UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SUMMER BOARD MEETING

July 12, 1994

Red Lion Hotel
3203 Quebec Street
Denver, Colorado 80207

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Donald Langmuir, Session Chair
Dr. Clarence R. Allen
Dr. Garry D. Brewer
Dr. Edward J. Cording
Dr. John J. McKetta

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Ms. Nancy Derr, Director, Publications
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

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OTHER TRANSPORT ISSUES

INFLUENCE OF ENGINEERING ON RADIONUCLIDE RELEASE AND NEAR-FIELD TRANSPORT

P R O C E E D I N G S

(8:00 a.m.)

DR. CANTLON: My name is John Cantlon and I chair the Nuclear Waste Technical Review Board which I now call to order.

With me are other members of the Board. Dr. Clarence Allen, Geologist, Cal Tech; John McKetta, Chemical Engineer, University of Texas; Pat Domenico, Geohydrologist, Texas A&M; Ed Cording, Geoengineer, University of Illinois; Dr. Garry Brewer, Resource Economist and Dean of the School of Natural Resources, University of Michigan; Don Langmuir, Geochemist, Colorado School of Mines. Also present are Dennis Price, Professor of Transportation and Safety Engineering, Virginia Polytechnic University; and Ellis Verink, Metallurgical Engineer, University of Florida.

As most of you know, the Board was created in the Nuclear Waste Policy Amendments Act of 1987 to assess the technical validity of DOE's efforts at managing high-level nuclear wastes including siting assessment for the repository, transportation, and storage of civilian nuclear spent fuel.

Our program for the next two days is on the agenda which is available at the back of the room. First, we will have presentations on OCRWM's proposed program approach from

Sam Rousso and Steve Brocoum of DOE followed by Malcolm Knapp from the Nuclear Regulatory Commission who will provide some of NRC's early responses to the proposed changes in the program plan.

Next, we will have a series of presentations on radionuclide mobilization and migration, a segment to be chaired by the Board's geochemist, Don Langmuir. Tomorrow, we will have an update on the nuclear waste negotiator's efforts to identify a voluntary site for interim storage of spent fuel. This will be followed by a series of presentations on transportation of spent fuel. This session will be chaired by Dennis Price. Questions will be solicited from Board members and its professional staff and consultants and, if time permits, from the floor. However, at the end of each session, a roundtable discussion by all of the formal participants will take place with opportunities for questions from the floor. We ask that all questioners identify their name and their affiliation since our entire proceeding is being recorded and will be accessible later to anyone who wishes to have access to it.

Let me now introduce Sam Rousso, Director of Waste Acceptance, Storage, and Transportation, Office of Civilian Radioactive Waste Management. Sam?

MR. ROUSSO: Thank you very much, John.

Good morning, ladies and gentlemen, members of the

Board. As John just mentioned, I am the Director of the Office of Waste Acceptance, Storage, and Transportation. That's effective as of yesterday. I'll talk a little bit about the reorganization we've just gone through at headquarters.

The subject this morning--I hope you can see this and I'm not blocking the screen--is the update on the proposed program approach. Dr. Dreyfus regrets being unable to attend this morning. However, I shall try and give you an overview of where we are, what we're trying to accomplish, and what it involves. Steve Brocoum who will follow my quick overview will be addressing specifically the questions raised in the Board's letter of May 17. There were 10 questions specifically on what we plan to accomplish.

I'll quickly give you a little bit of background. This is just a simple schematic. The senior management following the directions of the Department as a whole has gone through a strategic planning process. This is a simple sketch of a TQM journey, if you will, started approximately last fall/late summer where the senior management of the program got together and had several sessions. We affirmed our mission and got together what the current situation was, what people expected of the program, what the law expected of the program, went through and produced some strategic goals and objectives which were shared and I believe discussed by

Lake Barrett at the last Reno meeting. We then had a major stakeholders' meeting in Las Vegas just before the international conference at the end of May. We're pretty much up into this area where we continue to solicit feedback. I believe there's a couple of stakeholder sessions planned at this point for late August. We'll be getting more specifics on that. And, it's an iterative process; we get feedback, we've revised, we go back again, and eventually we accept the plans and approaches and codify those in multi-year plans and then we manage to get that effected.

As I said, the Department went through and produced some Departmental objectives. From that, we evolved our vision and mission and came up with the OCRWM goals. The goals, I believe, you've seen before. There are eight of them, not necessarily priority order, but easily grouped. The first two are the leadership ones. We obviously have as our prime directive, if you will, to lead the collaborative development, to lead the effort for waste disposal, high-level waste permanent disposal. We also intend to participate actively in other disposal questions that involve radioactive waste that are percolating around in the Department and we may eventually have to play a significant role in.

The next two follow in trying to execute any program. You want to have efficient and easily understood and watchable fiscal and management practices and watch how we work with our contractors and get a program run. To do that, you need to have capable people and resources are a very important part of the program; trying to get the right folks, trying to train the right folks, and keep them motivated and actively occupied.

The last four goals break into the major project lines. The first two of the MRS waste acceptance, MPC now, it's to resolve the waste acceptance expectation issue, the 1998 issue, and we're proceeding with that. We have an NOI that's been issued and we're soliciting public comment. We ought to get that information back, I believe, by September. And, we also have received some indications from the utility lawsuits I think you're aware of and we'll have to see how that plays out. We have to provide for the storage, the acceptance, and the transportation of the fuel.

The last two refer to Yucca Mountain. Clearly, we have to determine site suitability or non-suitability. The sooner, the better, so we know whether we have a viable site and to eventually go and do the actual waste emplacement for permanent disposal.

I mentioned a bit earlier that I am the Director of the all waste portion effective as of yesterday since the headquarters, the RW, Radioactive Waste Program, has reorganized effective as of yesterday. And, this is what the new chart looks like. Some of you may recall that the Yucca Mountain part had reorganized sometime earlier. There's been no change in that area.

We have eliminated some of the smaller boxes and combined functions. QA effort continues the same. The human resources has very little change, other than we have moved by creating a new group here under Ron Milner the program management integration function and we have placed the planning and moved the budget and the project control mechanisms into this same group and the systems engineering. So, this is all into one easily focused section. We have also eliminated and reduced the number of layers. We have removed branch chiefs. And so, now, we have a flattened structure which is in line with the program the Department and the administration is attempting to accomplish. We have managed to increase the ratio of employee to supervisor from something that was in the range of 3 or 4 to 1 up to 7 to 1 and we intend to try and make that better if we possibly can without losing focus and decent management control.

The proposed program approach, the major driver for this was a recognition that we could not continue to promise a program without commensurate funding being available. The program that we laid out under Admiral Watkins in 1989 had a funding profile which in subsequent years the expectations were not met. Consequently, it became more and more difficult to hold those pieces together. We made a considered effort in the '94-'95 budget cycle to propose a new funding mechanism which would enable the program to have access to more of the Nuclear Waste Fund monies which were collected, were adequately collected, but through the administrative process, the appropriation process, were not available to the program. So, we took a recognition of what is a rational spending profile we could hope to get? What is the program we could hope to run with that spending profile? Those two have to join together and that was the major driver; was trying to do this. So, we wanted the realities of the management that we had to address the spent fuel question and we wanted to make progress towards early suitability of Yucca Mountain or, if that failed, what do we do next?

We had to put together a technical approach and schedules that were realistic and consistent with the funding expectations. We put together a program that does not require changes to the Waste Act. It may require some modification to the standard contract because MPCs are a part of this approach and that did not exist at the time, 10 years ago roughly, that we signed those contracts with the utilities.

The graph I showed, that circle looping curve, showed one point of soliciting information and feedback.

Obviously, it's not a one point exercise. We have had many iterations, meetings with State folks, Tribal interests, local governments and so forth, utility people, and stakeholder meetings. We will continue to have that interaction to come to closure on this. The "culminated" is a poor choice of words; it's really just that we had a stakeholder meeting that was fairly broad-based just at the end of May.

I mentioned a little bit about the budget situation. Let me point to this which shows you the differences between the '94 funding and the '95 request that's on the Hill now. A little update on the '95, the House Appropriations Committee gave us $50 million more than the '94 total. The Senate Appropriations gave us the full request of the 532. That's approximately a $100 million difference between the two committees. That goes to conference. I expect that may happen this week; hopefully, before the summer recess. Then, we'll get a picture of what our '95 number is and we can see how close we are to the game plan.

The point I wanted to mention on this chart is that the percent of dollars that were going for, what's commonly termed, real work or work that the projects were doing in a direct fashion was about 76% of the total funds available in '94, with the remainder going to the management oversight administration functions that are necessary, but nonetheless are supportive. In the '95 compilation, the dollar amount goes up approximately 40%. The dollar amount for the support functions remains virtually constant. So that the net effect is that there is an increase to 83% of total budget dollars going to do the project work and the waste acceptance and transportation work, and the support work drops from 24 to 17, or a drop of about 25% in that area. That's to be expected as the budget goes up. We have not shown anything like a proportional rise in these other functions.

The key components of the approach, we've gone forward, as I've mentioned, with a public dialogue notice published May 25 and expect to get responses back towards the end of September. That's open to anyone. That's the utilities, the folks at the meeting today, any interested constituency, any member of the public can put their thoughts together and come back with what they think we should be doing as a nation in this area. I mentioned that, unfortunately, we did get a utility response that we'll have to address and there are various reasons for their positions, but I think they have a right to know where things are.

MPCs, Multi-Purpose Canisters, are a big part of this effort and the site characterization activities will be focused early-on on the site suitability questions. We hope to get to a site suitability decision in early fashion. I'll show some milestone scheduled dates in a moment.

Differences from the approach, the MRS actual design work has been put a little bit to the side until we actually get a host site. We continue to support the efforts of the negotiator. I believe he will be speaking to us shortly on the agenda and we'll find out where we are in that process. We have added the MPCs. The emphasis has been changed and focused so that we can't do everything for everybody at the same time. We're trying to get the site suitability indicators on the table first and focusing the effort on doing that. And, we've gone to, let me call it, an incremental licensing strategy where we will not go for the whole position at one shot. We will try to get a license if the site proves to look suitable to us--to get a license to construct and then a separate amendment to the license to receive and possess the fuel and then, sometime later, when the system and the society deems it appropriate, we will go for a closure amendment.

Some milestones, we are scheduled to begin the underground exploration. We have the TBM on sites and, if you have seen it, that's under assembly right now and I believe we should be ready to start that at the end of this month or early September. The dialogue, again it's not to begin the dialogue; we've been dialoguing for quite a while, but that references essentially the NOI I just addressed. We will be going through the NEPA process obviously for both the site and the MPCs. MPC design target is the end of 1996 with MPC certification to follow and any site suitability determination in 1998. We also plan to have the MPCs available to utilities by 1998. And, the site recommendation report, if the site is suitable in 2000, leading to the date we've had on the table for some time and that's the license application for 2001, but it would not be a total license to build, construct, receive, emplace, and close. It will be that incremental licensing. And, repository operations on that timetable are still planned for 2010.

The Multi-Purpose Canister provides standardization and reduces handling, has many benefits, we feel, to the program. We plan to have that available. And, the title transfer to DOE is when we remove the spent fuel from the utility sites.

In the transportation area, we continue development of the transportation casks. That's a 75 ton and 125 ton rail cask. We also are doing development work on the high-capacity truck casks, the GA-4/9, which will enable us to take fuel from those utilities that don't have easy rail access. And, we feel very strongly that we have to, to make this a successful program, continue to have an iterative, open public process where we have this exchange of views and everybody knows where we are and where we're at and what the problems are and has a chance to get their views on the table.

For the repository, I've mentioned the initial focus on site suitability going forward through license application on an incremental basis, following NEPA, and extending the retrieval period.

In conclusion then before we pass to Dr. Brocoum, we've got a proposed program approach which is consistent with the funding proposal. Now, if we don't get the funding proposal--and I don't say it has to be 99.5% of the fund proposal, but reasonably intact with what we've put on the table--we will not be able to go forward with what we've planned. What we've got together is both a funding approach with a profile and an ability to get that funding which is with the Congress now and a planned technical approach that can react to that funding level. We feel that that program approach addresses the issues that the program faces that have been raised by the many different constituencies. We will continue as a strong part of the program, the stakeholder involvement, and try to be sensitive to and respond as best we can or at least respond as to why we took the path we did when those points come to the many interested stakeholders in the program, and we're positioned to do that as of now.

If there's any questions at this time or over the next two days, we'll be covering much of this and we'll get on with the specifics.

DR. CANTLON: Okay. Any questions from the Board? Ed?

DR. CORDING: Just one question. The schedule, as I see it in many areas, there's very little float. A lot of major items have to be accomplished within the next, say, three to four years before the 1998 date. Even with the funding, if there's slippage in achieving some of those objectives, exploration objectives and things, is there going to be a delay in the 1998 date or how would one accommodate that?

MR. ROUSSO: A good question. I think it's no secret that the schedule is very tight. We've laid out a program, we've laid out a funding basis, we think it's achievable, it does not leave room for much error. That does not mean--I'll emphasize it does not mean schedule driven. We intend to run an honest, open, safety program/environmental program that's going to meet what needs to be met and it will be available to all to see. We hope we can make that schedule. We still feel that that is a reasonable possibility giving the funding profiles being met and the reorganization and the emphasis that we've put. We've changed things. We've held dates on all--the milestones aren't what they used to be. The dates are there. It's not the closure of the license that we had planned before. It's not the gathering of all the SCP data in advance. We're doing parts that, I think, make sense and make sense as to where we are today. I can't go back to '89 and revisit, but we'll do what we can.

DR. LANGMUIR: Sam, your budget sheets show an increase in funding for characterization. We've all seen a substantial growth in the hiring of the M&O in the last few years and that seems to have continued. Will that stop as of this coming year? Will they be continuing to hire more people and, if so, are they in the characterization area? What are they doing?

MR. ROUSSO: Well, we're still going through the breakout of the funding. As I said, there's about a $100 million difference between the House and Senate marks at this point, but the growth will be in the actual work. I think most of the growth in the dollars is to support TBM operations which is not done by the M&O. It's done by the drilling team and the operating team. There is some increase in M&O work. As you know, our breakout between Feds and contractors--as most of DOE programs, there are few Feds relative to the number of dollars that drive the work done by the contractors. So, our FTE is actually going down Federally. It's roughly 245 for the program. That's maybe $20 million or $25 million of the 532 is Federal people. All the rest of the money is either equipment or contractor support dollars to do the actual work. Depending on the scope of work the M&O has, they'll be a player to that and I would expect there would be some growth, but I don't think it would be major.

DR. BREWER: On Page 14, you have a number of milestones noted. With respect to the NEPA process, you've not mentioned the rail line spur. Was that an omission or how are you going to handle that?

MR. ROUSSO: The NEPA process is still evolving as to whether we have separate EISs or combinations thereof, or a programmatic EIS. And, I don't have a specific answer for that one. I don't--Steve, do you have any information on that?

DR. BREWER: Because the MPC and the repository are obviously two things, but there's a third issue and the State of Nevada has--

MR. ROUSSO: Yeah. There's a rail spur question in Nevada.

DR. BREWER: --made much of it. I just wondered whether you were paying attention.

MR. ROUSSO: Paying attention is true, but resolution or position at this point, I'm not sure what--

MR. BROCOUM: We have a route analysis in the '96-'97 time frame. I'm not sure about where the EIS stands. Does anybody know? They talk about analyzing for the route in '96-'97, but you start actually designing for it in Title I way off in the year 2005.

DR. BREWER: Okay, thanks.

DR. BARNARD: Sam, you mentioned that DOE has issued a notice of inquiry in April of '94. Why was it issued at this time as opposed to two years ago or five years ago?

MR. ROUSSO: You're asking me one day into that, Bill. I would have to say that because we didn't issue it two years or five years ago, it was time to issue it. I don't have a good answer to give you, but I think we tried to work a dialogue. We had many meetings. This was not certainly the first series of meetings. I think at that point in time we felt more comfortable that the dates could be reached along the program we were trying to run. As that became more and more apparent and more and more divergent, it got to a point where, hey, look, '98 is not that far away for our guys; we have to do something. What can we do? What are we physically capable of doing? What's the expectation realistically out there from all standpoints; from the parties, the utilities, from the utility commissions, from the public-at-large? Let's get it on the table; let's get the issue straight. And so, that's what we're doing.

DR. CANTLON: I think we better push on.

MR. ROUSSO: Okay.

DR. CANTLON: Steve?

MR. BROCOUM: My name is Steve Brocoum. I'm the Assistant Manager of Suitability and Licensing of the Yucca

Mountain Site Characterization Office. I currently reside in D.C., but I'm in the process of moving out to Yucca Mountain and I'll be out there mid-August.

I want to talk about four things today: the background and overview, a little bit of background; status of PPA and what our planning process is within the project; a summary of the responses to the Board's questions in their letter of May 17 and I have a chart of each question; and, just a quick summary diagram.

Now, some of this was already said. So, I'll just go over it quickly. Under the PPA, we're trying to set goals and schedules are, you know, being developed. We're hoping to make it a reasonable probability of being successful and we hope they're consistent with the resources that can be allocated to the program. One other major thing is that they allow us to demonstrate incremental progress. That's almost the most important thing. It should have been on the viewgraph.

We think that the PPA is consistent with some of the recommendations of the NAS report, "Rethinking High-Level Waste", concerning that you can't know everything up front, you can't learn everything before you will go underground, and in fact, you can't know everything even before you start to emplace. We've tried to incorporate a step-wise approach to decision-making and we think many of the Board's recommendations are incorporated. Get underground quickly, start thermal testing, concentrate more on the waste packages, those kinds of things from the Board, we think are being addressed.

Where are we in PPA? Well, we've come up with an overall strategy. We've held lots of strategic plans under leadership of Dan Dreyfus. We've had various stakeholder meetings and Sam alluded to some of them. We've done tops-down planning. We are getting ready to issue guidance to the participants in Yucca Mountain Project. That has not happened yet to my knowledge. I've got to look back at people. I don't think it has. It will probably go out this week. And then, we need to get our appropriations from Congress. Then, we need to complete our bottoms-up planning. At that point, we'll have an idea of the strategy we propose and whether we can really afford it.

The detailed testing and analysis that will be done over the following year will be described in the Technical Implementation Plans which exist in draft form today and it will be finalized in September of '94. We will approve our '95 cost schedule baseline. We will complete our bottoms-up as we need to adjust. Then, we're going to re-baseline the whole program from '96 to 2001 by March of '95, change any documents that need to be changed after that. And, if we still have to go through the Energy Systems Advisory

Acquisition Board, we need to get independent cost evaluation and then we have to get board approval. That's a headquarter level/Secretarial level board that approves major programs.

This is our top-level milestone for the project. These are our top-level milestones; technical site suitability, Draft Environmental Impact Statement, final EIS, LA, site recommendation report, and so on. The point I want to make here is in suitability we're going to do it in iterative fashion and try to make one or several high-level findings each year, so that when we complete it, we'll essentially have evaluated site suitability.

In the NEPA area, we're going to try to put out a notice of intent and start the process of NEPA. In the licensing area, we're issuing a series of annotated outlines. Annotated Outline #4 will go out this year. It will be a DOE document. In the past, it has been a contracted document. Annotated Outline #5 will be the first complete annotated outline of the proposed license application. If we're successful by the time we get through #9 or so, we'll have pretty much the contents of what a license application will look like. The point here is these are iterative. These are steps. And, whether we do them by the years we say or we have to stretch out the program because we can't accomplish the work as fast as we think we can or whether we don't have enough funding or some combination, the fact is we can demonstrate progress whether we do these once every year or once every year and a half. It doesn't really matter in the sense that you can demonstrate progress. That's the point I'm trying to make here. The program is designed to allow a demonstration of progress. I've said this before. We haven't made a statement on site suitability since 1986. I'm looking to get the right year here. And, since then, the program has made no formal statement on suitability. Here, there will be some kind of a statement every year on some portion of the suitability question.

We're intending in terms of much of the information needed for post-closure performance to use bounded information to the extent we can. A major decision that we have to make this year is on the Calico Hills. We're evaluating options for going to Calico Hills and then we will implement them. Whether we're actually going to penetrate the Calico Hills or not is one of the major decisions coming up this year.

We have shown this diagram before. This is what we call our waterfall diagram. The only reason I'm showing it again today is to make the point that we have bucketed the suitability issues in a series of buckets--we call them buckets--and we will try to make one or more of these every year. A very key concept of this process are these technical reports. It says report here or report on erosion, for example, which we'll try to pull together all the information that we have on the subject to make our suitability and, later, our licensing arguments. And, determining what those reports are and the contents of them is one of our big challenges.

Now, I was going to go through the questions one by one. I don't know if the audience has seen these questions. I can't really tell. So, maybe I'll read them off if that's okay with the Board. The first question, (a) what are the specific technical bases for the decisions that led to the development of Scenario A; (b) will the site characterization plan be modified to reflect the new program design; (c) if so, what process will be used to modify it; (d) if not, what will be the status of the existing site characterization plan in structuring the technical investigations at Yucca Mountain?

Sam kind of went over this a little bit, but basically we could not achieve the expectations that we have laid out in the SCP without historical funding levels. We never got funded to do what we said in the SCP. We didn't think that science could meet the unrealistic expectations regarding the level of knowledge we would get early for long term performance. We had a series of strategic planning workshops of high-level management where we discussed all this thoroughly and then that's how we developed the PPA.

So, we're trying to realign the program with the original intent of a legislative/regulatory framework.

The changes to the site characterization program by law are reported in the semi-annual site characterization progress reports which we submit to the NRC twice a year. One of them, I believe, is just ready to go out right now looking back at Claudia back there. I don't think it's out yet. There's one coming out which will actually start to discuss the new PPA.

The program is controlled through the divisions of the site characterization program baseline and other documents that we have in our control of our controlled documents. The SCP, itself, was a one time document required by law which we issued, I guess, it was in 1988 and we never had any intention of rewriting the SCP, but any changes to it would be shown through the progress reports.

The second question, "At the January '94 Board meeting, you said"--I assume Dan Dreyfus said--"that the 'institutionalizing stakeholder interaction' is one of the OCRWM program's important short-term goals. How does DOE decide which decisions are 'key decisions' requiring stakeholder input? How and to what extent did the DOE obtain stakeholder and public input prior to formulating Scenario A? Which stakeholders were involved? What specific mechanisms is the DOE using to obtain stakeholder and public input?"

The managers have been directed to identify key decisions. We think some of the key decisions are higher-level findings on site suitability, our technical site suitability determination, DOE decision--if we get that far --to recommend a site, our initiation scoping under NEPA, preparation of EIS. Any key decision, any major decision that the program makes, and any decision which requires a new or changed allocation of resources would probably form the definition of a key decision.

We had a series of strategic workshops. We made a series of initial assumptions consistent with what we thought was our '95 budget request. For example, in the suitability area, we reviewed all stakeholder positions going back five or six years. In refining the strategy, Sam mentioned this, we had all these meetings. Stakeholder interactions aren't just the big stakeholder meetings like we had on May 21, but there are a lot of smaller meetings that various program people have day by day. We have, for example, at the project every month the effective units of local governments' meeting and that was a meeting I participated in a few weeks ago to update the people on the PPA. So, there are lots of small scale meetings that happen all the time.

How will we continue to address stakeholder input? One of the ways is through the OCRWM strategic plan. All these strategic planning meetings that we have had off-site will lead up to a strategic plan. A draft plan is scheduled for August of '94. It will be finalized on or about December '94. That plan will go through stakeholder interaction. There will be some sort of a meeting that I believe is being planned for the fall. This is under the responsibility in our new organization of Ron Milner in program integration. That plan will also be updated every year as the program proceeds. I think if one wants to understand the status of the whole program and where we think we are going in a strategic sense, this is the document to be looking for.

Question 3, "Scenario A calls for increased budgets, a decreased scope of near-term site characterization activities (e.g., potentially less tunneling), and a demanding schedule. What specific studies previously planned under the SCP and in the study plans (i) will be completed before application for a license to begin repository construction, (ii) will be deferred until after repository construction, (iii) will be deferred until after repository operation begins, and (iv) will be deleted? What criteria were used to assign particular studies to one of the four categories?"

A very difficult question. I want to take a little exception to the word "decrease". This is only to the program that we had wished to do earlier, not to the program we were actually executing. Compared to the program we've actually been executing the last few years, site characterization activities will increase.

We're going to focus on activities needed to determine site suitability. We're going to focus particularly on operational and waste package containment. That's part of our strategy. We'll have less priority because we're going to use bounding arguments to the extent that we can for the post-closure demonstrations. And, other testing will be deferred to the performance confirmation program in kind of a summary fashion.

So, some of the things we need to do are to excavate into the potential host rock at Topopah Spring unit to support findings on pre-closure rock characteristics; in other words, safety and constructability. We have to evaluate the seismic design basis to support suitability finding on reasonably available technology proposed for pre-closure tectonics. We have to characterize the near-field environment to understand all the coupled processes; hydrology, geochemistry, thermal loading, and mechanical behavior. And, we obviously have to look at the potential fast flow paths through and along the Ghost Dance Fault in terms of evaluating groundwater travel time. We're going to excavate to the Ghost Dance Fault in the Topopah Spring unit. That's going to help us decide whether we need to go for Calico Hills. And, also, a lot of this is being supported by the surface drilling program. The question implied that site characterization and tunneling were synonymous. I need to point out again that we had a $70 million program last year on surface-based testing and a proposed larger program coming up this year on surface-based testing.

Question 4, "The OCRWM has asked for increased program funding because it believes that the scientific work has been under funded. If Congress provides the requested funding for Scenario A, specifically how much will allocations to underground excavation, waste package and materials research, and other site suitability activities be increased? How much will be allocated to overhead and infrastructure? Will these allocation priorities change if funding to the program is not increased to the level requested?"

We're obviously in the middle of our planning cycle, as I showed earlier. So, we don't have definitive answers to all those questions. But, for example, if we go to a three shifts per day TBM operation, our budget will approximately double when we allocate in fiscal year '94 and currently the ESF budget exceeds what was planned by over $12 million this year. The remainder would be obviously allocated to site characterization design activities. Management costs will not increase. I think Sam showed that nicely in his diagram. If the funding is not consistent and we don't get enough and if the prognosis for future funding is similar, the program will be re-evaluated. We may do the program sequentially. We're trying today to do suitability, NEPA, and licensing in parallel. We're trying to do surface-based testing and tunneling and underground testing in parallel. Those things may somehow be sequenced differently so we have a much different program. So, allocations for a changed program will depend on the nature of what program results. However, if we don't get close to our requested funding, a full program, including all licensing activities, will probably not be undertaken.

Question 5, "Scenario A calls for the completion of a five-mile main loop with additional drifting only if necessary." I'm not sure where those words came from. Maybe, Dan said them; I don't know. They were in italics. "What is the technical basis that supports this change from the current program design? What technical criteria will the DOE use to decide where the five-mile loop is sufficient for a decision on site suitability? If a five-mile loop is insufficient, how will DOE decide how much additional underground excavation will be needed?"

Well, we gave you our rationale a little earlier. In terms of ESF construction, our emphasis, our focus today is two-fold. One is to obtain access to the Ghost Dance Fault in the Topopah Spring and the second is to get access to the thermal testing along the north ramp extension. That's our primary focus this year. Because of this, completion of the five-mile loop is not of major emphasis. Our major emphasis is to get access to start the tests. So that the rate of the TBM advance after we get the second access or the second drift from the main tunnel to the Ghost Dance Fault will be depending on the resources needed for other ESF activities. For example, we may decide, if we have the 18 foot TBM procured, to start the north ramp extension. So, we will be modulating between the main TBM and the smaller TBM in light of balancing the whole program and doing the testing and that will determine the rate of extension/the rate of progress for the loop.

Again, the Calico Hills is one of the major decisions we have to make this year. The NRC has been very clear over the years in telling us that we must characterize the Calico Hills, but they've been very careful not to tell us how to characterize it. Some people in the program think we can characterize the Calico Hills from drilling. Others think we have to access it with a tunnel. We're going to do a systems study to look at all these issues this year and, as suitability proceeds, we may have additional excavation. So, we're trying to remain flexible.

Question 6, "Thermal loading is a key parameter associated with various waste isolation strategies and repository/waste package designs. Under Scenario A, when will a preliminary decision about thermal loading be made? When will a final decision be made? What specific information does the DOE believe will be required to make sound technical decisions on (i) repository design and (ii) a waste package design that is compatible with the MPC? How will the timing of the DOE's application to the NRC for a construction license affect the DOE's thermal loading decision?"

The major issue within the project here is that the engineers would like to make the thermal loading as hot as possible. From their perspective, making the repository as hot as possible is the most economical. It requires the least amount of air, the least tunneling, and so on. From a regulatory perspective, my perspective in particular, to go to the NRC with a license in the year 2001, I think it makes more sense to go with a thermal loading that's low enough that we can defend with the information we have at hand at that time. So, we may submit a license application for less than the 70,000 metric tons under a lower thermal loading scenario. We need to resolve those issues among ourselves and the engineers in the project and that's one of the reasons we're doing a thermal loading study this year.

As we were planning for this, we identified at least four thermal loading studies that various people in the project were planning to do. One was a systems study, the second was a thermal loading decision analysis that the engineers wanted to do, the third was the thermal loading studies that the site characterization people wanted to do, and the fourth was the environmental people wanted a study of thermal loading to see what the impact would be on the biota. Well, we're going to try to build an umbrella over all these studies and pull them all together in the thermal loading systems study.

Even if we go with a license application to the NRC with a relatively low thermal loading that we think we can defend at the time we submit the license application, we are not precluded from attending to amend the license at a later time or even after we're operating if we think we have the information to support that.

The kind of information that is needed--this is a laundry list of information--the big issue here is pulling all this information together, integrating it so we can make our arguments both in a suitability and licensing sense. This is an issue we have across the whole project. It's pulling together the information and integrating. That's why on--I showed the waterfall diagram earlier, but the technical reports that pull together these pieces of information will be crucial and, in fact, we're having regulatory people and site characterization people meeting to try to define what kind of information is needed for each technical report.

Question 7, "Under Scenario A, the waste will 'remain retrievable' for about 100 years. What contingency plans for retrieving the wastes will be developed before deciding whether to adopt Scenario A? When will retrieval plans be developed? How will these plans affect the total system life cycle costs and the adequacy of a 1-mil-per-kilowatt-hour fee?"

M&O has developed a draft concept of retrieval operations which is in draft which addresses both normal and abnormal retrieval conditions. That is being reviewed by DOE. They also have a draft position on retrievability for the DOE. So, there's a second study that's not listed here; a draft DOE position on retrievability. Finally, there's a retrievability period system study is being developed which is looking at 50, 100, and 200 year retrievability periods to see what are the pros and cons of each one, basically. That is due to be completed by September. The only thing I've seen so far--and this is an outline; I haven't seen a draft.

Using the assumptions made for the 1989 TSLCC, in that TSLCC, we were using the ESF and the concepts we had in the SCP which were shafts rather than ramps, a totally different concept. We were also using non-robust waste packages. Anyway, using that assumption, you would add about 1.2 billion. It didn't include retrieval costs, but it did include removal of waste packages for various kinds of testing for performance confirmation.

We need to adequately address this fee adequacy issue. We need to do additional engineering on advanced conceptual design for the current concept of the repository and we will revise the TSLCC during fiscal year '95. At that point, I think, we'll be able to address the fee adequacy issue in a more robust manner.

Question 8, "Descriptions of Scenario A refer to a 'site suitability evaluation', 'technical site suitability', and 'a site recommendation report'. When and how will the DOE identify specific tests and data necessary to support these site suitability determinations? Does the DOE believe the siting guidelines are adequate for determining site suitability under Scenario A? If not, what amendments are envisioned and what process will be used to adopt them?"

My first point here is that the proposed program approach allows us to make early decisions on specific guidelines in a phased manner. The specific tests and data needed to support the suitability are being identified and they will be listed in the technical implementation plans which exist, I said earlier, in draft form--it will be finalized by September--and a followup long-range plan which takes us out to the year 2001.

The point I'm trying to make, in developing these plans, we're having a lot of interactions between the people that collect or responsible for overseeing the collection of site characterization data and the people that are responsible for doing the regulatory type assessments and suitability and licensing. For example, for surface processes, we've identified in fair detail what kind of information is needed. For the other buckets, we have identified the information in more general terms, but that's what we're working on right now.

With regard to 960, the DOE elected to elicit public comments on the evaluation site suitability including the role of the siting guidelines in that evaluation. We issued a Federal Register Notice on April 25. We had the stakeholders' meeting on May 21. The next steps that we see coming are a Federal Register Notice which we are preparing now on the proposed process for site suitability evaluation and then we envision two public meetings towards the end of August; one in the Las Vegas area and one in the Washington, D.C. area to get input on the proposed process for site suitability evaluation. And, finally, we envision some sort of interpretation of 10 CFR 960 published in the Federal Register. This is primarily a legal interpretation which will be done by general counsel.

"The NRC's regulation, 10 CFR 60, requires the DOE to demonstrate, prior to repository construction, that there is 'reasonable assurance' that the facility will perform safely. The SCP outlines a testing plan that implies an agreement between the NRC and the DOE about how 'reasonable assurance' will be demonstrated. Under Scenario A, some of the tests will be postponed until after the repository operation begins. How will the DOE demonstrate the level of assurance in the performance of the repository that would have been obtained under the SCP? Will it be necessary to reinterpret or change the level of assurance? If so, how will it change?"

If one goes back and looks at the history of 10 CFR 60 in the Nuclear Waste Policy Act and looks at what the expectations were back in the early days, we were talking about a three year characterization program of $50 million total budget to characterize the site; one single shaft and a small test area at the bottom of that shaft. Those were the original expectations. So, the program we've outlined in the SCP including changes since then reflect expectations that go beyond what we think is needed to comply with the original intent of the regulations. That's the point I'm trying to make here.

The strategy we think will provide sufficient information to enable the NRC to make reasonable assurance findings, the point I want to make here is that even the reduced--if we have a reduced ESF, it is more than the ESF we had in the SCP. The SCP had two shafts, three hundred feet apart, and a testing area at the bottom. It did not have the extensive drifting that we've developed since then. So, even with the PPA, we have a more extensive proposed ESF than we had with the SCP.

We think that the PPA strategy is consistent with both the letter and intent of 10 CFR 60. And, just a few quotes from the regulation, 10 CFR 60.24, "the application will be as complete as possible in light of information that is reasonably available at the time of docketing." In other words, you cannot expect to have every single piece of information. 10 CFR 60.101, "Proof of the future performance over time periods of many hundreds or many thousands of years is not to be had in the ordinary sense of the word. What is required is reasonable assurance, making allowances for time periods, hazards, and uncertainties involved."

Obviously, we will not submit a license application to the NRC that we don't think they will--either we don't think will give them the information to make a reasonable assurance finding on construction authorization or that we don't think they will docket. We have lots of mechanisms to interact with the NRC. We have our annotated outline. We'll have at least nine iterations before we submit the license application. We have our semi-annual progress reports, two a year. We have our topical reports and we have technical exchanges and now we have quarterly management meetings. So, these are some of the formal ways that we interact with the NRC and we expect them and we want them to respond to us in the formal sense to help us understand and help us better define reasonable assurance. So, you know, we want help in doing that. That's what we're asking for.

