# UNITED STATES

# NUCLEAR WASTE TECHNICAL REVIEW BOARD

## FULL BOARD MEETING

## NATURAL and ARCHEOLOGICAL ANALOGUES

April 16, 1991

Peppermill Hotel
2707 South Virginia Avenue
Reno, Nevada

# APPEARANCES

Executive Director, William D. Barnard
Deputy Executive Director, Dennis G. Condie

<u>BOARD MEMBERS PRESENT</u>:
Dr. Don U. Deere, Chairman, NWTRB
Dr. Ellis D. Verink, Session Chairman
Dr. Clarence Allen, Member
Dr. John E. Cantlon, Member
Dr. Melvin W. Carter, Member
Dr. Patrick A. Domenico, Member
Dr. Donald Langmuir, Member
Dr. D. Warner North, Member
Dr. Dennis L. Price, Member
<u>SENIOR PROFESSIONAL STAFF</u>:
Dr. Sidney J.S. Parry
Dr. Leon Reiter
Dr. Sherwood C. Chu
Russell K. McFarland

Paula N. Alford, Director, External Affairs
Helen W. Einersen, Executive Assistant

# INDEX

I N D E X (Continued)

P R O C E E D I N G S

8:30 a.m.

DR. DEERE: Good morning. I am Don Deere, Chairman of the Nuclear Waste Technical Review Board. It's a pleasure to welcome you to our second meeting of 1991 of the full Board.

Most of you know that the Board was created by Congress in 1987 to review the technical and scientific validity of the Department of Energy's program for managing high-level radioactive waste disposal. In the same law, Congress directed the DOE to characterize a site at Yucca Mountain, Nevada as the possible location for a geologic repository for the permanent disposal of high-level radioactive waste.

The Board's charge includes the evaluation of the DOE site characterization activities at Yucca Mountain, as well as activities involved in the packaging and transportation of the waste that could ultimately be stored there.

In its second report, the Board made 20 recommendations to Congress and the Secretary of Energy. Under the general heading of, "Risk and Performance Analysis," the Board recommended, among other things, that the DOE consider investigating more extensively the use of natural analogues to support performance assessment for a potential repository at the Yucca Mountain site.

Because of the Board's continued interest in this subject, we will be devoting the next two days to a discussion of the use of analogues in performance assessment in this country and overseas.

I wish to thank Dr. Jack Parry, Senior Professional Staff of our Board, and Russ Dyer and Ardyth Simmons of DOE for organizing these sessions. Several of the Board's panels are involved in aspects of this topic. I have asked the chairs of those panels, Dr. Ellis Verink, Donald Langmuir, Warner North, and Clarence Allen, to preside over the individual sessions today and tomorrow. Dr. Verink will chair our first session.

As always, the opinions expressed by our Board members during these discussions reflect their personal points of view, and not necessarily official Board policy at this time in time.

Before Dr. Verink takes over, we will hear three special presentations; two from representatives of the State of Nevada on issues of concern to the state, and one from Dr. Gene Roseboom of the U.S. Geological Survey.

Our first speaker will be Mr. Bob Loux, who has been Director of Nevada's Nuclear Waste Project Office since its creation in 1983. Mr. Loux has been employed by the State of Nevada since 1976. His work for the state has been primarily in the energy policy area, most recently on high-level radioactive waste management issues. He will introduce our next speaker, Mr. Peter Hummel of the Nevada State Commission on Natural Resources, who will give a presentation on the petroleum potential of the Yucca Mountain area.

I have been asked to remind you that we have a great number of microphones out and, therefore, you must speak almost directly into them to be picked up by our sound system and to be recorded.

I now will turn the meeting over to you, Bob.

MR. LOUX: Thank you, Don.

I guess on behalf--first of all, on behalf of the State of Nevada, I'd like to welcome the Board to Reno and to Nevada. I hope your stay here is productive. If there's anything that we in the state can do to further your meeting and make it more productive, please let us know. If we can help out in any way, shape, or form, please let us know. We'd be happy to help out.

I'd like to, of course, reinforce the state's motto of keeping Nevada green, and I'm sure you'll all help out in that regard as well.

I must tell you that it's been awhile since I've talked to the Board directly. I know my staff interacts with the Board and its various committees rather frequently as they meet. Carl Johnson of my staff is here today, and Carl will be here throughout the day. Also, Steve Frischman and Susan

Zimmerman are here from my staff, and they'll be available for most of the day as well. I must apologize, and I'm going to be having to leave fairly shortly, but having said that, let me reiterate that the State and the Board, I think, have had a rather productive relationship in the number of years since the Board has been in operation. We have found that working with you all has been very productive and we've admired your professionalism and scientific integrity.

I'd like to also compliment Dr. North--however, he's not here--and tell him that I appreciated his involvement with the State Commission on Nuclear Projects, which he graciously agree to, over a year ago--or nearly a year ago--to come and address in Las Vegas, and I think that we found his presentation on the Board's activities, and I know the Commission--my Commission--felt it was very valuable to have direct interaction with him and understand more about the Board's activities, and also that he could hear their concerns about the program in general.

Having said that, let me turn to the couple issues I want to address briefly this morning, and then I'll let you get on. The first one really has to do with, I want to clarify for the Board and for the audience, a little bit about where we are currently; that is, the State of Nevada, vis-a-vis permitting issues and other kinds of activities, legal proceedings which have been much in the news and much before your attention recently, and talk a little bit about some of the state activities, and then turn to one other issue.

First of all, let me reiterate--and I know Don knows, and Bill, from being at the hearing in March in front of Johnson's Committee, where the Governor, indeed, testified --that there are two, I guess, principal overreaching aspects of the state's concern with the entire project that, I think, have at least some bearing and understanding, and they are the issues, generally, of fairness and trust, besides other issues, of course, that are more of a scientific or technical nature, but they are the overreaching issues that really are guiding much of the state's thinking, much of the state's policy as it relates to the program right now.

I don't need to reinforce for you or the audience, necessarily, the activities that occurred prior to 1987, but certainly, from the state's mind, the '87 Act is a watershed in this program and really has a great deal to do with where the state is currently, and as you know, prior to 1987 there were at least a scheme of three sites to be characterized in an eastern site and, of course, it has been, essentially, the Governor's perspective--as that of many state leaders--that with only a single site to be looked at, that you really can't convince the public of any degree of fairness, equity; in fact, may not be able to convince them of scientific integrity in any process that's going on. There simply isn't anything else to compare this process to.

But having said that, let me reiterate the process, briefly, for you. As you know, shortly after the '87 Amendments Act, the Nevada legislature passed several pieces of legislation which, at least the Governor and the Attorney General, construed as meeting the form and content of the state's noticed disapproval under the Nuclear Waste Policy Act, and shortly thereafter those actions were taken, the state, in fact, through the Attorney General and the Governor, declared that they had, indeed, vetoed the site and it was terminal from that point on, at least in their mind, especially as it related to three environmental permits the Department of Energy had pending in front of review agencies at the time, as well as a number of other issues.

And since that issue is largely--that is, the lawsuit relative to the state's vetoing the site has been largely resolved at this point, let me reiterate a couple points that I think are worth mentioning.

The state initiated that lawsuit essentially for three reasons. Number one, of course, we thought that there was an opportunity that, in fact, the state had exercised its veto under the law, relying on the legislative provisions and, in fact, we believed--as you may know--that that was somewhat of a long shot and it was not supported by the courts.

I think more importantly, I think, than that is the viewpoint that after the '87 Amendments Act passed, there was a need to clarify, at least in our mind if not many others' minds, indeed, when in point in time the state was to exercise its veto or notice of disapproval under this law.  There are many who thought that the President's selection of the site, or signing of the Amendments Act, which selected the site as the only site for characterization, could have constituted the Presidential recommendation of the site and, clearly, that point had to be clarified, at least in the state's mind.

And I think thirdly, given the Department of Energy's track record and the way in which they uniquely interpret law, I think that many of us believed that at some point in time later on, the Department of Energy may likely have come back and said, "Geez, the point in time you needed to veto was in 1987, or after the President signed the Amendments Act, and you missed your chance and we're moving on with the program," and I think the state believed it was vitally important to protect its interest in that regard to ensure that, in fact, the Department of Energy couldn't, at a later point in time, claim that the '87 Amendments Act was the selection of Yucca Mountain and, indeed, the state had lost its veto and notice and opportunity.

I guess in many ways the state felt it was very important to set this in concrete, have the courts, rather than some administrative agency at the federal level, make this determination and indeed that's, I think, one of the objectives that we clearly established via the lawsuit, and I think it's quite clear now, vis-a-vis the Ninth Circuit decision, and, indeed, that of the Supreme Court, by not taking the case, that the time to veto that site is later on in the process. But, indeed, that was the purpose.

And as you know, during that period of time, in 1988, the Department of Energy applied for three environmental permits from various agencies at the state level: an air quality or surface disturbance permit; one for use of underground injection for--under the underground injection control regulations for monitoring groundwater; and also, a water appropriation. The state, having resolved the issue of the veto in the courts, has announced not only to Congress--as Don and Bill are aware from being there--but also to the courts that, indeed, the state is moving forward with the processing of those three permit applications once this issue has been resolved and, indeed, I think those applications for the most part have been filed and the process is going forward.

I may note that, in fact, several of them--at least one of them, let me correct myself, one of them has been refiled with numerous questions that have been asked, again, by the permitting agencies, the same questions that were asked in 1988, and have not been responded to as of this date, and it has to do with the approach, I think, the Department of Energy is using, and views this permitting activity as somewhat of a done deal and something that is not requiring a lot of analysis.

For example, DOE has yet to provide the permitting agency with what substance they'd like to inject in the underground through the boreholes to monitor groundwater movement.  In the applications, they've failed to even identify whether it's a chemical or radioactive tracer, or even which tracer it is, something that the permitting agencies asked DOE for in 1988 and still have not had an answer for.  So the idea that, in fact, this permitting process is an automatic is quite far from the truth, and DOE has been less than a full applicant in terms of interacting with those agencies and providing them with all the information that they need in order to evaluate those applications.

And I think the problem has been somewhat more acute by the Department and some of their supporters on the hill, which now have demanded a yes or no answer from the permitting agencies within a date certain about these actual permit applications; 75 days, in one question, from a senator to the Governor was:  "We want an answer, yes or no, in 75 days whether these permits are going to be issued so that we know where we stand," and I think that there's many who view that as an interference in the regulatory process that is, as you know, very independent.

The other issue, of course, has to do with trust in the Department, and these issues are also pertinent to that process as well. As you know, the State of Nevada has very little confidence and trust in the Department's program, and, indeed, in the Department itself. It certainly is reflected in the way the Department treats the state; the way it's handled, for instance, its permitting process; as well as it's handled a number of other issues, both historically and in the very recent past, but not to belabor that point too much, suffice to say that that, in fact, is one of the principal reasons that the state has embarked upon the kind of program and kind of posture it has relative to where things are at, and it certainly has to do with DOE and the lack of trust therein, and also, as I mentioned at the outset, the fairness.

Having said that, let me just finalize my comments with a couple notes that I think would be worthwhile. Going to the very positive things, of course, as you know that's come out of the Amendments Act, and I might add the state views that probably there's only one or two, and certainly, one of them is the creation of this Board and their involvement in the process.

They have, I think, interjected a very positive atmosphere to the program, and I, again, want to reiterate that our relationship and interactions with the Board, I think, have been quite positive. Let me, in that vein, at least, make two or three suggestions that I think might be important to take a look at as the Board proceeds down the road with its activities, more, I guess, in the administrative area; perhaps slightly into the technical area, and I think the concern is, is relative to preservation of the Board's independence.

As the Board is aware, certainly the state and a number of other parties have an independent oversight role relative to the Department of Energy's program. Ourselves and the NRC and the Board are the principal players in that process, and the state, of course, has been looking at this issue for quite some time and been involved with it in looking at independence from DOE's program, and has resulted in some legal actions, as well as other kinds of things.

But there is a role out here that the public plays in this process, and certainly, their perception of what is going on is somewhat important, as well. I think it might be important that we make a distinction between independent oversight and what's commonly looked at as DOE needing a board of directors in any sense. I know that Bill and I, we talked about this issue in Denver recently, at the strategic principles meeting the Department of Energy was involved at, and there was some discussion about whether certain sorts of groups--and namely, the one that we were involved with--ought to play a role of being somewhat of a board of directors for the Department's program to guide it along, and I think rightly so.

Both ourselves and Joe Youngblood from the NRC had a very strong reaction to that particular notion, in that this preservation of the independence would likely be compromised in such a process, and so I think there's a real need to separate out the view of an independent overseer of DOE's program, and one that gets too close in the context of perhaps being what many might view as a board of directors in any part of the program.

I think there's also a concern--and I know both of our United States Senators have raised the issue in some vein --relative to the jurisdiction role of the Board, and as Don noted at the outset, the law requires, or talks about the review of the validity of DOE's scientific and technical program. I think there's some who view that the Board's role might be to eventually pass judgment on the suitability of Yucca Mountain itself, and I know the Board views that differently, but I want to caution that there are some who view that that ought to be the Board's role, and there are some who have some concern about advocating in the regulatory role as a activity that the Board perhaps ought to have some caution with, in addition, relative to the EPA standards and the like; that that may, in some people's mind, go outside the bounds of jurisdiction.

There are also some, obviously, who have a much more narrow view of what the Board ought to be in looking at; that they ought to not be looking at actual DOE research results, but ought to be examining mainly techniques and methods. Are the plans adequate? Are the techniques and methodologies adequate to actually review what data is out there? And so those are--that's another area that I would express some caution in.

Lastly, or next to last, let me indicate, also, I think there's probably a need for a little bit better documentation; substantiation, if you would, about conclusions. I think that many of us--and I know in the public--would also appreciate some substantiation or documentation of various conclusions or recommendations reached in the Board's annual report through whatever mechanism might be available; through other subsidiary reports, or memorandums, or informal kinds of communications within the Board itself.

And having said that, let me indicate that, as well, to the extent that the Board hasn't, I think there's probably a need, also, to examine the full depth and breadth of DOE's database, not just the data which DOE provides and not just DOE's interpretations of that data, but let's look at the entire database. I know that the state has looked at that database, and that's caused us to look at the program, perhaps, scientifically or technically a little bit different than everybody else. I don't know that there's another entity besides the state and DOE at this point who has really looked at the data itself, or the full range of data, and not just the interpretations provided by the Department, or the data that they provide.

And lastly, let me suggest that I think there's probably a need--and we may not get a lot of agreement on this point, but--to take a look at the current, now, validity of the site characterization plan. I think there's many in the state, as well as a number of other parties who believe that the program has changed substantially since the SCP was originally written. There have been, I think, a great deal of changes in the ESF, in the, also, introduction to the prioritization activities. Site suitability methodology is coming. There have been some study plans issued, obviously, and a great many more to come. Are those still the same, right study plans? Do they still do the job? Do they still accurately reflect the program that's needed to characterize Yucca Mountain?

Those are all questions that I know our researchers, and many in the state are asking about, whether the SCP still reflects a good plan to evaluate the site, given the changes that occurred in the program, and the approach to investigating the site since the plan was first issued, and I would urge the Board to take a look at that particular issue in depth itself.

Mr. Chairman, having said those things, and again, welcoming you to Nevada and Reno, in particular, I'd like to go ahead and conclude and introduce Peter. However, before I do so, I'd be happy to answer any questions or inquiry you might have.

DR. DEERE: If you could stay around, perhaps, just for a few minutes, after we finish the first three speakers, maybe we can take questions of all three of them.

MR. LOUX: Okay. I'd be happy to do that, to the extent I can.

Let me introduce to you, then, Peter Hummel. Peter is on the state Commission on Mineral Resources, a state commission that was created--Peter, correct me if I'm wrong--in 1983, and the state at one point in time had a division of mineral resources, which the legislature eliminated in the early seventies, and then with the renewed interest in minerals and mining, in general, in the early eighties, recreated a Commission on Mineral Resources and Department of Minerals, which it oversees and administers.

But having said that, let me introduce to you Peter Hummel.

DR. HUMMEL: Thank you, Bob.

How many know where the best on-shore producing oil well in the United States is located?

DR. DOMENICO: Can we take a wild guess?

DR. HUMMEL: 150 miles north of Yucca Mountain.

Let's talk about oil in Nevada for a few minutes. As Bob mentioned, I have two hats; one as an active exploration oil geologist, and the other as vice-chairman of the state Minerals Commission, and I've got them both on this morning.

Now, let's get back to that biggest well. You know, actually, oil was discovered in Nevada at Eagle Springs in May of 1954, and it had an initial production, that first well, of 343 barrels, and this, after my professor at Stanford--a very highly renowned professor--proclaimed that he would drink all of the oil produced, all the commercial oil produced in Nevada. Well, at year's end, that Eagle Springs field has produced about almost four million barrels of oil.

Well, after that came Trap Springs, in 1976, and that produces from volcanics, volcanic ignimbrites between four and seven thousand feet. Total production there, about nine million barrels. Then came Current, then Bacon Flats and Blackburn, and then Grant Canyon. The Grant Canyon field produces from 150 feet at Devonian Reservoir at 4,000 feet. That's a shallow well. The two wells in the field, since their completion in July of 1984, have produced over 14 million barrels of oil, and the big one, the number three, has flowed 4,000 barrels of oil a day from 4,000 feet. It's done that every day for seven years. It's getting better as time goes by.

Let's just put this in the right context. That Grant Canyon No. 3 well cost about a half a million dollars to drill, and that well has paid back, every thirty days for seven years.

While we're doing this, I have this map here. It shows the overthrust trends we're going to talk about, the Grant Canyon well. The red circles here are some of the wells in the state that have had oil and gas shows. There are many more. The pink are oil shows--the blue are oil shows and the pink are oil and gas shows. These green triangles are oil wells that have cut the cretaceous thrust belts that we'll be talking about. You can see it all up and down the state at outcrops. Where the three wells have gone deep enough, that's the thrust. Then down here we've got Yucca Mountain.

Now, to date, all the production in Nevada comes from what we refer to as the shallow fault block play, 5-6-7-8,000 feet, but you can be certain that all that oil coming from Grant Canyon is not coming from 4,000 feet. We're convinced that it's fed by a much deeper structure, and that's what's fun to think about.

Now, this Great Basin that we're in, which runs from Winnemuka, Nevada to Salt Lake City, is 36,000 feet deep. It's the deepest basin in the United States, and it contains massive thicknesses of Paleozoic, reservoir-quality limestones, and organic rich shales.

I listened to a paper several years ago about the potential of oil in Nevada by the USGS, and the final two slides he put up just on a situation like this. On the left he had a cross section of the Great Basin, and on the right he had a cross section of the basin that underlies the Persian Gulf, and the similarity was astounding. I'll tell you, it was even scary. And this same author, considered by many to be the father of geologic thinking in the Great Basin, projected total cumulative reserves in the 1950's to be substantially less than the 30 million barrels the state's already produced. Well, he now talks 300 million barrels of recoverable reserves, but we think he's just as far off now as he was then, and we know how far off that Stanford professor was.

Now, how many of you remember Pine View? Pine View was the discovery of the Wyoming thrust belt, billion barrel trend. Everybody in industry--I mean everybody--looked at that play, including myself, at least once, and turned it down. And finally, after about ten years, American Quasar drilled it, and thus began the events that led to the development of numerous billion barrel fields in western Wyoming.

Well, we have all the ingredients here to bake the cake in our thrust belt that runs from Elko, Nevada to Yucca Mountain, and with the right exploratory climate, we think we can find some monsters.

At this time, I'd like to introduce to you Alan Chamberlain, who's completing his Ph.D. work at Colorado School of Mines in Golden. Alan and his brother have prepared a balanced cross section, across the thrust belt and Yucca Mountain, and he's going to take over now and show this to you. I just might say that his brother, Randy, is probably the most experienced geologist in thrust structures in Wyoming that there is, so he brings that knowledge to this Wyoming thrust belt that's very special.

Alan, why don't you come up?

MR. CHAMBERLAIN: Well, thanks for the introduction. It's going to be a lot of fun to talk to you for a few minutes about the most exciting exploration play in North America, maybe in the world, and I'm real excited about it, and like Peter mentioned, these are just a few of the oil and gas shows found in wells in Nevada.

What I'm going to try to do--you might want to dim the lights just a little bit so you can see the slides a little better, if you can do that. For the next 15-20 minutes, I'm going to try to show you some of the intense research I've been involved in for the last 15 to 20 years. So we're going to try to melt it down real fast, give you a lot of geology, oil/petroleum geology for a few minutes, and then after, if you have questions we'll talk about them. I've got more detailed stuff, and maps and that.

But what I'll be talking about mostly is the central Nevada thrust belt. It's a brand new play breaking loose in Nevada. It's brand new right now. It has not been explored yet. It's an area that's receiving a lot of attention. This spring I'll be taking a bunch of oil company executives on a field trip in May to take them out and show them this oil spill. I just got through taking another company out with five executives. They decided they're going to come out to Nevada with both guns blasting to explore for this thrust belt. It's an exciting play. It's big.

What I'll talk about, I'll give you a little bit of an introduction. I'll cover a little bit of the stratigraphy of the eastern Great Basin. We'll touch on the source rock, briefly, some of the reservoir rock, some of the structures, some of the methods used in the balancing of the cross sections that I'll show at the end, and then some of the analogues that are involved in the play in the eastern Great Basin.

So the introduction, I'll show you where the thrust belt system goes. This is the central Nevada thrust belt, right down through here. Here's Yucca Mountain down in this area. Here's a Railroad Valley play, where Peter talked about that large producing well that's flowing--well, two wells there, flowing 6,000 barrels a day, unheard of in North America for so many years. And here's Pine Valley, another play, and in comparison with the Utah/Wyoming overthrust belt, which is over here, in comparison of amount of area covered, the Utah/Wyoming overthrust with billion barrel oil fields--and many of them--covers an area about 40 miles. We're talking about 400 miles of potential in Nevada. It's a big play and a big area.

So just briefly, on the stratigraphy, like Peter mentioned again, we come from the Utah hinge line, which is over here, where the Paleozoic rocks up on the Utah/Wyoming overthrust are just a couple thousand feet thick. We come out across the passive margin sequence. We have up to 45,000 feet of stratigraphy to work with, many potential source rocks, especially in the Mississippian, and really good reservoir rocks, especially in the Devonian. These others can be good reservoir rocks, also, but the main source rock is the Mississippian source rocks. The main reservoir rock for these oil fields is the Devonian, that we know of right now.

What about source rock and depositional setting of it, the organic richness and organic maturation of the source rock? In order to get oil, we've got to get a source rock, and this is just a time slice based on palynomorph biostratigraphy from--the database I used is over a million-foot measured sections that I've been involved in measuring, and this is just one time slice of the Mississippian in late Chesterine time. We have a bunch of deltas and a lot of Lacustrine deposits out here, that's inter-bedded with some shale, marine shales. Right through here is the best source rocks in Nevada. It runs right down through this area here. That's the richest. Some places it's up to 4,000 feet thick, and if you do some number-crunching with it, it can generate up to trillions of barrels or oil. Big.

Here's a average organic content map. Now, these are--just took a series of points from measured sections and from wells, took the organic richness, and just contoured it up. And we see the organic richness increases to the west, so here in Railroad Valley, the organic richness increases, increases over here toward the Blackburn field. I got new data. We need to update this slide. We've got new data down in here that shows that the organic richness increases over into here. So we have a very good source rock in the Mississippian. If I redid that slide, I'd put thrust teeth along here, because the very richest rocks are tucked underneath the thrust, and we'll talk about that a little bit more.

Maturation. This is just one of the many maps we did. I processed literally thousands of conodonts, which are little microscopic fossils you see in limestone, and by the color of them, you can get an idea of what the rocks--what kind of temperatures they've gone through. To get oil to generate, the temperature's got to be just right to generate hydrocarbons. It's got to be high enough to get it out of the oil shales, but not too high to burn the hydrocarbons.

The rocks within the oil window, the Mississippian rocks, are located right down through here, again, from Elko, Nevada, all the way down through this part of the world. Again, the good rocks are tucked underneath the thrust belt, so there's some real good rocks tucked underneath there. Again, as we get into the thrusts, we'll talk about that a little bit more.

What about the reservoir rock? We'll talk just briefly on depositional history or setting, the porosity, the probability, some of the volume involved. One of the techniques that I came up with when I was measuring all these rocks in Nevada, I came up with the idea of logging the outcrop with a scintillation counter, just like you log a well. So we generated well-like data. When I gave my poster session at the National American Association for Petroleum Geologists in Dallas in '83, I took first place in the session because it was such a simple idea, and nobody was really practicing using that.

Well, all million and a half feet of rock that I've measured in Nevada has been used to log the outcrops, and as you do that, you can get a better definition of some of the depositional environments. Here's just a short section of 500 feet of the Devonian rocks. Here's a reef in here. Here's a fore reef, the reef complex, and the back reef environment. You can see it very nicely on the well log or on the gamma ray log. You can compare these with mountain range to mountain range. If we ever drill a well in the valley, we can compare it with the wells, so we're beginning to generate data for the first time that we can really use to explore Nevada.

Well, the slide on this side was, because I have all the data--I'll tell you what it is--I have all the data on a-- I have it all computerized. It's all in disk. It's the first time somebody's ever put all these measured sections on disk, and I could press a couple buttons and I get a limestone dolomite ratio map, which this map was, of the first 500 feet of the guilmette formation, and then by using that, along with some of the depositional indicators within the rocks themselves, we can come up with the depositional environments of each time slice through all this Paleozoic sequence in Nevada.

This particular sequence is the main reservoir rock of the Grant Canyon field. There's a high area right in here of inter-title or subtitle, supra-title environment. We've got the same thing down here. There's a bunch of patch reefs developed right around here, and the Grant Canyon field is right close to one of these patch reefs within the Devonian formation. That's the rock that is flowing at 6,000 barrels a day out of two little wells, okay, so it gives you an idea of the environment. The reservoir rocks are just fantastic.

Some of the porosity, this is out of the Grant Canyon No. 1 well. You look right through the particular core. Here's the little fossils that have a lot of vuggy porosity. Here's the big vugs in here. This is kind of like the billion barrel Yates field down in Texas, which is flowing, you know, tremendous amounts--has tremendous amounts of oil in. Well, we've got the same thing here at the Grant Canyon field, only we've got a lot more rock to deal with. We've got thousands of feet to deal with, but this is just one little piece of core showing the amount of porosity you can have.

I did a detailed study of the petrology of this last semester at school, and it looks like the oil is flowing from a deep reservoir, around 12,000 feet, into this cold reservoir. The isotopes show that these are--it's a cold reservoir. So oil is migrating up into this field right now from a bigger field, probably around 12,000 feet up into a little shallow field around 4,000 feet. That's based on fluid inclusion at temperatures.

Well, the volume. How much rock do we have to deal with? Here's one of my measured sections of the Egan Range. We measured up through the Devonian rocks at the Eureka quartzite, up over the top of the slurry, and then back over the range of the Devonian rocks. This particular section is just one section of rock, one package of rocks all together, 21,000 feet of rock. Now, in the overthrust, is you get a couple thousand feet, you're doing real good. Here we've got 21,000 feet of rock to deal with. It's big. It's huge.

Well, what about structure? Well, I'm going to talk about a little bit of the compressional features, and then the extension of overprint, and what we have, we have the source rock that's been thrust up over the top of a potential--or reservoir rocks that's been thrust up over the top of potential source rocks. On the outcrop you can see a cliff or a little box of rock that's erosional remnants of the Devonian rocks, older rocks that are up over the younger rocks, okay, and this is just a model near Railroad Valley.

The oil at Grant Canyon is probably flowing out of a feature right here, up into a little fault block, and that's what they're producing out of over here in Railroad Valley. The big field has not been drilled yet.

Well, this is a balanced cross section. Now, a balanced cross section is where you honor the geometry of the rocks; the bed lengths, the cut-off angles. You have to put it all together. There's quite a few people that's worked this up on the overthrust belt. We've taken that same technology and brought it down to the overthrust belt of Nevada.

This right here is a Yucca Mountain feature. Yucca Mountain is located right in here, so we have series of thrust imbricates. The fields up in the overthrust belt that produce billions of barrels of oil could be a little wrinkle like that. We have huge structures that we think there's potentially buried below Yucca Mountain.

Now, this is a pre-extension, the way it looks like before it was eroded and extended. Here's what it looks like today. Again, here is the Yucca Mountain area with these thrust imbricates. The source rock that can generate the hydrocarbons is located underneath. Now, this is around 20 to maybe 25,000 feet, so it's fairly deep, and that oil could generate here and migrate up into these possible reservoir rocks. So we have a tremendous potential for a giant oil and gas field, billion barrel-type oil fields in Nevada at Yucca Mountain.

Some of the methods involved in this balancing of the cross sections, first thing, we have to generate a stratigraphic database, and when I was working for Placid Oil, Bunker Hunt turned me loose in the Great Basin and says, "Alan, you can have anything you want," and I says, "I need helicopter and consultant," so I went out and found the old shale hands that had measured a lot of sections of Nevada.

I flew around in a helicopter for months with those guys, looking at all the shale stuff they did back in the fifties, and went back to Bunker Hunt and says, "I need 15 dirt bikes and geologic assistants." So I got a fleet of dirt bikes and bunch of assistants. I didn't know how to use one of those silly things, but I knew how to use the--so I had to learn how to use a dirt bike, and we went out and we bit the rocks. We measured over a million and a half feet of rock.

Now, this is one of my geologists going up over one of these Devonian reefs. We did it right. We didn't go around the outcrop, we went through the outcrops, right through the best outcrops to generate the stratigraphic database. This is just how we got into some of the areas, where we could be careful with the environment. Dirt bikes are much easier than four-wheel drives. We could penetrate the desert a lot easier. Sometimes the desert penetrated us.

Okay, and then the next step, after we had this massive stratigraphic database that's bigger than anything Exxon or Chevron, any of those companies ever generated--it's the biggest stratigraphic database ever put together, we used that stratigraphic database and began to put regional transects together. I personally flew in a helicopter with

Anschutz. My job was take the video camera and record the geology as we flew these great big regional transects. Then we went out with a team of geologists on dirt bikes and mapped strips of geology, re-mapped those. We use the existing theses and geologic maps and any other data we could find. We used gravity data, magnetic data, well data, seismic data, and anything else we could find to put together these regional cross sections, just like Chevron did to find those billion barrel fields up in Canada back in the fifties. We're doing exactly the same thing, using that same technology looking for the thrust belt system of Nevada.

The area I'm zeroing in on for my dissertation here is the Tempahute Range. That's one of the unique areas where you have an east/west exposure of these big thrust belt systems going through.

This is just one of the cross sections through the Railroad Valley area, where we have that flowing field. Here's the--it'd be right on strike with this field here, and they only drilled down to this little teeny--right here to the--just got to the main part of the cake. Just got through a little bit of the icing and got into the cake a little bit. They have not drilled the whole cake yet, and there's some big structures that have not explored, even in the Railroad Valley area. But you can see the structure of the balanced cross sections are very similar to the Wyoming cross sections where you find billion barrel oil fields.

DR. DOMENICO: How did you come up with a balanced cross section?

MR. CHAMBERLAIN: Okay, what we used is stratigraphic data. The question is, is how do we come up with a balanced cross section? We used stratigraphic data generated from outcrop. We used--there are some Paleozoic wells. We used every well we could find. We used regional gravity of magnetics to give us a basement, so we could put the basis of the cross section so we knew where to start with the thing, and then we went ahead and generated the cross sections, using some of the geometry that we saw in the overthrust belt, and brought it down into Nevada. So we used the stratigraphic data on outcrop, mostly, is where the data--because there's not that many wells in Nevada that's penetrated.

DR. DOMENICO: Well, you did use geophysical information to ascertain the basement?

MR. CHAMBERLAIN: Yes, we sure did. Gravity of magnetics. We got regional gravity of magnetic maps of--what we did, we purchased every bit of magnetic data and gravity data we could possibly find, and all the public domain data we could get, and compiled it all and computerized it, and then came up with basement--different levels of magnetic and gravity susceptibilities. We cut different slices to come up with a basement, and these are--some of the experts--I'm not the expert on gravity magnetics. We had some of the experts that work for Amoco came in and joined us to do that part of the work.

DR. ALLEN: But of course, any geologist, including Rick Schweikert (phonetic) would claim there is major strike slip displacement right through your Yucca Mountain cross section, which means there are all sorts of problems trying to create a balanced cross section.

MR. CHAMBERLAIN: Well, yes.

DR. ALLEN: Do you accept strike slip displacement?

MR. CHAMBERLAIN: Oh, absolutely, and when we laid out this cross section, we laid it out purposely to stay away from the strike slip faults, the major strike slip faults. In fact, in my Tempahute area, I have a whole series of them here, and I tried to lay my cross section out, when I did the cross section of the Tempahutes, where I avoided those strike slips. But yes, there are, and you have to be careful with that.

DR. ALLEN: Well, certainly there are in your Yucca Mountain section.

MR. CHAMBERLAIN: What's that?

DR. ALLEN: Your Yucca Mountain section goes right across Crater Flats, and--

MR. CHAMBERLAIN: That's right. We took that in consideration. Yes, we did.

Here's some of the work I did in the Tempahute Range. Again, it had been mis-mapped by the Survey up in here. They had mapped a Pennsylvanian Permian, where it's really Devonian over Mississippian rocks, and the geologic maps were all wrong. You have to go back and look at the maps, and that's where I identified a new thrust, the Money Mountain thrust, which is just a little thrust play.

And what I'm doing in the Tempahute Range, I'm using the stratigraphy of the Devonian to help me sort out the structure. We found a time stratigraphic unit. We found a sedimentary breccia about two to four hundred feet thick, and it's a time stratigraphic unit and we're looking at the rocks immediately above that. Above that breccia over here, we find a big patch reef, a carbonate reef. Over here in the central part of the Tempahutes, we find a 700-foot sandstone bed above that breccia. Over here in the west Tempahutes, I find deep water carbonates. Well, that doesn't make any stratigraphic or depositional reasoning, or it just doesn't fit together stratigraphically until you put in the rest of the structure and restore that.

