerals are there, in the near-surface we have ptilolite which is currently named mordenite--it's a very high silica zeolite which is found at Yucca Mountain--co-existing with montmorillonite, a smectite, a common association. And, as you go down, it grades into muscovites and chlorides. And, at higher temperatures in the system, you get albites, wairakite--it's a calcium aluminum zeolite--albite, epidote, and quartz, and whatever, all types of minerals that we can handle in a geochemical code.

So, a lot of information is there for us to start with, but how do we start the problem? What do we intend to do? Whether our site is New Zealand or whether it's somewhere else, how do we approach this natural analogue program? Well, there's three different paths you take. The first thing you have to do is understand the system. Then, you've got to develop scenarios that you're going to study and then you've got to carry out the actual geochemical modeling of these scenarios. And, so I'd like to talk about each one of these in turn.

Okay. Understanding the system, this is crucial. You can't devote good scenarios until you fully understand the system and its analogues to the Yucca Mountain system. Now, this field is very well known. So, the first thing we have to do is establish collaborative contacts, make efficient use of our time, take advantage of the people who really know the area. Collect available data, acquire new data as needed. We might need additional data for mineral chemistry since we have fairly sophisticated models in this regard and use all this information, develop our understanding of the physical and chemical evolution of the system. If we understand the physical and chemical evolution of the system, we could make analogies to what we expect to find at Yucca Mountain. And, we say, oh, well, we like this phenomena happening at New Zealand. We want to study that one because it focuses on a certain process we may be expecting at Yucca Mountain. And, that basically is our scenario development.

From our understanding of the system, we'll be developing specific scenarios of fluid-rock interaction and this is the key part of the natural analogue study. An example of a possible scenario is a simple situation like fluid-rock interaction accompanying fluid flow of a temperature gradient. It's a very simple manageable problem. We do not intend to develop a whole system model. This relates back to what Russ and Julie said, we can't model the whole thing. We have to really focus our study into certain specific processes and that's what we're going to do with the developed scenarios. Look at given manageable problems with relation to the Yucca Mountain. And, the choice of each scenario will embody a set of processes. Like, we may choose one scenario to test rates of mineral precipitation and dissolution on our models adequate in that regard.

And, finally, once we've developed these tightly constrained scenarios, or as tightly as we can considering it's a natural system, we want to carry out the geochemical modeling of those scenarios and carrying out that modeling using available chemical codes, such as EQ-3/6 and reactive transport modeling codes. Like, there's a version of EQ-3/6 which is coupled to a certain 1D isothermal fluid flow mode and these comparisons between the modeling results and the observed chemistries that allow us to test, refine, and gain confidence in our modeling capabilities. And, it's this comparison between results and predictions that produce our results from this study because the degree of match between the modeling results and the observed reactions will help us to identify whether our mathematical descriptions of the physical--it's the chemical processes. Cross that off, typo. It's, rather, our mathematical descriptions of the chemical processes are adequate. You see, that was what some people used as the definition of validation. We're validating our mathematical descriptions of the chemical processes in our code.

It also allows to determine whether we have correctly identified the controlling processes. We may be assuming that a certain processes are going to be controlling the fluid-rock interaction. We might find that we've totally discounted something. We may not have thought of something. This is a way to test it in the field. Also, to allow us to test whether we use appropriate data and input parameters. Now, when we do this comparison, then we proceed to refine and/or add to our models and identify additional data needs. And, that's where we go into this loop; run the scenario, do the geochemical modeling. We may need to revise a certain area and then go back in a repetitive.

Now, the ultimate questions is--I mean, people always ask this--how do you know when you've got the right answer? I mean, what is the right answer? Are you trying to match like the sodium concentration with depth? Well, it's our goal to be able to match, observe trends and fluid chemistry, mineralogy and mineral chemistry for each scenario. We do not intend to achieve a perfect match between our models and the field data. I don't think we understand the systems enough or their evolution enough to do that. But, I think if we can reproduce observed trends, we'll devote great confidence in our ability and the ability of our models to model natural systems. And, we've strengthened our case by choosing to study specific defined scenarios of fluid-rock interaction. And, it also allows us to get results throughout the study rather than just starting it and then, six years later, not having any intermediate results.

So, through this natural analogue study, what we intend to do, we hope to be able to do, is to improve the ability of the geochemical models to describe fluid-rock interaction in complex natural systems over long time periods. And, thereby, increase our confidence in forecasting fluid-rock interactions at Yucca Mountain.

Now, we're facing the same problems as everyone else in the projects right now, our budget is going down year after year and what I've described to you is the planning process that took place over this past year. But, I don't want to leave you the impression this is something we're going to be able to carry out because the geochemistry program at Livermore is being eliminated next year, essentially. So, this is something we really want to do, but in the face of budget cuts, we don't know whether we'll be able to do it. But, I think it's a worthwhile project that I hope the project considers supporting at some point in time.

Thank you.

DR. NORTH: Dr. Domenico?

DR. DOMENICO: A devil's advocate question.

DR. BRUTON: Sure.

DR. DOMENICO: If we make the assumption that the initial conditions for Yucca Mountain are, more or less, the same as the initial conditions for, let's say, the Wairakei Field 5 500,000 years ago before the thermal pulse and you've had 500,000 years to evolve the Wairakei system and your observations are made today, you only have 200 years or so of a thermal pulse in the Yucca Mountain system. So, how can we look at the effects of a half a million years of temperature and the rock-water interactions that took place over that time when we--well, let's say, we just know the initial conditions and these thermal conditions, and how do we now know what happens at the year 200 after this thing starts?

DR. BRUTON: Well, there's a range. Like some of these fields are really old. That's the oldest one. Some are only like 10,000 years old. But, that's where the advantage of New Zealand comes in because man's intrusion into the field in the short-term--I mean, it's been developed since '53, right? And so, we've lost some springs since '53 and we've produced wells. And, so we've converted water-bearing saturated zones into vapor-bearing zones because we've drawn down the water. So, we've changed our system from, say, saturated to unsaturated. There's also been cases where they've reinjected their waste water into the reservoir and looked at the cation exchange accompanying that and those are more short-term effects. So, I think if we look at the system as a whole, we can find facies has been going on for hundreds of thousands of years. But then, we'd be able to find gradations in facies that have been occurring over much shorter time spans.

DR. DOMENICO: And, those are the ones you would focus on, the short type--

DR. BRUTON: It depends on what our goals are for the Yucca Mountain Project. The study says, okay, what do we want to look at first? You know, what's of a high priority for Yucca Mountain? Do you want to study potential fluid-rock interactions in the unsaturated zone in elevated temperature? Now, if that's the case, then we might want to go to one of the areas where we find we've gone from saturated to unsaturated because of human intrusion, because of pumping the wells. It sort of depends what process we want to focus on Yucca Mountain as to what area of New Zealand we choose because there's a lot of different scenarios there, potential scenarios.

DR. NORTH: Any other questions from board members?

DR. LANGMUIR: You've touched on one of my concerns which was the analogy of saturated flowing at Wairakei or those fields to Yucca Mountain and some of the ideas that--we are looking at vapor transport now and perhaps there is some analogy there. Another question that relates to the system as opposed to Yucca which is my presumption that if we're looking at waters around a repository in the porous media, they're going to be at equilibrium with the rock reasonably soon. Whereas in a thermal field like this, you're looking at waters you're sampling at some distance from the rocks in which they equilibrated initially and they're in a state of flux in terms of their chemistry and their temperature and pressure. And, this certainly complicates your interpretation of what they mean. If you could talk about that for a second?

DR. BRUTON: Right. And, there's a variety of different areas there. I mean, they talk about--it's relatively well-studies. I mean, they produced some of these wells in a specific reservoir, aquifer. And, they say some of these fluids are produced at the speed of sound. That's how fast they come up. And, you know they catalog how much gas and steam are produced so you can put them back in, recombine the water with the gases, and then get at it that way. There's also some fields like one in the Ohaaki field. There's a big pool on the surface and that's just a hot water pool that they think came up without any fluid loss from below. We could use that as analogy because we don't have to worry about that on mixing. So, they have hot springs, warm springs, fumaroles with different degrees of unmixing of the fluids as they come up to the surface. So, we'd have to pick the ones that are best constrained. There are also some situations there that they have lake bed formations at Hula--Hula formation, I don't know how you pronounce it--which forms cap rocks with a permeable pumiceous--pumice rocks below that form aquifers. Now, there's a limited fluid flow in those, but they're hot water aquifers. Since there's no fluid flow, it's sort of at static. We may be able to look at the fluid rock interaction in that system which doesn't have--it's not vigorously flowing to test our models in that situation. That's what I'm saying, we're going to have to do a lot more study. There's so many potential areas. We've just begun to scratch the surface in searching for specific areas. But, I think there's enough variety there, we may be able to find these analogies.

DR. LANGMUIR: I'd like to ask one more question that's unrelated. You just dropped at the end of your presentation the comment that you thought your program at Livermore was on the way out next year. What does that mean? Are we going to lose funding for the geochemical codes or the modeling work? That's the intent? That's where things are headed?

DR. BRUTON: Yeah, we're losing all our funding for geochemical modeling, development, maintenance, data base, maintenance development, and perhaps we're losing our--maybe, we'll be able to support our computer service contract, but no computer technicians. So, we're in problems now. Like, I'll be supported probably next year, but I need tools.

DR. LANGMUIR: Well, are you saying, in effect, then that programs like EQ-3/6 will no longer be supported and will no longer be available to the program because they won't be functional?

DR. BRUTON: In their present state. In their present state.

DR. LANGMUIR: But, no one knows how to run them?

DR. BRUTON: I mean, everyone will still have them, but that's it.

DR. DYER: Let me clarify this point. As a result of my last conversation with Les Jardine of Livermore, there is a cutback in that area, but there's a version of EQ-3/6 that's frozen. It's going to be released. The future development of EQ-3/6, for instance, is being throttled down under the current funding scenario. We cannot accommodate future developments of EQ-3/6 or of the data base. The support for EQ-3/6, they're may not be somebody at the end of the telephone whenever somebody calls with a question, but there is a version of the code that will be available.

DR. LANGMUIR: Well, what about those aspects of the program which clearly are not at this point proven accurate? There's clearly problems with the permanent data base in some aspects. Is that going to remain that way? You will then have a program that is not ever going to be cleaned up to the point where we can have confidence in its total function?

DR. DYER: No, we're going to have to develop that capability. It's just that--or next year under the current funding scenario, that's not in the cards.

DR. NORTH: Do you wish to respond?

MS. SIMMONS: Yes, I'm Ardyth Simmons from the Department of Energy and the situation that Carol and Russ have addressed is true. However, we recognize that the geochemistry studies that Livermore is doing are very important studies and I would say that, although the present scenario that's been described is one that appears to be very bleak, we believe that we have been able to find some additional money to put into this work and what will happen in truth is not as pessimistic as what has been described. We've just been going through some budget exercises right now and we will be able to improve the situation quite a bit. So, I just want to make that clear for the record that--

DR. LANGMUIR: What kind of a staffing level will you maintain to support the program? You currently have, what, half a dozen people?

MS. SIMMONS: We're going to maintain the same staffing. If you're speaking about the Livermore work specifically now, we should be able to keep all of the people that are currently involved.

DR. LANGMUIR: Not just sitting behind their desks, but actually--

MS. SIMMONS: No, actually doing the work that is in their work scope to do now, so that none of the activities that have been set up for the '92 fiscal year will be dropped.

DR. LANGMUIR: I'm still a little confused, though. Does that mean that we're frozen in terms of development of the programs or are we going to be able to improve them, improve their function in this program overall?

MS. SIMMONS: In the long-term, we should be able to improve the situation. We should be able to develop this work a lot more because it's something that we consider a priority.

DR. BRUTON: Well, I think I put some people on the spot here.

DR. LANGMUIR: Thank you, Carol.

DR. NORTH: Very useful--

DR. BRUTON: But, this is why we come to these meetings because we think it's important and I like seeing if anybody else thinks it's important, too.

DR. NORTH: Okay. We have several other comments.

MR. BIRCHARD: George Birchard with USNRC. Yeah, I would comment. I, personally, as a representative of the USNRC, looked into New Zealand area a number of years ago and thought it had excellent capability for helping to go through, what I consider, the true QA process of looking at natural systems to validate and test your data base in your model, the results of your calculation. I see this as being the true QA. I don't see that the QA approach that is being proposed where one tries to anticipate one's results in research before one has done the experiment as being a realistic form of QA. So, I fully, personally, endorse the kind of approach that's being suggested here to develop a basis for examining the results of your models and going about identifying which areas have problems with the data base and then sending people back into the laboratories to improve the data base, where required, so that the model results will match a range of field conditions. So, you'll cover a wide range of possible results and, thus, when you look at different scenarios for Yucca Mountain, you'll be able to bound those scenarios in a range of calculations.

DR. NORTH: Thank you for that comment. There's an interesting and creative suggestion with regard to your funding problems, as well, take it from the QA budget.

DR. BRUTON: Well, I think Julie would like that better coming from Los Alamos, but--

DR. NORTH: Carl Johnson?

MR. JOHNSON: Carl Johnson with the State of Nevada. Carol, I want to pick up on Pat Domenico's line of thought in his question. I can appreciate the selection of New Zealand for an analogue study, but I wonder why the hydrothermal sites in the Yucca Mountain that are in the same hydrologic system as Yucca Mountain were not selected for study?

DR. BRUTON: You mean, like studying Yucca Mountain as a natural analogue?

MR. JOHNSON: No. Specifically, there's Bailey Hot Springs near Beatty, there's Tecopa Hot Springs which is at the south end of Amargosa Valley which are all within the Yucca Mountain hydrologic system, and therefore certainly would be very close analogues to Yucca Mountain.

DR. BRUTON: We have a program right now. It's sort of like we're looking at analogues very close to Yucca Mountain. But, we're already using Yucca Mountain as a natural analogue now. I mean, I'm studying Yucca Mountain in that regard. It's like one of our considerations was why choose a natural analogue that's exactly like Yucca Mountain because we need to look at the impact of different variables. See, if you study an area that's slightly different chemically, it can also help you determine the limits of applicability of your model. It's not studying the same system over and over again, but you're really developing confidence because you're studying another system. And, so what if you can just model Yucca Mountain? I mean, always the thing is, you already know what's there, you know, and you've already been working on it for years, but you can't show that you go someplace else. If you can show you can model Yucca Mountain and you can model other geothermal areas, that means you know you've got the processes down pretty well because they work in a variety of different areas. And, so going to an area that's not exactly the same expands the bounds of your capabilities and tests the bounds of your models. So, I think there is an advantage to going to different areas. And, there's a lot of different processes going on in these areas because we're studying near-field environment after the emplacement of the waste. And, the environment that's there now at Yucca Mountain isn't the type of environment that we expect after we emplace the waste. It doesn't have the temperatures, it doesn't have the degrees of fluid flow that we'd expect after the waste is emplaced. And, that's why going to a geothermal or a hydrothermal system is useful.

DR. DOMENICO: I don't think there's any assurances that you could model Yucca Mountain right now.

DR. BRUTON: Oh, no, there's no assurances.

DR. DOMENICO: I think I heard you say that.

DR. BRUTON: Well, we've modeled certain--see, looking at Yucca Mountain is a total--

DR. DOMENICO: I'm talking about thermal modeling, though.

DR. BRUTON: Oh, the thermal history of it?

DR. DOMENICO: The thermal modeling of it, yes.

DR. BRUTON: Oh, through time, it's--you know, the old volcanic events to the north and what did that do? No, see, you'll never get an exact match to anything. But, between Los Alamos and Livermore, I think we've done some preliminary work saying that, yes, we basically have a good basic understanding of what's controlling the alterations at Yucca Mountain. We don't pretend to understand it perfectly, but I think we've got a good feel of like how the mineralogy is controlled by the alteration of the silica polymorphs and so on in some of the controls.

DR. NORTH: I'd like to put a few questions of my own to you. One of them which has just been addressed was the issue of quality assurance and, without cracking a joke this time, let me ask you to what extent that will be a problem if you go ahead with this work in New Zealand?

DR. BRUTON: Well, we'd like to classify it as non-quality effecting work because we don't--our results are not going to be used directly in license application. QA will come up if we want to use it for validation because that's a very strict exercise, but you notice I didn't specifically say validation. We haven't specifically addressed how this will fit in with the Yucca Mountain quality assurance program. We've basically been focusing on, all right, this is what we want to do scientifically and then we have to integrate this better, okay, is it okay? What are the project goals in terms of QA because we're not going to say this is an exact analogy to Yucca Mountain, we're going to use this in our defense. No, what we're doing is testing whether our mathematical descriptions of the models are adequate. So, I can't say that we've done that much work and how it's going to work under the QA.

DR. NORTH: Next question, it seems to me there are some specific issues from performance assessment that your work may be very valuable in supporting, but as yet, they haven't really been defined. I can define one as a straw man where I think what you propose might be extremely valuable, if for no other purpose than some communication with the non-scientific community that has great concerns about the Yucca Mountain site. And that is, we take the scenario that is the result of seismic events, we have intrusion of fluids, and perhaps there is some heat associated with the geological processes adding to the heat from the waste itself. How well will our engineered barriers stand up and what alteration to the rock might occur that would have a major impact on the integrity of the geologic barriers? Perhaps, the New Zealand site could give us some very interesting and useful answers. I think, in terms of communication based on work that I did with the Department of Energy as a consultant more than 10 years ago on geothermal energy, that you have some incredible corrosion problems in dealing with some of those hot fluids. So, whereas yesterday we were looking at some beautiful pictures of artifacts that appeared to be extremely fragile and had lasted for tens of thousands of years, we could also have some pictures of some cement and steel from geothermal energy facilities where some of the more corrosion-resistant materials that our human engineering has created had done rather poorly on a time scale of decades against the onslaught of such fluids. Now, how have these problems been solved in New Zealand to make their geothermal energy process work, so that these materials can withstand this kind of challenge and are our codes up to being able to predict how to solve these problems well enough so that we can say with some assurance that in temperature regimes of under 200 degrees Centigrade and with certain rock types we can be assured that some calculations as bounds are, indeed, very secure because they have been tested against this kind of field environment and found to be excellent predictors of what has been observed over a time scale of decades?

DR. BRUTON: Two of our people are now on sabbatical on a Fulbright Fellowship down in Australia. One is Anne Marie Meike who is the--of man-made materials at Livermore and both she and Bill are actively communicating with the DSIRO in New Zealand on their--that the work they've done looking at the degradation of those man-made materials and the well workings, they're now collecting data and establishing those contacts to work on it when they come back. It's a fascinating opportunity, real time, high temperatures, active, dynamic systems.

DR. NORTH: Well, I think the need is to focus on some specific scenarios and some specific problems within the performance assessment program, such that when you go through the system for your support, this can be justified on the basis of this is going to contribute the following very specific things in performance assessment which management has decided are very valuable in performance assessment. In other words, this is not just pursuing interesting science on the state-of-the-art with a very interesting analogue system, but rather you are going to come up with some information that may be absolutely crucial in the future of the license application process and that's why it's worth doing this.

DR. BRUTON: Um-hum.

DR. NORTH: And, my sense from especially the presentation that you made is that that kind of specific interchange between analogues and performance assessment hasn't yet been made and I think you need it very much. So, I would urge that the colleagues in the performance assessment area and those of you working on the analogues engage in some extended dialogue and, as a part of that process, the Risk and Performance Analysis Panel is having a meeting in May on the 20th and 21st, I believe it is, to review performance assessment. And, I'm going to be asking them the same kinds of questions, have you talked to the analogues' people about what they may be able to do to support your program? I think the take home message is that you ought to be talking to each other a lot and formulating specific plans and priorities together.

DR. BRUTON: Makes sense.

DR. NORTH: Do we have any further comments or questions?

(No response.)

DR. NORTH: Let's go on.

DR. DYER: Our next speaker is Dwight Hoxie of the U.S. Geological Survey. Dwight has been with the USGS for 15 years, five years with the nuclear hydrology program. He's currently the project chief of the unsaturated zone modeling, synthesis, and integration project with the principal responsibility for developing and applying models to simulate the unsaturated zone hydrogeologic system at the Yucca Mountain site. Prior to joining the nuclear hydrology program, he was the project chief for several saturated zone hydrologic modeling studies within the Wyoming district of the water resources division of the USGS. And, Dwight's talk will be on the paleoclimate and hydrology analogues.

DR. HOXIE: First of all, I would like to begin with a couple of caveats. First of all, I notice the title says Paleoclimate and I'm really going to be talking more about paleohydrology analogues, rather than paleoclimate. And, my second caveat on that grounds is that I'm very interested in the paleohydrology program, although I've never really worked in it. So, I'm actually going to be representing a number of my colleagues at U.S. Geological Survey. And, because I'm representing work that is in progress and work that they are doing, I'm sort of feeling a little bit like an unnatural analogue myself.

I would like to begin talking about natural analogues for the unsaturated zone hydrologic system at Yucca Mountain and one of the most fundamental natural analogues which is the basis of not only our site characterization work and our modeling, but also--I've got to get this in, I know--are analyses that involve performance assessment; that is, the transport of radionuclides, release of radionuclides, and transport in the unsaturated zone by moving groundwater. And, our natural analogue, our fundamental hypothesis, there is that we can treat the unsaturated zone as an equivalent Darcian continuum porous-medium system. And, the analogue there, of course, is unsaturated soils. And, we know full well that Yucca Mountain is not an unsaturated soil column, it's indurated fractured rock. And, so we know at the very outset that we have a very serious problem because we have a fractured system and this was brought out yesterday.

We can have the possibility of decoupling between the rock matrix and the fractures and have water move rapidly through the fractures out of equilibrium with the surrounding rock matrix. To understand these kinds of processes, we can go to look at other natural analogue sites and one that's already been mentioned several times is looking at Rainier

Mesa. We have not done that ourselves in USGS particularly, but the Desert Research Institute has certainly done work out there with regard to infiltration and recharge in the higher precipitation zones at Rainier Mesa. But, we have looked underneath Rainier Mesa in the G-Tunnel facility and we have done a considerable number of experiments there which were mentioned yesterday, Lawrence Livermore, Sandia, and USGS, and we are combining all of these results, as Charlie Voss pointed out, as part of our commitment to the INTRAVAL international validation project.