The last question, "According to presentations made at the panel meeting on March 22, 1994, by representatives of the Council on Environmental Quality and the DOE's General Counsel Office, the Yucca Mountain Environmental Impact Statement should include a discussion of various repository waste package design alternatives. Under Scenario A, what alternatives will be sufficiently well understood to be evaluated? Will separate impact statements be prepared for MPC procurement, repository development, and transportation? How will the interdependencies among those activities be analyzed?"

I want to make a few comments on this. OCRWM is reviewing its NEPA strategy. It's responding to the Secretary of Energy's meeting on June 23 where she discussed the DOE policy on NEPA. That includes more active developing of draft environmental impact statements within 15 months, more efficient hiring of contractors to do the work, more efficient review process by the General Counsel's Office and environmental health within DOE. So, taken along with that, we're reviewing our strategy. And, this review will include the alternative NEPA approaches and the various interdependencies. And, obviously, any issues raised will be addressed in the scoping activities.

So, just as a closing slide here, we have to resolve the disconnect between expectations and our ability to achieve them and that's what the PPA is all about; trying to resolve the disconnect. We're trying to align the program closer to the original intent of the legislative and regulatory framework. We're hoping that the majority of details on implementing will be finalized by the end of the year. We don't have all the information now because we haven't done the bottoms-up. We've done the tops-down and now we're going out to do the bottom-ups. Obviously, we will continue to interact with our stakeholders in various forms including for the strategic plan. And, we have to reconcile the PPA activities with the level of Congressional appropriations the program will get.

That's it.

DR. CANTLON: Okay. Thank you, Steve.

Questions from the Board?

DR. LANGMUIR: Steve, I think it's very obvious that this approach of identifying milestones and attaining them is a very healthy and pleasant way to do it in the sense that you feel progress and you can show progress along the way.

As part of that process, you're describing efforts to bound uncertainties with regard to certain characteristics of the site; for example, geochemistry, hydrology. Looking at the total systems performance reports recently published out of Sandia, it showed that much of this approach to bounding involved the use of expert judgment. I was concerned because it looked to me as if it was being used in place of collecting data and really characterizing the site in some cases. I worry that the bounding may be rather large and, if we're using experts on one end of it, it's subject to quite a bit of criticism ultimately.

MR. BROCOUM: Over the years, the NRC has told us in many different forms that we cannot use--I'm not sure of the exact words--expert judgment in place of reasonable, attainable information. So, I think we always have to keep in mind where the information is reasonably attainable, we have to gather the information. But, it may take us many decades to gather enough information in some areas to really understand the phenomena and we need to keep that concept--we have to keep that in mind that we may not be able to solve everything in five or ten years. So, the choice is whether we can move forward somehow as we try to understand or whether we say in the current context we cannot do this kind of program that was mandated in the Waste Policy Act.

So, what we're trying to do is find a way that we can move forward and loosen the confines of the Nuclear Waste Policy Act and get enough information that the NRC can make their reasonable assurance finding, but be able to supplement that information later. That's kind of the best answer I can give you right now. There's no very simple answer to that.

MR. APTED: On Page 19 and on Page 27, you touch upon the issue of understanding a couple of processes. You mention on Page 19, "gain sufficient understanding of a couple processes". How are you going to judge what sort of technical criteria are going judge sufficiency--

MR. BROCOUM: I'm going to have to let--you know, the technical people will have to answer that, but I think our approach is the less thermal loading you have, probably the easier it is to understand them. And so, that's why I'm arguing from a regulatory respect that we ought to go in with a low thermal loading for our initial license application.

MR. APTED: So, the technical people later will be dealing with that, addressing that?

MR. BROCOUM: I don't know if they'll be doing that today. Ardyth?

MS. SIMMONS: Although that's not part of our agenda for today, specifically with coupled processes, we have a number of studies that are going on in this area that are going to be continuing. They will be addressing the thermal loading systems study that Steve mentioned and also we plan to have a review of all of the thermal--all of the coupled processes work that would be input to that and to other regulatory decisions. So, those investigations are going forward. And, if you want specifics of what those consist of, we have both experimental work and modeling work going on in that area with coupling of hydrology and chemistry and also mechanical properties.

MR. APTED: Yeah. I'm not so much worried about the programs. I understand that they're in place. It's more the question of how to interpret sufficient understanding. I mean, where the cutoff is and how DOE will--what are the units of sufficiency, I guess, that will be applied?

MS. SIMMONS: Well, I think those are going to be partly developed in an iterative process as we go along and as the people in the scientific programs continue to meet with the people in the suitability and licensing area to define what will go into these technical reports that you've seen listed in Steve's presentations for the various guidelines that have to be met for 960.

MR. BROCOUM: We're also trying to develop--I don't want to use the word "accept"--but, essentially, acceptance criteria when we think a report adequately addresses. Before we do a study, we're trying to say what will adequately address this issue? So, one of the things we're looking at when we decide to do investigations is when do we know that they've been completed? That's a big issue with the project right now.

DR. LANGMUIR: Steve, Overhead 9 is a flow chart showing preliminary site suitability decision scheduling. My question is more general, though, than that diagram in a sense. It suggests--at least, we've been discussing the idea that one criteria, one observation, might be a disqualifier when it comes to site suitability. But, is the organization considering the possibility of a cumulative effect of half a dozen or two or three or four characteristics of the site which are less than desirable and cumulatively could lead to a disqualification?

MR. BROCOUM: My guess is that would be addressed through the total systems performance assessment. If that's the last one that we actually do there. When you look at the total systems in total. I mean, if you have a bunch of detrimental aspects and they're all in a sense--sums up to something that gives you unacceptable performance, I think through the performance assessment is the way you would address that kind of--

DR. LANGMUIR: That's what I would think, but it's clearly in your thinking as a possibility.

MR. BROCOUM: Yes. Oh, yes. That's in our thinking, sure. I don't want to prejudge what the suitability team is going to do in their public meetings and prejudge what our interpretation of 960 is going to do. I mean, I have to be a little careful when I talk about suitability here because we've started this public process and we want to continue this public process to say how we're going to go ahead and do suitability. But, I think what I said, you know, still will be said, I think. Like I said, it still holds true.

DR. CORDING: Steve, you mentioned the north ramp extension and I assume that's basically an east/west traverse that goes on across the site?

MR. BROCOUM: Yes, that would go all the way to, essentially, Solitario Canyon and that would be the equivalent of a complete drift across the site.

DR. CORDING: It seems to me that you have selected some very important exploration goals in going down there and that seems to also be one of them. I was hearing some, say, possibilities that you might or might not do that. I was just wondering, it seems to me that also is a very crucial issue because you have the north/south trending major structural features, faults, in particular, that you really can't explore unless you go across them.

MR. BROCOUM: I didn't mean to imply we're not going to do north ramp extension. The only thing I was saying is how we were going to modulate the two machines and at what rate do we--

DR. CORDING: Sure.

MR. BROCOUM: But, the main drift and north ramp extension. We may slow down on the main drift while we do the north ramp extension if we can get into the thermal testing area, for example. But, I didn't mean to imply we were not going to do the north ramp extension.

DR. CORDING: I'm not sure you did. I was just trying to clarify my own--

MR. BROCOUM: Okay. No. I think that's--we're planning to do that.

DR. CORDING: There's a reference in Question 6 or response to Question 6 on the--it's a thermal question and there's a reference to thermal tests. The reference here to the coupled processes which also include the thermal and I know you're trying to get the thermal tests started. The question, I guess, is what do you see in ability to be able to utilize actual thermal test information?

MR. BROCOUM: How much if you're--

DR. CORDING: From underground in these various milestones like the 1998 and 2001?

MR. BROCOUM: I'm not sure how much information we'll have by 1998. Very little. But, I think we will have some information from the block experiment going on on the surface there at Yucca Mountain. It may not be adequate. By 2001, we'll have, of course, four more--three or four more years of data. So, you're right, we may not have adequate--in terms of thermal testing, we may not have adequate information in '98.

DR. CORDING: Okay. Just on quick--just let me finish up with one quick one. On Question 3, there was a comment about--the response was that there might be delaying tests on longer term radionuclide transport and release and I'm wondering if--is that delaying longer term tests or delaying evaluation of longer term radionuclide transport? It wasn't clear to me what was being said there.

MR. BROCOUM: I'm not sure what you're referring to.

DR. CORDING: It says--

MR. BROCOUM: Oh, you're looking at the letter itself?

DR. CORDING: I'm looking at the letter itself.

MR. BROCOUM: Okay.

DR. CORDING: And, it says, "A lower priority will be given initially to those tests that support demonstration of compliance with requirements related to longer term radionuclide transport and release."

MR. BROCOUM: We're prioritizing because if you take the sum necessary to do the whole program, we exceed even the PPA amount. So, we have to make some priorities. So, the priorities we're making is to make sure we have the engineered barrier and the waste packages as well defined as possible. That's our highest priority right now. In fact, the budget for that is proposed to be tripled over the next year. To the extent we can support the other testing, we will. But, that really depends on the amount of funding we get.

It's also kind of balancing the program. So, I mean, you're asking a very difficult question and we haven't really fully confronted that yet. We've met together, we've gotten what we think is the whole PPA program that exceeds right now even the PPA budget, and we have to now work it from the bottoms-up to see what we can actually do and in what sequence we do it in.

DR. CANTLON: Okay. In order to keep us on time, I will let one more question. But, before I do that, I would say that copies of our questions and DOE's answers are at DOE's request available to anyone in the audience. They are back there on the back table. So, if you'd like to see the full written material, that's available to you.

Don, last question?

DR. LANGMUIR: A short question related to retrievability. We're all concerned that, in fact, it's truly possible to retrieve the waster from a repository that's open for, say, up to 100 years. Is it really an option to select the highest thermal load? Is that going to provide conditions which make it extremely difficult to retrieve the waste, to get individuals down in that waste site, or get the machinery to work with backup and so on?

MR. BROCOUM: I think you'll have to, you know--whatever thermal loading we end up with, we'll have to have, you know, a demonstration if that thermal loading retrievability is possible. You not only have to retrieve under normal; you also have to retrieve under abnormal. I mean, there's a collapse of a rock or a canister may have fallen off a rail car. There's all kinds of requirements on retrievability. For redistributing the canisters for change in thermal, that is not considered retrievability. Retrievability is only specific for two things. One is the NRC determines we're not meeting our criteria and we need to retrieve it. Second is if DOE determines that that waste has a value and they need it. That's also a definition of retrievability.

DR. CANTLON: All right. Then, we'll have Malcolm Knapp, Director of the Division of Waste Management, Nuclear Regulatory Commission.

MR. KNAPP: Good morning. It's a pleasure to be here.

The comments that I'm going to give you on DOE's proposed program approach are necessarily very preliminary this morning. We know little more about the approach at this point than the presentations that you have heard and DOE's answers to your questions. So, a number of the comments that we have reflect the lack of knowledge we now have that we expect will increase substantially over the next six months and year. One thing I did note in hearing Steve's remarks and hearing some of your questions, they're going to be very close to some of the comments that I have to make in the next 20 minutes or so.

There are four areas I'd like to talk about this morning. Our views on the impact of the PPA on the sufficiency of DOE's license application. I'd like to talk about the relevance of some of the site suitability decisions to the license application and to the success of licensing. I'd like to speak to two of the issues which you asked that we talk about; our views on the adequacy of bounding or conservative assumptions, particularly with respect to such things as thermal loadings, long-term testing, and our views on the adequacy of the Title I repository design.

To begin to speak about license application sufficiency, certainly there is no in principle regulation or thought within the NRC that would preclude DOE from going forward with the proposed program approach. In fact, a couple of quotes of the regulation that I was going to bring forward, Steve's already given to you this morning. The regulation says that tests and studies need not all be completed at the time of licensing. On the other hand, there is another quote that might be interpreted two ways. Part 60 says that the license application be as complete as possible in the light of the information that's reasonably available at the time of licensing.

That leads to the question, how much information is reasonably available? One view could be that this gives DOE some flexibility. The other view could be that it holds DOE to a standard which is that that information which is reasonably available should definitely be in the application. I look forward to working with DOE over the next two or three years to insure that we have a consistent approach to that standard so that their application will be sufficient from our perspective.

Regulatory words aside, simply as a practical matter, the less information that DOE is able to bring at the time of license application, clearly the greater the risk that the application might not be sufficient to carry the case. Simply a fact of life. Again, in working with DOE, we would like to be in the position to advise them as to areas where we believe they will not be able to carry the case.

That gets me to the last thought in this area. As DOE develops its decisions--for example, in particular, with respect to the answer to your Question 3, what areas will be deferred, what areas will be retained, what areas may be deleted--we very much intend to work with DOE on that to insure that we are comfortable with what they will retain and what they will delete. In fact, I would hope that we can go beyond that and be involved with them in the decision criteria that they will use to make decisions as to what they will retain and what they will defer to insure that the NRC is also comfortable with those criteria.

An issue that we are concerned about with respect to the high-level findings or site suitability findings that Steve showed you in the viewgraphs deals with the impact of these findings on the license application and their relevance to the application. The issue has already been partially addressed this morning, but our concerns lie in how much a finding--for example, a finding in 1995 on surface processes --DOE would intend to complete and then barring surprising new additional information would not really tend to further address until such time as it was incorporated in the technical evaluation or the site recommendation report or in the application. If that's the case that they would essentially hope to be able to set aside further work on surface processes once that finding has been made, then, of course, we would want to be well-involved in commenting on that finding to the extent that it would affect any of the '97 or so findings which we must make within Part 60. Certainly, if we felt that they had not reached enough information that they could set this work aside, we'd have to tell them that. So, we will need to work more with DOE to understand how these findings will be incorporated in the application, to the extent at which they would be considered essentially complete, to the extent at which they would be reconsidered down the path.

And, as the Board has already mentioned this morning, there is concern about whether all these findings can be reached, more or less, in isolation. Perhaps, the surface processes could. On the other hand, the higher-level finding that they anticipate making in '97, seismic, tectonic, volcanic processes, I believe we would hold at least at this time that that would require fairly serious consideration of the overall performance of the repository. So, we would need to look at what performance assessment would tell us about the impact of those phenomena, on-site suitability, and we think that the separation of those findings from perhaps the overall consideration could be pretty difficult. Now, again, I don't see any show stoppers here. It's just a recognition that some of these findings that they make we would anticipate at this time would need to be reconsidered in view of additional information later on. Where I look forward to working with DOE as saying, okay, what specifically would you expect to find, to what extent do you anticipate further reconsideration, and it would be my expectation we would reach a mutually agreeable way in which these things would be done so they would proceed along the course; yet, we would have confidence that the findings they were making would withstand the scrutiny of our review of the license when the findings were incorporated in their application.

You had some concerns as to how the NRC reacted to the adequacy of bounding or conservative assumptions. I think, first, the idea of bounding assumptions does not seem to be unreasonable, but it would appear to add a level of complexity to the application. For example, Steve has mentioned that DOE may be moving in the direction of lower heat loadings in the application. I'll note in a moment that there's a body of technical thought which the NRC, I think, is a member--yes, that would make things simpler and I think that could ease the NRC review of the application.

Alternatively, if DOE were to choose in this particular case to set down the heat loading--say, you're going to come in and say, well, we might come in as low as 25 kilowatts, we might come in as high as 150 kilowatts, and we have found that it works at both the low and the high heat loading. Obviously, the repository is an extremely complex process. If we were to model it, we would have all kinds of non-linear equations. So, from the NRC's view, the fact that it worked at high and low heat loadings does not necessarily imply that it would work at intermediate heat loadings.

Now, let's say--just make things up--that DOE did come in and they wanted to cover a range like that. For other reasons, they might want to come in with a range of infiltration rates and perhaps a range because they didn't have sufficient data to have high confidence in the pH that the waste package might encounter. At this moment what I would envision as necessary for them to make the case would then be an analysis that covered the ranges of these parameters. And, if they've got end parameters, then I'd be looking for something like an end dimensional response surface with a demonstration of everywhere on that surface they are meeting the goals that we have in Part 60 and meeting the EPA standard. Hopefully, for a number of these bounding assumptions, that would not be necessary. They can pick parameters and say if it's less than this or greater than this, we have no problem. But, we envision that there may be a number of parameters where a bounding assumption will simply not by itself work. We may have to establish two bounds and analyze the distance between the bounds. Having said that, that's not necessarily an unreasonable way to proceed. I think it would depend on the amount of complexity and uncertainty that would be further introduced.

One could also make the argument with respect to some of these parameters, such as heat loadings, that the repository is going to go through a pretty significant thermal cycle no matter what. And, that the uncertainly introduced by the changes in temperature as it goes through this thermal cycle may be large compared to some of the uncertainties introduced by certain heat loading patterns.

That's an argument to be considered.

What I've mentioned here are considerations that we have and, until we get into the detailed structure working with DOE on this, we won't be in a position to say the extent to which the concerns that I'm mentioning here are substantive or the extent to which they can be readily resolved.

To get into a little more detail with respect to thermal loading itself, I have some concerns about high thermal loadings because my staff has not yet been able to demonstrate to me that we have a sufficiently good understanding of the dimensionless groups that describe the phenomena of interest for me to believe that we really have high confidence that some thermal loading studies can be extrapolated into centuries. With respect to the length of the tests, though, it could be argued that if initially we had anticipated extrapolating five years to five centuries that the difference between a five year test and a three year test may not be great in terms of the additional uncertainty. I can only say that absent a really good understanding of how to extrapolate the results, clearly the more conservative we are, the longer the tests that we can run, the lower the heat loadings that we get to, the more likely it is that we would have confidence in the extrapolations that DOE would bring forward; which would certainly get to the point that from our viewpoint, as soon as DOE is in a position to do so, we would encourage starting up heater tests and corrosion tests. The sooner they can start, the more data that can be taken over time, the more comfortable we think we would be with receiving their application.

My final point this morning has to do with our views on the adequacy of using a Title 1 repository design. I need to get and my staff need to get a better understanding of exactly what the Title I design would be before we could give you sound comments on this. As I understand it right now, Title I design would be about a 30% complete design. It would demonstrate the functional properties of the various elements of the design, but would not be sufficient complete that you could, say, take this design to a contractor and seek bids. Given that, some of the earlier comments I've mentioned on bounding analyses apply. If there is a range of parameters or a range of drifts, a range of sizes, that might be in the Title I design, I think we'd want to look at whether or not that range would be consistent with meeting Part 60 and meeting EPA standard. Given that that could be done, I think a Title I design is something that we could analyze. It falls into the same concerns that I've had earlier. If we have less data, the analysis would become more complicated and the risk that DOE might not have a sufficient application would be increased. But, this is to a degree qualitative hand-waving. Until we see more detail, we're not going to be able to say whether we would have serious concerns or we believe that the DOE approach would be entirely workable.

One note that we have on the concept of a Title II design on the waste package compared to a Title I design on the repository, obviously the waste package is going to have to encounter whatever environment the repository produces. If DOE were to take the position, as my staff currently understands DOE is headed, of having the Title II design or the Title I design as the basis for perhaps separation of the waste package from the repository and, say, for example, we're going to have a waste package that can withstand a temperature range from X to Y, get back to some of the other numbers, a humidity or water range from X to Y, a pH from X to Y, now that's the spec we're going to put on the waste package and we would like the material scientists and engineers to go design a waste package that can withstand all those conditions. Then, we will do a Title I repository design which will fall within those conditions. That development of separation between the package and the repository can conceivably be very attractive in that it would provide direction to the waste package designers now and would relieve them of the obligation of waiting for some years until a repository design was more complete. Now, to the extent to which this is a fundamental part of DOE's thinking, while we see pitfalls with it, we certainly are prepared to listen to it and why this might be a workable idea.

That pretty much completes my overall comments. To summarize, we still need to learn a great deal more about their program. It's certainly our intent to work with them as they flesh it out. As Steve alluded to, we have management meetings. We'll be having our next one with them on July 26 and I would anticipate today's discussion will be providing grist for that meeting. The more we can work with DOE to understand what their plans are, I think the more likely we are to be able to give constructive advice to have a sound license application come in.

I'd be happy to address any questions I can.

DR. CANTLON: Questions from the Board?

DR. LANGMUIR: You mentioned that the NRC at this point is favoring a lower thermal loading choice and I assume, therefore, that you would have to conclude that there would be more water in contact with the waste sooner and for longer periods of time. Does that then mean that the NRC is favoring a more robust waste package?

MR. KNAPP: You're interpreting things, I think, beyond what I intended. My intent is to simply say that I believe and my staff believes that it is simpler to model a repository with a lower heat loading. We're not in a position to say whether we favor it or not. The point I wanted to use to illustrate is that--and, it's not, I think, inconsistent with what Steve has said. If DOE would be coming in with more limited information, it might be necessary to use models and designs which are less complex. The only point I would make is my staff believes it's less complex to model a lower heat loading repository, but that's a view which is arguable. There are those that will say that even--you know, you could have a hot repository, as Tom Buscheck would say, that very much simplifies what you're going to have to put up with in the waste package. The question I might ask is how much data do we need to model a hot repository to have high confidence that we're not going to have some sort of resaturation or internal rain? Will the new DOE program provide that data so that if we have a high heat loading repository, we have high confidence that the model, in fact, will describe what will happen? But, we don't have a position one way or another on whether high or low is better. We're waiting to hear what DOE would have to tell us and those are just some of the places we'd be coming from when we heard what DOE had to say.

MR. APTED: In the last year, the NRC staff has brought out a position paper on use of probabilistic approaches for low-level wastes, risk assessments. Do you know, is there any plans within the NRC to extend that same line of reasoning and paper to the area of high-level waste?

MR. KNAPP: Right now, my plans for high-level waste are very much up on the air. Our intent, of course, is to look at the program from the perspective of DOE's PPA and to be responsive to that. That might involve a certain amount of negotiation as to what guidance we can provide, when we can provide it, and what we can do on performance assessment. What I would anticipate doing right now is working with DOE between now and about September when they have the program, their program better defined. It is my commitment to the commissioners, I will have our program much better defined by the end of September and those are the kinds of decisions I'll be making then. Certainly, that will be on the table.

In our view, performance assessment is going to be pretty crucial to a number of things that I've mentioned today including such things as knowing what the timing and the quality should be on some of these higher-level findings. How we will document that in terms of, for example, something comparable to the low-level waste report, we really haven't decided. It's going to be basically a mix of what do we think we need to tell DOE and the rest of the community and what resources have we built.

DR. REITER: Mal, in reading over Part 60, it points out very clearly that during the--between the construction authorization and the license to receive waste, DOE is allowed to conduct tests which allow them to modify their thermal design. The words used in Part 60 are the original design. They have alluded to that they sometimes might not come with original design or the range of designs. I think you stated--I want to verify this--that you're not going to a priori reject coming in with a range of designs as long as they can justify that. Is that correct?

MR. KNAPP: That's entirely correct.

DR. CANTLON: Let me ask the regulations are currently being looked at at the Academy and soon EPA will be running forward its standards. In the feedback that you're getting in that process, do you foresee any substantive change in the way you will approach it by, say, 1998?

MR. KNAPP: I'm not really ready to second guess the Academy or how EPA might deal with the Academy's responses yet. What I'm hearing right now suggests that much of what we have in Part 60 will remain intact. Certainly, we will meet whatever EPA standard that agency eventually establishes, but I don't see substantive change to Part 60, such as, for example, changes in the favorable or potentially adverse conditions, changes in the performance objectives. The bottom line number may change. We may be talking about dose versus health effects versus radionuclide release. But, I would expect that in terms of the topic of this meeting, I think, that Part 60 will remain very much the same. But, I have been surprised before and I might get surprised again.

DR. CANTLON: All right. To pursue that, the original regs were really designed to choose among sites more than to evaluate the interplay between an engineering system and a site. The allowance for the performance of the engineered barrier is not as well-defined in the regs that you work from as maybe perhaps they might be were the regs written differently.

MR. KNAPP: Forgive me, is that a comment or a question?

DR. CANTLON: Well, I'm wondering whether or not NRC is thinking about giving more allowance for the performance of the engineered barrier?

MR. KNAPP: We certainly are prepared to listen to what DOE would have to say in terms of where they would like to take credit. And, as I think most of us know, the various performance objectives on parts of the repository system are numbers or whatever numbers the Commission itself would choose to apply to give the commissioners the freedom to change those. I can't really speculate on what the commissioners might do at that time. I can offer a few thoughts.

As one of the principal authors of Part 60, it was our intent to have a multiple barrier approach. That we would have confidence that in the event that our understanding of the engineering system went awry, we would still have considerable comfort in the way the geologic system would perform and vice-versa. I certainly do not see any change to Part 60 or to NRC's interpretation of it that would relax that fundamental concept. That we would want to be in the position where we would have high confidence in both the geologic and the engineering system. So, with respect to that, if DOE were to come in with an application that placed almost no reliance on the geologic setting, I think the NRC would look askance. On the other hand, we certainly would be willing to recognize--and, as I mentioned, the reg has the flexibility that if DOE brought in an engineering design that was super that we would be in a position to reconsider how rigorously we would need to see them meet the requirements for the geologic setting.

That's the best answer I can give you.

DR. CANTLON: Thank you. That's what I wanted.

Other questions?

(No response.)

DR. CANTLON: All right. Thank you.

Don, you're going to chair.

DR. LANGMUIR: I'm Don Langmuir, Co-Chairman, with Pat Domenico of the Board's panel on hydrogeology and geochemistry. I'll chair today's session on radionuclide migration.

The presentations will address a variety of concerns related to the potential for radionuclide release and migration from a proposed Yucca Mountain repository. Specific topics to be covered include current and planned research related to radionuclide transport; performance assessment and radionuclide transport; the potential effects of various thermal loading strategies on radionuclide transport; waste form dissolution and alteration; and gas transport of radionuclides. The last talks will consider the effects of engineered barriers and Mick Apted will give us a talk on engineered backfill on radionuclide migration and retardation. Ardythe Simmons then summarizes the DOE effort on radionuclide migration.

After Ardythe's talk, we will hold a roundtable discussion of important issues and concerns relevant to the day's presentations, including further questions for the speakers if we need to do so, if there's insufficient time during the day. The roundtable will include the speakers, interested Board staff and Board members, and our three consultants to the Board who are sitting here at the table; Mick Apted, you've already heard from this morning, Rob Bowman, and Don Rimstidt. Our consultants have been invited to participate in questioning of today's presenters that you've already heard, as well as serving on the roundtable.

Many of you have already met Mick Apted. Mick received his PhD in geochemistry at UCLA and has had 13 years experience in national and international research management and consulting related to nuclear waste disposal. This has included four years on the Basalt Waste Isolation Project and six years at Battelle Northwest Laboratories in Richland. Mick is presently at Intera Sciences in Denver where he is a manager for the performance assessment group and projects involving nuclear waste disposal for the DOE and eight international clients.

Rob Bowman is a professor of hydrology at the New Mexico Institute of Mining and Technology. He has an undergraduate degree in chemistry from UC Berkeley and received his PhD in soil chemistry from New Mexico State University. Before his appointment at New Mexico Tech, Rob was a soil scientist with the USDA. Over the last 13 years, he has published extensively on the geochemistry and hydrology of soil waters and ground waters. With papers that have dealt, for example, with the transport of solutes including nonreactive tracers, unsaturated zone fluid flow, and sorption of metals and organics. Recently, he evaluated the performance of potential hydrologic tracers in Yucca Mountain tuffs in a subcontract for the USGS Yucca Mountain Project.

Don Rimstidt is professor of geochemistry in the Department of Geological Sciences at Virginia Polytechnic

Institute. He holds a master degree is geology from Indiana University and a PhD in geochemistry from the Pennsylvania State University. His doctoral thesis title was the "The Kinetics of Silica-Water Reactions". Over the last 23 years, he has published extensively in areas that have included the kinetics of rock and mineral-fluid reactions and the aqueous geochemistry of hydrothermal and geothermal systems. Don recently published the results of a study titled "Element Redistribution in a Yucca Mountain Radioactive Waste Repository Produced by Evaporation and Condensation of Water in the Thermal Field". Another recent paper is titled "An Experiment to Simulate Mass Transport Near the Yucca Mountain High-Level Radioactive Waste Repository". I believe both studies were funded by the State of Nevada.

With that, on behalf of the Board, I would like to welcome you all for the second time today and turn the meeting over to Ardythe Simmons of the Yucca Mountain Site Characterization Office. Ardythe's presentation is titled "Current and Planned Radionuclide Research".

MS. SIMMONS: Thank you, Don.

I'm going to set the stage for the talks that you're going to hear subsequently. By doing that, I'll discuss a bit about recent progress that we've had in the radionuclide transport program, proposed changes to the program, some of the transport features that may lead to poor site performance, the key assumptions that go into our transport work, the integrated approach that we've been following.

We had two major meetings in 1993 that summarized the work to date in the area of radionuclide transport; the Colloid Workshop and a Technical Exchange with the NRC. Since that time, quite a bit of new work has been done and you'll hear some rather exciting results of that during today's presentations. One of the things that I will be talking about a bit more and Ines Triay and others will be following up with is our development and implementation of the colloid strategy that was developed at the workshop.

In the area of solubility and speciation, some of the things that you will hear about today coming from Drew Tait are the fact that we have determined the hydrolysis constants for neptunium. We've finished looking at neptunium, plutonium, and the Americium solubility in UE 25-p-1 water from oversaturation and we're completing that work now with regard to undersaturation. We've incorporated solubility data into the total system performance assessment that was recently completed.

We've done some work with sorption onto organic molecules and although that's not specifically going to be addressed today, we've been able to demonstrate that, so far, we don't believe organic molecules will have much of an effect on sorption.

And, we've looked into the methodology to study sorption in unsaturated tuffs and this will be discussed later by Jim Conca. In addition, we've been able to provide an improved source term for TSPA and Ray Stout will be talking a bit about this. And, collectively, the people working in the radionuclide transport program have been able to have an improved understanding of how TSPA will use the data. This drives the collection of data to greater sensitivities and more focused approach. Some of these areas are in temperature effects on solubility, on neptunium sorption tied to calcite which Ines will talk about later, the distribution of sorptive capacity by stratigraphic unit, and we're taking a look at the major performance uncertainties with the potential releases of colloids. We'll have several presentations in the area of PA that will help to address these issues a bit more.

Now, I'd like to say a little more about where we're going with regards to colloids. As a result of the May 1993 workshop on colloids, we first had to decide how we were going to identify different types of colloids. So, we have used this nomenclature which we will continue to use throughout our investigations. And, basically, there are two categories; groundwater colloids which are naturally occurring and anthropogenic colloids which are all those which result from processes acting on human-introduced materials. Those include degradation colloids which come from the waste form itself; precipitation colloids which include radiocolloids, those that are formed by hydrolyzed actinide ions, particularly plutonium; and then, pseudocolloids which are formed by the attachment of radiocolloids to other colloids like groundwater colloids. And, i want to mention that nomenclature because that plays an important role in our colloid strategy.

We have here three boxes dealing with the different types of colloids; degradation colloids and pseudocolloids and precipitation colloids across here. And, for each of these types, we have to answer various questions related to do the colloids form, are they stable, and will they be transported? So, first, we ask that with regard to the degradation colloids. If they're not formed, we don't have a problem. If they are, we come down here, we look at are they stable? Do they travel through the unsaturated zone, do they travel to the saturated zone? We do the same thing for the pseudocolloids or the precipitation colloids that are formed in the unsaturated zone; follow the same set of steps. And, we do the same thing with the pseudocolloids or precipitation colloids which are formed in the saturated zone. When we've been able to satisfy these answers, we will provide performance assessment with the colloid concentration that is obtained through these processes, the radionuclide content associated, and the attachment/ detachment parameters for the stability of the colloids. So, those questions have to be asked in all three areas.

We have some ongoing work in the area of colloid investigations. And, these studies include the formation and stability of plutonium and other precipitation colloids; the presence of past colloids at Yucca Mountain which Schon Levy will be talking about a little bit later today; and, the stability and sorption reversibility of colloids through saturated column experiments and unsaturated fractured column experiments. Both Ines and Jim Conca will be talking about that a little bit.

We also have some planned work in this area on the generation of degradation colloids from spent fuel dissolution. We have some results from the generation of these colloids on glass waste form which was conducted by John Bates at Argonne National Laboratory and we have some plans for investigating their generation from spent fuel, as well. One study that is planned for fiscal year '95 is the C-Wells transport experiment and, in conjunction with that, we will be introducing colloid-sized microspheres to take a look at their ability to transport. Then, we'll be looking at the colloid stability under heated conditions. This comes under the integrated testing effort.

In addition, we'll look at the formation of colloids by EBS materials, such as those that are produced by corrosion products of the waste package or by manmade materials, cements, and so forth. Then, we have a plan for a field-scale transport test of colloids that will be conducted at the Nevada Test Site which I'll talk about a little bit more. And, PA will incorporate the information from these various tests regarding the number of colloids, their stability, and so forth. And, when PA is able to demonstrate that the colloids are insensitive in the model, we will terminate this work.

This is a schematic of the test that is proposed up on Rainier Mesa in an area where there was a previous nuclear explosion underground and it's at a location in welded tuff in the unsaturated zone; however, under more saturated conditions than at Yucca Mountain. What we have proposed to do is have two angled holes, one that would come directly beneath the puddle glass and then one that would come at a slightly deeper depth and I want to point out that we're not sure of the location of the water table here. So, this second hole may go beneath the water table or it might be at a great depth, whereas the water table may be down here somewhere. The idea of this test would be to try to find out to what extent radionuclides have been transported in colloidal form. This test was conducted in 1969 and so it's old enough that we would be able to see, we're fairly sure, some idea of transport if it had taken place. This test is planned later, not in 1995.

We have some changes to the transport program with regard to the proposed program approach that both Sam Rousso and Steve Brocoum talked about earlier. One of the things that we have done is to apply the existing formalized strategies that we have to reduce the testing matrix to a minimum that would be needed for confidence with the radionuclides that are listed there. You've heard before about our Kd strategy, about our solubility strategy, and so forth. These are designed to focus the work. Secondly, as you've already heard this morning, there is an increased emphasis on the near-field geochemical investigations, as well as the coupled process work and we will be investigating those more thoroughly.

We have deferred some of the work on the colloids to license application time rather than site suitability time in 1998. An example of that would be the tests that I just described on the Nevada Test Site, the field test. However, we are planning to initiate a radionuclide transport experiment in P-tunnel and some information for that would be available by the time of the site suitability determination. The purpose of the P-tunnel experiment would be to give us some information on field-scale transport which we would not have otherwise. And, the P-tunnel site is in tuffs very similar to the Calico Hills.

Now, some other features that would lead to poor site performance which are things that we must keep in mind in all of our investigations; what would be the most conservative scenarios? And, these would be if we had high radionuclide solubility, low or no sorptive capacities in the tuffs, significant transport along fast pathways, high flux, if the transport pathways bypass the Calico Hills--such as going directly down the Ghost Dance Fault or some other major feature like that--if colloid transport was significant, and if we had features of the EBS that would be detrimental to waste isolation, such as introduced materials that might alter the pH significantly. We would hope that all those factors would not be in combination, but those are the things that we have to look at.