When you restore these cross sections and take Sections 2 and 3 and put them out to the west where they belong, you have a siliciclastic input coming in over on this side. We have a deep water carbonate over in here, and then we have--we come up onto the shell water, carbonate shell for the reefs over on this side, because reefs and the sandstones don't go together very well, and so we've been able to restore the cross section. We also looked at other units besides that; the Mississippian, the Devonian, the Ordovician, and they all fit together nicely once we restore that. So we have a major compression during the Mesozoic time, where you take all this material and compress it, and put it up into a--wad it up into this feature we see now.

On the field trip where I take my oil company executives, we'll take them out and show them that outcrop. If they don't believe in it, they can go see it, and that's my challenge to anybody that don't believe in the thrust belt. Come and look. I'll show you. It's a neat one to look at. It's brand new. We just found it two years ago.

So what are some of the analogues we can draw in for the Nevada area? The source rock area is like the Cretaceous seaway of Wyoming and Colorado, where it's produced billions of barrels of oil. Well, the Cretaceous seaway and the Mississippian seaway we have out in Nevada is very similar source rock, where this has generated billions of barrels of oil. We think we have the potential of generating billions, and maybe even trillions of barrels of oil in the Mississippian Foreland Basin that was created during the Mississippian time.

Also, we have an analogue of Canadian reefs. The

Canadian reef trend comes right down through Nevada. These produced billions of barrels of oil up in Canada. And also, we have the Utah/Wyoming overthrust, which also contains billion barrel oil fields.

Here's that reef trend coming down from Canada. Here we see those billion barrel fields up in here. Here's the Williston Basin, with tremendous hydrocarbon reserves for America, and again, here we come right down through the Nevada area, so we're right on trend with this reef trend of the western United States or western North America.

The thicknesses are also very similar. We have--if you look at the blue here, which is a similar thickness as the material up in Canada, and so we have the thickness, the right kind of lithology, the same kind of material, the stromatoporoids, amphipora, those are the things that make up the reefs in Canada. They also make up the reefs down here in Nevada.

And then we look at the Wyoming overthrust. Now, these are published cross sections from the Rocky Mountain Geological Association by Paul Lamerson of Chevron, who also does balanced cross sections, and we can see some of the same --similar type of geometry. Here's the Painted Reservoir, billion barrel oil field, with--you can see the structure there, just a little teeny wrinkle. Again, we're talking about a much smaller stratigraphic interval than we have out in Nevada. Again, here's another one of the overthrust things in Wyoming. You can see the little teeny wrinkle here, another--this is over the Yellow Creek field, another multi-million barrel field, maybe close to several hundred million barrel oil field in that part of Wyoming, and you can see the rest of the overthrust geometry. It's very similar to what we see down in--across the Nevada thrust belt system.

So again, here's our pre-extension Yucca Mountain cross section. Here's the Yucca Mountain area. We're working way out across. I couldn't get it all on one slide. Here's the rest of it as you go out across toward the Mormon Mountains. Instead of bringing the basement up, like Brian Warneke would, we have a deep test here. We have Mississippian is at 17,000 feet below the surface, and so it's contrary to popular opinion that the basement is down there a lot farther than has been guessed by other people, and so we have a bunch of thrust imbricates of the Mormon Mountain area. We used seismic data here. We had a bunch of Chevron seismic that we were able to pull in, along with the gravity and magnetic data. So this is the cross section that we were able to construct in that part of the world.

So, again, I've used the Tempahute area, the geometry here, to bring us into the Yucca Mountain cross section that we see down here, and some of the features, some of the size of the features, this is just north of the

Tempahute Range. That's a poor picture. I can't help that, but we see here as entire middle Paleozoic rocks are folded over. Underneath here is the source rock, and we just got--just barely got these analyses back early this week. These are high organic rich shales tucked underneath this great big thrust feature of the Golden Gate Range. The Grant Canyon field or range is just north of here. The Tempahute Range is on a strike to the south of us. These are the kind of features that we see along this thrust belt. They're huge. They have the potential of producing not only billions, but billions and billions of oil, you know. That's a lot of oil.

And so, in conclusion, there is a hydrocarbon potential along this thrust belt trend all the way down through Yucca Mountain, and recommendations, we might--we need to go in and look very carefully at the structural models. We need to drill some wells to test them. We need to look at some of the thermal maturation. I talked to Doug Waples, who does that kind of work, and he's anxious to look at some of the thermal maturation, do some thermal modeling of this kind of geometry, but you have to have a model to work with, and so we have a thrust belt model, or a thrust model to work with down in the Yucca Mountain area. We've put this on the computer. There's a, oh, a $75,000 computer program that was loaded on. We've gone through and checked all the geometry of it and if fits nicely with that, and so we just need to go in and test now the model that we've come up with of the Yucca Mountain area.

So that's what I have to present to you this morning. Any questions?

DR. DEERE: Okay, I think we'll move on to the next, and if you will be here, there may be questions in just a few minutes.

MR. CHAMBERLAIN: Okay. We'll stay here until the break.

DR. DEERE: Okay. At this point, we'll ask Dr. Gene Roseboom, Deputy Assistant Director for Engineering Geology, with the U.S. Geological Survey, to provide us with a brief update on the status of USGS involvement at Yucca Mountain.

DR. ROSEBOOM: My responsibility in the USGS is to keep an eye on this program for the Director, and my normal role at Technical Review Board meetings is to sit back there in the audience and listen to what's going on, and I thought until last week that I would be doing just that, and then received a call from the Technical Review Board, with some questions about some recent newspaper articles regarding the USGS role in the Yucca Mountain project, and so I'd like to try to explain what is going on.

Having completed a reorganization of their own, the Department of Energy has been looking at the management of the rest of the program. In the USGS, DOE sees a need for clearer lines of programmatic authority in the geologic part of the program. DOE has no problems with the hydrologic part of the program. Carl Gertz has indicated that if the USGS cannot make changes that will satisfy him, he will, if necessary, look to replacing the geologic division with contractors.

I'd like to make it clear from the very start of the talk here that the independence and objectivity of USGS studies and reports are not an issue in this matter. It is more a management affair.

Over the past six months, there's been a dialogue going on between DOE and USGS on the management of the program. The geologic part of the program is, to a large extent, run by matrix management. In matrix management, control or management of a program cuts across normal lines of authority of an organization. This is usually done when one needs to assemble an assortment of talents or types of people who are located in different parts of an organization, but you do not want to move them around in the management structure. It's called a matrix because there are two different lines of authority; the normal lines of management, and then the programmatic lines. Thus, the regular managers have to turn over some of their normal authority to a program manager. Of course, this results in divided responsibility, and divided responsibility is generally frowned on, since it becomes harder to pinpoint the trouble when things don't work right.

There can be real benefits to some degree of matrix management, particularly in scientific work. For example, in the USGS, we've found that there are advantages to putting specialists, such as isotope geochemists, geophysicists, paleontologists in separate branches, and then they participate in programs or projects that need them, but remain with their peers. We find that this is better for their long-term professional development, and provides the USGS with greater strength and capabilities in these disciplines.

And so that this is background, let's briefly look at how this program in the USGS differs from other USGS programs, and how these differences affect the management of a program.

The USGS involvement in the Yucca Mountain project, of course, is a direct growth from work in the nuclear weapons testing program which began in the early 1960's, when underground testing became necessary. With the about 15 years of experience that had developed by the mid-seventies, it was suggested, in fact, by the Director of the USGS that the Nevada test site, with its extensive database on the geology, might be a possible source of potential sites for high-level waste repository, and through that, it was a natural that the USGS weapons testing program would develop a component to look at the test site within four repositories.

In addition, there were other activities in the USGS in the high-level nuclear waste business that have gone on for some time; involvement in the salt program, the crystalline rock program, and also, from 1978 to the present, we've had a small, independent research program on aspects of high-level waste.

Okay. What are some differences, then, between the USGS Yucca Mountain program and some other programs? The first one--and I think this is probably the most important one--is the extreme breadth and diversity of subject matter in the earth sciences. This is not always appreciated, but because of this breadth and diversity, many different specialists are needed, and these people are often scattered across an organization. The diversity has especially been growing since the site characterization plan was completed. As I think you know, there are over a hundred study plans planned in there, and approximately half of those are one assigned to the USGS.

A second problem is the start-stop nature of much of the work. Often, typically, a study plan will be prepared, go through the review process, and then there will be a period of waiting until the work can actually be done. Much of the work is phased so that much of this is natural. There are also factors such as stop-work orders stemming from quality assurance that have interrupted the program in the past, and at present, for instance, there are many scientists who are waiting for new boreholes, trenches, and the exploratory shaft facility.

This means that there are a lot of people who are needed only part time, or intermittently in the program, and so that that has to be factored into the management of the program.  This is probably less true on the hydrologic side of the program, because there are, at present, about a dozen ongoing hydrologic monitoring activities going on, and so there are many people who can be involved in those, and then work on additional study plans and other activities, fitting them in.

Of course, another problem--or difference, let's say, not a problem necessarily--is the administrative and quality assurance requirements.  There are lots of planning exercises.  Reporting to DOE crosses normal USGS lines of communication, and this--some of these can be a burden to parts of the organization that have a relatively small component of involvement.

Going back to the problem of the breadth of the subject matter, I just made this up the other day, looking through the lists of programs--of projects, and a similar one could be made for the hydrologic program and the field of hydrology.  It is extremely broad, running from meteorology to surface water to a great thickness in the unsaturated zone, where there's an enormous amount to be learned, down to saturated zone geology, hydrology, plus geochemistry and lots of subjects, but here you can see we range through volcanic rocks, and the volcanic rocks out there are an unusual type; caldera rocks, which are--with welded tuffs and ash flow tuffs. Quaternary deposits include, of course, marsh deposits, fossil spring deposits, aeolian deposits. I won't go through the list. You've just heard a new look at the petroleum resources question, which is one we have really not even begun to look at.

In the area of geophysics, we have exploration geophysics of just about all kinds planned for one stage of the program or another, so I think you can see with all of these subjects, we have a very complex program. It's much broader and more complex than anything else we have in the organization.

The general lines of authority in the USGS start, of course, with the Director, and three main divisions are the National Mapping Division, which makes the quadrangle maps that many of you are familiar with; and then Geologic Division, and Water Resources Division. Most programs are--nearly all programs are in one or the other of these three main divisions.

This program, for a starter, straddles the two divisions here, and, in fact, in terms of the funding, it amounts to about 3 per cent or so of the funding of each of these divisions. There's a total of about--the manpower amounts to a total of about 150 people in that.

Looking now at the organization of the Geologic Division--and I'll give you a blow-up of part of this in a minute so you can read things better--you can see it's a normal organization pattern, with the chief geologist up here, and four offices; Mineral Resources, Regional Geology, Energy and Marine Geology, and then the hazards over here, earthquakes, volcanos, and related engineering hazards. And nearly all of the programs in the Geologic Division come down and fall into one of the--or all of the programs, with the exception now of climate change, which is quite a new one and is also relatively broad, come down and there are lead offices for all of the existing programs.

In the case of Yucca Mountain and its predecessor, the weapons program, the Office of Regional Geology had the lead. We'll look at those offices, the blocks at the bottom there, in a little larger print so you can see what's listed there. The ones I have marked with stars are those that are involved in the present program, and there are a total of approximately--about 12 of the 24 branches are involved to some degree or another. Some of that is relatively small amount of involvement; as an example here, the petroleum geology is simply--is something that's just about to become involved. So that we are already crossing a lot of the organization.

If one were to--this is a sketch I made up to try to explain how the management of this program--the matrix aspects of it fit together. This portion is like the Geologic Division diagram I just showed. I've only put three of the offices up there, and then these are the branches, and I've only shown eleven or so, which would be the number actually involved in the program. The light lines show the normal lines of authority in the Geologic Division. Crossing over here to the Water Resources Division side, the program is relatively simple. There is a technical project officer, Larry Hayes, who answers directly to DOE, to Carl Gertz, and a single line of authority down to an organization about the size of a branch here, and all of the hydrologists--nearly all of the hydrologists in the program are working directly for him in that organization. Occasionally, there is some additional work, such as that done by the Nevada district office, where other parts of the organization have some special talents or background that's needed.

Now, the heavy lines, then, show the programmatic lines that are involved here, the flow of funding and the flow of reporting, and I think you can see, on the water side, there is no problem. It flows straight down to the working scientists at the bottom. There is, in crossing the divisions, in the Geologic Division, there is a lead division, the Office of Regional Geology, which has a staff that duplicates some of the activities carried on here, and organizes the geologic portion of the program.

Within the branches that fall under that office, the funding authority and the line authority coincide. This is the office, I would remind you, where the weapons program originally resided, so that that was the--one of the branches was the original main part of that program. With regard to some of these other offices, though, I think you can see the programmatic lines run across the normal chain of command, and so you are presented with a matrix-type of situation.

What we are trying to do--and we're in the middle of a reorganization now to attempt to simplify this--is to arrive at something like this. This is the current plan, and water side of it would essentially remain the same, with--except that there would be a second unit here set up to handle geologic investigations. This is the part that we looked at that handles the hydrologic part. That would be unchanged.

Within this unit, we would arrange a transfer of a number of the people who are working over in these branches, those who are working full time, in particular, and are willing to make the move, would be transferred over into the Water Resources Division and work under this structure. The transfers would be administrative only. They would not be physical transfers. They would continue to work with their peers, be in offices and associated with their existing branches, because the benefits of close contact like that for review purposes and such, and continuity of their careers is very important. So that would be one part of it. They would simply move over into that group.

Another part of the plan would be to essentially shorten these lines of programmatic control, and deal directly with those individual branches where people were needed part time and for specific pieces of work. So we shorten the lines of authority, and reduce them in number, also.

We also have not put it up here, but at present, there is a group from the Bureau of Reclamation who are working as subcontractors to the hydrologic program, and actually, in the mapping of the shaft and some other activities, as subcontractors to the Geologic Division, and they would work out of here, and some of these activities might well be transferred over to them, but that would not be a change, particularly, in the present arrangement, as they're already a subcontractor.

So this is the general shape of things that we're trying to put together, and when this has been--the plan has been completed, it'll be presented to Carl Gertz, and he can decide whether this will prove acceptable or not. So that's the general picture for what's been going on.

DR. DEERE: Thank you very much. I think that's very helpful.

While you are there, are there any questions from the Board members? Yes?

DR. CARTER: Gene, one question. What are the comparable sizes over some reasonable period of time of the folks you've had involved in the weapons testing for all these years, versus the size of the organization that support the Yucca Mountain project in terms of people as well as budgets?

DR. ROSEBOOM: The weapons testing program was considerably smaller. It was one branch with probably, oh, on the order of 30 people, 30-40 people originally. It did have some--it was augmented by a small group of hydrologists, and, of course, there was also a seismic network operated because of the concerns of nuclear testing triggering seismic activity. So there has always been a certain amount of cross-lines, matrix management because of the--the needs couldn't be isolated to a single group.

DR. ALLEN: Gene, of course, it's not the purpose of this group to advise anybody on managerial problems, and so I think we want to try to stay out of that except insofar as it affects the nature and the quality of the scientific and technical work. This, of course, has been a matter of great controversy, and even bitterness, within the Survey, and I guess at least the rumors that we hear are that many people associated, many Geologic Division people associated with this program--many of whom we've worked with or interfaced with--

are pulling out of the program lock, stock, and barrel.

Can you give us any idea of your evaluation of how this is going to affect the actual scientists working in the program?

DR. ROSEBOOM: Well, there's a wide diversity of views among the scientists, needless to say. There are some that feel that enough of quality assurance and other problems, and they'd be happy to get out. There are others that are very interested in the program and have a great deal of professional attachment to the program, and are quite ready to accept any administrative changes that will be necessary to continue their work in the program.

We are going to sample that viewpoint with--we're in the process of--we're going to question--have questionnaires out to everyone and arrange for as much as they might wish in the way of conferences to clarify any concerns they might have. Also, some cases, we will probably need to encourage some people to continue on a temporary basis until we can make a smooth transition. Whatever happens, that would be necessary.

I don't think I could make a stab at numbers at this point, because a lot of that is--we have a lot of individuals involved.

DR. ALLEN: More specifically, though, people--associate geochemists who have been working, say, on Trench 14, but not as a full-time effort, that's just one of the things they've done, is there any reason why their relationship would be any different under this program? They don't have to transfer to Water Resources in order to continue part-time work on specialized aspects of the program?

DR. ROSEBOOM: In that particular case, I think it's quite clear what's going to happen because, as a group, they are quite ready to transfer administratively to Water Resources Division, so they would answer directly up the chain of command I've shown there. But they would remain right where they are in the present isotope labs, which are operated by a Geologic Division.

DR. ALLEN: Well, many of these people are spending 95 per cent of their time on other projects, and only 5 per cent on this. So do they have to transfer into Water Resources to even spend 5 per cent of their time on it?

DR. ROSEBOOM: No, no. This would--I'm talking about those who are 100 per cent, or very near 100 per cent involvement. There is a fair number that are involved, say, as much as 85 or 90 per cent in the program, and may have something else that's going on; foreign work or something they're finishing up, and so those who are predominantly involved would probably administratively be transferred.

DR. DEERE: Okay. Well, thank you very much. I think we're pleased to see that there is an effort made to sort out the management of the structure, and--yes?

MR. JOHNSON: Carl Johnson with the State of Nevada.

We, in the state, have a lot of concern about the future management and operation of the Southern Nevada Seismic Array. Could you give us some of your thoughts on what the future organization and management of that array is going to be, if you can?

DR. ROSEBOOM: I believe at this point no immediate change is planned in that one. For one thing, the net is in the process of being upgraded, and so that clearly needs to be done by the people that have planned the upgrading, and it will also--it's also been planned that it will be tied into the National Seismic Network, which was formally announced created two weeks ago, for the present, we don't see any change in that network.

DR. DEERE: Okay. Thank you very much.

When Bob Loux was speaking, I had word that Carl Gertz asked if he'd be able to have a few minutes, I guess, to comment on some of your points. Since then, he's heard three other speakers and he may wish to increase the scope of his comments, but Carl, I'll give you the chance.

MR. GERTZ: Thanks, Don. I'm Carl Gertz, DOE's Yucca Mountain Project Manager, and I think I needed to just, once again, reemphasize to the Board and for the record that we're pleased with the state's initiatives to act on our permits, and we have resubmitted them. Certainly, we're answering any questions they may have.

To set the record straight, we believe some of the questions that they've asked weren't asked previously; however, we intend to have questions, all questions answered on the underground injection control permit by Friday, so we hope to move expeditiously in the process with the state so we can get on with the scientific studies.

To allude to one thing that Bob said, insofar as our underground tracers were concerned, in our 1989 submittal, we certainly did have a list of those tracers we wanted to use. We included that in Appendix E. The state indicates that as of April 1st, they wanted some more information on that, and certainly, we'll provide that, as I said, by Friday, but I guess my point is that we are interacting with the state's regulatory agencies, and we hope they can expeditiously work on our permits so we can get on with the scientific studies. That's what, in effect, they've committed to do, and that's what we're committed to do.

A second comment, I'll comment on what the USGS has said. Yeah, there's no doubt, I've asked for some streamlined management, and I think some of the charts that Gene pointed out shows you some of the issues we've had, but certainly, I don't want to at all compromise the scientific independence. I've not asked at all for full-time people, 100 per cent. I recognize the value of matrix management on part-time people. It's just a matter of streamlining it so there is one person in charge at the Survey from my program, and that he's responsible for what's going on. And right now, the Survey has chosen to make that person the TPO, and I just want to see a management structure that supports that TPO.

And I guess I'll do one more aside on the permits. As I said, we are eager to get on with the scientific studies. Just to put things in perspective, there's a gold mine in the area of Beatty, and in the area of water appropriations. They use as much water in three months as we'd use in ten years of the site characterization program, so that puts that in perspective. In the area of air quality permits, they disturb many, many times more surface disturbance area than we will in our ten years of site characterization, and they obtained their clean air permit in three months.

So our issue is, we want to be treated like any other entity in the state, insofar as the permits are going. We recognize our unique position for the isolation of nuclear waste, and that's another issue, and that's part of licensing things. But to get on with the scientific studies, we hope that we're treated like other commercial entities within the state. That's, we believe, what the judge has asked the state to do.

That's all I have. Thank you.

DR. DEERE: Are there questions of the Board members of any of the speakers?

DR. DOMENICO: I have a question to Alan, if I can.

The oil you found so far is in the White River drainage; Railroad Valley in particular, is that correct?

MR. CHAMBERLAIN: Yes. The major oil in Nevada is being produced out of Railroad Valley, and there's another small--well, several million barrels being produced up in Pine Valley north, which is out of that drainage area.

DR. DOMENICO: Anything east of--anything west of the White River drainage?

MR. CHAMBERLAIN: Well, I don't know if Railroad Valley would be called White River drainage or if it'd be a different entity. When I took groundwater here at UNR, it was a different basin at that time. They've changed that.

DR. DOMENICO: It still is. That's why I'm asking.

MR. CHAMBERLAIN: Yeah. So it's a different drainage than the White River Valley. The White River Valley lies out in front of the thrust belt. There's oil shelves in those wells, but that is in front of the leading edge of the thrust, and when I take people on the field trip I take them and show them the leading edge of the thrust on the east side of the Grant Range, on the west side of the White River Valley and show them that.

DR. DOMENICO: Okay. In terms of data, it would seem that with all the boreholes at the test site, both in association with this program and with nuclear testing, did you see any evidence of these massive structures, or weren't they deep enough?

MR. CHAMBERLAIN:  Most of the wells in Nevada, including the ones down on the test site, there's only--for example, I did an analysis one time.  There's only 3 per cent of all the drilling in Nevada has ever drilled Devonian rocks, so you really haven't drilled significant wells in Nevada.  There's a few oil wells that are significant.  The ones that are drilled on the test site are probably not significant, other than they give a little bit of control what's just barely underneath those volcanics.  A few have hit the Paleozoics and can help us get a little bit of stratigraphic control, but we really need to see more drilling.  Same thing with Pine View before it was discovered.

DR. DOMENICO:  Well, isn't there a temperature consideration where liquid hydrocarbon goes to a gas?

MR. CHAMBERLAIN:  Yes.

DR. DOMENICO:  Aren't you really thinking if you're going that deep, that you may have billions of gas, as opposed to oil?

MR. CHAMBERLAIN:  Trillions of cubic feet of gas, yes. There is.  There's a good potential for natural gas, as well as the overthrust belt, where they have natural gas in the overthrust. I had Doug Waples, who's the expert on this kind of stuff--world expert on it--he came to my office when I started my office here in Reno and we went through a lot of the data, and his comment was the thermal maturation data that he was looking at indicated there was probably some thermal reversals and underneath some of these hot plates, there can be some cooler rocks, just like the overthrust belt. So yes, that was a major concern. That's why I spent several hundred thousand dollars doing thermal maturation sampling with--for conodonts, palynomorphs, vitrinite, and a bunch of other stuff. So I have answered that, or worked on that.

DR. DOMENICO: Gas not being as precious as liquids, is that one of the reasons why the oil companies haven't rushed out there and put in an extensive drilling program?

MR. CHAMBERLAIN: Well, that's a different issue. The oil companies, I think, have been led kind of down a different road. Peter mentioned there's two different plays in Nevada. The one play, the first play, like Eagle Spring and Trap Spring, has a little fault block play and that's what people's been playing in Nevada for years and years. No company--other than Shell started doing it back in the fifties and never finished--have gone through and systematically worked the Paleozoic rocks where they could begin putting together regional cross sections. We're just beginning to do that, so that's a brand new thing, and that's why I've got so many oil company executives come on this field trip in May. I had some come last year, and this is all brand new stuff coming out.

On a geologic maps, a lot of the thrusts aren't mapped. You don't see them on the Survey maps. They just haven't recognized them.

DR. DOMENICO: With regard to Yucca itself--this is my last question or point, I guess.

MR. CHAMBERLAIN: Okay.

DR. DOMENICO: You really don't have to go too far into the Paleozoics before you're into a hydrothermal regime.

MR. CHAMBERLAIN: Well, that's correct.

DR. DOMENICO: Do you expect to find any sort of hydrocarbon reservoir in hydrothermal regimes? I'm asking. I don't know.

MR. CHAMBERLAIN: Well, sure. Well, exactly. The oil at Grant Canyon, for example, is very hot oil. It's been heated up, probably in the deeper reservoir, moving up in that colder reservoir, and there is a lot of thermal problems in Nevada, but, for example, at Trap Spring, those rocks are producing out of volcanic rocks. Now, you can't get any hotter than that, but the oil's migrating into those. So it depends what structural plate you're in. There's a lot of complexities and we have to look at each one of those individually.

DR. DOMENICO: It also could be that the White River drainage is of a different thermal regime than the drainage systems further to the west; perhaps cooler.

MR. CHAMBERLAIN: A possibility, but that--again, you're out in front of the thrust there, as I mentioned.

DR. DOMENICO: Yeah, that's why.

MR. CHAMBERLAIN: But the big oil is found behind the leading edge of the thrust. That's where the oil is found. All oil, every drop of oil in Nevada so far, commercial oil, has been produced behind that, over in the higher regime, if you want to call it that, in the thrust belt. But no oil, commercial oil, is found out in front of it in White River Valley.

DR. DOMENICO: Okay. Thank you very much.

MR. CHAMBERLAIN: You bet.

DR. CARTER: Don, could I ask Alan a question?

DR. DEERE: Sure.

DR. CARTER: It seems to me about 35 or 40 years ago, there was a considerable amount of oil exploration in the Las Vegas area, supported by Joe Brown and other business people. What was the experience with that?

MR. CHAMBERLAIN: Okay. A lot of wells around Las Vegas have oil shows in. The big question is--and that's why I briefly touched on it here--is what is the source rock at Las Vegas? And as you looked at the Antler Basin, you're coming up onto that Paleozoic shelf there, and you're running out of source rock, so there's not very much source rock to call on as you drill wells around Las Vegas.

As you go farther west and to the north, you come back into the Antler Basin where the source rocks are. So you have to have the source rocks before you can have hydrocarbons. But there are some shows there. In fact, you can find just west--or east of Las Vegas, you can go up and crack some of those ammonoids out of the Triassic rocks, and you can pour live green oil out of the rocks. I've done that. You can see it, but it's not commercial. There's not enough there to be commercial.

DR. CARTER: Never any commercial operation?

MR. CHAMBERLAIN: No, because the Mississippian source rocks are not there, but they're farther off to the northwest from Las Vegas.

DR. CARTER: Thank you, sir.

MR. CHAMBERLAIN: You bet.

DR. DOMENICO: You've got to have those source rocks, but you're going to have to have that structure before those source rocks pop, or you don't get it. So what's the age of that structure?

MR. CHAMBERLAIN: Okay. Well, the best we can date it, we found--the Newark Canyon formation is the cenerogenic conglomerates associated with the thrusting. That's similar to the Wyoming overthrust belt, and that's been dated as Cretaceous. Some argue if it's early or late Cretaceous, but we've found some places where we actually have Devonian rocks thrust over the Cretaceous, over those Newark Canyon formations. I can take you on the outcrop and show you that in Railroad Valley.

There's other places where the Cretaceous is over the top of it, so it's bracketed by the Cretaceous Newark Canyon formation, saying it's a Mesozoic thrust belt the same --similar age--I don't know if it's exactly the same age, but similar age to the Wyoming overthrust belt.

DR. DEERE: All right. Thank you very much.

Are there any questions from the audience?

DR. BIRCHARD: I'm George Birchard, USNRC. Along the same line, it looks to me like you've done pretty extensive investigation in the northern part of Nevada. What concerns me is you're crossing from--according to Warneke's theories and some other people's theories--from a rather modestly extended terrain where the extension's been spread over a huge number of miles, to what may be a highly extended terrain, and you may be taking your cross section across the boundary from a less extended to a more extended terrain when you come into Yucca Mountain.

I wonder what kind of evidence or data you have to support your position that that structure underlies a number of the basins in that area?

MR. CHAMBERLAIN: Okay. That question came up last week at the National American Association of Petroleum Geologists' national convention down in Dallas, and that very same question came up and a lot of Warneke's followers came over to look at this cross section. I had the whole cross section laid out, and after they looked at it and could see the balancing involved with it, and also the well data that Warneke must not have been aware of--the Grace Petroleum well, for example, penetrated Mississippian rocks at 17,000 feet--that changed the whole perspective of Warneke's model. So he apparently did not use the seismic data, nor the well data that's available for doing that kind of work.

So we took that into consideration, as well as all the outcrop work. He suggested in his thesis, for example, that the Meadow Valley Mountains extended or fell back off the Mormon Mountains. That's not right. The rocks have been compressed from the west to the east, and you can see that on facies changes on balance--or measured sections, and that's where I do a lot of stratigraphic. I asked Brian on a field trip if he measured sections, if he bothers to do that. He doesn't bother to do that, he said, a lot of measured sections. I do. I'm a stratigrapher.

DR. DEERE: All right, thank you.

Well, this was a very enjoyable presentation by all of you. We thank you very much. It's run the rest of our program a little behind, so what I think we'll do is take a ten-minute coffee break so we can get all steamed up, and then we'll get into the symposium, and thank you again, very much.

(Whereupon, a brief recess was taken.)

NATURAL ANALOGUE SESSION

DR. DEERE: Good morning again.

I would now like to turn the gavel over to Dr. Ellis Verink, who is chairman of our panel on engineered barrier system. He will preside over this first session on the use of analogues.

Ellis?

DR. VERINK: Thank you very much, Don.

Let me apologize ahead of time. I seem to have a little throat trouble, so we'll do the best we can. I have a special announcement for those who will be presenters this afternoon. Mrs. Einersen needs to get together with each of you to be sure that your needs so far as projection equipment, et cetera, are accommodated. Would you kindly see her before you go to lunch so that these arrangements can be made?

My name is Ellis Verink. I'm the Chair of the Nuclear Waste Technical Review Board's panel on engineered barriers and engineered barrier systems. The purpose of this meeting is to provide the Board with an opportunity to learn about past and current activities related to the use of natural analogues as a technique to assess the probable performance of a geologic repository.

Because of the broad interest that the Board has in this topic, we've decided to adopt a meeting format which is somewhat different from previous Board meetings. We expect that the presentations and subsequent general discussion will consume the rest of today and a good portion of tomorrow's scheduled time. Now, as Dr. Deere has indicated, I and three of my colleagues on the Nuclear Waste Technical Review Board--Drs. Langmuir, North, and Allen--will share the responsibility of chairing individual sessions.

We expect that a result of these presentations and the subsequent general discussion by the Board and others will be able to address the question of whether or not the study of natural and/or other analogues can be used to reduce uncertainties associated with predicting the expected performance of the proposed repository at Yucca Mountain.

Presentations will include information on studies performed at the Nevada test site, the use of archeological analogues, and various mineral, glass, and metallic systems. For native metal analogues, it also is important to characterize the surrounding the environment as a potential basis for the selection of backfill materials in adjusting the environment around canisters.

The DOE program, which includes cooperative efforts on an international scale, also will be discussed. In addition, representatives of NRC will present their research program and individual staff views on the appropriateness of natural analogues in the licensing arena.

The final portion of the meeting will be devoted to a general discussion among the Board members and the presenters. Would any of the present Board members wish to make any preliminary comments?

Dr. North?

DR. NORTH: I'd like to say a few words on the important relationship, I believe, between analogues and performance assessment. With Yucca Mountain, we're dealing with a very complicated system, and we have a need to predict its performance thousands of years into the future. How well are those charged with making such predictions going to be able to carry out this job, and how convincing are the predictions going to be to scientists and to non-scientists?

I'm concerned about the potential for performance assessment based on computer models and the relatively limited set of data that we've been able to get in planned investigations at the site. I think, ultimately, where our predictions are going to stand or fail to be convincing is the degree to which they reflect insight and understanding about the geological processes involved.

I see analogues as an opportunity to study and increase our understanding of situations where similar materials and processes to the repository have been in place for thousands, perhaps millions of years, and we can add to our understanding with respect to those materials and processes. This may allow us, for example, to be able to carry out validation of the computer models, or it may enable us to be able to back into some data that we haven't been able to get in another way.

It's been my impression that many of the other national programs have placed a great deal of stress on analogues as an avenue of research, and many of their spokesmen, in describing why analogue research is important, have stressed its relationship to performance assessment in similar, and perhaps better chosen words than those I've just used.

So I'm very excited at the prospect of spending the next two days--today and tomorrow--looking at analogues in relation to Yucca Mountain. I think we have a great deal to learn. I think this may be an area in which there is, perhaps, a good deal more that might be done in research and support of Yucca Mountain than what I presently understand to be the case.

DR. VERINK: Thank you. Any other members of the Board with a preamble statement?

(No audible response.)

DR. VERINK: Hearing none, the first presentation will be by Dr. Larry D. Ramspott. Larry is Associated Energy Program Leader at Lawrence Livermore National Laboratory, and in this role he assists in managing nuclear waste research and the development of applications at Lawrence Livermore National Laboratory, and acts as the liaison with DOE and other government agencies and industry. He's been involved in DOE's radioactive waste management program since 1976.

Larry?

DR. RAMSPOTT: I'm speaking today, the topic I chose is underground nuclear explosion in test locations as analogues for a high-level waste repository.

I was looking back--in fact, I gave the Board members this brief bibliography, and I remember that I gave a talk in 1977 at a Geological Society of America meeting, and at that time--with I.R. Borg. It was titled, "Underground Nuclear Tests Below the Water Table as Waste Disposal Pilot Plants." We talked about pilot plants at that time.

I was thinking this morning about, What is an analogue?, and I wondered if a bicycle is an analogue for a passenger automobile, and I thought, really, basically, it's how you define things, because a bicycle is an analogue for a wheeled transportation system of some sort, but not necessarily for an automobile. So I think whether or not it's an analogue, we can maybe, after the talk, it might be a little more evident.

I'm not officially representing the Radionuclide Migration program, which is funded by the weapons program, or OCRWM in this talk. This is work that I've done in the past. I was one of the initiators of the Radionuclide Migration program in 1973 at the test site, and I stayed associated with that through the early eighties, and I led the Livermore work in the Yucca Mountain project for OCRWM from '77 to '88, and I really have no direct association with either of those programs at the present time.