And, another place in the same sense is that we want to look at the Apache Leap site, the University of Arizona/NRC site, in Arizona because it is also a natural analogue for a fractured tuffaceous hydrologic system in a region of much higher precipitation than we find at Yucca Mountain. So, we do have these nice hydrologic analogues that we need to pursue. And, I might also point out in the site character-ization plan, we don't mention analogues, as such, but we certainly have intended to rely on the use of analogues extensively for site characterization and performance assessment.

But, now getting on to the paleohydrology analogue studies, we need to look back in time at the hydrologic system and paleoclimates because what we see presently in the unsaturated zone at Yucca Mountain is not the result of processes that are occurring today; rather, they're the result of processes that occurred some time in the past. Consequently, the unsaturated zone geohydrologic system is, itself, a paleosystem in that it has existed and developed in response to past climate events. So, we need to track down and identify the past climate events in order to decipher what the hydrologic history of Yucca Mountain has been.

But, on the other hand, another thing that we need to understand is how a hydrologic system in the Yucca Mountain environment is going to respond to climatic change. So what we need to do is go back, not only unravel or reconstruct what the past climates have been, but we need also to examine hydrologic systems that have responded to those past climate events and try to make the correlation of response to stimulus, as it were.

So, we have a number of sites that we are looking at to look at various kinds of hydrologic processes under different climatic conditions. We have a set of two hydrologic analogue recharge sites that are in the vicinity of Yucca Mountain. They're on similar geologic terrains, but they are in different current climatic conditions. The first of these is Stewart Creek in central Nevada which is a moist, sub-humid, Continental climate and represents Yucca Mountain as it might have been, say, 17,000 years ago during a glacial maximum. We have Kawich Creek in Nevada, also. It's classified as a moist steppe climate and so represents an intermediate stage of climatic conditions. And, we are also looking at another site that is not in Nevada, but down in Oregonpipe Cactus National Monument in Arizona, which is a prime example of a monsoonal, arid kind of environment. And, just for your information, Yucca Mountain sits sort of on a north/south boundary between monsoonal rainstorm events in the summer from Gulf air moving north, and a more arid type of environment to the north. And, we don't know what the climate is going to do in the future, but of course, that boundary line could move north or south. So, it is relevant to try to understand the kinds of processes that would be going on, say, in a monsoonal type of climate.

So, just to give you an example, I'm just going to show you qualitative kinds of things, just photographs actually. First of all, this is Stewart Creek site. But, if you'll notice the scenery here, we have a considerably different environment than we have at Yucca Mountain currently. And, the type of vegetation that we have at this site, according to the packrat midden data, is indicative of what Yucca Mountain might have been some time ago, say, 17,000 years ago or so. And, again, you can see that we have a much different kind of climatic conditions here and this site is at an elevation currently of about almost 9,000 feet. And, the kind of work that's being done with these analogue recharge sites is not only typical, straight-forward watershed kinds of studies, but also geochemical studies to get an idea of how much water is being recharged into the system, how much is being discharged in response to whatever precipitation falls.

The Kawich Creek site is shown on this map and, again, just to give you an idea of what it looks like, you can see it's distinctly different both from Yucca Mountain and the preceding site. So, it represents a state of intermediate climatic conditions. And, again, just some of the instrumentation and the work that is being done in the watershed studies.

DR. CANTLON: What's the elevation there, Dwight?

DR. HOXIE: This is probably around 6,000 to 7,000 feet.

We're also looking at analogue discharge sites and the idea is to try to get how much water is moving out of a groundwater system in response to climatic conditions. And, this is a case where we're really going back and looking backwards in time. And, Kawich Playa, Nevada is an old paleo-lake site. It's currently dry, but at one time in the past or several times in the past, it has actually been a permanent lake. The other site that we're looking at is in northern Nevada up in the Ruby Mountains and it's called the Ruby Marshes. It's a discharge site. And, one of the reasons that we're looking at that is that the hydrologic system that is discharging into the Ruby Marshes has been well studied. So, we already have that information available. And, just to show you locations, I don't have any slides of the Kawich Playa site, itself. I can give you the location, but there is one thing I would really like to talk about just very, very briefly.

Yesterday, when we were learning about the oil resources in Nevada, it was pointed out that a little creature called the conodant is actually very useful for studying petroleum reserves. Well, we have our own little critters, too, for studying paleohydrology and these involve little crustaceans called ostracodes. And, it turns out that ostracodes, there are a number of species that live in fresh water lakes and brackish lakes and so forth, but all these different species are very sensitive to different kinds of water quality and temperature conditions and different kinds of environmental conditions. So, not only can we identify the presence of water in the past, but we can also identify what the conditions were in the water samples. And, so what I'm showing you here is an auger sample taken from a depth of approximately 400 centimeters out of the Playa sediments. An analysis was done to look at the lithology itself and the important thing is that for the first, what, 150 centimeters or so, we have very, very fine grain materials. What I'm showing here on the left of this diagram is the fractional sand content, essentially, by weight of the sediments. So, below 150 centimeters or so, we have a very high sand content. This would indicate that the waters that were feeding the lake at that time had high energy and, therefore, were capable of transporting large grain material. But, after about 150 centimeters, whatever that represents in time, the lake dried out and we see this because if you look at the other plots here, we have a total number of shells, essentially, per weight and we also have the various kinds of species of ostracodes that were identified. And, one thing I want to point out is that a carbon-14 sample was obtained from intervals between 200 and 225 centimeters and the age of that particular sample, uncorrected age, was about 27,000 years. So, you can get some idea anyway what kinds of times are represented by these lake deposits.

One thing I would like to point out and why the ostracodes are actually very useful is that this particular critter over here, whose name I can't really pronounce, is very, very sensitive. It likes warm water and water that has very high total dissolved solids. So, it likes dirty water, as it were, whereas the other two prefer clean water. So, we can get some idea that not only was the lake filled during this interval of sedimentation, but we can get some idea of what the environmental conditions in the lake were. And then, we can try to infer from that what kinds of climates were probably feeding and supplying the water to the lake. One thing that I should also point out is that the ostracodes have life cycles that are a minimum of about a month. So, they do indicate the presence of a permanent water body and perhaps even the groundwater supplied body.

The Ruby Marshes site again is up by the Ruby Mountains and I've already discussed that. It's kind of an analogue for what we have down in the Ash Meadows area. This is a photograph of Crystal Pool. So, we have this kind of spring deposits up there, but they're also natural analogues apparently or a current analogue for the old paleo-lucusterne deposits that we find south of Yucca Mountain between Yucca Mountain and Las Vegas. So, this is one reason why the Ruby Marshes area is being studied.

Another series of analogue studies that are being done have to do with the problem of the vein fillings at Trench 14 and this is the calcite silica problem and the question is, of course, where does the calcite silica mineral deposition originate? Is it something that is coming down from the ground surface, infiltrating, and being deposited under pedogenic conditions or is this water that is being upwelled from some kind of depth and, therefore, being the so-called Giamanski Hypothesis?

So, in order to pursue these kinds--well, to decipher the history of the Trench 14 deposits, we're looking at three analogue sites. One of them is a very well studied pedogenic site at Mormon Mesa which is on the Utah and Nevada border. A lot of work has been done there by the University of Utah scientists and we are using that work to try to find characteristic features that would say yea or nay, this is pedogenic or it is not. And, I might just point out, I do have a slide of a pedogenic kind of deposit over by Busted Butte at Yucca Mountain where we have calcite-cemented sands.

The other possibility, of course, is that we have water moving up from depth. So, that means that we would have a spring discharge and we have a very nice analogue for that at Travertine Point in Death Valley. This is an old paleospring deposit. So, this is being studied to try to determine if there are similarities and what kind of similarities, the degree of similarity with the Trench 14 deposits.

And, finally, in response, as it turns out fortuitously to Carl Johnson, we are studying Bailey Hot Springs. But, we are studying it not from the standpoint of an analogue to geochemical processes in Yucca Mountain, per se, but really as an analogue to the deposition of hydrothermal siliceous center and these kinds of deposits as an analogue to Trench 14. Now, I'm not familiar with the geology and plumbing system at Bailey Hot Springs, but I'm sort of guessing that--I'm not sure if it's discharging from tuffaceous rocks and so would, therefore, actually represent an analogue for groundwater flow in Yucca Mountain, but that's just in kind of response to the concern that Carl raised.

And, finally, I guess what I would like to do is close by saying that what we are doing with our paleo-hydrologic analogue studies is really trying to put into practice the idea that in this case the past, as we can unravel it, is going to be the key to the present conditions at Yucca Mountain and possibly to future conditions. Because, presumably, these analogues will represent analogues to responses to changing climatic conditions. And, the only problem that we're really going to have is are the climatic changes that are going to occur in the future going to be analogous to the climatic changes that occurred in the past and that is something that future climate modeling will have to try to predict for us.

So, with that, I would like to close. Thank you.

DR. NORTH: Thank you.

Questions and comments?

DR. CANTLON: What is the criteria that make that a spring derived, as opposed to a pedogenic?

DR. HOXIE: I may defer to Ike. I think you've probably looked at this more than I have, but if I may--

DR. CANTLON: Sure.

MR. WINOGRAD: There's several criteria. The main one is that the sub-horizontal light colored deposit is a tuffa deposit. It contains fossil vegetation, gas deposits which are hard to find. I've yet not found and others have. You can trace the vertical deposit disseminating into the horizontal deposit, but perhaps the strongest evidence is that this is part of a vein swarm. There are thousands of these things. They occur over vertical reliefs of tens of meters. But that the morphologies that we see here, we can see in a living system in Devil's Hole. And, there are many other criteria. These are very dense deposits. They have no resemblance, whatsoever, mineralogically or isotopically to the type of stuff that you see in Trench 14.

DR. CANTLON: This is what the cold springs are supposed to be like?

MR. WINOGRAD: It depends on what you call cold. We think these in a range of 30 to 50 Centigrade. If that's cold, yes.

DR. ALLEN: And, also, isn't it fair to say, Ike, that this is not a matter of controversy? Almost everyone who has studied this agrees these are--

MR. WINOGRAD: I think that's correct, yeah.

DR. NORTH: Other questions and comments around the table or from the audience?

(No response.)

DR. NORTH: Then, it comes time for a break and we are a few minutes behind schedule. Let's resume at 10:35.

(Whereupon, a brief recess was taken.)

DR. DYER: The speaker is Burt Johnson who comes to us courtesy of Battelle Pacific Northwest Laboratory. In the early 80's, Burt wrote one of the classic papers on the topic of ancient materials which we asked him to share with us today. Burt's areas of investigation include the effects of nuclear plant aging, technical basis for interim wet and dry storage of spent fuel, the effects of radiation on metallic corrosion, behavior of zirconium alloys and reactor service, investigations of stress corrosion and cracking of stainless steel and anconal, studies of reactor decontamination, durability of ancient metals, materials in nuclear fusion. He's the U.S. representative to several international Atomic Energy Agency consultant groups and the INFCE working group on spent fuel.

Burt's topic is ancient materials, analogues for repository materials.

DR. JOHNSON: Thank you, Russ. I'd like to begin by thanking the committee and Russ and his staff at Nevada for this opportunity to revisit what has been a substantial, but somewhat dormant interest. Russ mentioned this study that we did in 1980. I was invited to participate in the third NAWG meeting in Utah. I was close to signing a contract with the Basalt people when, in a matter of speaking, that went down the tube or down the hole, I guess. So, otherwise, the interest here has been unrequited. So, I appreciate this opportunity to revisit it.

The materials that we will address here briefly are metals from the prospective waste containers; concretes, I will treat very briefly related to shaft closure; and, two later speakers will discuss glasses which I will not address. Now, there are other materials, such as backfill and marker materials, that we are not addressing here today.

I've represented here the formidable challenge to qualify repository materials over time frames of centuries to a millennia and perhaps longer. And, there are various approaches that can contribute to that qualification. Certainly, the controlled experiments with modern materials, but with the problem of having only limited time frames for extrapolation. We can look at the other end at the ancient materials having relevant time frames, but the drawback of estimated environments and only marginal relevance to the precise environments of the repository. These elements then need to interact with the theory and modeling. And, it occurred to me that I have some major deficiencies with this diagram which I propose to address here. Dr. North began addressing them for me, but let me proceed.

Certainly, there needs to be interactions here and, although I'm speaking from a limited perspective here, I'm sure that there are strong interactions here with the laboratory studies. I propose that there need to be strong interactions here between the ancient materials people and the theoreticians and modelers, I think both ways. The ancient materials people need to look down their list of lessons learned and share those with the modelers and say have you considered the effect of coatings, have you considered the effect of gettering, and so forth. Now, the answer may be yes or it may be quickly that these can't work. But, at least the lessons learned here need to be overtly discussed with the theoreticians and the modelers. On the other hand, the modelers may say here is a problem that we're wrestling with. Can you find a natural analogue that might help us to sort that out.

So, the other leg that's missing here is this one (indicating) and there are several things that come to mind here. The laboratory staff may be able to give some idea as to how much effect radiation and thermal conditions that are missing here might have contributed. This is kind of a wild thought, but it's also possible that the ancient materials people might share some of their materials that would go into autoclave experiments that would provide some cross check. So, these are just some thoughts that I think were needed to complete my diagram.

The classes of ancient materials are archeological metals, metal meteorites, and native metals and I'll just give you quick perspective here. The seven metals of antiquity are shown here. Copper, iron, and lead have particular potential application in a range of repository studies. The metal meteorites, there are approximately 530 metal meteorites that have been identified, 30 of these are called falls where a human actually saw the meteorite fall. So, the date is fixed, but relatively short time frames, a bit over 100 years for the oldest. The compositions are 6 to 35% nickel. I've heard of one that goes to 60%, but I haven't tracked down the details. The types and their corresponding nickels are shown here in the most common sizes are 10 to 40 kilograms, although I'll talk later about one which is much larger. The native metals are shown here, at least the most common. Again, copper and the nickel iron alloys seem to offer the best potential for relation to the repository issues.

There's several approaches that we can take to provide the basis for container performance. Thermodynamic basis where we demonstrate perpetual compatibility of the metal with the repository environment and the Swedish concept that I'm sure that most of you are familiar with illustrates this; copper in low-oxygen, low-sulfur groundwater shielded from radiolysis. If we don't have the prospect of a thermo- dynamic basis, then we need to look at the kinetic basis measuring corrosion rates of candidate materials over a period that permits extrapolation. And, I have found one that I will cite here, although there are many other kinetic studies, and that is the reference container material, at least at the time of this study, was 304L. There was a general corrosion study in Yucca Mountain repository conditions that was extrapolated to 1,000 years and showed that at least for uniform corrosion that the material would perform quite well.

Going on with the other potential bases, notice that I've divided here the ancient material data into two categories. The first one I call the natural analogue basis where this is a study of a behavior of a candidate material in a relevant environment. In other words, a well-focused relatively relatable analogue. An example, Michigan native copper deposits in basalt and groundwater--these are sub-surface deposits estimated to be deposited about 500 million years ago--that has resided in the basalt under a range of thermal conditions relevant to a repository, groundwaters that are similar to what was expected to be the basalt groundwater conditions. Relatively hard to come by, but in some cases, perhaps highly useful.

Then, we turn to the more general perspective of lessons from metal durability. What have we learned from the studies of the ancient metals that tell us how certain metals were able to endure for centuries and, in some cases, several millennia. It's important in these studies that we be able to index the ages and to index the amount of weathering that has gone on. And, I will not take time to go through this in detail, but it illustrates that there are ways that the ancient materials can be dated.

I'll show you some examples here with the meteorites of ways that we can index how much weathering has gone on. When the meteorite is entering the atmosphere, the surface heats and there are heat effects then from reaction with oxygen where we get the fusion crust which is a combination of magnetite and mustite. There is an Alpha-2 metal zone here which of heat altered metal, but the bulk of the meteorite will not be altered by heat. So, if we find a meteorite which has either or both of these features, we can then index that there has been minimal weathering to the meteorite. The other feature is so-called regmaglypts that are caused by ablation as the combination of heating and motion through the atmosphere. And, again, if these are readily evident, then it helps us to index that relatively little weathering has gone on.

To discuss now very briefly, and these are illustrative rather than comprehensive, some of the perspectives, we see cases where durable coatings appear to have contributed to the long life of certain metals, fusion crust with some meteorites. You may be aware of the case where there were 606,000 terra cotta warriors that were found in China and associated with them were bronze and iron weapons and implements and there is a theory that there was a coating on the bronze that extended its life. Gettering, an example here is that a horde of Roman nails was found where the outer nails had preferentially corroded seeming to protect the inner nails, an example of what we call gettering. Whether this is of any value to the repository, we don't know, but I'd like to sit down with someone in the repository program and say, "Now is there any way that if we dumped X tons of highly oxidative metals somewhere in the repository that that would extend the life?" They may quickly say no way, but if they blink, then I know that we're on to something.

Low-oxygen conditions, a case where cannon balls that were found submerged in the mud and sealed at the bottom of the ocean were corroded much less than their cousins that were exposed to the sea water. Orders of magnitude, difference in corrosion for what presumably were the same compositions of material.

Contact with the soils, I'll show you a case of a metal meteorite which was partially buried and there are some considerable differences in corrosion between the buried and the unburied sections. Copper and bronze seem to have been much more durable in contact with soils, in general, than the iron-based materials. I've seen iron swords in a French repository where there were--not repository, but a museum--where there were dozens of swords which were all that was left was the hilt. The metal, the iron blade was missing.

I have mentioned that iron has been relatively non-durable in numerous finds and, in general, this statement is true that the iron-based materials generally had lower durability. I've mentioned the cannon balls, though, as an example where we see some suggestion of how to extend the durability. In the Tutankhamen Tomb which was, in fact, periodically moist, there were free-standing miniature tools that were rusted, but apparently largely metallic. And then, there was an iron dagger that was wrapped up with the mummy that was essentially uncorroded. Now, I'm not going to suggest what application that has to waste canisters, but you can draw your own conclusions. And then, there are selected meteorites which are, as we've seen, an iron-based nickel alloy largely where fragments of meteorites have been impressively durable.

Let me just show you a couple of things before we move on. This is one of the Roman iron nails. I believe that the horde was discovered--well, that it dates back to about 500 A.D. Here's an interesting case where an iron cannon, as it was retrieved, looked like this. After it sat in the atmosphere for a while, it rapidly corroded just out in the atmosphere. The famous iron Pillar of Delhi dating from about 400 A.D. The Canyon Diablo Crater, about 1200 meters across, resulting from an enormous impact. The interesting thing here is that we see a whole range of corrosion on what presumably was the same metal composition. So, it's a valuable opportunity to study the range of conditions here that contributed to this broad response in terms of corrosion. And, we have some hypotheses that I won't have time to go into, but a very interesting site from the standpoint of both metal degradation and metal preservation.

Here's a section the field museum kindly gave us a piece of the Canyon Diablo meteorite. You see a piece here which is still largely metallic. The crater is dated to about 20,000 years ago, at least. There are small metal inclusions here which obviously were somewhat more durable than the base metal and we have done some characterizations on those.

Here's the case that I mentioned to you where a meteorite was partially buried in the soil. There are very thick oxides here and here (indicating). This one can be found at the Smithsonian. Up on some of these upper surfaces which are exposed to the atmosphere, in fact, there was still Alpha-2 metal, which you remember is one of our indexes, which remain suggesting that the upper surface is corroded much more slowly than those which were buried in the soil.

And then, this is a meteorite, the Ider from Alabama, dated to greater than a million years, still showing some metal, but obviously largely degraded.

And then, this is a copper boulder that we obtained from Michigan through Michigan Tech and it had very thin, less than one millimeter, oxide surfaces, though it apparently was deposited by the glaciers approximately 8,000 years ago.

I will touch only briefly on the concretes. This is not an area that I have studied in detail. So, I'm quoting from Langton and Roy. The materials that were studied were plasters, mortars, and concrete, ages of 1400 to 3000 years. In general, they were still well cemented, but some of the concretes were friable presumably due to high water to cement ratios. The materials were favorable, generally accomplished by careful selection of proportioning and processing; clean, well-graded, inert aggregates seemed to be common in the durable products. And, some of the structures are still functional.

To summarize then, native metals, such as copper and iron, can exist for very long periods in a reducing environment. In an oxidizing environment, such as Yucca Mountain, these metals are more durable under dry conditions. If you gave me my choice of how we would construct the repository based on metal preservation, I would want free-standing metals in a relatively engineered dry environment.

Ceramic materials, such as alumina and titania, and minerals, such as synroc B, could survive under a variety of geologic conditions. And, as you perhaps know, the Swedes have looked at alumina as a canister material, although they're currently not highly enthused about it. The Yucca Mountain Project is currently studying copper, copper alloys, and austenitic materials in oxidizing environments.

Potential applications, certainly to public perception of durability; application of lessons learned overtly sitting down with the repository modelers and saying here is what we've learned, is there any way that you can use that knowledge; a general ranking, we can tell you which metals have been most durable over centuries and millennia; selected processes and performance issues might be addressed selectively by analogues; and then, selected validation of computer models. Now, I'm not promising a lot here, but I'm saying it's something that should be systematically assessed.

We admit that there are limitations. Thermal conditions are not generally addressed in the ancient metals; however, cooking pots are an example where the metals have been heated periodically and I have seen a reference to a cooking pot that had been traced through a family for many generations. So, that could be a fairly well-defined object. Radiation effects are not addressed. Again, it's a matter of interaction with the people who are doing the experiments to explore how important that is and then the environments are not often well-defined, but again this should be a matter of discussion.

I wanted to leave you with some statements. Do we have time for a few statements by the ancients about the metals? This is presumed to be the first "sorry, but your shipment is late letter" from the King of the Hittites to Rameses II. "There is no good iron in the house of my seal at Kissawadna for it is a bad time to make iron. I've written ordering them to make good iron, but so far, they have not finished it. When they do, I will send it to thee. Behold, now I am sending thee an iron dagger blade." Well, I've seen other references that this didn't pacify Rameses and the King of the Hittites got in deep trouble over this iron.