There are some key assumptions that go into the transport work. One is because we're in the unsaturated zone, we have oxidizing conditions. That is an assumption that we need to question as far as what happens near the waste package where we could have more reducing conditions potentially and also it's possible that in the saturated zone we'll have more reducing conditions. So, we're looking at that, but right now, that's an assumption.

Also, we're going with the assumption which has been provided to us by the designers that we're going to have a thermal loading of 80 to 100 kilowatts per acre. And, I would say again that that's an assumption, not a decision, but we need to understand the capability for transport under those conditions where you're going to have altered tuffs.

And, third, we're bounding the groundwater chemistry by J-13 and UE 25 p-1 water. As you probably remember, the J-13 water is in the welded tuffs and the UE 25 p-1 water is in the carbonates below. In this diagram, we see the compositions of the waters and I'd like you to focus just on the lower left here. The J-13 is this triangle right here and the UE 25 p-1 values are within this range here. Now, the other water compositions that we need to look at are what happens when we evaporate the waters at the site to more concentrated conditions and what is the water like in the unsaturated zone; like what Al Yang has been squeezing out of the tuffs. And, we have found that Al's water plus where this red circle is right down here--it doesn't show up on your hard copy, but it's right here on my viewgraph--it's a bit lower magnesium, but it compares in sodium, potassium, and calcium within that range that you see here. The evaporated waters plot down here at this apex for sodium and potassium. So, within this range, we have a good bounding of what the water chemistry would be.

Now, we have tried to provide more integrated approach to the understanding of radionuclide migration because each of the components in the system is critical to the multi-barrier approach. For that reason, for the recognition of the need for greater integration, we've caused some programmatic restructuring. Without belaboring the point, this is the flow of information on all of the activities that we have in the radionuclide transport program. Mostly on the left hand side, you see what we've traditionally called the far-field program. There are some other studies in here. And, up in this area is mostly what was formerly called the near-field program, but all of these studies are linked together as you can see by the massive network of arrows.

And, finally, here's our repository performance defense in depth. And, if you think of today's presentations with this diagram in mind, following my talk we're going to have some presentations by performance assessment including TSPA and that of the waste form and waste package PA with regard to the source term. And then, from there, we're going to start from the far-field and the conditions and move our way inward to talk about the near-field environment.

So, the first set of presentations will be by Bob Andrews and Ralston Barnard on PA and then we'll hear from Ray Stout on the source term. After that, we'll hear some presentations about what the flow conditions would be and the transport model for the far-field by Ed Kwicklis who is replacing Bo Bodvarsson and by George Zyvoloski. And then, we'll get into our far-field transport presentations by Ines Triay, Jim Conca, Schon Levy, and Arend Meijer. And, finally, we'll conclude with our near-field presentation by Rich Van Konynenburg.

And, that's all I have to say. If you have any questions?

DR. CANTLON: Thank you, Ardythe.

Any questions from the Board?

DR. PRICE: Ardythe, I'm just a little confused about that first YMP colloid strategy figure that you showed when you go down to do the travel through the saturated zone. I noticed you have unsaturated knocked out on yours and--

MS. SIMMONS: Yes, this is a typo I forgot to mention on the diagram. This should read do they travel through the unsaturated zone, do they travel to the saturated zone. So, you want to correct that.

DR. LANGMUIR: I have one quick question. You showed a range of compositions of waters being used in your studies including Al Yang's unsaturated zone analyzed waters and also an evaporative water. Are you also looking at evaporative waters created at higher temperatures as kinds of water to deal with in your modeling?

MS. SIMMONS: Yes. That range of conditions included the waters that we would have from repository heating conditions.

DR. LANGMUIR: I think we need to go on. Pat Domenico, a short question?

DR. DOMENICO: Ardythe, I recall reading some literature where some scientists have stated already that some radionuclides have moved from the underground testing and they have attributed that movement to the presence of colloids. Are you familiar with that literature?

MS. SIMMONS: Yes.

DR. DOMENICO: Are you in, more or less, agreement with that?

MS. SIMMONS: We have one very comprehensive study of the Cheshire Site at the test site in which we have had some movement of colloids. There have been other tests that were done at the test site and that were investigated later to look at transport that have not had colloidal transport associated with them. And, all of that work was presented at our colloid workshop.

DR. DOMENICO: Thank you.

DR. LANGMUIR: We're a little behind schedule now. Thanks Ardythe.

Our next presentation is by Ralston Barnard who will speak about performance assessment and radionuclide transport.

MR. BARNARD: Good morning. I'm going to talk about performance assessment and radionuclide transport, especially as it pertains to the recently completed total systems performance assessment that, hopefully, everybody in the world has gotten a big, fat copy of.

One of the things requested to talk about was what kind of parameter abstractions have we been doing and do we have any success stories; successes in doing parameter abstractions. And, an important concern throughout all this, we also are very interested in making sure that we don't collect data for the sake of collecting data. In other words, what is the sufficiency--do we have any understanding of the sufficiency of the data that we need so that we don't continue collecting ad nauseam. In the Board's tenth report, there were recommendations specifically to do the sensitivity studies for source term and for near-field modeling. I'll cover some of those also.

Well, talking about a success story in parameter abstraction and model abstraction, the development of our source term model, I feel, is definitely that. When we first did PACE-90, with all due respect to Mick and others who helped to come up with the source term, there was considerable smoke mirrors and wizardry going on and it truly was a black box. For TSPA-91, we cracked open the box a little bit and started to understand some parameters going on in there. For the most recent one, TSPA-93, it really required very little invocation of magic for us to come up with a model and we had a genuine phenomena logical model of the source term and we feel considerably more comfortable with what we ended up using. An important point that I want to make here is that studies of parameter uncertainty and variations when done at this level are essentially meaningless because we have no concept for models. So, a variation of the parameters in there is not going to teach us much. By the time we've gotten down here, we do have some confidence that if we do investigate/explore ranges of parameters that we're going to find out something useful about them.

To continue with a discussion of the source term, it's based on Lawrence Livermore's YMIM module and I've kind of color coded here the different components of the module because different speakers later this morning are going to cover some aspects of this. And, I'm not going to go into that much more detail. I'll just tell you for TSPA-93 what some of the assumptions are. It's important that everybody realizes that we made assumptions at the time we started the analysis and whether those assumptions are now current or not is not something which we can argue at this point. But, our assumptions were based on the YMIM that the waste package corrosion is strongly temperature and water contact dependent. We assume for in-drift emplacements that the repository was backfilled after 75 years and some of the results of our TSPA analysis are a direct consequence of that assumption.

We also assume that the cladding was not a barrier to radionuclide release. That if you got that first hole in the cladding that it went poof. I guess this is the invocation of magic at this point. But, there was very little retardation or reduction of mobilization of radionuclides based on the cladding. We also assume that the mobilized radionuclides from the waste package are easily transported from the EBS to the near-field rock and then off by far-field transport mechanisms with little or no time delay. Mick, later on, is going to talk about a possible modification to EBS structures which would introduce a significant time delay, but understand that we considered none.

For solubilities, we did considerably more for TSPA-93 than have been done previously, but I won't dwell on that because that was discussed in January. But, I do need to point out that the knowledge that we have was that of far-field parameters and not the near-field conditions. So, although solubilities occur under near-field conditions, what we knew based on expert elicitation and so forth is only the far-field parameters. So, that's what we had to use.

For the transport model, we made two extreme conceptualizations for water transport; the Weeps model and the composite-porosity model and those have been discussed, I think, to the point of glassy-eyedness of people. So, I won't dwell on them. What we considered in our model is that sorption strongly controls which radionuclides can be transported to the accessible environment. There will be one distribution of radionuclides which escape from the EBS from the waste package, but there is another distribution of radionuclides which are those that reach the accessible environment.

We did not consider colloid transport and the reason for that is at the time we did this analysis we did not have a model of colloid transport that could be abstracted to be used in the TSPA. As this work continues on development of colloid models, we hope to get a model that we can include in our TSPA analysis. I fully expect that it will follow the same steps as it has for the source term where we start with a lot of smoke and mirrors and develop it further as we work with the process modelers.

Lastly, thermal effects used in TSPA-93 only apply to the near-field. Once we said we have far-field transport going on, it was completely isothermal.

Some of the consequences of our TSPA-93 assumptions with respect to source releases can be seen here. Under some circumstances, we have large releases from the EBS; such as points here, for example, for technetium. That line is neptunium, protactinium is there, the yellow is iodine, and plutonium-239 is the red line there. And, roughly speaking, the only releases which exceed the NRC's 10^{-5} of the repository content at 1,000 years are those of technetium which is that line there, a biggie, and selenium didn't make it. That's one of the other--no, sorry, that wasn't it. Yeah, technetium is one of the biggest releases of the NRC release criterion. This is the vertical placement, 57 kilowatt per acre with average parameter values. The reason the releases are bad is because of the threat window which we have described previously which is from about 100 degrees down to about 70 or 80 degrees centigrade which, if the waste package is at that temperature and there is water present, the YMIM model predicts rapid corrosion and the waste package can go away in a matter of 100 years, at which point you get releases like this.

Now, remembering this curve, we have to look at see what happens when we talk about releases at the accessible environment, the doses after a million years. Now, we see the majority of the doses come from neptunium-237 which has a Kd, a small Kd retardation value, of protactinium and technetium, but most importantly, the only releases we are able to detect from plutonium which has a very large Kd is that tiny little bit of the pie right there and only for the Weeps model. The difference is that the composite-porosity model predicts on average very slow travel in the unsaturated zone and during that time, the plutonium is effectively retarded to the point that it's not available to be transported in the saturated zone.

I'd like to discuss some of the uncertainties in the transport model. What this amounts to is a wish list of enhancements that we feel we need to make for future TSPAs. The transport process from the waste package to the near-field host rock may indeed be a significant barrier to releases and we have not yet modeled that with the YMIM model. It does not include a component for that. We recognize that releases are sensitive to the choice of the flow model that we use and we also recognize that a 1-D composite-porosity model really doesn't describe fracture flow in a realistic fashion. We are developing a 2-D model which will be dual-porosity or dual-permeability to allow for analysis of both fracture and matrix flow, as appropriate.

We recognize that we have to include colloids in the models and we feel that there are three important source terms for the colloids. One is spent fuels, as Ardythe has pointed out, from the defense high-level waste glass and we are concerned that a large amount of iron in the form of the outer layer of an in-drift waste package could produce a lot of Fe_xO_y iron oxides which could act as colloids, the pseudocolloids, and provide a ready source of that mechanism. I know there's a lot of work going on of fractures' surfaces and how they may interact with colloid transport and fracture filling and we hope to be able to include some of that information in an abstractable TSPA model.

How do we expect to interact with the site and process site characterization of process modelers? We feel it is necessary, very necessary, to have a better understanding of container corrosion processes. One of the great sensitivities of the YMIM model is to the corrosion process, as I have mentioned. Certainly, we can't continue forever using far-field parameter values for solubility when a priori we recognize that those are not the conditions that are obtained in the near-field. So, this is something that we consider very important and we need to work with process modelers to get something to include in TSPA analyses.

The YMIM model does not include a process description of transport under dynamic conditions from the waste package to the host rock, the near-field host rock. And, it's necessary to include factors such as high temperatures and limited amounts of water to model the transport that we're looking at. It is quite possible that there will be an aggressive geochemical or radiolytic environment depending on the waste package that's used.

Radiolysis may not be important outside of the waste package, but it would certainly be important inside. We are working with Lawrence Berkeley Labs and the USGS. Their site stratigraphic model, their site scale model, is going to be extremely useful for helping to understand those areas of the site where further analysis would be particularly fruitful in terms of expecting fast paths to occur. We expect that site data--we will incorporate new site data and recommendations and use those to feed back to the site data collectors for even further analyses.

At Sandia, we are attempting to improve our PA models by means of what we call the PA integration tests and those are four in number. One is to try to model flow in discrete fractures. We are looking at heterogeneity and the importance of upscaling of parameters from the parameter measurements that are made on core the size of your fist and scaling the range of parameter values that you measure to mountain-sized or, at least, room-sized modeling domains. We're looking at non-isothermal flow and the geochemical retardation.

There are two important PA models that we will be abstracting these results into. One has to do with groundwater travel time. That's going to be an extremely important factor in the PPA in evaluating the suitability of the Yucca Mountain site. And then, radionuclide transport, as we develop and abstract these models, that will be used in future TSPA analyses.

You can't keep collecting data forever. There's got to be a point at which we decide that we know all that we need to know and that further data will not have any worth for the analysis. From the previous slide of the heterogeneous groundwater flow domain that we anticipate developing for the groundwater travel time analysis, it is going to be based on a three-dimensional geostatistical rock model. Chris Rautman and Tom Robey are developing that. This will allow studies of the sensitivity of fast paths to hydrologic structure. So, once we start to see which hydrologic structures and which areas and what data are important and the results are sensitive to those, those may help us to decide whether it's necessary to collect more data of that type. It may turn out the data from faults and fractures are more important than just drilling another hole and collecting matrix data from punching in another hole. This, we hope to pursue further and be able to provide feedback to the data collectors on a timely basis.

Now, we've heard a lot about the pursuit of the MPC, the in-drift waste package, and certainly the repository layout associated with that. That may end up developing the thermal regime. Certainly, in TSPA-93, we had a matrix of four possibilities, both the vertically emplaced SCP, hot and cold, and the in-drift hot and cold analysis cases. If it looks like the SCP is going into eclipse, then there are fewer analyses that we need to do, but we hope that we are--that as a result of a design constraint, we aren't precluded from determining what performance assessment results are--will result in the best behavior of the repository. And, I won't dwell again on colloid transport except to say that until we know more about colloid transport, we're going to have to keep modeling, we're going to have to keep doing experiments because we will be unable to bound its performance.

Whenever we write a TSPA document, we always put a chapter in which lists tons of recommendations of what to do. In the past, we felt a little like Rodney Dangerfield on occasion and maybe that's just because we've provided a list of recommendations in the PA document. This time, it's really gratifying because being allowed to make formal presentations to groups such as this and to the TPOs and the WBS managers gives the worker bees a chance to really tell the opinion molders and shapers what we think is important. Peer-to-peer interactions where the participant PIs get together in informal exchanges of data and results and feed back to each other the types of results are also important resulting in, in both of these first two cases, we're starting to see inclusion of PA recommendations into the future planning activities and milestones and budget activities. So, it is certainly rewarding that we're doing this and seeing some kind of active response to our recommendations.

So, to summarize this, we certainly have some strengths and weaknesses in our recent TSPA analysis. The source term is both a strength and weakness in terms of having a more--we're more comfortable with the model of what's going on. The flow and transport models, we recognize are preliminary, but we hope they do bound the most extremes of the types of flow that would be expected there, although they are certainly not realistic and we expect to make future models more realistic. And, the geochemistry, we have a start, but we expect it will take iterations of this to improve our confidence in what we have.

The whole emphasis of this is an attempt to reduce the uncertainties that we have and to have confidence in the results that we have that we can use them to make a judgment about site suitability. The PA integration effort that I described is an attempt to reduce some of these uncertainties. And, lastly, I would just remind you again of the process uncertainties which as they get resolved will allow us to have more confidence in our TSPA analysis.

Thank you.

DR. LANGMUIR: Thank you, Ralston.

Questions from the Board?

DR. CANTLON: Ralston, I don't know whether I understood your correctly, but in your overhead #11 you were commenting on the movement toward the large in-drift robust container and you commented that you hoped that that selection would not constrain your exploration of some of the other models. Could you enlighten me as to why that would be important?

MR. BARNARD: Well, this may be heresy, but the TSPA analysis, if you have had time to look through it, the in-drift waste package configuration is not a clear winner. It has certain--based on our models, it has certain performance assessment results which are not as good as the SCP model. I am concerned that the choice of the MPC is going to preclude --we are going to be boxed into accepting performance assessment outcomes which are not the optimal, the best, and there's going to be no recourse for us. It will do no good for us to analyze and point out that a bigger target for human intrusion drilling, of course, is going to result in a bigger release. A bigger target for Weeps, a fracture, a water flow to hit is going to provide a bigger target and, thus, a greater likelihood of having waste package degradation. Those are just two examples that I think you'll see appear in there. I was trying to couch this in really politic language, but I just blew it. Okay?

DR. CANTLON: That's the role of the Board.

DR. DOMENICO: You mentioned one assumption that thermal effects apply only in the near-field. I didn't see any place where thermal effects could be incorporated in the Sandia model. What do thermal effects do? In the sense that you said solubilities were based on far-field information.

MR. BARNARD: It appears mostly in the waste package lifetime analysis. Here, you see the four temperatures that we looked at and at the risk of opening up a full-fledged donnybrook, you can see that with the model that we used for the in-drift or the 114 kilowatts per acre, backfilling at 75 years, you get this monstrous spike in the waste package surface temperature. It results in taking a long time until it reaches 100 degrees down to the threat zone where it starts to corrode, but this big spike and the time that it takes for the temperature to reach 100 degrees control the waste package lifetime, essentially.

Let me also add that this is just waste package lifetime, but also there was a question about the dryout volume of the rock which was modeling assumptions, the work done by Eric Reiter and all. And so, the volume and the extent of timing of dryout, of course, also depended on exactly this because you can see that a 500 degree spike is going to propagate and really dry out the repository.

DR. LANGMUIR: Okay. I'm afraid we'll have to close discussion now. We're already about 15 minutes behind.

MR. BARNARD: Sorry.

DR. LANGMUIR: But, keep the overheads, keep the questions; Rob Bowman had one, I had several. We'll get back at this again and continue discussion at the end of the day either right before the round table or during it. Let's take a break and since we are behind, let's reconvene in ten minutes rather than fifteen, at 10:38.

(Whereupon, a brief recess was taken.)

DR ANDREWS: I'm actually presenting for Dr. Srikanta Mishra, who when we started planning and preparing the preparation of this talk, got chicken pox--I hope there's no correlation between his talk and chicken pox--so I will give it instead. And we'll focus on really two different aspects of the significance of radionuclide transport to he performance, long-term performance of the Yucca Mountain site.

One is the affect of time, because that significance varies with time that we might consider, or with type of performance measure that we might consider and, secondly, the main issue is the impact of alternate thermal management strategies on the significance of radionuclide transport processes.

So the overall outline; first to reintroduce what Ardyth talked about, the relevant transport domains and processes at Yucca Mountain very generally, the relative significance of those processes over different time frames and different performance measures. These are results that came out of TSPA-93 and other supporting calculations done for the NAS committee on Yucca Mountain standards. Possible effects of increased temperatures, increased thermal loads, increased thermal management on radionuclide transport processes, and finally the significance or potential significance of those on performance.

To reintroduce you to the overall system that we have here when we do a total system performance assessment, and a number of these bubbles in this particular case--they would have been squares on Ardyth's viewgraph--will be presented in more detail at the more detailed process level by presenters later on this morning and again this afternoon. But you can see the thermal aspects in the particular areas; one is just thermal hydrology impacting the waste package degradation, waste form alteration, waste package and EBS release, the near field thermal chemical also impacting degradation, alteration and release, and actually in the unsaturated zone, the thermal perturbations that affect flow and ultimately transport.

To point out, we need to go for some performance measures all the way to the biosphere to look at dose or health effects, and there are, of course, transport processes that occur there as well that we generally won't discuss this morning.

This is a little matrix diagram relating to different domains, which you can look at as different scales of problems, and the dominant processes going on, transport processes now going on within each of those domains. So within the waste packaging, yes, we have dissolution, solubility control, diffusion through the EBS and through the package, advection through the EBS if that occurs, and of course radionuclide retardation to go out into the geosphere, the saturated aqueous and the saturated zones, you're dominantly calling advective retardation and dispersion.

I've added down here, just so we don't forget it, matrix diffusion in both the aqueous components in the unsaturated zone and the saturated zone. Sometimes I'll call that matrix inbibition/matrix diffusion. So one's a flow process; one's a transport process.

Now let's talk about the relative significance. These are a summary in very capsulated form of the results that we presented to you in January and Rally has talked about just a second ago from TSPA-1993. And I want to talk about the significance of the transport being a function of the time period that we're looking at. We looked at time periods from 10,000 years out to a million years. It's also a function of performance measure that we're considering, whether we're integrating a release over time, which is the current CFR-191 standard applicable to WIPP and all other non-Yucca Mountain high level waste sites, or whether we talk about a peak dose or peak concentration.

And also it's important to bear in mind that these significances that I'm going to point out to you in the next two tables very much depend on some fundamental assumptions that are built in or buried, if you will, into the analyses themselves. Those fundamental assumptions revolve around--and this is just a partial list, I think we could spend the whole rest of the morning talking about the detailed list--but how we define failure. In all the analyses so far, it's the first pit that goes through the can is quote, unquote, failure. And the cladding, as Rally pointed out, is not taking into account, there's no credit given for the cladding.

So second major issue is, you can call it different things and different people will call it different things, either water contact mode or maybe a better way of thinking of it is percent of the waste form itself that is in contact with the aqueous phase. It's a very uncertain parameter, if you will, and it drives the actual dissolution of the fuel and, therefore, the releases from the package and the EPS sent into the geosphere, but there's certain assumptions made in the TSPA regarding that particular thing. Ray Stout will talk more about this particular issue in some detail with respect to partial cladding failure, et cetera.

Another major assumption is how the fracture-matrix interacts with regards to flow and transport. And finally, not to forget that in the TSPA type analyses done to date, we have relied on, you know, the wealth of data collected by LANL over the last ten years and other organizations on what are appropriate retardations applicable in the geochemical environment at Yucca Mountain for the major nuclides of concern.

I think you've been briefed on the quote, unquote minimum Kd strategy, and those quote, unquote minimum Kd's were used in the analyses. So of course if Kd's were zero, if retardation was one, if the nuclides move at the same speed as the water, other nuclides would become significant in the ones I'm going to show you.

In January, you saw a lot of results from TSPA. You saw a lot of CCDF, you saw release curves over different time periods with different thermal loads, with different waste package designs, with different assumptions of the near field initiation of corrosion, different assumptions on corrosion rates even. What I've tried to do here is quickly capture the relative, from the radionuclide transport perspective only, the most significant processes involving both cumulative release and peak individual dose. And it's different and it's important to point out that it's different.

Let's start out--perhaps I shouldn't have them both up at the same time. That might be a little bit much. Let's start out with cumulative release. So Table 1, 40 CFR 191, and let's look at different time periods of the analyses, so whether we stop our analyses at 10,000 years, 100,000 years or a million years. Now, the Board's already asked the question in January why in the heck are you looking at different time periods. Well, we don't know, and the NAS panel has given very little indication of what an appropriate measure of time is, either from a regulatory point of view or from a technical point of view. They're looking at the technical components of it. So we've looked at all the times.

Over 10,000 years, I think all the analyses done so far indicate carbon-14, the gaseous releases integrated over the 10,000 years are by far and away dominant. Technetium is very, very minor. It's like ten to the sixth of the release of carbon-14 integrated over that 10,000 years.

What's important? Well, the key things in the gaseous are all near field, it's all the can, it's all the waste package failure and the time distribution of the waste package failure and the actual spent fuel dissolution. Here I'm not talking about just dissolution rate per se, but that rate times a real area of exposed fuel.

Everything else--well gas transport is also relatively important, but by and large, as we pointed out in January, most of the times under the thermal loads looked at the gas phase transports are fast enough that that's not really a retarding mechanism in any sufficient or significant degree. So it's much less significant. And what happens in the aqueous component becomes really insignificant, except as it impacts the waste package failure. Clearly, the aqueous component is driving the corrosion rate and driving the failure itself.

Let's look at the aqueous components too, though, because there are things that drive that 10,000 year conclusion that technetium releases integrated over time are very small. First of all, these same first two, failure and dissolution, but now we have what you might consider more the transport, the flow parts of transport processes, both the flux and dispersion, because now we're looking at the arrival of the beginning of the break-through curve, more or less, in this 10,000 year time period. And that's why this little dispersion term due to local scale heterogeneity that it's impossible to incorporate in the model itself becomes a fairly significant term. How nuclides move through the fracture matrix system also becomes significant.

Note what I don't have up here is that minimum Kd, because that was my basic assumption on the first slide, that the minimum Kd holds, and those are some high retardations for most of the nuclides, with the exception of technetium, iodine and a few others which have essentially no retardation in the oxidizing environment we think exists.

Over 100,000 years, now technetium has broken through, and so dispersion becomes insignificant. We're not any more looking at the arrival of the break-through curve; the break-through has occurred for reasonable ranges of aqueous flux. Aqueous flux is still relatively significant if you're at the lower end of the flux distribution.

At a million years now, neptunium starts playing a role. Now, remember, I'm integrating, so it's not just a snapshot at a million years. I'm integrating from zero to a million years, or from release till a million years.

Spent fuel dissolution is still there. And you'll see when I go to dose, and I'm looking at the peak individual dose, I don't care when it occurred in time, I'm just looking at that peak and the magnitude of that peak value. Over the

10,000 year period, most of the analyses done to date, whether you're talking about carbon-14 releases or the aqueous releases, the individual doses are minuscule. They're in the microrem or less range. What controls those are still things that happen in the near field, the waste package, spent fuel dissolution, and EBS diffusion.

When you get up to 100,000 years, now there's technetium and iodine, the non-retarded nuclides are essentially coming out and they are contributing to the dose. And then you have what you might consider your normal flow transport processes in the geosphere taking hold. The waste package failure, and lest you envision very long lived packages, on the order of several tens of thousands of years, if not hundreds of thousands of years, the waste package failure becomes insignificant. Also UZ dispersion, although it has some effect, factors of two or three, and in this discussion we don't talk about factors of two or three.

Neptunium retardation here is significant, otherwise, that neptunium peak might have come out with the other non-retarded nuclides as opposed to coming out much later. This is what it does. But over the million year time period, now things become controlled by neptunium by and large.

Okay, now let's go on to switch gears a little bit, quite a bit, and talk about temperature and thermal effects, what the impact of alternate thermal load might be on the things I just discussed with you. Some of these have already been discussed, but worthwhile reiterating them I think.

First off, the main thing that the thermal regime is going to do is delay initiation of aqueous corrosion. That's clearly some function of the humidity, temperature environment in which the package sits. And you've heard discussions from Dan and you'll hear some discussions from Rich later on this afternoon about this particular issue.

Above 300 to 400 degrees C., a whole different mechanism applies. That's the oxidation of the mild steel, and that can go relatively rapidly, as Rally pointed out this morning.

Once it's initiated, and if I'm at that 80, 90, 95 degree C. range, several other things occur at a little high rate, at least from the laboratory information available to date. We increase the corrosion rate, we increase dissolution rate. The solubilities can be increased or decreased, depending on which nuclide you're looking at, and increases CO2 release.

What to do through the package EBS? Well, one thing is that with a giant question mark there, is it tends to decrease the average advective flux. It also, if I think of having a capillary sort of barrier either entirely around it or just underneath the package, it decreases the water saturation. This, I think Mick had pointed out several times, and Jim Conca in his work, clearly diffusive transport through the EBS is highly dependent on the water saturation and the water saturation is highly dependent on the thermal regime in which the backfill sits.

The gas transport, with increased temperatures, there's increased gas phase advective flux and increase in the gaseous component of the carbon. There's also a reduction, an effective reduction about a factor two in the retardation of carbon as you increase the temperature.

Modifications in aqueous transport in the geosphere, there's some--of course, logically you've heard a number of discussions from Tom Buscheck, several LBL people like Karsten and Yvonne Tsang on what are the potential flow regimes in the far field due to alternate thermal loads and alternate parameterizations. There's also the possibility of reflux water carving some local fracture flow, as Karsten Pruess has pointed out.

There's generally--and this might be a very general sort of statement--an increase in absorptive capacity with temperature. This is coming from some preliminary laboratory data from LANL. That was not incorporated in any analyses so far, so that's on the good side. And, of course, with modified liquid saturations locally and, therefore, the effective transport porosity.

Okay, which of these things did we include in this last iteration? And now I'm going to talk about in your brief in January, there were two TSPAs, one that the M&O was responsible for and one that Sandia was responsible for. We made different assumptions in how to incorporate the thermal dependencies into the near field modeling.

Clearly, it delayed initiation of the corrosion processes, it modified the rates of pitting, corrosion for both the mild steel and Alloy 825, those coming from laboratory data primarily from Livermore and also literature. It did modify both the spent fuel and glass alteration rates, those coming from laboratory data from P&L and Livermore.

We did modify the radionuclide solubilities, even though this was a factor of two or three change with increased temperatures, modified the water content and, therefore, the diffusion coefficient in the EBS, and modified the gas-phase velocities and carbon-14 retardation. That was Ben Ross's work. We did not, however, include modifying the aqueous flux due to that thermal perturbation.

What are the consequences? We talked about these in January, but just to reiterate, higher temperatures delay the corrosion initiation by several thousand years. It was, of course, dependent on the location physically of that package in the repository, you know, were you at the edge, et cetera. It also depended on the thermal load.

If the packages were contacted by moisture and the temperatures were below boiling, but at the higher end of the temperature range, so in that 80, 90, 95 degree C. range, the corrosion rates were generally a little bit higher and, therefore, the failures times were a little bit shorter and, therefore, the carbon-14 release rates were a little bit higher.

If the packages failed at these temperatures, dissolution rate is slightly higher, the releases for the dissolution-limited nuclides were also, of course, higher. Solubility limits could be higher or lower, depending on where you were and what nuclides you were talking about. So the actual releases in the package could be higher, lower or relatively unchanged depending on which nuclide you were looking at.

And although there was an impact of reducing EBS diffusion rates due to the thermal regime due to lowered water saturation, it was not significantly reduced where it had a dramatic impact on reducing releases.

Let me try to conclude here quickly first looking at more or less the same chart we looked at before, but now looking at thermal impacts on those processes that we talked about before. And I think we have to acknowledge that the impact of the thermal management strategy also is dependent on the time period that we're really concerned with. You know, at a million years, with the possible exception of some very long-term changes in rock mineralology that might have some bearing on retardation, there's very little impact of alternate thermal management regime.

But as we come in closer to time, or shorter in time, some other things become really dominant, the initiation of aqueous corrosion is the obvious one. If I keep it hot enough long enough so that the water content is driven away from the packages, then I have a package that never sees water and the aqueous processes of corrosion would occur at much larger times than what we're postulating now.

So in conclusion, the results that I've shown you both in January and now--I didn't actually show you this time, I'll save you that horror--help identify the relative significance of some of the transport processes. And that significance, it's important to point out, is a function of the time period of concern and a function of performance measure of concern. And neither of those two things, given the NAS panel is still out on how EPA might choose to implement what the NAS recommends, we don't know, in a way. And it's also dependent on the fundamental assumptions in the analyses, which of course need to be checked and verified.

And the relative significance of those different transport processes is impacted by that alternate thermal management scheme.

So with that, I'll close and thank you very much.

DR. LANGMUIR: Thank you, Robert.

I'm going to start with something here. Ralston talked about total system performance, and in his presentation suggested they didn't have the effects of temperature in the models when it dealt with solubility or absorption, which bothers me because obviously the data is out there for some of these reactions and should be. It's within the program, in fact. You seem to have found that data and you're suggesting that you know something about the effects of temperature on sorption and solubilities, which I think we have to know those things, obviously, to go on with this.

Where are you obtaining your data? How much confidence do you have? And a larger question, what are the major uncertainties in what you presented that suggest--what you presented was a lot very quickly. It sounded like you knew a great deal about what might happen up-T. What are the larger uncertainties that remain in your analysis among the things you think you know as assumptions?

DR. ANDREWS: Let's talk the first one first, and that is thermal impacts on some of the geochemical very near field processes. You know, LANL have, and their contractors in this case, LBL I think was working for them, have done a number of experiments of solubilities under different geochemical environments and different thermal environments.

Now, I think for a number of those tests, as I understand it, they've only come at them from the under saturation side, and so there's some--there's not question in those data, but they want to come at it from the over saturation side to see how those solubilities match or reproduce. So there are some laboratory derived data for solubility variation as a function of temperature for some of the key nuclides that we're talking about here.

We, in our TSPA, tried to incorporate that in a relatively crude way with a very simple functional relationship. That I think needs some iteration back with the actual data gatherers, with Ines and her troop, on maybe better ways of incorporating that functional dependency on temperature into the solubility data, or solubility values used in the TSPA.

On the retardation side, there are also data available on retardation as a function of temperature, again from laboratory measurements at LANL. When we--it wasn't me personally--but when our people sat down with LANL, they said well, there is a tendency to increase, for some nuclides of importance, increase the Kd, increase the retardation as a function of temperature. It's only factors of two, three, four. We don't feel comfortable in having you embedding that into your TSPA right now, so we did not embed those into the TSPA.

DR. LANGMUIR: But how sensitive are your conclusions to those uncertainties? Have you tried that?

DR. ANDREWS: On the second one, not very, because it's all the non-retarded species that become crucial. On the first one, the sensitivity to solubilities is almost, especially for some of the key ones like neptunium, is almost one to one. So one thing we did not do, we assumed that that functional dependency of temperature on solubility was fixed and well known. What we should have done, and comments we received afterwards said you should have assumed perhaps that was a very uncertain dependency and modeled it as an uncertain dependency, which of course you can very easily do, but we did not do in that particular iteration. So we assumed the dependency on temperature was a fixed dependency.

Now, your second question was what's key, I think, or something like that. There's a number of things in the package in terms of water contacting waste and actual dissolution rate and effective surface area. So surface area in contact with a liquid phase that become very key and pretty uncertain and very difficult to envision a laboratory test that helps you come out with that, with the exception of what I think Ray will talk about, and that is that the cladding only partially fails and, therefore, the cladding will only partially see water--or not the cladding, but the waste form inside the cladding will only partially see water. That's, you know, a very significant effect.

MR. APTED: Bob, there seems to be a plethora of these codes, we saw the YMIM earlier for source term, I guess, and the M&O talked about AREST in the past as sort of a reference code, and you've got a source term here I guess in RIP. To what degree have any of these been bench marked or compared? Because, in particular, I'm surprised somewhat at the emphasis on spent fuel dissolution as such a major effect of all condition and all times and all standards in terms of releases.

Just because, let's say iodine, I assume iodine right now, you're using like 2 percent fast release and then something 98 percent slow, based on the models I've seen, the most it can be is 50 times higher than that. It won't continue to--over the surface area, which you seem to imply that somehow the surface area was an important factor in driving this model to higher and higher releases of iodine. So I guess my basic question is to what degree is any sort of comparison planned among these codes and against maybe better tested codes in other places?

DR. ANDREWS: Let me try to answer that. I mean, AREST is still undergoing some development to make it more specific to a Yucca Mountain kind of configuration, but be flexible enough to handle a lot of possible designs. So it has not been tested, and as of this TSPA, the actual results from YMIM versus the waste package EBS component of RIP have not been directly compared because the assumptions that are varied in them regarding vective versus diffusive releases, for example, regarding the temperature regime in which the packages are sitting that Rally talked about this morning, are so different that it would be kind of useless to compare them.

We have, at least on the planning phase for the PPA in the next fiscal year, just are comparing, you know, kind of internally what's going on within YMIM and RIP, and AREST will come in probably after that.