The level of today's talk is an overview of possible applications of data to a Yucca Mountain repository. There's a great deal of information out there, and I think if the Board is really interested in details of this, there are a lot of people that are currently working on the RNM program, or its successor program, that you can get in. I'm not covering other media than tuff. There's been a lot of work in rocks other than tuff, but I'm not looking at any of those. So those are just some background information.

What I intend to come to at the end of the slide--this is sort of the approach of tell you first what the summary and conclusions are, and then get back to it later--I intend to come back to these at the end of the talk, but basically, I hope to be able to show you that the data from the RNM program can support bounding risk assessments for a Yucca Mountain repository, and it can do it, I think, in two areas.

I think you can confirm the theories about equilibrium radioactivity levels in water where you have bare vitrified submersed in groundwater, which is, of course, not necessarily the case at Yucca Mountain, but if we do calculations of that sort. You can also confirm theories about the retardation of radionuclides during flow through saturated tuffaceous rock, I think in those two areas.

I think there is an opportunity existing for the field studies of colloidal migration, which I do not necessarily have a lot of data, but that opportunity does exist, and I think the opportunity exists for field studies of migration of radionuclides from an existing source vertically downward through unsaturated rock. So those are some conclusions that I hope will be substantiated by the talk that I'm going to present right now.

The outline of the presentation, I'm going to give a little bit of background about the phenomenology and history of underground nuclear testing, and general information about the RNM program. Then I'm going to talk specifically about what's called the Cambric experiment and other results. I'm going to say a little bit about what I think the relevance of nuclear test data to repository assessment is, and then go back to the summary and conclusions.

One of the things that I've been thinking as I put together this talk and I had to pull out boxes that have been --I haven't even looked at for a decade, to put together some of this, is there is a lot of perspective on this and a lot of information, and even as I was putting together the talk, I realized that things in my head--I've been associated--I started out with the Plowshare Program in 1967, and so there's a lot that I just assume people know, perhaps. So if you have--I want to urge you, if you have any questions about something that I say, to go ahead and ask it, because I'm not sure I've got it all at a level which--being a geologist, I'm not necessarily going to be speaking to everybody in the audience here.

The purpose of the Radionuclide Migration Program, prior to that program, there were some theoretical risk assessments at the test site, based on geologic and hydrologic data that were available. We're talking now when we started this program in 1973, and even at that time, there were risk assessments that had been done by various agencies. Paul Fenske, who later was here at the Desert Research Institute, was one of the ones who did some of those, and there were others who I may not happen to recall right now. But there was very little information on the distribution of radioactivity and its availability to groundwater at the test site, so the program was started in 1973 to address the relation between that radionuclide inventory, and the groundwater source term for transport calculations. That was really the purpose that we were undertaking.

The idea was that if very little radioactivity gets into groundwater at the source, the potential for migration is going to be limited, and the ultimate aim at that time was to evaluate the potential for off-site migration of radionuclides in water at levels above the Radiation Concentration Guides. That's really what we were looking at at the time. We weren't looking at 40 CFR 191, or what would happen over 10,000 years, but basically, what's going to get off-site above the RCG that would be available to the public.

Now, the history of underground nuclear testing at NTS, the first contained underground nuclear test was Ranier, in September, 1957. The original work, starting in the early fifties, history of testing was that it was above ground. It was air bursts, and Ranier was really the first shot to see if we could contain a test underground and get all of the test data that were necessary so that we could stop putting so much radioactivity into the environment. So if you want to look at it that way, one of its purposes was an environmental-type of a test to see if we could shift from testing above the ground to underground.

Since it was successful, since July, 1962, all NTS tests have been underground; not all of them fully contained, but they have all been underground. In this particular talk, the distribution of tests and radioactivity is summarized as of June, 1975, and the reason I did that is that we put together a compendium of information, which there are not a large number of these copies available, so I've given one set of them to Jack Parry. But basically, it's information pertinent to the migration of radionuclides at NTS. It's a review that Iris Borg, Randy Stone, Harris Levy and I did, and published in 1976, and so I just took the data from that. I haven't updated it since that time. The reference is at the bottom of the view graph.

Now, through 1975, the location of tests with respect to the water table, I'll go through why I think it's important to give this kind of information. The spherical volume, which is created by the underground nuclear explosion, is termed the cavity, and this illustration shows the relation of the cavities to the water table for the number of tests that have been at the test site. And in the left column are all the tests where more than just the bottom hemisphere of the cavity lies below the pre-test water table; in other words, this thing here, which is called the shot point or the working point or various things, where the nuclear device was when it was detonated, that is below the water table for all these, and there are 55 in that configuration.

The next column is where some portion of the cavity, but not the shot point, lies below the water table, but some portion of it, and then these other two columns here are where you have successively greater distance from the water table. And when we started looking at this, the distinction is made because we assumed that the tests where the cavity lay above the water table were isolated from dissolution and transport processes, and in the RNM program, we focused our attention on tests below the water table, on the premise that the tests like this would be where the water was most available for dissolution and transport, because this was really a safety study, more than a--

DR. CARTER: Larry, can I ask you one question about that slide?

DR. RAMSPOTT: Sure.

DR. CARTER: Is the 75 meters, is that possibly related to communication of the cavity to the water table, or the water in the water table, below the water table?

DR. RAMSPOTT: I should be able to answer that, because I made up this view graph more than ten years ago, but I don't remember why. It was one that was easy to pull out of the database, and I think it was at shorter distances you can have material going along the fracture, but I don't know why I chose specifically 75 meters at the present time. That's an embarrassing question. I should be able to answer it.

DR. CARTER: I'll try to do better next time.

(Laughter.)

DR. RAMSPOTT: This is an old diagram, an even older one from the Plowshare program, and it shows the time history of formation of a cavity, and also shows the final configuration, which has been called a chimney. There's a very large amount of unclassified literature which was generated by the Plowshare program, and it's summarized in this book on nuclear explosion phenomenology, the Borg, et al. report. Even I call it the Borg, et al. report, even though I'm one of the authors.

Chimney formation can occur from minutes to hours to days to years, and it depends upon the rock properties at the explosion site. Now, with weak rock, you get collapse of material into the cavity as soon as the gas pressure in the cavity declines sufficiently for that to operate, but with strong rock, you can get a key block effect that holds this rock in place, and you can keep collapse from occurring for years.

Now, the distribution of radioactivity in this chimney is somewhat dependent on the time of cavity collapse and on when the chimney forms. I'll just go on with that. Basically, radionuclides are fractionated within that cavity chimney system by their volatility, and this actually is a view graph showing the kind of thing that might happen at Yucca Flat. In hard rock, you'd have packing of the collapsed rubble, and that was shown on the previous view graph. That was more of a hard rock-type of test where it ultimately fills up, because the hard rock, as it falls in, the packing ratio is such that the void gets distributed down through here and then it can't grow anymore toward the surface, so it can go five or six cavity diameters, and maybe that's a maximum.

Whereas, with the softer rock that you get in Yucca Flat in the alluvium, you get the material repacked as it falls in, so you can translate quite a bit of void volume toward the surface, and those of you who have toured the test site have seen these collapse craters that form all over Yucca Flat.

Now, within the chimney, there is a distribution of radionuclides.  The melt itself contains high boiling point refractory materials, such as the rare earths and zirconium, the alkaline earths, and plutonium, but the chimney will contain lower boiling point materials, such as the alkaline metals, ruthenium, uranium, antimony, tellurium, and iodine, and it will also contain materials such as strontium-90, which we'd expect to be down in here, but that's because it's a decay daughter in the decay chain of gaseous or other low boiling materials.  So basically, whether it's distributed here or there depends upon the whole decay chains and the origin of the things.

While I have this view graph on the screen, I'd like to point out there are three ways to obtain data from one of these chimneys. You can do some kind of mine reentry. You could mine down and mine over. You could do slant-drilling reentry, or you can set up a drill rig right in the bottom of the collapsed crater here, and you can just drill right down the middle, and all three of those have been done at one time or another.

My next view graph is an example of slant drilling from the surface. That shows the Starwort test, which we actually attempted in 1972, before the RNM program formally started in '73, and we came over away from the collapsed crater. Actually, there is a crater here, not as shown on the sketch there. We drilled a slant hole down and then completed it on down through here.

I'd point out that this is a very difficult undertaking. It's due to you working in a slant hole in unstable ground at great depth. At least, this is what we consider great depth in this kind of rock. I realize that oil wells are drilled down to, you know, in excess of 20,000 feet, but they're not drilled on a slant, and they're not drilled in unstable ground, and there are different issues. So this is difficult types of drilling.

Once you get the drill hole here established and see whether you're really going to be able to work in this area, then you have to wait until refill occurs. You remember that all this stuff is very hot. It's been molten rock. If water comes in here, then it boils the water and drives it off, and it takes a long time for refill to occur and re-establish the pre-shot water table that you had, and you don't want to start taking samples until you've done that. So we typically have put these holes in, gone ahead, taken samples, gotten background information, and then left them sit there. Well, what happens, this particular hole was sheared by a nearby nuclear test before we could gain any useful information, and we've had this happen a number of times; either--sometimes not with any evident test, or sometimes we just lose the hole for one reason or another. So it isn't simple and easy to gain information in this way.

This is an example of mine re-entry at the original underground nuclear test, which is Ranier, and I want to point out that mining was facilitated by the fact that the test was above the water table, like Yucca Mountain, and re-entry was from the side of the mesa. This is all these things like G-Tunnel. We could go back in.

The lighter material at the bottom of the picture here is original rock material, although it's been displaced by the explosion. This material which you see right here is nuclear melt glass, and then these lighter-colored materials up here are blocks that have fallen into the cavity during the collapse.

Just very quickly, this is a picture of melt glass from a nuclear explosion. You can note the spatula here, and the ruler, which is in inches, for scale. Note that there's crystalline material, little blocks of crystalline material scattered throughout the glass, and also note that it's quite porous. So this does not look like the glass that you would get, necessarily, from a commercial reprocessing.

The next view graph I have is a comparison of properties of vitrified high-level reactor waste and melt glass from nuclear explosions. This is a redo or recopy of an old view graph from the late seventies, and I'm not sure that the properties for borosilicate glass here are the ones that exactly are going to go into the Savannah River or, for example, the ones that are used in France, but this is fairly representative of the range of properties at the time. I'm not going to go through property-by-property, but I'd point out that there are apparently large differences here in density and $SiO_2$ content and porosity, and so forth, and also in the radionuclide contents of the various glasses, and the devitrification.

These differences may or may not be significant. I think it depends on circumstances and we have to look at some leach data, but there are apparent differences here. I'd also like to point out here, before leaving it, that the commercial waste will be in a--at that time, we thought double steel wall container. We still don't know what it is. It's probably a double wall container. There was absolutely no container here. This glass sits right there in the water without anything around it. So these are differences. I think the radionuclide content is something I'll come back to. I'll just mention here you can see that there is quite a bit of difference, and particularly for the fission products, because a very high proportion of them escape from the melt, and the question is mentioned a little bit later, is that really significant in trying to look at it from an analogue viewpoint?

I dug some of these data up from reports that were done in the early eighties to do a comparison of leach rate data. I'm not sure that there actually is a specific publication that does compare them, but I did take these three reports, which are available, and look up the information. I need to do some qualification. The range of results in the references is much larger than what you see here, but the numbers I gave here in this view graph are for highly soluble elements where precipitation or absorption would not, or should not affect the results. I think these elements were ones that would reasonably be expected to be dissolving at a rate which would be similar to the glass itself.

Now, these NTS glass experiments were run up to 420 days, and that would be down two more orders of magnitude. It would be $10^{-6}$ if you went out to 420 days. I didn't show that because I wanted to get comparable data. These are all about in the two-month time range. The higher rates that you see here are two months. These are the rates that you initially get in a matter of the first few days.

They're all tests at room temperature conditions. This DWPF test was a static test, and these two were a flow-through type of test in a flow-through apparatus which we originally designed for the NTS glass. If you really want to make detailed comparisons, you can look the material up in the reference, but the conclusion that I draw from this is that, generally speaking, the leach rate data for the two types of glasses, despite the large differences that you could see in the previous view graph, the leach rate data are approximately comparable.

This view graph gives a summary of material which is in the Borg, et al. report. I'm going to talk about RNM studies in each of the three test areas. There really are three major test areas at the Nevada test site; the whole Yucca Flat area here, and the Pahute Mesa test area are active ones. This Frenchman Flat area is an older, inactive test area right now. There have only been three tests below the water table here. There have been 22 in Pahute Mesa and 53 in the Yucca Flat area. As I said, this is through 1975. There are different numbers of tests available at the present time. Here and here they've increased. There have been no more tests down here, but I was trying to get some basic information so that we could begin to look at what the situation was with respect to off-site migration.

You might say, well, why, with all the radiation concentrated here and here, why did you go down and do a test down in this area? In fact, this was the first RNM test that we did. That was because the test--the water table here is comparatively shallow. Here, it's like at Yucca Mountain. It's 1800, 2,000, 2200 feet; the same up on Pahute Mesa. Here, the water table is within, some places, 500 feet of the surface, and so we could get a reasonably shallow test that was below the water table, and it would be easier drilling and a little bit less expensive.

Also, there was a test not only with shallow, below the water table, but it had a well-defined tritium source, and it was in a hydrologic environment where there was very little natural flow. The water table over a very wide range here is fairly flat, so there's little natural flow and we could induce the flow there.

The overall groundwater motion direction here is from north to south, however, and there are water wells down in this area for the Mercury camp, and so we were also interested in whether there would be any migration down toward those water supply wells. I would like to point out this blue line here, and I'm sure that Ike can tell me what the more recent interpretations are, but for a very long period of time it's been believed that the test site really divides into an eastern and a western groundwater areas, and that the flow here and here are fairly separate, and also, that the main way that we were concerned with here is down into a deep underlying carbonate aquifer, and then out, and ultimately, on down to a spring discharge to the south, and here it's in--stays totally, as far as we know, within the volcanic rocks, and not into kind of deep underground stuff.

Fortunately, you have an expert on that, the next person who's going to be talking. He can give you any information you need.

I'll give you some wrap-up type of stuff as far as the overall information that we've gotten. As I said, there have been three sites that we feel we have successfully reentered. This one is in Frenchman Flat. This is Pahute Mesa, and this is Yucca Flat. We've lost one experiment at Yucca Flat and one at Pahute Mesa. There is another experiment called the Bullion test, which has been prepared for re-entry, but it can't be tested until refill occurs. And then there have been a number of other places that we've looked at for a variety of reasons, but not ones where you had the full cavity below the water table.

The interesting thing about all of these is that only a few radionuclides are right there in the cavity itself, in the groundwater above RCG. Cambric, it's only tritium and strontium-90; Cheshire, only tritium and krypton-85; and only tritium at Bilby. Now, with this, we lost the cavity access. This is one of the holes that we drilled straight down instead of from the side, but we've retained chimney access. So this is up in the chimney, not down in the cavity itself.

There are very few radionuclides. Actually, right there in contact, or very close to contact with the melt glass do you have radionuclides in the water above MPC.

DR. NORTH: Could you remind us what RCG is?

DR. RAMSPOTT: That's the Radiation Concentration Guides.

DR. NORTH: So that's not a detection limit?

DR. RAMSPOTT: No, that's either for effluent to an unrestricted area, or it's for drinking water.

DR. NORTH: Do you have the data for how many you can observe above the detection limit?

DR. RAMSPOTT: Yeah. I'll show you some view graphs later on that actually lists the radionuclides.

DR. DOMENICO: So you've found some above the detection limit. You're reporting only those above the health standard?

DR. RAMSPOTT: Oh, right; yes.

DR. DOMENICO: But that's interesting. We know tritium is not retarded, but is there anything about leach rates with tritium that would make them leach a little bit faster than some other things?

DR. RAMSPOTT: Well, by and large, most of the tritium is not in the melt glass. The glass--in fact, these melt glasses, because they're formed at a very high pressure, have more water than--well, actually, there is no water whatsoever in the commercial borosilicate glass, but these have a fair amount of water for natural glasses. It's a few per cent, but even so, compared with most of the tritium, it's out in the cavity itself. It's in the gaseous phase, and then condenses.

DR. CARTER: Larry, what's the theory for finding strontium-90 contrasted to cesium-137, or something like that?

DR. RAMSPOTT: It may be that the cesium more easily sorbs. I really don't know. We have not taken these data and gone back and looked at them in the way that Warner would like to have them looked at, from his speech. We have not compared that with theory in every case. There have been isolated cases where we wrote that paper in Science, that we talked about the migration of ruthenium-106, and we used EQ 3/6 and did calculations and looked at speciation, and we compared that in theory, but I don't think that we've actually looked at the nuclide-by-nuclide as to whether or not it should be in the water.

DR. CARTER: Well, I presume you looked at carbon-14?

DR. RAMSPOTT: By the way, there is cesium-137 there, but it's just not above the RCG.

DR. CARTER: What about carbon-14?

DR. RAMSPOTT: We didn't look for carbon-14. I should say that most of the things that have been looked at here, are things that can easily be done by gamma analysis.

DR. CARTER: Not strontium-90.

DR. RAMSPOTT: Well, no. We had to specifically look for that.

DR. LANGMUIR: Larry, these depths don't tell you the distance away from the device that you found the nuclides?

DR. RAMSPOTT: No, no. This is the shot depth here.

DR. LANGMUIR: So how far away are these analyses taken?

DR. RAMSPOTT: Oh, they're very close to the shot depth. It's this kind of a--if I can find it--it's this kind of situation, or I'll go forward and talk about another talk here, but basically we've come down and taken water samples right down in this area. Those are the kinds of things we're talking about.

DR. NORTH: So this is water that's right up against the melt glass?

DR. RAMSPOTT: Right. In the case of Bilby, it's a hole going down the middle and it's up in the chimney somewhere, but the other two are right down in this area.

DR. LANGMUIR: You concentrated on the saturated zone. Was there any work done at all on unsaturated analyses for radionuclides, above in the unsaturated zone?

DR. RAMSPOTT: Not a great deal. One of the recommendations--we made a number of recommendations when we put this report together, and one of the recommendations is that we verify the isolation of the unsaturated zone deposition from the water table, and I believe that the Desert Research Institute and the USGS have looked at a number of studies of recharge and whether or not things get down to the level of the radionuclides, but I don't think anybody has gone underneath and checked that, to my knowledge.

This speaks specifically about the Cambric test. That's the one that's been most extensively carried out and most information is available for. The test was detonated in May, '65, at NTS. It was a small, three-quarter of a kiloton high explosive--

DR. NORTH: That's extremely small, relative to some of the others, isn't it?

DR. RAMSPOTT: Yes, it is. Six grams residual tritium, and the cavity water samples were first taken in '74, late '74, so that basically there was a little less than a ten-year lag time, and it was one that we knew the source pretty well for.

DR. NORTH: With a test that small, was the yield of radionuclides abnormal relative to that from a larger test, and was this a particular design such that this spectrum of radionuclides would be very different from what you might expect from one of a larger size?

DR. RAMSPOTT: Well, I don't know the answer to your question, and if I did, I couldn't tell you. That's classified, so... But I will say that in all of my talking with the people, the radiochemists and others, they've never said anything about this having any significantly different yield than anything else. But details like that, I think, I can't get into.

The Cambric site cross section, this was the emplacement hole. This is the cavity area, and we think the chimney. This is sort of an estimated growth of the chimney, but we really don't know where it went up here. We came over and drilled a--well, actually, we drilled an observation well over here first, and then we drilled the hole down through here. We drilled below, cased it off, tried to clean it out as well as we could, then we left an open hole and we went back and we pumped. We pumped through a series of zones here, sealed this off, perforations, and so forth; took a lot of samples and tried to get a background here, and then we started pumping over in the satellite well, so I'll give some information and data from that.

DR. LANGMUIR: The fact that you can drill these so routinely implies that it's safe to drill them; that you're not going to get in trouble going down there with a hole and taking those materials out. Those risks were presumably assessed?

DR. RAMSPOTT: Yeah. We have blowout preventers and ways of keeping the materials. In fact, this has been a standard kind of a technique at the test site for decades.

Here are some of the activity levels; tritium, $^{90}$Sr, $^{106}$Ru here, and you can see the concentration guides that I was speaking about. These are old view graphs and I looked at other old view graphs. I believe the concentration guide here is not the EPA one, but DOE Order 54-something, or it's one of the DOE Order concentration guides. They are sometimes similar and sometimes a little bit different, but DOE Order 5400.5, I believe, but these are the concentration guides.

You can see that tritium and $^{90}$Sr are higher than the concentration guide, but you can see that, for example, $^{106}$Ru is lower. I believe these values--sometimes they were taken back for shot time and sometimes they were at the time of--oh, that says--excuse me--says at the time of sampling there.

DR. CARTER: Larry, I presume that those units for the drinking water concentrations are in the same units as your column heading?

DR. RAMSPOTT: Yes, they are. That's right.

DR. CARTER: It's a little bit misleading, since tritium would normally be considerably higher than the others.

DR. RAMSPOTT: They're in the same units. In fact, trying to look through all this data, six different types of units, and compare them back and forth is a little bit difficult.

Again, you have three others, $^{125}Sb$, $^{137}Cs$, $^{239}Pu$. You can again see what we have here. The below cavity type of stuff is background, and you get these levels, which are well below the concentration guides. Now, this information is in a variety of publications which are in the handout that I gave to you. There's a lot of different information. I'm just trying to run through it very quickly here, and scratch the surface.

DR. NORTH: But, for example, that's telling us on the plutonium, you actually could see some in the water and you could see a difference between the upper cavity and the lower cavity?

DR. RAMSPOTT: Oh, right; definitely. Remember, this was right back in the cavity, and up in the chimney.

DR. LANGMUIR: Was there any attempt to see if this was colloidal, as well as dissolved, or was it simply a total analysis?

DR. RAMSPOTT: We were not at that level of sophistication. This was done in 1975, and so we were not looking, at that time, for that kind of thing. I think more recent work, work at Cheshire and others, they've tried to look at whether or not it was in colloidal form. In fact, I know there's been work on some of the materials from a viewpoint of colloids in the Cheshire test work that's been done.

To summarize that, most activity was still confined to the region of explosion cavity at the first reentry ten years after the test. That's not evident from the data I showed you, but we think that most of the tritium at that time was still in the cavity region. There was no activity above background found in that water from 50 meters below the cavity. Water from the bottom of the cavity contained these various materials, and I showed you data from some of them, but not all of them. Only the tritium and the strontium-90 were found at levels above RCG for drinking water, and after 16 years of pumping, which is data I did not show, at the satellite well, concentrations in the cavity have decreased to levels which are barely above the detection limits. In fact, I think there's only one radionuclide right now that's above the detection limit, and my memory says it's cesium-137.

DR. DOMENICO: Is that satellite well the offset well in your figure; what is called RNM-2S?

DR. RAMSPOTT: That's this one, 91 meters away.

DR. DOMENICO: What happened? Why did pumping there reduce the level in the chamber?

DR. RAMSPOTT: Well, we think it's sweeping out the radionuclides that were originally here, and they haven't replenished into the water table.

DR. DOMENICO: But you haven't picked them up in the discharge of the satellite well?

DR. RAMSPOTT: No, we have not.

DR. DOMENICO: You've moved them.

DR. RAMSPOTT: We've moved them, right.

DR. NORTH: So you're pumping, in this case, a lot of water through that cavity?

DR. RAMSPOTT: Yes, we are. I will give some more information about that.

DR. NORTH: I think it would be very interesting to have a similar situation where the water has been allowed to stand all that time in the cavity, and see what sorts of concentrations one might build up.

DR. RAMSPOTT: Right.

DR. LANGMUIR: You'll give us some information on volumes of water that you've pumped at some later point here?

DR. RAMSPOTT: I will give you a little bit of information. In fact, I have just a couple of view graphs along. This one, the Cambric satellite well was 90 or 91 meters from the source. It's pumped nearly continuously from '74 to '91. There's been four billion gallons, and 92 per cent of the tritium source has been pumped through the well.

DR. LANGMUIR: That's a calculation you've made based on the magnitude?

DR. RAMSPOTT: Based on the original amount of tritium.

DR. DOMENICO: Did you measure tritium in the discharge of that well?

DR. RAMSPOTT: Yes. I'm getting really good questions here.

DR. DOMENICO: You need another straight man here.

(Laughter.)

DR. RAMSPOTT: I'm trying to get through in my allotted 45 minutes, so I can come back to some of these if you want.

Tritium concentration versus the volume pumped for the Cambric satellite well, basically, this is the tritium, and you can see it's a very beautiful curve. This represents 92 per cent. Actually, I think this curve--this is Los Alamos data. I want to give credit to Los Alamos. They've been doing this faithfully for years, and I took this out of one of the more recent Los Alamos annual reports, but I think that the figure that I gave you of the 92 per cent and the four billion gallons is, I think it's on down here by now, something like that. So we've come farther on down on this tail.

DR. CANTLON: Now, in this well, the chimney did not break all the way through to the top, or did it?

DR. RAMSPOTT: In this site, the chimney did not break all the way through to the top, that's correct.

DR. CANTLON: Because in some of those, there was tritium picked up in the transpiration water out of desert shrubs.

DR. RAMSPOTT: Right. As far as we know, all the tritium was pretty well capped and kept down in this particular site.

DR. BIRCHARD: Well, that isn't corrected for decay. What time did it take to do that? I said that it wasn't corrected for radionuclide decay. How long did the groundwater pumping continue?

DR. RAMSPOTT: It's been pumped nearly continuously from '74 to '91, so basically, that's coming on 17 years now, so that's the kind of time frame. There are--in the Los Alamos report, they have this same curve, and it looks almost exactly the same versus time as versus volume pumped. I just chose to use the volume pumped curve, but there is a curve for versus time.

And what they tend to do and what you have to do when you read all this stuff, is they tend to decay correct back to the time of the nuclear explosion at the start, and so that you're looking at--not looking at decay as a function in these types of things, and you have to read all the literature very carefully to see what it is exactly that's being put there.

So the satellite well results, basically, we've got tritium, Cl-36. I think somebody asked the question of what else has gotten over there; Kr-85, Ru-106, and I-125 have been observed, but of these, only the tritium ever exceeded the RCG for effluent water in an uncontrolled area. I think it exceeded that by a small amount for about seven years, something like that. But other than that, everything was below the guides.

DR. LANGMUIR: These are all gaseous elements, essentially, except perhaps the ruthenium?

DR. RAMSPOTT: Well, this migrated as an oxyanion. I don't know in what form that migrated, and this is an anionic thing, and Los Alamos has a number of papers on anion exclusion; in other words, there's some evidence that the Cl-36 is actually moving slightly ahead of the tritium, but as you know, this is an extremely difficult, complicated, and expensive kind of testing to do, and so it has been done in some archival samples, but what we were really monitoring when we originally did the test was the tritium, and then work on Kr-85.

All these things, other than the tritium, we had to look for very, very hard, and the ruthenium-106, since it has a one year half-life and we didn't start the test until nearly ten years after the test, we were already nearly ten half-lives down, and so you can't find any of this anymore in any of the samples.

DR. DOMENICO: Ruthenium is the only potential one that could be retarded up there, though. The other ones are--so how do you--why do you find it?

DR. RAMSPOTT: Because of the--there's several papers that we've written about that. Basically, the speciation and Eh and pH, if you do want an Eh/pH diagram and look at the speciation, you're in a field right there where it migrates as an oxyanion, and there's a very small stability field and that stuff simply will not precipitate out or sorb on anything.

We have thought that this is a--the interesting thing about this is--to the commercial program, is that that may be an analogue for technetium-99, but we have looked in this well for technetium-99 and not been able to find it. But it may be too much of a dilution.

DR. LANGMUIR: The krypton's going to be gas.

DR. RAMSPOTT: Yes.

DR. LANGMUIR: So presumably, some of these things could have gone off as gases before you ever got to look at them at the test well.

DR. RAMSPOTT: Well, Los Alamos took specific samples where they sealed them and took them back to the laboratory in sealed sources. They always use krypton-85. They had to seal and then open them in the laboratory and analyze them. So it isn't just simple to just send off a sample to a friendly analytical lab and get all these results.

I'll give you a brief summary of some other RNM results. At Cheshire, only tritium and Kr-85 were above RCG for uncontrolled effluent water in the hottest samples obtained, and I say hottest samples obtained. There's some question as to whether we actually really had communication directly with the cavity. We were in very good communication with the cavity at Cambric, but here, we may not have been. There's still arguments about that, but still, in the water that we were looking at, the interesting--

DR. NORTH: How big was Cheshire?

DR. RAMSPOTT: Oh, it's one of these tests up on Pahute Mesa, which is--I should have written that down, but it's one of these things with a low intermediate yield. It's a couple hundred kilotons or something like that.

DR. NORTH: Okay, as opposed to three-quarters of a kiloton?

DR. RAMSPOTT: Right.

DR. NORTH: So it's much bigger?

DR. RAMSPOTT: It's much, much bigger. It's in that general range. I don't remember what the official is, but it's something like that.

The interesting thing here is that over a period of time, and without pumping, the decay-corrected concentrations of these radionuclides have decreased in the Cheshire chimney where we think we have connected and are making samples, and they are now present in an observation hole 300 meters away, and so there's an apparent natural migration. In fact, the observation hole was put in, hopefully, to be down gradient so that we could pick it up.

Now, there's been an explicit search for Tc-99, and it detected it at Cambric, Cheshire, Bilby, and Faultless, orders of magnitude below the RCG for drinking water, but we did not detect it at Cambric in the pumping well, the satellite well. We only detected it in samples back from the cavity itself.

DR. DOMENICO: Are these all in tuff?

DR. RAMSPOTT: Cheshire is in tuff. The Cambric is in tuffaceous alluvium. Most of the fragments in the alluvium are tuff, so it has the same general chemistry. Bilby is in an alluvium which probably has a pretty fair amount of tuff in it. Faultless is in tuff. So they're all in tuff. I didn't talk about some of the other tests that we have done in limestone.

The other interesting thing at Cheshire is that Mn, Co, Ce, Cs, and Eu fission isotopes are associated with colloidal material at Cheshire. I say associated with. I was told by my technical colleagues not to say that they migrated as colloidal material. We don't know that. We haven't established that we haven't somehow changed it from being in solution to a colloid in the process of sampling. What we've done in the well, we have never really been able to pump this well. We're having permitting problems at Cheshire in terms of being able to pump, and it's difficult to get the information.

DR. DOMENICO: The day you picked those up at that observation hole is--that's where you found those; that 300 meter observation hole?

DR. RAMSPOTT: I think that's the case. There's several papers that are referred to--

DR. DOMENICO: When you see dock 2--

DR. RAMSPOTT: --by Buddemeier and others in the handout that I gave you, but I believe this is in the observation well.

And there's been a lot of work here in taking the water and putting it through a whole series of different filters to get it out the various size ranges, to make sure that it really is a colloid. So there's several papers on that background.

DR. LANGMUIR: That's recent work?

DR. RAMSPOTT: Yeah. That one on applied geochemistry, Buddemeier and Hunt, "Transport of Colloidal Contaminants in Groundwater: Radionuclide Migration at the Nevada Test Site," the Applied Geochemistry, that's one that has that information in it.

Okay. What is the relevance, then, of this RNM program to a potential repository site at Yucca Mountain? Because I think that's really--that's why you asked me to come here and speak. I think the RNM program has focused on sites below the water table; whereas, the proposed repository is unsaturated.

Tests in the RNM program, tests above the water table have been assumed to be isolated from aquifers where a flow occurs, and that's based on the great depth to the saturated zones, the low rainfall at NTS, the distribution of caliche, bomb-pulse tritium, other evidence against vertical recharge. So basically, there's been this assumption that it doesn't get down there, and we've really focused on what's already in the water table itself.

Direct measurements below a cavity have not been made, and you know, you see these view graphs after you make them. I think what I have to point out here, direct measurements in unsaturated rock below a cavity. We've made a number of measurements below a cavity, or several in the saturated zone. What I'm saying is to go into one of these cavities that's been in the unsaturated zone and see if there's been migration downward, we haven't made, to my knowledge, any direct measurements of that sort.

The opportunity exists to make measurements, I think, that are relevant to the Yucca Mountain situation, to go in and look at some of these tests in tuff that are above the water table. They haven't been done.

Nuclear explosion melt glass is a waste form. It's about the same as borosilicate glass. You can judge that from the view graph that I showed you earlier, but it's not a good analogue for spent reactor fuel, in my viewpoint.

Then the question is: Are the observed low levels of activity in groundwater due to the low radionuclide concentrations in the melt glass? You remember, I showed you a view graph earlier that showed that the concentrations are significantly lower. I think what you could say about that is the concentration of actinides in groundwater appears to be solubility limited. Of course, there are arguments going on about that, but in that case, the concentration in the glass may be irrelevant. The concentrations in the glass are high enough to still be solubility limited.

Many fission products are deposited outside the melt glass, so a lot of that stuff gets up into the chimney and is not in the glass at all. The low concentration of radionuclides in the groundwater may be due, in part, to sorption on the rock, and so it's difficult to answer this question about what's in the melt glass or not, because you have this sorption issue, and so you're getting sort of a lumped parameter. You get the final result, but you don't know which of a half dozen processes is causing the low concentration.

DR. CANTLON: Have you looked at the rocks pulled out in the drill core for that?

DR. RAMSPOTT: Oh, yeah. What we did is took sidewall cores and sometimes direct coring, and we've analyzed what's in the core. I didn't go through all that process, but they've analyzed what's in the core, analyzed what's in the water, ratioed those back and forth, tried to look at things which were comparable to Kd's, and so forth.

DR. LANGMUIR: That's published, Larry, somewhere?

DR. RAMSPOTT: Yes. The main study on the initial Cambric cavity reentry is in that list. It's a report; Hoffman, Stone & Dudley, "Radioactivity in the Underground Environment of the Cambric Nuclear Explosion at the Nevada Test Site." That's a 1977 publication, but there are others in there.