Here's a statement. I'm not sure who it is attributed to, but it reflects the role that iron played. "Gold is for the mistress, silver for the maid, copper for the craftsman cunning at his trade, goods to the baron sitting in his hall, but iron, cold iron, is master of them all." Now, if he was going to use it for waste canisters, he might have more reservations.

And then, finally, a very interesting insight about how well the ancients understood metals. This is from Bartolommeus Anglicus, "After the mind of Aristotle, iron is gendered of quick silver, thick and not clean, full of earthy substance and of brimstone, brazen, boisterous, and not pure and has less of area in watery moisture than other metals." Now, what this suggests is that the ancients didn't understand metals very well, but they still made great use of them. And, perhaps, this is an analogue for the use of natural analogues.

DR. NORTH: Comments, questions?

MR. ROGERS: My name is Rob Rogers. I'm from the Idaho National Engineering Laboratory. I'm wondering, Dr. Johnson, in the studies that you've done on durability of metals and others if you've looked into the effect of microbial-induced corrosion on metals or the effects of microbial-induced corrosion on concretes or ceramics?

DR. JOHNSON: Not in this context, but I have a program currently with the NRC on the corrosion in surface water systems and I am very familiar with the fact that this needs to be addressed. And, one of the problems is that if you do sterile laboratory experiments, this may be missing. So, in cases where it is potentially important, it needs to be factored in.

MR. ROGERS: So, you would suggest microbial studies in the materials? At least, testing of the materials that would be used in the Yucca Mountain containment systems?

DR. JOHNSON: Unless microbial effects can be clearly ruled out at these depths and temperatures. But, otherwise--and there can be some very surprising things. I'm told that there was a microbial colony sitting on the fuel in TMI-2 at 11,000R per hour.

MR. ROGERS: That's correct.

DR. JOHNSON: So, let's not be fooled into thinking that the radiation may make them go away.

MR. ROGERS: In fact, we did the studies at TMI and cultured those organisms, but I'm relieved to know then that at least with the service water systems and, as I'm sure you know now, there's a large body of which are being put out or at least coming to the surface on microbial effects on many materials. As far as cement goes, many of the monuments and buildings in Europe that are weathering quite rapidly have now been shown to be affected by microbial-induced corrosion and microbial-induced weathering. So, I just wanted to bring that forward to the group. Thank you very much.

DR. JOHNSON: Thank you.

DR. NORTH: Thank you.

Other questions or comments, either the board, others around the table or the audience?

(No response.)

DR. NORTH: I think we go on to the next speaker.

DR. DYER: Our next speaker is Bill Bourcier from Lawrence Livermore National Laboratory. Bill's been with Lawrence Livermore for five years. He's a geochemist in the earth sciences department. His research interests include aqueous geochemistry, hydrothermal ore deposits, and computer modeling of water-rock interactions. And, Bill will talk to us about the natural analogues for nuclear waste glass.

DR. BOURCIER: Thank you, Russ. I hope what I have to say improves on what the ancients did. I'm not so sure sometimes that it will.

What I want to do today, I have a 10 minute time allotment here, is get into specifics and look at two examples of how data we've obtained from natural analogues has been used to guide and develop a program we have at Livermore to develop a long-term model to predict borosilicate waste glass dissolution. So, what I'm going to do is start off and just show you the approach we're taking to developing this model, look at a quick cartoon about how radioactive waste glasses dissolve, and then look at two examples, that of basaltic glass and that of tektite glasses, in natural environments. The work I'm presenting in those two areas, I haven't done. My role in this is to incorporate the results from those studies into our development of a model.

Our approach to solving the problem of predicting long-term dissolution rates of borosilicate glasses is to use a mechanistic modeling approach. We have to understand the fundamental chemical mechanisms that accompany glass degradation in order to construct a model that we can, with some credibility, extrapolate to long time periods.

Our approach to doing this is, first of all, to identify the chemical processes of glass dissolution and we've used a lot of different tests of natural and synthetic and radioactive waste glasses over the last 10 or 15 years to really put together, sort of synthesize qualitatively, all the different chemical processes that accompany glass dissolutions; the hydrolysis of the bonds, diffusion, all the secondary phase formations, all these things.

The stage we're at now is to generate a model of glass dissolution. This is a model we've incorporated into the EQ-3/6 code and we're also in the process of performing experiments. Unlike these experiments, we're doing experiments now that quantify the model parameters. Simple experiments like quantifying the rate constant as a function of pH. And, at some point in the future, we've started thinking about this, and at this point we've done little else than that, is to validate the model with natural analogues and also with site-specific experiments.

So, what happens when glass is dissolved? I'm going through this so I can show you how basaltic glass is a good analogue for nuclear waste glasses and also to develop some of the terms I'll need to use later. Put a glass in water, water diffuses into it, breaks the silicon-oxygen network forming bonds in the glass, releases soluble elements to solution, insoluble ones re-precipitate on the surface of the glass. So, you have the bulk glass here (indicating), the solution. There's an interface here (indicating). Well, actually, it's the solution hydrated glass interface where these so-called diffusion and gel layers form. Outwards from that are various thicknesses of amorphous to crystalline residual and precipitated secondary phases, not plates. So, that's sort of the framework of how glass reacts in a natural system, nuclear waste glasses.

Key questions then. We've noticed in experiments that we tend to get sort of a steady state regime where those diffusion and gel layers maintain, more or less, a constant thickness--I should go back to this one. This whole process happens in such a way that we started out without this thick sequence of secondary phases. You just have the glass and this layer. With time, this layer (indicating) essentially migrates into the glass and this layer thickens. You have sort of a steady state situation where this layer remains constant in composition and thickness with time. That's what we see in experiments. That's what our model is assuming. Is that something that we're going to see in glasses over 10,000 year long time periods? That's the first question or one of the questions we can address with natural analogues.

Does the steady state-type reaction continue with time? Another important question is we believe, again based on a lot of experimental results, that the rate determining mechanism for a long period, or at least for our test results, is the rate of surface reaction at the glass, gel layer, solution interface. The breakdown of the network of the glass, the hydrolysis of those bonds seems to be what controls the glass dissolution rate. Does that mechanism change over long time periods? We see it in five or 10 year tests, what happens in 10,000 years?

And, finally, another key question is again in our short-term tests, other than providing synchs for elements leached out of the glass into these phases, including the actinides, these phases and the amorphous surface layers don't seem to provide any transport barrier to glass dissolution. Water seems to be able to freely get into this area and dissolve the gel layer of the glass. The question is with time, in five or 10 year tests, these don't make a difference. In 10,000 years, will these things effectively armor the glass surface, prevent water from getting to the surface, or on another level, at least significantly alter the composition of the fluid that does get to the glass surface to dissolve it? So, these are some key questions that useful information can be obtained from natural analogues.

I've included in your viewgraph packet a list of compositions of radioactive waste glasses and basaltic glasses. I won't talk about that, but just for your information.

Basalt glass is the material most similar in composition and in durability to nuclear waste glasses. I've plotted here the rate constant for dissolution in moles of normalized desilica per centimeters squared per second versus pH. This is the work of Jim Maser, Northwestern University. You see curves for silica glass, dacite glass, and intermediate volcanic rock, and basaltic glass, and also plotted from the same sorts of tests, a rate constant derived for an SRL-165 glass, one of the more durable waste glasses.

We see, first of all, and believe me, this is the most important point I want to make, is that these natural materials that we know exist in nature and have existed for millions of years, in fact, billions of years--there's examples of basalt glass from 1.2 billion years in Michipakote Island in Lake Superior. Similar durabilities of nuclear waste glasses, these glasses have survived for millions of years in natural environments. So, the question really is what can we learn from nature that allows these glasses to be preserved for those long, tough time periods?

This viewgraph was in my packet. It belongs to someone else. So, if you see it and recognize it, I'll leave it up here. I can't tell from looking at it, what it--I don't know the acronyms. So, I don't know. Otherwise, I'd probably find the right person. It's right here.

Okay. This is one I didn't include. I thought this might be important to make two points. First of all, this is something that Rod Ewing will talk about in a little while, too. He's done probably the most work of anyone--in fact, I'm sure he has done the most work of looking at basaltic glasses and analogue for nuclear waste glass dissolution. The first point is I showed you the cartoon of how waste glasses look with the alteration layers, the secondary phases, the amorphous phases. If you go and characterize basaltic glasses and look at their alteration products and the mechanisms of alteration, they look very similar to the radioactive waste glasses. They appear to be a very good natural alloy for waste glasses. You see the same sort of surface textures. We have gel layer on glasses. We have something called polaganite on basaltic glasses. There's a lot of similarities and you get similar sets of secondary phases when you adjust for the compositional differences between basalt and radioactive waste glasses. So, to put it in a nutshell, everything we know about basaltic glasses and how they react to natural environments supports their use as a natural analogue.

The other point I want to make is that another question we have in dealing with this problem is how can we do tests that will accelerate the rate, so that we can say we can do a five year test and simulate maybe 1,000 years of relative performance? We have a lot of ideas by raising temperature, raising surface area. One way to use natural analogues, though, is to go ahead and do these accelerated tests with natural materials and then go to a natural system that has evolved for thousands of years and compare the results and see how well it compares.

So, one of these test types that we've done--actually, John Bates has done this at Oregon--is the so-called vapor phase alteration of glasses where you take--instead of putting a glass in liquid water, you put it in water vapor and look at the alteration rate. What happens in that situation is that you have a thin layer of water that condenses on the glass surface and essentially, by doing these vapor phase tests, you have a very, very high surface area to volume ratio because you have a very thin film of water and a lot of solid and essentially can accelerate the reaction mechanism that way. I think this is Rod Ewing's photo-micrograph of a natural basaltic glass.

This is a hydrothermal basaltic glass--sorry, a hydrothermally altered basaltic glass, again from experiments where a piece of the basaltic glass was put in, I think, J-13 water, and reacted at 90 degrees C for some time period. And, in this case, we just see a smectite alteration phase. Do the same thing with one of these accelerated tests, you not only see the smectite, but also see zeolite phases, analcime, thomsonite, argonite, a calcium carbonate phase, and smectite, as you do in all three. But, essentially, we see we can accelerate the tests and we have evidence from this natural analogue that we see the same phases with an accelerated laboratory test at the same temperature as we get in the natural basalt. So, we can sort of use natural analogues to validate our choice of ways to accelerate experimental results.

So, what are the impacts of the basaltic glass analogues? Maybe, most importantly, it demonstrates that glasses with similar reactivities as radioactive waste glasses can remain stable in the upper crust for millions of years without any sort of engineered barrier system. So, we may be in good shape if we can figure out any other clues as to how they are preserved in nature. Again, whether these alteration layers do form on the surface and provide the transport barrier--in other words, we want to compare how fast we predict. I think, essentially, these two are related in that eventually we want to take the model we develop to predict nuclear waste glass dissolution. Since it's a mechanistic model, we should have no problem in just applying it to a natural system where basaltic glass is the thing being altered and provide validation support of the model.

Most of the time when you do studies like this in other areas of geochemistry, you find that in nature things react a lot slower than they do in the laboratory. If you take laboratory data, extrapolate, and apply it to a natural system usage, you find that natural systems react much more slowly. So, there may still be some secrets we don't know about that will help us in that; for example, licensing the repository.

That's it for basalt. I just a have a couple of words. This is sort of a different tactic in using natural analogues. There's glasses called tektites. Tektites are small fragments from a few microns to a few millimeters or centimeters in size. They're believed to have formed in an impact event on the earth from a meteorite impact or some people believe they formed from volcanoes on the moon and were incorporated into the earth. Either way, they're examples of systems where you take natural glasses, put them in earth surface conditions, and they react. So, they're a natural analogue in that respect and they cover the compositional range of nuclear waste glasses, at least some of them do. They get down to about 50% weight silica. In work-related to understanding the origin of tektites, people have looked at their degradation rates in natural systems. This is work of Ronnie Barkatt in Catholic University and two scientists at NASA Goddard (phonetic), John O'Keefe and Sid Alterescu, and Billy Glassey and Richard Delaware. The key observation, though, is that tektites in fresh water dissolve on the average of two orders of magnitude faster than tektites in sea water. They looked at DSDP, deep sea drilling cores, get tektites out of those and look at their dissolution rates or what they infer are the dissolution rates in those conditions versus those in lakes and rivers or on the surface. You find a big discrepancy. The things in sea water dissolve a lot slower. So, they thought, okay, let's go to the lab and see if this is really true in the lab. This is the work that Barkatt did and he found that the leach rate versus time for a tektite in distilled water is much faster, as the data from the real system supported, than sea water. This is about two log units if you look at the actual numbers. And, so they said, well, this might be helpful in radioactive waste glasses. Let's do the same thing with the nuclear waste glasses and they did that and found the same effect. So, one-by-one, they went through the components of sea water until they finally found out that magnesium was the element making the big difference. Adding magnesium to the distilled water as a chloride or a sulfate would reduce the dissolution rate by a couple log units. So, here's some key information. You know, we kind of suspected that there were effects of other cations on glass dissolution rates, but here's a fairly benign metal that can be added to water to greatly affect the rate of dissolution of the tektite or of the radioactive waste glass.

So, actually, all this work that I've talked about tektites was unfunded. We're trying to get some money to actually better quantify. We actually incorporated magnesium into our test fund. We're looking at leach rates and magnesium--we also want to look at the effect of different concentrations of magnesium. We know that up to a certain point that performance isn't proven. Beyond that point, you don't get any additional performance. So, we're trying to quantify that factor to incorporate it into our model.

The impacts of the tektite analogue, of course, magnesium is the component that decreased the rates. We've written into our test plans experiments. We ought to be able to get to those in a couple years, actually no sooner than that. And, the other key part of this that we don't know yet is what is the mechanism that causes the magnesium to slow down the rate? We know magnesium is concentrated at the surface. We don't know if it's the formation of the secondary phase adhering to the surface or magnesium poisoning the surface reactive sites where dissolution is taking place. And, we can get at that with some surface matrix. That just hasn't been funded. So, it hasn't been possible to do that, yet. But, it could be very important in the design of a repository.

Okay. That's it. Summary, what have natural analogues done for us in the glass task? They felt in the case of tektites, identify important effects on glass dissolution. There may be more out there that we need to find out about that we could get by doing more of that sort of work. Influence our experimental plans and also, at some point when we decide on it, we're going to use probably basaltic glass on various sites, one or more, to provide validation support of the kinetic model we're developing.

Thank you.

DR. NORTH: Thank you.

Questions or comments?

DR. EWING: A minor comment, Bill. Rod Ewing, University of New Mexico. On the role of magnesium, Barkatt infers that the dissolution rate decreases because of the lower silica concentrations in solution and that's simply caused by precipitating magnesium silicates when you add magnesium to the solution. It doesn't really change the dissolution rate of the glass.

DR. BOURCIER: If you lower silica, the rate should increase, according to the--model. So, it would go in the opposite direction. Iron has that effect as you make iron silicates and it lowers the iron activity and speeds up the rate.

DR. EWING: Well, we can discuss it, but I would simply point out he didn't identify the solid phases that are--solubility rate.

DR. BOURCIER: No, he didn't. That needs to be done.

DR. EWING: And, that's the key step in determining whether--

DR. BOURCIER: That will give us the mechanism, yeah, okay.

DR. NORTH: Further comments? Anyone in the audience?

(No response.)

DR. NORTH: Okay. Let us go on to the next speaker.

DR. DYER: Our next speaker is Dave Bish of Los Alamos National Laboratory. Oh, I'm sorry, it's Everett Springer who is with Los Alamos National Laboratory. Everett has been with Los Alamos for six years. He's a staff member of the Nuclear Waste Management research and development group of the earth and environmental sciences division. He's worked on various studies of surface and sub-surface transport of radionuclides, characterization of hydrological systems, and sub-surface migration of hazardous waste. And, Everett is going to talk to us about the anthropogenic analogues, specifically radio-nuclide transport at Department of Energy sites.

DR. SPRINGER: Okay. I would like to switch gears a little bit from natural analogues terminology of anthropogenic analogues. I'm not sure necessarily where the term anthro-pogenic came from, just that we view these as man-caused and, in particular, I'm going to talk about potential application of these type of analogues to the radionuclide transport program within the Yucca Mountain Project.

A little bit of background, to pick up on Julie Canepa's talk from earlier this morning, the radionuclide transport program, particularly in terms of the site characterization efforts that we're doing at Los Alamos, validation, we feel, is required for both our models and our parameters and I'll define this. In particular, we're worried about process models that we use to describe the various processes that occur in radionuclide transport, such as sorption and transport, physical processes. And, in terms of parameters, many of the parameters for the radionuclides will have to be developed in laboratory and we have to develop the confidence and the capability that we can take these parameters from the laboratory to the field.

Within the SCP and this activity--I've reversed my first two overheads, I'm sorry. I meant to say that at the beginning. This is Activity 8.3.1.3.7.2, and in order to accomplish this type of validation, we use a combination of controlled field tests, intermediate-scale type experiments. These are both in the realm of controlled experiments. And then, we also have listed natural analogues and anthropogenic analogues and I have here for radionuclide transport to tie in with the particular talk and with our particular efforts. Within this study, we will use both controlled experiments and analogues, natural and anthropogenic, for model and parameter validation.

Let me preface up front, in terms of my own, I am a principal investigator for the study and my own areas are directed primarily right now towards looking at controlled field experiments at Yucca Mountain and the anthropogenic analogues. I am not and do not consider myself an expert in the area of natural analogues and we would look for another principal investigator in that particular area.

The advantages that we see with analogues are that they're associated with longer time and space scales these. This has been said considerably time and time again the past couple of days. Longer time scales in laboratory and field experiments which we have to look at in terms of the repository and, generally, the space scales are larger even though this is a relative term because some field tests can be conducted on relatively large space scales, especially in the saturated zone.

Again, some of the disadvantages, analogues, as we have heard again and again over the past couple of days--I just don't really want to beat this down too badly--suffer from a problem of initial conditions. This gets critical in terms of application of models and how we define initial conditions, particularly from what we know about non-linear type behavior. If we don't define initial conditions readily well, we can be considerably off in terms of our final prediction. And, processes are a couple within analogues and so we have this identification issue of how we pull apart various processes when we're doing work.

The role of analogues, again this has been talked about time and time again. I'll move through this rather quickly. They just indicate important mechanisms not included in the model. I think Mike Shea demonstrated that well with Pocos de Caldas analogue. You can do some testing on the relevancy of laboratory measurements over long time frames, the capability using Kd type measurements, say, in sorption. One of the areas is looking at the relevant migration rates of different species of a given radionuclide. You can test model predictive capability over relevant time scales. And, I, you know, have predictive capability in quotes because we really need to work on defining what we're really looking at; what is, one, the relevant times scale, and two, what do we want to really predict from the behavior. And then, as Dwight noted in his talk, they're potential indicators of important climate and hydrologic changes.

Now, I'd like to move into more of the nature of my talk, the anthropogenic analogues, and note that at many DOE sites throughout the weapons complex, in particular, there's been an introduction, unintentional or intentional, of radionuclides into the environment. And, this time frame is relatively short if we consider that Los Alamos, being one of the initial sites, began in 1943, when you compare this with the natural analogues that we've been talking about where the time scale is on the order of thousands of years. But, they still provide us with valuable information that we would not have otherwise on radionuclide migration.

And, as an example of sites located in the arid and semi-arid conditions at various DOE sites, Nevada Test Site, obviously, is one. Larry Ramspott did a good job yesterday of talking about that. The Hanford site near Richland, Washington; the Idaho National Engineering Laboratory; and I couldn't leave out Los Alamos as a potential site. Each of these sites have had radionuclides introduced in their environment. They all possess unsaturated zones. The geology, obviously, is different between the sites. So, there's that particular issue there. But, in essence, they all can provide us with information.

Why at this point in time--well, we would want to address it, but at this point in time, it's particular fortuitous to address these issues because environmental restoration activities and also waste operation activities occurring at these various sites will allow the Yucca Mountain Project with an opportunity to obtain data on radionuclide migration at a minimal additional cost. These activities will be going on due to various agreements made between states and the sites and the EPA and a somewhat extensive characteriza- tion will be occurring and, for a minimal additional effort, I believe Yucca Mountain Project can obtain rather valuable information.

I want to go into an example. Julie, in her talk earlier today, mentioned it a little bit. It's Area T at TA- 21 at Los Alamos. It was a former waste disposal site for defense waste liquid effluents containing plutonium. It operated from about 1948 to 1953. This is a schematic of the waste absorption area. We had four absorption beds and this gives you a cross section of what an absorption bed looked like. They have a 15 centimeter diameter inlet pipe into a cobblestone layer. They have a gravel, sand, and soil layer above that and then a soil berm. And, we have a distribution box here (indicating) that would put the effluent into the various absorption beds. And then, also, for overflow from these beds, there was pipes connecting Bed 1 with Bed 3, Bed 2 with Bed 4. We're primarily going to concentrate in the majority of the effluents that were received in Bed 1 and Bed 2. And, I'll be referring back to this figure so I'm going to slip on the--

DR. DOMENICO: Was this an experiment or this was--

DR. SPRINGER: No, this was an actual waste--

DR. DOMENICO: You actually did that?

DR. SPRINGER: Wait a minute. I did it? I wasn't even born yet. No, this was actually done. This was the--it started in 1948.

DR. DOMENICO: I guess you pay now, you pay later on that.

DR. SPRINGER: I think that's what DOE is finding out at this point in time, yes.

I'm going to go into a little bit on the history there.

DR. NORTH: 20 by 100 feet, is that the way these compute out?

DR. SPRINGER: Something to that effect. I'm not--I haven't really worked out the units that well.

DR. NORTH: These are not small scale?

DR. SPRINGER: Beds? No, they're rather large. They're not labs.