DR. LANGMUIR: I think we need to go on. Our next presentation is by Raymond Stout, Waste Form Dissolution and Alteration.

MR. STOUT: Good morning. We always have the same talk, but we have changed the title here, but it's simply the waste form alternation/dissolution for release rates. And you'll notice that perhaps there's a little change in what we're talking about in terms of dissolution versus what Bob just said recently.

When we talk about dissolution, we're talking about what's coming off in some units like grams per meter square per unit time, and what he's saying is more like release rates because he brings in the area into that, and perhaps that will be clearer when I go through.

I know we're a little bit behind schedule, so some of the things I'm going to go through perhaps faster, and many of these are cartoons to give you a feel for what we're trying to do, and I'll just try to hit on the significant pieces then that go on.

What I'd like to do is tell you why we're doing this, what we're doing, and then some results and where we're putting those results in terms of PA and design of waste package.

There's two objective kind of statements in the regs, 10 CFR , part 60. One talks about substantially complete containment and the other is release rate limit. Waste form characteristics feed into both of these.

The characteristics which we're most concerned with and which we have actual activities are when--we don't have activities in the inventory evolution; this is pretty well done and has been done by Oak Ridge historically, but they factor into what we're doing. This is the alteration response and the dissolution response.

Alteration for spent fuel, we really talk about primarily the oxidation of it and its impact then on the dissolution and the release rate.

Dissolution we'll try to go into in some detail and show you the different kinds of tests, why we're doing those tests, and how they're beginning to feather together to make a picture so it would help perform its assessment and really simplify what they're doing.

The focus of this presentation will be on spent fuel alteration, dissolution testing and modeling. I have some minor remarks to make about glass. Dr. John Bates at ARGON has given a viewgraph here on some glass response. It's significant. We do not have significant dollars in that program, and we'll just primarily say that the program in spent fuel is analogous, if we had a program in glass, would be analogous in the sense the same kinds of responses are necessary.

So with that, I think we'll speed through some of these things fairly quickly. I do want you to understand, though, hopefully as best as I can articulate it, what happens in terms of release rates for aqueous release, which is a primary topic. You have to talk about the repository, how many containers are failed, and you have to worry about the waste area exposed per container, and this is the alteration part that comes in, and what happens if you expose this and spent fuel oxidizes, the area increases, it can be a factor of, say, two or three orders of magnitude.

Then this is hydrology, how much is wetted, water volume, Bob Andrews mentioned this, and finally we get to the other term that we do, is inventory dissolution rate. So it's these two which we continue to come back and talk about, and this is how much is getting into the water without worrying about solubility limit constraints or colloidal solutions. But you have to come back and do this piece, and in our dissolution testing, we'll tell you what we're doing to worry about that. So these are the two pieces that factor in.

Now, the rest of these viewgraphs are just to set the stage, and so very quickly, what we have to worry about is the waste form from its initial state to some final state at time-t, and you have to worry about what boundary conditions you put on that waste form. And if we go up this way, what's its temperature history, if it's exposed to air, and then if it's exposed to water. And if you have waste form, then that will give you hopefully the release rate history, given these histories.

If we do a test program then, we can control these and give you response functions for the waste form, which then these will feed into PA and they will integrate out to give you the total dose or total release.

The next viewgraph I think has been presented before, but it just points out that things don't come nice and neat for us. There is an ensemble of temperatures, ensemble of water, water chemistries, and some way we have to feather out and say how can we test in this ensemble and make sense out of waste form response, given that waste form itself is an ensemble.

I'm going to show this viewgraph very quickly, but just to point out the significance of the early time period and how important it is to keep it substantially completely contained. This shows the curie load for the first thousand years, and you can see there's three orders of magnitude decreased in that. And so this is one reason, of course, that we worry about getting exposed area. The other reason is its got a high heat load, and if we go to thermal designs, the temperatures here will be higher. If you expose it and you oxidize it, you will probably go to U-308 during that regime, which will increase the surface area.

You have seen this viewgraph several times, and it only says that we only have access to the yellow and green kinds of waste forms, and these are BWR, the boiling water reactor, and PWR, the pressurized water reactor. This is the inventory which is projected. We have samples, ATMs, at this range here. We have one out at Urotis (phonetic) 40, but it has some segments at 50 gigawatts per metric ton. So we have some samples in this range and I'll talk a little bit longer and then I'll skip some viewgraphs. We'd like to get a sample perhaps that is higher in burnup. We're testing essentially unirradiated UO2. This gives us zero burnup. So we have a couple or three points in our test matrix for this.

I don't have a viewgraph which shows well what we're getting, but I'm going to skip this next viewgraph which talks about our test matrix design. This viewgraph will feather into something when we talk about dissolution. We have to worry about--this viewgraph feathers into something I'd like to talk about in a little more detail in the future here. This is the water contact mode, and essentially there may be lots of water on a few packages which will be kind of a flow-through regime. It may be intermittent water and then periodic water.

The regimes we test in are a flow-through mode, an unsaturated mode, which looks something like this where you have dripping water, and then what we call a saturated mode where everything is wetted, but the water isn't moving. They call that a bathtub mode. And these regimes come out of performance assessment.

Now, the next viewgraph is very, very busy. We're only doing a little piece of it, so I am going to skip it. I will tell you and confess up that we do have problems like everybody else that does testing. That's a length scale. You've seen some of this before. We're only testing fragments and grain size pieces of fuel, which you know comes in sections which are rods that are several meters long. People are going to put tons of this, of course, in the MPC and, of course, you're going to put thousands of tons.

We believe, though, if we have the right coordinates when we test, I'm talking about variables where we talk about thermonamic kind of variables, intensive, extensive, that we can integrate up and get the response for the larger scale tests.

This viewgraph just shows you the sequence in time of what Bob addressed, in a sense that when you uncover a container, you will have gaseous release. I think many of the Board have seen this viewgraph before. Here, we have furnished an instantaneous release rate for the gas, and that shows up as just an impulse. We're not doing anything now in cladding failure response. We believe it will be a holder for the fuel. It may be defected, and this becomes important when we talk about oxidation in the sense that a defect, if you go up to higher oxidation states, particularly U-308, this has a significant volume expansion from these two, you will then split open the rod.

And finally the dissolution response. I'll give you quite a viewgraph on this, and then there's a couple viewgraphs on--not on solubility limits, I have nothing to say about that in this talk, but colloidal response and what we're seeing there. Those are preliminary, though.

This viewgraph is from Pacific Northwest Labs, PNL, this is Bob Einziger and Larry Thomas's work. And essentially I don't know whether this shows up, but there is a defect, a small hole in this rod. As you oxidize up, you go to U-409. This is a slight volume shrinkage. What happens is the grains then have grain boundaries which pop open, and this gives us an increase in surface area potentially to degrade fuel. We don't see any U-307. If you go to U-308, you get the volume increase and then the rod does split open, and there's not a significant difference between these two phases, and then you can form some hydrates.

But in forming these phases, though, you do break open--actually the fuel, you expose grain boundaries, and you break down the grains themselves. And that's where you get the significant increase in potential surface area that can be wetted.

Now, the next viewgraphs just indicate that there are two temperature regimes which are important, that below 200 degrees where you're getting the U-409. Even though it's not stoichiometric U-409, we do see this plateau. We think that this is a metastable state and may remain there if you get below the 200 degree limit. However, there is data, and we're getting more data, that if you go certainly above 250, that you will go on to U-308 and U-03. And so we're studying this regime below--well, trying to get what the response would be between 250 and 200, 190, and saying that this regime probably would be quite a few waste packages in the repository, but that will be where your temperature is and we want the kinetics of that and what the metastable state is.

The next picture just is to give you an idea that we're trying to get the time response for all those spent fuels. If we switch into dissolution and release, here this goes into the three types of tests that we have. We are only doing what we call the flow-through dissolution test, and then John Bates is doing the unsaturated test.

I said a series of tests on saturated dissolution was done in the late Eighties by Chuck Wilson. That data is available. And this cartoon essentially shows waste form in a container. All three of these tests have this same mass balance equation. What we did when we started out, we picked the one which we could solve the equation easiest, give us the--it's the best test in terms of going through the inventory, different temperature conditions, different water chemistries. But it says the rate of concentration change in the water, and here we're talking about the ionic form in the water, is equal to what comes off of the spent fuel. That's what's dissolving off that's going into the water, and then you have to subtract out, if you want this ionic form in the water, the rate that you precipitate out of the water, and then the rate the radionuclides and other things are going to colloids. So this would be what people would measure at and below solubility limits. That's what the water will hold in its ionic state.

If you're doing flow-through tests, some of these are fairly straightforward. I hope I don't bore anybody, but this test is fairly quick, it's cheap, you can do a variety of fuels, you could do a variety of water chemistries. You control bulk properties coming in, measure bulk properties coming out, and from that you infer a dissolution rate, because you've controlled the flow rate so that you have no solubility problems, you see no colloids, and so then you get this dissolution rate coming off.

Now, this is the kind of information we feed into performance assessment. There is always a question if you have these other kinds of tests are there different mechanisms going on and do you have to worry about that. So in this case, this is Chuck Wilson's test where he has this closed. He has stagnant water. He did see some precipitates, he did see some colloids. They were not a large amount. But from the soluble species like cesium, strontium, then you can infer a dissolution rate.

We have looked at that, and they are in the same ballpark as our flow-through numbers. Chuck's test was done in J-13 water. We're using a much more aggressive water, but still the hard thing to do for these is this is a dissolution rate per unit surface area, and the surface area then tends to cause us some problems in the sense that it evolves as you are dissolving because grain boundaries are opening up.

The last test which John Bates started up is the unsaturated test. It's also called the drip test. Here you have very small amounts of water. I keep forgetting, but I believe this is one drop every three and a half days. This is the slow, and then he has a factor of ten on that. But here you have film flow over the waste form. You do see colloids, you do see precipitates, and what we have to do here to infer a dissolution rate, what's coming off the surface, we know the surface, we know the time span, and so we put everything that he gets back into solution, either--measuring the amounts of these things, and then we can put it back in and infer a dissolution rate.

Now, that dissolution rate is also in the ballpark of our flow-through test. The important thing, though, this test has such little water that the water chemistry will evolve and it will be nothing like what you started with. And so, in fact, this water is depleted of silicates and some of the other ions that started out in J-13, and it's gratifying, I guess, that we're not seeing anything that's surprising because here you have such significant concentrations that things could be quite different from our flow-through testing.

This gives us some confidence then, and this is a sum-up or rap sheet on our testing methods. This is a test we use, it's cheaper, it's quicker to scan through the waste forms. These tests were done. We've checked back. We're getting dissolution rates which are somewhat in the ballpark between these two. John's rates, some are higher, some are lower. There's not a lot of data there yet, so they're preliminary. He sees some speciation. He sees colloids, a lot of these, whereas, Chuck didn't see so much. You might expect it because there's much less water.

The tests that Bob mentioned we'd like to do in start up is where we have a defected clad. Here you're not going to expose all the area. Here we can take this kind of data, do some predictions and infer then the area effect that is only for the exposed fuel.

The kind of data which we have been giving PA, I'll just show this and show you that people have often said well, there is the effect of radiolysis. We believe this is an effect of radiolysis where we have a coupling between temperature and oxygen, whereas, in the unirradiated UO-2 we did not see that. If we go back and say let's look at the data where we only have oxygen fugacity of 20 percent or atmospheric in the liquid, then we get rid of some of the oxygen. This is oxygen fugacity on dissolution rate. And we'll see that the spent fuel and the UO-2 look very similar.

This is good news because for Yucca Mountain, of course we expect the water to be fully saturated. The reason we looked at oxygen, people said that that will help us establish a chemical mechanism, a model. It told us some things, but because the radiolysis on spent fuel, you don't get a good clean model.

What this data says, though, is that if you look at these two and you took the average of all the 20-some sets of data, UO-2 with about 9 milligrams per meter squared per day, the spent fuel was about 3 or 4 milligrams per meter squared per day, they both varied the same way in terms of the parameters of temperature and Ph.

The colloids, and this comes from John Bates primarily, with some report of Chuck Wilson's down at the bottom, John had very little water. He saw lots of curies per ml. When he goes to filter solutions, he gets this. The colloidal content then is significant for these. We do not know any characteristics for spent fuel in terms of their stability, their Ph. response, we hope to get that in terms of what the area or size classes. This is planned work.

This is Chuck Wilson's, what he got in his higher volume of water versus surface area saturated test. And this was filtered at this 400 nanometer, but his total solutions, he did not see significant colloids. He had to go to very, very small filters to actually find colloids. And you might expect that, but I certainly don't have any appeal.

The glass work, which I had mentioned, this is not glass work which is currently supported by our program. This is EM supported work that John has published and has furnished, that there are colloids forming here. These are often silicate related colloids in the sense that the glass forms silicate polymers, and you can see that again there's significant contribution to the curies just from the particulate or colloidal response. He has given us some different examples of glass aged with vapor and then water contacted. Sometimes surface does fall off, but he sees then that these colloids are iron rich clays, silicates, some calcium actinite, phosphate--these are probably minerals--uranium silicates.

And here, this is glass aged contact, water dripping, so these are two different modes of testing. This would correspond to saturated; this is unsaturated. And after several years, he sees these two kind of blend together. So that says that the release rate for PA people may not be--they may not have to do so many different scenarios.

And, finally, this one is fresh glass. He has high surface area and it's not congruent release with the technetium and neptunium. As I remember, he said it comes out about the same rate as the boron, so he's altering the glass here and releasing these two species non-congruently.

I think in terms of summary, I have one more viewgraph, but just to go back to what performance assessment needs for waste package, they need this release response. This is container materials. Waste form provides them information on this for both spent fuel and glass, glass we call alteration, and of course to provide this information. There are other pieces, the rapid release, which was referred to, and the rest of this probably is for another talk.

In summary, we have provided preliminary models. Some of these for the alteration response have been what they call in their document expert witness. We're still getting data to substantiate those. We're worried about the oxidation response. This can certainly significantly alter your release rate response that goes in U-308, and we're studying dissolution over the inventory oxidation phases of spent fuel and the water contact modes trying to bring these together.

The thing that is beginning to show up and will have to require some additional characterization work is the colloidal characterization.

Thank you very much.

DR. LANGMUIR: Thank you, Ray. You've exactly filled the 25 minutes. Can we please hold questions until the discussion at the end of the day, and we'll go on to the next presentation.

Originally, this talk was to be given by Bo Bodvarsson. He's had serious personal problems and wasn't able to do it. Ed Kwicklis, on quite short order, I think like six hours or something, has put this together for us, and we wish him well and look forward to the presentation. The topic is Moisture and Gas Flow in the Unsaturated Zone at Yucca Mountain.

MR. KWICKLIS: Well, given that Bo is so characteristically terse in his overhead, it left me with a lot of freedom to interpret around the few words that Bo has on his viewgraphs and, therefore, you're going to get a little of me and a little of Bo in this presentation. Bo, just as Don Langmuir suggested, I just got these viewgraphs late yesterday afternoon and so have had little time to interpret them.

The outline of the talk that we're going to follow today is we'll talk about the parameters of the work for the site model, the current level of understanding, the major unknown parameters, some supporting isotope data, and then we'll plunge into the LBL/USGS site model, and I'll try and tell you what we know from those model results, as well as from some of the other data about flow paths within Yucca Mountain so that you may be aware of the bearing that might have on some of the transport studies that are being discussed here this morning.

The LBL/USGS model is going to--we'll present a couple of cases for assumed flux distribution, for uniform flux, non-uniform flux. We'll talk a little bit about gas flow patterns and what impact they might have, and important future tests and model plans.

Among the important moisture flow parameters, we're interested in the infiltration patterns at Yucca Mountain because these patterns describe how water enters the mountain and, therefore, what the point of origination is for the water moving through Yucca Mountain.

We also need to know what the rock and fracture properties are because these define the pathways or potential pathways for flow through Yucca Mountain, and knowledge of these properties describes to us how flow might be concentrated within the mountain.

We need to know the characteristics of major faults. These are the larger scale identifiable heterogeneities in the mountain, and the simulations that have been done to date have indicated that what is assumed for the faults in the numerical models exerts a profound influence on the flow patterns throughout the entire mountain, as well as the faults themselves. And it's recently been proposed that the gross distribution of moisture flow patterns throughout the mountain can be used to infer the properties for the faults themselves, in that these moisture patterns are diagnostic of certain--allow you to infer certain types of behavior for the faults.

And, finally, another bit of information for the site model are the capillary pressures, temperatures and saturations, and these define the gradients for moisture and gas movement and possibly vapor movement and give us some kind of gross indication of the patterns of flow throughout the mountain.

The gas flow parameters are similar, in that there's a lot of complex surface boundary conditions that we'd like to know. It's become clear that things like wind effects, barometric pressure fluctuations, as well as topographic effects significantly effect gas movement under ambient conditions.

And we're interested in gas flow for a number of reasons; one is that we're concerned to some extent with the transport of gaseous radionuclides, but also it's been argued pretty convincingly that following waste emplacement, should Yucca Mountain be accepted as a repository, that there's going to be a massive redistribution of moisture and, therefore, knowledge of gas phase pathways throughout the mountain tells us where we might expect moisture to accumulate following waste emplacement, and that these pathways are important not only under the pre-emplacement, but also post-emplacement conditions.

Also, because the gas is going to move primarily through the fracture network, we're interested in the fracture network continuity, particularly across the non-welded tops that overlie the proposed potential repository horizon.

Again, we're interested in the characteristics of major faults and also in state variables such as air pressure, capillary pressure, temperature and saturation, which control the vapor pressure gradients and direction of overall vapor movement.

So what's our current level of understanding? We've made a lot of progress in the last year in better understanding the geologic framework of the USGS. In particular, Rick Spangler has made a lot of progress in identifying the layer thicknesses, the locations of faults and the fault--the stratographic offsets across faults at Yucca Mountain. And because the geologic framework essentially defines the distribution of hydrologic parameters in the mountain, this is very important progress that's been made.

There's been some preliminary maps of infiltration patterns proposed based on the types of rock or outcropping in the near surface, and I'll show you such a map shortly. But while these infiltration maps are very useful, they should be viewed as extremely preliminary, in that they ignore the contributions that fractures may make to infiltration.

Rock matrix parameters, the current level of understanding is highly variable, depending upon the parameter in question. Certain things like saturated connectivity, porosity and bulk density we have accumulated a large amount of data. We have moisture retention data that was developed under conditions of drying, and statistical correlations have been derived between these parameters that allow us to extrapolate to areas where only partial data is available.

On the downside, we still lack a lot of important understanding about rock matrix parameters, including hysteretic effects, the maximum achievable field saturation and relative permeabilities of the rock matrix. And because one of the things we're trying to do is estimate what the flux of the mountain is based on matches to the saturation and moisture content and water potential profiles, our uncertainty in our estimates of matrix relative permeabilities casts significant uncertainty on our estimates of flux.

Fracture permeabilities, a lot of progress has been made this year, UZ-16, which was the first test in the unsaturated zone. For the proposed repository horizon, we've got estimates of permeability on the order of a darcy, plus or minus an order of magnitude, and these air permeabilities were remarkably uniform over many hundreds of feet within the proposed repository horizon, suggesting that the fracture network there was well connected and that permeability wouldn't be a limiting factor in restricting access to water.

Unfortunately, there were borehole--in the borehole, it was too rough to test, and a few very important horizons, namely the Paintbrush Tuft and the basal vitrophere, and very few measurements were made in those intervals.

State variable data, we've made a lot of progress. We have approximately a dozen holes now where saturation and water potential data have been measured on core retrieved from those holes. Unfortunately, most of those holes, with the exception of UZ-16, are extremely shallow and penetrate only into the upper part of the Topopah Spring. We also have moisture content data collected from 91 neutron holes that Alan Flint has been measuring.

Perched water zones, we've discovered a lot about perched water this year. It's been basically impossible to drill a hole in part of Yucca Mountain without encountering perched water. Perched water was encountered at or below the basal vitrophere, in UZ-14, NRG-7 and ST-9, and there are a few competing conceptual models of why that perched water is there. The simplest and most straightforward is that water perches when the infiltration rate exceeds the permeability of the most restrictive layer in the sequence of layers, and that we know from precipitation maps of Yucca Mountain that precipitation is highest in the northern part of Yucca Mountain, and the simplest explanation is, therefore, that the infiltration rate is exceeding the permeability of the most restrictive layer.

There also have been more exotic conceptual models proposed, such as the perched water being related to the steep hydrolic gradient in the north, and as the water table plunges just to the north of Pure Hole Wash, there is some diversion of that water along some low permeability units, and that water is what we're encountering.

Also, isotope values at a few locations, we know from elevated--chlorine 36 which--or elevated tritium measurements in at least five holes that I know of, that there is rapid fracture flow in near surface fractures, and that it's most likely water that's traveled through those slow paths that we encounter when we test water taken from cores in the non-welded Paint Brush Tuft. And I'm also going to describe to you some recent data that Al Yang and June Martin have provided us.

This figure just summarizes some representative values of what we know about matrix connectivity fracture, hydrolic connectivity and porosity for some of the layers in the model. The green is the Tiva Canyon, the purple is the Paint Brush non-welded unit, the Topopah Spring unit and the Calico Hills unit.

Okay, major unknown parameters I've talked about. Hydrolic--I've talked about this already. This show a schematic example of the different kinds of things that may occur on a fault face. Hopefully, not all of these things occur in any given fault. The fault can behave as a capillary barrier if the fault separation is relatively wide and, therefore, the fracture has very little capillary attraction for water. It can flow, if the fracture is tighter, capillary processes control the distribution of moisture and the flow within the fracture. It may just be sheet flow if the aperture is so wide that it behaves like water running down a wall.

So the most complex model of fault properties would then have several elements of these. The simplest model would--a fault would have one, but probably not all of these. It's our hope that the simplest model would prevail and that fractures of a given tectonics style might exhibit similar hydrologic behavior. By that, I mean to say that normal faults at Yucca Mountain exhibit a certain type of behavior, a similar behavior between faults, and that behavior is identifiable and distinct from that behavior of a slip fault such as those that occur in the north part of Yucca Mountain.

So although we're not going to test all the faults there, we hope that by testing a few, that we can infer the hydrologic behavior of similar faults of a similar tectonic style at Yucca Mountain.

Some of the isotope data, this has been very valuable in constraining us in our estimates of flux and in allowing us to infer flow mechanisms and flow paths through Yucca Mountain. Among the isotope data that has been particularly useful are the chlorine 36 data that Los Alamos has been collecting, and I allude to some of the inferences that were made from that data earlier. There's C-14 data collected from a variety of locations. The thing I'm most familiar with is C-14 data from Pagany Wash, which is in the northern part of Yucca Mountain, that's allowed us to infer long-term water fluxes on the order of several tens of millimeters per year over the last several thousand years.

The tritium data has been very useful in identifying fast flow paths. And a new line of evidence that has emerged are the calcite coatings, and we have long argued that it's likely that fracture flow would tend to--that within the Paint Brush non-welded unit, flow would tend to be matrix dominated. And very preliminary evidence concerning the absence of calcite veins in fractures in that unit suggest that even under wetter, past wetter climate, that flow was matrix dominated within that unit, and while a lot of additional data needs to be collected, this is kind of one line of evidence, the absence of fracture coatings in that unit may allow us to infer a dominant matrix flow in the past.

So unfortunately the chlorine 36 data isn't always that easy to interpret. At UZ-16, Los Alamos provided several chlorine 36 age dates of on the order of several hundreds of thousands of years, and sandwiched between those are a C-14 age date of about 5,000 years. And I'm certainly not going to be the one to reconcile the differences between the age dates. It is possible, based on simulations we've done elsewhere, that as water moves down some preferential flow path, it hits a low permeability unit and flows laterally, and this is a mechanism for getting these kind of age inversions with local spikes in the C-14 activities.

The UZ-16 is at the intersection of two major fault systems, the Imbricate Fault system and the recently described Sun Dance Fault system, and so relatively recent water movement down a fault system is not out of the question here.

At NRG-7, which is again one of the north geologic holes being drilled to investigate ahead of the tunnel boring machine, there has been age data of between 1,000 and 5,000 years, which is uncorrected for dead carbon, which would only make it younger, though. At the contact between the basal vitrophere and the Topopah Spring and the Calico Hills, there's been some relatively young water discovered, and I'm going to use that isotope data as a means of constraining the flow models later.

I'll briefly describe to you now some of the model calculations. Those of you who have seen Bo's presentations have probably seen this before. This is a surface expression of a computational grid. The cells were positioned to take advantage of existing and proposed boreholes and to coincide with the surface traces of major faults. This is the Abandoned Wash Fault, the June Wash Fault and the Ghost Dance Fault, and these are explicitly accounted for in the model and their offsets across these normal faults are accounted for by prescribing a certain set of properties for these notes that are distinct from the surrounding rock.

There's also three strike slip faults explicitly accounted for in the model. This is the Teacup Wash Fault, the Pagany Wash Fault and the Sever Wash Fault, and these are strike slip faults and at present, they're not explicitly accounted for in the numerical model.

So we're going to first look at moisture flow, the predicted moisture patterns for uniform flux at the ground surface. First, I want to say that some of the early work focused on the effect of what different assumptions for the fault properties would have on the flow system, and what we found is that there are kind of diagnostic flow patterns based on the properties assumed for the fault, which lead us to have these cartoons in our mind that say if we go out in the field and we observe these certain types of moisture patterns, we may be able to infer through a combination of those observations and direct testing what the fault properties may be.

In this particular instance, the fault had a high-- there a uniform percolation flux of .1 millimeter per year at the ground surface. The faults had a permeability of 10 to the minus eleventh meter squared, and what we observed in this simulation was that very little water moved down the fault, as indicated by its relative dryness. Here the red is dry and the blue is wet. And what we also saw was there was some modest accumulation of moisture in the updip side of the fault as a result of downdip flow, as well as some modest drying beneath Yucca crest due to lateral moisture movement in the overlying units.

When these same fault properties were changed to reflect the possibility that the fault may consist of very broken up rock that essentially functions as a porous media, with moderately high permeability and moderate capillary attraction for water, we observed that the lateral flow in the overlying units is greatly enhanced, that there was a lot dryer adjacent to the fault, and that the drying over here was very pronounced relative to what's in this figure.

What we're interested in is looking at how moisture might be focused for uniform infiltration as it encounters successive layers through the mountain. And this shows the normalized moisture flow, normalized relative to a uniform .1 millimeter per year flux at the base of the--at the top of the Paint Brush Tuft. And what you can see, the green indicates about 100 percent of the prescribed--of the .1 millimeter per year flux. What you see is there's very little lateral moisture movement in through the overlying Tiva Canyon member, and that flow through the Tiva Canyon is mostly vertical.

At the top of the Topopah Spring, however, you see that moisture has been fairly dramatically redistributed, and the blue indicates then above the average of .1 millimeter per year, and the red indicates relative dryness. And what we see is that updip from those normal faults that are explicitly accounted for in the model, we see an accumulation of moisture. Downdip from those same faults, we see a drying, and this enables us--again, guides us in the types of things we might look for in the field to infer a certain type of fault behavior.

This is at the base of the Topopah Spring. It hasn't changed much in the Topopah Spring unit, again indicating that flow is mostly vertical through the Topopah Spring. And this is at what you would see in terms of flux distribution at the moisture table--at the water table.

So if you accept our hypothesis about the behavior of faults and our assumed spatial distribution of moisture, a plot such as this tells you where you might want to concentrate your efforts in characterizing the transport properties of the rock. What the models are presently saying, and there's a lot of uncertainty, is that either updip or within the fault, the normal faults at the southern part of the mountain would be the places where flow would be most concentrated given a uniform flow pattern at the ground surface.

This is distribution of travel times based on the same rock properties, and this includes UZ-16. The travel times here are in thousands of years, so the dark blue is like 4 million years, and this is like 66,000 years. So depending on whether you want to believe the chlorine 36 data or the carbon-14 data, the model did very well with .1 millimeter per year flux. It predicted ages of 2 million years versus June's 500,000 years. But when you want to compare it to Al Yang's 5,000 years, then something is obviously wrong with the model.

Let's look at the non-uniform moisture flux. This is--

DR. LANGMUIR: Ed, two more minutes.

MR. KWICKLIS: Okay. Based on a pattern of infiltration that Alan Flint has presented, and this is based on the relative permeability of the outcropping formations at a depth below where he has seen transient moisture changes over the last five or ten years, and I say it should be viewed as preliminary and somewhat simplistic and it assumes that the volumetric contributions of fractures in the welded units are minimal or negligible and that we are only going to concern ourselves at this stage with the matrix properties. So it says that most of the recharge is occurring in the washes where the non-welded units outcrop in the northern part of the mountain.

This is then the distribution of--there is an average moisture of 1.2 millimeters per year over the whole site. Most of it's concentrated in the washes in the northern part of Yucca Mountain.

When we look at--this is at the top of the Paint Brush. This is at the top of the Topopah Spring after it's passed through the Paint Brush non-welded unit. This is at the bottom of the Topopah Spring, and this is at the water table. And what you notice is that there are still--the distribution of flux at the water table still reflects the pattern of infiltration at the ground surface. And what this model says is that you've given this very concentrated flux in the northern part of Yucca Mountain based on the assumed infiltration patterns, that these are the likely zones where one would expect high fluxes at the water table, again suggesting that if you're going to use these models as a guide to where you would concentrate your efforts to characterize the transport properties, these might be the areas that you would pick beneath the washes in the northern part of Yucca Mountain and beneath another wash in the southern part of Yucca Mountain.

In these simulations, the results were fairly insensitive to what was assumed for the faults because the infiltration was concentrated at quite a distance from those faults that were explicitly representative in the model.

So you see that we have considerable uncertainty about fault properties at the moment and about infiltration patterns, but we have some ideas of where one might look to focus one's efforts in characterizing the transport properties based on these simulations. And as our knowledge accrues as to what the fault properties are and what the distribution of infiltration at Yucca Mountain is, we can certainly hope to remove some of the uncertainty in our predictions of where the most moisture is going to be entering the water table.

This is a distribution of ground water travel times for that assumed distribution of infiltration. There were several areas where infiltration from the ground surface to the water table was under the 10,000 year limit, and those were also coincident with those areas beneath the washes in the northern part of Yucca Mountain.

The last thing I'll show you before our conclusion slide is these are travel times that Bo calculated by adding a little bit of carbon dioxide to the infiltrating water. And it showed that at the Topopah Spring, the Topopah

Spring/Calico Hills contact, there was a ground water travel time of about 6,000 years. And when compared with Al Yang's estimate of 5,000 years at that contact in NRG-7, it provided a pretty good estimate, and this supports the argument that fluxes in the northern part of Yucca Mountain are quite high, possibly on the order of several tens of millimeters per year, and that high fluxes in our concentrated flow mechanism were necessary to reproduce the travel times in the northern part of Yucca Mountain.

So I'd just say that one of the things that we're also looking into is the possibility that vapor moving up faults may pre-condition the fault to accept more moisture than we're currently estimating, because as water moves up--moves down the thermal gradient, it would tend to condense in the upper part of Yucca Mountain, and possibly create wetter conditions that may facilitate the movement of water that may enter through the ground surface. So amongst the issues in looking at gas flow, we're interested in the redistribution of moisture as vapor.

So I think I've used up all of my time.

DR. LANGMUIR: Yes, and a couple more. But thanks, Ed, very much. Thank you and Bo.

I think we're going to have to go on, and our next presentation is Unsaturated Zone Radionuclide Transport

Modeling. The speaker is George Zyvoloski.

MR. ZYVOLOSKI: My talk is an appropriate follow-on to Ed's, and it concerns itself with when we know the flow of both liquid and gaseous material, how does the transport go after that.

I am definitely an integrator of various studies at Los Alamos and other places. Where I fit in is the migration part, and as you can see, other studies feed in either indirectly or directly to this study.

I would like you to keep in mind during this talk that my results are sensitive to a number of different things; first is flow. My results are only valid to the extent that the quality of the flow model is valid. And that's why I have to be in very good contact with the USGS, LBL, Ed Kwicklis, Bo Bodvarsson and so on.

I would add a couple of other things. Sorption is equally important. You can change the permeability by an order of ten and it's the same as changing the sorption by an order of ten in a particular unit.

I have additional constraints, even though the flow model is very sensitive of course to the kinds of discretization, the kind of largeness of blocks, and so on, it's even more important in the transport model because I have additional numerical problems with the advection solution, especially when we're talking three dimensional models and we're talking on the order of 50,000, 100,000 and ultimately about a million nodes.

Outline of the talk; technical challenges, give some of our solution approaches, give a little application, concerns, and then some future developments so you know where I'm headed.

Technical challenges are obvious. Ed mentioned a few of them, and I'll just keep this in front of me. Very long time frame, enough said about that. Complex geologic setting; we have to be able to capture that geologic setting in a very accurate way, and I would propose an additional constraint that we have to capture it on a crude model, one that we play around with, has a fewer number of nodes, up to a fine model, and we have to make sure that the integrity of those models going from coarse to fine is maintained.

Complex flow and transport mechanisms; fracture flow, obvious. Transport mechanism is we have competition between diffusion in the matrix versus flow in the fractures, some that I will also address later.

Complex geologic setting, once again addressed before me, lots of units. Topography certainly affects infiltration and gas flow, as Ed Kwicklis mentioned.

I'd like to use this slide just to enforce where we get our data from. The stratigraphy and topography data, which I will be using as a basis for my calculations, comes from Sandia and also the USGS and LBL. Other sources of data which were alluded to in that connection slide which I showed second in this list are intrinsic permeabilities we have to get from other folks, relative permeability, fracture volumes and spacings are very important and I'm going to show you some double permeability results, and also faulting.

What all this says, and what we are working on at Los Alamos, is an integration of things, and for that I would make a--communication, the ability to derive this data from other sources in a straightforward and easy manner is very important. I talked to Cady Johnson about the database management, and once we get these databases going and we're able to access this data, we're able to then go through the steps in a very uniform manner, and that kind of communication I think is needed at this project. It's not that we're doing badly. Everybody is groping with this, too. Our connections with the oil are trying to do this exact same thing. I think they use these names, virtual corporations now where people doing geology are in one city, people doing reservoir engineering in other cities, and that communication has to be fairly transparent.

Complex flow and transport mechanisms. This is like preaching to the choir because a lot of these have been addressed in previous comments of this Board. Of course, air, water vapor, water and heat are important. Fracture flow is important. And I would stress dry unsaturated media. This is a tough numerical problem, but it's also tough conceptually for physics. I'm not sure we know the processes involved in that area. Complicated sorption and diffusion and, of course, coupled flow and geochemistry.

The first few topics have been addressed in the flow talk ahead of me, so have the retardation aspects. We obtain our data from Los Alamos studies, as I talked about. Validation studies, we have to address those. Fracture data, and the fracture data I'm talking about here is the sorption characteristics. And, of course, difficult can be important. I will show you an example of the effect of the diffusion. But once again, the studies of the diffusion coefficients and getting those results to my study is important.