Okay, to come back to the summary and conclusions, I think that you could say the data from the RNM program can support bounding--I think the word "bounding" risk assessments for a Yucca Mountain repository. I think you can confirm theories about equilibrium radioactivity levels in water where bare vitrified waste is submersed in the groundwater; in other words, you can look at this idea of solubility limitations. You can confirm theories about the retardation of radionuclides during flow through saturated tuffaceous rock, and by the way, when I was thinking about this, I'm assuming that people are aware of the fact that all of the tuffaceous groundwater here is fairly similar and it's a bicarbonate groundwater. It's oxidizing. The composition is fairly similar, so geochemically, what you're seeing in these tuffaceous rocks is not all that different from what might be out there underneath Yucca Mountain.

The opportunity exists for field studies of colloidal migration, and I think you can look at some of the work that's there. We actually can look at those samples and see if they're migrating as colloids, and then I think, also, the opportunity exists for field studies of migration of radionuclides from an existing source vertically downward through unsaturated rock, but these are what I would see as opportunities, not necessarily work that has been done.

Thank you, and I apologize for taking a little longer than...

DR. CARTER: Could I ask you a couple of questions, Larry? One, is it still necessary for all underground tests that the melt be sampled for confirmation of the yield, and so forth?

DR. RAMSPOTT: Yes. A sample is taken at every test. That's right.

DR. CARTER: The other thing, I wonder if you could generally describe where the groundwater wells are that provide the potable water for use at the test site; like for Well 6 in Frenchman, and so forth?

DR. RAMSPOTT: Ike, can you help me with that? I know that--in fact, I'm not sure I have a good map to show it on, but I can put this one on. The main water supply well for

Mercury, Wells 5-A and 5-B, and they're right down in this area, and then Well J-13 actually is the water--you hear about that all the time being used for the Yucca Mountain project over here, and I think the J-13 well is about in this area. It was a water supply well for the activities over in Jackass Flats. I don't remember where the water supplies are for the camps up here and others, do you?

DR. WINOGRAD: Northernmost Yucca, Well 2 in Area 12, east of Area 12. About there. And then Army Well 1, southwest of Mercury, pumped back to Mercury along the highway.

DR. CARTER: Okay, thank you.

DR. LANGMUIR: Larry, you raised a point that maybe you're not the one to answer, but perhaps someone in the audience would be. You mentioned the Hoffman, Stone & Dudley paper in which some Kd values had been derived from field measurements. I'm wondering if, for certain radionuclides, where it's relevant, such data has been compared to previous work by DOE in the lab, the tabulated Kd data that's going to be used for performance assessment? Has there been comparison work done on that? Perhaps that's for Julie Canepa or someone else in the audience.

DR. RAMSPOTT: I believe there were some comparisons made, and I have to caution you on that Ed-type of data, that it isn't as good as you'd like to see because of some of the ways you have to do comparisons to avoid getting into classification. So you'd have to find a source where you didn't have to worry about that, I think. You can't just publish exact quantities and amounts of everything that's there.

DR. DOMENICO: You've got some things that have migrated that perhaps shouldn't have. You mentioned perhaps Eh, pH control or transporting, being transported with the colloids. It seems that those are questions that could be addressed now rather successfully in the laboratory, in the sense to put more--to throw more light on the reasons why you observed what you have observed in the field.

DR. RAMSPOTT: Yeah, and you can also use geochemical modeling codes and things like that. I think that we haven't gone back and looked at these data as systematically as maybe we should.

DR. VERINK: Thank you very much, Larry; appreciate it very much.

Our next speaker is Dr. Isaac Winograd. He's going to be talking on archaeological analogues, Yucca Mountain alternative perspectives. As many of you know, Dr. Winograd is a research hydrologist for the U.S. Geological Survey. His publications on the potential utility of thick unsaturated zones of arid regions with the isolation of solidified toxic waste have played an important role in the Department of

Energy's decision to explore Yucca Mountain as a possible repository for high-level radioactive waste.

Dr. Winograd asks that any questions on his work be reserved until the completion of his formal presentation. We're very pleased to have you with us today, Dr. Winograd.

DR. WINOGRAD: Thank you. Good to be here.

Good morning. I've been asked to review for you some notions published half a dozen years ago that archaeological and other natural analogues may be important to us in an evaluation of the fate over 10,000 years of high-level radioactive waste buried at Yucca Mountain or at any other proposed site.

Why are we seeking analogues for high-level waste disposal? Clearly, it is because we are acknowledging the limitations of prediction in the earth sciences. Reasons for questioning the accuracy of such predictions were outlined in Geological Survey Circulars 779 and 990, and in references cited therein, and need not be belabored today. Basically, the reasons are threefold: An empirical database does not now exist with which to validate, and perhaps even to calibrate our models. Second, the track record of prediction in the earth sciences, including soil mechanics--the oldest quantitative branch of earth science--is mixed; and thirdly, strong philosophical arguments exist for believing that explanation and prediction in natural sciences are not symmetrical; that is, understanding a modern, geologic process process or a past event does not mean that prediction is attainable. Stated in the simplest of terms, our ability to evaluate predictions of the fate of buried toxic wastes over millennia is severely limited by our lack of experience. Hence predictions of the fate of such wastes, whether generated by complex, physiochemical models, or by back-of-the-envelope computations should be viewed with caution.

So what are we to do? Certainly, process studies and modeling efforts related to understanding the fate of proposed buried wastes must go forward because such efforts frequently identify the weakest link in our knowledge of a system, and may even lead to early disqualification of a site, of marginal sites. But other endeavors are of equal importance, as Dr. North suggested, lest we be lulled into believing that numerical models assure certitude.

I hope to suggest to you in the next 20 or so minutes that a synthesis of the global archaeological record of man's use of thick unsaturated zones, and the paleoecological record preserved in caves of the southwestern United States provides an invaluable empirical database pertinent to the trans-scientific problem of high-level waste disposal.

Before proceeding to give you an introduction to the archaeological and paleoecological records and their purported bearing on the high-level waste problems, some caveats are in order.

First, Winograd is a geologist. I am not an archaeologist, although it is a hobby of mine. Second, I am not a paleoecologist, though I follow that literature very closely because my research interests for the past decade have been principally in Pleistocene paleoclimatology.

Let's review Webster's Third Unabridged Definition of "analogy," which states: "inference that if two or more things agree with one another in one or more respects, they will probably agree in yet other respects." How about that? Or, "resemblance in some particulars between things otherwise unlike."

Okay. So how do we go about knowing whether things really resemble one another or agree with one another? Specifically, if Ike Winograd tells you that Archaeological Site X is analogous to Yucca Mountain, you should immediately ask him how can he be so sure that the paleohydrologic, paleoclimatologic, and geochemical conditions at Site X are similar to those experienced at Yucca Mountain during the past 10,000 years, and likely to be experienced there in the next 10,000 years? This is an important question. I will return to it later.

And, even if we can convince ourselves that one or more archaeological and paleoecological sites are acceptable analogues for physical conditions at Yucca Mountain, a major problem still remains. Current plans call for the disposal of ten-year-old spent fuel, which will result in repository temperatures of up to 250° C. Clearly, no archaeological or paleoecological site was subjected to such temperatures.

And lastly, might not the archaeological and paleoecological records mislead us into thinking that the excellent preservation provided by arid and semi-arid unsaturated zones is much better than it really is; that is, archaeological and paleoecological records, just like stratigraphic records, are incomplete, perhaps notoriously incomplete. Thus, these records are probably strongly biased toward successful preservation; the unsuccessful ones having left no exciting finds to be described or displayed.

Let me address these important caveats head-on before proceeding to show you some fascinating archaeological and paleoecological finds.

First, with regard to whether or not Archaeological Site X, or Sites X, Y, and Z, resemble the paleohydrologic and paleoclimatological setting at Yucca Mountain clearly would require detailed, site specific studies, and truthfully, it is unlikely, in my opinion, that any site will match Yucca Mountain in all, or perhaps even most, key respects; that is, we will never find a perfect analogue.

Second, I fully acknowledge that archaeological and paleoecological records are incomplete.  Nevertheless, the value of these records arises from the vast number of such sites.  Unlike Oklo, which records a fascinating but unique geochemical event, there are globally thousands of examples of archaeological and paleoecological sites demonstrating that well-drained unsaturated zones, even as you will soon see in humid climates, provide environments favoring excellent preservation even of the most delicate and water-soluble of man-made objects.

Thirdly, in order not to oversell you on the importance of such analogues, I have deliberately chosen to show you examples of largely accidental preservation for millennia of delicate objects, such as foodstuffs, leather, wood, and water-soluble minerals such as gypsum and calcite; that is, I will not parade before you slides showing beautifully preserved glass vases or diorite statues several millennia old.  I do this in order to try to balance the obvious and previously cited limitations of the archaeological records.

Lastly, with regard to the difference between the proposed repository temperatures and the much lower temperatures to which archaeological and paleoecological objects have been subjected, I will simply state that a strong case has been made repeatedly over the past dozen years for keeping repository temperatures below 100☜ C.  This case is reviewed in Open File Report 91-170, which is one of two handouts that I have given the Board today. To the extent that we can keep repository temperatures below 100☜, the vast and global archaeological and paleoecological records become relevant, I believe, to high-level waste disposal.

To summarize what I've said to this point, the large number of unsaturated zone archaeological sites, their occurrence in Holocene and Pleistocene climates ranging from arid to humid, and the great variety of materials buried in them should permit us to glean a wealth of qualitative to, perhaps, semi-quantitative information, bearing on the preservation and relative weathering of materials in unsaturated zone environments over millennia, to tens of millennia. Such a synthesis can provide an independent evaluation of the efficacy of the unsaturated zone under what can only be viewed as worst-case conditions; that is, early man's burial of unshielded objects at shallow depths, or his subsequent engineered placement of precious objects that invited repeated entry into his structures by thieves. In contrast, solidified toxic wastes of low solubility presumably will be emplaced in the unsaturated zone at depths of tens to hundreds of meters, will be encapsulated in--may be encapsulated in low-solubility containers, and will be placed in burial chambers designed to conduct vadose water around the containers.

The proposed synthesis of the archaeological record, admittedly, is likely to yield only qualitative information regarding the expected fate of materials buried in the unsaturated zone over millennia. Yet, such a synthesis can constitute an invaluable supplement to computer-generated predictions that, although quantitative, cannot be evaluated in the absence of an empirical database.

Time now to examine the record. I will give you, at best, a micro-sampling of what is out there, and I seriously suggest that the committee or subcommittee may wish to spend a week at the British Museum in London to get a firsthand appreciation of the vastness of the archaeological record, and of the magnificent--I thought you'd like that. Please take me back there.

(Laughter.)

DR. WINOGRAD: --and of the magnificent preservation of sacred and ordinary objects obtained by late Paleolithic, Neolithic, and more recent man.

There are three environments that consistently provide superb preservation of archaeological or paleoecological remains. These environments are: peat bogs, which due to their low pH and anoxic conditions, essentially pickle human remains; glaciers and ice sheets, in which you know mammoths have been fast-frozen for 10-15,000 years; and thirdly, well-drained unsaturated zones in all climates, but especially in arid and semi-arid terrain. Obviously, peat bogs and ice sheets are not of interest to us today, and in any event, they provide only a fraction of the archaeological record.

Let's look at some slides showing the types of preservation afforded by well-drained unsaturated zones during the past 20,000 years.

You are familiar with the excellent preservation afforded by burial in pyramids. Not quite as well known was the Egyptian practice of burial of noblemen in tombs cut into cliff faces bordering the west bank of the Nile, in the Valley of the Kings, across the river from Thebes.

This slide is of a wooden statue found in the tomb of the nobleman--let me try to pronounce this, I never can--Wepwawet-Embat. The eyes are inlaid gypsum and schist. The rest of the specimen, which is one meter high, is wood. It dates from the first intermediate period, which translates to roughly, it is 4,000 years old. Groups of wooden statues depicting everyday chores were common in the graves of noblemen, as seen in the next slide.

This scene is from the 4,000-year-old tomb of the nobleman, Meketre. The nobleman is shown reviewing the state of his herd of cattle.

Another common burial practice of the Egyptians was the mastaba, or a flat-roofed tomb with sloping sides. Most of the early pharaohs were buried in such tombs. Often, the mastaba concealed a shaft and a drift into which the mummy and sacred objects were placed, as seen in this slide. The upper part of the sketch is a section view; the lower part is a plan view.

This sketch is of an excavation at Giza of a burial site dating from 4,500 years old. Note that the shaft extends well below, about two meters below the burial chamber. We can speculate whether this was done--this was done repeatedly, by the way, and one speculates whether this was done intentionally to allow the deepened shaft to act as a still well and prevent flooding of the burial chamber. Speculation.

These three slides you have just seen were deliberate attempts at preservation by the Egyptians, though we know, of course, that grave robbers plundered most of the royal tombs, however well hidden. With one exception, all the remaining slides are of delicate objects that survived for millennia to tens of millennia without deliberate efforts at preservation by our ancestors.

Here we see a 34 cm. high alabaster statuette from Tell Hariri in Syria. It is 4,500 years old. A "tell" is a raised mound, with generally sloping sides and a flat top. It has been formed by human occupation of the site over a long period of time. In fact, a tell is a great mass of debris, rubbish, dust, and soil accumulated over millennia of human occupation. Let me remind you that the term "alabaster" is used interchangeably for either massive gypsum, or for travertine. Both minerals, especially gypsum, are highly soluble in vadose water; that is, such detailed preservation in a tell environment is truly remarkable, especially if the specimen is composed of the mineral gypsum.

Here we see life-sized plaster statuary from the Neolithic village of Ain Ghazal in Jordan. These statutes are 9,000 years old.

From another tell, Tell-Es-Sawwan in Iraq, 8,000 year old alabaster female statuettes were commonly placed in graves. The eyes are inlaid shell.

The next five slides are from Masada, a small butte on the west side of the Dead Sea. This site is famous as a fortress of Herod the Great, built about 40 B.C., and as the site of the mass suicide in 72 A.D. of nearly 1,000 Hebrew zealots in the waning hours of their defense of the site against the Romans.

We're looking at Masada now, looking from south to north. The Dead Sea is visible in the northeast corner. This butte rises about 1300 feet above the surrounding plain. It is 1900 feet long, and only 650 feet wide. The site has been thoroughly studied by archaeologists, in fact, for over about 150 years.

Two thousand-year-old foodstuffs found within debris and ashes of the upper two meters of the top of this butte. Clockwise from the upper left, we see dates, walnuts, olive pits, pomegranates, grain, and salt. Even more impressive were some 7,000-year-old finds of grains buried in the jar in a 7,000-year-old village in the Negev. I was unable to get a picture of the condition of those grains.

Plaited hair still attached to a scalp found next to a skeleton, not shown, from one of the people who committed suicide.

A mosaic floor of one room of Herod's palace, preserved under approximately one meter of debris.

Part of the scroll of Ecclesiastes, again found under several feet of debris. Once again, the Masada slides are of objects 1900 to over 2,000 years old.

At this point, you should be asking yourself, well, so what? Rainfall at Masada is probably on the order of a few inches at most. No wonder the preservation is excellent. Good point. So let's look at unsaturated zone preservation in humid terrain.

This is the Etruscan Tomb of the Reliefs near the west coast of Italy at Cerveteri. It is a rock-hewn tomb dating from 300 B.C. The bas-relief of common household items and weaponry is of stucco. The Etruscans commonly used the scarp slope of plateaus as sites for excavation of tombs. The colors are as found.

The next six slides are of Ice Age art dating from 10,000 to 25,000 years ago. The art you are about to see is not unique. There are over 150 limestone caves in southern France and northern Spain containing such art. One book claims 200 caves. Actually, the spelunkers, as they commonly do, are protecting some of these caves and not letting all of them be known, for good reason. At least 150 of these sites. The climate is humid. We start with a few slides of paintings made using iron oxide, manganese oxide, and white clay for pigments.

This is Lascaux Cave in France, one of the most famous of the Ice Age art caves. The animal murals you will see on this slide and the next two slides are several meters long, and more than a meter high. These paintings are 17,000 years old, based on carbon-14 dating of charcoal associated with the pigments.

Another shot from Lascaux Cave, my favorite. Still another one. That book that I showed is referenced in one of the handouts. In fact, everything I refer to is referenced in that handout or in Circular 990, which you had earlier. In some caves, the paintings are mainly--this is a very famous one, many meters wide. It's from the Hall of the Bulls. In some caves, the paintings are mainly on the roof; in others, as well, on the walls; in still others, in natural shafts which are found in case. These Upper Paleolithic men were also sculptors and engravers.

These clay bison are from the cave, let's see if I can pronounce this right, Le Tuc D'Audoubert in France. They are 60 centimeters long, and dated at about 15,000 years old.

An engraved reindeer on the foot bone of a reindeer from the Cave Chaffaud in France; length, 13 centimeters; height, 4 centimeters. Here, the age not too well constrained. Based on associated artifacts, it's believed to be in the range of 10,000 to 15,000 years.

A spear-thrower in the form--supposedly in the form of a mammoth. I'm not sure I can see the mammoth--carved on antler. Twelve centimeters long, the source is from the Cave Tarn et Garonne in France. Again, the age not too well constrained, but between 10-15,000 years.

The excellent preservation of such Ice Age art in the caves of southern France and northern Spain is attributed by archaeologists to the near constant humidity and temperature, which typifies caves in almost all climatic zones. Once again, the preservation is not perfect. In some caves, the art is buried under or coated with calcite. In other caves, it has been destroyed by the collapse of the roof. Yet, such remains are plentiful in over 150 caves, and they range in age from 10,000 to 25,000 years, a truly remarkable record.

Let's close our archaeological tour by returning to the southern Great Basin, and looking at some paleoecological remains. Almost 30 years ago at the Nevada test site, Phil Wells, a young Ph.D., discovered well-indurated packrat nests, or middens, in the Spotted Range just east of Mercury, Nevada. The middens were comprised of fossil vegetation and small mammal bones, and were indurated by the rats' own urine. The middens also are readily dateable with carbon-14. Because packrats forage only within 100 meters of their nests, and because the vegetation could be identified to species levels, these middens provide a remarkable record of vegetation changes during the past 40,000 years.

I refer you to a wonderful new volume, entitled, "Packrat Middens, the Last 40,000 Years of Biotic Change," by Betancourt, Vandevender, and Martin, University of Arizona Press, 1990, for a summary of the exciting record given to us by the middens the past 30 years. Of interest to us is that these delicate middens--they are readily disaggregated by soaking in water--that these middens have survived for up to 40 kilo years. Let's have a look at them.

Eleana Range, Nevada test site, roughly 30 miles north/northeast of Yucca Mountain. This midden is about a meter high and half a meter deep, and occurs in a small cave. It weighs more than a ton--it's been estimated to weigh more than ton, and ranges in age from 10,000 to 17,000 years; that is, the stratigraphy, ten at the top, 17 at the bottom.

A close-up view showing Jeff Spaulding, who found it, sampling the midden.

Still another midden in the much wetter Sheep Range, roughly 60 miles east of Mercury, Nevada, at an altitude of 2,000 meters. This one ranges in age from 17,000 to 19,000 years.

Let's look at a close-up of a relatively young midden showing needles of limber and bristle cone pine, despite an age of nearly 12,000 years, a radiocarbon age of 12,000 years. Such middens are common. They occur in caves, open joints and rock shelters all over the southwest, in climates ranging from arid to nearly sub-humid.

Southwestern United States caves in general--or caves all over the world, but--are a prize source of animal and plant remains for paleoecologists and are the topic of the second publication that I've handed out for you today, the publication entitled, "Caves as Sources of Biotic Remains in Arid Western North America." It is written by a prominent paleoecologist, Owen K. Davis, who would probably be pleased--or perhaps shocked--to hear someone suggesting that his work may have practical fallout to the high-level waste disposal problem.

Okay. I've shown you a micro-sampling, and I underscore micro-sampling, of a vast archaeological and paleoecological record spanning 40,000 years in climatic zones ranging from arid to humid. The preservation of delicate objects emplaced in the unsaturated zone can be dramatic, indeed. One might expect that more durable objects, such as spent fuel rods, might fare as well or better, depending on temperature and humidity conditions within a repository. Temperature and humidity, in turn, will, of course, depend on how the repository is loaded and constructed.

While the archaeological record clearly suggests--to me, at least--that high-level waste disposal for 10,000 years should, with great care, be doable, it also issues us a stern warning, future human intrusion. Despite major efforts at concealment, which included false passageways, chambers, and rooms, the tombs of Egyptian nobles were found and plundered. In my opinion, one of the most serious technical issues in high-level waste disposal may be the matter of future human intrusion.

I also note that several papers on the subject of how and where to mark repositories in order to warn our descendants not to excavate them have already been published by archaeologists, and I've cited all those I knew about when I published Circular 990. I'm sure there have been others since.

The archaeological analogue, though hardly perfect, is valuable in still another respect, and I think Dr. North already touched on this; namely, providing the public and the courts with a readily understood basis for solid waste disposal in arid unsaturated zones. Public perception and acceptance are critical to any successful waste disposal program, yet the ability of the public and the legal community to understand the results of interdisciplinary computer models, however well accepted they may be in the scientific community, probably is limited.

On the other hand, the public and the courts are likely to understand more readily a qualitative, but strong analogue approach, which a detailed examination of the archaeological record may provide. The public can nearly touch and see the archaeological record and, therefore, may find it more credible than computer-generated numbers of perceived mysterious origin. At the same time, though, the archaeological record explicitly acknowledges the future human intrusion problem.

The above material essentially highlights the contents of Circular 990, which was written half a dozen years ago, and which I was asked to review. Since then, my thinking has matured a wee bit, and I would like to briefly share some new thoughts with you, some of which appeared in my Environmental Science and Technology paper on Yucca Mountain, and in the handout that I gave you today. This will just take another five, six, seven minutes, perhaps.

The ubiquitous fractures in the Topopah Spring member, do they spell doomsday for the Yucca Mountain site, or are they a major asset of this site? We've heard a lot about --and undoubtedly we'll hear more about--the difficulty of modeling the flow of vadose water or, for that matter, phreatic water through the ubiquitous fractures in the Topopah Spring member or fractures in any formation.

Bob Scott, of the USGS has measured 20 to 40 fractures per cubic meter in these densely-welded tuffs, a fact which critics like to point out translates into billions of fractures beneath Yucca Mountain. They are correct. There are billions of fractures beneath Yucca Mountain, but let's look at the fractures another way.

Their presence virtually assures a naturally drained rock mass in the event of rare, but significant, recharge events; that is, Yucca Mountain naturally possesses that chief characteristic that made possible the excellent archaeological and paleoecological preservation that you had a glimpse of. The fracture transmissivity of the Topopah Spring member has been estimated by Ed Weeks, or measured by Ed Weeks via borehole air permeability measurements to be as high as 1,000 darcies; hence, it is safe to assume that the Topopah is largely self-draining.

Moreover, as explicitly pointed out by Gene Roseboom in his Circular 903, simple engineering measures can be used to prevent or greatly reduce the possibility of high-level waste canisters ever encountering standing water. These measures, unfortunately, have not yet been applied to repository design, as I point in my handout. So, are the billions of fractures in the Topopah Spring member a major liability, or major asset of this site?

I suggest the latter, especially if we put the high fracture transmissivity to work for us with intelligent placement of the waste canisters. If vadose water rarely contacts the canisters, what, pray tell, is there to model?

Relative humidity within the repository. After sealing, it is likely that the relative humidity of the repository will be very high, owing to saturation of the pore spaces in the bedrock. However, the humidity and temperature could be lowered sharply if the repository is constructed in a fashion to permit natural air circulation. Gene Roseboom first suggested such cooling in Circular 903. In general, lower humidity favors preservation and perhaps should be sought for at Yucca Mountain, though recall the excellent preservation of Ice Age art, even in the well-oxygenated and humid caves of France and Spain.

Retrievability. In addition to its very high fracture density, the unsaturated zone at Yucca Mountain--or unsaturated zones in general--have another major attribute seldom accorded it; namely, ease of retrievability. Unlike other proposed sites--for example, in the Deep Sea, in basalts hundreds of feet below the water table, or in bedded salt--wastes disposed of at Yucca Mountain are readily retrievable should unforeseen events make removal of waste necessary or desirable.

Indeed, as has been mentioned repeatedly in the literature, one of the principal advantages of solid waste disposal in such zones in arid terrain is ease of retrieval. High-level waste emplacement in thick unsaturated zones is, in reality, protracted storage in deep tunnels rather than irretrievable disposal. It has, in fact, been viewed by Luther J. Carter as a shallow subsurface MRS facility.

Moreover, because the waste emplacement is in the fully accessible unsaturated zone, current estimates of groundwater recharge, repository temperatures, natural air convection, and so forth, can be checked repeatedly by direct monitoring for as many decades--and perhaps centuries--as is deemed necessary.

In summary, the archaeological and paleoecological record examined does, in general, support the utility of arid and semi-arid unsaturated zones for the burial of solidified--underscore solidified--toxic wastes. This record provides an invaluable, although qualitative, supplement to the quantitative, but untestable, computer-generated predictions of the long-term effects of buried wastes on the hydrosphere and biosphere.

Once the trans-scientific nature of the toxic waste disposal problem is understood, the qualitative conclusions derivable from the archaeological record may suffice to convince the public that solidified toxic wastes perhaps can be safely isolated from the environment by burial in carefully chosen--underscore carefully chosen--thick, unsaturated zones. One would hope that modern man will be able to equal and improve upon the practices of his ancestors in his attempt to isolate solidified toxic wastes from the environment.

And I'll close with a confession. On a recent vacation, after spending two full days in the British National Museum of Archaeology, I could not but ask myself the following question, and I've asked myself the question several times since that trip, which was made in '89. The question: Are we, in the RAD waste endeavor, in our quest for certitude, making a scientific and regulatory mountain out of an engineering molehill?

Thank you.

DR. VERINK: There would be time for one or two questions, I think.

DR. WINOGRAD: I want to add once again, I'm not an archaeologist. This is reconnaissance study. I would like to get--George Dinwitty (phonetic) and I, a few years ago, tried to get some archaeologists at the Smithsonian interested in this problem. They, in essence, told us it's not sufficiently academic for them.

DR. CANTLON: I notice that you didn't have very many metal objects in your things, but the Etruscan collections in Florence are just magnificent, and very, very fine detail.

DR. WINOGRAD: Right. Again, I was deliberately trying to show you worst case; man-made stuff, organic stuff, not the really beautiful, fascinating things. If I did that, then I'd be accused of overselling. I was trying to go the other way.

DR. BIRCHARD: Yes. Ike, I enjoyed your presentation. I would comment that if these analogues could be used in a way to more carefully--to define the conditions under which preservation occurred or some loss of preservation occurred more carefully in terms of geochemistry, flow of water, and so on, one might be able to get at least a semi-quantitative approach developed from this. So far, I think we've seen a lot of interesting material on it, but not direct--it's only indirectly useful from a point of view of design, I think.

DR. WINOGRAD: Yeah. In my circular, I don't know if you had a chance to read the circular or not, I did address that and, in fact, in the circular, I suggested that one might be able to get semi-quantitative information out of detailed studies of specific sites. I agree with that and I stated it in the circular, but my gut feeling is that the main value will be from the big picture, not from individual studies. But certainly, I would not discourage someone from making a detailed study.

For example, in the circular, I mentioned a simple thing, and I think Dave Bish and his colleagues are already doing this; namely, look at a vitrophere. In fact, they are doing it; the same vitrophere above water table and below water table at Yucca Mountain and elsewhere, and see what changes have occurred. Compare two archaeological sites next to one another. One has good preservation, one doesn't, and then ask the question, why? So I certainly would agree that one should try to get semi-quantitative information if you can, but I would hate to see DOE hire a thousand archaeologists to study 50 sites with the hope of finding an exact analogue to Yucca Mountain. I don't think one will be found.

DR. BARNARD: Ike, did somebody support this work, this reconnaissance work of yours, or did you do it on your spare time?

DR. WINOGRAD: No, I did it--well, I did it--I have a lot of leeway, fortunately. I've been with the Survey a long time, and--

DR. BARNARD: I understand.

DR. WINOGRAD: And part of it was done on my own time. Certainly, the trip to London was entirely on my own, a part of a vacation. Part of it was done on Survey time, but I was very tempted, really, to--and I could have sold it--to take off six months and just really get into the archaeological literature. It's a vast literature. It's a vast literature, and also, many people are highly specialized in that field, and I think--I had trouble finding the generalists that I needed to talk to. So at any rate, in terms of my effort, it probably, overall, was less than three months, so the ground has barely been scratched.

If anyone thinks there's any value to it, what I did was a very, very brief reconnaissance, a very exciting one for me, and I still keep my eyes open for appropriate literature in National Geographic and Science and other places, but to get into the archaeological literature is--really requires several full-time slots.

DR. VERINK: Thank you very much for a most interesting presentation.

Next we're going to hear from Dr.--I think we've got time to get Dr. Eisenberg's in before we break for lunch, and we're going to put the NRC presentation after lunch, if that's okay.

DR. BIRCHARD: That's fine with me. Thank you.

DR. VERINK: Okay. Dr. Eisenberg is Operations Research Analyst with NRC. He's a coordinator of the study of performance of the proposed Yucca Mountain repository, and is performance assessment lead for the NRC's review of the Yucca Mountain site characterization plan.

DR. EISENBERG: I think I'm ready to take off.

I was asked to talk about a subject of a paper that I gave about five years ago on using natural analogues for the validation of performance assessment models. I should point out that the views I'm presenting are my own, and do not necessarily represent those of the NRC or the NRC staff.

Since this is an old paper, I thought about perhaps trying to update it with some more recent information. I didn't really have too much time to do that, but I did try to amend the recommendations where I thought it was appropriate.

Let me start off with what I like to use as the definition of performance assessment. It's a type of systematic safety analysis. It's a subset of a lot of other safety analyses. It's used to predict potential health, safety, and environmental effects of the repository. It's a quantitative methodology, and talks about things in terms of both their magnitude and their probability. It includes a comparison, and must include a comparison to acceptability standards, which has some influence on the way you do the analysis, and you have to present the results in a format that's useful to regulators, scientists, decision makers, and the public.

My favorite paradigm of performance assessment is something like this: It starts out, as almost any risk analysis does, with a description of, or familiarization with the system to be analyzed. There's two parts of the analysis. One is concerned with the probabilities. In the case of performance assessment for a repository, it's usually a scenario analysis, with a description of the scenarios, a screening of them for analysis, and a consideration of their probabilities. In parallel with the scenario analysis, there's a consequence analysis which, on this particular chart, is cut short, because you can extend it to biosphere transport, but includes an analysis of the source term, flow and transport in the far field, and then if you wish to include biosphere transfer, include that, also.

You combine the results into some kind of performance calculation, which for the United States--and, really, for the rest of the world--is usually a probabilistic performance measure. You almost always do a sensitivity and uncertainty analysis. I would claim you really can't compare things to the regulatory standards until you do a sensitivity and uncertainty analysis. You can do it on both the total result, or the consequence analyses by themselves; then, of course, comparison to the regulatory standard.

It was suggested that I might try to show how analogues might help, natural analogues might help this kind of assessment, so that, for example, if you have waste form analogues such as the behavior of glasses, that can help you describe your source term; engineered barrier analogues, including archaeological-type analogues, native metals, meteorites, magmatic intrusions also help with the source term, this last because you have the combination of heat effects as well as the migration of certain elements.

Site analogues are a migration from mineral deposits--usually uranium deposits--and migration of natural radionuclides. For biosphere transport, you have natural radionuclides migrating in the biosphere, and for the scenario analyses, you can look at multiple occurrences of events in, say, similar situations, looking especially at disruptive events like volcanos, earthquakes, and glaciers. This gives you some basis for estimating probability.

I didn't have a definition of natural analogue, so I created one, and it's a rather broad one. A natural analogue is any occurrence or instance of any naturally occurring or man-made materials, geometries, configurations, processes, and/or phenomena in a geologic environment that can provide some insight or information about the behavior, especially the long-term behavior, of a nuclear waste repository, its components or subsystems. So I would like to use as much as is available.

Why are we interested in using natural analogues, or why might it be useful? First of all--and this is in some opposition to what Ike said--I think the principal means of demonstrating safety of a nuclear waste repository will be the predictive models.

Unfortunately, we can't use the usual method of evaluating or validating predictive models that has been used in engineering for a very long time; that is, comparing the predictions of the models to the results of experiments which are to be predicted. You can't do that for a 10,000-year type of process, and in addition, the space--in addition to the time scale being too long, the space scales are too long to often be able to take effective measurements in any reasonable amount of time. The future environments are variable and uncertain, and the geologic system is heterogeneous and cannot be precisely defined, so that it's hard--it adds an additional complication to model a system which you have not made and whose parameters contain a great deal of uncertainty. So these are some strong motivations for using natural analogues to validate performance assessment models.

On the other hand, simple demonstrations of repository safety, by comparison to natural analogues, may not be effective. As Dr. Winograd said, there are unlikely to be an exact analogue to any repository, and the differences can then be used to undermine the effectiveness of the analogue. This is, I guess I'm thinking about the NRC licensing process where all the assertions of the licensee are subject to challenge, and that would certainly be a way to challenge any type of evidence of this nature.

The other point that was also made by Dr. Winograd was that the analogues that show that things are safe and that the environment is benign and the processes do not destroy the waste or make it move, have survived, but the ones where that is not the case are no longer there to be examined. The material has corroded. The radionuclides have migrated. So that's another way to undermine the evidence for natural analogues, if they are used in a rather simple fashion.

Well, what I did in this paper was look at an assemblage of natural analogue studies, and I did not try to be comprehensive in looking at these studies, or exhaustive, and I'm not even sure it's representative, but I looked at a group of studies and then tried to classify them and see how they might support the validation of performance assessment models.

To do this, I separated them up into component studies and system studies, and under components, waste form engineered barriers in the site. I also tried to, if you will, categorize the various natural analogue studies in the way it might assist in validating performance assessment models. So a very simple way of assisting in validation, perhaps the first step would be the identification of processes and phenomena. A second level of validation would be the termination of parametric values. A third, more difficult level would be having enough data in your natural analogue so that you could both fix the parametric values needed in your models, and have enough data left over to compare to the predictions of the models on an independent basis.  When you have redundant data of that nature, you could actually do some sub-model validation.