We were talking about performance assessment and what you're reading right here is the performance assessment done for this particular activity, even though the results were reported later than the actual activity. What was done was cores of Bandelier tuff from around Los Alamos were obtained, and if you think back to the bed if you can get to the cross section at the bottom here, basically the material is moving into the Bandelier tuff which is unsaturated. And ambient water conditions at Los Alamos from Bandelier tuff run about volumetrically 6 to 10%. Saturation-wise, that's anywhere from a maximum of about 30% or so saturations. Average of precipitation at Los Alamos, in case you're interested, is about 18 inches a year, half of which is snow and half of which is rain.

But, what it was is that Christensen and others obtained seven cores of Bandelier tuff and went into a laboratory and did some experiments. They ran three of radionuclides, three of plutonium, strontium, and cesium. Ran them through in volumes individually and then in kind of a soup. I'm just going to talk kind of about the individual measurements here. What they found is that in their core plutonium penetrated about two inches. So, this was the performance assessment, basically what you're seeing. So, they felt, all right, there's no real problem. They ran several pore volumes through on these columns. It's not a matter of just dumping a single pore volume through. They kept the column saturated and kept it running through.

They did find localized hot spots of plutonium and strontium associated with ion exchange material in the particular columns when they dissected them. They found that cesium was retained in the top inch. And then, they did a particular experiment with the strontium where they varied the water, Los Alamos half water versus distilled water, and they found a difference in the breakthrough curve. But, the key was really this result (indicating) and this result (indicating). Even though the strontium broke through in the liquid effluence that went into the particular beds, there's not an exceptional amount of strontium and cesium. It was, you know, primarily of plutonium and americium that was of the major concern. So, essentially, the belief was and the feeling was that this was a safe operation.

DR. LANGMUIR: Everett, did they understand why the strontium breakthrough was different?

DR. SPRINGER: No. No, they never investigated that very well, at all. They just noted it. Actually, there's some very interesting information when they mixed them with the soup, too, but they don't really go into a lot of detail as to what really occurred.

So what came along about 1960--well, these absorption beds not used any longer after 1953 because they had essentially become clogged, so to speak, with suspended material. So, they were no longer functioning. Also, about that time, another waste treatment plant came on line and it cleaned up the stuff well enough where they actually diverted the effluent around the beds and down to one of the canyons and let it run out of Los Alamos. So now they've got another problem, in addition. The characteristics of the effluent also were not very well done in terms of what other constituents were in the waste stream besides the plutonium and americium. So, it's not really clear what else might have gone into the beds.

Along about 1960, they decided, well, we'll flush these beds and see if we can, you know, see what happens. And, what they did is they only used Bed 1. Again, this was an experiment. They used Bed 1. And, they diverted 200,000 gallons of effluent over 25 days and this was the effluent from the treatment plant over 25 days. Then, in the next 32 days, they added 200,000 gallons of tap water on top of it. Then, in 1961, they did a second study. They had 211,000 gallons of effluent in 23 days and there's a bullet missing on here, but there was another, about the same amount, about 200,000 gallons of tap water put on top of it in about 30 days. So, this was an equivalent. They added about 9 meters head of water to these beds over the two year period there.

They were not looked at--I mean, they were looked at kind of casually, but they were not looked at again in detail after 1960--well, they measured water content with neutron probes and they had some suction sampling devices down about 30 feet and they were looking at some of this behavior, but they really didn't look at anything again until about 1978 when Jack Nyhan came along and drilled four holes, which are noted here, into the absorption beds. This was Hole 1 and 2 in Bed 1 and Hole 1 and 2 in Bed 2.

What we see here from this particular study, his results on radionuclide concentrations. And, if we look at Bed 1, we see that--and this was the maximum depth they drilled at that time--they found they were still finding some plutonium down through approximately 100 feet and we see that the effect of the infiltration had moved the plutonium quite a distance. Bed 2 which had just received effluent--it didn't receive anything in the 1960-61 studies. So, it just had received effluent. We see that the plutonium is--it's up in the upper 5 meter range, but if we go to--

DR. DOMENICO: Was that all saturated due to the--

DR. SPRINGER: No.

DR. DOMENICO: It was all--where's the--

DR. SPRINGER: Oh, wait a minute, wait a minute. Go ahead?

DR. DOMENICO: Due to the effluent, you probably built--

DR. SPRINGER: Yeah, yeah, there was.

DR. DOMENICO: Was it all saturated?

DR. SPRINGER: No. The whole bed?

DR. DOMENICO: No, it was not?

DR. SPRINGER: No, it was not.

DR. DOMENICO: What was the unsaturated zone--thick?

DR. SPRINGER: Well, I mean, you know, the bed was saturated with a--they introduced the effluent and then it would drain down through.

DR. DOMENICO: So, that was--your first 100 meters or so are dry?

DR. SPRINGER: Now, they are, yeah. Yeah, the water contents are still somewhat higher, but over ambient, they're still basically--they're not saturated. The saturation in the process from what was observed from neutron probes and observations, saturation was pretty much retained to the bed in the upper layer and then it, you know--unsaturated flow took over below that pretty quickly. You know, the saturated conductivity of Bandelier tuff runs about anywhere from 5 times 10^{-6} to 5 times 10^{-7} meters per second. So, it's relatively quick when we compare it to some of the material we see at Yucca Mountain or other places. Relatively permeable. And, also, the processes are 30 to 50% depending on where you're at within the material. If you do modeling of the pulse, you do see some saturation, but it becomes unsaturated relatively quick.

The key here is that plutonium--and this was the maximum depth they could go at that time--was found at approximately 100 and then we do not see--well, we saw some movement in Bed 2. If we look at several studies conducted over time at the particular site, we see we have three different studies related here or connected. In 1953, Herman of the USGS did some work on this particular absorption bed. Now, these holes are not all located, you know, simultaneously. They're sampling different locations around a particular bed. If you look at his data, this was the distribution of the plutonium. If we look at the 1960 data following the initial infiltration pulse, this was the distribution of the data. But then, we go to the 1978 data, this was the distribution. So, we see that we have moved the plutonium over time and that the infiltration, whether it was effective in cleaning the bed out or not, it was effective in moving the plutonium further into the system.

What we have learned, there's been several attempts to model this particular site and model the behavior of the pulse, also. And, our initial conditions, at least, were reasonably well-defined. We know there was no plutonium there prior to the introduction of a bed. And, we also have a fairly good idea what the ambient water content and water conditions were at the site. But, if we took like data from Christensen, et al. and used a linear approach, we cannot reproduce the response that was seen in Bed 1. So, automatically, you know, it indicates either, one, we don't have the right hydrology or we don't have the right chemistry, one or the other, but we're missing a particular process or something is not occurring correctly.

Other modeling attempts have been made in order to --in order again, using a porous medium continuum and trying to using a porous medium continuum approach on this particular site. They've used a colloid transport model, in terms of--this is NuHall and Travis--to describe the behavior of the plutonium and that was in terms of trying to get reproduction on the 1978 data over time. If you use a linear Kd, you actually don't even see this much of a transport. You actually see it much closer within the 5 meter realm, unless you--well, depending how you--you know, if you want to put the Kd down to .01 or something like that, then you can move it a lot further, but using values of Kd obtained from the laboratory. So, NuHall and Travis used a colloid transport model. They were able, they felt, to reproduce the curves adequately, but again this was a type of a fitting exercise. And, also, we came back in and have used a spatially distributed absorption model and we've been actually--I mean, again, it's a matter of gaining. We've been able to reproduce the spikes and the serrations that you see there and also to move it down depending on how you distribute the absorption term.

What it keeps coming back to, though, and whenever we do modeling--and this is, I think, one of the things that is important--we like to close on certain things and, obviously, one of the first things we like to close on is mass in terms of how these systems behave. And, we really have very limited knowledge of the source term, what went into the system not only chemically, but even in terms of magnitude. Estimates have run about approximately 10 curies of plutonium, but this can vary quite a bit. So, if we don't have a very good knowledge of source term, we continually play games with these particular parameters in the model and, therefore, we can make everything look good.

But, the key for the Yucca Mountain Project, it's now environmental restoration is coming back into the site within the next two years and they will be investigating at a much more detailed level in order to meet EPA regulations and they have to do their own performance and risk assessment. And, at Area T, itself, they're going to come in now with 21 vertical and slanted--a combination of vertical and slanted drill holes to characterize the hydrologic system a little bit better in terms of fracturing and also to characterize the distribution of the plutonium a little bit better. And, I feel that this represents an opportunity for the Yucca Mountain Project and some of the things that can be done is we can look at the difference in migration rates for the various isotopes of the plutonium of the plutonium site. We can do some model testing since the '78 work. We can analyze--and it really hasn't been done. Even though they find fracturing in the core in the '78 study, they really didn't look at a distribution of radionuclides between the matrix and the fractures and this might give us some indication of various flow paths and processes. And, if we have localized high concentrations of radionuclides, which we did see on the distribution, we might be able to look at the associated mineralogy and see what might be retaining the radionuclide at that particular site. So, these are some potential studies.

DR. NORTH: I think before we go on, I'd like to add a comment. I mean, I have not seen this data before and am really impressed with its importance. Here, we have a situation where plutonium has migrated the order of 100 feet in tuff in a few decades. And, from what you told me, I'm not sure we understand why.

DR. SPRINGER: I think that's correct.

DR. NORTH: It seems to me that for the assurance of safety of Yucca Mountain that has got to be a crucial question for performance. We need to understand why and we're going to need to explain to the public why this can't happen at Yucca Mountain.

DR. SPRINGER: I agree, but I would like to relate as to why they never studied all the processes that we are necessarily studying at this point in time. So, they did not take the same approach and they have to answer why, too, now because they're under their own guiding set of regulations in terms of trying to figure this out for Yucca Mountain. I think this does represent a good opportunity to come back and study it. The key is can we go in now and study something that occurred 20 to 30 years ago or 30 to 40 and figure out why? Because if we don't know all the inputs--I mean, there may have been a high organic slug coming in on the waste stream or something. If we don't know all the background here--

DR. NORTH: Okay. Let's try that. Suppose the issue is organics in the waste stream, I've heard that in other situations where plutonium has migrated, there appears to be a strong link with the organics. Can we find out what that is? Can we study the organics that are still in the minerals?

Can we run some experiments, perhaps, to try to confirm our ability to deal with the organic complexes and then can we also look at the implications of that hypothesis which perhaps we could start doing right now? If we conclude that it is organics that make plutonium susceptible to migration of the kind that has occurred here, then it may be a very crucial issue for repository design to make sure that no such source of organics is present. This might include no use of fossil fuel underground. We run everything on electricity. Very stringent work force rules with respect to disposal of garbage and other waste. It is not going to be left in a hole somewhere. And, very strict use of paints, organic solvents, lubricants, et cetera, so that that stuff does not get spilled underground. And, if you want to place for quality assurance, I think this is going to take a considerable effort to be able to assure that no such source of organics, at whatever limits we decide we might need out of the calculations we've made, can be done. Maybe, we conclude that one half a ham sandwich thrown down a hole will permit kilograms of plutonium to migrate.

DR. DEERE: I think you've carried the supposition quite a ways. I'd like to say that the geological medium has a fair amount to do with where things are moving and permeabilities and, obviously, this was not well understood at that particular time.

DR. SPRINGER: That's right. Yeah. I want to--

DR. DOMENICO: If plutonium has a distribution coefficient in laboratory, you would expect it to go nowhere.

DR. SPRINGER: That's right.

DR. DOMENICO: Plutonium has been found several tens of yards outside of Maxie Flats to a shallow disposal site in Kentucky.

DR. SPRINGER: That's right.

DR. DOMENICO: That's been attributed to kelating which is your organic or it could basically be colloids. One or the other will move that stuff at the speed of groundwater, regardless of what your laboratory Kds will say.

DR. SPRINGER: That's correct.

DR. DOMENICO: So, those two ingredients, obviously, are very important in terms of control of the migration of highly retarded radionuclides.

DR. SPRINGER: And, a followup to that, this was a waste treatment process, the tail end of it, which they may have been flocculating because currently they do do some flocculation at the tail end. So, you may be inducing some colloids. But, the other key is if they did flood the site and move this down a considerable ways also and, you know, we have to go back and try to recreate something that's already occurred which always opens us up to skepticism about what really did happen.

DR. CANTLON: The comparison, though, between Bed 1 and Bed 2, I think, needs to be emphasized here. What's the water equivalent? You said 30 meters?

DR. SPRINGER: Nine meters.

DR. CANTLON: Nine meters--

DR. SPRINGER: That was an infiltration experiment, yeah.

DR. CANTLON: Nine meters of water put in in one annual disposal period--

DR. SPRINGER: Over a two year period, yeah. Basically, two--yeah, basically a year, right.

DR. CANTLON: Two year period.

DR. SPRINGER: Right.

DR. CANTLON: So, you moved a lot of water and what was the source of the water?

DR. SPRINGER: Well, it was some effluent and then some tap water.

DR. CANTLON: Some effluent, you see, that's the important point.

DR. SPRINGER: Yeah. And, having spent the past year investigating the site in terms of getting ready for its ER effort and gone through the records, believe me, regardless of what we believe, the records are not there to back up what the waste stream looked like at this point in time. And, a lot of this stuff has been declassified since this time and we're obtaining access to it and it's just not--the information is just not there. Obviously, conditions were different at that point in time in our history and things were looked at differently. I think what I'd like to stress--and I agree with your comment. I mean, I think this is--we feel it tells us something and it's again looking at processes and what we would predict with our models and what we see and so we try to learn from that. And, I think this is the advantage of analogues and that's why we'd like to go forward and look at these types of situations. And, I don't think the cost is enormous because a lot of the work is already going to be done. If the Yucca Mountain Projects sees that it wants something off these, then it can take advantage of that. But, otherwise, these type of data are going to be generated because they have to be generated for their own risk assessment efforts at these particular sites.

DR. LANGMUIR: Everett, it sounds like you've optimized transport in this case, though.

DR. SPRINGER: Well, that's my--

DR. LANGMUIR: --water on top of the system to drive it --and probably follow it--and complex system, it's the maximum possible transport--

DR. SPRINGER: Yeah. Sure. Sure it is.

DR. CANTLON: --unplug the bed, remember?

DR. SPRINGER: Yeah. Yeah, I agree and this is the example--to begin with, as Julie noted earlier, Dave, this example is in Chapter 4 of the SCP. This is not necessarily anything new we're bringing forward. I mean, I do have some further data since Chapter 4 or some further investigation because I was working on this for a while. Second, yeah, we have optimized transport because that's my slant. I have to try to build studies to look at the particular problem. If somebody came to me and said we need effort in sorption or something like that, I might go a little bit different way. But, I anticipate the sorption PI would identify something that he could--you know, how he would attack this particular problem for sorption or early in something else. And, there are other analogues. You know, yourself, the Hanford site has particular ones. There are other analogues at Los Alamos where plutonium are available under much different conditions that could be used. And, this one is convenient and it's relative--they're instrumatic, too. I can't argue that.

Larry Ramspott, yesterday, did a good job on the radionuclide migration program, but I just want to note that that is another area. As Larry noted, it was established in 1973 and managed by Nevada Operations. The participating organizations have been DRI, LANL, Livermore, USGS, and we always, you know--I believe this is one reason why these three organizations are involved in this particular project, they have a history of experience working at the Nevada Test Site. And, the program goal, at least initially, was to determine the extent of movement away from underground nuclear explosions and to look at these particular mechanisms.

Two particular shots that Larry discussed yesterday also and I just want to run back through them again real quick. Again, the Cambric materials in Chapter 4 of the SCP, but Cambric shot was conducted in '65 at approximately 73 meters below the water table. We had a satellite well used for pumping about 91 meters away. This well has been pumped almost continuously since 1974. And, what we've seen basically in the satellite well pumping has been tritium, krypton-85, and chlorine-26. Cesium and strontium, as Larry noted, were detected in the test cavity, but they have not been detected in the satellite well. So, this is one particular event on one particular set of information.

The Chesire shot, we know that it was in 1976 in brecciated rhyolite at the Pahute Mesa and it's approximately 544 meters below the water table. There's a satellite hole drilled 300 meters from the cavity. And, I believe, as Larry noted yesterday, this hole was not going to be pumped. This was drilled basically in line with the regional gradient, downgrading it from the particular site. And, they've found tritium, krypton, strontium, cesium, antimony, cobalt, cerium, europium in this particular hole, and some of these isotopes, as was noted by Larry yesterday, have been associated or are believed to be associated with colloids. I think the jury is still out on that.

DR. DOMENICO: What does that mean? He wouldn't explain that yesterday. Would you explain it?

DR. SPRINGER: I won't explain it neither. I think one of the things in this colloid--and we've been going around at Los Alamos and this is kind of the way we're seeing it--we're not sure about sampling because when we start pumping, are we creating things? But, I assume it means that when they took these particles out and they did the filtration, they found--at least on that particle side, they found--

DR. DOMENICO: But, you're not pumping that?

DR. SPRINGER: Well, they're still pumping to get to the surface. Yeah, still pumping to get it up. But, I think that's one of the concerns in this colloid thing right now is how to sample and how to get out that particular problem because when you start pumping you may be generating these things locally.

DR. LANGMUIR: Everett, just a curiosity, maybe you wouldn't know this, but when you develop a well after recovering the water from a system like this and you don't use traditional well installation procedures, you're looking at pumping forces against the bedrock. You're going to create colloids every time you turn it on.

DR. SPRINGER: Um-hum, right.

DR. LANGMUIR: So, they may or may not be related to a naturally flowing system. They may be induced to come up to the well by the surging process when you pump.

DR. SPRINGER: Right. I think that's--

DR. LANGMUIR: Were these wells developed in the traditional way?

DR. SPRINGER: I'm not sure on the Chesire well. I'm not sure on the Chesire well, how it was developed. So, I couldn't tell you that at this point in time. The key is that it was--like I said, I believe it was particle size association. So, the assumption is, you know, that they--

DR. LANGMUIR: So, the sizes are--

DR. SPRINGER: Yeah.

DR. LANGMUIR: Somewhat larger than that.

DR. SPRINGER: Right.

DR. CANTLON: But, the fact that they're 300 meters from the shot means that they got there somehow.

DR. SPRINGER: Yeah.

DR. CANTLON: So, it couldn't be from the colloids developed in the well.

DR. SPRINGER: Yeah, I think that's the assumption.

I want to talk about the tunnel complexes just for a minute. Again, from the standpoint of our program, Dwight noted a little bit in his talk about using Rainier Mesa. Yeah, we believe it's a rather valuable analogue also. If we look at the N-Tunnel complex, I'm not sure who has or has not been there, but you're basically looking at zeolitic tuff occurring with perched water zones and there is some flowing water. If you talk to the personnel in the tunnels, the --water that flows, flows for a limited time even though there are some faults that have flowed continuously at, I believe, rates of around one to two gallons per minute. But, they find this rather consistently in the zeolitic tuff. If you go to the vitric tuff at P-Tunnel, there's very few defined fractures and there's a vitric/zeolitic interface and perched water was only recently encountered in P-Tunnel in the zeolitic zone. There was no perched water encountered in the vitric zone.

The reason I bring these two tunnels up is because when we talk about the Calico Hills unit at Yucca Mountain, we're basically looking at zeolitic to vitric zones as we go from north to south at Yucca Mountain and we also know the importance of the Calico Hills unit and so we have again a type of an analogue for that particular area.

Possible areas of investigation in the tunnels that could be done, we can look at isotopic compositions of matrix versus fracture water, at least fast flowing water, coming out of particular faults. We can look at variations of water chemistry across the mesa. It's a little bit wetter because, I think, the average precipitation at Rainier Mesa, the top is around 14 inches a year. It's a little bit wetter. So, we might be able to look at some variation of water composition across the mesa that we might not be able to, you know--whether we can get that at Yucca Mountain or not, it's a little bit wetter. So, we might be able to not have to use as much squeezing. And then, analysis of flowing and perched zones in terms of occurrence. This is basically just a statistical look at things. The fractures and the system has been mapped relatively well. We go in and mark the ones that have flowed, haven't flowed, and just kind of statistically look at the behavior and just give us an idea of maybe what might occur at Yucca Mountain or what might be possible in terms of some sort of statistical distribution.

I'd like to conclude by saying that I think these data will provide where we can use DOE sites, provide us some valuable information about radionuclide transport, and as noted, these analogues can be useful for indicating whether important processes are included. I think, you know, we've gone around on that. This expertise, you know, developed by these DOE programs, not only environmental restoration, but the Yucca Mountain Program, they're complimentary for each other and, more or less, they can both--they can work synergistically.

And, a final note, this last one's just that I think those that worked like Larry Ramspott that whenever you do this work, there's other activities that precede what we do. More or less, we might be tagging along. And, like, when you're working with the weapons community, the importance is the shot, not what we get out of it. So, we have to understand that there's operation constraints and we don't come in and make these dictations, you know. They kind of say when we can do these things and when we can't. And, I'd just like to note that what we need to get out of this is again trying to find some clear objectives. If YMP does participate, we would obviously want to find some very clear objectives.

So, that would conclude and I'd like to thank you.

DR. NORTH: Thank you.

Questions or comments?

DR. CANTLON: When you were talking about the Nevada Test Site where you had no data on any plutonium movement there--

DR. SPRINGER: Not that I--yeah.

DR. CANTLON: I think from yesterday, too, we--

DR. SPRINGER: Yes.

DR. CANTLON: The plutonium in the shots was held very close to the shot hole.

DR. SPRINGER: I guess it gets--Larry can answer this better than I. It goes to the--

DR. DOMENICO: Explain something to me on these shots. I always thought that you fused the glass all around, the bulb, and everything was protected inside. That's not what happens?

DR. DEERE: No, it starts caving in.

DR. SPRINGER: Yeah. It breaks--

DR. DEERE: It works out. It may be minutes, hours.

DR. DOMENICO: So, this is not a case of glass dissolution and radionuclides working out. This is a case where they were just free to move?

DR. DEERE: And, the glass often is driven out like a dike or a sill. So, when you come back with re-entry, you'll come through and often run into these basalts.