We have created a model that is based on the needs that have been described previously. All of these aspects have been designed to meet a problem and hopefully to solve it. Of course we don't have to talk about fully coupled and implicit numerics, the same as some other codes used. What really are important is grid generation.

I mentioned the integrity of coarse versus fine models. The models that Lawrence Berkeley and the USGS, as well as us, are able to take data and have the flexibility of the grids to represent that data from a coarse model going to a fine model, we have to be able to generate, however, those grids effortlessly from the database of the stratigraphy and be able to incorporate other databases, for instance, the Min/Pet database, apply sorption parameters to that. We have to be able to incorporate that, and I can't, I guess, stress that enough.

Finite element, finite volume numerics solve a number of problems especially in transport, allows us to take the best of both worlds in terms of the grid generation of finite elements and the stability of finite differences internal to the code, non-isothermal multiphase code, just to address the problem of the fluid physics and thermal physics that goes along with the near field.

Dual porosity/dual permeability is an important aspect, and I'll show you some results that show that the travel, the radionuclide release is much different between an equivalent continuum and a dual permeability model. Comprehensive transport and geochemistry model; lots of things affect the sorption, and in the near field, the geochemistry can affect the flow.

I'd like to just briefly go through some applications. Some of you, perhaps most of you who were at Los Alamos last October saw a presentation by myself where I gave some of the 3-D results of our integrated model. We used the data from the Sandia data set and we did a full 3-D simulation of air and liquid flow. I'm not going to show that this time. I'm going to show some studies, smaller studies that we're doing to look at effects, certain small parameters that affect the major solution and so on. And so I'm going to talk about grids, I'm going to talk about a number of transport studies. I'm not going to talk about the 3-D flow and transport model. Hopefully that was covered last October.

I would like to talk about grids. And once again, it bears a lot of relation to the whole idea of databases. In 1988, '89 time frame, Los Alamos did a 3-D flow and transport simulation of Yucca Mountain. We used a finite difference code, tracer 3-D, and it has a structured grid shown above, basically finite difference, finite element technology above and below. It has a certain bit of problem when you look at a database, and now we're talking not just of 2-D, probably you can go in and look at how the units are continuous and how they should look. We're talking about a manipulation of at least 100,000 nodes. We're talking up into the 100,000 to a million nodes. You just can't go ahead and find the so-called--no pun intended--but real holes in the data.

And as you go to a finer model in these kinds of grids, you'll see that while it looks continuous, that if you use a five point finite difference stencil--well, I seem to have lost the little clip, but never mind, I've got two hands. Okay, if you look at even the fine grid, the finite difference solution, you notice that if there is significant lateral flow in this bright red unit, that using a standard five point finite difference stencil, at this point, it would have to go out of the red unit into a different unit and back into the red unit.

Even in a situation that looks fairly continuous, you would get this continuous flow, and if the properties are much different, you would miss a lot of information.

I'd like to use this slide to illustrate some of the work, and this is basically cutting edge grid generation here. You notice that it doesn't stay directed horizontal versus vertical. That's for a particular reason. First of all, you notice going from coarse grids to fine grids, the units remain continuous. The second thing I'd like you to glean from this viewgraph is that there are a lot of processes going on here in terms of decision making while the grid is being generated. That is to maintain a number of compatibility conditions called Delanauy conditions, so we generate something called Delanauy mesh or in other terms, it's related to something called a Voronoi mesh. And this allows us to get particularly good solutions to the wave like portions of our solution, both the transport and the two-phase solution with the saturation variable.

Okay, I'd like to give three examples in our transport studies. The first is chlorine 36, which is a residence time indicator, and like Ed, we worked with June Fabricka Martin. We had a couple of graduate students working with this as well. And then I'd like to talk about some neptunium calculations, and then some dissolution/precipitation with repository heat.

This year, we will be doing the unstructured grid simulation at--on the site scale, and for right now, we are generating these grids from the database, but we are generating structured grids. You can see the blockiness of the nature--the blockiness of the grid. This is the cross-section near Antler Ridge, and this is what comes out of the Sandia database.

Have a quick look at the chlorine 36, what I call the low infiltration case. This is an anomaly close to the point where I think it's actually .03 millimeters per year, and you'll notice that--first let's look at the low saturation case. You see the fluxes, even though you have a dipping grid, look fairly 1-D. And the interesting thing, and once again we go from older to younger, bottom to top, if you look at the high infiltration rate--and we've put--once again, it's a uniformed low, and now it's a uniformed high, and we do this to look at the sensitivity of that parameter, and it would be--it's important for us eventually to get the real variable infiltration, what the USGS uses.

Anyway, going down now from top to bottom, you will see patches of different age in the chlorine 36 concentration. This takes into account the half life of chlorine as well, and you'll see patches of young, old, young, similar to what has occurred in the field data.

Using that same grid and the same data on flow, we have investigated the double porosity behavior of neptunium along with the continuum behavior of neptunium. In this case, we have in the Calico Hills, we have a Kd of four. Elsewhere, we have a Kd of about .5. The half life of neptunium is of course around 200 million years. And in this case, we look at a repository that is slanted and it has a top, a middle and a bottom, and we have with our low infiltration rate, and this is 0.3, we know that the time for 1 percent of neptunium to reach the water table is on the order, for the bottom one, 2.3 million years, middle is 6.4

and the top for this particular low infiltration case, it never reaches the water table. If we have a uniform release for the entire repository, it's 4.8.

Now, if we go to a higher infiltration rate, we will see that the amount of time needed is significantly less, going from about .25 to 2, going from the low end to the high end of the repository. That in itself is not as interesting as running that same model with a dual porosity. That was with a continuum, equivalent continuum model.

Using a dual porosity model, where now we have the same parameters as used in the continuum model, and the code has separated the properties and allowed the fractures to behave essentially independently of the matrix material, but allowing fracture to fracture, fracture to matrix, matrix to matrix flow and so on, in this case we have a fracture spacing, and it says 10 meters. That's input to the code. That's the average distance that a fracture sees a matrix, so it really corresponds to a fracture spacing of about 20 meters. And we have an infiltration rate on the high side, and once again, this is a typo, this should be .225 millimeters per year. The Kd for the fracture is zero. The Kd for the matrix material remains as before, and in this case, the first one, we have no diffusion from the fracture to the matrix. We see that we get a very fast travel time, on the order of 25,000 years.

If we look at Case 2, however, and we have diffusion from the fractures to the matrix, we get on the order of ten to the minus eleven meters squared per second, we get a transport time of about ten times longer. And, of course, we can conclude from this that transport time increases significantly if we can take into account diffusion. And this brings back the importance of those studies at Los Alamos and integrating those results.

I'd like to, for the last example, I'd like to show some dissolution/precipitation with repository heat. This is some preliminary calculations, and it gives me a little bit of time and reminds me to tell you that I think this is a very fruitful collaboration with Bill Glassley of Livermore. Ultimately, we hope to explore reactions with EQ3/6 and download a small set, 5 to 10, say, to FEHM, which then solves those small sets coupled with the flow in an implicit manner. And then we simulate coupled--with simulated coupled flow and geochemistry.

And so to assess a problem, this is a very hypothetical problem, and the boundary conditions here are 150. What we did is we cooked the--some Topopah Spring at 150 degrees C., and the temperature distribution at 10,000 years is given by this viewgraph. If we look at a chemical constituent that has behavior similar to silica, and the scales here, the middle of the graph is essentially the middle of these bars, is essentially ambient distribution of a solid reactant like silica, and with temperature, we have a couple of competing things happen.

The first is is that we have an increasing solubility with temperature that would tend to make the constituent less concentrated near the repository. But competing with that is the boiling off of material. So while in one sense the solubility wants this material to be more soluble and take away things in the hottest part, we have the competing reaction of we're boiling away liquid. So we're actually beating that solubility limit by concentrating the constituent by boiling away water. And so what you have is you have an increased amount of material being deposited near the repository and a less amount of material being removed at the saturation halo on the outskirts of the heated region.

MR. LANGMUIR: George, you have one minute. Question; are you talking just about the silica reactions being moved from EQ3/6 into your model?

MR. ZYVOLOSKI: I'm talking about--

MR. LANGMUIR: Just silica reactions?

MR. ZYVOLOSKI: Yes, this is a very simple reaction on the solid and the constituent in the liquid as well. So it's just that very simple reaction.

And I guess what I wanted to show here quickly is that you have a different--and this is a crude coupling of that material that has been removed affecting the porosity and, therefore, the permeability of the flow. If you look closely at the top and bottom viewgraphs, you'll see a distribution of reacting solid at 10,000 years for both. But you'll see one with strong chemical effects on permeability and you can see a darker amount at the outskirts of the--you can see a different distribution of the material with it affecting flow as well.

And I will skip over what--some concerns and let's just look at the future work. Future work; what I believe is that we have to of course incorporate the results that are coming out of all of these studies into the retardation sensitivity, and that will require first of all a lot of fine grid computations. That will, in turn, force us to go to parallel computation. We're going to be working on GUI interfaces because there's a number of people using these codes now, and then stratigraphic interfaces and faults require special consideration, and we're working on that right now. And, of course, particle tracking is one way that we can assess the numerical accuracy of our continuum advection scheme.

And with that, I'll just throw up the summary. And the summary is, first of all, transport, nothing surprising here. Complex flow requires 3-D models and need to incorporate Min/Pet studies. I didn't get a chance to talk about that too much. It bears relation to my comments on the databases.

Coupled flow and geochemistry is important near the repository, and what I mean by coupled flow, I really do mean that the geochemistry can affect the flow, and it is technically feasible. And chlorine 36 is useful in residence time. It can indicate fast paths. And in neptunium, significant retardation of neptunium in the Calico Hills is important, and if significant fracture flow exists, then matrix diffusion or fracture sorption will be required to increase travel times.

And with that, I will thank you, and I'm sorry for taking so long.

DR. LANGMUIR: Thank you, George.

I think we need to put off the questioning and I would want to remind all speakers to please be here for the round table discussion this afternoon so that we can get at you again, and the audience will also have an opportunity to ask you questions at that time.

I'm going to try and make up some of the time at our break here by having us reconvene ten minutes earlier than the schedule suggests, at 1:40. I'm told that there's a deli buffet set up both in the cafe and the restaurant for fast service here in the hotel.

Before we leave, though, John Cantlon would like to make a statement.

DR. CANTLON: Several speakers had asked where the Board member, Warner North, was. Warner North, as you recall, is a member of Decision Focus, a firm which has now undertaken a very large grant from a contractor from DOE, which puts him in a conflict of interest and, therefore, he has resigned from the Board.

DR. LANGMUIR: Thank you. See you at 1:40.

(Whereupon, the luncheon recess was taken.)

AFTERNOON SESSION

DR. LANGMUIR: Please take your seats so we can start the session.

Our first presentation, starting the afternoon session, will be status and priorities, solubility/speciation of long-lived radionuclides. The presenter is Drew Tait.

DR. TAIT: Well, as was just stated, I'm going to talk about the status and priorities regarding the solubility tests, and presently, it's a collaboration between the groups from Los Alamos and from Lawrence Berkeley Labs.

We basically have two prongs to our approach. One is bulk solubility, and the second is speciation and thermodynamic database development, and the last part of this talk will include summary and research priorities.

The priority elements are set into two levels. The highest level involves the actinides, uranium through americium, plus technetium. Uranium has been dealt with with other groups, so we haven't dealt with that, and so far, we've concentrated on neptunium, plutonium, and americium, with technetium coming up for next year.

The first prong of our approach is the bulk solubility measurements, and the nature of these experiments is that they are long-term experiments. You should do them from both under and oversaturation in order to get confidence that you've reached the steady state. You need to get quantitative separation of the liquid and the solid phase, so you can analyze correctly, and you need to be able to analyze what the solid and the solution are, especially the solid.

Included in this approach, since they're such long-term experiments, and you can't do an infinite number of them, is bounding conditions, in which carbonate has been chosen as the top priority ligand, and, basically, three different carbonate levels, from no carbonate baseline, to carbonate levels typical of J-13, to carbonate levels in UE25 water used as an upper bound.

Temperature has been an important variable for us. TSPA has solicited temperature results from us. pH, obviously, is an important variable, and when we get to technetium, the E_h will also be very important.

This is an example of the results you get from a bulk solubility experiment. We have here the concentration of neptunium, basically, on a log scale in time and days, and the first thing you can see is that the typical experiment is run for on the order of half a year. If you concentrate on these top two curves, the solid circles are from oversaturation, in which you dump in a bunch of neptunium, in this case, and it will precipitate, and over the course of this experiment, it should find a fairly stable, solid form. You hope it finds the lowest energy solid form until it gets to a steady state at longer times.

You can then try to figure out what the solid is, and introduce it into the water that you're interested in, and it will start dissolving over time until, hopefully, they come to about the same level, in which case you've bracketed the solubility from both oversaturation and undersaturation.

So, each of these points represents an experiment from oversaturation, which is on the order of half a year, and it includes solubility on a log scale, and temperature on the X axis, and you can see, it has UE25 water and J-13 water.

DR. LANGMUIR: Drew, are you dissolving neptunium oxide? What is your starting compound? What's the phase?

DR. TAIT: This is from oversaturation.

DR. LANGMUIR: Of what? What's the phase that's being dissolved? Okay, well, you're dissolving something from under. What's the phase that we're looking at here?

DR. TAIT: From underneath--

DR. LANGMUIR: Well, you're going to the same side, but it was some phase, whether you go over, down, or up.

DR. TAIT: That's right.

DR. LANGMUIR: What is the phase we're looking at?

DR. TAIT: From undersaturation?

DR. LANGMUIR: Either way.

DR. TAIT: From oversaturation, we basically dissolve a neptunium oxide in acid.

DR. LANGMUIR: Neptunium oxide?

DR. TAIT: Right, but the undersaturation are carbonate species, but there is some question as to what that should be right now.

DR. LANGMUIR: And that's what you're pursuing?

DR. TAIT: Yes; exactly.

Again, these are temperature dependent, and you can see that neptunium is basically the highest solubility species present, followed by plutonium in green, and, finally, the americium, and one thing I'd like to point out is that the TSPA has sort of oversimplified this data by assuming a very simple reaction of one single precipitate going to one single solution species over the entire range, and fitting that thermodynamically, whereas, in fact, the solid form does change over this temperature range; for example, in neptunium. So, the curves that they've drawn really don't have the thermodynamic validity that we might hope for.

The status of the bulk solubility so far is we've done the oversaturation experiments for all the top priority elements, except for uranium, which was left to other groups, and technetium, which we plan to do next year.

We've started the solid precipitation characterization, and, as I said before, there is some question as to the neptunium. It's not constant throughout the temperature range, and, basically, the stoichiometry really doesn't match what you would expect from a finite number of possible crystal structures that we'd have.

So, we need to do solubilities from undersaturation. Now, we've got a pretty good handle on oversaturation. We need to start the undersaturation work, especially for J-13 and neutral electrolyte species.

The second prong to our approach is to develop a thermodynamic database; in other words, do speciation. And the justification for this is that the bulk solubility experiments alone only give you a finite number of discrete points, and we'd like to be able to calculate what the solubility is for a general case, for a general water.

For instance, bounding waters may not bound in all cases. You could have boiling for your pore water, which could change the pH or the carbonate concentration, or you could have things introduced into the engineered barrier system, such as cement, which could cause cement waters, which are very high in pH relative to what we've been looking at so far, so we'd like to get a thermodynamic database so we can calculate and model the general case, and to do that, we need to get speciation information, because that's a prerequisite information to do any thermodynamic modeling. You have to know what's there.

It's also possible that this could help as an input to repository design, and Rich Van Konynenburg is going to talk a little about speciation and EBS design. And, finally, it's our key input to PA models.

The basic approach that we've been using for speciation experiments involves a combination of complementary techniques. The x field here is the concentration of the radionuclide in log terms, where these techniques are valid. Some of these techniques, such as vibrational and NMR spectroscopies are very structure-specific. We can get a very good idea about what's in solution. However, they aren't very sensitive. We can't go down to low enough concentrations to study the environmental conditions directly, the conditions you would expect in a repository.

In order to do that, we can match up the known structure that we get with these techniques, to a spectrum, either an absorption, or photoacoustic absorption spectrum, or a fluorescent spectrum, and then we can use that information and go down to the concentration range that's relevant, directly relevant to the repository.

DR. LANGMUIR: LIF is laser-induced fluorescence, is that--

DR. TAIT: Excuse me?

DR. LANGMUIR: What does LIF stand for?

DR. TAIT: Laser-induced fluorescence.

So, I'm going to give a couple examples of some of these spectroscopic techniques. From potentiometry and solubility work, Grentha's (phonetic) group has proposed a mechanism in which a monomeric neptunyl carbonate under acid conditions forms. What they propose is a trimer, and we can use NMR, Carbon-13 NMR to look at the carbonates on each of these structures.

For the monomer, you would expect only one single signal, because all the carbonates are equivalent; whereas, in the trimer, you would expect two signals, one from these terminal carbonates, and a second signal from the bridging carbonates. Again, these were only proposed from very indirect evidence, but the NMR gives direct support for that indirect evidence.

Here we see the Carbon-13 NMR's signal from the monomer, and here are the two signals that come in tandem, so they're the same species, and that's exactly what you would expect from the trimer system, to have both a terminal and a bridging ligand, and of course, you can get thermodynamic information by integrating under the areas of the curve, so you get the relative amounts of the species present, and the thermodynamic data from the NMR experiment agree quite well with the fitting that had been done by Grentha's group.

To show what we can do for very low concentration actinides, this is a photoacoustic, a series of photoacoustic spectra. Photoacoustic spectroscopy gives you the same information you get from the traditional electronic absorption spectrum, except it gives you that information at three orders magnitude more sensitive, so you can go very low in concentration and still get an absorption spectrum.

This series of spectra here are taken at 250 nM Pu, and you can see the peak due to a coordination of a carbonate ligand onto a plutonium complex as you increase the carbonate concentration. You see this peak clearly go in at 485 nanometers. This technique is also very amenable to do temperature dependence. You see here, we can increase the temperature up to the boiling point, basically, and we can see that the 485 nanometer peak of this carbonate species becomes less important at higher temperatures, but that change is reversible. There's actually two spectra here at the 25° reading, and that's before and after heating it up.

What we've been working on most this year is neptunium data, and we were initially interested in getting carbonate information, but we had to back up, because even the fundamental reaction of hydrolysis was not well understood. I've written the hydrolysis reaction here in terms of the fully aquated neptunyl, plus hydroxide, going to the neutral species, and this plot here shows the log of that equilibrium constant, ionic strength on the X axis, and the different results from the literature, depending upon which group you want to look at, so you can see it varies over several orders of magnitude, so the very basis of looking at neptunium is not well understood.

One of the reasons, I think, that the data is so discordant, as shown in this slide--this is basically a fit to the data from two different solubility studies; the log of the neptunium concentration on the Y axis versus pH, and there are some important differences here, but one thing that they both say is that you need to work at very dilute solutions, or else you'll get problems in precipitation that will add into your mix, if you're not careful.

You also need to be very careful not to have carbonate, or carbon dioxide present, because that will add on to the fully aquated neptunyl species very easily.

So, what we've done is, we've had to work in a glove bag, under an argon atmosphere, and if you're very careful about it, you can subtract out the water background, which is very strong in near infrared region, and still see the strong absorption peak from the fully aquated neptunyl ion at 980 nanometers.

We can use that peak to monitor the fully aquated neptunyl species as we add hydroxide by increasing a pH, until, finally, it starts disappearing, because you form the neutral species, and we can fit that curve and come up with an equilibrium constant, and so this is where our data fits into the literature data. And this is at room temperature, and we are presently--hopefully, as I speak--starting to do the higher temperature regions, up to the boiling point.

So, as a summary of our speciation studies, we have developed a complementary set of spectroscopic techniques. We've looked at plutonium (IV) hydroxide carbonate system. We are looking presently at the neptunium (V) hydroxide carbonate system.

We have, I think, a good room temperature hydrolysis constant. We are doing the higher temperature experiments now, and we plan very shortly to start using NMR and absorption to look at the carbonate complexation as well, as a function of temperature.

Just to emphasize the point, the drivers to this study is to develop a thermodynamically defensible input for PA on the effectiveness of a solubility barrier, so, therefore, we won't have to rely on conservative, which are generally higher, estimates, and the speciation information we use can also be used by other tasks, such as sorption with the neptunyl work. There we're interested in what species were present, and, possibly, also, the EBS task; for instance, would it be worthwhile to add some sort of redox buffer in the near field for a case like technetium.

So, our research plans, which we hope to get done by '98, would include, again, the neptunyl studies which I just talked about, to include bulk solubility and speciation.

The technetium work is a top priority, also. This shows the technetium (VII) solubility in near neutral conditions, and this is the technetium (IV) solubility, and this technetium (IV) solubility is not very well known. That's a very approximate number, but it shows what you can gain, possibly, by trying to reduce the technetium.

So, we need to get the bulk solubility under reducing or near-field conditions, and we'd like to develop some idea of the database for hydrolysis and carbonate complexation. These experiments will be difficult, because Eh is a very key variable.

The plutonium studies, I think I'll let Ines talk about plutonium, and, finally, we'd like to do some modeling and database evaluation. We need to be able to test the self-consistency of the database and to make sure that what we get corresponds to the bulk solubility experiments that we have solid data on, so that we'll feel good that we can model the general case, and that's what I have to present.

DR. LANGMUIR: Thank you.

Do we have time for questions from the Board or the Board consultants?

Don Rimstidt?

MR. RIMSTIDT: Yes. You show some solubilities in your concentrations for neptunium and other species that are actually quite high compared to what I would have expected in repository solutions. Is there any reason to believe that the solutions will ever become super-saturated with respect to these phases?

DR. TAIT: One of the reasons that these experiments take so long is that you try to avoid exactly that. You try to let that solid phase get to its lowest energy state so, in fact, you are in good equilibrium.

One thing that might be tricky, those units were the log of grams per cubic meter as opposed to molarity, so it's what TSPA actually used in their study, so that might be why they look strange to you.

MR. RIMSTIDT: Can you tell me, then, approximately what kinds of concentrations we're talking about?

DR. TAIT: Sure.

MR. RIMSTIDT: In molarity.

DR. TAIT: These right up here are on the order of 10^{-3} moles per liter.

MR. RIMSTIDT: But that's still quite high. It seems to me that you'd have to dissolve an enormous amount of waste in order to get that kind of concentration in solution in the repository system. Is that feasible?

DR. TAIT: There is, ultimately, a lot of these actinides present in the waste form.

DR. LANGMUIR: I had the same question, actually. Maybe Inés would comment on it, the issue of whether there's enough of many of these radionuclides ever around to make solubility a limiting factor on the higher concentrations.

DR. TRIAY: Look, I mean, the PA will use either the solubility limit or they would use beta, based on dissolution rate, whatever is available, and this is what was available, so this provides a conservative calculation, so that's essentially the answer. I mean, PA has the intent of using these solutions rates whenever they are available.

Now, remember that when we talk about dissolution rates, now we're back to the talk that was given this morning by Stout and others, and the idea there is that you actually provide a neptunium concentration or an actinide concentration that is rather large, because these are associated with particulates, rather than soluble species, so you're kind of caught here, because this truly provides the solubility limit.

The other one would be based on a dissolution rate.

However, if you combine that with the fact that particulates are seen when coming from glass and spent fuel, you're going to have a hard time saying it's less than that just based on these other studies that they are doing, because then you're going to get caught into the argument of the actinides are associated with particulate, and, of course, then the amount of actinide in the solution is going to be a lot higher than that.

DR. LANGMUIR: And then we can discuss separately whether particulates will go anywhere.

DR. TRIAY: That we will discuss, Don, I assure you.

MR. APTED: I wondered if you've done, in terms of constraining the phases that you want to look at in these solubility tests, looked at any of the service and phase characterization that comes out of the waste form tests on either fuel or glass, because, often, those are much different, more garbage-can minerals than what you have here, things, you know.

Neptunium and plutonium all don't necessarily form their own separate phases. They're going into these other complex solid solutions in these things, and one has to wonder what's the relevance of nice, pure solubility determinations when what is going to happen in the repository is going to be somewhat complex, more complex, and how you're going to relate your data to that complexity.

DR. TAIT: Well, you need to start some place, and this is, like you said, a clean system is where to start, and then to fold it into the processing model.

MR. APTED: This is your first year of collecting solubility data?

DR. TAIT: No. But again, they take quite awhile to get, each one of those points.

DR. MEIJER: I'd like to add just one thing to that, and that is that if you're going to make long-term predictions, then you'd like to do that based on a good theoretical framework, and that's what thermodynamics, chemical thermodynamics provides you, and if you're going to use that framework, you need those kind of data, and if you're going to make the long-term predictions, you need that framework, so you can't really do without this stuff.

DR. LANGMUIR: Your name, Arend Meijer?

DR. MEIJER: Yes, I'm sorry; Arend Meijer.

DR. TRIAY: Yeah. The idea is that if you're going to use any kind of modeling approach, you saw the uncertainty that he showed to be now existing in the literature, just on the formation of these hydroxyl compounds, so, you know, if you're willing to say, well, I put neptunium in J-13 with a bunch of minerals, and I observed what happened, and if that, in your mind, has any value for extrapolation, then that's fine, but I contend that that would have no value for extrapolation whatsoever.

DR. LANGMUIR: I had one last question for Drew. You've used carbonate as a choice ligand to decide what the maximum effect might be on mobility due to complexation, and I wondered, fluoride's a good complex, phosphate's a good complex. There are lots of other things that could be in the natural system, including organics; hard ligands that might go with the hard cations, radionuclides, and I wondered how you discarded them, or what the next step is to take those things into account as possible movers of the nuclides?

DR. TAIT: Well, part of the reason is carbonates are a very strong complex, also, and there's so much more of them present than something like fluoride or phosphate. Phosphate might become a very good news in terms of performance of the mountain, because phosphates of uranium, for instance, are very generally insoluble, so otenite forms very readily, but, again, we've chosen carbonate mainly because there's so much more of it, and it is a very strong ligand, and it does tend to solubilize species.

DR. LANGMUIR: I would assume at some point you're going to consider all these other ligands in some way, at least estimating stabilities and estimating behavior?

DR. TRIAY: And, remember, that when he was only considering bicarbonate or carbonates was for his speciation studies. The studies of solubility are done in J-13 and P-1, and those have the phosphates in it, and the fluorides, and whatever else, so, presumably, he could take that data, which is what he's going to do, and say, if I can predict, based on complexation of bicarbonates alone, why would I start these other studies that are going to take some time?

So, that is the intent of this exercise, to do something that is empirical observations, and then take the data that is done with these very clean systems, and try to model or predict what you already observed. If you're lucky, you're done.

DR. LANGMUIR: I assume you've given part of your talk, Inés, already?

DR. TRIAY: No, no. I'm sorry. No, I want all my time. I'll shut up.

DR. LANGMUIR: Well, thank you, Drew. I think we'll go on now to the next presentation, which is status and priorities; sorption of long-lived radionuclides. The speaker is Inés Triay.

DR. TRIAY: Okay, status and priorities regarding sorption. Let's start with a reminder of what a minimum K_d strategy, developed by Arend Meijer, is. We already have discussed this in previous meetings, and what we have here is just to summarize. You have questions on this, we can discuss it, but I think most of you are aware of this, is that we are essentially going to have a minimum K_d for some of the radionuclides for a minimum sorbing major mineral. In some other cases, we have a minimum K_d for a sorbing rock in Yucca Mountain, and that's for these radionuclides.

In other cases, we're going to have a minimum K_d for sorbing rock in a particular hydrologic unit, and that's for these radionuclides, and this is what remains to be considered. Let me just stop here for a second and tell you that all of the information that is available already in the sorption databases that Los Alamos has cover all of these radionuclides here. So, essentially, we think that we can make an argument, based on the empirical data, as well as sorption mechanisms that are somewhat well understood for some of these radionuclides, and be able to defend these choices for K_d values. The choices that are now in the performance assessment calculations that were presented by both groups here this morning include this sorption data.

Now, what remains? We're only talking about uranium, neptunium, selenium, technetium, iodine, carbon, and for technetium and iodide, essentially, what we're saying is assume for now no sorption barrier. That's what was assumed in the performance assessment. Now, bear in mind that the reason we're assuming this in this particular case, is because we are assuming oxidizing conditions. Under those conditions, these are anions, technetium, pertechnetate, anion TcO_4, and essentially, not only doesn't sorb, but it gets excluded from the pores of the tuff in terms of size and charge.

Now, that leaves us, then, with uranium, neptunium, and selenium. Our efforts have been on neptunium, because it has a high solubility, and because of the performance assessment calculations that are already available, and I am going to concentrate on that today, but I'm going to touch on two other things.

Uranium, we think that there's enough data in the literature that, perhaps, by doing a sorption modeling approach, and trying to predict what was empirically observed with these tuffs, there is a very limited amount of work that needs to be done for uranium.

DR. LANGMUIR: Inés, could I interrupt you for just a second?

DR. TRIAY: Yes.

DR. LANGMUIR: We all assume that we all know what K_d is, and you chemists in the audience do, but I think if you could define it for the rest of the audience, it might be nice.

DR. TRIAY: Fair enough. I will do that. The K_d--and that is a really very excellent point, because I'm using K_d here as a measurement, not as a model. We get into very long, overdrawn discussions as to the value of a K_d, you know, and so on and so forth.

Let's say for the remainder of this talk until the end, where we're going to start valuating K_ds, with K_d as a measurement, and what I mean by that is that the K_d is simply given by the moles or the amount of a radionuclide per a gram of the solid. That's in the numerator.

The denominator, you have amount of radionuclide per milliliter of solution phase. The K_d implies equilibrium, which we cannot tell you that we actually achieve equilibrium in these experiments, so, again, what I'm talking about when I talk about a K_d is an actual measurement of the distribution of radionuclides in the solid phase versus the solution phase.

DR. LANGMUIR: Maybe you can tell them what the number one for a K_d might mean, versus a number 100, or a number 0.

DR. TRIAY: Well, no. Let's talk about retardation factor, perhaps. If you have a K_d of zero, essentially, what you have, is in the retardation factor equation, you know, you have one plus rho K_d divided by epsilon, have K_d of zero, have a retardation factor of one. So that gives you some calibration, and the higher the K_d is, the larger the retardation factor, if you're a modeler, of transport is going to be.

All right. What we have done in terms of neptunium, to try to resolve the issue of what actual sorption values to use for neptunium, is to divide the type of tuff into vitric, devitrified, and zeolitic. Let me say a couple of things about sorption.

Number one, sorption essentially--what we are studying when we study sorption, is a chemical reaction of a radionuclide with a particular mineral in the tuff. These chemical reactions, if they're defined correctly, owe nothing to whether the mineral is part of the tuff or part of the fracture, something that I want to address a little bit here later on, and I want to, out front, make sure that you realize that when you study a chemical reaction now, you're not constraining hydrologic flow. That comes when you actually start talking about transport. So, let's divide things a little bit so we know exactly what is it that we are evaluating and considering.

Okay. So now we have the vitrified, vitric, and zeolitic tuff. Our model zeolitic tuff, for the purposes of this talk, is going to be G4-1510. For the purposes of this talk, GU3-1405 and GU3-1407 are going to be our vitric tuffs, and you can see here the per cent class, of course, those tuffs.

Now, for the purposes of the devitrified model tuff, we're going to consider G4-270 and G4-268, mainly feldspar, okay? Now, why do I have G2-723? G2-723 is a particular tuff that is high in terms of calcite content, so let's just keep that in mind; zeolitic, vitric, devitrified, and a tuff that has a high calcite content.

The next view graphs that you have, we're now going to--well, the next view graph we're going to discuss. The other ones, we are not, but it's for you to have some data if you want to go back home and think about this.

Surface area of tuffs and minerals. Again, vitric tuff, devitrified, zeolitic, talking about on the order of 20 m2/g. We have here the natural calcite that we use for our sorption experiments. This calcite does not come from the tuff. This is a natural calcite from another site; quartz, hematite, and clinoptilolite. All of these are pure minerals, and I have given you, in the next, let's see, three view graphs, which we can skip, the actual information on surface area, as well as mineralogy obtained by XRD and BET analysis. So, if you care to look that over a little bit later, you have it there.

Chemical analysis of J-13. Let me say a couple of things. A lot of people are extremely worried always, you know, what happens to J-13 when you pull it from the ground. Do you precipitate calcite out? Well, frankly, that doesn't look here to be the case. I have given you, in your handouts, these actual numbers for the J-13 reference, you know, the one that was actually analyzed at the site, and then this other J-13 unfiltered, filtered are obtained as a function of time. I mean, in the handouts that follow this one, I have told you when those samples were collected.

As you see, this is a pretty stable water as a function of time, and it's not clear that anything is precipitated out, because you pull it to the surface. That is not the case with UE-25 p#1 water.

The reference water here--this is data obtained from the Olgard report that we have discussed many times during these meetings. Essentially, what you see here is, this is calcium. This is the bicarbonate content, the reference water. All of this UE-25 p#1 waters that were actually used at the lab, calcite already precipitated out by pulling it, by pulling the water from the subsurface to a surface, taking it to Los Alamos. We filter it, and that is essentially what is observed here.

DR. LANGMUIR: Inés, from what formation does this water derive?

DR. TRIAY: This is the Paleozoic carbonate aquifer.

DR. LANGMUIR: The same formation that J-13 comes from elsewhere?

DR. TRIAY: No, and I have a view graph that you don't have in your handouts, but I guess that we're going to use it anyhow. Here is what we're talking about, okay? That's UE-25 p#1 right here, okay?

DR. SIMMONS: The depth of the carbonate is about 3,000 feet or more. The Paleozoics are way beneath all of the tuffs at Yucca Mountain.

DR. TRIAY: I think that the J-13 water comes from about 350 meters from below the surface, so you're talking, I don't know, a thousand feet, something like that.

Okay. J-13 chemical analysis--oh, and so a thousand feet versus three thousand, which is what Ardyth just said.

Okay. So, J-13 and UE-25 p#1 chemical analysis. Here I have told you what date they were collected, and we're not going to go through that, but you have it in your handout in case you want to look at it in detail.

Okay. Batch sorption experimental procedure. This is what was done for the data presented here. We're going to discuss a little bit what is the next step after this, and then Jim is going to take it from there.

Okay. So, essentially, what you have here is you pre-treat this soil with the groundwater that you're going to be considering. You actually then take that pre-treated solid, and equilibrate it with a solution, separated at phases by centrifugation, and determine the amount of radionuclides in each phase. Please note that the amount of radionuclides in the solid phase for the purposes of this presentation was calculated by difference.

Controls. We always use a solution in the same bottle that we use for the rest of the experiments, with no solid phase in it. In that case, if there is sorption onto the walls of the container, or precipitation, you will be able to see it.

Definition of a batch sorption distribution coefficient, or K_d. Again, in this case, I already told you, moles of radionuclide per gram, moles of radionuclide per milliliter of solution, and, again, this is a measurement. It's a actual measurement. It's not a model at the moment.

Now, when we go from K_ds to Kas, we are going to talk about Kas here in a second. The only thing that I did to go from K_ds to Kas was that I divided this K_d by the surface area of the tuff, and adjusted the units to come up with meters, and the units are given in the view graphs, but that's what we referred to a Ka, just K_d divided by surface area of the solid phase that one is looking at.