And then a final level might be to attempt to model the entire performance of the repository, which would involve either a probabilistic-type of analysis, or perhaps the linkage of several sub-components of the repository system, something that I believe would be very difficult to obtain.

Well, I looked at a number of studies, some of which I believe you may hear more about.  This table is made up the form of, on this first part, the component studies for waste forms, the engineered facility in the site, the author and date, the subject of the papers, and what level of validation might have been obtained, and as you can see, the higher levels are very hard to come by.  It's very hard to get redundant data sets that have enough information to compare model predictions to modeling results.  There are a lot of papers that describe the phenomena that might be encountered in building or operating a repository, and a few more that have been used successfully to determine the parameters used in performance assessment models, such as retardation coefficients in this series of studies.

This is the rest of it, looking at system studies.

One of the most interesting ones was this study by Gilbert, where he looked at the models used for biosphere transport of radionuclides against the compartment models that are normally used for that type of analysis, and the interesting thing was, is he found that they could be as much as two orders of magnitude too optimistic, just based on the results of the movement of naturally occurring radionuclides.

As you can see here, the range of studies is quite wide, but the ability to extract from them enough data to actually do model validation is limited. So that got me to think, well, why is it that this occurs? And these are, perhaps, some reasons why it's difficult to use natural analogues for this purpose.

First of all, you can't control the environmental conditions as you can in the laboratory. This is a constant problem, or often is a problem in natural analogues if you want to apply them in this sense. Another problem is that the characteristics of the geologic system can only be specified in terms of uncertainties, because the system is heterogeneous and natural, and very often quite complex.

Another problem is that typically in laboratory science--and one reason it has made a lot of progress in the quantification of process and phenomena, is that you isolate particular processes. With geologic systems, you often have several processes occurring simultaneously and, therefore, a validation of a particular process is not possible.

As Dr. Winograd said, the quantitative predictive models for geologic systems is not as advanced as is found in many other fields of science and engineering, and that's an additional problem, and the other issue that I've alluded to before is that natural analogues, at least as studied at the time that I wrote this paper, are often data-poor, since the same data are used to fix the parameters in the models as are used to validate the models. So, therefore, you may only be calibrating a model, rather than validating it. I believe that since that time things have improved.

The recommendations I came up with as a result of this little study, and you'll see a couple of footnotes. The first footnote is that since these recommendations came out, it appears that some of these issues have been addressed, and where you see two asterisks, it appears that there's been quite a bit of effort to address the issue.

The first recommendation is that perhaps looking at simpler systems, rather than a complex system with many elements, even radio elements in them, might be more useful for obtaining the quantitative data that you can use for model validation.

An example of that might be for the advective-dispersive equation, you don't necessarily need to use a radionuclide to validate that equation in a particular system.

You could use some more usually occurring element to do that. Admittedly, then your problem is whether the constants for the radio elements are correct, whether the Kd's are correct, but that's another issue. The important thing is, is there is some question that has been raised in the scientific community about the validity of the equation itself. You need to make sure of that before you can go on to arguing about the values of parameters.

That leads right into the next recommendation, that broadening the range of interest might be something people in the natural analogue community related to waste repositories might do to help obtain the data that might be more useful.

A third recommendation was that more emphasis needed to be placed on obtaining data that would reveal the environmental conditions that prevailed over long periods of time when the natural analogue was developing. This is something that has been, since that time, addressed to a greater degree.

A fourth recommendation is that some consideration might be given to collecting data to validate scenario formulation, and the probability distributions, or probability determinations for scenarios that are very important to the performance assessment context.

A fifth and final recommendation is that there should be better coordination between modelers and the field geologists that are looking at natural analogues.

Let me just bring up two other things. One is something that was given to me, a set of draft recommendations, I believe, from the natural analogues working group, and let me see if I can expand on this. The first recommendation is pursuant to my first recommendation. A number of the other ones; for example, the magnitude of areas, physicochemical parameters involved should be determinable, preferably by independent means, and should not differ greatly from those envisioned in the disposal system. This is much along the lines of determining the environmental conditions needed for the prediction.

So here is one example of the kind of attention to some of the issues raised in my paper. I certainly am not claiming that this was because I raised them. I think a lot of people, at the same time that I wrote the paper, were thinking along similar lines.

One final point, this is an introduction to the INTRAVAL project, and you will notice that they're looking at, as well as laboratory and field experiments, they're looking at natural analogue studies; in particular, the Alligator River project and the Pocos de Caldas project, and as they say, it is a pronounced policy of the INTRAVAL study to support interaction between modelers and experimentalists in order to gain reassurance that the experimental data are properly understood, and that the experiences of the modelers regarding the type of data needed from the experimentalists are accounted for.

This study, which is run by the Swedish Nuclear Power Inspectorate, was designed specifically to get this interaction, and is entering into its second phase now, where some of the feedback from Phase I has been given to go out into the field and get additional data.

Thank you very much.

DR. VERINK: There will be time for one or two questions.

DR. PRICE: May I ask, to what extent, to your knowledge, have attempts at data-poor environments where model validation and a hold-out sample become a very difficult problem, have there been the application of things like the press statistic, where you hold out a single data point, recalculate, hold out another data point, recalculate, and so forth, those kinds of attempts, have they been applied in this area?

DR. EISENBERG: Yes, they have been, a partitioning of data sets to see if you can predict the part of the data not included. Yes, those have been tried. Those were tried even in the INTRACON study, which was many, many years ago, in the late seventies.

DR. PRICE: Well, some of this has been fairly recent, its statistics, in an attempt to solve the problem of model validation by development statistical attempts to be able to determine the stability of a model.

DR. EISENBERG: Well, I'm not familiar with that exactly, but the idea of partitioning the data and using some of it to calibrate the model, and then including additional ones to see if you can predict has been used.

DR. VERINK: Well, thank you very much.

Let's reconvene, then, with the next paper by Dr. Birchard at one-thirty.

(Whereupon, a lunch recess was taken.)

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

DR. DEERE: Good afternoon. We will continue with the presentations. The Chair this afternoon is Dr. Langmuir, who is co-chairman of the Board's Panel on Hydrogeology and Geochemistry.

Don, I'll turn it over to you.

DR. LANGMUIR: Thank you, Don.

We are sorry, that Dr. Linda Kovach was unable to join us today. In her place, we will hear from Dr. Birchard. Dr. Birchard got his Ph.D. at the University of California at Los Angeles. He has been involved in the nuclear waste effort for the NRC in geochemical aspects of it for about ten or eleven years. He tells me he knows the day he started.

He is going to speak to us with regard to the NRC's Natural Analogue Program plans.

Dr. Birchard.

DR. BIRCHARD: Hello. I am George Birchard of the USNRC and I happy to say that Linda Kovach is getting better, but I've been forced into duty at the last minute here. I discovered that it is going to take a little bit more than a half an hour, because I've got a number of different packages, not just the plans which are going to be relatively brief, but also I've been working an the Alligator Rivers package. We have another Alligator Rivers package from Johns-Hopkins University. We also have a package from the Vallez Caldera study. So, I have about four different things to talk about.

With respect to the question of analogues, I like to look at it from a slightly different perspective than my colleague at the NRC has looked at it. Not from the point of view of saying what is wrong with analogues, but just looking at it from the point of view of how are we going to test the models that we are going to use in the waste business? How can we? Because, if we can't have models that have been shown to be applicable to a range of conditions and show to have results within some degree of accuracy, then we cannot justify the claims we make about the safety of the repository. And in fact the models can be justly criticized as not being scientific, but as being some sort of political exercise rather than a scientific one.

This is a diagram drawn up by Jean Claude Petit who is part of the NEA, showing the validation, the nature of the analogue problem, prediction, and validation going from short-term to long-term. In general, our analogues at NRC are focusing on the issue of the extrapolation to long periods of time, although there is also an aspect of scaling up in hydrology experiments. We won't talk about that for the purposes of this plan.

There is a definition from Natural Analogue's working group for natural analogue. Neal Chapman of the NEA has drawn up some guidelines that were shown in the previous talk, and although that document says draft, it really is a final document drawn up in 1984 by the Natural Analogue's working group. I have a slight disagreement with part of this. I don't think it is simply the job of a natural analogue to be directly analogous to a repository or site. I think what one needs to do is develop an understanding of a range of processes under a range of different physical, chemical and environmental conditions so that one has a good understanding of a sensitivity of a system to change. How stable is that system? How predictable is it? It is not just a question of quantification or precision or accuracy, it is also a question of system stability.

These are some of the aspects of natural analogue's work. I am looking at the slides too, because, I haven't worked from these before. So, excuse me for not looking at you at all times.

Natural analogues are useful for: scenario development, sensitivity studies, looking at processes, model development and data base validation. I'll talk a little bit later about some work that I have done in the data base validation area where, when you do a study of a geochemical aspect, you can find out the problems in the geochemical base and it gives you insight on how to improve it. But scenario development studies might be studies of volcanic processes, for examples and looking at what might happen if a volcano were to occur in the Yucca Mountain area and what scenarios would be serious and which ones would have an effect.

Sensitivity studies and analogues, as I said before, can look at a range of conditions and a range of processes and so give you an idea of how sensitive a system is to a change in environmental conditions. One doesn't just do sensitivity studies using a computer. Nature has often done many of its own studies, and if you like, and if you are willing to go and look at what's happened in nature.

And certainly, coupled processes such as if you are looking at hydrothermal systems, if we do go above 100 degrees celsius as it is still planned in the U.S. (isn't planned in the rest of the world) then one needs to have an understanding for example of when is a heat pipe effective for under Yucca Mountain conditions. We might be able to learn this from looking at some natural systems. So, it gives insight in a couple of processes.

Data base validation, for example, the work I looked at show there are some problems with uranyl silicate data base. We'll discuss that later. We have a conceptualization here. It is not a perfect time/temperature curve, but it is an approximation of the thermal regime that we might have in a repository given the present plans. And you see depending on your distance from the repository. You go through a range of thermal profiles.

That range of profiles can be used to look at a range of analogue studies. One can look at systems in terms of the temperatures and time frames that they apply to. From this we see that industrial analogues and laboratory studies cover short periods of time, cover a full range of temperatures, whereas you can look at geological, archeological, epithermal systems to cover that range of longer times, over a full range of temperatures.

Dr. Kovach is developing a research plan which is going to especially look at a number of the questions about going above the ambient temperature. Issues that are brought up here are: waste form stability; host rock stability of temperature; looking at the transport as you go up temperature. And, she is particularly interested or research work has been in the area of volcanism. I am particularly interested in looking at the tectonic problems. Although I am presently called a geochemist, I am very interested in the regional tectonics of which I think are a very important part of the scenario development problem for Yucca Mountain.

In the current studies, we are starting work at Pena Blanca, which addresses saturated and unsaturated conditions. Presently, it is unsaturated. I believe it formed under saturated conditions. We'll hear more about that from Dr. Murphy who will follow me. Alligator Rivers can be applied both to saturated and unsaturated conditions. We have studied it and thought originally it was simply a saturated zone analogue. But, the more we study, we realize that it has actually also been an unsaturated analogue and if you study the ore formation process, you may look at temperatures in the chlorite metamorphic grades, which may be a couple of hundred to three hundred Cs. So it can also be studied from a point of view of elevated temperatures, although the primary focus of the Alligator Rivers project is in the ambient temperature regime as radionuclide transport analogue. And certainly the European interest is in that area in testing their transport models. That is the main focus of the project.

Vallez Caldera was looking at issues of host rock stability and how the rock behaved under a thermal condition. I'll talk about that later.

We are looking into studies--Santorini is one word. As I said before, I'll get it right when they send me there. I actually didn't type it up though.

We are looking at McDermitt Caldera as a possibility. DOE may be looking into McDermitt which is in Nevada and in tuff. It may be a useful place. I don't know that much about it, but I guess Bob Levich might speak of it later for possible DOE study.

I think Oklo is well-known by the audience. We may do further work. Professor Ewing is in the audience and he has been working with Oklo as a waste form analogue. It may be an area where we are further involved.

Disko Island is an area where there is a metallic phase in basalt, which may be an analogue to corrosion processes.  We are considering that.  I don't think there is a strong interest at this point, but we are considering these.

Alligator Rivers isn't all--if you look at the dispersion zone, it also goes to shorter times.  And I think both of these can be taken over a full array of temperatures.  This gives a feeling for where the analogues address time/ temperature curves.  Again, this would be a range of waste package type analogues where you could address different time and temperature aspects.

The Vallez Caldera study, I'll explain a little bit more about it now, is a study where there were obsidian flows which are very hot that went over tuff in the Vallez Caldera which is near the Los Alamos labs.  The site is about 30 or 40 miles away.  There are actually several occurrences where the hot obsidian flow has gone over tuff.  We were looking to see to what extent the host rock would either lose its physical properties, the physical properties would change in some way, or, there would be evidence of radionuclide surrogate transport.  So, we would look for element mobility at Vallez Caldera.  And as you can see, the range of temperatures that we did modeling of the cooling curve.  And as you can see there is a fairly tight period of time in which it was heated up. The good thing about that particular regime is that it is easy to do a thermal model on that kind of system; relatively easy.

We are planning a natural analogue workshop. Based on guidance that your group has, we will come up with further plans.

I have decided to start with the Vallez Caldera study so that I could put together the two studies on Alligator Rivers. Vallez Caldera, the Sandia National Labs have been doing research for the NRC. You can see Harlan Stockman and Jim Krumhansl were the principal investigators. The thermal pulse was approximately 300 years and we were trying to understand what kind of response, and in particular what sort of elements might occur under that thermal regime. It is a little hard to follow, but it is actually a huge caldera over ten miles across. It's a little hard to see on here, but it pretty near a road on a steep cliff in New Mexico, as I said, about 30 miles from Los Alamos.

Schematic cross-sections shows the BBO unit is a Banco Bonito Obsidian, and you can see it was heavily sampled as it overlies the Battleship Rock Tuff. And also, if you notice below it there is another obsidian unit, the VC-1 unit that overlies again another tuffaceous unit, VC-1 Tuff. So there are two sites in that system that can be studied for migration. As you can see we concentrated on the top one.

It's not exactly perfect, but that is a pretty steep cliff and it is hundreds of feet high.  If you go there in person you are impressed by the difficulty of the field work. The advantage of doing work along the cliff, is that it forms a vertical face.  In fact there is a paleo canyon exposure, and one can look at what was a relatively on-weathered system, The primary difficulty in interpreting the results of this study are understanding the effects of weathering on element migration because you had a non-conformity between the flows. You have to make sure you have accounted for the simple weathering effects on element migration.  We tried to get around that by looking at paleo canyon vertical wall situation.  That involved a lot of hiking up steep faces.

This gives an idea of what they looked at.  They did extensive characterization of the rock.  They also did laboratory experiment to try to see if they could reproduce in a lab the model that they created for what happened in the field.

The one thing that should be remembered about natural analogue studies is that they are not simply field studies.  There is always room for careful, experimental work to be done in cooperation and collaboration with analogue studies to however you want to look at it.  You can see these analogue studies extrapolating the experimental work, or you can see the experimental work is making the analogue study more precise.

I've always thought that it is necessary in this line of work we are in to have a combination of laboratory, field and modeling studies to be integrated and to make sense of these very complex systems we have to deal with even fine analogies to sub-systems of repository. Here is a contact drawn in and they are walking up a face here and getting samples across. This is Banco Bonito Obsidian. Here is a tuff. So, it is a very steep contact, difficult to sample, but they had a good time.

Here are some pictures of what it looks like. We'll just go through them quickly to give you an idea. They did careful characterization to ensure that the samples were not affected by weathering and that they understood the mineralogy and the process that had gone on. I won't get into the details here because it won't be particularly helpful.

This shows the difference between units. You can see one is glassy and one has devitrified.

The guts of what we found in this study was that fluorine and chlorine were mobilized. You have halogen mobilization in the system. This one shows the association between devitrification and CL loss. But, the guts that we found, and I will show it in a later slide, was no movement of a wide range of elements. And movement was apparent only for chlorine and fluorine. The exact nature and distribution of the movement depends on where it went from and where it went to. And it seems to be localized depending on the fractures. So it is a complex system.

We'll discuss it a little more. Here shows the study of the contact and devitrification process. I guess this is showing chalcedony developing as a coating on glass, the vertical contact. If Dr. Stockman were here, he could give a much more detailed description.

What we see here is pretty stable, except for fluorines jumping around. Every iron oxide, sodium, potassium are relatively stable. Fluorine is the only element here that seems to have any significant mobility.

In this case, again we are seeing a lack of mobility of any of these trace elements. We have done some dating on these units and found some subtleties. We found one unit, it might actually be two units and so on and the age dates aren't terribly accurate. The Kr dating that was used is not always precise.

DR. DOMENICO: What are you dating?

DR. BIRCHARD: I was dating the various units. They are looking at the Banco Bonito Obsidian.

DR. DOMENICO: Did they see any new minerals come in that weren't relatively young? Were they looking for that? Or a phase transformation?

DR. BIRCHARD: As a result of heating and so on.

DR. DOMENICO: Were they looking for that?

DR. BIRCHARD: You know, I haven't gone through that detail. I better not try to answer precisely what they did with regard to that question.

DR. DOMENICO: Did they do anything to ascertain the mechanical effect of heating on the rock, which was probably a far more important question than the mobility question. Has anything been done in that area?

DR. BIRCHARD: I believe that the answer would be that given the nature of the outcrops, that it would be a very difficult exercise, indeed. I don't think they were able to do much about that because these are isolated, rather steep outcrops. I don't think this is a great place to answer that question. I am not sure about the subtleties of the age dating question, so I'll just pass on that.

I can tell you it is a difficult field site to get the kind of two dimensional structures that you'd be talking about to answer the question you are asking. It is a good question.

What we see here is looking at this vertical Site 12 contact, I think it was pretty much like the picture I showed you where you had the steep wall. You see as you move away from the contact that there is a fluorine enrichment chloride depletion showing that fluorine was mobile. It probably moved up from the heated unit below and migrated into the unit above. The details of the movement--I want to be cautious about that. I talked with Stockman for sometime. He said it isn't immediately obvious because he couldn't get a unique solution as to what extent it was taken from one place and moved here or moved out of here. I'll just leave it at that.

This is a previous study that showed chlorine and fluorine did not behave identically. Here is Site 13 again showing the fluorine and chlorine ratio. We are seeing that chlorine and fluorine don't behave similarly. They get various ratios. Perhaps fluorine was being taken up on one of the devitrified phases in the unit above the contact. It's below, now we are looking the other way. It's mobilized.

The BBO unit shows the range of temperatures of this depth; temperature of the one obsidian versus the other obsidian.

I did an experimental study to try and verify what they had seen in the field. They set up a laboratory analogue to see how chlorine and fluorine would be mobilized. This shows introduction of fluid into a carburetor, then heated up and goes through the tuff and they have a deposition out of the tube.

What they see are different partition coefficients for fluorine and chlorine. Both of them are still well below 1, so most of the fluorine and chlorine are remaining in the rock, but the fluorine is more mobil at lower temperatures.

In all, the conclusions from this study were that there is no evidence of mobilization of anything but the halogens. And that was to some extent verified in experiment. They found out from the chronology that the Banco Bonito may be two units. They did an extensive study of the devitrification.

DR. DOMENICO: Give us a chance to read it.

DR. BIRCHARD: Sure.

DR. LANGMUIR: Was anything done with fluid inclusions, George, to establish what kinds of fluids might have been involved in these processes? These changes?

DR. BIRCHARD: Well I believe a vapor situation. So the thought is it moved as a vapor. In the way of fluids, I don't believe there is much present.

Basically, the results are that in this case there is little evidence of migration as a consequence of the heating. However, there may be leeching of fluorine and chlorine and particularly what the laboratory experiments were showing was that the fluorine and chlorine could be on the surface of fractures. For example, if you cleaned a rock off in the processing, you might remove a fair amount of the fluorine and chlorine. So, from a Yucca Mountain point of view, they have to be careful about how they treat the samples in terms of chlorine and fluorine.

Another matter is that in doing calculations of the water that is going to be interacting with the waste package, there needs to be a careful, I think, series of properly designed hydrothermal experiments to come up with water chemistries for interacting of wastes package. It's quite clear to me that J-13 water should not in anyway necessarily be similar to what we are going to see in the repository for a variety of reasons, heating being one of them. The other is unsaturated water chemistry may be very different anyway, not to mention differences in $CO_2$ levels.

The conclusion is that alteration of the host rock was limited to a few meters. Devitrification was extremely variable. I think that part of the mechanical question is going to relate just to the variability in the rock. I don't know if doing a mechanical study at an analogue site is going to give you a whole lot of information unless you have a specific model that you want to test. I guess that is what Climax was done for and so on. That was kind of an engineering analogue I suppose. Heating of a host rock caused mobilization of halogens. So, that is something we will have to consider.

Maybe I ought to ask if there are any questions about this study before I go onto the Alligator Rivers areas.

DR. DOMENICO: I am not familiar with happened here. What caused the thermal pulse?

DR. BIRCHARD: It was simply the obsidian flow went over--okay, you have a caldera that is active. It has periods between eruptions. The obsidian flow was a thermal source for alteration of the tuff below.

DR. DOMENICO: So that would be a lot different than any thermal heating we would expect at Yucca Mountain. In other words you--

DR. BIRCHARD: I don't know what you mean by heating. I mean it is a thermal source. It certainly is different.

DR. DOMENICO: Well, you would expect baking at the contact, for example.

DR. BIRCHARD: Certainly a lot hotter at the contact itself.

DR. DOMENICO: Well, in the repository environment you would expect a large volume to be brought up to some sort of a temperature.

DR. BIRCHARD: Right. Obviously it depends on the distance that you are away from the contact as to the degree of analogue between some part of the repository.

DR. DOMENICO: Was that pink rock detected as the result of heating?

DR. BIRCHARD: Well, that would be a result of heating, yes.

DR. DOMENICO: That was a result of the heating.

DR. BIRCHARD: Right.

We'll take a quick trip to Australia. In this map of Australia we see that Alligator Rivers area is in a north central part of Australia. There are a number of ore bodies in the Alligator Rivers region. And a number of Alligator Rivers, too. There is East Alligator and South Alligator.

Here's Jabiluka, Ranger and at present we are studying Koongarra. There are also a number of deposits of uranium that are not economic at this time that are in the region. I think Ranger has over ten separate deposits around the main Ranger mine. In Koongarra there are also a number of anomalies through the region. One advantage of studying Koongarra deposit is that there are other ore bodies one can study to see if there are differences in the processes to establish some baseline conditions, and to look at consistency of your models from one site to another.

What we notice geologically in this area is that there are Archean granites which are uriniferous as a basement. There are not many surface exposures of these. They are highly weathered, but very old rich granites. There is a about a billion plus year-old sandstone and there are proterozoic schists primarily in the Koongarra area of the Cahill Formation. In Koongarra, the schists--in fact of all of these ore bodies on this map, the schists are the site of the mineralization.

Here is a local geology of the Koongarra ore body. There are actually two ore bodies. There is a number 1 ore body and a number 2 ore body.  The number 2 ore body is at depth.  The number 1 ore body has undergone weathering at the surface and there is a dispersion plume which I will show you on the next slide.  It's associated with a reverse fault.  All of the ore bodies shown on the previous map are in one way or another associated with shirring and faulting.  The Koongarra fault has more displacement that many of the other systems. So, the faulting is more obvious here.

The geology ranges from am amphibolite grade.  The ore zone goes into a retrograde metamorphism to chloride grade.   There is a very big association in the region of magnesium in the rock with mineralization.

Here is a cross-section of the ore body showing there is primary uraninite, there is an alteration halo, there is a graphite confining layer.  This layer is the fault.  So, you have presumably water.  The sandstone is a higher permeability unit.  Water is forced through the ore body and forms a dispersion plume at the surface.  This is a phosphate zone, and this is a zone of presumably absorbed uranium.  We are doing detailed studies to ensure that in fact it is an absorbed phase, or to determine precisely what phase it is if it isn't absorbed.  But there is a uranyl phosphate, uranyl silicate, a uraninite zone and a zone of alteration and a graphite confining layer.

Personally I took to trying some EQ 3/6

modeling of the silicate zone because there is reason to believe based on modeling, that uranyl silicates may form at Yucca Mountain. So the three silicates observed at Koongarra are kasolite, sklodowskite, uranophane. Sklodowskite is actually the most abundant uranyl silicate. And there are a range of uranyl phosphates and other minerals.

I won't try to go over the details of this table, but the most significant aspect of this table, which I hope you can see is only at this one site, PH 49, a depth of 28 to 30 meters do you have a high uranium level of 93 or 94 micrograms per liter of parts per billion. So, this is about 100 parts per billion and is the highest uranium level you have despite active transport of fluid through the system. These other levels, you can see, can be about one part per billion uranium. So even in a uranium ore body that is close to the surface with active fluid movement, presumably solubility or other kinetic controls are keeping the levels of uranium down to one part per billion.

If you notice Eh's, the Eh isn't terribly reliable. It is moderately oxidizing water flowing through the system, yet the uranium in solution is low.

DR. LANGMUIR: Actually, since you've got dissolved oxygen, that is really the key. The Eh is fairly meaningless. You've got real dissolved oxygen values, so you are basically oxidized.

MR. BIRCHARD: Right. I believe that you are quite correct that the Eh values are meaningless based on the modeling. Maybe not meaningless, but we can't believe them.

In particular, the Kd-1 data for iron made no sense whatsoever. The EQ 3/6 crashed on it because the iron levels and the Eh and so on were not consistent. That may be a case of interaction at the piping or some other problem. We dropped the Kd-1 samples altogether from modeling, and went to the PH 9 which is the silicate zone and if you look at the log Q/K which is an indication of saturation, you see that the minerals haweeite and soddyite (these are uranyl silicates) would precipitate in the model. These are not the same uranyl silicates observed in the field. But, it is reassuring at least that they had uranyl silicates close to equilibrium. You can see there are a range of other iron and minerals. This is the Eh problem that you talked about, but it is probably not meaningful.

DR. LANGMUIR: George, does EQ 3 even have thermometry data for those other uranyl silicates you mentioned? It has one or two graphs in there.

DR. BIRCHARD: Well we had stars on the ones that have, but the answer is, some of them. Sklodowskite is in there but I don't believe it. It doesn't make any sense as you will see in a minute.

I looked at PH 55 which is the phosphate zone, and no uranium minerals are even close to saturation. This means that the system under present day conditions would be actively leeching. Again it is a magnesium bicarbonate groundwater. These are all magnesium bicarbonate groundwaters.

You notice copper is in this system as fairly close to saturation. Actually, it is a real thing that copper will control some of the secondary uranyl minerals, or it may anyway. It's been looked at.

And there is a far amount of silica in the water. The source of silica may be from some feldspar in the Kombolgie formation. It is not a pure sandstone. Apparently there are some other minerals such as feldspar in it.

So we did EQ 6 modeling on PH 49. We titrated the PH 49 water with uraninite. It started precipitating uranyl silicate but at that measured Eh value, the uranyl silicate all went out of the system. And this uranium, U02.25 phase of uraninite was present. So that indicates that the Eh value was probably not the proper value to apply. It was probably in fact a little more oxidizing. I would have plugged in an oxygen value for that but that one didn't have a measured value. It just had a blow at detection limit value, so I couldn't plug it in.

We did a more complicated case. I was working with Bill Murphy and he may have run even more cases since we did this and maybe he'll mention it later. We did a more complicated case with uraninite chlorite and pyrite and used kaolinite to fix the aluminum levels which were below detection limits in the measurement. And in this case we were happy to see that haiweeite did precipitate. Unfortunately, fixing this $CO_2$ has the reaction of all created and non-realistic situation where the solution became highly enriched uranyl carbonate. As we went into the model run it became a bit unrealistic. But at least we saw a reasonable series of alteration products, not precisely those of the field. We saw gibbsite and hematite which were realistic and manganese oxide is probably realistic, too. We saw a reasonable reaction, but it wasn't the right set of uranyl silicates.

What we concluded based on that was that the model results are consistent with modern day formation of the uranyl silicate zone, although it may have been forming for at least three million years. Based on other studies of the phosphate zone which we dated kaolinite crystals. We have done extensive uranium decay series dating on the dispersion plume which are presumably based on geologic evidence, not any older than the uranyl silicate zone. We don't have dates on the uranyl silicate zone. It has probably been forming for thee million years now. It continues to form through the oxidation of the uraninite in the system. We have a rather slow chemical attack. It takes a long time to mobilize the uranium in the system because of the solubility controls and because of the large buffering capacity of the system, apparently.

That leads us to a conclusion with respect to a repository that what one needs is to have a coupled model that has a complete mass balance kind of treatment. So you would have a coupling of uranium transport; you'd have a hydrologic and chemical model that would be coupled. And that will bring me into the next talk where we are actually working on looking at coupled models. That should give us a quantitative basis for looking at Alligator Rivers as an analogue to repository for testing coupled models that can be applied to that repository. Anyway, in this system where there isn't nearby uraninite, you are way below saturation with respect to uranium.

Dmitri Sverjensky of Johns-Hopkins has modeled the formation of the uranyl phosphate zone using EQ 6 and has reported that it will form under unsaturated conditions. You have equilibration with atmospheric $CO_2$ and perhaps back in the pleistocene, there is good evidence to think that this was the case. Because, under pleistocene conditions, sea level was lower and the monsoon was further to the north. So this was a desert environment back in the pleistocene, not that dissimilar to some of the desert areas in Australia. And perhaps not that dissimilar from Yucca Mountain, although that is taking a big step from Northern Australia to Yucca Mountain. Nevertheless, there were unsaturated desert conditions here.  Under those conditions the near surface zone became unsaturated; silicate minerals apparently didn't form; and,instead you had alteration to uranyl phosphates. So, there is a sensitive balance between the uranyl silicate and the uranyl phosphate in the system depending on the saturation versus unsaturation and depending on the buffering with $CO_2$. Another thing to observe here is that you have to be very careful about your scenarios and about looking at the speciation in your system.

Clearly, also, a better data base is needed for uranyl silicates which may be a phase that precipitates at Yucca Mountain. DOE actually has done work recently trying to improve the uranyl silicate values. Bill Murphy and I took a look at it and we are not sure that they resolved the problem yet. We think they may have a little more work to do.

In a test case for Bruton and Shaw, which comes with your brand new version of EQ 3/6, they have a test case of reaction of J-13 water with uraninite. And lo and behold, in their model uranyl silicates precipitate particular haiweeite. And as I said, it appears to be analogous with Koongarra, however one must be careful about this assumption about conditions and about speciation. And as our work in the previous talk showed, you can have changes in volatile element chemistry such as fluorine and chlorine levels. And I think the smart thing to do would be due to some laboratory experiments to bound that, you could plug it into the models to see how a change in fluorine and chlorine levels would affect your speciation. Therefore, by using a combination of analogues, experiments and modeling would be able to get an understanding of the sensitivity of Yucca Mountain to a change in chemistry that might occur in the repository.

A number of investigators have been the basis for this work. These are just a few of the references that are going to go into the paper I am trying to write up on the uranyl silicate part. Peter Duerden, over the last number of years has been the manager of the Alligator Rivers Project. He has done a heroic effort to pull it together. Andrew Snelling did his Ph.D. work there over ten years. And he is an invaluable source of information on the geology.

Moving onto developing these coupled models, hydrochemical modeling and modeling analogues of repositories, Grant Garven and his student Jeff Raffensperger have been working for several years at developing a coupled model which they are going to apply to Koongarra. And then they will perhaps also, if there is sufficient data, look at the systems in Canada which Garven worked on in his development of the Basin Brine Flow formation of the uranium deposits there. Garven has looked at a number of years at the evolution of unconformity type uranium deposits.

What we are trying to do here is extend the hydrological model into being a coupled transport model to do a better job of modeling the evolution of these deposits. This is a grant proposal that NRC has supported. It is not intended to be a direct study for repository development. It is supposed to be for educational and academic purposes. Don't criticize this for being academic. It may also be practical. It was designed to be academic.

Again we have a model of the schematic of Koongarra, where they show the various zones of mineralogy and this dispersion zone where transports occurred near the surface.

Here is a model of formation where you get a thermal plume that causes mobilization in the Athabasca Basin which is an area that Garven has worked with extensively. A thermal plume model that would cause fluids to rise up, and as they cool down, the speciation of uranium changes and uranium will precipitate in the system. At least, that is a simplification of what probably happens.

What Garven developed was a hydrologic model for basin transport. Raffensperger has taken that model and taken a simplified portion of the EQ 3/6 model, I believe, and coupled that with the hydrologic model. And what we will see are some test cases that he has run on that today. This describes some of the attributes of the model. I won't read them to you.

This shows the process of trying to develop a fully coupled model.  As you can see, this is the area that Raffensperger is working on for his dissertation.  He is looking at putting temperature in; kinetics perhaps.  And unlike other coupled models available, he incorporates the effects of geochemistry on porosity and perhaps permeability, too.  I hope he gets permeability in there.

So this would be, I believe, a unique coupled model. It would have the full coupling of the chemistry on the hydrologic aspects of the system, although obviously, simplified.  He shows some of the governing equations here, which I don't think we need to go through in this talk.  But he has considered them.  He has developed several simplified test cases to see that his model is behaving reasonably.  This is a case where you have a more permeable zone on top; less permeable zone with a pipe of a fracture zone or fault zone simulation.

What we observed in the model given that case is formation of convective cells.  It makes a lot of sense.  It seems to be working.

This is showing stream functions.  Again the difference in colors are just showing different direction of rotation of the system.  It is basically symmetric.

Here is a model of the temperature in this model system.  You can see that sure enough, it is warming up as a function of the plume is moving up.  It makes sense.

I took a slightly more complicated case and added a higher permeability zone on the left margin, which created a slightly more complicated field.

Here is our stream function reflecting the nature of the field and the temperature distribution. We can see now that we have a fracture over here that sure enough is getting warm as a function of flow upwards along that fracture zone. So far so good on the hydrologic aspects of the model.

Then he went to some simplified estimates of chemical interaction. This shows some solute transport calculations using the same model for a simplified set of calculations showing the movement of a plume over a period of, well say 20 years here. What this shows is the evolution of a system in the model where he is modeling calcium carbonate as components. It is just a calcium carbonate change. I don't even see uranium in here.