DR. DOMENICO: So, it's a myth that you presumably encapsulate all of these dangerous materials when the shot goes off. That's correct?

DR. NORTH: Everything that's reasonably volatile isn't going to go into that glass. It's in a gaseous form as that glass is being formed. And, so it doesn't go into the molten slag, it's elsewhere.

DR. CARTER: Yeah, but I think it's a mixed bag. Some of the things and presumably the plutonium is, in fact, primarily fixed at the site. Other things may not--

DR. SPRINGER: May not be, yeah. Some of the volatile ones actually will break down in the fractures as gas and then condense out in the medium somewhere.

DR. DOMENICO: Is that what you're observing, the volatile ones in these wells 300 meters away?

DR. SPRINGER: No, not all of them. No. I mean, what you're seeing, the volatiles, you're seeing tritium and krypton, yes. Yeah.

DR. DOMENICO: You're seeing the mobile ones.

DR. SPRINGER: The mobile ones, that's for sure. Again, I'm not enough of a nuclear chemist or a nuclear physicist to relate all this to you, but that's--you know, we haven't seen a lot of other things, that's for sure.

DR. NORTH: To me, the overwhelming issue here is here is an opportunity to observe plutonium and the spectrum of fission products being injected, as it were, into a geological environment. And, we have a time scale of decades over which that process has gone on and we can look at the results of what's migrated and what hasn't and try to understand why. Not a perfect analogue, by any manner or means, but certainly a potential to learn some things of potentially great importance regarding the performance of the proposed repository.

DR. SPRINGER: I think we would agree with you.

DR. CARTER: Warner, I wouldn't give all the credit to Los Alamos. Some of the other DOE sites that also compete for that honor--

DR. SPRINGER: Well, I don't think we'll take all the credit. As a matter of fact, we're relatively benign. Most of the lists that come out, we're down near the bottom. So, that gives you an idea where some of the others sit.

DR. DEERE: Was that well, the Chesire, the satellite well, a pumped well?

DR. SPRINGER: I don't know. It was only pumped during sampling. It was basically drilled down-gradient. It was not like the Cambric situation where they drilled it and then pumped it continuously to keep the water. It was basically drilled down-gradient in terms of trying to intercept a plume that would come away from the site.

DR. DEERE: Did they bring the plume in the process of drilling and original--I don't know why the question arose in my mind today and not yesterday.

DR. SPRINGER: Go ahead, Larry?

MR. RAMSPOTT: To my knowledge, and I haven't been associated with the program closely for a number of years, but basically it was a pump well in the sense that they had to pump it to complete it. And, going through some of the completion type of stuff that was pointed out by Don Langmuir, that is the reason--even though the water samples now, we believe, have the radionuclides in the colloidal form, we have no way of knowing that the water actually got that 300 meters as a colloid. It may have come over, dissolved, and then somehow associated with colloids because of interactions with the metal casing or with the material or the cement and things like that as we have surged and pumped the well. The well is not being pumped, however, right now to get samples. I think they pump to take samples and then shut it off. But, it's not a continuously high volume pumping like at Cambric. And, we're having problems with getting even samples out of it. I was talking to the people at the Nevada Operations Office and now we used to pump wells like that and haul the water over either with a line--we'd pump it or we would reinject it in a nearby shot location and now we can't do that. We have to put it in the tanks. And then, we have to let those tanks evaporate and then we have to scrape the sludge from those tanks off and put them in barrels and haul them down and dispose of them as low-level waste. And, so the amount of pumping is very restrictive compared to when we used to be able to pump it over to another contaminated site and just reinject it at the other contaminated site. So, that particular experiment is not really moving forward, at all, right now. They're looking at permits so that possibly we can go back to reinjecting the contaminated water. And, so we're getting very little data out of that.

DR. DEERE: Well, the reason for my question, I wanted to make sure that we didn't get the wrong impression that all these materials were at that depth and that position 300 meters away at the time the well was put there because it may be in the process of drilling the well, of developing the well there was enough water to induce a gradient. I mean, you could have perhaps drilled it upgrade and not down-gradient and the pumping would have caused flow in that direction, which we have done in some cases in making tests. We create the gradient and, with time, watch the materials arrive as a plume of material.

MR. RAMSPOTT: I think people believe that it's a natural gradient. We may have affected it somewhat in the development of the well.

DR. DOMENICO: But, your colloids aren't going to move any faster than your tritium or your chlorine, maybe slightly because of what you call electrokinetic effects which I don't believe in, but it's basically going to--those are all going to move at the same velocity. So, what you're getting in that well are those that are not retarded, at all. Colloids are not, basically, which is still pretty fast.

MR. RAMSPOTT: It seems to be moving associated with the tritium and other gaseous material, things that move fast.

DR. NORTH: Any further questions?

(No response.)

DR. NORTH: Okay. We have one last speaker before lunch.

DR. DYER: Our last speaker is David Bish of Los Alamos National Laboratory. Dave has been at Los Alamos for 10 years. He's worked as a staff mineralogist in the geology and geochemistry group. He's participated in the Yucca Mountain Project since 1980. His interests include the use of x-ray powdered fraction methods for quantitative analysis and crystal structure refinement. He's investigated the behavior of zeolites and clay minerals at elevated temperatures and under varying pressure conditions. In addition, he's been active in unraveling the diogentic and alteration conditions of--tuffaceous rocks and I would add that Dave was recently elected a fellow of the Mineralogical Society of America. And, he's going to talk to us about the natural analogue mineral studies at Yucca Mountain.

DR. BISH: Thank you, Russ. It's a pleasure to be here today presenting this information. I'm going to break with tradition and talk about something that actually was funded by the Yucca Mountain Project, believe it or not, QA and all. I'll present some, what I think are particularly intriguing mineralogic data that allow us to get some information that we've had a hard time obtaining in the past by other methods. It seems kind of unusual to talk about using the observed alteration at Yucca Mountain as a natural analogue for a repository-induced alteration, but in fact, I think that's what we can do.

Not to belabor this too much, but there's two things or two points here I wanted to make. First, as the type of information that I'm interested in obtaining, is the long-term behavior of the minerals, primarily the secondary minerals, in a repository environment at Yucca Mountain. Because of the low temperatures and long reaction times involved in the repository environment, namely generally below 100/C and times upwards of 10,000 years, it's pretty difficult to obtain useful information in the lab. We've found that it's practically impossible to obtain what I think are equilibrium results.

You've seen all this before. I put this up. I wasn't in cahoots with any of the other speakers when I put this together. I put this together based on our experience with some natural analogue studies, again funded by the Yucca Mountain Project, at sites in New Mexico and Nevada, including Bailey Hot Springs which we've studied a fair amount looking at the water chemistry and the mineralogy. In the end, we concluded that we really couldn't get much useful information from any of these analogues because of these caveats. We had difficulties defining the past conditions. If we wanted to be able to understand the past conditions properly, we'd end up having to do a site characterization study at one of these sites like we're doing at Yucca Mountain. It's difficult to locate both representative conditions; temperature, water vapor pressure, for example, and representative mineral assemblages. And, in fact, we concluded that, for example, Bailey Hot Springs was not representative of much of anything at Yucca Mountain.

I'll put this up just to put everything in context. The type of thing that I'm going to conclude with today is a bit of information on the behavior, as I said, of the secondary minerals. This is in your packet towards the end and you don't really need to go back there yet. The point I want to make is that the repository horizon is purposed to be in this region right here, in this depth. This is for G-1. And, the important thing to note is that on a relatively short distance beneath this depth are large concentrations of clinoptilolite which is a fairly sorptive zeolite--it's probably the most important sorptive zeolite at Yucca Mountain--lesser concentrations of mordenite and with greater depth, we go into things like analcime. So, that's the type of thing that I want to examine today. I'll come back to that in a few minutes.

DR. DOMENICO: Is analcime a zeolite?

DR. BISH: Well, it's called a zeolite. It's not a very good zeolite and, in fact, I don't think it's important to the repository, other than as a potential reactant. It's pretty deep, as you can see.

I preface everything I say with this nice little figure from a paper by Joe Smythe. This is results of some work he published in 1982 and it got a lot of attention. I believe it's still getting a bit of attention from some of the members of the board, included. Joe was interested in determining the approximate temperatures that we might expect the clinoptilolite, which underlies the repository horizon, to react to phases, such as analcime. Joe put together a plot here of sodium ion concentration on this axis versus temperature based on a variety of types of data, primarily based on the work of Iijima, in which he plotted, what he called, Zone 1 that contains--or rather Zone 2 that contained clinoptilolite, Zone 3 that contained analcime, and then Zone 4 containing authigenic albite. And, Joe postulated that there was a relationship, and his figure bears that out, between the sodium ion concentration and the temperature that these reactions would occur. Joe plotted what he thought was the Yucca Mountain repository water composition up here at 200 something ppm sodium. In fact, today, we know that it's down in the 45ppm range. I'm not sure that would make much difference to Joe since this is just about vertical. But, the important thing is that in the end Joe concluded that clinoptilolite would react to an analcime in a repository environment at about 105/, and applying various caveats to that, he suggested that the clinoptilolite-bearing rocks be allowed to be heated to no greater than 85/C. So, we'll examine that whole tenant there.

What I've done and I must admit that this didn't start out as a natural analogue study; it started out as part of the site characterization effort that I was involved in. But, essentially, what is involved is that I've used the transformation of smectite to illite throughout Yucca Mountain that we've observed over the years to determine the approxi- mate paleogeothermal gradients. In other words, I've been able to estimate the maximum temperatures to which the minerals that we observed in the drill holes have been subjected to in the past. I've also used fluid inclusion homogenization temperatures from secondary minerals, although I admit that we have very few analyses and I'm not quite sure whether they're representative of the reactions that we're seeing here. Then, later on, I was able to use potassium argon dating of the illite smectites at Yucca Mountain to constrain the ages of the alteration event that we're seeing evidence of at Yucca Mountain. The end product that we're after is to be able to determine the apparent long-term stabilities of minerals. In other words, get rid of the problems that we have of trying to react clinoptilolite to analcime in the lab at 100/C. I don't think it would happen in my lifetime.

DR. DOMENICO: How did you handle the time element? There's temperature and time in these transformations.

DR. BISH: Correct. Maybe, you'll see--if that question still remains in a few minutes, let's get back to it.

DR. DOMENICO: Okay.

DR. BISH: Maybe, I'll answer that as I go along.

Just going first to that first bullet, how do we determine the temperatures of reaction? There's been a fair amount of work published in the literature that allows us to relate, as I said, the type of illite smectite to the temperature to which these minerals have been subjected. This top figure is a relatively old one from some work on Gulf Coast sediments which shows an okay relationship between the percent of expandable layers or the smectite layers and temperature. Something funny happens down here (indicating). Essentially, that's the onset of ordering that we see here (indicating).

This paper here summarized the results of studies on illite smectites in a large variety of environments including the Gulf Coast sediments, politic rocks; it also includes tuffaceous rocks from Wairakei and Broadlands, tuffaceous rocks and volcanic rocks in California. And, these two authors, both of whom have a large amount of experience in studying illite smectites, concluded that the temperatures of transformation amongst all of these different rock types were pretty consistent. Here, we're down in this random zone, which was that upper part of that plot that I just showed up above, and around between 90 and 100/C, we transform to an ordered--I won't really go into what that is, but suffice it to say it's easy to identify using x-ray diffraction and these are all different. Around 175/C, plus or minus of a blob--it could be plus or minus 20, 30/ probably--we transform to a more highly ordered illite smectite and then we go into illite. And, we don't need to go any higher because we don't see this transformation in the rocks at Yucca Mountain. And, I've used these types of systematics to extract information on the paleo-temperatures at Yucca Mountain.

I should mention, the question of kinetics will come up or it kind of already did. The reaction from the random illite smectites to the R1 ordered illite smectites does appear to be affected by kinetics. It's at a low enough temperature that in some geologic systems, such as intrusion of a dike into clay-bearing rocks, this reaction does not go at that temperature. It's inhibited. It doesn't go fast enough before the rocks cool down. I believe that there's no evidence that in any large scale system like Yucca Mountain, where it's not going to heat up quickly and cool down quickly, that kinetics are a problem even at that temperature. And, there's no significant, there's no evidence of significant retrograding. In other words, if we get up to here and we cool back down, do we come back down to here? There's no evidence that that happens.

Using that information and my observations at Yucca Mountain for three drill holes which just show up here--and, I'll just leave that up. This is a large scale map that's in your packet, I think, twice actually. Using information from G-3 which is the southern end of Yucca Mountain, G-1 which is just about the northern end of the repository block, and then G-2 which is just a little bit north, using information from these drill holes, I was able to construct a schematic paleogeothermal gradient for each of these drill holes; namely, G-2, G-1, and G-3.

And, I've also put on these diagrams the present day geothermal gradients. Oddly enough, they're in the same order. That's probably not surprising. I should emphasize that these are schematic and, in fact, if I adhered strictly to my mineralogic data, which I wasn't too sure about them when I did it because of the way it would make this curve look, but essentially this curve would come down about like that and you'll see that when I'm talking about it in a minute.

But, we see at the bottom of G-2, we've got evidence for temperatures in excess of 250/C. So, we can look at the types of things that have happened to the zeolites when they've been exposed to these temperatures. We can go down in temperature with the same mineralogy going south through Yucca

Mountain.

I'll just throw up the fluid inclusion data quickly for you. The temperatures are lower. I'm not sure what that implies. There's some difficulty using fluid inclusion data from calcite that we've experienced, particularly in the shallow rocks, and there are several papers in the literature talking about errors in fluid inclusion temperatures from calcite due to incomplete filling or excess gas obtained. In any case, these temperatures are lower than the illite smectite temperatures, but they're significant. We get up into the 200/ range. The other problem is we don't really know when these were formed. These could have been formed millions of years after that, although I doubt it.

DR. LANGMUIR: How much lower are they, Dave?

DR. BISH: Well, you can probably--it's easiest if I put this over here to answer that. You can compare a couple temperatures across here. This deepest one isn't really a problem; 5820 depth, we're at over 200/. So, that's really not a problem. The problem is when we get up to say 5400 feet down here, we're still up in the 200s, whereas the fluid inclusion temperatures are down around 100.

We have a limited number of fluid inclusion data, as you can see. It's very difficult to find them and, in fact, it was originally suggested that we look for fluid inclusions in zeolites and I don't think that would provide us any useful information because of the micro-porosity of the clinoptilo- lite. In fact, isotopic data suggests that the minerals would come to equilibrium very quickly with any new fluids.

Now, the second part, I mentioned that we were interested in obtaining information on the timing of this reaction. So, I got together with a gentleman at Case Western Reserve University, Jim Arenson, who had a lot of experience dating illite smectites. It's not something that many people can do. I'm not aware of too many other people that can do it well. But, we got some pretty consistent results on a variety of clays from both G-1 and G-2 and clays with varying potassium contents and varying degrees of order. You see, we've got an average age of 10.7 million years. And, just coincidentally, the Timber Mountain-Oasis Valley Caldera which is up here (indicating) was most active between 11.5 and 11.3 million years and the intracaldera lavas were erupted around 10.7 million years. And, I understand from the people I work with who are into volcanology that it is this type of process that would produce the hydrothermal plumes that would affect the rocks to the south of Yucca Mountain. So, we're perfectly consistent. The ages of our illite smectites are perfectly consistent with the ages of suggested intracaldera lava at the Timber Mountain Caldera.

So, I've kind of put together a cartoon. We've had lots of caveats at this meeting. Remember, I'm a mineralogist. So, I'm not--if you hydrologists want to attack me on this, that's okay. This is something that, essentially, what we envisioned existed 10.7 or 11 million years ago at Yucca Mountain. And, part of this information is based on the similarity between the paleogeothermal gradients that are constructed at Yucca Mountain to modern day geothermal gradients. In fact, that would be a good time to quickly show you--well, let me just go over this one first.

What we believe happened is that we had a convective zone in the collapsing caldera and we had essentially what we've termed the conductive zone and that's mainly from the geothermal literature. Because of the nature of the profile where we come down relatively straight and then we go steeply up, we go from a cold convective zone to a hotter conductive zone. And, the changes in depth or the changes in temperature as a function of depth between G-2, G-1, and G-3 are consistent with this type of outflow plume from the Timber Mountain Caldera.

The most interesting mineralogic information we got was from G-2 and it makes sense since that's reflective of the highest temperature alteration. This was another one of those, what we call, pagoda diagrams. The width of these little pagodas is related to the amount of the phases present and the scale is up here in the upper right for all of the three in your packet. In addition, I've plotted the number of illite layers or collapsed layers in the illite smectite here, going from 0 to 100. You can see why I said that that temperature was kind of schematic and it would go up more abruptly. We see no evidence of any increased temperature all the way down to about 3500 feet in G-2 and, abruptly, we get higher temperature illite smectites. You see a lot of extra data points in there. I wasn't sure if that was real. So, I sampled it very finely and got reproducible results. And, we are down to consistently having only illite at this depth.

The same types of systematics were seen in G-1 which is farther to the south, farther away from the source of heat, but it occurs at a much greater depth where we go from the random illite smectites to almost pure illite. And, finally, when we get all the way south to G-3, there's really no evidence of any high temperature activity at the bottom of the hole which wasn't quite as deep as G-1 and G-2. This scale, by the way, is expanded. So, really, nothing much is happening in G-3.

If we go quickly back to this figure for G-2, and just keep the shape of this illite smectite curve in your mind, I mentioned that I inferred some of the nature of the hydrologic system from comparisons with present day geothermal systems. And, this is something that I came up with from an article in EOS on Newberry Caldera. And, the resemblance to our illite smectite data is striking. Come down to, essentially, no increase in temperature here to, oh, 3200 feet and abruptly increase in temperature and then we take on, what hydrologists, I believe, call, a conductive character. This difference is attributed to the fact that in this upper portion we have essentially a cold, convective, somewhat of a rain curtain type system, and then here we're taking on a conductive, essentially going up the geothermal gradient. This is remarkably similar to what we see at G-2. And, essentially, we postulate that that is the sort of thing that we're seeing. We're examining, using the illite smectites, a fossil hydrothermal system that existed 10.7 million years ago. It's very likely that we had a cold convective rain curtain type of system at shallower depths at that time.

I looked up as much climatologic data as I could find on the climate about 11 million years ago at Yucca Mountain. There's not a whole lot of solid information, but the information that's there suggests that in any case it was considerably wetter than it is now. Most of the information you've heard earlier, for example, doesn't go back anywhere near that far.

DR. DOMENICO: What was the reason they gave for this very sharp increase in temperature at that 3400--

DR. BISH: Change in permeability is what gives rise to that.

DR. DOMENICO: That's what they said caused that?

DR. BISH: Right, right.

DR. DOMENICO: Okay.

DR. BISH: We don't see any evidence of a significant change in permeability today in G-2 that would have given rise to that.

DR. LANGMUIR: That illite smectite transition does not contribute to that or are you suggesting it does?

DR. BISH: I think that's an overprint thing. That's a result of this type of thing, not a cause of it.

DR. ALLEN: This could have no relationship to that current groundwater gradient?

DR. BISH: I don't think so because we see the same types of systematics going south across that gradient.

To conclude, I think it's fairly obvious that a hydrothermal system existed beneath the north end of Yucca Mountain about 10.7 to 11 million years ago and we've been able to use that as a natural analogue to what one might expect in a repository environment. Now, the caveat there is that this system was saturated and I think that it's important to point out that these reactions of the sort we see here and of the sort that Joe Smythe proposed occurred will only occur in a saturated environment, at least not a dry environment, not an environment that was dried by the repository. The alteration timing is very consistent with Timber Mountain volcanism and, just to restate what I said earlier about the paleogeothermal profiles, they are consistent with the change from a meteorically-cooled zone at shallow depths for a conductive zone.

Now, to the important conclusions and we can compare these with the conclusions that Joe Smythe obtained by essentially a totally different route. We conclude, using these little pagoda diagram things again, and I'll put it up here so you can see where this comes from. We conclude that clinoptilolite was stable only to about 100/C and we see that, as we go down through Yucca Mountain, clinoptilolite disappears at this point. We still have zeolitized rocks beneath that point. This point corresponds with about 100/C.

Going deeper, mordenite, we see that it persists to greater depths and it's very unusual at Yucca Mountain to find mordenite without clinoptilolite. So, this is an unusual little snapshot right here. But, it persists to greater depths and higher temperatures, estimated around 130/C. You can see that analcime never really completely disappears, but we get down to sporadic and trace occurrences of it about this point which I estimate the upper temperature limit to be between 175 and 200/C. That agrees, pretty well, with limited experimental data. We're up to the temperature range here where you can get things to happen in weeks to months.

DR. DOMENICO: Which well are we looking at, G-2?

DR. BISH: This one is G-2, yes. That's the one where we get the highest temperatures.

And, I put up here that cristobalite appears to have disappeared around 90 to 100/C in G-2. This is cristobalite right here and included in that, for those of you who are familiar with some of our reports, is Opal-CT which is included with the zeolitic minerals quite often at Yucca Mountain. So, we see that disappeared right around the point where clinoptilolite disappeared. It's important to note that the temperature disappearance of cristobalite must have been lower than that in G-3 and, here, we're starting to see the effects of water chemistry and kinetics. That's certainly an important factor. For the purposes of using this as a natural analogue to higher temperature repository-induced alteration, it's not germane or as germane.

DR. DOMENICO: Where does cristobalite disappear in this hole, what temperature?

DR. BISH: In G-3?

DR. DOMENICO: In G-2, the one we're looking at?

DR. BISH: Oh, in G-2, around 100/C, right here. This is cristobalite.

DR. DOMENICO: So, it's the same in G-3?

DR. BISH: Let's put up G-3.

DR. DOMENICO: Oh, I'm sorry, I'm misreading that. Go ahead? Lower in G-3?

DR. BISH: A lower temperature in G-3, right.