Neptunium sorption in J-13 as a function of tuff type. Bottom line, just zero for devitrified and vitric tuffs. It's not worth, in my opinion, to go any farther than that, and I'm not only going to show you the tuffs--this is the devitrified tuff. Remember, this is mainly alkali feldspar. This is the vitric tuff, mainly glass. I'm also going to show you some mineral data.

Next view graph, we have the zeolitic tuff, okay? Now, remember, that you've heard talks here this morning on PA, two of them, and sensitivity analysis, radionuclide transport. In those cases, neptunium, a value of neptunium of K_d of about two in the PA calculations, of about four for the sensitivity analysis. Those values are supported here in this graph.

By the way, I hope that you, of course, read this. This is sorption coefficient as a function of initial neptunium concentration in micromoles per liter, so you're really talking about, in this case, 3×10^{-5} moles neptunium solution.

Okay. So, just a second, let me focus your attention on the fact that the tuffs were wet-sieved. We have discussed these procedures for fast sorption in other meetings; particle size, 75 to 500 micrometers. The experiments that have been performed--and I believe that you have seen these during the last DOE/NRC interaction, and there is a paper by Rogers and Meyer. There's a report coming out as well.

This type of approach to batch sorption seems to give the optimum results in that these K_ds are conservative, because when you wet-sieve the tuff, you can get rid of some clay particles that may be just exposed during the batch, which are not exposed when, actually, radionuclides are flowing through the tuff.

Now, let's compare that data on G4-1510, which, remember, it was about 60 per cent clinoptilolite, with data on pure clinoptilolite. I have not corrected for surface area yet, and that is extremely important. All I did here was K_d as a function of initial neptunium concentration, pH 7 and 8.5. Let me tell you how these pHs were obtained.

When you pull J-13 water or p#1 water from the mountain into your lab at Los Alamos, CO_2 escapes, the pH goes up. This pH of 7 was obtained by having CO_2 over pressure, and that is how we were able to get the pH back down to 7. These experiments are performed inside glove boxes.

Okay. Now, let's look real quick here, albite, quartz, as you can see, zero K_d as a function of initial neptunium concentration. Okay, now, very important view graph because, remember, that we said that everybody had used a K_d of 2 or so, 2 to 4 for their PA calculations. When one goes from J-13, right, to UE-25 p#1, which has a lot more carbonate in it, and I showed you those figures, essentially, the K_d goes down tremendously, you see here, by a factor of two or more.

The same thing happens with clinoptilolite, so we're essentially comparing pH 7, pH 7 for UE-25 p#1. pH 7, clinoptilolite, J-13, pH 7, UE-25 p#1 for clinoptilolite. We believe the reason for that is that clinoptilolite is actually sorbing neptunium by a combination of ion exchange and surface complexation, when it is an ion exchange in J-13 is because the neptunyl cation concentration has gone up when you go from pH 8.5 to pH of 7.

Okay. Now, neptunium sorption in J-13 onto other minerals and this tuff that is high in calcite. Now, again, let me point out something here. Sorption. I have given you the sorption time, reason being that, normally, sorption is fast, and we have found it to be very fast for the tuffs. We have done experiments as a function of time.

As a matter of fact, in the case of calcite, and in the case of hematite, not in the case of clinoptilolite, this is only true for calcite and hematite, sorption seems to increase as a function of time, so there is some kinetic effect, surface effects.

Okay, so G2-723, remember that this was the tuff that had a high calcite content. In fact, the calcite content was about 30 per cent.

I hope I haven't confused you. I'm plotting here a log of K_d, rather than just K_d, like in the previous view graph, so now we're talking about, really, a lot of sorption compared to the sorption for neptunium that you saw in the previous view graph. We're on a log scale now.

All right. Extremely high sorption for hematite. There's a paper coming out by Leckie, et al. on this subject; very high K_ds for calcite, and, as a matter of fact, if we don't correct for surface area, even if we take the data as is, one can predict from this number--it's about 100, this is about 30 per cent calcite, essentially, the sorption of that you see here for GS-723.

Okay. Sorption as a function of surface area. Now, what I'm going to try to do is show you all the sorption coefficients that we obtained for different tuffs that were considered by the Los Alamos group, and then I plotted that on the same graph as the surface area obtained for all of those tuffs.

All the surface areas that are high here, you know, on the order of 20 or so, correspond to samples that have a high clinoptilolite content. You can see that on the next view graph.

Let's assume for a minute that we know the K_d for pure clinoptilolite. We also know the surface area, so we can get a Ka based on the sorption coefficient and the surface area. Now, can we predict, just in that Ka, based on the amount of clinoptilolite in the tuff, what is going to be the Ka of the tuff? And, as you can see, you get reasonable predictions for samples that are high in clinoptilolite content.

Now, I have been talking about calcite a lot, and let's state again, with the chemistry. These are near surface samples, 25 feet below the surface, so I'm not suggesting that the calcite in these particular samples are going to retard neptunium, but if the calcite is associated with fractures at Yucca Mountain, we have done some experiments where we actually can predict very well the sorption coefficient of the neptunium associated with the calcite in the fractures, and this is just an illustration. This is the per cent calcite in some arbitrary near-surface tuff samples, plotted with K_d, and you see that there is a very reasonable correlation between the K_d and per cent calcite in those samples.

Now, where are we in the sorption program of Los Alamos National Laboratory? A couple of things that I was pointing out before, in the previous, in the minimum K_d strategy that you actually had in front of you at the beginning of this talk.

If you notice, plutonium, in some of those minimum K_d strategy view graphs, had a question mark associated with it. Why? Reason is that plutonium can exist in several oxidation states.

Let me point out something to you. G2-723, right? Again, that tuff that has so much calcite in it. Meece and Benninger, 1993, published a paper saying they actually were able to measure plutonium in calcite as a function of oxidation state, plutonium-3, plutonium-4, plutonium-6. They compared the plutonium data with thorium and americium data, and they got extremely high sorption coefficients. In fact, 99 per cent of the plutonium at those oxidation states, as was the americium and thorium, went into the solid phase.

What happens when they looked a plutonium-6? Well, then the distribution coefficients decreased tremendously. Well, guess what? That's exactly, that correlates exactly with what is seen here. In fact, they report a uranium sorption ratio of about two milliliters per gram for their arbitrary calcites. So, as you can see here, this agrees with the general trend for sorption mechanisms of radionuclides associated with calcites.

The problem, again, the concern here is that in the case of plutonium, we have all those sorption coefficients. Based on those sorption coefficients, performance assessment used a value of 100 for their calculations. There's a lot of plutonium in the inventory, and they need a value of 100 to give you the calculations that you just saw.

If plutonium exists in the four oxidation states, no problem. This is absolutely right. If it doesn't, and if it exists in the six oxidation states, now you're maybe more looking at these kinds of values, rather than these kind of values.

So, what do we propose to do? In the next view graph. Go there with me, and I'm trying to hurry up, because I want to leave time for some discussion, if possible.

We think that this is where we're at in terms of the sorption program of Los Alamos. We reduced the number of radionuclides that need to be looked at to these right here; neptunium, selenium, uranium, and plutonium. Now, I want to tell you why, as well as what is it that we think needs to be done for those radionuclides.

On neptunium, as I said, sorption is very high onto calcite and calcite-rich tuffs, and this may be very important, because if the calcite is associated with the fractures, then you have a very good case to make for neptunium retardation through a fracture flow control scenario. The chemistry doesn't do anything to the hydrology. This is a reaction that can be looked at in the absence of discussions on hydrologic flow.

Now, the devitrified and vitric tuffs suggest we consider the sorption to be zero, negligible. Now, in zeolitic tuffs, if you are in UE-25 p#1, doesn't really matter what pH range you are at; negligible sorption is what should be done.

Now, if you are in less concentrated waters, more dilute waters, then you have this chance for two to five milliliters per gram of sorption coefficient, and that is in the pH from 7 to 8.5.

Now, sorption increases with increasing pH in zeolitic tuffs in J-13. Notice that a lot of people have been concerned about iron oxides, and maybe iron oxides are all dominating actinide sorption in the tuffs has been proposed by us as well as others. It's not an unreasonable assumption; however, it doesn't look like that is the case. This trend would be reverse if we were considering hematite. It would go exactly the opposite.

Sorption onto clinoptilolite-rich zeolitic tuffs can be predicted reasonably, based on the amount of clinoptilolite and the surface area of those tuffs, which, by the way, doesn't seem to vary very much when one has a lot of clinoptilolite in the tuff samples.

Iron oxides appear to be passivated in the tuffs, and one of the reasons is the argument that I just made about this trend, and what is the work remaining? I think that what we should do is concentrate on only clinoptilolite, as well as calcite as a function of pH, ionic strength, and bicarbonate concentration. The reason for clinoptilolite is to make absolutely sure that when we use a number from zero, essentially, which is what we would have to do, to about five, we know exactly what we're talking about in terms of correlations with the water chemistry.

Calcite, for the obvious reasons, if the calcite is associated with the fractures, then you would have a very good argument to say that neptunium, as well as other actinides, would be retarded.

Conclusions on selenium and uranium. Selenium and uranium sorb very poorly, and that was in a previous view graph. The work remaining, in my view here, is to identify the minerals dominating the sorption, especially of selenium. Selenium will exist either as selenite or selenate, and this is an anion, and, however, we observed some measurable sorption. We need to identify where is that coming from to be able to technically defend the numbers utilized.

Now, for uranium, I think that there is so much data in the literature that the next step simply should be to try to figure out whether, based on the data in the literature, we can model or predict what has been empirically observed for the tuffs.

Plutonium. If plutonium exists in one oxidation state versus the other, we're going to have to give distribution coefficients for that. Worse yet, those sorption coefficients that you saw in the previous view graph were based on, as I said, centrifugation, separation of phases by centrifugation.

If plutonium forms colloids, polymeric colloids, true plutonium colloids, that data that was in the previous view graph would have some artifacts in it, because, essentially, what you did was separate the phases without knowing whether the plutonium was actually complex to the tuff, or just simply agglomerated and sedimented out during centrifugation, but this is not hard to validate what needs to be done here, and, I mean, essentially, we're almost done, I think, with this work, is to validate these batch sorption coefficients using column data.

What we observe is that there is some kinetic effects related to mass transfer here of plutonium, and, actually, we think that we're going to be able to validate the sorption coefficients in the previous view graph, provided that plutonium exists in the four oxidation state, four, or five, perhaps.

Now, what else I need to tell you? I also need--

DR. LANGMUIR: Inés, you're six minutes into Jim's talk.

DR. TRIAY: Oh, I'm sorry. Okay, well, let me introduce Jim, then.

The next step is, of course, to validate these batch sorption coefficients, very selected sets of these batch sorption coefficients that we have, using column experiments. It was brought up by this Board that if you use batch sorption coefficients to predict what would happen under unsaturated conditions, what is the validity of such an exercise. Jim is going to address that, and, of course, we use the same type of approach to validate sorption in fractured tuff as well as solid tuff.

DR. LANGMUIR: Thank you, Inés.

Jim Conca's presenting the next talk; selenite transport in unsaturated tuff from Yucca Mountain.

DR. CONCA: Thank you.

Specifically, I'm going to talk about selenite transport in unsaturated tuff, but, in general, I'm going to talk about a method for doing these type of studies for which the selenite transport work is the first step in it, and, to do that, this is actually what I'm going to talk about.

I'm going to talk about how to determine transport parameters using this new method, the UFA method. I'll talk about some validation experiments, talk about different transport parameters that we've measured, and then end with the batch sorption studies compared to the retardation tests, the column tests.

Now, this may be redundant, and most of you probably know this, but when you talk about transport, you need to know the three primary transport parameters which govern the transport of any species within solution, and the first of that is the hydraulic conductivity, K, which, with respect to water in this case, which indicates the carrier fluid speed. It's the actual advective flow within porous media.

Diffusion coefficient is the static diffusion of ions within that media, separate from the advective flow going on, and it's very important. If there is no advection going on, then the transport can be diffusion controlled, especially near the waste packages.

And, finally, the retardation factor, which is the term which describes or includes all of the geochemical and chemical effects, such as sorption, precipitation, and even dissolution, and, essentially, is defined as the ratio of the velocity of the carrier fluid--in this case, the groundwater--divided by the velocity front of the contaminated species.

So, as the carrier fluid is moving through the porous media, behind it, retarded somewhat, is the flow front of the species of interest. If the retardation is one, the K_d is zero. There's no sorption, or no chemical interaction between the species, and the substrate, then that means that the contaminated solution is moving at the same speed as the carrier fluid, and that's bad for radionuclides.

Now, the method that we've developed over the past few years to look at transport in unsaturated media, as well as saturated media--essentially, permeable media--is the UFA method. It combines ultracentrifugation with precision fluid flow through a rotating seal from some external source, so this is actually a little Darcy's Law machine. You control the flux, you control the driving force, so you are actually imposing a hydraulic steady state upon the system. The system responds by either desaturating or resaturating, depending on which state you start from.

The nice thing about using a large driving force, such as a 500 g, 1,000 g, is that you reach steady state very fast; in hours, instead of months to years. So, you can actually carry out experiments at some very low water content, or in some impermeable material on the order of days to weeks, as opposed to months to years.

Excuse my typo here, but because an acceleration is a whole body force, analogous to gravity, the mechanism of the flow is actually pretty much the same within the sample, much better than using pressure apparatus, because pressure is not a whole body force.

The operating limits of the instrument are -20 to 150°C, you can get accelerations up to 10,000 g, which is usually rare. We usually go from about 10 to 1,000 g, and flow rates from saturated, which is about 400 as a maximum, to .001 ml/hr, so those are the operating parameters, and if you care about multiphase flow, that's possible, too.

I'm trying to rush through this. This is what it looks like. It's a washing machine-sized ultracentrifuge made by Beckman Forrest, and these are precision fluid flow pumps which you might recognize from a hospital. That's how we discovered them. I had the most fortunate kidney stone attack of anyone, and these were pumping into me.

Anyway, they had just swept the field, so it was actually an incredibly ideal infusion pump for unsaturated media.

Anyway, this is what the rotor looks like, two samples, two fluids, each into the sample from an external source while it's spinning. We have two different sample sizes. In fact, there is two different samples there; show and tell. One sample size is 50 ccs, the other's 100 ccs. We have a third one out now that will be about 200 ccs, so that's the limit of the system in terms of the fracture size and distribution, things like that.

Fluid flows in through the sample and gets collected in an effluent collection chamber, which you can view with a strobe light while it's rotating.

Anyway, once you've reached steady state, you can do with it as you will. You can either determine the hydraulic conductivity which you're actually imposing upon the system. You can do other things. If your sample holder is an electrical conductivity cell, you can measure the electrical conductivity of the sample as a function of water content, and using the Nernst-Einstein equation to calculate the diffusion coefficient works very well at very low water contents, and very important for these type of studies, you can collect, monitor, analyze the effluent solution to look at break-through behavior of species of interest.

To show that this works, these are four different methods for measuring the, or for determining the hydraulic conductivity as a function of water content in this nice, sandy loam soil, and they all agree very well. Not to belabor the point, this is run over three days. These are traditional one-year column phased by Mike Fayer (phonetic). This is Glendengeis (phonetic) 13-year field lysimeter, which is very nice, because it ties the field to the laboratory data, and then there are various estimation techniques for estimating it. So, they all behave very well in that; besides, the theoretical aspect should give you great confidence that this might actually work.

Also, we've done some studies at some Hanford soils, looking at uranium breakthrough as function of volumetric water content. These are two water contents in the sample, and, again, you saw in these soils, the uranyl carbonate species dominates and it's actually very mobile. The retardation factor is about 2.3 there, so this gives us some confidence that it actually works. Actually, that last view graph is not in the handout. I just wanted to show that we've actually done studies like this on soils.

So, the first thing we can look at in transport is the hydraulic conductivity of the porous media of these tuffs, and this is what we've done to date on Yucca Mountain tuffs, and for comparison, some Bandelier tuffs from Los Alamos. Again, you get, essentially, these types of curves where the saturated hydraulic conductivity is in some range, and that decreases as the volumetric water content decreases.

The welded tuffs and the fairly non-porous materials all plot down here. The non-welded and vitric materials all plot up there.

But, fracture flow has its own peculiarities, and the UFA can actually separate out matrix from fracture flow very nicely, because you can control the flux and the driving force, so you can preferentially desaturate different sized fracture sets within a sample.

Because the matric potential is no longer a Darcy driving force in the system, that's fine. However, this acceleration is a medially-symmetric one-dimensional drive force, whereas matric potential is operating at three dimensions, and it still determines the fluid flow paths within the system, so just like in the field under 1 g, the mechanism of flow through the sample even under 1,000 g will still follow the same fluid flow paths. So, capillary bundle theory should still hold, if you hold to that.

So, to demonstrate that, we took a fortuitous core through which--this is the Topopah Spring tuff. This is a core that had a 200 micron fracture that almost, but not completely spanned the entire core, so it was actually a solid, good, hard core, but it had a 200 micron fracture starting at the bottom that did not quite make it to the top, and did not quite make it to the other side, essentially, so you had a situation where you had a nice, well-defined fracture. I've actually darkened it in so you can see it. It's only 200 microns, and in the surrounding matrix that should behave as predicted in the system.

So, from playing with other cores that had no fractures in them, or no large fractures. We knew that 2 ml/hr flow rate, 7500 rpm would give us a not quite saturated matrix flow with no fracture flow, so we put the core on dry, so that we could actually see the flow front, the transient flow front as it was moving through it. We ran it for two hours, took it out, and this is what it looked out.

Unfortunately, CT scanning and tomography are not what I'd like it to be, so this is creative xerography. This is a Xerox copy of the core, the only way to portray it, and this is the actual flow front as it looked from the outside, so, unfortunately, you can't see into the core, but you can basically see around it, and you had the normal fingering that one would expect moving through the material like this.

There was no obvious flow along the fracture in two hours, so putting it back in, upping the flow rate to 5 ml/hr, which I knew beforehand would saturate the matrix, ran it for one hour, carefully looking in the strobe light to see if there was any flow, and there wasn't any, and then, suddenly, after 50 minutes, there was a constant drainage of 6 mls over a ten-minute period. Took that out, looked at it, this is what it looked like.

So, obviously, what happened was, saturating the top of the core began to saturate the matrix along the fracture walls. You had to saturate the fracture, the matrix surrounding the fracture before you would maintain flow within the fracture, so that's kind of nice. That 6 ml flow over a ten-minute period is actually the hydraulic conductivity of the fracture. The previous flow slide was the hydraulic conductivity of the matrix. So, this should give some feel that the UFA is a useful method for looking at matrix and fracture flow in porous media.

Recapping the definition of the retardation equation, the retardation is the, again, the velocity of the carrier fluid divided by the velocity for another species equals 1 plus rho K_d over the porosity, and if K_d is zero, then the retardation factor is one.

And there's been some debate, although I think it's pretty died down now, what you should use for that porosity, whether you should use the total porosity, which is usually used, or the volumetric water content, just that porosity that is filled with water, whether you should eliminate dead-end pores or diffusional porosity or that kind of thing.

DR. LANGMUIR: I have to ask you, what do you think?

DR. CONCA: You actually should normalize almost every transport parameter to the volumetric water content. It's sort of a master variable, like pH in solution chemistry, because you're actually normalizing to the water that's there, and if you don't do that, then you have to really know what sample you are looking at, and it's hard to compare.

The batch sorption experimental procedure, actually, I don't need to go over that, since Inés already did. Taking J-13 well water spiked with different concentrations of selenium as the selenite species, choosing selenite because it's fairly stable at low temperatures, it usually requires biological mediation to oxidize, so it should be very stable.

We see that, over these concentration ranges, which are actually fairly high, the K_d varied from very low, you know, below one to almost zero, up to 30 or 40 as the concentration of selenium went down to hundredths of ppm. These were Inés's batch K_d sorption experiments, and they can be related to the retardation factor using the retardation factor equation.

Now, these are the first results from the UFA, again, using a selenite solution that had a concentration of selenium, 1.31, very close to the 1.1 ppm of the first batch experiments. So, what we did was collect and analyze the effluent solution over the course of a three-week period, and then we stopped the experiment. Unfortunately, we stopped it a little too early to get total breakthrough through zeolitized non-welded tuff, but the behavior is pretty well the same as the vitric member.

What this gives is breakthrough curves for these two materials that give about 2.5 K_d, about the same--I'm sorry--a retardation factor of 2.5, and then they give the respective K_ds; again, low, less than one, but slightly higher than the 1.1 ppm with the batch test, but comparing these type of completely different methods, that's pretty good comparison.

As we speak, we're doing the next lower concentration, the .5 ppm concentration, and, again, we'll continue to step this down to see if the retardation behavior matches K_d behavior over the entire range of selenium concentration, and my feeling is that it will, and--although that's very premature, but, actually, I think of those three transport parameters, hydraulic conductivity and diffusion coefficient are highly dependent on the volumetric water content or the degree of saturation, but the retardation factor is going to turn out to be much less dependent, and only--perhaps if you get very highly unsaturated conditions will the mechanism of sorption change, but pretty much if you have free water films, you're going to have pretty much the same mechanism of sorption.

I think I should stop there. Oh, I'm sorry, let me throw the summary slide up. Available information suggests that batch sorption data might be used to predict transport under unsaturated conditions, and that would be very nice, because you could then be able to use all of the K_d sorption data you have collected in the past, and not have to re-do everything under column conditions.

We are continuing the selenium sorption experiments. We will also do the uranium this year. We're looking at both the Topopah Spring, the samples from the same boreholes that Inés talked about, and also the Calico Hills, and the UFA can actually be used as a conservative colloid instrument insofar as colloids will move through the material, either fracture or matrix. And you can select which, under 1,000 g, if they will not move through the material under 1,000 g, then they certainly will not move through the material under 1 g.

And, also, because you can do UFA experiments fairly fast, you know, we can run a couple of week experiments, and you haven't lost a couple of years, so it's actually a reasonable thing to do.

Why don't I stop there.

DR. LANGMUIR: Thank you, Jim.

Questions from the Board, Board consultants?

DR. DOMENICO: To both talks, did you use crushed samples in this? Was the tuff crushed in the batch test?

DR. TRIAY: Crushed tuff in the batch test?

DR. DOMENICO: Yes.

DR. TRIAY: Not in this.

DR. DOMENICO: In the batch tests, they were crushed, but not in this one?

DR. TRIAY: That is correct.

DR. CONCA: In fact, those whole rock core columns, which I don't know if they made it around anywhere.

DR. TRIAY: They made it around. You saw the cores that he's talking about, and those are solid core, like you probably saw, and the intent of this exercise is to see whether, indeed, we're exposing new mineral surfaces during the batch sorption experiments, as opposed to when we actually tried to predict with that batch sorption K_d what would happen in terms of retardation through a solid tuff, or a fractured tuff.

DR. DOMENICO: What temperatures were these run under?

DR. CONCA: These are all at 25°C.

DR. DOMENICO: Thank you.

DR. LANGMUIR: In that same vein--Langmuir, Board--can you increase the temperature of your equipment? Can you look at upped T sorption?

DR. CONCA: Yes.

DR. LANGMUIR: What are your limits?

DR. CONCA: From -20°C to 150°C. Of course, above 100, I don't really want to be dealing with the apparatus, but we have done other tests for other programs up to 90°C. It works very well.

MR. BOWMAN: Rob Bowman, consultant to the Board.

Based on your work to date, do you care to make any comments on what you think the importance of colloid transport is, or do you feel that you're too early in the game yet?

DR. CONCA: Probably too early. My gut feeling is that it probably will not be as important as one might think, first off, because the colloid filtration will probably be very good, but you really need to do very careful experiments in which you are not producing colloids after the experiment, which is actually kind of difficult to control.

MR. APTED: A question to Inés. So, this has been done now in selenium. Are there plans to do sort of this cross-comparison between the batch test in the UFA for other, like uranium or--

DR. TRIAY: You just saw the last summary here, summary slide.

DR. CONCA: Right, uranium, definitely.

DR. TRIAY: All of that is being planned, selenium and uranium. Now, remember that Jim cannot deal with radionuclides, so it's selenium and uranium as a function of tuff, which means devitrified and zeolitic tuff, exactly what he had in that summary view graph. Remember that we validate our sorption K_ds for radionuclides at Los Alamos using saturated fractures and saturated column experiments, just like these ones, but under saturated conditions.

Essentially, what we're trying to say is, can we validate the chemistry, the chemical reaction between that radionuclide in the mineral phase? The hydrology is up to the hydrologists to determine the flow model of the mountain.

MR. APTED: Hypothetical question: If the uranium data is very different, that comes out of the UFA, for example, than the batch test--and I don't know what that would be, very different. I mean, these were different by a factor of four, ten, or whatever, would there be, then, consideration of setting up something like this in a place to do radioactive, to look at plutonium and--

DR. CONCA: Let me break in. We actually have built a

UFA and it's in a hot cell at PNL. It's for low-level waste work, but it is possible to do that.

DR. LANGMUIR: A related question: How comfortable are you getting now with the assumption that your saturated K_ds apply to unsat conditions; that you can take them from one kind of situation into another, since the literature is just full of saturated K_ds.

DR. TRIAY: Right. I mean, we've talked about this before, and that's essentially what Jim was trying to address. As long as the water chemistry is not changing from one place to the other, then you're going to be able to extrapolate them.

Now, let's assume that the water chemistry changes, if it changes in a way, like Ardyth was showing this morning, where we can bound our sorption data using the saturated sorption data, the batch sorption data that we already have, then there's still no problem, because we would be able to say, okay, in p#1 it's this, in J-13 it's this. I have already told you that we need to select certain cases where we actually collect sorption as a function of several parameters to be able to determine whether, empirically, what we're obtaining is correct.

If that is the case, and we actually know what is the difference between sorption when one concentration of something is high versus the concentration of that same thing, lower or higher under unsaturated conditions, again, it's a chemical reaction that is perfectly predictable.

DR. CANCO: And the natural variation within these systems, you have to use different samples, and although they're the same tuff and the same unit, there are natural variations even within what looks like to be a homogeneous unit, and things, you know, are the results, the UFA results, you know, .7, .9 versus .3, .20, you know, how important is that variation? Here are two samples identical that were run identically, they're a factor of two off, and so you have to decide what is the same behavior and just natural variations between different materials versus different methods.

DR. LANGMUIR: Just one last thought. When you're doing your kinds of work in the cores, you're in a rock-dominated system, and if the experiment runs long enough you're going to get to saturation with the rock phase in the matrix.

DR. CANCO: Well, actually, not in--

DR. LANGMUIR: Not in the short times?

DR. CANCO: Well, I'm sorry. You're actually imposing steady state at some unsaturated state, and you never will saturate the system.

DR. LANGMUIR: With respect to the mineral phases, thermodynamically is what I'm talking about.

DR. CANCO: Oh, I'm sorry. Yes.

DR. LANGMUIR: I mean, whereas, in the batch tests that you've run in the past, the literature's just full of them, I would imagine that most of them are not in equilibrium with the minerals in the batch test, so there's a different kind of chemistry there than in these studies, where the pores are in intimate contact with the fluids, and you reach equilibrium thermodynamically, presumably with the fluids going through.

DR. CANCO: Well, thinking of the residence time of the fluid in the UFA runs, if the volumetric water content is, say, 10 mls and you're flowing at .1 ml/hr, then your residence time is about 100 hours, so that's, you know, about the, you know--well, it's sort of on the order of a couple of days of run time batch test, so it may be the same.

In order to change the residence time of the fluid in the flow experiments, you have to back the flow rate down, and so, you can adjust that to some degree.

DR. TRIAY: And, again, we're not saying that if we see differences we're going to ignore them. What we're saying is that if we don't see differences, then we're done. We have proven our point. Now, if we see differences, that would be one of the things that we would consider to explain those differences.

DR. CANCO: And, actually, I would expect for a lot of the tuffs not to see much differences, but perhaps the ones that are calcite-rich, there is some specific phase which, in a column experiment, isn't seeing the solution as in a K_d batch experiment. Then that's where you'll see differences.

And tuffs, you know, it may work out very nicely for tuffs. For things like bentonite and something else, you might have very different results.

DR. TRIAY: And if the differences can be explained, that's not a problem. You still can use the database. It's when you cannot explain the differences that now, you're in trouble.

DR. LANGMUIR: Thank you. Thank you, Jim.

We must go on. The next presentation is a new face, Inés Triay. The topic is colloid formation, stability, and transport.

DR. TRIAY: And in this talk, you do have to stop me, because Schön is going to also speak, so I don't want to take too much time.

DR. LANGMUIR: Maybe you should tell me ahead how much time she's going to get.

DR. TRIAY: She's going to get seven minutes with that chunk. That's what she wants, seven minutes.

DR. LANGMUIR: Okay.

DR. TRIAY: We're going to get through all of this one way or the other, so we're going to go in parallel here.

Let's see. What I was asked to do in this talk is try to put things in perspective based on existing information from the literature from anywhere that we could get. So, on the right here, I have shown you the colloid processes that will be discussed, and on your left, we have the mathematical description of the colloid processes.

Essentially, what we have here are production rates, as well as disappearance rates, and they're based on aggregation or disaggl as erosion in the case of production rates, erosion, you know, was fragmentation and then resuspension of those particles, and in the case of disappearance rates, we have here the attachment rate, attachment to the matrix itself or the fracture, the walls of the fractures.

Now, colloid generation. The primary generation of colloids from erosion is given by a parental power law. This means that the differential of the colloid concentration with respect to size now is given by this power law. A and B are constants.

Now, just to put things, again, in perspective, here we have all the colloid distributions, with respect to size, for Alpine waters. These data were fitted for the crystalline reference water in these Alpine waters, which has a particular water chemistry, and what we want to do here today is not concentrate on the details of this information, because this information is, of course, site specific, but, rather, the idea of obtaining colloid information.

Now, with the crystalline reference water, essentially, what you have here is the A factor being 10.5, the b factor being 3, which leads us to, then, the conclusion that in these particular waters, we have less than or equal to 100 ng/ml concentration for sizes in the order of 10 to 1,000. You're going to see later in the presentation that we actually calculated or measured--not calculated, measured--this quantity in J-13 that has been measured over the years, and certainly, lately.

In our case, we're talking about 30 nm/ml, or a particulate loading of 10^6 ppm, and you have that in your view graphs. That's the first comparison, so that you know more or less where our side stands, versus what we are seeing here on the screen for a study that has already been completed.

This information that I'm referring to here--most of it--and I have told you where the references come from, but in most cases, they come from a very comprehensive report to actually assess the role of colloid transport in the NAGRA project, the Swiss project.

Now, colloid population stability, this equation right here puts in perspective the role of temperature, as well as ionic strength, which is hidden in this factor here called A, which I'm saying is the attachment factor. I will discuss why that is, but it is actually fairly obvious.

Now, let's assume for a minute that we increase the temperature from 298° Kelvin to 373. That causes a decrease in viscosity of the water from .9 to .3. That effect on the half-life of a particular colloidal particle is a decrease of a factor of four. Compare that to increasing the sodium concentration in a solution from 5×10^{-3} to 10^{-2} M, a factor of two. That causes a decrease in the half-life of this colloid of a factor of 25. The actual data is given in this view graph right here.

In this case, essentially, what is being plotted is the colloidal concentration versus time, and it's speeded to obtain these attachment factors. Now, these attachment factors, or the idea that ionic strength increases actually increase the attachment factor and, therefore, decrease the half-life of a particular metastable colloid in suspension come from the DLVO theory. This is a very pedestrian explanation, but we don't have a lot of time. Essentially, what we're saying here is that a double layer develops of thickness, one over kappa, once that the particulate, or the particles in suspension get charged at their surface.

The DLVO theory essentially uses the total potential energy of interaction between two particles, and the actual parameters that go into that total energy is the repulsive parameter, the attraction, as well as the repulsion due to solvent layers.

It has been well-documented in the literature by Grauer in 1990--and that's given right here in this view graph--that the attachment factor is to be measured. It cannot be predicted based on DLVO. However, you can make some statements concerning the effect of increasing ionic strength.

The well-known Schulze-Hardy Rule states that this critical coagulation concentration--and here I'm using coagulation to be the same as agglomeration. If I use either one of the two, that's what I mean by this--essentially is --proportional to the charge of the electrolyte to the power six, so you see that there's a very big effect with changes in ionic strength.

What is interesting about this is that this data here that you see was obtained with montmorillonite colloids, 100 nm in size, as a function of sodium and calcium concentration at a pH of 8. As you can see, or if you remember, this minus log of A would be equivalent to saying stability of colloidal particles in solution, because A was in the denominator in the half-life of a colloid expression that I discussed with you.

So, here, what you see is that the colloidal stability is decreasing as a function of calcium concentration, as well as a function of sodium concentration, but the effect of alkaline earth is more dramatic than the effect of a sodium ion.

Now, let's compare that for a second. Let's also put something else on the screen. That was montmorillonite; right, and we saw that clay particles seem to be the particles that one observes in many instances during nuclear waste issues.

Now, let's consider a hematite suspension. This comes from Liang. And, again, you see stability and we can discuss what this is, but I don't think it's extremely important. Let's just say that it is how stable a colloidal suspension is with respect to a diffusion control aggregation mechanism. You can see that at the point of zero charge is when the colloids are the least stable, and you can see, again, decrease in colloidal stability as a function of ionic strength or electrolyte concentration.

Putting that in perspective, let's consider here what has happened by actually looking at sites, arbitrary sites in, essentially, these Alpine waters that I was telling you about in the Swiss program, as a function of calcium and sodium concentration. You see essentially the same trends.

If you actually, now, look at J-13, 10^6 ppm at a particular sodium and calcium concentration, that actually would fall pretty well here within this chart. This is the subject of a science publication by Degueldre, Triay, and others.

What I'm saying with this is that, presumably, as a first approximation, we can use the stability diagrams that have already been published to determine the amount of colloids that could be found as a function of water chemistry at the potential repository.

All right. Radionuclide sorption onto colloids. Here, what we're saying is that radionuclide, essentially, is associated with the colloidal phase versus true soluble metal in solution. That's what we call the R_p. $_p$ stands for particle, and this gives you some equations of potential mechanisms for that actual sorption. Sorption mechanisms would be no different from the sorption mechanisms that you would normally observe, but the reason I bring this up is because one could calculate, or it has been calculated already in this view graph, SOM K_p, and, essentially, the way this is pronounced is "kah-pay," not "K sub p," which is easy for me, perhaps not for you, but this is for me.

So, essentially, here, I have given you the equations for that radionuclide sorption, as well as the expression for K_p. Bottom line, the observations that have been made in the literature seem to say that the amount of sorption is correlated with these hydroxide compounds, these hydrolized species. So, essentially, what you observe here is that that K_p values increase from something like a clay, because these K_p values are actually calculated only for these active sites in the clay, so this is a very conservative prediction to something like iron oxides and oxyhydroxides, and then the last, or the least sorbing would be something like silica, and they are given here. Again, the details are not extremely important, but know that these values are available.