DR. DOMENICO: Those are all one dimensional models. Have you resorted to one dimensional modeling for the kinetics?

DR. BIRCHARD: This is simply a first test of his coupled model. It isn't a one dimensional model. It is simply that this is the first thing you do. He starts with the simplest case just to test and see if his coupled modeled seems to be working properly. And, the good news is that he put it in here but it didn't make sense. But, obviously he reduced it to the simplest case. He didn't want to run a two dimensional case before he had done a one dimensional case. The model will ultimately be applied to two dimensions. I don't think he has a goal of applying it to three dimensions.

DR. DOMENICO: But, at least two?

DR. BIRCHARD: Yeah. This is just a first effort. Give the guy a break. He still has a lot of work to do.

So again, here is another, I think this has evolved a bit longer, but again the same case of first test of his coupling and so far so good.

The moral of the story is that I believe that based on a concept of a full mass transfer model using a coupling of hydrology and geochemistry, that one can test this against natural analogues; test your models against sites like Koongarra for your source term calculation, and I think develop a much more quantitative basis for estimating the release of radionuclides from a near-field of a system. As to the details of how this would apply to an unsaturated system in Yucca Mountain, I believe you might need a different kind of coupling. Your hydrologic model might need to be different. But, this concept of the buffering of the system by silica, keeping a relatively low solubility of uranium in the system, the formation of uranyl silicates and using your coupled chemistry and hydrology to account for the transport of the small amount of mass that actually goes into solution, I think would give a quantitative approach to assessing the release from the engineered system in the repository.

I think this gives an approach to a much more quantitative performance assessment for your near-field.

Any questions?

DR. LANGMUIR: Questions from the audience?

Thank you, Dr. Birchard.

DR. BIRCHARD: I'm scared. No questions.

DR. LANGMUIR: We'll get you later.

DR. BIRCHARD: I will say that there is an awful lot more work that has been done on Alligator Rivers that I haven't talked about related to the transport modeling of the near surface, the whole INTRAVAL and model validation efforts. A number of soil studies related to the sorption process, coupled models of sorption are being tested against Alligator Rivers and I think are showing great promise. So there are a number of different aspects of Alligator Rivers. I've just talked about a few of them here. It is a very complicated study. It has taken many, many years to get it up to the point of where we could actually start talking about validation of models. When you get into this kind of system, you can't simply look at one part of that system and understand it sufficiently because it is really a multi-disciplinary characterization process. It is very analogous to the process one has to do to characterize a repository. It has to be a large detailed study when you get to a site of this sort. So there is an advantage to looking at simple cases to test simple process models. When you try to get to a larger scale, you have to be willing to make a larger commitment, and make it a multi-disciplinary effort. And having the multi-national study at a higher level of effort that we now have is making it work. In the early years we were never able to get enough resources to be able to get into the mode of the model validation aspect of the analogue.

DR. LANGMUIR: Thank you.

Our next speaker is Dr. William Murphy. Bill in a previous life worked in the BWIPP Project until about 1987. And since 1988 he has been conducting research on natural analogues systems, performance assessment, source term modeling and natural resources assessment for the NRC at the Center for Nuclear Waste Regulatory Analyses at Southwest Research Institute.

Bill.

DR. MURPHY: Thank you, very much.

It is an honor to be invited to give this presentation. I am pleased to be here and was very pleased by this morning's talks and this afternoon's talks as well, which seemed to be laying a very firm foundation for natural analogue research and its applicability to the problem at Yucca Mountain.

I have been asked to talk about a couple of different things by a couple of different people. I generally title the talk natural analogue research at the Center for Nuclear Waste Regulatory Analyses and I will concentrate to a large extent on the Pena Blanca site, which we have recently visited.

I'd like to acknowledge my co-author and this talk and my colleague in this research, English C. Pearcy, who is in the audience here and will be prepared to respond to the questions I can't answer.

Here is a little more detail about the project at the Center for Nuclear Waste Regulatory Analyses. We have one research project entitled Geochemical Analogue of Contaminant Transport in Unsaturated Rock. It is a very broad scope and a very broad idea. It is designed to start at a background level and to build up to some more specific research activities. The objective is to design and conduct an analogue study relevant to Yucca Mountain; to evaluate the use of analogues in site characterization and model validation. This is sponsored by the Office of Research by the NRC. Linda Kovach is our project manager. John Russell at Southwest Research Institute is our project manager there.

The project status as it stands now has been underway somewhat less than year. Literature review has been completed. In fact, it has been completed only in draft form, and the form slide you saw a few talks ago was in fact an extract of our literature review and some of our reports we are repeating from the literature.

We have initiated site selection investigations. This included field work at Pena Blanca in March that I will be speaking about in a few moments.

The project forecast is the selection of one or more sites and then the design of specific goal oriented research at that site to meet some of these objectives, collection of data at the site, and interpretation of data.

Now, the issue of natural analogues has become a very philosophical one as it should be. I can't resist making a few philosophical comments of my own which I regard as based primarily on our experience and understanding of the natural analogue literature. Many of these points are reiterations of things that were made cogently earlier today, but I will go through it anyway.

I think a repository for high-level nuclear wastes requires knowledge and confidence in a system over a time scale exceeding that of human civilization. That goes without saying and is much of the fundamental basis for natural analogue studies. Only geologic systems, and I'll amend this to say, geologic to include archaeologic systems permit direct study of chemical isolation and transport phenomena over the required time and space scales. And, particularly tne time I am thinking of here.

The concept of a geologic repository is based on natural analogue reasoning. There are geologic sites in the world that can be demonstrated to have been stable, largely closed, over millions or billions of years that could serve as safe repository sites for waste. This is analogue reasoning. Analogous systems could be identified.

An important point, I think, in addition is that geology can provide good analogues of unfavorable systems. The case that I will draw as an example of this is ore deposits which are commonly regarded as proving the stability of geologic environments. Ore deposits prove massive transport over very large distances and concentrations of radio elements in the case of uranium deposits for instance, or other elements in other cases, as well. So, this is not necessarily a negative thing. It is a good analogue of a potentially unfavorable geologic system.

Natural analogue studies can be looked at in small scales as well as large and interdisciplinary scales. George was just addressing this point.

Key uses of natural analogue studies I think were addressed very well, earlier today, evaluation of large space and time scale processes; validation of qualitative scenario type process oriented; and quantitative models of repository performance. Validation is a very broad issue and can be taken in many different ways: identification of process; identifications of the validity of scenarios; all the way to detailed validation of numerical models for contaminant transport.

There are many limitations which were also addressed earlier today of natural analogue studies: the incomplete geologic record; overlapping phenomena; difficult assessment of initial and boundary conditions in natural geologic systems that occurred over a very long time; the partial or imperfect analogy of any potential analogue site; and finally, non-unique interpretations of the geologic phenomena.

Now in this project we are just really getting started and I can't say that we have made tremendous conclusions about anything. We are looking at sites. We are doing a site selection process. This slide summarizes some general characteristics of the Yucca Mountain site with the Pena Blanca site. Another analogue that we have considered conceptually at this point but haven't done any on-site research, just literature research, that is the Akrotiri Archaeological site on Santorini. I'll address these issues a little bit later.

Just to focus a moment on Akrotiri, this is really a remarkable place where the mineral and civilization was buried by a silicic volcanic eruption about 3500 years ago. The rock type is right. The timing is right on a time scale of about

3,000 to 4,000 years relative to the repository. The hydrology is unsaturated. The alteration there, there was just a recent paper on the subject, a very excellent paper, identifying clinoptilolite as one of the primary alteration products of the site.

Another issue that I brought up earlier was the difficulty in constraining the initial end boundary conditions in any kind of contaminant transport model that one would like to validate using natural analogue studies.

In this particular case there are metallic and other artifacts that have unique, well characterized chemistry that are embedded in this tuffaceous unit that perhaps even underwent a thermal pulse associated with that volcanism. We know exactly how long they have been there. We know exactly what their initial chemistry was. They serve essentially as point sources for very well constrained contaminant transport processes.

DR. CANTLON: Where is the Akrotiri?

DR. MURPHY: Akrotiri is on the Island of Santorini. It is a very well studied archaeological site and that is another advantage of this site. There is a huge data base with regard to the dating of the events, the chemistry of the environment,and the climate has been well charted throughout archaeological time. It is also being excavated and we've been in contact with sort of the ring leader of the Akrotiri archaeological  research, a Professor Dumas from Athens.  He has invited us to participate in his program.  They apparently are not going to the field this year, but we have plans in the future to participate at that site and in their research.

Turning to the Pena Blanca uranium district, this is another site that was identified in our literature work.  It has really striking analogous characteristics to the Yucca Mountain site which I think will become evident as I go along here.

It is in Northern Mexico, just north of Chihuahua, just south of El Paso.  It is in the Basin and Range province.  It's an uplifted block.  The climate is semi-arid; 24 cm/year, slightly greater evaporation transport than at Yucca Mountain; average temperature 19/C.   The geology is a silicate tuffs, 70 to 80 percent silica.  The host rocks are mostly about 44 million years old.  These overlie cretaceous limestones and paleozoic sedimentary rocks as wells.

The regional aquifer is about 200 meters below the present site of this repository.  It is way above the saturated water table at present.  Probably this is due largely to the uplift of this block.  There is a discontinuous and topographically controlled perched water table that we've identified in our work, in fact just last month, which leads to the presence of a few springs here and there in the area and intermittent streams.

There are numerous, a hundred or more uranium anomalies, some of which are relatively high-grade uranium deposits, some of which have been mined essentially but there has never been any production from the site. In fact, it is the largest Mexican uranium reserve. The uranium minerals consist dominantly in these many deposits of uranyl silicates, oxidized uranium silicate minerals, such as the ones George was speaking of, and such as the ones predicted in geochemical modeling of the evolution of Yucca Mountain.

The mineralization is associated with zones of faulting and hydrothermal alteration. And another remarkable aspect of this site is there are localized zones of probably primary uraninite or pitchblende essentially $UO_2$ or slightly oxidized reduced uranium oxide, which serves as an excellent analogue of spent fuel.

There is one particular mine that we have identified that is within this district that has also received quite a bit of attention in the literature recently. In fact, there is a paper published within the last few months by Ildefonse, Calas and another author from Paris, on some of the analogue aspects and in detail some of the mineralogical study they have done at the Nopal Mine.

The geology is it is a high-grade uranium ore deposit localized in a vertical, roughly breccia zone associated with intersecting faults. Uraninite or pitchblende occurs there in irregular pods. Uranyl silicates occur around this uranium in somewhat of a dispersionary but mainly concentrated in the brecchia zone. These consist mainly of uranophane and weeksite; calcium and potassium respectively; and uranyl silicates. There is also haiweeite. There is also other uranyl silicates in the system.

The primary silicates are altered in the ore zone, mainly to kaolinite at greater distances to smectite. There is some heulandite that has been observed as an alteration product of a vitrophere that is near this contact that is related to the ore deposit. The pitchblende, is very clearly altered to a suite of uranyl silicates. The exposure is excellent. You'll see that in some upcoming slides. The URAMEX, the uranium production organization originally started to mine this area, removed quite a bit of ground and piled up the dug adits and shafts and piled up quite a bit of ore, and never processed it. URAMEX went out of business in the early '80s, and it has been setting there more or less untouched since then.

There are excellent vertical and horizonal cross-sections cut right through the heart of this ore deposit. That access is great. The property is controlled by the Mexican Mineral Exploration Agency. There is no activity there now. We have been in contact with the head of this agency in the Chihuahua area as well as other mining agencies.

And for our field trip last month we had permission to visit and sample the site. We got excellent cooperation from the Mexican Government. They also have a tremendous wealth of data that they have generated on the site. Not detailed chemical I think, other than uranium and molybdenum assays and many, many gamma logs, but, excellent geologic maps of the entire area.

To locate it very briefly, as I pointed out, it is right in this range, the Sierra Pena Blanca north of Chihuahua. These are tertiary volcanic rocks which one can extend up in this direction, of course.

Here is a geologic map showing some of the local geology and structure. Very clearly Basin and Range topography. This is a range silicic rhyolitic volcanic rocks. These are the cretaceous carbonates. Here are some paleozoic clastic sedimentary rocks. This is the basin of alluvial material; normal fault blocks bounding the Sierra Pena Blanca.

It is 50 kilometers from Chihuahua to the mine which was originally discovered. In fact, in the underlying carbonates in domotia deposit, which turned out to be a rather small scale thing, the real mineralization is just across the contact there in the tuffaceous rocks.

Here is a topographic map which may or may not show up very well here, showing the bounding fault. Each one of these squares is a square kilometer, the blue squares. Here is a bounding fault on the east side. You see zones of mines. There are some sort of northwest trend to them. There is some more mines on this quest. The Nopal mine is on this hillside, on this hill right here (indicating), actually the Nopal I unit is on the south facing a portion of that slope. You see mines spread out. There is another mine up here (indicating), or prospect, I should say.

Here is a view from the south of this Nopal I deposit, and you can see the excellent exposure. This is the core of the breccia zone. The primary mineralization was probably pyrite, silica, uraninite deposit that was deposited under hydrothermal conditions characteristic of another hydrothermal vein uranium deposit. Subsequently, there was a very large scale, oxidative alteration of the site and an oxidation of the primary uranium and dispersion into the surrounding rocks. It is localized in this breccia area which we will look at a little more in detail. This is a horizontal level that cuts a fantastic cross-section right across the top of it. This is what Ildefonse and others did much of their sampling. There are shafts and adits that extend down the breccia about 100 meters below this level. They are presently unmaintained. I wouldn't feel very comfortable being in them. There was an unpublished French thesis with some detailed mapping of the interior of this mine.

This is the zero level and this is the adit right in this breccia zone. This is the boundary of the breccia zone. You see it is a fairly small area. Maybe the core of it is about 20 meters across. This is some ore that has been pulled out that is a very gossen-like material here, probably associated with the oxidation of pyrite. There is relic pyrite observed at the site. Lots of uranyl silicates coating fractures and replacing feldspars and so forth.

Here is a photograph of the unaltered Nopal formation. It is not particularly welded in this particular sample, but there are welded units. It is described as an ash flow. Its bulk chemistry is rhyolitic, alkali feldspar and silica minerals are the primary mineralogy, which should sound familiar.

This I think is an incredible slide. Here we have pitchblende, probably with silica as well. This is very similar to spent nuclear fuel. Uranium dioxide in a silicic environment, tuffaceous environment. It is oxidizing. It is generating a suite of uranyl silicate minerals of soddyite, uranophane and weeksite. We have x-rayed this and confirmed some of these studies. They have also been very much studied by others. I think that is a fantastic analogue.

Here is a shot of one of the more altered gossen-like blocks from this same breccia pipe. You see the brecciation very clearly in the previous slide and also blocks of brecchia here. This is highly oxidized and I am not sure what the mineralogy is. We haven't analyzed it.

So, finally in conclusion, I'll put up a slide which looks out to the east from the Pena Blanca area. It looks familiar for those of you who have been around Yucca Mountain. And to say at this point in the project, I don't have many things to conclude because the project is really getting underway.

We intend to continue doing research, to make a selection of a site, to develop a very specific, well constrained research project associated with some analogue site, and furthermore, to do some coupled modeling of whatever processes we deem to be most tractable in association with that.

Also I would like to reiterate something George said at the Center for Nuclear Waste Regulatory Analyses and together with the NRC. We intend to conduct an analogues workshop to address issues brought up here and other issues related to natural analogues. We are hoping to get participation from many of the people here.

Thank you.

DR. LANGMUIR: Thank you, Bill.

Question, what about this workshop? We heard George mention it as well. Is there a scheduled date and time for it?

DR. MURPHY: There is a tentative date that has been identified for this summer. I am not sure that that is completely realistic. The status of it is that a preliminary agenda has been compiled and reviewed by the NRC and the Center for Nuclear Waste Regulatory Analyses and has received feedback from the Department of Energy. It is presently being revised and we are still working on it.

DR. LANGMUIR: Any further questions from board members?

DR. CANTLON: What did you say the estimated age of this Pena Blanca was?

DR. MURPHY: The volcanic host rocks of this particular deposit and the most of the deposits is about 44 million years. The age of the mineralization is unknown, really. There are two tentative dates of which I am aware. One at 4 million years and one at 12 million years. But, I don't know how good those are.

DR. LANGMUIR: Thank you, Bill.

I would like to introduce Dr. Russ Dyer, next. Dr. Dyer is in the Yucca Mountain Project Office. He is Chief of the Technical Analysis Branch of the Regulatory and Site Evaluation Division. He is currently responsible for management and coordination of the Yucca Mountain Project's Performance Assessment Program. Russ has coordinated this part of our agenda, so I will turn it over to Russ.

DR. DYER: My introduction is going to be mercifully brief here, I believe.

What we are going to try to do is introduce you to the spectrum of things that the Department of Energy, the Office of Civilian Radioactive Waste Management is looking at under the loose definition or the loose guise of what might be called "natural analogues". And I put quotes around it here in the introduction, because I think as will become evident in the following talks, that we are covering a wide spectrum of things under this general category. Many of the things have been alluded to earlier, some of the things Ike talked about, pack rat midden studies and such. It is hard to constrain and to put a firm definition on the number of scope of things that we are going to cover here.

DOE's suite of presentations could be subtitled variations on a theme. And this is the theme that we are going to look at. Of course, this is one of the key tenants of earth sciences and it dates back to the very foundations of earth science over 150 years ago. The assumption is that the present is the key to the past. Our ideas of exactly what that means have changed over time. Our current working definition of current understanding of this principal of uniformitarianism holds that things have not always been the same, but that the processes and natural laws now operating on the earth's surface, have acted in the same regular manner, and essentially the same intensity throughout geologic time, so that past geologic events can be explained by phenomenon forces observable today. It does not mean that all change is at a uniform rate or that all processes occur at a uniform rate.

As Norm Eisenberg's talk about a little bit earlier and Dr. North actually addressed, also, there is a regulatory issue involved here. The EPA standard for disposal of high-level radioactive waste, that is 40 CFR 191, calls for the isolation of waste for a period of 10,000 years and more. A time longer than recorded history. And part of the problem facing us is how to achieve reasonable assurance about the long-term isolation of radioactive waste.

Dr. North talked about one of the recommendations about the use of natural analogues this morning. This is another one. This comes out of the National Research Council's 1990 Report. And I pulled this quote out of the text of the report: "Natural analogues - geological settlings in which naturally occurring radioactive materials have been subjected to environmental forces for millions of years - demonstrate the action of transport processes like those that will affect the release of man-made radionuclides from a repository in a similar setting."

The recommendation, or the suggestion is that: "Where there is scientific agreement that the analogy applies, this approach provides a check on performance assessment methodology and may be more meaningful than sophisticated numerical predictions to the lay public." And, we certainly agree with that.

I had talked about variations on a theme. Well, if the present is the key to the past, I guess the next variation of that is, is an understanding of the past a key to the future? Whenever looking at natural analogues, trying to extrapolate the observations from natural analogues, or analogues, the fundamental assumption is that the same processes and forces will act in the future as have acted in the past. We still are faced with uncertainty with past and future rates and boundary conditions. And, I might also note, the initial conditions also are a source of uncertainty. Norm, particularly brought this up this morning.

Well, what is a good working definition for natural analogue? The Office of Civilian Radioactive Waste Management has a draft natural analogue strategy plan that is currently in review. This is a definition that is in that document and I think it is very apropos, so I pulled it out. This is what I am going to be using. And I think this covers the spectrum of things that we would put under the general category of a natural analogue. It is a geologic system which one or more processes analogous to those that may exist to a site being characterized as a potential repository and/or induced by the storage of radioactive wastes are thought to be operating over long time periods or spatial scales.

The study of natural analogues is not a panacea, but it must be an integral part of a total program. There has been the point made by virtually every preceding speaker, and I'll make it again. There is no single, exact natural analogue for any site being characterized for its suitability for geologic disposal of radioactive waste.

One question we would pose and hopefully answer over the next couple of days, is in the term of natural analogue, how natural must a natural analogue be? And as Larry Ramspott told you this morning, and as Everett Springer will tell you tomorrow, there are some anthropogenic analogues that also provide important, and possibly unique information on processes that might operate at a potential repository.

What is the role of natural analogues within the Office of Civilian Radioactive Waste Management? Well, I will break them out into three general categories: quantitative, this would be validation of the applicability of process models for performance assessments that Dr. North alluded to; Geochemistry and transport; hydrology and flow; tectonics; material behavior; etc, etc. These are components of studies outlined in the Site-Characterization Plant. And either this afternoon or tomorrow, we will take you through some of those studies.

There is a qualitative to natural analogue studies. That is the benefit of communicating technical information to those that are not technically trained.

And finally, there is a little not so well defined field that could be called, maintaining scientific communication and credibility, both in the national and international scientific and technical community.

Our involvement in natural analogues has three general categories to it: the international cooperative efforts; site-specific analogues looking at some particular process that we think may either be now, has been or will be operative at Yucca Mountain; and, the non-site specific analogues.

And something George Birchard said tempted me to draw this out. This is not in your briefing package. This comes from my second talk. But we agree completely that at least for the earth sciences, you have to have an integrated program consisting of a theoretical side, an experimental side and the observational side. Observation can consist of just passive observation of natural systems. Of course, this is where you identify the initial identification of process that might be operative in the system. You may be able to bound process rates that have been operative in the system. And a second point that we will bring up in Charlie Voss' talk is that once you characterize the site, in order for you to approach validating a model, you need to predict how that site and how that system would respond to some forcing function, some stress, put a stress on the system, a thermal load or whatever, and observe the response of the stress system.

I am going to be followed by a team of speakers here. The first speaker will be Bob Levich of Department of Energy's Yucca Mountain Project, who will talk to you about the international program and some of the efforts we have going in the international areas. Some of which you have heard about from NRC and other participants. Mike Shea of Terracon will talk specifically about DOE's involvement in Pocos De Caldas. Dave Curtis of Los Alamos will talk about natural analogues and performance assessments, a historic perspective. Dave has been around the program for quite awhile. Charlie Voss of Golder Associates, will give you a perspective from our involvement in INTRAVAL, where we will talk about the stress systems.

Now, if Bob Levich will come up here.

DR. LANGMUIR: Russ, before we continue, we have missed our scheduled break by about 20 minutes. Might it be appropriate for ten minutes at this point before Bob starts?

DR. DYER: I think it would most appropriate.

DR. LANGMUIR: Let's do that. Let's reconve at 3:30.

(Whereupon, a recess was had off the record.)

DR. LANGMUIR: Let's reconvene.

DR. DYER: Our next speaker is Bob Levich of the DOE's Yucca Mountain project. I am going to give a brief, modest introduction for Bob here while he blushes.

Bob has been with the DOE for 15 years. He has spent seven years with the High-Level Nuclear Waste Disposal Program at the Yucca Mountain Site-Characterization Project office and before that at the Crystalline Repository Project Office. He has eight years with the National Uranium Resource Evaluation Program. He is currently the Yucca Mountain Project's International Program manager and serves as the principal interface with foreign national high-level nuclear waste programs. He is delegated as the U.S. representative of several committees of the Nuclear Energy Agency of the Organization for Economic Cooperation and Development.

Bob is going to talk to us about some of the international programs that the Office of Civilian Radioactive Waste Management is involved in. It is going to be a mixture of past programs, ongoing programs and proposed programs.

So, Bob, without further adieu.

DR. LEVICH: I'd like to thank the members of the Board and especially Jack Parry for inviting me here today. It allows me to celebrate my half century, instead of with my family. So, it is very appreciated.

Much of DOE's current international natural analogue program is derived from programs that were originally begun at the Crystalline Repository Project, DOE's Chicago, DOE's former second repository program.

The CRP had two basic concerns that were responsible for us stepping into natural analogue studies. Concern number one was, do numerical models and data collected in laboratories, realistically portray geologic phenomena over geologic time? The second was the fact that we wanted to be certain whether there were any interactions between materials and processes which control the transport of radionuclides which exist in nature but which had not been considered in the repository program.

My presentation is basically divided in two phases. One, the organizations related to the International Natural Analogue Program; secondly are four projects which have taken place, are taking place, or are proposed.

To start with a little history of natural analogues in the international community and the main international organization for scientists and other people concerned with natural analogues meet and exchange information as the Natural Analogue Working Group. This is sponsored by the European community, the CEC and has as its members, not only the nations that are interested in high-level wastes, but a number of other nations as well. You can note among them essentially all of the countries that have major high-level nuclear waste programs.

The Natural Analogue Working Group has three basic objections. First, is to facilitate interaction among investigators who are actively involved in natural analogue studies. Secondly, to promote discussion among investigators, regulators and technical managers of nuclear waste programs. And last, but certainly not least, to provide a forum for communication between those involved in safety analyses, performance analysis and the natural analogue investigators.

This is the basic structure of the Natural Analogue Working Group. The CEC is the sponsor or the umbrella organization, but essentially the basic work and planning is done by the core group of the Natural Analogue Working Group or NAWG as it is called. We have two U.S. representatives to NAWG. One is Linda Kovach who was unable to make her presentation today. The second is Michael Shea who will make the presentation after me. The NAWG core group is responsible for developing NAWG meetings, agendas for the meetings, planning symposiums and developing NAWG publications.

This is a little bit of a history of the NAWG. It began with the planning meeting in 1984 for which the Crystalline Repository Program, DOE Chicago was the host at Lake Geneva, Wisconsin. NAWG meetings have taken place approximately every year or two years since. The last meeting took place last year in Scotland and there was Pocos De Caldas symposium that took place with the NAWG meeting. And the next meeting is scheduled for some time in 1992.

Now, based on the contacts that our members of DOE had with NAWG, it became obvious that there were a low of people out in the international community with broad experience in developing multi-disciplinary natural analogue programs. So at the time that we recently decided to look at our natural analogue programs, look at a strategy and look at particular programs, it was determined that we really would like to have a peer review group who will examine our program and give us input onto that program. And this peer review group, we will reach into the international community, and this is one of the best examples of technology transfer from the international community. I believe we are going to have a similar meeting in July on international programs and we'll be talking about more technology transfer there.

Despite the title of the slide, actually the first three bullets do refer to planned role of the peer review group. It is basically in order. We expect that they will first review the draft natural analogue study that the Department is putting together. Secondly, they will interact with YMP participants who are actually doing studies for which natural analogue programs might be beneficial.

Thirdly, they will advise DOE on revising the natural analogue strategy plus specific programs which are applicable to the Yucca Mountain site and other programmatic needs as well.

We expect that the peer review group will save us both time and cost and help us develop a strong and workable strategy accompanied by a practical field program, including both ongoing international programs and proposed sites for domestic or international programs.

I'll go to the second part of the presentation which is a discussion of four particular study areas. I would like to note that I am presenter and coordinator of this, but I am not the technical expert on these sites. I am very fortunate in having around me all the technical or many of the technical experts: Dave Curtis and June Fabrica-Martin of Los Alamos, Ken Krupka and John Smoot of P&L, Michael Shea of University of Chicago. Any specific technical questions involving these particular studies if I can't handle it, they certainly can.

Natural analogue sites have been chosen not only because they are good sites and appropriate for the study of the movement of radionuclides in nature, but secondly, it is a matter of an opportunity having a program presented at a time where you can go into it in a convenient location and having the funding to do it at the same time.

George has already taken all the good geologic maps and cross-sections and there is no reason for me to repeat them. This is an aerial view of the Koongarra site. The main Koongarra fault runs through here and you can see all these drill roads essentially in which drilling was done for looking at the deposit.

DOE cannot claim to be an original participant in ARAP. As a matter of fact, the NRC has been involved in it for many years. The Alligator Rivers Analogue Project, per se, began in 1987 with the participants listed and the Australian Nuclear Science & Technology Organization as manager. Phase II began in 1990 and DOE is a participant in Phase II.

There were three principal objectives for the ARAP program. One, to contribute to development of reliable and realistic models for radionuclide migration; secondly, to develop methods of model validation using lab and field data from Koongarra; and, thirdly, to encourage maximum interaction between the modelers and those conducting the field studies. We have this again and again from all the investigators.

This is a list of the principal ARAP sub-projects. I am not going to bother to list them or describe them now. I think George did a very good job on some of them.

I will concentrate here on DOE participation in the ARAP program. As I said before, we will participate in the final two year phase beginning in 1990. Our participation will be to support Los Alamos' studies of plutonium, technetium and iodine. Los Alamos has been a participant at a somewhat lower level for a number of years, and we hope that by funding them for additional measurements we can get a much better handle on the movements of plutonium and technetium at the Koongarra deposit.

In addition, we will be supporting the work of Pacific Northwest Laboratory. They are going to be attempting to validate conceptual models, as well as numeric models of hydraulic flow through fractured rock using both discrete and equivalent continuum models integrated with the geochemical data from Koongarra to hopefully to validate flow/transport/geochemistry over geologic time scales.

Among the benefits for DOE, maybe the most important are being able to test models in hydrology, geochemistry and radionuclide migration and to demonstrate which data are needed to adequately characterize a site and provide confidence in modeling results.

Additional benefits are developing transferable approaches for predicting the evolution of geohydrologic and geochemcial systems, and providing data on past climatic effects on the formation of uranium deposits and radionuclide transport.

Project number two, Cigar Lake. The Cigar Lake Analogue Study is taking place in Saskatchewan, Canada. There is a Cigar Lake deposit. I might note that these little other squares note other similar protozoic unconformity type uranium deposits in the Athabasca Basin.

The Cigar Lake uranium deposit has been studied since about 1984. The current project began in 1989, and is running through '92. And there is most probably second phase which will run from '92 to '96. The managing participant is AECL, an the organization that is different from Alligator Rivers which is a multi-national project under OECDNEA. But this is organized by a series of bilateral agreements between Canada and other participants.

AECL has been working SKB of Sweden for several years. We expect that within the next few months DOE and AECL will sign a technical cooperative project agreement, which will cover a wide range of technical areas, eight specifically, but will include Cigar Lake. And there is currently ongoing negotiations with NIREX of Britain.

DR. ALLEN: Do these involve a financial commitment on part of the DOE?

DR. LEVICH: Yes. The Canadian studies do.

DR. ALLEN: In other words, you have to commit yourself to certain kinds of studies.

DR. LEVICH: Right. There is a detailed, technical program with detailed work scopes that have been negotiated over a period of several years and it will involve cooperation between the Department of Energy and AECL on a wide range of technical issues with work being done both in Canada and in the United States.

DR. ALLEN: For example, what kind of personnel commitment is involved by the DOE?

DR. LEVICH: It depends on the particular study. On Cigar Lake, I think it is several FTEs a year. The program is approximately a commitment by DOE for about $20 million over five years. And a equivalent commitment from AECL. Not is not the analogue study. That is the entire cooperative agreement, or what we call Subsidiary Agreement #2 with Canada.

The Cigar Lake uranium deposit is an extremely large and rich uranium deposit; 323 million pounds of uranium. That is one of the largest. The average gained is significant, 14 percent. And if anybody has been involved in uranium deposits in the United States, this is far more than ten times the average grade of uranium deposits in the United States. It may be approaching two orders of magnitude, actually.

The uranium ore ranges anywhere between one and 65 percent. I'll note again, one percent uranium ore would have been extremely high in most deposits. There are very few that average that.

The Cigar Lake uranium deposit lies about 430 meters below the surface within the Athabasca sandstone just about the contact with the Archean basement. Here is the Archean shield, the ore deposit: clay rich halo, altered sandstone, quartz cemented cap.

Cigar Lake is a particularly interesting deposit in the fact that it may be an in member of reduced ore. I think Dave Curtis put it that way yesterday when we were discussing it. It is a quite reduced deposit that has been essentially out of the main groundwater flow system for an awful long time. There are no detectible indicators of the uranium ore deposit at the surface, even though we are dealing with an ore body that is 1.3 billion years old. There was no radiation at the surface. There was no geochemical indicators. There were no fission daughter products.

DR. ALLEN: Why did they drill there?

DR. LEVICH: Great question. I think part of it is those other little uranium deposits that you see sitting around here (indicating), there is a whole number of these uranium deposits scattered around the Athabascan shield. And I understand this was blind drilling. It sounds very expensive.

DR. ALLEN: There may be many more Cigar Lakes out there.

DR. LEVICH: There may be many more. I don't think the U.S. Uranium Industry has an hope of recovery.

Another very good reason for studying Cigar Lake is the great deal of data that exists. There is more than 180 bore holes and 80 kilometers of drill core. Collected data concerns: mineralogy, geochemistry, hydrogeology and groundwater chemistry. And one of the reasons this is being set up as a series of bilateral agreements is that AECL has an agreement with the Cigar Lake mining company. They have the access and therefore the other agreements are with AECL. So the Cigar Lake mining company doesn't have to deal with a whole bunch of different international organizations.

DR. LANGMUIR: Aren't the French involved in this in terms of the mining process?

DR. LEVICH: Yes, they are.

DR. LANGMUIR: Yet, they are not among the participants.

DR. LEVICH: No, they are not. They are interested in the uranium ore for the french reactors. But, apparently they have not been interested enough to join the analogue program. The French are interested in other analogue studies as we'll see when we get to Oklo. Several other countries have talked about NAGRA for example. But, so far those are the only participants.

One of the most significant objectives of the Cigar Lake study is to observe processes both far-field and near-field environments involving radionuclide migration, retention, and both fracture and porous media flow. We also hope that we will be able to collect data for testing and validating radionuclide transport models.

This is a list of some the studies that will be conducted--that are being conducted or will be conducted at Cigar Lake: Trace-element distribution and transport; migration of selected radionuclides; effects of introducing oxygen-rich groundwaters on reduced uranium ore during shaftsinking. And I believe the shaft is being sunk there right now. Mining has not begun, but they have started to sink the shaft.

DOE participation will be by Los Alamos and Lawrence Livermore National Laboratories. This will include: Measuring the concentrations of technetium, plutonium and iodine and calculating the equilibrium abundances of these elements in both rock and water samples; modeling the geochemistry of groundwater composition under both reduced and oxidizing conditions; calculation speciation and solubility equilibria emphasizing the same three; technetium, plutonium, iodine, as well as uranium; predicting stable mineral assemblages; emphasizing secondary uranium minerals EQ-3/6 again; perform partial validation exercise by comparing modeling calculations to the field observations.