The important thing--it's kind of fortuitous that we came out with essentially the same temperature of maximum stability for clinoptilolite as Joe Smythe. He published 105/. The important thing to note, though, is that Joe assumed that the reaction of clinoptilolite to analcime was related to sodium ion activity. And, I think, this is an example of some of our interactions with other people on the project including people at Livermore, we've been able to demonstrate that this reaction is insensitive to the sodium ion activity, but sensitive to other things that correlate with that. It's sensitive to the activity of water, but it, primarily in these rocks, appears to be sensitive to the silica activity and that's why it appears that cristobalite and clinoptilolite both disappear at about the same depth. So, in a nutshell, the important result is that in order to get rid of clinoptilolite, you have to get rid of the phases such as Opal-CT or cristobalite before you transform to analcime.

DR. LANGMUIR: Dave, in a previous talk I've heard you give, you've shown a proposed distance out from a hypothetical repository within the system at which you'd get a 90/ to a 100/

profile which included a significant hunk of the saturated zone.

DR. BISH: Yes.

DR. LANGMUIR: Presumably then, most of the phases you're showing in G-2 would persist? They would continue to be stable under those conditions. How does what you're saying impact changes in mineralogy you might anticipate in the saturated zone or perhaps the unsat zone?

DR. BISH: We've worked together with the people at Sandia who are interested in repository design and people at Livermore to actually decide what maximum temperatures we can tolerate in these rocks. And, as a result, this has, I think, influenced some of the decisions on the maximum temperatures that can be tolerated X meters away from the repository. The difficulty in putting all that together is that, as I mentioned at the previous talk that you heard, it's still a little difficult to get accurate models for the thermal behavior of the repository over time. But, in any case, I think the most current models, around 57 kilowatts per acre areal power density, would lead to very little or essentially no alteration of the large massive deposits of clinoptilolite at depth. However, there is a small amount of clinoptilolite and smectite almost directly underlying the proposed repository horizon. This is near the base of the Topopah Springs, right above the vitrophyre. In fact, I think it's shown well on the G-1 figure. Well, not quite. There's a small amount of clinoptilolite and smectite-bearing zone right about this position (indicating). So, we're very close to the repository horizon. I think the potential exists for the temperatures to be high enough in that region to cause this reaction to occur. The question is whether the saturation conditions will be appropriate for that reaction to occur. And, it may be, in fact, that this material will dehydrate rather than react to some other phases. In other words, wherever the temperature is high enough to cause the clinoptilolite to analcime reaction, there's not enough water. So, using the current power densities, I don't think it's a concern.

DR. DOMENICO: I'm not much of a mineralogist here, but it seems that zeolites in some isomorphic series and what you find in the rocks today should be temperature dependent, correct?

DR. BISH: Yes, to a large extent.

DR. DOMENICO: To a large extent. But, do you find that --if you're really saying that there was such a difference in temperature between G-2 and G-4, those are large temperature distances over a few kilometers, mind you, which are not very common in modern environments. Do you note that you have some temperature dependence on surviving the nature of the zeolites that you find with depth? Because you told me clinoptilolite occurs in both of those at the same depth which suggests to me that you did not have such large temperature differences.

DR. BISH: The reason I said to a large extent--let's put up G-2 and G-3. G-2 is the highest temperature and G-3 is the lowest temperature. There's another factor that's important in the alteration of zeolites and it's well-documented in the geological literature primarily through studies of saline/ alkaline in lakes that are not only are the zeolite transformations subject to change through temperature, but they also change as a function of water chemistry.

In fact, there are a number of cases--and I have a viewgraph that I won't show unless you want me to, you don't have it in your packet--but, essentially, you find that in saline lakes, concentric zones where you go towards the center of the saline lake, you get to the quote/unquote, "higher temperature zeolites". And, I think what we're seeing--this is G-3. What we're seeing here is that these transformations to analcime here at this depth aren't reflective of temperatures on the order of 100/ or greater than 100/ that are reflective of the appropriate water compositions or a combination of the water compositions required and the temperatures required.

DR. LANGMUIR: Did your fluid inclusion data help you out with that one or do you--

DR. BISH: Well, it's difficult to--we have fluid inclusion data from G-3 primarily up in the shallower range from calcites. The interesting thing about all the fluid inclusions is that they're very low salinity. So, I don't think they're reflective of what was going on at the time of zeolitization which, from the effect of the temperature on the zeolitization in G-2, was obviously before 11 million years ago. So, in the first one or two million years of the rock's existence, they were zeolitized. And then, this temperature, this geothermal event, acted on that distribution. I'm not sure I got to your question.

DR. DOMENICO: The other point is you have done some work with illite smectites, the same way we would treat the production of a hydrocarbon from a--and we use the same equation as the space equations with usual activation energies, et cetera, and I could never, ever get a profile like you have there where you have a constant relationship between the illite and smectite over that depth. As a matter of fact, in the Gulf Coast, that reaction starts at about 65/ and it starts at 85 feet and goes on down. But, I could never produce one like that and I certainly never could produce one like you have over there where you have the very sharp change. I just couldn't--we couldn't do that mathematically. That's what you observe, but we could never, ever reproduce this by conventional means.

DR. LANGMUIR: You've got a lot sharper thermal gradient here for some reason than any Gulf Coast sediment--

DR. BISH: That's right.

DR. DOMENICO: Well, he's got two geothermal gradients, one is small, and then he's got that huge jump over a few meters which says all the action is taking place over this zone and, all of a sudden, it's holding itself and let's go over that zone. That's, you know--that's difficult.

DR. BISH: It is.

DR. DOMENICO: Maybe, there's another explanation for that.

DR. BISH: Well, I was totally baffled by this abrupt jump until I came up with the Newberry Caldera paper and, in fact, a number of other papers on modern day geothermal systems that show geothermal gradients that that looked just like that. We could change that to temperature. So, I think it's reflective of a past geothermal system in a wetter climate that gave rise to something just like what we see today in places like Newberry Caldera.

DR. DOMENICO: But, your illite smectite relationship is changing from a constant of 20 to something on the order of 80. Am I reading that correctly?

DR. BISH: In this case, it goes up to--

DR. DOMENICO: --over a few meters? Over a few meters?

In other words, all the action is taking place right there?

DR. BISH: That's right.

DR. DOMENICO: That's quick.

DR. BISH: This starts about a little under 3500 feet and that's 4,000--it's about 500 feet--

DR. LANGMUIR: But, isn't that consistent with the hot source of rock or fluid and those kinds of things? You've got sharp gradients, don't you, in--

DR. CANTLON: Based on your studies, what would you conjecture would be the impact of repository temperatures on the mobility of radioactive material?

DR. BISH: Impact of repository temperatures on the mobility? It's--

DR. NORTH: Yeah, I think we should note that we have a whole series of meetings coming up in October on the thermal loading issue where we will presumably revisit this issue and a number of related issues in much, much more detail and I think a few of us are beginning to get anxious for lunch. I would suggest that we postpone additional discussion until after lunch where Professor Rod Ewing will be making his presentation, and then we'll go to the general round table.

Are there any summary remarks that you want to make, Russ Dyer, or--

DR. BISH: I can answer his question real quickly, though. From what we've been able to find, it doesn't look like, for the most part, the repository will cause significant mineral alteration. So, it won't change. That's it.

DR. DYER: Dr. North, I think I could make my comments just at well at the beginning of the round table and it might form a good lead-in for round table.

DR. NORTH: Fine. Why don't we do them at the beginning of the round table.

Why don't we adjourn for lunch at this time and try to be back here within one hour, meaning by 1:45.

(Whereupon, a luncheon recess was taken.)

A F T E R N O O N   S E S S I O N

DR. NORTH: We have an additional speaker, Dr. Rod Ewing from the University of New Mexico, for the beginning of our afternoon program, which will be followed then by the round table discussion.

I would like to welcome Dr. Ewing. He has been involved in research related radioactive waste disposal for about 14 years, working in collaboration with scientists at all five of the national labs working in this area, plus scientists in Germany, France and Sweden. He has been involved in a wide range of professional society activities and his co-editor and contributing author to the recently published volume, Radioactive Waste Forms for the Future. We welcome Dr. Ewing.

DR. EWING: The last presentation and I'll try to say at least a few things that will provoke discussion in the round table.

For my presentation, I want to briefly touch on topics that have concerned me for the last ten years. Topics related to making long-term predictions, particularly related to long-term predictions of the durability of nuclear waste forms.

Most of this work was done in collaboration with colleagues at the Hahn Meitner Institute in Berlin. And, I am very pleased and proud to say for the past five years, the work has been supported by SKB. A number of previous speakers have spoken highly of SKB support. I would like to echo their praise, and suggest that in the round table we should examine some of the aspects of their procedures for funding that might benefit the U.S. program.

I should also say that I'll draw heavily on the work of two Ph.D. students, Mike Jercinovic and Bob Finch. Mike worked on corrosion of borosilicate glass/basalt glasses. Bob Finch now works on corrosion of uraninite as an analogue for spent fuel.

In my presentation I want to make a few obligatory comments about the philosophy behind the use of natural analogues. This will repeat some of the points of previous speakers, but I would like to add to what they said. Then I'll discuss basalt glasses and analogue for borosilicate glass and try to give you examples of where specific benefits have resulted from such work. And if I speak quickly enough, I want to talk about our present program, that is, looking at natural uraninites in corrosion products.

In the simplest statement of the problem, we simply want to predict and verify the long-term performance of the waste form. The key words are predict, verify and long-term. As we expand our efforts in examining that sentence we come to the common vocabulary of performance assessment, validation and natural analogues. And the key question is how do natural analogue studies tie into performance assessment and into validation. At the end I want to try to address that issue, but I should give you my conclusion, and that is from what I know and what I've heard presented here today, there is a vast gap between natural analogue studies and their application to performance assessment as it is defined by U.S. laws and our U.S. understanding of what that means.

When we use the phrase natural analogue, I think it is appropriate to pause for a moment and think about exactly what is meant by analogue. This is a definition of "analogy" from the dictionary, the second definition I think that Dr. Winograd referred to. Keep in mind we are simply taking two systems; two configurations. We are examining attributes or parameters or characteristics of these two systems. And, to the extent there are some similarities, we make inferences.

Now the inferences may be in exactly the area in which the systems or configurations are different. The strength of the analogy depends on how closely the two systems mimic one another. And, it also depends on whether we are shrewd in picking the parameters that we want to use as a basis for our analogy.

I started to pull this slide out last night, because it repeats what others have said, but I think it is worth leaving in because, if we think about the vocabulary of the last few days, people refer to things such as a perfect analogy.

Well, the only perfect analogy of a system is the system itself, in which case we don't use analogy. Others have referred to imperfect analogy. The reason we use analogy is because we have two systems that don't perfectly coincide. So I think our thinking could be a little cleaner in this regard.

I spent sometime years ago, looking into exactly what was meant by this process of analogy. And I would suggest and recommend reading John Stuart Mill. The discussion is very precise and it is also very eloquent. And I've extracted this quote. I think the important thing is that analogy isn't the best way to go about things. Sometimes it is all we have. I does provide a guidepost. And, in place of guideposts you might read other phrases such as a warning about something you've forgotten. It also provides inspiration and ideas. And that inspiration and those ideas are very necessary to designing new systems and solving problems in creative ways.

I would emphasize that this definition of analogy or this discussion of analogy doesn't require that you be focused in the beginning. It doesn't lend itself to normal programmatic organization. It emphasizes the fact that we look at natural systems, the fact that we use this process of analogy is just part of the normal intellectual process of attacking any problem. It is not a surprise. We haven't invented anything that is new. And in that regard I am disturbed that we speak of natural analogue programs as if they are added on as an increment to the effort to solve the problem. I wince a little bit at the idea of a strategic plan for natural analogue studies, because, by definition you are looking at creative solutions to very complex problems.

Now the other part of this phrase, meaning natural systems, the reason of course that we look at natural systems is because they represent large-scale phenomena comparable to what we have with the repository. They are complex so we can look for synergistic effects. But most importantly, the represent long time periods. And this is really the crux of the problem making long-term predictions.

This is a slide taken from a paper by Neal Chapman and Bernard Comes. And I like it very much because they have plotted geologic time scale with some of the important events in one column and then a typical dose demand curve that resulted from a safety analysis of a performance assessment. I like this because it emphasizes that first we are dealing with long periods of time, but it also emphasizes in a certain way what we do is ridiculous in terms of the geologic history that has gone before.

I'll show you a basalt glass with an age of about three million years, and that really pre-dates the evolution of modern man. So we have to, I think, keep our job in perspective keep the time scales in perspective.

Also, it is important to note that there are no data that go with the dose demand curve. There can't be any data. And this emphasizes the fact that a performance assessment is a non-testable hypothesis. We just have to admit that. And so we can't, by doing a lot of natural analogue studies, deceive ourselves that we've changed anything about our ability to predict the future. That is just what we are stuck with.

The natural analogue studies represent efforts on this column. I think Mike Shea made a very good point yesterday, that one benefit of natural analogue studies is that it is good practice for performance assessment; it is good exercise; but, it is just exercise. We still won't know who won the race until we actually run that race. And in this game, we won't have the answers. So, we have to phrase our questions very carefully.

Now on the left side of the column, just looking at the geologic time scale, others have listed some of the limitations and let me emphasize this as well, the geologic record is incomplete; age determination errors are large; critical parameters such as surface area, flow rates, initial conditions are seldom known; configurations are not analogous in detail; and, actinides are rare in the natural environment. So we begin with those limitations. And I think with natural analogue studies it is always important to list those first, and then try to phrase questions or come up with questions that we can in fact answer.

Despite all these difficulties, I would say that natural analogues--I wouldn't say they provide the only means now, this is an old slide, but they provide us a means by which extrapolated, long-term behavior can be "confirmed". But I've put confirmed in quotes, because I don't mean that we've actually proven anything, but rather we have proven something about the approach that we've taken. And I'll comment on how I view the word "confirmation" in a moment.

Related to borosilicate glass, I want to use it as an example because for a number of years we worked on borosilicate glass/basalt glass corrosion. What are the reasonable questions that one might ask of such a system? We can ask what are the alteration products? What is the long-term corrosion rate? Do the mechanisms change with time? We can ask what the corrosion rate is. We can ask what the effective environmental factors might be. And very importantly, we can take the short-term experiments that are extrapolated over long time periods and see if they are at least consistent with what we observe in natural environments. That is a relatively modest set of questions, but it is time consuming to address them.

Let me give you some examples of how we might proceed. First, for natural glasses, keep in mind that we have rhyolitic glasses, we have tektites with a higher silica content. I am talking about basalt glasses with SiO_2 contents that may be 50 weight percent.

Bill showed you a longer table comparing composition of the typical borosilicate glass to basalt glass. Let me emphasize that really the compositions aren't very similar. They are similar only as far as the total silica or perhaps the total silica and aluminum contents go. But keep in mind that the borosilicate glass has vision products; it has actinides; it has reprocessing components such as zinc; you have processing components such as boron. If you add up all of the things that are different, they can comprise as much as 40 or 50 weight percent of the total. So, they really not so similar. And to the degree that the behavior is similar it has to be based on total silica and aluminum content or perhaps the total alkali content.

Fortunately, that seems to be a controlling parameter in terms of corrosion. This is a diagram from the work of Carol Jansen and John Plodnick, in which they plotted for 28 day leeching experiments, silica release versus a variable, I don't care for very much, but it works out pretty well for this illustration, free energy of hydration of the component oxides that represent the glass composition. And we see that we have a roughly linear relationship. And I simply want to point out that basalt glass and waste glass is followed about the same position in this range of behavior in terms of silica release. So this is perhaps an indication that they will behave in an analogous way.

Another approach we've taken and this goes back to work we've published in 1985, is we simply took a basalt glass and borosilicate glass and at the Hahn-Meitner Institute with Wanner Lutze and colleagues, we corroded it in a saturated brine at 200 degrees for 20 or 30 days. The SEM micrographs on the left are the basalt glass; the micrographs on the right are the borosilicate glass.

So the corrosion products, the formation of the layer, the morphology of the layer are strikingly similar. At a higher magnification, we see that the similarity prevails. So, it looks like they corrode in the same way. And then finally, if we look at the micrographs for "g" and "h", these are the corrosion layers that form on natural glasses corroded by ocean water.

So, here we have a sample that is about a million years old corroded in a much less concentrated brine at approximately 3/C, but at least we still get a surface layer reminiscent of the experimental results.

Based on these types of observations, one can proceed with the hope that the behavior will be analogous and so let's look for a moment at how some of the environments in which basaltic glasses corrode. The corrosion product, that layer is called a palagonite which is just a pseudomorphic replacement product, this gel layer on the basaltic glass, usually enriched in iron and aluminum.

The environments that we looked at were sub-aqueous eruptions; sea floor; and then also eruptions under glacial sheets and finally glacial lakes. And then you have a characteristic geologic geometry where at the lowest level we have pillow basalts and the rims are quenched glass. Then as this pile builds closer to the surface, we have explosive eruptions, so we have fragments of glass call hyaloclastite.

And then finally when the eruption becomes sub-areal, we have these topset flows that form. So, we can recognize this sequence all over Iceland, for example, and this is where we went to collect samples.

This is a typical pillow broken into cross-sections and the chilled margins of the pillow are the glass that we looked at for the purpose of studying corrosion glasses in a natural environment.

We went all over Iceland collecting samples of different ages; different compositions. Some were altered by sea water and some by fresh water and so on, because, we tried to build up a research collection that would allow us to investigate environmental effects on the corrosion of basaltic glasses.

The results of this study are summarized in a JSS Report that I'll leave with the panel. And that reminds me I should say, you can tell because I don't have view graphs, I have slides, I don't have copies of my slides I just have reprints, that this is not a DOE project.

Now we did a lot of work. I think we accomplished quite a lot. I'll give you three examples of where we were able to address some of the questions that I've raised before.

The question of long-term alteration products; the question of what is the corrosion rate over long periods of time; and, can we use any of our observations to verify corrosion models?

Well, this is the palagonite layer again seen in a scanning electron microscope. The can be a few tens of microns thick to hundreds of microns thick. And Bill showed you a schematic cross section of these layers. This is the gel layer that we would call palagonite.

One contribution we made I think in looking at these layers very carefully, in this case using transmission electron microscopy, because these are supposed to be gel layers. They are not supposed to be crystalline. This is representative micrograph. These circles are the wholly carbon substrate that holds up the sample. It is very difficult to prepare these samples. We have to cut them with diamond knives. There is a high mortality rate. And then it really takes days to get a single good micrograph. But the point is that they are crystalline.

These splays or netting of dark material represent the formation of clays in a sample. This very delicate texture is preserved in a sample that is over a million years old. There are channels passing through the layer. One question is, is the layer that forms in the borosilicate glass protective? Certainly, it is not. We have evidence for fluid moving through the layers because we can analyze the composition of the clays and we have a very heterogeneous array of clay compositions.

In terms of modeling, I would say the modelers have to address this issue. This is not a gel layer. They have crystalline phases. They have complex compositions. A model that describes the corrosion process has to describe the phenomena of this scale.

Later in the paragenesis of these, we have the formation of zeolites. This slide has already been shown, but we have the formation of things such as phillipsite and then chabazite on top of it. The details of individual samples aren't important. But, for every geologic environment in our Icelandic samples, we also looked at Hawaii, Canary Islands, and Western U.S. samples, we can establish a paragenesis. And the models used in describing the corrosion of borosilicate glass should also be able to describe what we see in natural systems. Particularly, in natural systems where we've gone to a lot of trouble to constrain the environmental conditions.

Well, in terms of crystalline phases, identified and associated with the alteration products of the basaltic glasses, we identified maybe 75 different phases. The phase assemblages are to a large extent metastable. We have to think about that. And there are a lot of generalizations we can make about the formation of the paragenesis of the phases. As an example, if we form zeolites in a close system, we release aluminum from the palagonite. Things like this have to be explained by the model and in fact I can say with the help of Bernd Grambow who is a modeler, we went a long ways toward understanding the paragenesis of the corrosion products of basalt glass with the same approach used to describe what happens to borosilicate glass.

Now on the issue of corrosion rate, the simple minded approach was to just measure the palagonite rind thickness for dated samples and see what the rate must be. When you do that, as we did blindly, you don't get much of a pattern. But, if you look at the samples, sample by sample, as an example, this sample 4170, after we looked at it and looked at the descriptions, these are dredge samples, deep sea drilling program samples and so on, it was clear that it had been sealed. So it was cut off from water and so the age of the sample doesn't correspond to the time it was in contact with the solutions.

We could organize, this is rationalization after the fact, organize the corrosion rates into a category of forward rate of corrosion and a final rate of corrosion. The forward rate of corrosion associated with samples where the solution was not saturated in silica and the final rate associated with silica saturated solutions.

This is consistent with the model I think that is commonly used, I think in this community, where if we plotted silica concentrations as a function of time, we have first the forward rate and then a long-term final rate of corrosion.

And so we can organize the data we see for natural samples into these two categories and go back and look at the geologic environments for those particular samples and the samples that are corroding at the final rate are buried below sequences of sediment. The core water is saturated in silica. The samples corroding at the forward rate are dredge samples exposed to open ocean floor conditions. So, this is a type of model verification.

In terms of palagonitization rates and relating it to corrosion rates, it is not very satisfactory. We have a lot of difficulties if you dredge something up off the ocean floor its age is questionable. We never know for sure what the contact time is of the solution with the samples, because when the zeolites are precipitated, the cement cuts off the water. So, what we get is a minimum rate, the least interesting number. You are interested in the maximum rate, but that is just the way it goes.

At least we can say the rates we get, the two categories of rates, are consistent with the models we are using for corrosion and borosilicate glass. So, in this case I would say we had limited success.

Model verification. I think the point I would like to make is that model verification doesn't mean getting everything right. It probably just means finding the right sample and working very hard on it. This is a favorite slide of mine. The horizontal scale is 5 millimeters. This is basalt glass, 3 million years old from Tungufell, Iceland. And it is really a remarkable sample. The spheres are the vesicles left by escaping gasses when the basalt glass solidified. And if you look at it, you are struck, I hope by the fact that some of the vesicles are filled by this dark material and some by a lighter material.