Now, radionuclide sorption as a function of colloid type. Intent here is to give you an idea of what is it that we're talking about, what order of magnitude, R_p 10^4 for quartz, amorphous silica, 10^6. Notice that the fact that there is more active surface is going to influence your sorption values. So, also notice that these values may be higher than the values that you will predict with a K_d, using particles that have a smaller surface area. So, here, we have montmorillonite and illite.

Now, the question arises, is radionuclide sorption irreversible? There's a lot of evidence in the literature on a short time experiment, time scales, as well as mechanisms that have been somewhat confirmed by field experiments that sorption may be considered to be irreversible, even if it is not truly irreversible in the time scale of the repository. So, let's just leave it at that for now. There's several reasons why sorption would be irreversible onto a colloid. If the sorption is reversible, you won't have a problem, so it doesn't matter. I mean, there's no way that the surface area of a fracture can be compared to the surface of the available colloids at low colloid concentrations.

We're going to skip the next view graph. That's for your information. Those view graphs actually talk about the attachment, the actual attachment of colloids with quartz that were actually coated onto fractures, and you'll see that there is some attachment mechanism that actually would explain retardation due to colloids interacting with the quartz.

Okay. Now, conclusions. Where do we think we are in this? I think that a sensitivity analysis study can definitely be tried now. What would be the assumptions? Colloids generated are clays, silica, and iron oxides.

Negligible amount of organics in the groundwaters. Just so that you know, the organics tend to stabilize the colloids, so this is not a conservative assumption.

However, the literature suggests that if you actually measure TOC in the field versus taking the groundwater to your lab and doing whatever you're going to do, what happens is that you actually obtain TOCs that are orders of magnitude lower at the field that what you observe in your laboratory, due to contamination. So, if we're going to get into this, we absolutely have to do this on line, like this bullet here suggests.

Okay. Stable colloid population. We definitely can base the amount of colloids on the available stability diagrams, as well as the groundwater chemistry and expected temperature. There are many calculations just verifying these stable colloid populations that are already in these stability diagrams with information that we have from the groundwaters.

I propose that we use irreversible sorption of the radionuclides onto the colloids. Look, if you don't have enough colloids at step number three here, this doesn't matter, and now you don't have to get into those type of arguments, because this is, I think, going to be very difficult to sort out at the time scales of a laboratory, or even a field experiment.

Now, I suggest that we calculate K_p values based on either K_d values, or use the K_p values from the literature. Those K_p values seem to be very conservative compared to the K_d values that we already have from our sorption program.

I suggest that we assume no attachments of colloids onto fracture walls, and I suggest that we exclude the colloids from the tuff pores due to size and charge. Actually, I think that that's going to happen.

Okay. Now, what risks we're taking when we make these kind of assumptions, and how can we reduce that risk? You can experimentally determine the type of colloids generated from spent fuel because, apparently, there is some uncertainty concerning the type of colloids that can be generated, and that would be addressing Assumption No. 1.

Study Yucca Mountain as its own analog. We're going to hear about that. That is also to address Assumption No. 1. The TOC in groundwaters I already discussed with you. Stability diagram for silica. This is not actually, believe it or not, as much as silica has been studied, we don't have as good stability diagrams for silica as the ones that I showed you for hematite and clays, so I think that this is something that perhaps could use a little bit of more data, and that, of course, addresses Assumption 3.

Now, colloid population in groundwaters and size distributions using off-line particle counting techniques--I think those are the best techniques--and that would address 3 and 5. This, I think, needs to be done. I think that we should get groundwater chemistry from the site, and as long as we're at it, determine the colloid population and the size distribution. This is actually a relatively small effort once that you actually start looking at the groundwater chemistry, and it's critical, because, remember that I told you that if No. 3 here gets you to conclude that there's no problem, why do any more? So, this is a critical thing that should be reduced. And we have already done that with J-13 water as a function of time, which is extremely important.

Okay. So, experimentally determine selected K_p values. That is only if this is large, large enough to produce a significant release for specific radionuclides. Then you worry about whether those K_p values were too conservative.

And, of course, if you want to determine the degree of colloid attachment, you can use fractured tuff columns. That would address 6, which I assumed there was not going to be any attachment, and you can validate your information at the field scale.

Schön next?

DR. LANGMUIR: Yes, but let me ask you one real quick one here.

DR. TRIAY: Yes; sure.

DR. LANGMUIR: Your assumptions, why don't your assumptions include one where the colloids are competing with the wall rock, which you've acknowledged is an important issue?

DR. TRIAY: No, no.

DR. LANGMUIR: Is it down there?

DR. TRIAY: I am saying as a first step, in the first calculation that I'm proposing, assume that the colloids are not competing for the fracture walls. I've said that here: No attachment of colloids onto fracture walls.

DR. LANGMUIR: Well, that's the colloid on the wall. I'm talking about if you're transporting radionuclides, then the colloid has to compete with the walls as -- of the radionuclides. That's an important issue.

DR. TRIAY: Oh, okay. No, now wait a second. I addressed this, too. I said sorption is irreversible, and that's a big deal. Let me tell you why that is a big deal. There's data already in the literature suggesting that sorption of radionuclides is irreversible. In other words, once a radionuclide absorbs to a colloid, it won't desorb and go back into the fracture wall or whatever.

DR. LANGMUIR: This is true of all radionuclides?

DR. TRIAY: Excuse me?

DR. LANGMUIR: All radionuclides?

DR. TRIAY: No. What I'm saying is, the data that are available, in terms of kinetics, the reason for this is, the people who are doing experiments, in terms of days or weeks, sorption is fast. Desorption is slow, which is something that you, of course, know. That is a result of what they're saying here.

The other problem is, that mechanism that I put there, where I have the metal associated with the colloid, and then another colloid coming in, right? And now you have this thing here that is no longer sorption, is a different mechanism, is encrustation, is a coprecipitation, and Schön is going to talk about that, so I think that this, to actually say, no, I know that the radionuclide is going to see the wall as well is not an easy thing to prove and get data that is actually defensible.

So, I suggest that we concentrate more on this, than on this issue.

DR. LANGMUIR: Maybe that's something for Jim Conca's equipment, push those colloids through and see where the radionuclides end up.

DR. TRIAY: Well, he said that he was going to do that in his summary view graph, but--and, again, this thing right here that I have here, right? He said it, and I'm saying it again.

DR. LANGMUIR: Sorry I hogged the questioning here.

Schön Levy next, talking about natural colloids at Yucca Mountain.

MS. LEVY: We'll attempt a rapid transition now from Inés's work to natural colloids, and I'll do it by giving you a few items of information from uranium exploration literature that's pertinent to this issue.

Work has been done on studies of uranium-bearing silica deposits, and some experimental laboratory work as well. The lab work has found that the concentration of uranium in dry silicate gel products of these experiments over the initial solution concentration shows enrichment factors of about 400 to 1,000 times.

Studies of the natural materials, getting back to what Inés talked about, do seem to indicate that the uranium is immobilized within the opal, which is the silica deposit forming from the colloidal material, and the particular examples studies are all on the order greater than 10 million years old, so this is a long-term immobilization, with a caveat, perhaps.

These particular studies contain no data on simple surface adsorption by the opal deposits, and that the mechanism by which the uranium is incorporated into the deposits is one of coprecipitation with the colloidal silica material. Variations in the uranium content of the deposits do seem to correlate in some cases with differences in silica crystallinity, and I've not been able to determine if there's any information as to when these changes arose, whether this is an original depositional difference, or whether it reflects some kind of difference in post-depositional history.

I'm going to talk about some very restricted examples of colloid transport, using Yucca Mountain as a self analog, natural analog. The evidence we look at consists of deposits of crystallized gels, settled and adsorbed aggregates of colloids, probably polymerized material, and monomers as well. These are deposits that are large enough that they can either be seen in the field, or can be seen at the levels of magnification used in ordinary petrographic studies.

Here's an example of such a deposit that is readily visible in the field. This is a cavity filling from the lower part of the Topopah Spring tuff, and it contains a variety of silica deposits, including this -- silica, and several generations of layered silica.

This image of the same filling shows it doesn't appear as fluorescent under short-wave ultraviolet radiation, and the green fluorescence that you see is the result of the fairly high uranium content of the silica material.

The analog environment that I'm talking about at Yucca Mountain existed during the times that the pyroclastic units were newly-deposited and were still hot. They interacted with infiltrating water to create transient hydrothermal conditions, and probably conditions of localized and transient fluid flow, even though these deposits were within the unsaturated zone, and the pyroclastic rocks themselves were the sources of very abundant colloidal material. Just how abundant was shown by the example I just had up.

The types of colloidal material include silica, which formed deposits of amorphous opal, which is called opal-A, opal-CT, which has short-term, short-range cristobalite and tritimite ordering, and the more crystalline forms, cristobalite and quartz. There are also aluminosilicate materials that crystallized as smectites or montmorillonites and as zeolites. I'm going to concentrate just on the silica materials.

Now, I've used the total path lengths, based on field study, to place upper bounds on the distances that colloidal material could have been transported, but it's obviously not usually possible to trace the material in any colloidal deposit back to some source area within the transport pathway, so there's no real way of saying that colloidal material traversed the entire length of the pathway. That's simply an upper bound.

I'm going to show you two examples. The first is this part of southeastern Yucca Mountain known as Harper Valley, and the affected units are the middle to lower Tiva Canyon tuff and the underlying Paintbrush bedded tuffs.

The pathway exists from the densely welded devitrified Tiva columnar joints downward into moderately welded, mostly vitric tuff, with fewer fractures, but still, mostly a vertical orientation, and some of these appear to be either continuous or communicating with sparser fractures in the non-welded bedded tuffs. This is a pathway that contains considerable silica deposition along the entire path, with very large amounts even at the bottom of the system, where the fractures die out. The path length is about 15 meters.

An example from northeast Yucca Mountain is within the Topopah Spring tuff itself, and this fracture system extends from the top of the Topopah moderately welded tuffs down through the densely welded tuff, approximate potential host rock here, and terminates at the transition between the devitrified tuff and the basal vitrophyre.

This is a zone of very abundant devitrification cavities and gas cavities, and the vertical fracture system, as I said, terminates here, but it is in communication with another fracture system that connects the gas cavities and devitrification cavities that exist within the upper couple of meters of the vitrophyre, and this is the location where the silica-filled cavity example that I showed you came from. The path length here is on the order of 90 meters.

The geochemical processes limiting transport of the silica material within these pathways includes agglomeration, adsorption of the material onto fracture surfaces, crystallization, settling, and evaporation. In addition to this, the continuity and the connectivity of the fractures themselves also plays a role in both of the examples.

The bottom portion, sort of the end of the line portions of these transport pathways still contain abundant colloidal material in the deposits that can be seen, so that if the fracture pathways had extended further downward, there was plenty of material in the local vicinity to have traveled further down so that the chemical processes were not totally effective in immobilizing the colloidal material.

These examples raise some questions which are being addressed, in part, by further natural analog studies, and by some of the experimental work that Inés was proposing. One is: "What will be the condition of colloidal silica in the repository when canister leakage occurs?"

If the mechanism by which actinides are incorporated or adsorbed onto colloidal silica is one of coprecipitation, then the silica will have a much reduced effect if it's already precipitated before any of the radionuclide materials arrive on the scene, so there is a question as to during what portion of the lifetime of the repository hydrothermal regime will colloidal silica be present, and in what condition will it be? Will it still be dispersed in a fluid phase, or will it be settled out, and less able to act as an absorbent for radionuclides.

And the second question is: Will adsorbed actinides be remobilized if silica recrystallization occurs? As I said earlier, it seems as if the adsorption of the actinides onto the silica appears to be irreversible. However, if the silica is in one of its less crystalline forms, such as opal-A or opal-CT, and perhaps were subject to redissolution and reprecipitation, there's certainly a possibility that any adsorbed actinides could be released at that point and could reenter the hydrologic system, or could behave in some other way that we really can't predict yet.

DR. LANGMUIR: Thank you, Schön.

We're behind schedule, as we usually have been today. We need to proceed and hold our questions until later in the afternoon.

The next presentation is pneumatic transport of long-lived radionuclides. Arend Meijer will present the talk.

DR. MEIJER: Well, the original title was supposed to be pneumatic transport, but you've heard a whole series of transport talks already today, so I figured, at this point in the day, it might be worth talking a little more about the retardation potential of gas phase radionuclides, so that's what I'm going to concentrate on.

First, I'm going to talk a little about the gas-phase source term, talk a little about the gas flow models in the unsaturated zone, and some of the data that's available, some of which you've already heard earlier today; talk about retardation mechanisms in the unsaturated zone for gas-phase radionuclides, and then data and modeling needs to clear up loose ends, and then, finally, some conclusions.

Okay. So, first, we'll talk a little about the source term. I should start out by saying that in the back of your handouts, I've included Xeroxes of the first sheet of, I think, four different talks on Carbon-14 and gas-phase transport that have been given either to this Board or to, I think, a National Academy of Sciences Board, that talk specifically about the source term, so I'm sort of summarizing what was in those talks.

All right, and a lot of that--or most of it, in fact, is by Rich Van Konynenburg, who gave a presentation earlier--or, actually, will give a presentation later today, and Park, who gave one to the National Academy of Sciences Board, so I refer you to those, and I have copies, if you like.

The radionuclides of greatest concern here are 14_{C}, 129_{I}, 99_{Tc}, and 79_{Se}. In the unsaturated zone in the gas phase, these radionuclides would occur as carbon dioxide, elemental iodine, Tc_2O_7, and then, finally, SeO_2 gas. Selenium behaves a lot like sulfur, so you can sort of think of SeO_2 and SO_2, the same chemistry, essentially.

Out of this group, or actually, out of this group of radionuclides, Rich and Park have pretty much--well, have shown effectively that Carbon-14 really is the only gas-phase radionuclide of concern in the unsaturated zone in Yucca Mountain.

Out of the inventory of Carbon-14 in the proposed repository, or potential repository, something on the order of 2 per cent is available in a quick release fraction; that is, once a waste package breaks open, this initial pulse of

Carbon-14 is available to the unsaturated zone immediately, and apparently, if one waste package breaks open per year, the release of the Carbon-14 in that waste package exceeds substantially complete confinement, some term DOE has used.

The total Carbon-14 inventory in the repository is something on the order of 10^3 moles. That's an important number, because we'll compare that number to the sizes of other reservoirs in the unsaturated zone in Yucca Mountain, either man-made or natural.

Now, I'll talk a little about some models that are available for gas flow in the unsaturated zone at Yucca Mountain. I think you heard this morning about a model that Ben Ross has put together. That's now included in the Total System Performance Assessment, and that, basically, is a buoyancy-driven advection model. I'll show you a picture of some of the diagrams that reflect Ben's calculations.

In his calculations, the Carbon-14 is transported to the surface in the range of 500 to 2,000 years, depending on when the Carbon-14's released from the waste packages, the individual waste packages, as well as the retardation in the unsaturated zone that I'm going to talk about.

Okay. So, it was used in the Total Performance Assessment, 1993, and the releases that were predicted exceed the EPA regulations, and you heard about that this morning.

Another model which hasn't been applied to performance assessment, but happens to be included in the two-volume set that someone referred to this morning--I think Rally referred to that this morning, the two-volume TSPA-93 set. In the second volume, in the back of the volume, there is a section on other calculations, and in that section, there is a model for barometric pumping, by Nilson, et al., and in that particular model, the transport times are shorter, but there are a number of things that are not included. Temperature effects aren't included, and a number of other things aren't included.

It's significant here because I think it can be used to explain the data that we have on the natural system Carbon-14 right now. That's sort of a puzzle at the moment.

As was stated earlier, the hot repository likely will increase the transport rate of Carbon-14 to surface, and the question is, how much, and can we do anything about that?

Then let's look at some of the diagrams that Ben Ross and his people have put together, based on their calculations. The details of these diagrams aren't really too important. He has diagrams for all various kinds of conditions, permeabilities, various permeability contrasts, and different temperatures, et cetera.

This middle one happens to be for ambient temperature, with no permeability contrast between the lower unit and this Paintbrush Tuff nonwelded unit. The main point to get out of this is that there's basically a buoyant flow with cold CO_2 or cold air coming in on the flanks of the mountain, and being transported through the mountain, then coming out at the surface, or the top of the mountain. It's basically a chimney effect. All right, and this, then, has been applied in the TSPA-93 model.

With respect to the natural data, as we'll see in a little bit, this model would predict that ages of Carbon-14 should decrease as you go down the mountain. In other words, if I drilled a hole on top of the mountain and went down, and measured C-14 concentrations at different levels, they should be higher down here than they are up here because of the flow line. Young C-14 comes in here, goes all the way through this path, so it should be older when it's, or less C-14 up here than down here. Keep that in mind.

This, then, is the other model that I mentioned that was in the second volume of this two-volume set. In this particular model, what we've got is barometric pumping; that is, the changes in barometric pressure that you get on a five, ten-day cycle cause gas to be either forced into the mountain through fractures and pores, or pulled out of the mountain, depending on whether you've got a high pressure crossing the mountain, or a low pressure cell crossing the mountain.

If you have a source term above the mountain, and you want to--well, you have a source term, for instance, Carbon-14 in the atmosphere, a high pressure cell will slowly ratchet that Carbon-14 down into the mountain. As this high pressure cell pushes the Carbon-14 into the mountain, it diffuses into the matrix, low pressure cell comes by, stuff comes back out, not out of the matrix. The next high pressure cell comes through, forces it even further down into the mountain. So, little by little, you ratchet this stuff down, but at the same time, the Carbon-14 is decaying as you go further and further down into the mountain.

So, let's look at some of the natural data. The natural data that I'm talking about consists mainly of isotopic data that's been largely collected by the USGS, and Al Yang and his group.

The natural data suggests that there are basically two separate flow regimes, or at least two separate flow units, if you like. I probably could use a better term for that, but in any case, there is a shallow regime, less than about 50 meters, in which you seem to find very modern tritium, Carbon-14, and then there is a deeper regime in which you find, basically, a linear decrease in Carbon-14 with increasing depth, suggesting that you're actually moving things down into the mountain and having them age as they go down.

Another interesting item that will become important a little later on is that the Carbon-14 ages--actually, you heard this this morning--the Carbon-14 ages, if you want to call them that, versus the Chlorine-36 ages are quite different. Carbon-14 ages are something in the range of one to 10,000 years, whereas the Chlorine-36 ages, 50 to 750--actually, that would be 500,000 to 7.5 million units. Whatever. I mean, they're quite different.

All right. This, then, is a diagram out of a paper by Al Yang, in which he's plotted some tritium data down here at the bottom, in tritium units. This data's for UZ-4, this data's for UZ-5. The point to pick up in these diagrams is that at levels down to about 50 meters, in the case of UZ-4, you find tritium values that are well above zero values; that is, you find modern tritium, or a modern gas, if you like.

The geologic units are shown on the left in here, and, in this case, the tritium values are high above the bedded tuff that's just below the Yucca Mountain member, and above the Pah Canyon member.

In the case of UZ-5, again, 40 meters, 50 meters, you get high tritium values in or above the Yucca Mountain member and, in this case, well above the Pah Canyon member. The other data points that are on this diagram are some Carbon-14 ages, down here in this bedded tuff, Carbon-14 age of 1,000 years in UZ-4, a Carbon-14 age of essentially 5,000 years in UZ-5.

One more natural data set here. This is a different well, again, a UZ well. This is UZ-1, and for those of you that don't know where these wells are located, I put a diagram--I think it's the last diagram in your package that shows where these things are, so you can take a look at that. UZ-1 is at the north end of the repository block. I think it's in Drillhole Wash, but, in any case, it's on the diagram.

What we're plotting here, or what Al has plotted here--this, again, is Al Yang in 1992--is per cent modern Carbon-14, which can be converted into an age, if you like, but per cent modern Carbon-14 going from 100 or greater than 100, depending on when the stuff actually got into the ground, down to 20 per cent. Twenty per cent relates to something on the order of 8-10,000 years, something like that, so we're going, basically, from zero years to, let's say, 10,000 years here, and we're going from zero meters down to a depth of on the order of 350 meters, and there's almost a linear decrease in Carbon-14 from about 80 meters, perhaps

70 meters, down to 350 meters.

This can't be explained by the model that Ben Ross has. He would explain the opposite. He would explain this diagram, or this diagram should go this way, or this data set should go this way according to Ben Ross's model.

I suggest that perhaps one way to look at this is through this barometric pumping, in which you're basically pushing this stuff into the ground, so the deeper you go in terms of a drillhole, the older the Carbon-14 is going to be, because the longer its residence time has been in the mountain.

That doesn't mean that Ben's model isn't appropriate for doing performance assessment calculations, because the present mountain doesn't have a big thermal source in there, and thermal source is going to drive this buoyant flow that Ben's talking about, so both models are appropriate. It's just that the barometric pumping model probably explains this data a whole lot better, and, by using this relationship here, this observed relationship, in a model, you can probably bound gas flow rates within the mountain, because you have to be able to explain why these things age to 8,000, 10,000 years down at 350 meters, and so you can develop a gas flow model that would constrain, or, actually, this data would constrain that gas flow model.

Okay. So that's about it for the models. I'm now going to talk about retardation mechanisms. Possible retardation mechanisms for C-14. What is it in the mountain that might retard the flow rate to the surface of C-14?

First of all, there's a potential for low redox potential in the--there might be a low redox potential in the engineered barrier after the repository's closed. We'll talk about that.

Sorption onto metal components or oxides in the engineered barrier, another possible retardation mechanism. Coprecipitation with dead calcium carbonate in concrete or cement that's put into the repository. Coprecipitation during dryout phase calcite precipitation, so the heat in the repository drives off the water, the salts in the water crystallize out as calcium carbonate and whatever else, and if it's calcium carbonate, then, presumably, you can coprecipitate calcium-14 with it.

Isotope exchange with bicarbonate in the pore water, isotope exchange with calcium, dead calcium, or calcium-12 in the cement or concrete, and then, finally, isotope exchange with calcium carbon-12 in the dryout zone. So, I'll go through each one of those.

Okay. First, we'll talk a little about this low redox potential in the engineered barrier. At the moment, as far as we know, the gas phase in the unsaturated zone at the repository level has an atmospheric oxygen content. In fact, all the gases, except CO_2, are basically there in atmospheric proportions, suggesting strongly that that zone is communicating with the atmosphere fairly regularly, on a fairly short time scale, the point being that you start out with oxygen there.

Secondly, radiolysis after the emplacement of the waste, radiolysis tends to produce various oxidizing agents, or oxidation agents. I don't want to go into the details of this. I would urge you to look at the material that Rich Van Konynenburg has presented, and others as well, the point being that oxidizing conditions are likely.

The Germans found that even a very small amount of oxygen gas available in a waste container, or at least in contact with fuel, tends to form carbon dioxide, or tends to oxidize the carbon so you end up with CO_2. It doesn't take much O_2 to produce the CO_2.

The bottom line, then, is that it's my opinion that it's pretty unlikely that we're going to have sufficiently reducing conditions in the repository to keep carbon in the elemental phase, all right, so that initial release fraction is going to be CO_2 no matter what you do, but there is also carbon left in the fuel that hasn't yet migrated out of the fuel, through grain boundaries and such, and that stuff will come off depending on what the oxidation rate of the fuel pellets and then other things in there are, so you're going to have a constant source term of Carbon-14 coming out of there, but that initial release fraction will be carbon dioxide, regardless, so we have to deal with that. So, I tend to discount redox potential as a way to get around that problem.

Okay. Then I mention the potential for sorption of both CO_2, or, if there's a water phase there, carbonate or bicarbonate onto iron oxides. There is some data available that suggests that--well, that gives some numbers as to what the potential is for this adsorption behavior, so I did a rough calculation to see how much CO_2 you could get onto this iron oxide.

From Rich, I got some mention of these MPCs, or the waste package containers, and out of that, I came up with something on the order of 5 m^3 of carbon steel used per waste package, and I just came up with a number of 2,000 waste packages. Maybe somebody out there has got a better number than that, but we're just trying to rough this out.

So, we end up with something on the order of 10^4 cubic meters of carbon steel. Then we allow that all to oxidize, and we say, well, not only do we allow it to oxidize, we force it to have a surface area of something on the order of $10m^2/g$, which is not crazy. I mean, there are plenty of iron oxide preparations that have 600 m^2/g. I haven't actually seen numbers on surface areas of oxidized metals, but maybe somebody out here knows what those numbers are. Pick up a round number of $10m^2/g$.

Then, finally, according to a recent paper by van Geen, et al., in Geochemica Et Cosmochemica Acta, the amount of CO_2 or bicarbonate--in their case it was carbonate, bicarbonate--that you can get onto the surface in terms of adsorption, something on the order of 3.8×10^{-6} M/m^2, so you just do the math there, and you come out with a total reservoir on the iron oxide of something on the order of 3×10^6 M of CO_2.

The total inventory is something on the order of 10^3 M, so you've got half of this amount in the inventory. You also have a very large compliment of dead CO_2 in this repository, which is going to compete with C-14 for the surface sites on that iron oxide. So, my guess is that this is a potential reservoir, but it's certainly not going to be a major reservoir for adsorption or retardation of Carbon14.

Okay, the next one is coprecipitation, and by coprecipitation, basically what I mean is we put carbonate in solution, there's a water phase. That solution already has bicarbonate in it. The C-14 goes in and exchanges with the dead carbonate that's already there. You heat the system up. The calcite precipitates out because it has a retrograde solubility. The calcite precipitates out, and not only Carbon-12, but also the Carbon-14, so you end up basically silting out or dropping out the Carbon-14.

This is also happening in the concrete that's emplaced in the repository; that is, the concrete in the repository will have calcium carbonate or calcite form within it right after it's poured, so that represents a potential reservoir for exchange of Carbon-14.

DR. LANGMUIR: One minute, Arend.

DR. MEIJER: Okay. The problem with that is that this repository's going to be open for 100 years, and carbonation of concrete occurs within a period of 20 to 50 years, so by the time this thing gets closed, all the carbonate that's likely to form in that cement will have formed, so we're out of luck on that one.

Then, secondly, there is coprecipitation with dead carbonate during the dryout phase, where the water is driven off near the repository, and you might include some precipitation of Carbon-14 there. The problem is that then you have to time the release of the Carbon-14 with the dryout, because if the Carbon-14 is released after the dryout, you're not going to coprecipitate it with the dead carbon.

All right. This is actually probably the most important diagram. As I said earlier, the total C-14 inventory in the repository is something like 1.3×10^3 moles. The reservoir of carbon in the unsaturated zone water above the repository is on the order of 10^9 moles. Okay, now we're talking some real numbers, and we can exchange the Carbon-14 in the repository as it comes up with this carbon that's already in the mountain, the dead carbon. This is actually what's been done in the Total Performance Assessment '93, and the retardation factors are 40 to 180.

We can kick these up some as a result of increased weathering rates that will occur as the water is raised in temperature, and I can't go into the details of the weathering rates. Basically, the feldspar weathers so that you end up with sodium coming out of the feldspars, hydrogen ions going into the feldspars, and you kick up the carbonate content in solution. So, we might be able to kick this up by a factor of two, I would think.

We'll forget this one, and then, finally, the Carbon-12 reservoir that's present in the concrete after closure is available for surface exchange; that is, Carbon-14 can be exchanged with the Carbon-12 that's on the surface layers of this carbonate. If that carbonate is very fine-grained, which it appears to be in most concretes, then you have a substantial surface area with which to exchange Carbon-14.

And I have one diagram in here about the exchange kinetics, and I'll just put it up. You can take a look at it. The bottom line is that the outer two or three atomic layers of the calcite are available for exchange, and so that provides another reservoir. I've given you a number for that reservoir in the last slide.

All right, so let me then sum up. Data and modeling needs. It would be good to get an idea of how much Carbon-14 you can get into this concrete, and into carbonates that might form in the dryout zone, so some experiments can be done with this, and they shouldn't take forever to do. There's some data already available to back that up.

I won't talk about this one. I will talk about this one. I think it would be very useful to improve the gas flow models that we have, or expand them, and try and explain the natural system Carbon-14 that's in the mountain right now, and that will give us bounds and a number of flow parameters, so I think that's definitely worth doing.

And, finally, a couple of conclusion slides. Clearly, the aqueous phase is the largest potential CO_2

exchange reservoir in the mountain. That's been included in TSPA-93. It could be enhanced, probably by a factor of two to four, I would imagine.

The dryout associated with a hot repository will tend to decrease that aqueous reservoir, and I originally thought it was going to decrease it substantially, but now, according to Bob Andrews, apparently, it's not going to dry it out that far, so that one remains to be determined, but the further this dryout zone goes, the less we have to exchange the Carbon-14 with.

And then, finally, the carbonate exchange on the surfaces of the carbonated minerals, or the C-14 exchange will be another reservoir which is uncertain at the moment, but could be evaluated experimentally.

I've already said that. Basically, I've said that. Experimental data needed to evaluate these alternate reservoirs, and then we need some additional modeling to improve that correspondence. I've said that.

That's it.

DR. LANGMUIR: Thank you, Arend.

I think we need to take our break, and, again, hold question until the end of the day. Let's try and hold the break to about seven minutes, and return promptly at four-fifteen.

(Whereupon, a break was taken.)

DR. LANGMUIR: Our next talk is titled, "Potential Effects of Engineered Barriers on Radionuclide Migration." The speaker is Rich Van Konynenburg.

MR. VAN KONYNENBURG: Okay, that's the topic, and here are the things I'd like to touch upon in the next few minutes:

First of all, what barriers am I talking about, what are they made of, how do we think they're going to behave in terms of oxidation and corrosion, what will result from that, what radionuclides do we care about, and then what will be the effects on radionuclide migration, and then some conclusions.

I'm lumping together here things that are traditionally thought of as part of the engineered barrier system that we're trying to use for containment, along with some other things. First of all, under metal barriers, I'm including multipurpose canisters and glass pour canisters, even though we're not taking credit for those in terms of substantially complete containment.

We're also looking at ceramic. I've got fillers here, even though we're currently not planning to use them; just for talking purposes I've included them here. Packing material outside the packages, we haven't made up our minds on that in the project. Backfill is not currently planned to be used, but I'll talk about it anyway, and then the concrete and shotcrete parts that are really to support the packages and perhaps the drifts.

Okay, now you may not have seen this list of candidate materials because it's been changed some in the not too distant past. We have here four categories. The first is the so-called highly corrosion-resistant, then moderately, then what we call corrosion-allowance, and then finally I've put the austenitics down here for the glass pour canisters and the multipurpose canisters.

You'll see I've got big Xs and little Xs, and rather than dwell on the details of the composition, I just want you to note where the big Xs are, and in particular, notice the iron and the manganese because those oxides tend to be good sorbents, and that's what we're going to be talking about later on.

And also notice the corrosion-allowance materials, which are basically iron, and they're going to be in a thicker section so we'll have a lot more of those products. So the main thing to get from this viewgraph is that we expect a lot of iron oxide now with the more robust package and using carbon steel based material on the outside.

And then some of these other materials, these are the two ceramics that we're looking at as alternates, TiO2 and aluminum oxide. There has been some look at fillers. As I said, we're not planning to use those right now, but the M&O has looked for example at iron and steel shot and has considered these other possibilities.

For packing materials, these have been mentioned in the past and as I said, we haven't got a conclusion on whether that's going to be used or not. Backfill, these sorts of things have been talked about. Again, we're not planning to use those right now.

And then, finally, for these other parts, we're really dealing with concrete and steel, perhaps some metal fibers, rebar, pretty conventional stuff. It may be that some of this material has to be more corrosion resistant to last this longer period of time, 100 years that's being talked about now.

Now, the type of scenario we're looking at, as has been discussed before today, the primary design case now is pretty high waste loading. With that loading, we expect to see water driven out and things will be dry, above the boiling point for a long time. Under these conditions, we can't have wet aqueous corrosion going on, and so initially we'll have dry oxidation of the metal barriers.

Later on after cooling and return of the water, which could be quite a long time depending on things, then we could have aqueous corrosion, and so you should expect to have first oxidation products, then corrosion products, and there could be some alteration of the oxidation products when the water comes in. So it's not a simple thing, and it's going to depend on timing, and all those things aren't quantitatively known yet.

So what I've collected here, and this is based on literature, I didn't do any of this research myself, this is rust. All right? We're all familiar with rust, and it may seem very simple when you look at it, but when you look at it in detail, there are a lot of species there. And it essentially proceeds from the top more or less down, hematite being the thermonamically stable phase, and the ferrous being the lowest oxidation stage.

Above that, it's just ferrite, iron, metal, alpha iron. And you can see there are a lot of species in between, and all of these have been observed in one type of corrosion environment or another. A lot of it's by Mössbauer spectroscopy, some of it's by infrared, x-ray defraction for the things that are actually crystalline, and so on. So there's quite a variety.

And then for the Monel and copper-nickel, these sorts of things have been observed, and notice I've got a carbonate phase here, malachite. That's the beautiful mineral you see in the nature store or whatever. And then for the nickel alloys and the higher corrosion, you can see the species there. And again, these more corrosion-resistant metals, it's going to take a while for those things to form, whereas the iron we would expect corrosion to occur more rapidly. So it would be there sooner and it would be in larger quantities.

Now, we've seen lists of radionuclides and so on today already. I think this is--I've just sort of arbitrarily taken those with half lives larger than 1,000 years and have inventoried, their significant in comparison to the old 40 CFR 191 limits, even though they don't apply anymore, and also looked at solubility and sorption data from Los Alamos and came up with this list.

And you'll notice here that I've got nickel, but other than that, I think you've seen the rest of these on the list of the ones that are being looked at most carefully. And I've also got speciation here, again based on literature and advice from Los Alamos. And you'll see primarily anions here, and that of course is behind the problem of why they don't sorb very well. So those are the things that we were concerned about.

Now, what effects could there be based on these materials? I've come up with ten from thinking about it, talking to people and so on. There may be more that I haven't thought of, and if you've got some ideas, I'd like to hear about it too, but these are the ones that I have come up with and I'd like to talk about each one of these in order.

Now, this one is perhaps obvious, but I thought I would bring it up because there might be some conflict between this one and some of the others. The primary purpose of the engineered barriers is to prevent the migration of radionuclides by containing them completely. That's been the whole idea since 10 CFR 60 has laid down a regulation in that regard. And the trend in recent years, in no small part because of the efforts of this Board, has been toward more robust packages and longer intended containment lifetimes. That's the trend we've been looking at in the last few years.

Now, attempts to use these barriers to do other things, such as preventing collapse of the drifts, which was the reason for backfilling that was given in TSPA study, restricting water ingress, which is I think something that's going to be discussed by my colleague here, or retarding radionuclide transport, which I think was raised by Don Langmuir, those goals, while laudable, have to be balanced against the containment goal for overall system performance. And that big picture I think has to be kept in mind, because some of those can conflict.

Now, how could that happen? Well, one of the most important ones I think is in the temperature. Use of packing and/or backfill is like a blanket. These large packages we're dealing with now have quite a heat output, something in the order of 15 kilowatts for each one. If you cover them up, they don't transfer the heat the way they would if you let them convect and radiate to the walls of the drifts. So the temperature goes up, and we saw this morning in Rally's talk, it reaches incandescent, over 500 C. That's how we're heating the packing. So it's an important issue.