DR. PARRY: What are these sources of the technetium and iodine?

DR. LEVICH: The natural uranium, I believe.

DR. PARRY: Just the natural.

DR. LEVICH: Technetium is a fission product.

DR. PARRY: Then you've had a small amount of fission going on there?

DR. LEVICH: In U-235 you have a small amount of fission going on.

DR. PARRY: Spontaneous.

DR. CURTIS: Spontaneous fission of uranium 238 is one source. Neutron induced fission, U-235 is another source, although probably a rather small one. But not a natural reactor.

DR. LEVICH: Now let me talk about a prospective or possible DOE and perhaps NRC participation in another natural analogue program. I'm sure you have all heard of Oklo. In Dave Curtis' presentation, he will describe the past Oklo project, the Oklo natural fission reactors.

The location of the project is Oklo at Oklobungo uranium deposits in Gabon, which was formerly French Equatorial Africa. The duration for the newly proposed project, which the French refer to by that title, Oklo as a natural analogue project, is planned for 1991 through 1994. The CEA, France's Atomic Energy Commission, is the managing participant. And there is a whole list of possible participants. It will be a CEC sponsored program.

To say a little bit about the Oklo natural fission reactors. It is a unique occurrence. It is the only known occurrence in the world of natural occurred fission reactors. It was discovered during open pit uranium mining in 1972. It wasn't actually discovered in the mining but in the sampling during the planned enrichment process when it was found that the amount of U-235 was not the predicted amount of U-235 of natural uranium which had been found everywhere else in the world except at this location. There were checks of samples and then they went back and looked at the deposit and they discovered that there were fission daughter products. And some of the ore is very depleted in U-235.

The reactors are contained in uranium deposits and uranium miners of course are not interested in the uranium at the reactors when they are depleted. And you want as much U-235 as you can get or you want normal amounts to be able to enrich it. You don't want uranium that is depleted in U-235.

The rocks are about 2.1 billion years old. They are sedimentary rocks of the Franceville Basin in Gabon. The uranium ore that forms the reactors apparently achieved criticality, the latest dates I have seen are between 1.9 and 2.0 billion years ago. I think the old literature had something like 1.7 to 1.8 billion years. The French are using these dates now.

The first discovered reactor zones were discovered during open pit mining and lie very close to the ground surface. And you can see one of the reactor zones here (indicating). This is the reactor zone here, and as you see it is within the weathered rock and the radionuclides have been partially distributed by weathering effects close to the surface.

DR. NORTH: Has that been excavated at all or is that simply natural rock?

DR. LEVICH: No, I believe that it is the pit wall. I believe that is the pit wall of the deposit.

This was one of the reactor zones that was studied during the earlier Oklo studies in the 1970's.

DR. NORTH: What fraction of uranium in the minable grade ore has had the reactor effect? Are we talking about a very small fraction of this total uranium ore body?

DR. LEVICH: The answer is yes. There is approximately, I think some of the reactor zones in the area are up to numbers 20 and 21, but that is actually outside the Oklo area. The Oklo area had something like 14 or 15 very small reactors. They actually form pods of flat lenses and by far the majority of the uranium ore has normal levels of 235.

DR. NORTH: As I understand my physics, maybe one of you could refresh this calculations. This reactor phenomenon was only possible very early in the history of the earth, when there was a lot more U-235 to 238, relatively because of the decay processes involved. So, essentially anywhere else you found a 2 billion year old rich uranium deposit, you might have had the same phenomena but we haven't found any of the others.

DR. LEVICH: That is correct. Right now there is approximately I believe .71 percent U-235 in natural uranium ore. Approximately 2 billion years ago, it was on the order of 3 percent, I believe of U-235. So, the combination of the

3 percent, plus pods of this ore run as high as about 60-65 percent uranium, and then having water available and not having any of the poisons, rare earths, nickel or anything that would prevent the reaction from taking place. So it is a very unusual occurrence. Similar deposits have been searched for but none has ever been found. And they've looked in old deposits. Apparently the combination has never been found anywhere else or never existed anywhere that has been located so far. But modern uranium deposits, no matter how rich they would be could not go critical because of not having that much fissionable uranium in it.

There are a number of benefits of Oklo. One would be understanding the effects of climate and weathering on radionuclide transport. Because the new Oklo project has one additional factor that the old one didn't that a number of reactors that were known have been actually mined to underground and have been drilled into. These reactors are about 300 to 400 meters below the surface. And as you see by looking back and forth between the others, you can see the weathering effects here, but on the reactor zones that will be the primary concentration for the new study, they are very fresh and unweathered. And by comparing the studies of the two, we can see the effects that weathering and surface redistribution and surface groundwater may have had on it.

DR. DEERE: Is this photo also of an excavated pit wall?

DR. LEVICH: No. This is a mine wall about 300 to 400 meters underground. So it is an excavated underground tunnel.

An additional factor in the new study will be the fact that in the old studies there were no baseline geology, hydrology or geochemistry. It was essentially going in and collecting the sample and everyone having a great deal of fun studying those samples. And it was a great deal of fun and a lot of very useful information came out of it. But there was really no understanding of the relationship between the natural reactors, the redistribution of the radionuclides in relationship to the regional and local geology, hydrology and geochemistry. So, that will be a major part of the new study.

And so I note that baseline studies of geology, hydrology and geochemistry will be done as a part of the program and again we will be concentrating on these previously unstudied reactor zones.

This slide and the following essentially lists some of the studies that will be taking place by the French, the European Community and some of the other participants at Oklo. So it is a widespread multi-disciplinary study to understand as much as we can gain on the Oklo project about the Oklo reactors.

One of the things that we hope is to be able to find aqueous tracers within the reactor zones themselves. And then evaluate their movement from the far-field by migrating both vertically and horizontally.

I have been told to stress this last bullet very strongly. DOE's participation right now is up in the air due to budgetary considerations. We very much want to participate. CEA's invitation to DOE is a unique opportunity. There is a short range of the project that is three years. We can get in. We can get the information. We can do the work. But, the opportunity may not be repeated. And I for one feel that it is very important that we fund this program, but there are other urges on this same amount of money.

DR. NORTH: What is the amount of money in question?

DR. LEVICH: The first is that supporting about half an FTE at Los Alamos for the measurements of plutonium, technetium, iodine and probably additional work by Livermore. The first part, Los Alamos' work would be about $150K a year and additional studies by Livermore, Bill Glassly who isn't here today would probably range in the $50-$100K range. It is very small in this program, but very large within budgetary priorities.

This would essentially be DOE's participation in the program by supporting our own investigators. Neither the French nor the CEC is asking for any contribution by DOE for collection of samples or to support other parts of the program. In return, the CEA will provide all data collected to DOE.

DR. NORTH: Could they measure the plutonium? For example, do other countries have the same capability to make those measurements?

DR. LEVICH: No. That gives us a great advantage because everybody likes Los Alamos to participate in their natural analogue studies. The capabilities of Los Alamos' mass spectrometry laboratory unique in the world.

DR. NORTH: So if you really want to learn something about plutonium migration which some of the performance assessments have indicated they might be the leading term that you want to worry about, here is an opportunity where you can get the rock essentially for free and all DOE needs to do is pay for the use of an in-hand technology to do the measurements.

DR. LEVICH: That is exactly correct.

And that essentially closes my presentation.

DR. ALLEN:

is the work you do on a project like this subject to QA?

DR. LEVICH: The basic problem with QA in an international program is the fact that we cannot lay our QA program onto other countries. And the samples that have been collected and are being collected in Oklo and Cigar Lake, are essentially being collected under good scientific and engineering procedure, but not into the quality assurance requirements. Therefore, since the samples have not been collected under those requirements to lay a full-scale QA program on it doesn't seem to make a great deal of sense since you can't guarantee where the samples came from in the first place. And it would be done under what we used to call QA level 3, good scientific practice and good work that is normally done at Los Alamos.

Are there any other questions?

If not, I would like to thank you and I would like to introduce the next speaker.

DR. CANTLON: May we wish you a Happy Birthday?

DR. LEVICH: Thank you.

DR. DEERE: You'll have to do what I did on my 50th birthday. I went in and resigned from the University of Illinois after 20 years and moved to Florida.

DR. LEVICH: Well, I'll consider resigning from the University of Illinois as well. I enjoy what I'm doing too much. I am not going anywhere.

The next speaker will be Michael Shea. Michael is President of a consulting company, Terracon and is currently at the University of Chicago, completing his doctoral study on another natural analogue program which isn't being discussed here and that is because it is a domestic program at the Marysvale, Utah uranium deposit. And he did this initially as a sponsorship of the U.S. Department of Energy, the

Crystalline Repository Project, and his former employer which is Battelle Memorial Institute, or Battelle's office of Crystalline Repository Development.

Michael is going to speak on the Pocos de Caldas Project. Michael and I had the honor of being asked in 1985 to travel to Brazil and join with scientists from Sweden, Switzerland, Brazil and Great Britain in looking at the Pocos de Caldas site. Initially at the suggestion of Merrill Eisenbud of New York University's Medical School. Merrill had been conducting a study at Pocos de Caldas for over 20 years primarily on health physics effect of the thorium deposit at Morro do Ferro which has been referred to as one of the most radioactive areas on earth.

We visited the area and found that not only was Morro do Ferro interesting, but as you will be able to see, uranium mine at Yuclapros, the Brazilian National Uranium Company was mining was even more fascinating for studying the movement of uranium. Michael is a good friend, and we've brought him in especially to talk about this program.

I will say this, the Pocos de Caldas project is a project that was completed about a year ago. The reports are currently in the stage of either having been printed or in final editing. And Michael is the U.S. representative on the technical committee of Pocos de Caldas project, and is right now preparing and editing a special issue of chemical geology on the Pocos de Caldas project.

So, with no further adieu I would like to introduce Michael Shea.

DR. SHEA: I am going to try to summarize the results of a study that was conducted over five years in about five minutes. Bear with me and we'll be just fine.

As Bob mentioned, the Pocos de Caldas project was conducted in Brazil and there were two main study sites, the Osamu Utsumi Uranium mine and the Morro do Ferro thorium/uranium deposit. It started in 1985 with Bob and I and other people going down there to investigate the feasibility of participating in this study site. And it is still in the final closeout activities in terms of those technical reports and special issue of chemical geology, which will take our technical results into the scientific and public arena.

SKB was the managing participant. And these were the other participants and here is the DOE down here. You can Switzerland, Sweden and the UK.

The DOE was involved from the very inception, both in terms of the technical as well as the programmatic planning of the project. That involved Bob and I going down there and follow-ups as we developed the program. The DOE participation continued in terms of project management with members of evolving membership on the steering committee, which were the

DOE people and on the technical committees. And the DOE was also involved all of the time in terms of actual active research. So it was a full participatory role.

Pocos de Caldas project was studied at an 80 million year old volcanic/plutonic complex, which was approximately 35 kilometers in diameter. And again, the key features of that caldera which is very elevated in uranium, thorium and rare earths was the open pit uranium mine which is named after the Japanese geologist Osamu Utsumi and reportedly the most radioactive location at the earth's surface, Morro do Ferro, which is Brazilian for hill of iron. There are some magnetite dikes. And that is particularly rich in thorium and rare earths. The uranium mine also has thorium and rare earths but it is secondary. Morro do Ferro also has uranium but it is secondary.

Here is a location map. This is South America. Brazil is in this area. And this little red dot here is the study area caldera. Here is a blow-up of the caldera. It has an indurated rim. This is the city of Pocos de Caldas and here are the two main study sites, Morro do Ferro and Osamu Utsumi.

This is a picture of the city of Pocos de Caldas. And this picture is being taken from the vantage point of up here on the rim, on the northern end, looking out over the caldera plateau. And if you can see on the horizon, there is actually the elevated rim observable.

This is a partial picture of the open pit uranium mine. It's a typical cloudy, rainy day at this time of year. There are pools of water down in the pit. And you can see if you look really closely, little blue splotches in there with the brown splotches. The blue areas is where the primarily reduced rock, not oxidized yet, the brown being where it is oxidized from the secondary enrichment and weathering processes there.

This is a closeup of that. You can see the brown and the blue and the sharp contact. The brown is caused by precipitation of iron, oxyhydroxide minerals, and the blue is most likely caused by molybdenum minerals and perhaps also by the disseminate uranium minerals, uraninite, and pitchblende.

This is another closeup of the contact and can you see how sharp it is?

And this is a picture of some samples, hand samples, which are taken. This shows the relationship between uranium nodules and their very close proximity to the redox front. This being the oxidized side; this being the reduced side. And note the sharp contact, and also you can see little ghosts of where uranium nodules used to be. They look a little like white, bleached out spots. And the uranium has been transported and precedes the iron part of the redox front. And there is also some little fine features in the redox front and the iron part, which no one really knows what they are and how they got there.

There were four principal objectives for the Pocos de Caldas project. The first one was basically a study of the hydrochemistry and principally at the mine, and comparing what was observed to what would be modeled using equilibrium models, geochemical models as well as kinetic geochemical models.

The second objective was looking at the influence of colloids, particulate transport for the radionuclide elements that were seen there. The third one was looking at the redox fronts and seeing what control redox processes would have on radionuclide elements.

And the fourth one was looking at the primary high temperature mineralization in the mine before the secondary mineralization at higher temperature look and modeling that and studying that in terms of what was observed and what was modeled.

What I would like to do now is give the performance assessment implications of the results.

The first is that there were natural processes which we observed at Pocos which were not in the models that we used and these models are very likely what will be used in some form in the performance assessment here in the U.S. and have been used and are being used for performance assessment in

Europe. And for example, this included the sorption onto a morphous phase, which morphous phases do not typically exist in your thermodynamic database, or they are not kinetically understood. Microbial chemical reactions, were not in the models.

The flow channeling and matrix diffusion which controlled the movement of the redox front and the elements across the redox front, those are not generally in the models. And redox retardation which was something interesting we observed, for the redox front, where the iron part is delineated in the redox front, on the oxidized side and on the reduced side there was a bump of elevated chemical concentration for various elements on the oxidized side and on the reduced side. And these were for some elements which are not redox sensitive. Apparently it is because they were co-precipitated with iron oxyhydroxides, and that is not in the models.

DR. LANGMUIR: Does it really need to be in the model, Michael. It is not in the oxidized system at Yucca Mountain. Are you going to have redox interface?

DR. SHEA: That's a valid question. If you were going to model, if you need to model redox front migration, then you would want to have that. You may not need to have that in unsaturated. But if you have to go to the regulatory body and say, well if it is saturated and you have your repository and there is air that has been introduced and you've got an oxidized repository setting in a reduced environment, you've got a redox front, you may have to explain and model what that redox front does.

DR. DOMENICO: Excuse me. This is a conclusion that is based on the fact that you are saying, that based on your data, your analysis, these things have to be incorporated to account for what you have seen, is that correct?

DR. SHEA: They were not in the models when we went to model them.

DR. DOMENICO: But you need them to describe what you see.

DR. SHEA: If you want to model it, that's right. If you want to quantitatively try to describe what is going on, you can waive your arms.

DR. DOMENICO: Well several of them do the same thing. The chemical reactions, the diffusion, and the redox retardation, they all do the same thing in terms of just retarding what you are observing.

DR. SHEA: Right. But the microbial one--this would be conservative. If that's occurring then that is good. These other ones may or may not be conservative. This is conservative but that is not necessarily conservative. If you've got a microbial catalyst reaction going on, that could make the transport of certain elements occurring faster than what you would normally model without know it.

Secondly, we found that there were either mineral phases observed that were not present in the thermodynamic database, or the values that were in there were off. We had five basic geochemical models that we used and compared. In general, there was very good agreement between the models. But there were certain instances for certain elements or certain models that gave either overly conservative or very under conservative values for solubility speciation, which would be of concern for a performance assessment.

DR. DEERE: Let me ask a question. At what depth is this front, or is the weathering extending down below the present surface.

DR. SHEA: You already appreciate that this is an open pit.

DR. DEERE: Yes.

DR. SHEA: What has basically been done is that they have gotten rid of a lot of overburden of weathered rock and exposed and got down to the redox front, which is where this secondary highly-enriched uranium is. So economically speaking, they wanted to get down to the redox front. And it is all fingered, it's controlled by fracture zones in this rock. So you've got fingering of oxidized rock and the redox front has this very corrugated look in three dimensions.

As they go on down into the open pit, you sort of see redox, reduced rock, oxidized rock and just keeping your way down. Finally at some point in time they will get down below any of the secondary enrichment.

DR. DEERE: Are they at the natural water table at the present? Are you have water in flows into the pit?

DR. SHEA: There is one artisan flow, but it is at the very bottom of the open pit mine and it is drained. It has perturbed the paleohydrologic regime, putting the open pit in. There is now water that comes in from sides. It didn't used to go that way. It used to go out.

DR. ALLEN: But do you know this is related to the present environment or something inherited from a former environment?

DR. SHEA: The redox fronts are old. The age is not particularly known, but it certainly is not occurring rapidly today. We can look at the uranium series data and it shows that the redox front is moving very slowly. But we did look at the influence of the redox front to waters as they are passing through it today to see what the influence was if there was a water passing--going from oxidized reduced, oxide reduced. We did look at that also. But basically we found ourselves having to try to unravel the paleo secondary regime.

DR. CANTLON: And the finger penetrations are faults, you say primarily?

DR. SHEA: Fractures. Jointing or something like that.

DR. DEERE: Well this is fairly typical in one of the deep excavations in Brazil. But, still this inter-fingering usually takes place near the transition, where you go from the oxidized into the un-oxidized materials. But they will extend down in no more than several meters in general.

DR. SHEA: That's right.

DR. DEERE: And I would think above that you could have as much as 150 or 200 meters of completely oxidized materials. We have very great weather at the Ita Vita Mine (phonetic) and some of the other projects.

DR. SHEA: That's right. You have very weathered rock. And then you get down into something that is much more competent, but oxidized. That is exactly right.

Continuing with this, echoing what I said about the comparison of the various hydrochemical models, we found that the current approach to calculating solubility limits was fairly robust and it worked well with just a few exceptions.

The models that we used, many of them tended to be conservative and in some instances overly pessimistic if you will, and you may think, that is a great thing, but maybe from a cost benefit point of view, you may not want to do that. You don't want your model to be overly, overly conservative. For example, we found that there were certain reactions again with these oxyhydroxide that the sorption appeared very much to be irreversible especially in the time frame we were looking at, not reversible as is treated in the models. And that would give you very conservative values.

And also, we looked at the colloids and we found, particularly at the Morro do Ferro which is where it was studied mostly, there was a concern at one time, there still is a concern, but it was presented that colloids or particulate materials that might be passing through the rock and in the water, various elements, for example thorium could become chelated on those particulate materials and pass through the rock more quickly not being sorbed on the mineral phases, could act as a short circuit and this sort of phenome-non is not in models and has not been thought about previously.

What we found at Pocos, is that indeed the thorium does chelate on the colloids, but the colloids were filtered out in the rock. The net result was that there was no thorium transport in through the rock. It got caught up on the colloids, but the colloids didn't go anywhere, and so it turned out to be a retarding phenomena.

DR. DOMENICO: Is that an observation or a model result?

DR. SHEA: That's an observation.

DR. DOMENICO: I'd like to make a distinction between the observations and model results if I can.

DR. SHEA: These are observations. Yeah, that is an observation.

Now that should be, if possible reproduced in other areas before we start saying there is no colloid problem. But at Pocos, the evidence was in that direction.

Finally, we used the same physical and chemical models that we used to model the Breccia pipe, which was the conduit for the hydrothermal fluids of the primary uranium realization at the mine and turned it onto various parameters that we got for a hypothetical repository for the same rock type there to see what sort of interesting results we would get in terms of scaling. It turned out that the circulation system was remarkably similar for the hypothetical repository that we turned on and let it run. It was a 70,000 metric uranium ton repository, and the heat would come from that to what was observed for the Breccia pipe, although the Breccia pipe was a thousand times more altering than the repository was. The repository was 1/1000th as effective as altering the rock in our model as the Breccia pipe. The natural system was stronger.

And now I would just like to kind of arm chair lessons learned observations from Pocos.

The first one was sort of the lament of the geologists I guess or anybody that goes out into the field, that there are heterogeneities in the physical and chemical properties that we saw there that just made it at times almost impossible to properly characterize. This is what you will find, I think in any site including a repository site. And in that vein we found that robust models are required in order to interpret the data that we did observe. And that would be very likely the same thing that will need to be done for the repository.

The third one might be a little surprising because after everything I have said about the movement of uranium, but the key thing here is that there was no large scale rapid transport of radionuclides that we observed at Pocos. Though things are moving around, it is very slow.

Fourthly, data collection cannot be rushed, no matter how much money you can throw at it. I'm reminded of a democrat trying to fix a flat tire throwing dollar bills at it. We had all our resources set, and we had all our own smart people out in the field, and midway through the project, we found ourselves throwing out bad data.

DR. ALLEN: Is that an observation or a fact?

DR. SHEA: That is a fact.

So you've got to take it slow. You can't rush it.

Another thing that we tried there, we tried a fairly novel in situ speciation measurement where you take a water sample and you try to determine what is in the anionic state, what is in the cationic state, what is maybe kelated with carbon and stuff like that. And that was very important in comparing, because some of the models give those results. And what oxidized state those species are in is very important in terms sorption for performance assessment.

No matter how bad you may want to find out something at a study site, sometime you have to cut your losses and walk away. It will not be possible to figure it out. It can be tantalizing, you can taste it and smell it, but you can't get it.

This is addressing sometimes the criticism of the transferability of results at various places, natural analogue studies here; natural analogue studies there. The philosophy here is that if you can make an adjustment or refinement of change to the fundamental things that you are using in order to study a natural analogue site, then that will be transferable to any other site, assuming that what it is that you adjusted is something that will also be seen there.

And the last two are along the lines of orienting the natural analogue to performance assessment and getting the performance assessment people involved. They are a great opportunity for performances from people to cut their teeth. And the two schools of people, the people out in the field, the geotechnical people as well as the performance assessment people need to get together as soon as possible and start discussing things. Both need to realize that some things will not be possible to get, especially the performance assessment people. They've got little blanks on their computer sheets and they want a value and they just may not be able to get it.

And now what I would like to do is conclude in terms of one of the slides that Bob put up.

DR. DOMENICO: You missed a slide.

DR. SHEA: I did.

DR. DOMENICO: I'm very curious about the slide you missed. Provided data for parameters including.

DR. SHEA: I pulled that one out. I did, in the interest of time only.

DR. DOMENICO: Is that a fact?

DR. SHEA: Sure. Is this the one?

DR. DOMENICO: That's the one.

DR. SHEA: Yes. I think so. Yes. Yes. Yes. Sure.

DR. DOMENICO: I'm satisfied. Now I know why he pulled it. It doesn't tell us anything.

DR. SHEA: There was another one that was just as bad and I pulled it also.

DR. DOMENICO: When is this report coming out?

DR. SHEA: The technical reports are coming out as I speak.

DR. DOMENICO: The Journal?

DR. SHEA: Oh, the special issue of Chemical Geology, or the technical reports. They are two different things.

DR. DOMENICO: Well, both.

DR. SHEA: Okay. The technical reports--

DR. DOMENICO: The quickest ones.

DR. SHEA: The quickest one is the technical reports and they are being published right now. They are being published at NAGRA and they are being published pretty much as soon as we get them to them, they are printing them out. And there is 15 of them in the series. By this time approximately three of them should be printed, and the others will be finished within a couple of months at the latest.

DR. DOMENICO: And this whole series will be published in Chemical Geology?

DR. SHEA: No.

DR. DOMENICO: Will some of it?

DR. SHEA: Some of it, yes. But, it will be resubmitted and go through the whole peer review process that you do for a journal article. And there are some things that will be a little different than those papers.

DR. ALLEN: Do you read Portuguese?

DR. DOMENICO: We will get these, won't we?

DR. SHEA: The DOE will get them and I'm sure they would be happy to let you have them.

DR. NORTH: Are all these being published in english?

DR. SHEA: Yes, with abstracts in Swedish, German and French.

DR. ALLEN: And Portuguese I hope.

DR. SHEA: No, actually. We thought about it.

DR. PARRY: I believe I have a copy of the draft.

DR. SHEA: That's right. Jack was there at Pitlochry and he got the draft.

DR. PARRY: I could make that available for you.

DR. DOMENICO: I'd like to see that. Thank you. This sounds like magic.

DR. SHEA: So, in conclusion, what we found at Pocos in addressing what Bob mentioned earlier in terms of these bottom two bullets, I'll let you read them and I'll say this part over here. We found that our results were successful at least partially validating numerical models and confirming or correcting laboratory measurements in terms of the thermodynamic data values and some of the processes we observed.

Also, investigators there identified materials and processes that had not been previously identified as having anything to do with radionuclide migration. One example would be like mineral phases not included in the thermodynamic data base. Another one would be that I don't have here, the redox retardation.

DR. NORTH: Could you tell me what the overall total was of DOE's contribution to this project and what the total funding of the project was?

DR. SHEA: Help me on this one, Bob.

DR. LEVICH: DOE funding was I believe--I was in at the start of it and then I transferred from Chicago to Las Vegas in '86. Our funding was approximately $80,000 a year to the project for initially three years, and then we added a fourth year and possibly a fifth year. We added five years. In addition we funded the work of Michael Shea as a principal investigator. I think Michael is the author of more reports than any of the other investigators or at least co-author of about four of the reports of the 15 reports. So, we funded Michael's work as part of Battelle project that supported the Crystalline program. And then we supported Dave Curtis' work very briefly. He got cut off very fast, I understand.

DR. NORTH: Were you functioning essentially as individuals or did you have a team paid for by the U.S. supporting.

DR. SHEA: Maybe I should put it this way, there were project funds. So all the people who were participating like the USDOE, the UKDOE, SKB, they all put in the same amount of money each year, and those were project funds. And then as best as each project could do, for example myself and Dave being thrown in, that SKB supported other people, NAGRA supported other people and money that the project never saw. But in their time, and even analytically, it doubled the amount of money that people actually really saw. The Brazilians gave us the site and supported us in terms of mining and stuff like that. They were not able to support us with funds.

DR. NORTH: Well, if you put payments in money and payments in kind together, what percentage of the total was DOE's contribution?

DR. LEVICH: Probably about a quarter.

DR. NORTH: As high as that?

DR. SHEA: I would have guessed a fifth.

DR. LEVICH: Well if you throw in Brazil's effort, probably it would go to a fifth, because it was Sweden, Switzerland, Great Britain, United States and Brazil. So, maybe 20 percent.

DR. SHEA: Something less than 20 percent, probably.

DR. NORTH: So overall, next to the numbers I am used to thinking of for the DOE project, this is very, very small scale.

DR. SHEA: Yes, but it also had an inordinant amount of visibility from the DOE also because it was international. But it was small.

DR. LEVICH: We think we got a lot out of it for the amount of money we spent.

DR. NORTH: Well, given your documenting, how much you got out of it, I think the question is what is the potential for getting more of such high grade ore out of similar excavations?

DR. LEVICH: Personally, I think it is very good. I think Oklo would be an example of the essentially very similar, not only supportive work of Los Alamos with Livermore or any other participants who want to join in. It is essentially supporting the work of Americans in the United States at their laboratories.

DR. NORTH: I think the question for us all to talk about at the round table tomorrow, are what are the opportunities for supporting performance assessment out of analogues. Oklo sounds like an excellent one from what I've heard.

What are some of the other good opportunities and why are they very good from the point of view of supporting performance assessment? I have heard a great deal from the European side with respect to some of their proposed repositories. We've got one in the tuff and it seems to me that much could be done beyond what I think is in place in making a bridge between performance assessment and analogues. For a start you want two communities to talk to each other but then we want to focus on some very specific items. I think this project has shown us some excellent examples of how this kind of cooperative effort can lead to some very important new knowledge for performance assessment. The question now is where else can we do it and what can we expect to get. And then I think you have a case for why it might be worth a lot more money than this project cost.

DR. CURTIS: I was only peripherally involved in the Pocos de Caldas work, but in my opinion, one of the reasons that it was so successful is because it was incredibly well-managed. Very beautifully managed. The people involved in it were committed up to their teeth. That is why you got so much value for the money.

DR. NORTH: Is that SKB? Maybe there was some lessons to be learned there?

DR. CURTIS: Well, I believe that whenever you find the SKB involved you are going to find a well-managed project. That is my prejudice.

DR. LEVICH: The same.

DR. LANGMUIR: Questions? Further questions from the Board at this point for either Bob Levich or Michael Shea, or from the audience.

MR. EISENBERG: I'm Norman Eisenberg from NRC. The slide that Michael had over there, the first bullet, can you tell us what the numerical models were that were validated and what you mean by validated?

DR. SHEA: The variable codes were codes such as MINICULE, KINTARD EQ-3/6, your basic, typical geochemical code. What I mean by validating is we compared what we observed at Pocos to what the models would show. And these predictions of the models would unblind. We sort of gave them starting groundwater compositions with real values and they knew what the rock values were and they turned them on. And then they came back and they told us, well here is what our model showed and then we showed them what was really observed.

Then we actually did two phases. They learned their mistakes on the first time, so they went back and did a second phase and the results were much better, the coherence. But, there were still things that were inherent in the models and in particular they the way models were treated, thermodynamic database or just values, which are granted at least kind of fuzzy that they couldn't overcome.

That is what I mean by validating, that is why I said partially validated. I would not call it a formal validation. I'd want to avoid that.

DR. DOMENICO: Those are chemical codes. They don't include things like physical transport processes, things called dispersion and probably diffusion in to the matrix and things of that sort.

DR. SHEA: Those do not. No, they don't. But, we did have those that did and we used them for example on the redox front.

DR. DOMENICO: You did use actual transport codes for some of it?

DR. SHEA: That's right.

DR. DYER: Let's go onto the next speaker who is David Curtis of Los Alamos National Labs. Dave has been with Los Alamos for 16 years. Currently he is the group leader of Isotope Geochemistry Group. Chemistry and geochemistry are his professional interest. Nuclear reactions in nature have been a long-standing theme of his professional career. This interest has been used in meteorics and lunar science. Dr. Curtis has studied the geochemistry of nuclear products in uranium deposits, publishing papers on the geochemistry of fission products at the Oklo natural reactors.

Most recently he has developed methods for studying the geochemistry of natural plutonium in technetium. His work has been in support of programs to plan and develop geologic repositories for high-level nuclear waste. He is going to talk to us about the natural analogues and performance assessment from a historic perspective.

DR. CURTIS: It is a dubious distinction to speak at this time of the day even to such a distinguished body. To prevent total paralysis of the head, I would encourage people to interact. If you have questions, if you've got comments, if you want to throw some rocks, anything to keep the conversation lively at this time of day would be greatly appreciated. I would encourage the Chairman, if things get totally out of hand to bring out the hook and terminate it at some point.

There is a mistake in your agenda. I think the title that is in there has something to do with our studies of technetium, plutonium and iodine. You have heard that mentioned several times by Bob Levich. That work, I think, is pretty immature at this point. We've been doing it for some time but with very, very scant resources. Thanks to Bob's efforts we are anticipating a fairly substantial increase in the resources. That work will be headed by June Fabrica-Martin, not by me. And, I think it holds a lot of promise. But, I choose not to talk about it today.

I was hired into this group in 1978 to work on the studies of the Oklo natural reactors. And so I thought perhaps since I had been involved in this kind of work in one way or another for such a long period of time that perhaps this was an opportunity to present a unique perspective on the natural analogue work. This represents my view of things. It does not represent the view of any particular body. This is just me talking.

I have a message. The message is that at one time, when I started working in this program in the '70s, I think the DOE was pretty high on natural analogues as an integral part of the high-level waste repository program. I think in the '80s this concept has fallen on hard times. I think the Oklo work which I was involved in is partially responsible for the bad name that analogues seems to have achieved in the DOE. And I guess I would attribute that to unrealistic expectations. And I think that is sort of my bottom line at this time, that analogues work well. They are and should be part of all geologic repository programs. But, you've got to recognize what they are. I think the analogues at Oklo and other places is they provided a lot of answers for which there were no questions.

I think if the DOE is going to resurrect an analogue program that they had better make sure that the organization in place is in place, so that the questions are posed in a way that analogues can provide them with useful answers. And what I have tried to do is to provide you with some examples of analogue work which I hope will sort of give some examples of the way I think it should work.

Over the years I have given talks such as this to many organizations and I have tried to articulate the role of analogues in the repository programs, but I found that this nice little report by the National Research Council rethinking high-level radioactive waste disposal, did the job as well as I was ever able to, so the next slide is just a couple of quotations from that.

The role of natural analogues is two-fold. One is, "...check on performance assessment methodology;", and when I talk about performance assessments since I am not representing an organization, to me performance assessment means, do we have any idea what is going on? I mean this doesn't mean any kind of a code or any kind of formalism. This just means, do we have any understanding here at all? That is what performance assessment means to me.

Secondly, and I think this was one that Russ put up earlier in its full context, but analogues provide "...more meaningful than sophisticate numerical predictions to the lay public." I think we had a beautiful example of that today with Dr. Winograd's talk on archeological artifacts. I suspect that talk was the high point of the day in terms of the interest of the entire audience.

These things represented a really unique way for one human being to communicate to the other, even if you don't understand the sophistication of your particular craft. I will give a couple of sort of anecdotes of my opportunities to work in public relations and then I will go and give a few examples of where I think analogues worked well in this regard.

I've been invited and it was much more active in the late '70s and early '80s to give presentations on the Oklo work to a number of public forums; both pro-nuclear and anti-nuclear. And I was always amused because I gave virtually the same talk to all organizations. And I almost always had the same reaction. It is so nice to see work that supports our point of view.

George Cowan in 1976 wrote an article for <u>Scientific American</u> on the Oklo reactors. I have a few copies here if you are interested in seeing them. This has been the most requested technical paper I've ever read. It is a very, very nice exposition on the Oklo reactors and it has appealed to the broad spectrum of the technical public. I think it is a little bit too detailed for the lay public. If you are interested in reading that, if you haven't seen it, I have copies of that. Analogues are very useful for communicating with your technical colleagues.