The dark material are clays; the lighter material are zeolites. Also, if you look more closely at the sample you see that the rind thicknesses are different from vesicle to vesicle. And if we take rind thickness is an indication of reaction progress, then we have to explain how in over just 5 millimeters of sample in the single thin section, can we have a situation where we precipitate clays in some vesicles and we precipitate zeolites in other vesicles. How do we explain the wide variations in the apparent degree of reaction progress as is measured by the thickness of the palagonite layer. This is the palagonite here (indicating).

The way I think a natural analogue study works best and people have alluded to this, is if you are in close contact with the people doing the modeling or performance assessment, as soon as we saw this, you know I was anxious to set down with the modeler Bernd Grambow and say, now I have you. Tell me what your model can do with this. And he of course had more questions. And in this single view we probably made I would say 200 to 300 electron microprobe analyses.

We measured the vesicles and estimated volumes. We tried to do a complete mass balance on exactly what is going on in terms of the reaction. Some of the observations we can make is if we divide the vesicles into those that are filled with clay and those that are filled with zeolite. Those that are filled with clay, the rind thickness doesn't vary in any meaningful way with the vesicle size. Those that are filled with zeolite though show some rough correlation with vesicle size. The larger the vesicles, the thicker the rind thickness, indicating a greater reaction progress.

Also, with our estimates of vesicle size and volumes of material, measuring the composition of palagonite in all of the phases, we came up with our own diagram, which measures loss or gain of material on an element-by-element basis. The only important thing in this diagram is that zero in this delta percent, the vertical scale means there has been no loss or gain of material. Everything taken from the glass shows up in the products we have in the vesicles. If you are above zero, there has been some net gain of material in the system. If you are below zero in the system, there has been some net loss.

The error bars are large. The error bars include one sigma standard deviation. That includes all of the analytical error. The error associated with estimating sizes and volumes and they add up pretty quickly. But, the point is in the clay-filled vesicles, there has been on the average the addition of material. For the zeolite filled vesicles they are closer to zero, so it looks like a closed system. And in fact, what we have as we go back is a situation in which we can examine the effect of flow rate on reaction progress.

For those vesicles in general connected by a system of fractures, fractures often with palagonite forming along the fractures, we have a net addition of material. The flow rate is high. The reaction progress isn't as great. For those vesicles that aren't connected by fractures along which fluids can flow, and you can see these fractures, but no palagonite along them, the corrosion proceeds, reaction progress is greater. Zeolites form and pull aluminum out of the palagonite. And because reaction progress is greater, the palagonite rind is thicker. Now, usually, the audience cheers at this point.

This is to me an exciting verification of the model we are using. It indicates something about our understanding of the effect of flow rate on solution compositions and what is that going to do to the phases that form? And it is these small examples, these specific examples, the examples where we have some control over variables that allow us to exercise our models in a constructive and useful way.

And this just summarizes what I said. Mass loss is high where flow rate is high; mass loss is low where flow is restrictive. That is not as important as the observation about which phase is formed at different stages of reaction progress.

Well, for basalt glasses and waste forms in general, we can come up with this tripartite diagram. Others have had a similar arrangement. But I think it emphasizes where natural analogues can fit into the scheme of things. For the basalt glasses we have the empirical data; the reaction products; long-term results; large-scale systems. That can be immediately compared to the experimental data, short-term results, laboratory scale systems.

The extrapolated results of laboratory data can be verified by comparison to what we found in nature. To really do this though, you have to take a digression. You have to do a lot of experimental work on basalt glass. And people who fund you are surprised that you have an experimental program on basalt glass when after all you are trying to dispose of borosilicate glass. That is part of the process and I think people have to be prepared for those types of excursions.

But it is an iterative process. It is a process that takes some time. Thinking with all due respect to the problems of managers, it is very difficult to provide a focused program. What you need is steady funding on a small scale for a long time to work in the general area of corrosion glasses, whether it be basalt or borosilicate. And I think the real proof of your approach is not in any selected demonstration of the efficacy of your model using a natural analogue or experimental results, but rather is in the general applicability of what you are doing.

If your model for the last corrosion works for basalt glasses, works for borosilicate glasses, works for tektites, then I think you are justified in using it for long-term extrapolations. You've done everything that you possibly can. That is all for basalt glasses.

Our emphasis in the last year, our research program in this area has changed completely. SKB I think was satisfied with our results. There are a lot of results here and I commend this report to you. So we ask the same question for spent fuel. What are the natural analogues for spent fuel? And as it turns out we are probably better off when it comes to spent fuel studies. Natural uraninite, UO_2 + X, alters to a wide array of secondary phases. And in many ways we have a better chance at describing the corrosion of spent fuel over long periods of time using what we can see in natural deposits and so on.

Our effort concentrates on a deposit in Africa, Shinkolobwe in Katanga in Zaire. It is a classic deposit some 2 billion years old. And you can see the small grain of uraninite altered extensively. And the slight changes in color represents a vast and complex array of phases, a very complex array of phases. A real challenge for the modelers.

If you look at the literature on experimental studies of corrosion of spent fuel, this is the list of all of the solid phases that have been identified for sure and tentatively. If I go to my locality in Shinkolobwe, I have over 50 uranyl phases as alteration products. If I go to my mineralogy handbook, I have over 150 possible alteration products. This is the guidepost. This is the warning. Here is a case in nature we see a very complex result.

Now to be fair, I have to say I picked a deposit where I would get alterations. So this is a high rain fall at the surface for 50 million years. The alteration layers extend in the deposit maybe 80 meters. I wanted a lot of material to look at. This is the right place to start, because if you have oxidizing conditions, if you expect the alteration of uraninite, then we are going to have to work out the crystal chemistry of the uranyl oxide hydrates and the uranyl silicates, phosphates and so on.

The other point in making this comparison, or one conclusion that has fallen out of our studies, is that on one hand, we have a good natural analogue, but we have to be careful. The natural uraninite is high in radiogenic lead. And as it happens in these uranyl silicates and uranyl oxide hydrates, the role of lead as far as the crystal chemistry of these compounds goes is very important. So we can get a different paragenesis depending on the presence or absence of lead for the phases. It is difficult to identify these things. These are experimental results. But you just can't take the alteration products we see in nature and expect those to show up in long-term corrosion and spent fuel. So a lot of analysis I think is required.

In collaboration with CEA, with support from SKB, we are also looking at Oklo. This is a classic natural analogue area. But I think this is an example where we can pay a lot more attention to the solid phases. This is back scattered electron image of the uraninite, the light color here, replaced by illite in the center in a matrix of apatite. I would simply point out that the grains are fractured, corroded and very heterogeneous. Here is a lead-rich core and then the lighter areas are lead deficient. Dave Curtis mentioned yesterday, this is a good analogue for corrosion of spent fuel. But it is also complicated. It takes time to do these types of studies.

Well, that is just a taste of what I think can come from looking at natural analogues on a small scale.

DR. CURTIS: Is the lead radiogenetic? Is it growing in place?

DR. EWING: Yes, I think so. And this is a later alteration event, but the alteration and dispersion and radionuclides we see at Oklo might not be related to conditions at the time the reactor was operating. That would be an important conclusion. This is really hot off the press. We still think about this quite a lot and we want to look at a lot more samples.

I'll stop there. In the round table discussion issues can come up. But, I do want to emphasize from my experience over the years that natural analogue studies won't be neat. They shouldn't be neat. They should be creative. You should have people out there looking for solutions; looking for phenomena; trying to understand things.

Don't expect too much focus, and probably the greater the focus, then the greater the disappointment in one or two years, because it just doesn't work that way.

Thank you.

DR. ALLEN: What I would like to do first, is Russ Dyer wanted to make a few sort of final comments regarding his DOE presentation this morning.

So, Russ, why don't you do that right now.

DR. DYER: I'd like to leave the Board with two messages here. I hope that from the sample of the studies that we've presented that we have demonstrated that analogues are an integral and viable part of the Department of Energy's program. In fact to paraphrase a comment that Dr. Domenico made at lunch, analogues permeate our program. But, they are not a panacea. They are part of a total program.

Another thing that should be obvious is that not everything that can be done is in our current or projected funding plans. On a related topic, there are a lot of other things going on, projects, programs, by other entities that we can gain information from. We can piggyback on the Radionuclide Migration Program at the NTS. We can piggyback on some of the ES&H programs from the Department of Energy. There are many, many other programs going on.

The last topic I would like to bring up or throw on the table is that this is a time for hard management decisions; something that came up a little bit earlier. Management is not very critical in an environment of abundant resources and funding. I don't think we've ever been in the situation where we could throw dollar bills at a flat tire. But our resources are limited and we need to determine what activities directly support our highest priority objectives and ensure that those parts of the program are funded.

That is what I would like to leave you with at this point.

DR. ALLEN: Okay, thank you, Russ.

May I ask in the session that follows that each of you, even the Board Members identify yourself so that the people doing the recording will know who is speaking.

ROUND TABLE DISCUSSION

DR. ALLEN: I have asked the various members of the Board to try and think up some provocative questions. We will get to those in a moment.

Let me ask first if any of the people who have spoken over the last couple of days would like to say something in commenting on what other speakers were saying. Or do you have any particular remarks that bear on other things that happened during the last couple of days?

Yes, Larry.

DR. RAMSPOTT: In noticing most of the work on the natural analogues, it seems to have been focused on cases where nothing ever got out or where we don't expect things to get out. And yet, in order to actually exercise the codes and to exercise predictive capability and also to have believability from the public, I think what we need to do is to have some things where stuff does migrate.

And example I would have is there are areas where there is very high uranium and high radioactive in natural waters, either springs or streams and so forth. We should be assured we can model that as well as those cases where for example there appear to be uranium ore bodies where absolutely nothing gets out and it is totally shielded like Cigar Lake. We need to go to the other end of the extreme also.

That was just a thought that I had.

DR. ALLEN: Further comments?

DR. SHEA: Michael Shea. One of the things that strikes me is that particularly based upon our experience from the Pocos project, is that we had a blend of not only national lab type capabilities from the U.K., Brazil and the U.S., but we also had significant, at least half of our people were university people. I don't know if it was only obvious to me but there was a lack of university involvement in the national analogue programs that were presented here.

That is not a slam against national labs; the U.S. national labs. I think it is just important that it includes universities. There is a significant geoscience capability in the universities. There is a high cost benefit. They are outside the program and maybe they can be a little more objective or creative like Rod is.

I think natural analogues are a natural fit for universities to be involved with our national program. So, again that is nothing against the national labs. What they had here was good applied science. I would just like to see the involvement of universities.

DR. ALLEN: Thank you. In our course of times of decreasing funding it makes it even harder to look outside for it.

DR. SHEA: I understand. Really, I understand.

DR. ALLEN: Other comments from speakers?

Okay. Well, let me do this. I asked each of our Board Members to write down a couple of provocative questions which they have done. I think for me to read these even if I could would be sort of absurd and a little bit too timely. So what I am going to do since you've all thought about these things now and all have some questions, I think I'll just turn to various people here, the members of the Board, and ask them to phrase their questions in their own way.

Don, since you are the senior person here, why don't you start off.

DR. DEERE: I think the meeting has been very helpful to focus us on the very broad range of studies that are going on. This has been extremely interesting and I think valuable to us.

I have a little of the fear and I think the last speaker with the statement, the greater the focus, the greater the disappointment. A lot can be learned, I believe that will help in the models and one will get a much better understanding. And I am wondering if it isn't the understanding that is going to be the most important thing to come out of this than the ability to actually get a numerical value for a particular equation. I think that is where the disappointment would be if we use an analogue and then in all of the studies able to come out with a value that we have confidence in because of the unknown relic information that control the formation that we don't really know too much about or have much of a hope of getting it.

But yet, if one has a model that is making predictions, at least you get the feeling that the model is looking at things moving in the right direction. And it broadens our understanding, and I think that in itself has a lot of value.

I believe we got this coming from several of the speakers. And this was certainly an impression that I got and it is a new impression because I haven't been very much concerned about this problem before.

Any comments from some of you on that?

MR. JOHNSON: I think that is in line with my point that perhaps there will be more value out of our lessons learned than there will be out of our specific application to well-focused analogues.

DR. ALLEN: Are you saying, Don, that you doubt that the analogues will be important in the licensing process?

DR. DEERE: I really wonder if they will be as important as our speaker yesterday, Mr. Eisenberg things that they have to be to be of value in the licensing process.

Am I misinterpreting what you said, Norm?

DR. EISENBERG: No, I don't think you are.

DR. DEERE: Yes.

Michael?

DR. SHEA: Yeah. I am thinking of the European experience as you are saying this. I am thinking of the project Gewahr, KBS-3, and Pagis, where they used natural analogues in those safety assessment evaluations. Those were real safety assessment evaluations for the Swedes, which was SKB-3, and for the Swiss for Gewahr and Pagis was CEC. And there is even reports that say that natural analogues were important in those safety assessment evaluations.

I don't know that it is going to be that different for us in the U.S. So I think that natural analogues will be important in the safety assessment process.

DR. DEERE: I presume more so in certain of the studies than in others.

DR. SHEA: Sure. That is just the nature of the way it is going to be.

DR. ALLEN: Larry.

DR. RAMSPOTT: Larry Ramspott. I think I can give you an example of the way we used the natural analogue. This is one of our anthropogenic one. When we published the paper in Science which I think was originally written in 1980, was published in '82, when we observed the ruthenium 106 migrating over to the well in cambrian, that was in contradistinction. I didn't spend a lot of time on that in the talk. But the current wisdom at that time was that ruthenium should not have migrated because it have a very high Kd and therefore should have sorbed on the rock.

When we published that paper at the time, we didn't really know why it migrated. And we put some speculations in the paper, one of which that it might have been migrating as an oxyanion, but we really didn't understand. With the funding that we had on the RNM program, we had a comprehensive review of the ruthenium literature done and we published that. That was a paper by Rard in the handouts that I gave you.

We put that ruthenium database in the code. We then went ahead and calculated and published a paper by Isherwood in which he calculated and showed that the ruthenium should have migrated. And so we were able to make an observation. It was an observation that was not in accord with the conventional wisdom at the time. We were then able to get more data and then go ahead and calculate that that actually should have happened.

The other interesting thing, however, is that we should have observed technetium 99 migrating if you do the same calculations. We haven't observed that yet. But it is that kind of sequence of things. It isn't any specific number as you point out, but it is the understanding, the flow of understanding.

DR. ALLEN: John Cantlon had a question.

DR. CANTLON: This comes from the use of the analogue studies in licensure decisions and the need to pay some attention to the QA that NRC and the general public are going to demand for the data. Particularly as budge constraints make it more and more unlikely that these analogue studies are gong to be generated by DOE explicitly for the decision. I don't think we have addressed the formality of how we are going to QA those kinds of studies and I think needs attention.

Does anybody disagree with that?

DR. BIRCHARD: I wouldn't disagree directly. I am George Birchard. I'm with the U.S. NRC. Those of us at the NRC who are involved in research as compared to those who are in licensing have developed a different perspective, I believe. We recognize that the research area has its own needs in which one cannot predefine results.

There are engineering areas where I think things are well understood and you can predefine results. And I think that is where the reactor style engineering approach is appropriate. So I think in areas such as this, it is much more important as Professor Ewing suggested to have good creative people with a broad perspective trying to develop an understanding in a mechanistic way than it is to worry about a preordained approach to going about doing a problem.

DR. ALLEN: Rod, you made the statement a little while ago--you inferred that the Swedes were much better at supporting research than we were. Could you expand on that a bit?

DR. EWING: Happily.

I think this is an important point. The natural analogue studies shouldn't eat up everyone's budget. They can't be a large component of the effort. And I think those like myself who have a soft spot in their heart for such studies are willing to acknowledge that.

But they do need to be done. And I think there are a lot of fundamental data to be derived from them. Just the crystal chemistry of uranium 6 compounds. You'll need that for your modeling. I can assure you of that. And I can handle those samples. You can't handle material in a hot cell, so there is a real benefit.

Acknowledging the budget should be small, but what they do need though is that funding has to continue for some period of time. It can't be turned off and on. That is what kills the project. A successful natural analogue project means selecting a field area, getting a group of people together who can work together. And that means someone from performance assessment; someone from the regulatory agency; a physical chemist; a geologist; sorting through the vocabularies; getting ready to go. That takes time.

You can't assemble these teams. They can't be effective on a quarter-by-quarter basis. They can't summarize their results in quarterly reports or even annual reports.

They need some small amount of consistent funding. They need to be mixed with one another. They need to be in contact with one another. And I think these are in my experience the characteristics of the Swedish program.

The amount of dollars is not large. It doesn't make me famous at the University. But it comes year after year. The funding is flexible. I can change my ideas completely. I can be wrong. I can try to find solutions. It doesn't show up in the quarterly report as failing to reach a milestone. That is just one of my bad ideas and we'll put it to the side.

It needs to involve human contact. You need to be able to travel. You need that type of flexibility. The Swedish program really is all of those things. It's remarkable.

DR. ALLEN: If that's a challenge for the DOE, would anyone from the DOE like to respond?

DR. DYER: I guess that is me.

I would love to have a relatively level budget myself. If we had some stability in our budget process, we might be able to pass it onto our investigators. But if we look at 40% cuts one year and a 300% rise the following year, there is a certain instability there.

DR. WINOGRAD: With regard to the QA issue and how one melds us into research, there is an intermediate path that one can pursue and that is for projects such as some of the things we've heard about in the last two days that we acknowledge if we already have the research element to them, that QA is not required at the outset. There is a famous British biologist whose name escapes me, but know the laureate too is quoted as saying, "Most research leads nowhere, and the research that does somewhere is not in the direction that he expected." So the intermediate route is that you fund the research, DOE and NRC. If it turns out that the research is of value to your program and only a small percentage will be, then redo it perhaps another organization replicate it under QA procedures. But, not at the outset ordained that everything will be QA.

DR. ALLEN: You are in print according to that though.

Dave Curtis.

DR. CURTIS: I would like to build a little bit on Rod's comments about the SKB since I've had some experience too. I don't know what the formality of their licensing procedure is, but they expressed to me once and idea that they are not looking for the nail in the coffin with respect to licensing. What they are looking for is building a preponderance of evidence which was a notion I found very satisfying and one that I tried to convey in my discussion about the alpha-radiolysis.

No one of those studies suggests that alpha-radiolysis is something that you should be concerned about. It was the preponderance in the variety of the discussions which I think were important. And I think that is why they have a bent for supporting research is for building the evidence.

Another comment I would like to make about the QA. I sat her and listened--the QA thing which I'm sure you've heard people just gripe about this for years. But, one of the things which concerns me about the Quality Assurance program as it is now implemented is it tends to exclude things. If you have a natural analogue study, which in fact maybe very important. It may be too difficult to include in the rigid requirements of the Quality Assurance program, therefore it may not be done. It may be excluded. Or Rod's comments about how do you include publications in a QA program. If you don't do that you are neglecting all the accumulated knowledge up to the present.

The QA program, it seems like it has got some real problems in terms of excluding some important aspects of this whole thing.

DR. ALLEN: It's the first time I've ever heard that.

DR. DYER: Let me elaborate on that. This is Russ Dyer from DOE. Let me address Dave's comment.

Our QA program has a flexibility that allows us to bring in data ideas from outside the program, for instance out of the published literature. Under the procedure we have, we could take the data information models in a published report that was not accomplished by somebody in the program; bring it in and qualify it for licensing. And there are several ways in which you could qualify it. One of which is to be in a peer review that would essentially set and make a judgment of the applicability, the goodness of this particular body of information. And once it is given a pedigree by the peer review board that it is appropriate for the purpose intended, then we could take that forward and use it for the licensing process. That is under the formalism of our current QA program.

DR. ALLEN: Okay. Thanks.

Julie, did you have something?

DR. CANEPA: Yes. I just wanted to make a couple of comments. I think yes, we will have to qualify--go through some process to qualify data.

Some of the comments disturb me a little bit. One, the notion that QA demands that you preordain what you think your results are going to be, and I disagree with that completely.

We have been working under some type of moving target QA program for many years. But we have developed a program that I think that at Los Alamos at least, we have been able to do R&D. I don't want anybody to leave here to think that we don't R&D.

I think Dave Bish, is probably a good example for the ten years he has been working in the program, and I think it is his R&D that probably gave him the opportunity to be considered as a fellow. He might agree with that.

Sure, there ar impediments and it has been difficult, but I still think that we have been able to do a fairly good job of consistent research over the last ten years that Los Alamos has been involved in the program.

DR. ALLEN: Thank you.

Mel Carter had a question.

DR. CARTER: Mel Carter, Board Member. Not a question; I had a couple of observations. I would like to add a comment to the QA.

I think what we've heard for the last couple of days, we have got a mix of people here who are not only DOE and NRC folks. We've got a variety of investigators. We've got project managers, project officers, PI's and so forth.

I think what we are hearing as far as QA that the people in the trenches, if you will, the ones doing the scientific investigation and research are under general directions, of course by DOE to do two things. One, characterize Yucca Mountain; two, make sure that everything you do is going to be usable in the licensing process.

Certainly, what I have heard is the fact that these things then begin in the way of one another. Do we support natural analogue international efforts when it is not "QAed", for example. Or do we put our money into something directly related to Yucca Mountain characterization that will be usable in the licensing process.

So, somehow or other, I think there are decisions that have to be made either by the NRC or DOE, as far as direction to the people doing the work. It sounds to me like perhaps some command decisions need to made in those areas.

I would be interested in any comments particularly from the two agencies in that regard.

The other one, I would like to make a couple of observations. In my watching democrats throw money at flat tires as Mike said earlier, as far as I am concerned they always used much larger denominations.

I would like to make a comment on what Larry Ramspott said. He indicated there ought to be a balance between keeping things in one place and using a model of course where things may leave that vicinity. And I think we've done studies both ways.

One of them that we've been discussing for two days involved natural analogues. The other set of studies are called compliance of violations. Different categories.

DR. ALLEN: Norm, you wanted to say something. Go ahead.

DR. EISENBERG: I should say that first of all I would like to make a few comments about Quality Assurance.

DR. ALLEN: Let's not have the whole meeting on QA.