In the more public vein, this is a little pamphlet called Nuclear Reactions. Actually, when we made this, this was supposed to be public relations. This is called Nuclear Reactions. It was issued by the Wisconsin Public Service Cooperation. The guy called me up on the telephone and interviewed me after I was screened heavily by the public relations people at Los Alamos. And in here they have this great article called Jungle Secrets about the Oklo reactors. There is this communication with the lay public in that way.

I've had a number of people in terms of education call me up and talk to me and ask for technical publications so that they can build a discussion of the Oklo reactors into their academic curricula. I think in terms of communicating with the public, natural analogues are the best. People identify with them real, real well.

That is the last I'll say about public relations because there has been floating around this concept of what Peter Sergeant (phonetic), called the warm tummy feeling, the warm fuzzy feeling. This is really a concept which I hate. I implies that analogues can only contribute to this business in kind of a soft way and I don't believe that. I believe that it provides hard information which can be very critical to making decisions about design and assessment of repositories.

This a view graph that June Fabric-Martin created. I love it. You've got to ask the right questions before you can come up with the right answers.

The Oklo reactors. I am going to give you three examples of what I will call analogue studies. Unless the Chairman gives me the hook before I am through, these are examples that I am familiar with not necessarily that are the best I know of. They are just ones that I happen to know and like.

The first is the Oklo reactors. Let me familiarize you a bit more. Bob Levich was kind enough to provide that slide for me. I brought a cartoon. This is very old. I think I did this in '78. This has been shown many times.

The important thing to remember about the Oklo reactors is that they are a very small feature in the uranium mines. They are basically pods of few cubic meters in volume. The reactor themselves of course are loaded with fission products. They are mostly uraninite. They are anomalies in a sandstone environment. They contain no quartz. The silicon is basically dissolved and transported out during the nuclear reactors. So you have this small volume of reactor, which you see here.  It is surrounded by aureoles of rock which was altered by the reactor, the heat and the moving of fluids during the reactor process.  And then that is surrounded by sandstone, which is basically unchanged as a result of the reaction.

Remember that these are very small features.  Bob says there are 20  to 22 of them found now.  There is a number of them and they are sort of strung like beads along the ore bearing strata of the Oklo mine.

Let me talk to you a little bit about a study we did regarding technetium at Oklo.  I am trying to couch this in a way which hope will exemplify the way I think natural analogues are best used.  I am trying to couch it in terms of an assumption, a question that might be asked by people who are trying to understand the system.

In the case of technetium migration, the assumption which I think is still one which is commonly made, is that technetium is a very mobile element.  In fact, I think it is identified in the Site-Characterization Plan as a key radionuclide.  One that is a key problem.  The major reason for that is because it is thought that if it is ever impacted by moving fluids it is going to just haul out of there.  And the reason for that is that technetium is believed to be soluble under a broad range of natural conditions.  In this soluble form it is thought to exist as an oxyanion and hence be poorly sorbed.

This is some formalism that you try to familiarize yourself with. It is work we did at Oklo. This is a diagram which allows us to compare the relative retention of fission products. On this axis we have the fission production ruthenium. This is normalized abundance. And I won't boar you with the normalization process. On this axis we have the fission product technetium; again, the normalized abundance.

On this diagram, if there were no fractionation between the two fission products ruthenium and technetium, the data would fall on this line. This is a line of no fractionation.

If technetium were completely removed from the reactor zone and ruthenium retained, the data would all fall along this axis. If the ruthenium were completely lost and the technetium contained, the data would fall along this axis. If the technetium were partially retained relative to ruthenium, it would fall in this region. If the ruthenium were partially retained relative to technetium, it would fall in this region.

DR. NORTH: One basic question for some of us that haven't been in this. When you mean normalized abundance, are you normalizing for the decay chain?

DR. CURTIS: No. The normalization had basically--if I put units on this it would be in fissions per gram. I am basically taking the measurement and calculating the number of fissions that is represented by that measure.

DR. NORTH: But what you are recording in the data are the stable end products.

DR. CURTIS: Yes.

DR. NORTH: And then you are trying to track back to what was originally made when the fission occurred and then what became--

DR. CURTIS: I'm making no assumptions here about anything. These are observations. Now, the partial loss or partial retention may have in fact been done. In fact, the technetium because it is radioactive, is a transient element.

DR. NORTH: So we are not measuring it directly.

DR. CURTIS: Actually, I am measuring what I call fossil technetium which is isotope ruthenium 99. But that becomes rather involved to explain that, so I try to avoid it.

DR. NORTH: So, basically what you are doing is comparing isotopes of ruthenium with technetium?

DR. CURTIS: That's correct.

So, this is the formalism and if everybody is still with me, I'll try to show you where the results fall. If you look within the reactor zone, that is within these areas here, you find that the data fall in this region here. That is, technetium has been partially lost with respect to ruthenium, but not totally lost. There is still considerable technetium residing within the reactor zones. Now this is within the reactors themselves.

If you go out into the sandstone that surrounds it, you find that the data falls here, almost universally. That is technetium which has been removed from reactor zones moved out into the sandstone environment and then clung there pretty tightly.

What the data shows at Oklo is in fact technetium was very effectively retained. Some of it was in fact, we believe, retained in place; never moved at all. And that part which was lost moved a few meters and then basically was retained within the sandstone rock.

Now one of the major criticisms of Oklo over the year was, that is nice, but what does this have to go with salt or basalt or with tuff? This is a sandstone environment. Well, I think that is a valid criticism, so one needs to begin to understand what was the process by which this was retained. That seems like it is worth knowing if one wants to design a repository which is going to retain technetium.

This is a study we did on one reactor zone, reactor zone 9. We took a body of data from that. We measured the isotopic composition of ruthenium, which tells us about the ruthenium and the technetium, and we found this unbelievable correlation; a linear correlation on this diagram. But in the region of fractionation that is deficiency of technetium relative to ruthenium. And from this highly correlated set of data, we inferred that the technetium was being held and that really what was controlling the retention of technetium in the reactor zones was the stoichiometry of some mineral. That is the portions of technetium to ruthenium seemed to be fixed in the rock, even though some of the technetium was lost

So we inferred there was some mineral which was holding the ruthenium/technetium ratio constant, and we went to the literature and found that sure enough in spent fuel in anthropogenic spent fuel, ruthenium and technetium form metallic minerals. So we speculated that what was controlling the partial retention of technetium in these rocks was the formation of metallic minerals in the spent fuel at Oklo just as the spent fuel that you see in a reactor forms metallic minerals. It is my understanding that the French have actually confirmed the existence of these metallic minerals. I think they have actually seen these. This was only a hypothesis in the paper we wrote.

DR. LANGMUIR: Do you have any idea what those minerals are?

DR. CURTIS: They are metals. They are metal alloys.

DR. LANGMUIR: Just those pure elements alone?

DR. CURTIS: All these incorporated in here, ruthenium, technetium and I am not sure what else.

DR. LANGMUIR: In some major metal phase?

DR. CURTIS: Well, they are probably sub-micron sized metallic particles. Nobody observed them for lo, these 20 years. So they must be quite small. They are quite prominent features in anthropogenic spent fuel. But, there you are talking about much higher temperatures and quite different conditions.

This has been published in Applied Geochemistry. I have a few copies of those reprints if you are interested in seeing them.

Now, the technetium which is found in the sandstone outside of the reactor zones, this is only an observation. We know it was retained, but we don't have the foggiest notion of what the process of retention is. Whether this is a surface process or whether it is being incorporated into secondary minerals; we simply don't know. One of the reasons is that the abundances here are quite a bit smaller than here, and when we were doing this study ten years ago, we just simply didn't have the technology to make very good observations there. That technology now exists.

I would think that if one is interested in the retention of technetium in a repository that studies of ruthenium isotopes in the sandstones around the Oklo reactors would be a sort of a number one thing that you would want to look at and try to understand those processes.

DR. DOMENICO: I would think at least a technetium deficiency would be a relatively simple experiment for retardation people.

DR. CURTIS: I'm sorry?

DR. DOMENICO: I would think that your observation on the technetium deficiency relative to ruthenium, could be set up as an experiment in the sorption lab.

DR. CURTIS: Let me try again. This, I believe, was controlled by a solubility issue, that the technetium is forming minerals here and those minerals are basically insoluble in the conditions at Oklo.

DR. DOMENICO: Is it something to do with the conditions at Oklo that is doing this, or is this a general concept?

DR. CURTIS: I think this is probably quite unique to Oklo and I think it represents almost a perfect analogy of anthropogenic spent fuel. I think you probably wouldn't find these minerals any place other than Oklo.

DR. DOMENICO: Okay.

DR. CURTIS: Now the sorption experiments I think are probably up here. And why the technetium is held there, like I say, we don't understand that at all. It's only an observation.

There is a couple of papers here which describe these observations, but again very little speculation as to what it might be.

The conclusion would be, that at least at Oklo, technetium was selectively retained in the rock and there are at least two processes that were involved in that. One, being the insolubility of these metallic minerals; the other being some kind of a process associated with the transport.

The second thing I want to talk about is alpha-radiolysis. Now the reason I bring this up is not because I guess I don't know whether alpha-radiolysis is considered an issue at Yucca Mountain or not. But, the reason I bring this up is I think this is about as good an example of a good analogue program as there is. I hate to sound like an SKB fan, but this was work that was done by the SKB and I think that it is a beautiful example of how a repository program should work and why it works effectively at SKB.

This is one of the SKB reports from 1982. I am sure that Neretnieke and Ingmar Grenthe and Tonis Popp, were all sitting around having coffee one day, and one of them say, Gosh I wonder if alpha-radiolysis is going to be a problem at our repository site? And they said, Well are we going to find out? Well, we are going to hire somebody to model radiolysis. Now radiolysis is the interaction between nuclear radiation and water. It creates short-lived species which are highly chemically reactive. In the laboratory it induces permanent chemical changes. Whether or not this is a process which exists in nature, who knows?

So, they said, well first of all let's see if theoretically we can see whether or not this is a possibility. And they went out and they hired Hilbert Christensen and Erling Bjergbakke. And they did basically a computer study of radiolysis under conditions that were thought to be possible to exist should water intersect a repository of the Swedish type. And they came back and said sure enough, radiolysis might be something that happens.

So the next thing they did was they said, well what is going to happen if radiolysis occurs? And Neretnieke did a model. It is called, The Movement of Redox Front Downstream From a Repository For Nuclear Waste. This is again a model. And then they said, well what is going to be the effect of this moving redox front? And Ingmar Grenthe and somebody's name I can't pronounce and Jordi Bruno did a study called, The Possible Effects of Alpha Beta Radiolysis on Matrix Diffusion of Spent Fuel. These are all model kinds of studies.

Well then they called myself and Alex Gancarz, knowing that we had done work on the Oklo reactors. And they said we are postulating that it is possible that alpha-radiolysis would occur should water impact our repository. It could create a moving redox front and it may have adverse affects on the retention of selected waste products. And they said, can you tell us if there is ever any evidence any place of alpha-radiolysis occurring in nature? And they said, the Oklo reactors is certainly the place in nature where there has been larger fluxes of radioactivity than anyplace in nature. Would you go to the Oklo reactors and tell us if there was any radiolysis there.

Well the amount of money was so small that Los Alamos wasn't interested in this. So, Gancarz and I did this privately as consultants. We went to the literature and much to our surprise, we found this beautiful set of data having to do with the relatively abundances of Iron-3 and Iron-2. This is a remarkable set of data, mostly just published. And what we found, this is a summary of the data, but what we found was in the sandstone, that is the environs around the reactors that the Iron-2 and Iron-3 ratio was about unity in a sandstone, even in the aureoles, that is those portions of the sandstone that have been impacted by the hot fluids and in the fine sandstone. So this is what you see in the surroundings.

If you go down to the reactor fuel, I mean those areas which are 65 percent uranium and just loaded with fission products, you find the Iron-3 and Iron-2 ration is .2. So it looks like iron in the reactor zone is preferentially reduced relative to the environment. And in fact we think that the matrix, that is the material within the reactor zones, but not the stuff which contains such high levels of uranium appears to be kind of partially reduced.

The reason we plotted it this funny way, this is a plot of aluminum versus Iron-2 versus Iron-3, we wanted to see if you could evolve to here from here. In fact we drew some lines suggesting that if you took something up in there and didn't remove iron but merely reduced it, that you could go down into here someplace.

We did some hand waving about the processes, but the Swedes didn't much care about that. All they cared about was, yeah, it looks like at Oklo there was something going on with the redox conditions, so that we preferentially change the redox stage of iron in the reactor relatively to the rock.

They took this as yeah, this is evidence. There possibly was alpha-radiolysis affecting the redox conditions there. And they went on in '88 and published another report. And just before I came, I got a report called, Modeling the Movement of a Redox Front on a Uranium Mine in Pocos de Caldas, Brazil. This is Neretnieke and his students at the Royal Institute of Technology. Basically what they did was to refine their model or redox movement. You saw the redox fronts at the mine at Pocos de Caldas. Neretnieke took each layer and he basically had the information about where the reduced and oxidized areas and put that into a computer. So he basically reconstructed the whole redox front in three dimensions and then tried to model that. So, he basically was using the Pocos as a calibrator for his redox front motion.

I think that is one of the best examples of use of natural analogues I know of, because, it involved all of these interactive processes between the people who were doing performance assessment; the people who were doing models; the people doing laboratory work; and, people working at the field. Just all kinds of interaction. And it has been sustained over six or eight years now.

Finally, and I am now beginning to walk on not such firm ground, but this is the conclusion of our study that in fact alpha-radiolysis may have indeed produced reducing conditions in Oklo natural reactors. So we have basically encouraged them to continue with building up evidence for this process and its effect on the repository.

This is something right out of the Yucca Mountain Project. It is the study of Chlorine-36, which Ted Norris has worked on in the past and June Fabrica-Martin is working on now. And I always liked this because it seems sort of simply elegant. I am not a hydrologist and I don't understand this hydrology stuff too much, but as I understand the assumption some time in the past was that in unsaturated rocks, fractures don't conduct water into significant depths. The processes that were described to me is that the water is basically absorbed or imbibed into the rock matrix at the fracture surfaces and simply just can't move very deep.

The next is the plot of data from Ted's work. This is a plot of the Chlorine-36 to Chlorine ratio as a function of depth in rocks from one of the drill holes at Yucca

Mountain.

Now let me try to explain Chlorine-36 to you. Chlorine-36 is like Carbon 14. It is a cosmogenic nuclide. It is produced in the atmosphere by the interaction of cosmic rays with the atmosphere. It mixes with--it is radioactive and has a half life 300,000 years. It mixes with stable chlorine mostly from the oceans and it is believed that the ratio of the cosmogenic nuclides to the dead chlorine is a function of the geography of where the rain falls, where the precipitation falls, it is removed from the atmosphere as a function distance from the source of the dead chlorine that is the ocean. Stan Davis and Earl Bentley, I think, many years ago predicted what Chlorine-36, Chlorine ratios would be a different locations on the continent. I think they were even surprised at how accurate that seems to be.

At Yucca Mountain the prediction is that the Chlorine-36 Chlorine ratio will be this. I'll note that this is an atomic ratio and I multiplied it times $10^{13}$. So this number is 1 times $10^{-13}$. It is a very difficult measure to make.

And you'll see that in most of the samples, the ratio falls below what they call a meteoric recharge line and this can be explained either by radioactive decay of Chlorine-36 after it has been removed from the source, or it can be explained by dillusion from dead chlorine from the rocks. So, things that fall down here we don't have too much trouble with.

Things that fall up here, you have a lot of trouble explaining those as natural phenomena and in fact there are thought to be bomb pulses. That is the Chlorine-36 is not natural at all. It is anthropogenic. It was formed by the atmospheric tests in the Pacific in the later '50s and the early '60s. And then it acts as an anthropogenic tracer. It was basically deposited around the earth in that time frame, in the late '50s and early '60s and represents anomalies on the natural Chlorine-36 Chlorine ratio. So these numbers are thought to represent anthropogenic Chlorine-36, stuff formed of a nuclear test.

Near the surface, you see high numbers here and these seem to be fairly consistent I think, with what was understood of the infiltration rate in this kind of an environment. But this sample, at 150 meters seems to be deeper than one can explain by process of infiltration into unsaturated rock. And this is taken as evidence that in fact surface water can be transported to significant depths in a reasonably short length of time in unsaturated rocks. Now this is a pretty flimsy dataset and hopefully some day the project will think this is worthy of more study. I am not sure whether you want to call this an analogue or a Site-Characterization activity.

The conclusion, and I am a little embarrassed at putting this up here in the presence of June and Dr. Renegrad who really understand these things. But, the conclusion of this would be that there is evidence that water has been transported to significant depths in the unsaturated rock. And I guess this infers that the flow of water is transient and non-equilibrium along fracture surfaces. I think in fact the performance assessment are the people who are trying to understand the processes of water movement in these rocks, have in fact reassessed this and are using this as sort of the underlying assumptions.

So, those are the examples of things I like and of analogues that I think worked well. I'll note that in general they didn't validate models. I don't think analogues work too well. Note typically they were posed around a rather simplifying question and an analogue was chosen which allowed us to come up with a fairly straight-forward answer. But, something much more than a warm tummy feeling, I think.

Once again the problems with the analogues in the past with respect to DOE's perspective of the things are probably the result of things that are articulated in the rethinking of the high-level radioactive waste disposal volume. The analogues are no good unless they are an analogue of a repository situation. Again I think there has been a tendency to go out and study an analogue without knowing its relevance to a particular situation. And I think that it must address a critical element of the repository performance.

I have this wonderful story. When I first started to work, I went back to the Office of Nuclear Waste Isolation who was sponsoring me and I gave what I thought was this really elegant talk on Movement of the Fission Product Molybdenum out of the Reactor. I mean it was a universal sleep. Everybody was just dozing in their chairs. They said, who cares about molybdenum. So, it has to be something of interest to the people who are asking the questions. And I think that it is the people who want the answers who should be formulating the questions. And they should be working with the earth sciences to say, here is what we need to know, can you find an analogue that will work.

I tried to summarize this in my final slide which is my bottom line. It is the bugaboo of communication which seems to frustrate a lot of our human activities. But in order for natural analogues to work there has got to be effective communication between the people who are doing the performance assessment in trying to understand the problems and the people who are doing the analogue studies.

So the people who are asking the questions and the people who are providing the answers have got to talk to each other or it isn't going to work well. And I think that is why the SKB stuff has worked so well, because often it is such a small program that they are often the same people. People asking the questions and providing the answers are often the very same person or at least they have coffee together once a day.

Thank you, very much.

DR. LANGMUIR: Board member questions for Dr. Curtis?

DR. DOMENICO: We are aware of that Chlorine data and also tritium that supports it. But, this information was brought to us by Ted Norris.

DR. CURTIS: That is correct.

DR. DOMENICO: And I understand he is not on the project anymore?

DR. CURTIS: That's right.

DR. LANGMUIR: Further questions from the Board?

MR. HOXIE: I'm Dwight Hoxie with U.S. Geologic Survey, and I couldn't resist the temptation to comment on the point number 3 of yours. And I think that our longstanding conceptual model of the unsaturated zone processes at Yucca Mountain is based on the idea that we can have very fast, non-equilibrium flow of groundwater in fractures through the unsaturated zone, and this is probably the principal mechanism by which we can get liquid water infiltration into the unsaturated zone.

DR. CURTIS: I was sure somebody was going to nail me.

MR. HOXIE: And I think the performance assessment people and Site-Characterization people are talking to each other even though they are from different camps.

DR. LANGMUIR: Yet, an interesting thing is that on so many of the experiments that have been explained to us over the last two years, they say the results didn't really turn out like we thought, probably because there was a fracture that crossed the borehole. We have heard that at least three or four times on different types of tests. And it seems like it really has been a little bit of a slow process in developing the influence of the fracture.

DR. CURTIS: I really don't want to get into the hydrology modeling and movement of water. That is not something that I am real up on. I just always thought that this was pretty elegant that the observation of the bomb pulse at depth and given the importance of water movement at the site, that that would be something that the project would be quite interested in developing further.

DR. LANGMUIR: Other questions from the audience?

MR. CLOKE: Paul Cloke, SAIC. This again is in the form of a comment or a couple of comments. I was almost ready to come up there when Dwight had not. I should comment that first of all that some of the people at Livermore have been looking at and doing some modeling that seems to be explaining, although it is in the very preliminary stages of this rapid transport of water down fractures. They have the model that seems to do that.

Secondly, I thought I should comment that I think it was just two or three weeks ago that I was one of these technical specialists on the QA audit at Los Alamos looking at June Fabrica-Martin's effort to get things going into proper QA procedures.  Procedures are being written to continue the Chlorine-36 work.  So, that is not being forgotten.

DR. LANGMUIR:  Thank you.

Russ.

DR. DYER:  While Charlie Voss is getting set up, let me tell you a little bit about Charlie.

Charlie works for Golder Associates out of Seattle. He has been with them since 1990.  Prior to that for a decade he was associated with Battelle Pacific Northwest Laboratory. He has over 12 years of experience in geotechnical engineering.  He has been involved in a large number of activities for the U.S Department of Energy's High-Level Nuclear Waste Program.  He has been involved in Site-Characterization and Performance Assessments efforts for the Climax, Hanford and Yucca Mountain sites.

Charlie serves as the Department of Energy's representative to the INTRAVAL project, and also to the site evaluation and design of Experiments Advisory Group to the NEA/OECD.  And Charlie, following on the vein that Dave Curtis started is going to talk to us about the INTRAVAL perspective.

The interaction between modelers and field experimenters.

DR. VOSS: As a possible morale booster, I'll let you know that I am the last guy today.

What I was asked to talk about was the INTRAVAL Project, and give you a little bit of an overview of what it is in 15 minutes. I won't be able to go into a whole lot of detail. But, I do want to point out what DOE OCWRM's participation has been up until now in the first phase of INTRAVAL, and then I'll also get into what we are planning on doing during the second and final stages of INTRAVAL.

INTRAVAL is coordinated and planned out by an organizing committee called a coordinating group. It consists of representatives from the different countries that are involved. I am acting as the DOE representative to that group and that is why I am up here giving this.

INTRAVAL, is the third project in a series of international studies at the Swedish Nuclear Power inspectorate that SKI has initiated. Norm Eisenberg mentioned that INTRACOIN and HYDROCOIN, earlier this morning.

These previous studies each had three phases or levels: A model verification phase; a model validation phase; and then sensitivity studies. For reasons I won't get into, I think the majority of the effort and their success was in code-to-code verification, benchmarking type problems.

INTRAVAL began in 1987 and it consists of two phases, each two years in length. We've just concluded the first phase in October and we are just starting the second phase next week, formally.

Very quickly, the purpose is to increase the understanding of geophysical, hydrological, geochemical phenomena important to transport groundwater flow. There is nothing in here about engineered barriers or scenarios.

And the approach that we are using is to use information from laboratory and field experiments and from natural analogue studies in a very systematic way as input to models and then also for model prediction and experimental output comparison. A traditional validation approach.

Let me just say on these laboratory and field experiments that the first phase consisted of 17 experiments, not of all which survived the entire first phase INTRAVAL. And I'll show you a list of these in a minute and give you a very quick idea of what they involved.

Before I do that, just so you can get an idea, it is a very large program in that there are 22 organizations from 13 different countries that are involved. All of these countries are at least considering geologic disposal for radioactive waste. Obviously some countries are in much more advanced stages of these programs, but in addition to the organizations that are responsible for carrying out the feasibility studies to see whether they want to go forward with us, there is also the regulators and then a few other interested observers.

From the U.S. side there is the USDOE, of course, and besides OCRWM and the Yucca Mountain Project Office, there is also the Nuclear Regulatory Commission. They are heavily involved. There is the WIPP Project; they are involved. We have two observers--I'm sorry, three observers; the State of Nevada; the State of New Mexico; and then also, EPA has an observer.

As I mentioned earlier it consists of these experiments and the models are used to predict the behavior of the systems involved. And discrepancies between the forecasts of results and then what are actually observed in these experiments are discussed. What we do is we hold workshops. They are rather informal workshops where technical presentations are made both by the experimentalists and the modelers. So it tries to accomplish some of the things that Dave talked about in his last slide about to increase communication between those two groups.

During the first phase, we had five workshops for example. And I suspect that during the second phase we will have about the same number.

In addition to discussing the results of these experiments and then also the models, these workshops also provide a rather useful form at least in DOE's experience for us to present related items, like we presented the model validation methodology which we presented to you a couple of times over the years. Plans for validation of the type of experiments that Sandia is doing by Bob Glass which I think you probably heard about. And we've gotten some very useful feedback from the international community.

These are the test cases or experiments that were involved in Phase 1. I won't really get into it in any detail. The highlighted ones are the ones that the USDOE was directly involved. I'll just point out SKB Sweden, this is the Pocos de Caldas that you've heard about and the analogue study here is the Alligator Rivers natural analogue study.

In Phase 1 there was a rather large range of scales. Some of the laboratory experiments were only core samples, a few centimeters in diameter and then all the way up to the analogues. We also have a synthetic data experiment, which I can talk to you later about if you are interested. And, then quite a few field experiments. And we covered a large range of rock types.

DOE's involvement varied quite a bit because of the amendments to the Nuclear Waste Policy Act and then a shift in some of the emphasis in the way money was allocated.

DR. DEERE: What was the test on the clay? Was that just a consolidation test?

DR. VOSS: No, it was actually a transport experiment and it was an intact core sample of clay. And they introduce a radionuclide on one side and monitored the break through.

I should mention that as far as validation experiments, this is not a very ideal set of experiments probably. In the early stages back in '87, we wanted to get the ball rolling. There was a very large range of ideas about what validation really was and how you went about doing it. So these experiments really served in my own mind, as a way to help form some sort of consensus about how we should be going about this. And I think in Phase 2 we are going to have a much better design set of experiments. Plus, a lot of these experiments were already completed at the beginning of INTRAVAL. So there was no opportunity for any of this input from the modelers as to what kind of experiment they wanted to see.

Let me tell you about the G-Tunnel experiment which was one of the test cases. It was a sub-set of this dry prototype type drilling experiment. It investigated the effect of drilling on the hydrologic conditions because one of the holes is drilled dry; an adjacent hole was drilled wet. We looked at the representativeness of core specimen data to see how well that could represent the field scale processes, an again with that type of data we were able to get some kind of idea about the spatial variability in the field.

Alan Flint has given a presentation or two that you have heard on the data that he generated from this G-Tunnel from his imbibition experiments, that came up with some very interesting characteristic curve data. And that all came out of this study.

Here are some of the conclusions and recommendations from Phase 1. I'm sure you've heard all these motherhood statements before, but modelers do need to be intimately involved. It is just like David said. You have to have a problem or a question that you are trying to address when you design these things that everybody can agree upon or more than likely you are not going to get the kind of information out of it that you really need.

Experiments should be ongoing during the validation phases. I mentioned a lot of these were completed even before we started Phase 1. They are interesting experiments. And there was interest in modeling them so we included them. But the problem was is that, often times modelers needed some additional information that if these things had to have been ongoing they could have provided to the modelers.

And then modeling should also be done blind. By that I just mean we should withhold the outcome of these experiments from the modelers so they don't really know what the outcome is. In a lot of cases people were confused and what they were really doing was calibrating their models and then once they could make the output agree with the data they already knew was there, they claimed that they had validated their models.

Over that three year period that we finally got everybody to agree that that was nothing more than calibration.  It didn't really constitute validation.

We are now getting ready to begin Phase 2, or I guess it has actually begun.  We decided to have another group of experiments.  We wanted to concentrate this time on some larger scale experiments.  We felt that we had made some progress in studying processes and we wanted to incorporate the role that structure played in a lot of the phenomena that we were interested in.

They had some criteria though in order for an experiment to be accepted as a test case for Phase 2, one of them was that it had to be ongoing and it had to be ongoing, and it had to be ongoing for the next couple of years.  And like I said some of the conclusions that I mentioned earlier about that it was important that additional information could be asked for and obtained.

DOE and the participants got together about a year ago and we wanted to decide what involvement we were going to have in Phase 2.  What we really wanted to go forward and continue with the G-Tunnel experiments.  There is a lot of ideas we had about additional data and different ways to perturbed that system that would be very interesting.

As you know, G-Tunnel was closed down so that concluded that. We weren't optimistic about getting any permits to start any new experiments. So, we were left with--and this doesn't mean that there isn't a large effort involved, but we are going to be a participate only as a modeler. And I'll show you what the test cases are for Phase 2. And again I am not going to be able to get into detail and stick to my 15 minutes.

One that we've already committed to and we were working on this in Phase 1, is the Alligator Rivers Natural Analogue Study. And we are going to continue our participate in Alligator Rivers. In fact, we are actually increasing our level of involvement quite a bit. And you've heard probably enough about that.

The other experiment that we are going to be participating in and probably to a much higher level of effort anyway, is the Apache Leap experiments. These are experiments that are funded by the NRC and carried out by the University of Arizona. It is actually made up of laboratory and field experiments. There are four different experiments. The one that I want to spend the rest of my time talking about, is the field heater experiment out at Apache Leap. They are going to put in an electric heater and heat up the rock and monitor moisture movement and temperature and all sorts of other things.

Back to this planning meeting that I mentioned a minute ago that DOE had, we knew what the proposed studies were going to be. This tuff experiment that the NRC was heading up was an obvious one that we needed to be involved in. DOE has a great deal of experience in running these heater experiments. Sandia has run a heated block experiment in G-Tunnel. They have also done some small diameter electric heater experiments there. And then Lawrence Livermore National Laboratories did a very nice heater experiment study where they very carefully monitored the hydrologic and thermal conditions around the heater experiment.

Instead of just modeling the heater experiment at Apache Leap, we thought we would do the following as part of our involvement in Phase 2. One would help support the design phase of the experiment. I mean the NRC is obviously very much aware that we've got this experience in-house on running these experiments. There is a big problem with instrument failure, just designing these things so you place the instruments at a point where you are actually going to get some information on the experiment. And they have invited us as the University of Arizona has to be intimately involved in the design phase of the experiment. We are doing that.

The other one is to develop this integrated data base for the relevant G-Tunnel experiments. You take all the thermal data--hydrologic data that we have gotten from the

Sandia experiment and also the Livermore experiments, and then also take the G-Tunnel hydrologic data, the new stuff that Alan Flint came up with and develop this integrated base and model the G-Tunnel heater test, or test with this integrated data set to get practice doing this type of analysis in a very coupled sense. Not just doing the heat transfer, but also trying to figure out what is going to happen to the moisture movement. I suspect a fair amount of calibration and sensitivity studies will go into this.

We may also need to do a few additional laboratory measurements with the Apache Leap tuff to calibrate our models for that site, because Alan Flint's with his imbibition experiments and some of the other ones, I doubt whether they will have that data. And finally, forecast the results of the heater experiment and compare them through the INTRAVAL effort.

Very quickly here are the folks that is everybody in the program. Golder got in there. That's me.

This is my last slide. A little bit about this flow of information. We had this INTRAVAL workshops. We are having one next week in Seattle. DOE is hosting it and organizing it. And a lot of the folks from the OCRWM Yucca Mountain Project Office will be there. So that is one opportunity to exchange information and ideas; presentations like this and I made one a couple of weeks ago to our PA

Group.

The US INTRAVAL participation workshops in between the international workshops, the NRC and the DOE participants get together and have workshops among ourselves on our test cases and they are at a much more detailed level than time permits at the international meetings.  These turn out to be very useful workshops to attend.   In between these meetings, the DOE participants get together and prepare for both of these meetings.   And finally, a series of reports.

I didn't mention it, but during Phase 1, we produced five reports about the progress of INTRAVAL and now we are in the process of putting together a rather substantial technical reports on each of the test cases of modeling that was done and the experimental data, inclusions and that sort of thing.

That is it.  If here are any questions I'll be happy to answer them.

DR. BIRCHARD:  I have one question.  Were you showing an indication of DOE support for those activities at Apache Leap?

DR. VOSS:  I'm sorry, again the Phase 2 or Phase 1?

DR. BIRCHARD:  Well, the modeling and heater test work?

DR. VOSS:  Yes.  That is DOE's participation.

DR. BIRCHARD:  As far as DOE, any financial support for the activities?

DR. VOSS:  Oh, do you mean are we going to help pay for the experiment to be carried out?

DR. BIRCHARD: Was that in the plans?

DR. VOSS: No. I don't think we would be able to.

DR. LANGMUIR: Any further questions from the Board or the audience for Dr. Voss?

DR. DEERE: When will the heater experiments start at Apache Leap? This year?

DR. VOSS: I doubt it. They are having some physical problems of their own. And you know, we realize that in order to get any kind of useful data, I mean it takes awhile to heat the rock up, especially if you are interested in observing this boiling front, we realize that if we are going to get some useful data out of it. We are going to have to get it going very quickly. But as of right now, I know of definite date that has been set.

DR. DYER: Burt Johnson put me up to this. He suggested it was particularly apropos since we are not too far from Virginia City, where Mark Twain spent a considerable amount of his time.

We have come to a logical break in our presentation here. And before we go into the next sequence of related talks I would propose to the Chairmen, Dr. Langmuir and Dr. Deere that we break off for this evening at this point and resume in the morning. And what I would propose to you is to follow through on this particular agenda. These would be the speakers and the topics that we would be ready to run before you tomorrow.

DR. PARRY: Don't you plan to start off with the two or three sessions that were to be this afternoon?

DR. DYER: That's correct. I'm going to give about a five minute introduction in the morning, and then we'll pick up with Julie Canepa who I believe is the last person on the proposed agenda for this afternoon.

DR. PARRY: All right. Thank you.

DR. DYER: Everybody is already on here except for a brief introduction by me.

DR. LANGMUIR: We'll provide those to the audience with the revised agenda which takes into account these changes. In the morning you can pick it up at the table out there.

I'd like to thank Russ Dyer and the speakers we've heard for providing Board with valuable insight on the status of NRC's and DOE's Natural Analogue programs.

We will reconvene tomorrow morning at 8:30 a.m.

(Whereupon, the meeting was adjourned.)