DR. EISENBERG: I should say I am not an expert on Quality Assurance. But, I am from the licensing office at NRC, and I think we may have some differences in the way we look at things and the way the Office of Research does.

One point I would like to make which echoes something that Russ said, is that the NRC QA requirements are flexible. And as Russ pointed out that flexibility can be carried forward to the DOE program.

Secondly, I think it is a faulty dilemma to say that creative scientific research, can't be performed under the requirements of an effective, but benign Quality Assurance Program.

Another point is that the concept of quality or total quality management would say that you need to do some kind of QA for all parts of the program and from the beginning. And part of the reasoning for this, contrary to some of what I've heard, is that in licensing and in science, negative results are as important, sometimes more important that positive results. And one of things that QA does is that it keeps track; keeps records.

That's all I wanted to say about QA.

DR. ALLEN: Let me go around to some of the other Board Members.

Ellis, you have been very quiet, during the morning and you were awake, too.

DR. VERINK: I would like to make an observation with regard to analogues. I think that we shouldn't lose site of the fact that thinking in the terms of canisters for example. The possibilities for natural melt were very nicely set forth by Burt and I appreciated his presentation. But, the surroundings in which native copper or the iron nickel things on Disko Island provide is another natural analogue of the kind of surroundings which might well be used for placement and placement around canisters as back fill material. And, the mere fact that you've got close to a million years or more of experience with these materials in a particular kind of setting could very well provide a very strong hint as to the kinds of things that ought to be put around materials, such as that which might be used as canister materials to get a really quite secure sort of barrier system. And, at the same time, operate on the part of this picture which is important, the source term.

I wonder if there is any other viewpoint or any comments one way or the other on that. It would give an opportunity for some research for which there is a finite answer, in otherwords, what are the ingredients that are there? And have some place to work from in finite time.

DR. ALLEN: Any response?

DR. JOHNSON: I think this is a good point and time would not permit going into some of the details of the environment.

One point that I would make is that I wouldn't like to see these soils packed in around. I have a vision of free-standing canisters as being the most durable. So, I would be careful at how closely I packed the environment around the canister.

We can point to some cases such as the copper and the subsurface basalts, where I think the environment has been pretty well defined and stable over long periods of time.

There are other cases where the environment is not so well known. There are cases like Canyon Diablo where we see an enormous range of corrosion all the way from almost uncorroded to severely corroded. And we need to know how to interpret that environment to decide what are the principles that we impose on the repository.

A very fascinating observation, I think.

DR. ALLEN: Bill Murphy.

DR. MURPHY: I would like also to add to that comment. I think it is a good one. As an extension, in this program we are not a present capable of making decisions about which geologic environment we might choose to look at or consider. But, in fact natural analogues could provide that opportunity, not simply to define what would be appropriate back fill materials, but what would be an appropriate geologic environment.

DR. ALLEN: Pat Domenico, do you have a question?

DR. DOMENICO: I have a comment on the idea of analogues and validation, which is one big reason why you are into them.

And what I am saying is that if you are attempting to validate, either a transport or transfer process by means of analogues, you are not going to do it, and I'll tell you why.

You have several microscopic processes going on: Diffusion; diffusion into a matrix; dispersion; a non-equilibrium reaction. The microscopic outcome of all those microscopic goings on is a thinning of the mass.

In short, nature abhors a slug and she has built in enough redundancy to break it up. And all of them break it up. You cannot differentiate the extent to which any one of those contributes to.

As a consequence, if you are trying to validate that the process occurs when they all do the same thing, you aren't going to do it. The same way that we can't do that when we treat contaminant plumes. We treat them everyday; every week. Large organic plumes that have all of these same processes going on, you end up with a degree of non-uniqueness.

I am looking to your article, as soon as Jack sends it to me that says you have determined dispersion reaction with all of this, because, you can't. Not uniquely anyway.

That is my statement.

DR. DEERE: It's very strong.

DR. DOMENICO: Yes. Yes. It is, and it is true. It is very true, too.

DR. CANEPA: This is Julie Canepa. You must have a real love for a lot of the transport codes around the project.

DR. DOMENICO: I don't even want to look at transport codes.

DR. ALLEN: Any further comments?

Norm.

DR. EISENBERG: Norman Eisenberg from NRC. I have to disagree. I would say that perhaps you are correct that natural analogues alone cannot do the job for validation if we can even get validation. But, as many of the speakers pointed out over the last two days, if you couple the natural analogue work with laboratory work and field studies, then the combination of evidence may put you in a position to validate the models.

DR. DOMENICO: I say from the analog studies you are not going to validate a process when you have more than one process going on like you have. And, if you think about it there is absolutely no way you can, if they all do the same thing. They thin the mass. That is physics. That is not Operations Research, that's physics. If the physics doesn't work, then like Einstein without claiming parity, "I feel sorry for God". That is the way that those things operate.

DR. ALLEN: George.

DR. BIRCHARD: George Birchard, USNRC. I think that it is possible though to do field work and experiments that might separate out the process or at least some of the processes, I'm not sure all of them. You can certainly run tracer tests. You can certainly do a whole range of studies that you do for characterization of a site and an analogue site. So, it is possible to some degree I think through careful experimentation to separate some of the processes. But, I don't know ultimately if you would call it a full separation. I sure wouldn't want to say that you could.

DR. DOMENICO: Well I make a statement of saying one thing only once. This time I've said it twice. If you disagree with that, then I think that is fine.

DR. ALLEN: Dennis Price, do you have a question?

DR. PRICE: Well, somewhat related to that, I think that is probably why you buzzed my name here.

Are we not really, when it comes to model validation, when we are talking about a model validation for 10,000 to 100,000 year, with our state of a knowledge of relevant variables, what is really relevant and the data with respect to relevant variables, are we not really not in the sophisticated stage, but the somewhat brutish stage of this kind of thing, and can we talk about validation of models with this kind of a time frame at this time.

I have two specific things, but that is a real broad general comment. I have a question that I would like to get feedback on.

How confident are you folks that we can really validate models in general to ensure the safety of Yucca Mountain?

DR. ALLEN: I don't hear a resounding answer.

DR. EISENBERG: I will foolishly volunteer. This is Norman Eisenberg from NRC.

This is a subject that I have given a lot of thought to. I have written five papers on this subject. I think the ability to validate is very limited. I think there are a lot of problems. However, I think the program must make the effort if the public and the scientific community is to have any confidence in the estimates of performance which I still believe will be the foundation for the case of licensing.

DR. PRICE: You are saying, we've got to do it whether we are ready or not?

DR. EISENBERG: I think so. I think that is the problem. That is the issue before us. We have to do what we can do to try to address the issue. It may not be successful and the success may depend upon the nature of the site and how simple or complicated the various processes are there.

DR. ALLEN: Other comments?

Julie.

DR. CANEPA: I don't know how confident we have to be.

And I don't know if the Department of Energy is going to help us define what our confidence level is, or if that is something we are going to have work very closely with the NRC. Ninety percent confident; there's no hope right now. Fifty percent, maybe. But I don't know if a 50 percent confidence is acceptable. I don't know what that means. And that is something that we will have to struggle with in the next few years, I am sure.

DR. PRICE: Could I ask a couple of specific things, unless--

DR. BIRCHARD: I'm George Birchard of the USNRC. I think one needs to go through the sort of approach that we heard from several of the speakers to try to identify which processes in a system are critical to performance and to look at the stability of systems. And I think by doing that, one may not need a high degree of precision in the models and certain parts of the system. If a system for example, from a chemical point of view is very, very well buffered in a way by a large mass of rock and the amount of water flowing through the system relative to the mass of rock is small, one may with a reasonable degree of confidence be able to assess the response of the chemistry to a variety of conditions.

So I think in some cases one can validate some of your concepts with some degree of confidence because your system may turn out to be stable. On the other hand, you might find some other systems aren't stable. That goes over to the question I think brought up earlier about system design. One needs to find compatible systems based on looking at analogues, so your engineered systems are thermodynamically consistent, for example, with your host rock.

I think the Swedes have done an excellent job of that in their program. They have laid out an approach and I think it might serve as an example for other countries to try to follow, where an integration of design with performance assessment with validation.

DR. ALLEN: Thanks.

Dennis, could we continue around and then if we have time come back?

DR. PRICE: Sure.

DR. ALLEN: Don Langmuir, do you have a comment?

DR. LANGMUIR: I have a couple that Julie has been talking on. I'll focus on her.

She mentioned this morning that the DOE was proposing to limit work on colloids and not doing any lab work and look strictly at colloids in the analogue programs as a basis--am I quoting you correctly?

DR. CANEPA: That's right.

DR. LANGMUIR: The point I want to make, is if that is the case, I would suggest the cost of getting some significant information out of analogue study of colloids is probably far greater than a well set up lab experiment of colloid transport where you have control of what is going into the system and a better definition of conditions and you can focus on the questions you wish to answer.

DR. CANEPA: My response would be, yes, I am not prepared to choose a colloid natural analogue. We have cut back in our laboratory experimental work. We look at true radio colloids. We have been looking at colloid filtration, using polystyrene sols. We haven't done any colloid sorption experiments yet. We've got some colloid studies looking at gels and mineralogy. What you are getting at is kind of what I would hope that the team of scientists at Los Alamos would be thinking about.

It is not so much what analogue system can we get to. But, let's back off on all these--we have this rush of different experiments that we are doing, because I don't think we have thought out what that well-designed experiment is. And I'll admit that right now. And right now, let's stop the motion a little bit. I don't think we are answering some of the significant questions. Let's set back. Let's think about what component would be well designed experiments that we could conduct.

I'm certainly not saying that we are not going to do something like that. But, I will admit that it hasn't been done well in the last few years, although we have had some very excellent work that has come out of it. I just don't know where we are going with it and I wanted to stop the motion. The budget gave us an opportunity to stop the motion and rethink where we want to go when the budget allows us to come back in and begin our colloid work again. That was my decision.

DR. ALLEN: Warner, do you have something you want to address?

DR. NORTH: Well, I have a lot reflections on the past day and a half which I have found to be extremely useful, educational and encouraging in terms of the various efforts of using analogues, natural and anthropogenic.

I have, however, heightened concerns over issues having to do with the setting of priorities, and what are we going to do about colloids is a good example. I would love to see lists of such examples compiled. And, these become a major priority for management to work with the scientists on the program, to work out the answers to questions like this. What are we going to do about colloids?

And it seems to me the framework in which we want to work them, is the sort of thing that George Birchard was just referring to. We want to look at issues having to do with analogues as they relate to validation and as related to design, and maybe we throw in lab as an alternative to analogue. How are we going to get this kind of information? How can we get the appropriate degrees of synergism between these activities, get the different people and the different specialties and parts of the program to talk to each other?

Now, we've heard from a number of speakers that the Europeans, and in particular, the Swedish, seem to do something very right in this dimension. Maybe it is the size of their program. Maybe it is the scale in which they operate. Maybe it is a lack of some impediments that are designed into the U.S. program the way that it is chartered. But, it seems to me it would be awfully useful to try to understand better why it is that SKB and the European operations in general are doing so well.

I think the Board having had the experience of a week in Europe has had a little opportunity to do data gather for itself. But, I would very much welcome whatever parting comments this Panel can give on this subject, and on the subject on what they think could be done to improve the setting of priorities, and the degree of communication among the various parts of the program that ought to be talking to each other.

DR. ALLEN:

DR. DYER: This is Russ Dyer from DOE.

As far as priorities, remember we just went through an exercise to test prioritization task. We developed a set of priorities for part of the program and you see some of those priorities reflected in our planning for next year.

One of the things that came out of that exercise is that what we used as measure of performance there, that is a surrogate for total system performance, could not be the sole driver for our program. So, we were trying to strike a delicate balance right now, making sure that the mandatory parts of the program are in place and viable.

DR. ALLEN: I might just also respond that in talking with many of my European colleagues where we complain about bureaucracy in this country, oh, boy, we are not alone in having bureaucratic problems. And I think it would be naive of us to think that the Germans and the Swedes are somehow without some of these same problems.

Rod, you had something you wanted to say?

DR. EWING: To address both points, in terms of setting priorities and this may be an inappropriate comment, but I am involved with the WIPP site. I am on the panel that reviews their progress and to make recommendations. And it was very helpful when they finally began their performance assessment no matter how crude. A performance assessment is something you have to do. The sooner you begin, then you have tool that allows you to at least in your own mind perhaps quantitatively weigh alternatives. So, that would be one recommendation.

I won't belabor my enthusiasm for the Swedish program. But I will make one last observation, which is perhaps symptomatic of some of the problems with the American side.

It seems to me strange that we have international programs separated from our national programs. As a participant in the Swedish program, German program, French program, I am never identified as the international component. I am just someone there to work. And I think perhaps we make it too difficult and too elaborate to participate on the international stage. And in particular, we make it very difficult and certainly too elaborate to exchange scientists back and forth, supporting their scientists to join our laboratories in sending our scientists there.

I think the difficulty from the American side is if we invest a man year, we want to understand what we got as a product. And usually you don't get product by sending someone abroad for a year.

DR. ALLEN: Michael.

DR. SHEA: A comment on the viability of the European programs, I would also include NAGRA. This program is also well run. And I think it is unfair to lay the blame, if there is any problem with our program, at the feet of the USDOE. I think that is really unfair.

I think one of the major distinctions between SKB and NAGRA for example, is that those organizations are removed a fair amount from the politics of the situation. The USDOE is not. I mean they are encumbered with all the vagaries that happen in Washington. It affects the budget and everything. So, I think that is one of the main reasons that you get the steady funding from the European projects. Things are done without that concern of doing something politically wrong, and you can do the right kind of work.

DR. ALLEN: May I ask if any of our senior staff member people would like to step up to the mike and make a comment or ask a question.

Leo.

DR. REITER: I would just have a question of Mike Winograd. I wonder if he might expand on the comment he made that we are making a regulatory and scientific mountain out of an engineering molehill. And I wonder if other people would comment on that also.

DR. WINOGRAD: Well that was tossed out to get attention, which it did. It was tossed out because of a feeling that I've gotten watching the program from a great distance that there is a disparity in the number and there is also a disconnect between the engineering and the geologic studies of the sites. People aren't talking to one another.

It was a result of a feeling that each of our endeavors however fond we are of them, really are much more qualitative than we might like to admit. And I have to agree with Pat from a hydrogeologic cap side, that you can study a modern system for ten years and still have to admit to yourself that there are elements of it you don't understand. So then if you are going to turn around and say, I am going to look at wonderful analogue and everybody agrees that some of the things that we have heard the last two days, exciting studies obviously being done by enthusiastic, very bright people, but to imply that we are going to come up with pretty good answers from those sites, some of which are tens of millions of years old, is very difficult for me from a hydrogeologic perspective to accept as realistic.

And so I view that in the long-term, the only way to converge in this program is to approach it several directions, admitting at the outset that probably none of them will be as quantitative as we'd like. But if they all tend to give us the same answer, that perhaps then we can feel we are on the right track. So, that is what was behind that.

It's a philosophical problem I have. I think there has been over-emphasis by the NRC and the EPA that we will quantify things that perhaps cannot be unquantifiable. And of course, the Academy hit hard on this in their rethinking of radioactive waste and others have done it before the Academy. That's where that stems from.

DR. ALLEN: Any other comments on that?

Norm.

DR. EISENBERG: Norman Eisenberg, NRC. If I could try to respond to Dr. North's two issues.

About priorities as you all probably know, the NRC has gone on record recommending that DOE engage in iterative performance assessments. Part of the reasoning behind this is that from such assessment, you gain an appreciation for what is important and what is not. Not necessarily strictly by working with the models and codes, and quantitatively determining through a sensitivity or an uncertainty analysis what is important.

But, I believe that often the analysts knows best what data and knowledge are needed to improve the analysis and what assumptions or concepts are weak. That's one point.

With regard to the apparent greater success of the European programs, I believe the size is a factor in that. Sometimes smaller is better. It is easier to get things done.

I would also like to add the thought that the people outside the United States do sometimes take advantage of the fact that we have built up a technological base and have gotten a great deal of expertise in certain individuals. Then they can harvest that technology at a rather small price. And I think that may be a factor, also.

DR. WINOGRAD: Ike Winograd.

Another possible focus on the European programs is that in some of the smaller countries, particularly, Sweden, Belgium, Netherlands, they have fewer rock types. So, this right off the bat reduces their options. We have been blessed with all kinds of rock types, all kinds of environments, saturated, unsaturated, you name it.

Another aspect is with regard to the success of the Swedish program, I will just ask a question. And I admire their work also. Just a question, are they still planning to encapsulate their wastes in copper? Is that still the plan? Okay, well if one is interested in the human intrusion issue, are they not creating an ore deposit at shallow depth? I say they are.

DR. LYMAN: I say they are not. I am Stuart Lyman with the Copper Development Association. And I want to thank Dr. Winograd.

That is one of the greatest misapprehensions that exists within the nuclear waste disposal community. And I cannot imagine how it persists, much less how it began.

If the U.S. were to take the original Swedish copper approach, a solid copper monolith with rebars, using the spent fuel elements sort of like rebars in concrete embedded in a solid copper monolith, a concept, which by the way could certainly be sold to the public, I believe. But, if the U.S. were to take that approach it would take 200,000 tons of copper over 20 years for the first repository. If the Swedes persist with that approach it would take them 70,000 tons for their first, last and always repository.

Two hundred thousand tons over 20 years is 10,000 tons a year. That's 20 million pounds. We use more than that amount of copper for coinage in the U.S.A. each year. And the penny isn't even copper anymore. When the penny was copper, it required five times as much copper as it would take for the first U.S. repository.

If there is any future ore deposit being created, I think it is being created by you people, not by the nuclear community and not by the copper that might be put in the ground.

But, being on my feet, I would like to make another point of two. I was naturally disappointed to hear so little about the container in terms of natural analogues or archeological analogues, although copper was referred to very appropriately and nicely and other metals in two presentations.

It seems to me that the most important natural analogue to study are the container material analogues. The borosilicate glass and the transport of radionuclides, don't seem to come into the picture until the container is breached. So, let's look at the natural analogues for containers. And it appears that the native coppers have not been really looked at since 1980 or in '82 I think it was.

A very nice report was done on the Michigan native coppers Chrisman and Jacobs up in part of the BWIPP program. But to the extent that the native coppers seemed to be referred to, I think they have taken on sort of a stereotype that isn't correct. They referred to native coppers are coppers referred to in terms of the natural analogue as being material that can persist for hundreds of billions of years in certain reducing basaltic environments.

I'd like to recommend a more rigorous investigation along the Johnsford Mills line of the native coppers. Actually native coppers have been found many places besides northern Michigan. I don't know what the host rock was in New Mexico and Arizona and New Jersey and other places where native coppers have been found. I think it would be interesting to the U.S. program to have a little investigation of that.

Another thing that I think ought to be looked into is this business of this matter of the reducing environment. The only reason native coppers were discovered in the first place is because they were in an oxidizing environment. And they weren't discovered by geologists. They were discovered by appropriately enough, Native Americans. And there is a bridge there to archaeologic investigations.

The Native Americans, long before the Indians as we know them, used copper artifacts up in Michigan. There is literature on that that should be looked into to bridge over. And native coppers as Chrisman and Jacobs pointed out also existed in highly corrosive brines in the basalt environments whereas they put it they were surprisingly uncorroded.

So again I want to think Dr. Winograd for his question.

DR. ALLEN: Thank you, Mr. Lyman.

DR. WINOGRAD: Dr. Allen, can I have a half minute response?

DR. ALLEN: All right. Half a minute and then I promised Dennis I would get back to him for one final question.

DR. WINOGRAD: Ike Winograd responding to the gentleman from the copper institute.

The idea of the problem with the ore deposit, it is not its wealth. Those are the concerns of the future human intrusion issues. I am not saying I am one of them. I am concerned with the blundering into the sites of our "unaware" descendants. It is not the value of the copper. It is something that will attract people who are unaware of the radioactivity.

Now you can say that this is getting into the realm of science fiction if you wish. Some people have said it. I have even said it at times. But. that is the issue. It is not the value of the copper.

DR. ALLEN: Thank you, Ike.

Dennis, we have one final comment.

DR. PRICE: Okay. If it is only one, I won't make my question to Mr. Johnson about ceramics. I'll just let that one go as a natural analogue.

I'd like to ask Mr. Ramspott if he wouldn't like to comment on this observation, that transport to the water table seems to me to be a priority to Yucca Mountain. That is vertical. And the NTS test as I understood what you presented did not address this. Since this is a focus thing, should this not have some priority?

DR. RAMSPOTT: Well, I can't speak to the current priorities of the HRMP program. We went through in the back of that Borg, et al., we made a number of recommendations, including verifying that all of the test material above the water table was actually isolated from the water table. Not a great deal of work has been done in carrying that out.

Are you referring to this being funded by OCRWM?

DR. PRICE: Yes, by OCRWM?

DR. RAMSPOTT: Well, that would have to be up to OCRWM. But, it does provide as I said an opportunity to look at material that has been introduced above the water table, sometimes a considerably distance and sometimes fairly close, and actually see if anything has migrated.

DR. PRICE: Maybe I should have asked DOE this question.

DR. RAMSPOTT: But, as far as I know, it is also not being done with a high priority by the HRMP. They are still focused on the saturated zone.

DR. ALLEN: Russ.

DR. DYER: Let me just add one statement there, Dr. Price to follow up on Larry's response.

We have interactions regularly with the test site on this program. I don't know the status of whether we have requested that they add this particular test to their suite or not. I can check on it for you and let you know.

DR. ALLEN: Okay. Thank you, ladies and gentlemen. We could go on, I am sure for another hour or so and I wish we could. But, you and we both have other things on our schedules.

May I thank all of you as participants and turn the meeting back over to Don Deere.

DR. DEERE: Again, I wish to thank those who helped organize the meeting; those who were sufficiently interested to provide funding for some of you; and, particularly for the speakers that took the time and the energy to prepare and to come and share some of their knowledge with us.

I am sure we have all found it to be extremely beneficial. We just simply thank you all.

(Whereupon, the meeting was concluded.)