Now, there are some advantages. Under unsaturated conditions, the higher package temperatures would produce lower relative humidities at the package surfaces, which would delay the onset of the aqueous corrosion, the wet corrosion. That's an advantage.

The disadvantage, of course, we get these temperatures which raise the oxidation rates on carbon steel, earlier failure by oxidation, and then this is the other problem. If you're not going to put the backfill in early, when are you going to put it in? If you try to put it in later, it's not fun to work down there, and we've had discussions about ventilating vigorously in order to cool off the drifts. All of that has to be coordinated. So the backfill thing I think is really not an easy issue to decide. It has some advantages, but it's very difficult from other directions.

Another thing is to keep the water out. Backfill could produce a capillary barrier. I had Jim down for this, but it looks like you're on the program. Okay, well these two fellows are going to cover that one.

And we've already heard about this; the use of a lot of iron or something that has ferrous iron in it like magnetite could hold down the oxidation potential for a long time. The advantage of that is that we could extend containment lifetime, get protection for the package from this buffering, and also several of the radionuclides are less soluble or more strongly sorbed in lower oxidation states. And, in particular technetium, this may be about the only thing we can do for technetium. If it gets out of containment, reducing conditions perhaps are the only way we're going to stop it. So that's an important one for technetium.

But then the question is how long can we rely on that? And there's a lot of oxygen available, and to answer that question I think is going to take some detailed calculations with a real design, knowing how much material you've got there and what kind of gas flow you could have and how tightly packed some of these materials will be, producing certain permeabilities. That's going to have to be looked at to see will this redox buffering still be around when we need it because of those long half lives that I showed earlier.

Similarly, we could get pH buffering. Particularly we've heard about concrete a lot, and that could reduce the solubility of nickel and the actinides. We do have to be concerned, though, that the pH doesn't get so high that we produce soluble hydroxide complexes. So that's a condition we've got to avoid.

Again, the question is how long can we rely on it. And as Arend said, carbonation occurs with natural CO_2 and it goes as a square root of time, and I think the data in the literature for ambient temperature is something like 2 centimeters in 50 years. So depending on how thick things are, CO_2 coming from both sides of it, it might not be around for pH buffering.

Here I've got chemical reactions. I'm distinguishing them from sorption, which Inés will tell you is also a chemical reaction, but I'm talking about the more conventional type. Iron metal has been found to increase the dissolution rate of waste glass. And this was done by people at PNL several years ago and confirmed later on by others. And so as a result of that, consideration is currently being given to using the copper/nickel material, copper or nickel or Monel as the outside container for the glass waste packages to avoid this problem of iron increasing the rate of dissolution of glass.

Another thing under reactions here again is what we just heard before from Arend, that the CO_2 can react with portlandite and give calcite. And again, the same issues here; how long will it last and what about the timing.

Okay, then sorption. Just in general, these are the kinds of materials that have been found to be the best sorbents in the natural system. And happily for us, we're talking about producing a lot of iron oxides and hydroxides, and these have been found to be effective for the species that were on my list here, as well as many other things.

Other sorbents could be chosen intentionally and mixed with the backfill if they were particular ones and if there was a packing or backfill. They would have to be able to tolerate the elevated temperatures and still be good. For example, zeolites or things or that nature might not survive that, so that would have to be planned.

And how long could the sorbents be relied upon? I think it's more optimistic here than in those earlier ones, but there is competition for sorption, and so that has to be factored in when trying to use these materials for sorption.

Then colloids. It's conceivable that some of these materials could produce pseudocolloids, but it's also conceivable that colloids could be filtered out using something like a sand filter that's used in water treatment conventionally. Perhaps if something were put under the packages, they could filter out the colloids.

And then finally, there's the possible formation of a diffusion barrier having some kind of packing or backfill around that was unsaturated, which could slow down the movement of radionuclides at the diffusion barrier. And this would have to be modeled, and I think the same two gentlemen are going to cover that.

So then in conclusion, there are possibilities for some significant effects of the type we've talked about. The benefits that you seek to gain have to be balanced against possible detrimental effects on actual complete containment, which was the original mission of the barrier. And the accurate prediction of these things I would say is very challenging, particularly in terms of timing and longevity and these very long times associated with the half lives that we're trying to take care of.

Thank you.

DR. LANGMUIR: Thank you, Rich.

Questions from the Board or Board consultants?

Mick Apted

MR. APTED: Rich, you mentioned pour canister in terms of I guess not as a containment barrier, but possibly as a reaction retardation. What about cladding? Some of the briefings I've seen on temperature limits and so on, sometimes the temperature limits are imposed by some sort of upper thermal limits for cladding, and yet I've heard Bill Halsley say that number is pure smoke and mirrors. Any comment on the temperature limit on cladding?

MR. VAN KONYNENBURG: Well, since the beginning of the project, we've used 350 C as the limit for cladding. The reason for that is that in the reactor, that's about the temperature the cladding operates.

The logic behind that is that whatever irradiation damage that might have occurred has occurred, and if you hold it to the same temperature, perhaps not too much more will happen. And that's about as much science as has gone into that as far as I know.

DR. LANGMUIR: Langmuir; Board.

You suggested the possibility of redox buffering and I'd like to argue that at least I would doubt very much that it would work. If you're in unsaturated conditions and you've got gas flow, it's mighty tough to keep oxygen out of the system, so I would think that that would not be an option that would work very long or very well.

MR. VAN KONYNENBURG: That worries me, too.

DR. LANGMUIR: Well, thank you very much. We've made up some time. Thank you for the presentation.

Mick Apted is our next speaker. His topic is Engineered Backfill Approaches to Retarding Migration.

MR. APTED: Thank you.

As he said this presentation will be a joint endeavor by Jim Conca and myself.

I want to stress right from the beginning here that we are in this particular guise, myself, Jim and I, are definitely outsiders within the DOE. As such, we come to you as heretics. We're recommending a rather strong paradigm shift really to be considered in the program. I'm going to try to make a very strong case, and Jim also on why we think are the advantages of this type of shift in the strategy, where it ties into basically other approaches that have been very successful internationally in the area of waste disposal.

And at times, particularly I think on my side, it will verge on the sort of religious fervor and conversion. So the early part of this talk may seem a little bit like a sermon, but I really want to get across sort of the philosophical underpinnings of what it is we're suggesting, why we're suggesting it, why we think it's important.

The outline, Jim will, after this brief introduction on sort of the philosophical underpinnings, will talk about the concept description, materials, design and performance. I'll sort of jump in on the performance side of it to talk about if this barrier will achieve the conditions that we're proposing, what would be the impact on radionuclide migration out of the engineered barrier system and why we think that's important.

Jim will also talk a little bit about the previous work, natural analogs and some engineering scale models that have been done, and then there will be a summary.

So before we get into the concept, we've heard this word bandied around a lot about robust. I mean, it comes from the Scottish-Swedish program, robust packages. What is robust design in the international area of high level waste disposal? Basically looking at large reservoirs of design safety, redundancy in barrier functions, this is the multiple barrier concept, not necessarily emphasizing the site, not necessarily emphasizing the waste forms or any one particular barrier, but looking at all the barriers in their totality and also basically what can we use in terms of performance evidence to support that case.

A robust assessment on the other hand focuses on processes, models that significantly affect dose and risk, and also those processes that have a high confidence in achieving. And I think we can all make up our mind about a lot of what we heard today. It was very excellent science, but I think when we have to also wonder to what degree will we have high confidence within a licensing framework to take that kind of science to convince that we have a robust demonstration of safety. And, of course, we must also include detrimental processes.

Okay, strategies, how does robustness get translated around the world outside of the U. S. programs? One of the key features for demonstrating robust source term, and here, what we're saying is please protect us from the far field, if you will, and the uncertainties in the far field, as a decoupling from the site performance. We don't want to assist them which fails at the same conditions and at the same processes that the far field is having adverse performance.

We want to decouple these, and probably the strongest thing we want to decouple from is the site hydrology. Because quite frankly, probably the most problematic and highly uncertain aspect as we project our performance and safety case into the future, is in understanding and defending hydrologic models and conditions.

Here at Yucca Mountain, it's not particularly flow direction and flow rate; it's a lot of this episodic fracture flow. And we've seen a lot about the uncertainty, when that will occur, how long it will occur for, and so on. So we want a reliance on barriers and processes with high predicted reliability.

I think the type of--again, when we look overseas, we look at what other people are doing, what they're trying to evaluate, one of the key processes is diffusion, trying to limit the near field to a diffusion only transport regime, because that is one of the processes in which we have the highest predicted reliability.

So, finally, a robust engineered barrier system is the path that we're taking here in terms of this robust EBS. And source term, is basically designed to address, recognize irreducible uncertainties in the property and long-term performance of natural systems. We're not advocating this as any sort of compensation for a bad site. We're not saying don't follow through on site characterization and so on. That's very important in the multiple barrier approach.

What we are saying is that perhaps it's time and perhaps there are advantages to considering a stronger role for the engineered barrier system. And I was quite pleased to hear at least the NRC person this morning certainly holding out that olive branch that that certainly was in the realm of the interpretation that they had on the flexibility in 10 CFR 60.

Okay, enough of the sermon. I'll turn it over to Jim now, who will talk a little bit about this concept that we over the last four years have been bandying about and trying to get some interest from the program.

MR. CONCA: I'll try not to walk away with it this time. Actually, I feel that most people probably know what this sand/gravel barrier is because it has been talked about a bit, but this is what it looks like. And again, this is simply conceptual. It depends on what the final configuration of the waste site is and of the waste packages are.

But essentially you have the drift, you have the waste containers separated from the near field hydrology by a sand/gravel barrier, meaning you have gravel which is surrounded by a sand barrier at some slope. It has to be at least a five degree slope, which is very low, but the higher the slope, the better the performance. And essentially what this does is there will be no flow from the sand into the gravel until the sand is saturated. And under these conditions, the sand can never be saturated.

So it's not a very intuitive concept at first, because one usually tries to think of--one wants to emplace gravel in their drain field in their yard because it drains very well and it conducts water very well, but only under transiently saturated conditions.

So what you have is that under unsaturated conditions, water will not advectively flow across the boundary if you have sufficiently different pore sizes within each layer. So that's why you want to use sand and gravel, because the pore sizes are very, very different.

The boundary creates a capillary break between the host rock and the EBS system, and limits the transport to diffusion. Also, in the absence of advection into the system, the actual diffusion coefficient in a crushed tuff gravel, and in probably most gravels, but especially the tuff gravel, the aqueous diffusion coefficient will be below 10 to the minus eleven centimeters squared per second, which is excellent.

So, again, the idea that this system can address several features is very nice. And the temperature problem not withstanding, we can probably address that as well, I don't think these things are insurmountable.

The basis of the concept, here you have near field rock which will drift into the system. There will be at some point, whether--the actual timing of when the groundwater will begin to re-enter the system after the dryout period, that's problematic, but it will re-enter the system at some point. You will get episodic fracture flow through certain fracture sets, and that will drip into the system. I mean, there really is very little question that that will happen. The degree to which it happens, the overall flux is debatable, but it will happen.

So, you know, do you care about it drifting--do you care about water drifting on your canisters in the absence of any backfill, and maybe that is a problem. It isn't any big deal, but actually that could be a very large problem. Any water coming out of this system will remain in the sand layer and be conducted around this gravel barrier. And, again, this is just sand on top of gravel.

You know, there isn't any special geotextile there, although geotextile is very useful for emplacing it, but it does not depend on anything along that barrier, and we've done a lot of experiments to show that this works very well. And the water will be conducted around the barrier and away from the system, and there will be no flow into this gravel until the sand is saturated. And that's why you use the sand, because the conductivity, hydrolic conductivity is very, very high. Even under highly unsaturated conditions in this situation, you could never have enough flux through the rock into the sand that would ever even approach saturation, let alone get it above highly unsaturated.

These numbers here are actually real and measured in experimental situations. In this configuration, the water content of the sand will be very low, perhaps 8 percent, and this is using a flux into the system equal to the precipitation rate at Yucca Mountain, let alone any recharge rate or whatever, but using the precipitation rate at Yucca Mountain as an outrageously high flux. The sand will still conduct it away very, very efficiently. It will never get very wet.

Because there's no advection into the gravel and because the gravel is a dual porosity system, the gravel itself has a porous base associated with the gravel, and if it's made out of tuff material, then the gravel particles themselves have an interior porosity. So if that's the case, there's no advection, it reaches a steady state moisture content of about 2 percent, and that's experimentally determined. And under those conditions, the surface, the gravel is actually surface air dried. It has an internal water content, but the surface of the gravel particles are air dried, and the diffusion coefficient under those conditions is below our detection limits, which is ten to the minus eleven centimeters per second. So that's the basis of this.

Now, the thermal impacts that Rich discussed, one, you would like to avoid higher temperature, at least that's my understanding of it right now, you want to avoid higher temperature. You can do that by being creative, and you can actually emplace part of this gravel barrier first as a gravel barrier rind essentially. And then after 50 or 100 years, you can backfill the rest of the gravel barrier, which is a lot easier to do.

DR. LANGMUIR: You'll get us this overhead later, I hope?

MR. CONCA: Yes. Yes, this is hot off the press.

You might try to age the waste to delay the emplacement. That might avoid somewhat the higher temperatures. You might change the repository configuration. If you accept the higher temperature, you might say, well, that's not such a bad thing. And, again, some things will happen. One, you may actually get containment failure very early. You may lower the temperature within the surrounding rock. Actually if you are insulating the system with the backfill, you will lower the surrounding rock temperature. And then, of course, you really need to model this.

Adverse effects that you want a backfill for, you want to prevent roof spallation. I mean, that is a very real problem over such a long time period. And you want to mitigate the highly uncertain variable fracture flow.

So very quickly, other type of configurations, you can put in a gravel barrier rind. Before emplacement, you could go in while it's cold and easy and place just the gravel barrier rind, the sand, the surrounding sand, and then just a small amount of gravel, because actually the thickness of the gravel does not affect performance at all. And then you could have your very large air space.

Of course you have to come up with some structure which would maintain that gravel rind for awhile until you backfill it, but again this support structure is not important for performance. It only has to perform until the rest of the gravel is filled in later on. And to fill in this space remotely is a lot easier than going in later on while it's very hot and trying to emplace the entire gravel barrier.

Now, this actually isn't fantasy. There actually has been a fair amount of work on this. Glen Gee and I for the low-level nuclear waste, the Hanford site, did large scale experiments for exactly this design and they worked very well. Some NRC funded work by Schulz did similar very large scale gravel barrier systems for low-level nuclear waste disposal, and they worked exceptionally well. And there actually is some natural analog studies, Wanatabe has looked at some gravel barriers used to control just this exact problem in 1500 year old Japanese burial grounds where of course you were trying to preserve artifacts that were buried along with the leaders, and they performed very well, in fact. And they used the preservation of large iron artifacts such as swords and shields to determine that.

This is some large scale experiments that Glen and I did. We set up a large percolation box, essentially four feet by one and a half foot by four feet, and essentially we set up two sides of two waste packages. We had some coarse sand, and it really doesn't matter what kind of sand it is as long as it's a good, well sorted coarse sand. We had gravel, which was a one inch gravel. We had our "waste package" there, and we set up a little rain machine to essentially rain on this to provide as much of a recharge as we wanted. We tried to make this fail. This was the whole purpose of this experiment, was what recharge could this handle and under what conditions, and we were able to monitor the flow through both the gravel and the sand and it performed very well. In fact, I kept plugging the laboratory because I could not provide enough water to make it fail, which is aggravating.

This is what the backside of it looks like. We actually outfitted it with tensiometers to look at the surface tension, the matrix potential within the system while it was performing. These are some of the results.

This is the soil tension at selected tensiometers in that previous slide. This is the recharge rate. So what we did was progressively step up the recharge rate until the system failed. And, in fact, we had to get two very high recharge rates. In fact, this is a recharge rate--well, let me start over.

This is the expected recharge rate at this site, which is very low. This is the average Hanford precipitation rate, which is not too far off from that of Yucca Mountain. This is 1,000 times the precipitation rate. It was performing very well. We had to get it up to between 6 and 10 ml. per square centimeter per hour recharge rate to get it to fail.

And when it failed, it failed by dripping slightly in one portion in a part of the gravel barrier boundary that was poorly constructed, because it actually put in things like big coggles that broke up the barrier, we put in depressions that in case someone was emplacing this and they stuck their elbow in the thing. I mean, the integrity of this boundary does not have to be very good. It actually performs very, very well.

In fact, the degradation of this boundary--the tolerance for the integrity of this boundary and how well it performs is directly related to the flux into the system. So if very low flux is expected at Yucca Mountain, this can be a pretty ragged boundary and still perform very, very well.

And once we did cause it to fail, this is in fact a higher recharge than exists on the earth anywhere and is probably equivalent to a monsoon a week, and all of that is becoming recharged. So this is very, very high recharges and we could actually turn the system on and off like a light switch. We would raise the--that's why there's a little hysteresis effect here. We'd have to raise it above ten to get it to start failing, and then it would just start drifting. It would still--diverting 99.7 percent of the flow around the system. And then we decreased the recharge to less than 6 ml. per centimeters per hour and it would shut off. It was very, very reproducible.

The Schulz work for the NRC was also very, very successful for these kind of systems. They were using concrete surrounded waste. Again, they had their conductive layer which was the sand layer, which actually they were using diatomaceous earth. It really doesn't matter. There is no priority set of materials to use. You just want the difference in the porous structure to be large enough, and they were using regular gravel. I can't remember the size gravel, but it's on the order of one inch gravel. And, again, they performed very long-term experiments that worked very well.

They had no failure until they got above 1 ml. per square centimeter per hour, in their systems, which was very long, meters long. This is on the same order as our failure, again very, very robust.

These natural analog studies, these archeological remains, these Japanese burial mounds, essentially they were situations exposed to the elements and still many of them performed very well. Those that failed mainly failed because they were physically eroded along the surface because they were not protected very well.

The kind of configurations are again the inner space which was filled with artifacts, many of them iron, large iron artifacts which provided the basis for the performance. Again, there was gravel, they experimented actually even that long ago with different gravel configurations. They had pea gravel, they had well sorted gravel, they had gravel interspersed with masonry block and essentially surrounded by sand. Sometimes on top of these mounds they put further gravel in order to prevent the obvious erosion from obvious weathering effects.

Sort of a pseudo summary here. Gravel barrier performance is not sensitive to temperature, it's not sensitive to radiation flux, to the chemistry of the water. It's not sensitive to the waste package materials or the configuration, as long as you can maintain a boundary between the sand and the gravel that is sloped at least 5 percent.

It is sensitive to the water quantity only if you exceed this recharge failure rate of about 1 ml. per square centimeter per hour, or below that, you'll actually raise the water table to the surface. So I don't think that's a major problem.

Do you want to take over?

MR. APTED: Thanks, Jim.

As Jim said, I mean he stressed the nature of the design, why we think it will work, why it can be constructed, why it will persist. Now let's talk a little bit about what is its performance. Does it have any advantages? Are they slight, orders of two or three or four or are we talking about orders of magnitude?

Well, this graph out of Jim's lab is not familiar to most of you. It's a function of effective diffusion coefficient as a function of volumetric water content. Actually, the measured value for this dry gravel inner barrier is down actually below here. It's ten to the minus eleven centimeters squared per second, rather low, orders of magnitude below what it is for saturated conditions.

Does that have an impact? Well, Rally sort of showed wizards and so on back at the PACE-90 calculations. But if we just look at the PACE-90 calculations and compare the releases of these key dose nuclides with what would be their value if we had a conductive barrier in here, we can see that there's a drop off of many of the components. In fact, cesium now is entirely gone. Neptunium is only coming in here, out on the order of at the million year time frame. And even iodine and technetium, these anionic non-sorbing radionuclides, their peak releases now are orders of magnitude lower. That might not be bad.

Now, Rich mentioned something about containment, and I agree that the idea of containment shouldn't be compromised in terms of the haste to introduce other barriers and so on, that we need to look at the interactions and possible deleterious effects that may arise among these barriers.

But let's not lose sight of containment. I mean, here are some calculations again based on the PACE-90 exercise which assumed 1,000 years containment, and then let's say we went to a nice robust massive engineered package, thick Hastelloy, real expensive, maybe a 10,000 year containment, what's the effect on dose. Zip, nothing, nada.

So while I believe, and I'm not saying abandon the idea of extended containment, that may have merit, but don't lose sight that extended containment by itself doesn't buy us any performance advantage if we are faced with a dose type calculation for safety.

Another way to look at it is here's a reduction factor, think of a release of a conservative non-decaying component, and we're going to use that to normalize that release versus what would happen when we actually have radionuclide decay, which we all acknowledge is going to be an advantage in terms of the performance here. Well, if we look, most of the radionuclides of concern for dose are out here in this sort of radionuclide half life range. Here's the effect if we can go out to a 10,000 year containment in terms of reduction.

Now, if we put a conductive barrier, sand/gravel in there, and assume no retardation, zero retardation, this would be the reduction in terms of--as a function of half life for these nuclides. And if we have some modest K_d values, we can see progressively that we greatly knock down the release. This is not out of the system; this is just out of the engineered barrier system assuming a 2 meter gravel bed with this very low diffusion coefficient.

So I think Rich was maybe a bit too modest in some of the advantages. Let me point out some of the advantages of some conductive barriers and diffusion if we were to consider such a radical departure. Two meters of partially saturated gravel is equivalent to about 90 meters of compacted bentonite. Now, ask any of the other programs around the world that use compacted bentonite for their backfill, at the most, at the high expensive end, they can justify about one meter. Most of them have about 30 centimeters. So with two meters of partially saturated gravel, we'd get the equivalent performance of about 90 meters of compacted backfill, which is almost a far field in itself.

There's a significant delay in decrease in peak, as well as steady state releases of all radionuclides. This is not chemically based. We don't have to go out and measure separately sorption or solubility values for each and every one of the periodic tables. This type of information, this type of design knocks down release of all radionuclides, whatever their persuasion, whether they're highly soluble, non-sorbing radionuclides or what.

The near field performance basically will become decoupled from the far field hydrology, and we come back to that guiding principle of robustness, isolating ourselves from that uncertainty of when and where fracture flow will occur.

As Jim mentioned, this basically is gravel that's going to be air dried. It's hard to envision that there's going to be any transport of colloids across this barrier. That hasn't been shown. But if it can be shown, then perhaps we can begin to actually close the book on some of these issues on colloids, because quite frankly, colloid transport really scares me.

I think--I applaud the efforts in demonstrating that colloids can form and that we need to study them to the point of understanding their behavior, but if they don't transport it, we introduce a barrier which will prevent their transport, at least in the near field, at least we can shuffle the problem over to the far field perhaps and colloids there. But then colloids are working with much reduced amount of radionuclides that are released from the engineered barrier system. And I think again it would make the problem of colloids quite less debilitating to demonstrating safety at Yucca Mountain.

Jim mentioned NRC is examining this approach for low-level waste. Again, we can possibly trade on that type of convergence of ideas, and as Jim mentioned, there are these analog structures that have persisted in very seismically active areas for 1500 years. I mean, these are at the surface in Japan, which experience a lot more ground motion cumulatively over that time period than Yucca Mountain will see for a very, very long time.

I think we have one more viewgraph. I don't know if you wanted to--it's sort of your summary. Jim and I sort of did tag team on this.

Basically, we believe the proposed gravel barrier increases the isolation of the waste package. It will decrease the release rates and provide a barrier whose performance can be readily modelled and tested. We see there's already a lot of data that we can piggy back from other sources to use already. It uses inexpensive and readily available materials, minimal engineering development and emplacement.

Lastly, do we have all the answers? I mean, is this some sort of magical thing? We don't have all the answers. We'd be the first to say that. But quite frankly, site characterization, site properties, hydrology, thermal, containment, waste form have all had their day to look at performance in trying to establish some sort of credible performance and demonstration of safety.

I think in terms of why we need all that information, we need a multi-barrier approach, there's certainly some merit in considering the strong performance that might be derived from an engineered backfilled type as we have described.

Thank you very much.

DR. LANGMUIR: Thank you, Mick.

Questions from the Board or consultants or staff? Don Rimstidt.

MR. RIMSTIDT: Yes. Your diagrams basically show the flow being diverted around the canisters and then mysteriously disappearing into the floor. Do you have a plan for removing the water once it's been diverted?

MR. APTED: That's a good point.

MR. CONCA: Usually in most of the other designs for this kind of system, you had some kind of drainage ditch around the waste package. And, again, as long as the capacity of that drainage area can hold a maximum amount of recharge into the system, then you're fine.

And, again, the recharge into the system is so low that I think that's a fairly trivial engineering feat, and in fact we didn't talk about emplacing chemical barriers below the system because anything which does drift into this or flow into it or whatever, essentially will move downward.

And so it must pass from the waste package into the floor materials, and if you have some kind of chemical barrier, say a phosphate chemical barrier which is being studied now and actually demoed in some areas, then you can actually enhance the sorption properties and decrease the release from the system.

And the reason I stress phosphate is because phosphate is a good sorber of all radionuclides. And, in fact, it looks like it may even sorb technetium significantly, which is very nice. It certainly sorbs iodine. So that might be--so you need to play with the design in order to optimize all of these parameters, but I think it's very doable.

MR. BOWMAN: Yeah, again referring to your diagram, you show the water entry to the sand portion of the barrier through the fractures, which is going to tend to induce finger formation in the sand. Rather than getting uniform saturation of the sand, you're probably going to see vertical fingers, which is going to tend to maximize the potential to saturate that sand at the interface because you have low migration away from that finger due to the unsaturated sand surrounding it.

Did you consider that in your analysis?

MR. CONCA: Yes, and also experimentally, because we did induce fingering. We had, you know, large flows. Unless--there will be an optimal minimal thickness to the sand, and again, you know, even using the maximum infiltration rates, you'll never get the sand layer even in any kind of a fingering situation at all saturated. I mean, it will remain highly unsaturated because the hydrolic conductivity of the sand is so great.

And I didn't really show the conductivity curves--I probably don't have time to do it--but the reason you choose the sand and gravel is because of their incredibly contrasting hydrolic behavior. And, again, sand, a nice uniform coarse sand is incredibly conductive. It can handle anything that comes into the system.

DR. LANGMUIR: I'm sorry, we have to go on. This is the word of the day it seems for me.

Ardyth Simmons will sum up the Department of Energy's work on radionuclide migration, and then we will take a break and reconvene for a somewhat briefer perhaps round table discussion. Ardyth?

MS. SIMMONS: I think I can be relatively brief as well. We've heard a lot of presentations today and a lot of information was given, and I hope you've been able to recognize that a lot of work has been done recently in order to focus towards narrowing the number of tests and investigations that have to be done in order to provide the kind of scientific confidence that we need to demonstrate the capability for waste isolation at the site.

It is our intent within the radionuclide transport program to continue to focus on narrowing down the type of tests that would need to be done. And the way we've done that is by a series of strategies, many of which were presented today.

Within these areas, what we plan to do over the next few years during the period where we would be trying to demonstrate the site suitability determination and then into license application period, are to complete the solubility and speciation work on neptunium carbonate. And, yes, we do have some work in the plans on phosphates. That question was brought up before. To continue the speciation modelling that's necessary which Drew Tait talked about, to finish the undersaturation experiments on these three radionuclides, and to do some limited work on technetium. That was brought out as being important today.

In the area of sorption, we're going to continue to work on understanding sorption in the unsaturated zone and particularly in fractures. We need to complete the colloid transport work to be able to satisfy the strategy and the questions that we've asked for that.

And in the area of source term, we need an improved understanding of the timing and the release rates and the waste form degradation.

Now, in addition, we also plan to investigate the influence of the engineered barrier system materials and how they interact with the natural system. One of the things that I mentioned this morning regarding the bounding ground water compositions with that little triangle that I showed, and I said that we could bound it, the compositions, by J-13 and UE-25 P-1. I unfortunately neglected to say that that was in the absence of introduced materials. It would still be within a heated scenario, but the ground water composition could be very different once we're talking about adding a lot of cement to the system. So we need to continue to evaluate this work.

And in addition, we need to continue working on the transport model that George Zyvoloski showed earlier today by further incorporating the mineral dissolution work that he discussed and partially coupled reactive transport. And eventually this information will be provided to performance assessment as we have been doing all along.

In terms of future work, you heard earlier today that there will be a systems study to investigate the necessity of means--not the necessity, but the means by which we would get to the Calico Hills. And the Calico Hills remains a primary barrier to radionuclide transport, therefore, we need to do some field scale transport tests in that area. So that will take place in the future.

We will be closing on these strategies for the K_d approach, the solubility and colloids. We will evaluate carbon-14 transport, such as Arend Meijer discussed, and the PA model will eventually have transport with a full set of radionuclides of concern.

We plan to have an improved model of waste package degradation that incorporates the influence of EBS materials, and to have an improved understanding of coupled processes. This is the coupled thermomechanical-hydrologic chemical processes, and then finally the improved source term model that incorporates fuel dissolution and diffusive release.

So I hope that we have been able to leave you with the impression that the radionuclide transport program is an integrated program that requires input from all of these different areas and cannot have bits and pieces done in isolation. However, we are working to focus our energies towards collecting that information that we need for a bounding determination for site suitability and greater confidence for license application.

Thank you.

DR. LANGMUIR: Thank you, Ardyth. Questions from the Board or Board consultants?

I'd be curious if the proposed program that's going on now has looked--you've talked to those folks and you have some sense of what, if any, of these future work activities, other than the NTS well, which you have acknowledged was going to be later done, what of these activities are being put off? Which ones do you plan to do in the very near future, which ones do you not expect to be able to do in the near future, or do you know yet?

MS. SIMMONS: Some of the colloid experiments will be of more limited nature than what we had originally planned. But other than that, I would have to say that I don't know for certain. It will depend on what budget we finally get, and we feel that because this information is important, that it's difficult to cull out certain bits and pieces of it now as opposed to deferring it.

DR. LANGMUIR: This is a very ambitious list of things to do, clearly. Thank you, Ardyth.

Let's take a break and reassemble the speakers and consultants and interested Board members for our round table discussion in about ten minutes. We'll make it fairly short. I think we're all tired.

(Whereupon, a recess was taken.)

ROUND-TABLE DISCUSSION

DR. LANGMUIR: Please take your seats.

We had a very full schedule, with practically everybody taking all of their allotted time, so there was insufficient opportunity for questioning of our speakers of the day, by and large. For that reason, what I'd like to do for starters here is to give those of us at the table a chance and, if time, those in audience a chance to ask questions of individual speakers, in chronological order through the day. Try and hold that down to a few minutes, if we can, and make them key issues, key topics of concern.

Sam Rousso was our first speaker of the day. I had a question. I think all of us are interested in how the PPA is going to be applied to programs such as the geochemistry program, the hydrology programs, site characterization, and Ardyth expressed uncertainty because, obviously, it's a new program, as to what that might do to her program, the implementation of the PPA.

I guess I'm curious how the OCRWM plans to proceed with working their way through the prioritization effort, such as geochemistry and hydrology.

Sam, can you speak to that?

MR. ROUSSO: Speak to it very briefly, and I'd probably ask Max Blanchard to pitch in a little bit.

I just started to say that the breakout of what work can be accomplished in what time frame is going to be dependent on, again, the funding overall for the program, which breaks out into the funding for the project, and then it'll be a project call by the project manager as to how he assigns what work goes for surface, what goes underground, what goes with the studies, what goes with the modeling.

I don't know that a decision has been made at this point, as Ardyth had expressed where the different pieces fit together and to what extent they can be funded in the next near-term time frame, or what would be moved out for further consideration.

I can't speak to it technically, and Max, I don't know if you have anything that you'd like to add to that.

MR. BLANCHARD: I don't have an awful lot to add to it, just to say that in the construct we had for site characterization to determine near-field and far-field geochemical studies as it relates to site suitability and license application, originally, our view was a little different than it is now, in that we were going to count on geochemistry largely as a backup system to help us build reasonable assurance, if the assumption proved valid that we could meet the requirements, based on other more physical-type systems and parts of the site that were easier to validate, because it's a very complicated world in geochemistry, and there's a lot of debates about knowing three-dimensional space so you can apply some of these models with respect to retardation and sorption.

And if a site had to rely on that field in reducing uncertainty to a narrow band, then the site's not likely to make it through the process, because there's just too many variables there.

Now, that's looking at it from site suitability standpoint. Looking at it from engineered barrier standpoint in the near-field rock water waste package and interactions, you really need a good understanding of that, and I think what Ardyth is pointing out in this session, that the team in geochemistry is really integrating engineering hydrology, mineralogy and geochemistry in a way that you'd like to see it integrated to improve the understanding of the near-field.

They haven't gone far enough yet, and I don't think that the things that Ardyth talked about is just a mere shopping list. I think some of them are very important if you intend to answer the hard decisions that have to be made when you come down to select a material and decide, what are you really going to rely on for the next 10,000 years? We're not there yet, by any means.

On the other hand, we don't know enough about the funding profiles and where we're going to try to be in 2001 with respect to the proposed program approach to really say how much of that healthy budget that Ardyth is hoping to get for the geochemistry program can really occur in that time frame, as opposed to that which would occur a little later on.

So, I'd say if you want to ask that question again about six months to a year from now, we'd probably have a lot better understanding, especially if it was clear by then whether or not we had a revolving fund, and the program was not going to be seriously limited with respect to dollars, or we knew that we weren't going to get a revolving fund, and the program was on somewhere around a $300 million effort, at most, for site characterization for the next five years. Then, I think a whole lot of things would be re-thought in the entire program, and whether or not we continue with a large geochemistry program is more than likely not very clear.

DR. DOMENICO: All of the modeling studies so far, especially by Bob and the Sandia group, indicate that the site needs a chemical barrier, and it won't make it without it. That's my reading on what I heard today, so I don't know if you can--I think you said that we would not like to rely on the chemistry because of the uncertainties, but, it seems to me, unless Bob can correct me, without K_ds, based on what I've saw today, that we would have breakthroughs that would be in excess of any limits that EPA might put on.

DR. ANDREWS: Let me try to answer that a little bit, Pat. I tried to break it out into different time periods and different performance measures, because the relative importance of all the things that we've talked about today depends on those time periods and performance measure, and if we keep with the current 40 CFR 191, where Carbon-14 is an issue, the aqueous transport processes become almost insignificant, and you just focus on the Carbon-14, which means you focus on the very near field, and, of course, that very near field and the corrosion rates and solubilities and waste form dissolution rates are somehow geochemically related, but they're not dominant.

Now, if we change, and Carbon-14 is not an issue in terms of a cumulative release sort of standard, and then we're looking at aqueous phases, you know, over the 10,000 or even 100,000-year time periods, the aqueous phases that are most important, most of the laboratory kind of information that we have so far indicates they're not retarding, anyway. I mean, they don't retard.

DR. TRIAY: Did you use sorption data to come up with this conclusion?

DR. ANDREWS: For the technetium, we used your data, which said there was no retardation.

DR. TRIAY: How about for the others? Did you use sorption data to come up with the conclusion that you just stated?

DR. ANDREWS: Sure. I mean, we used the minimum K_ds. I mean, I think that's important.

DR. TRIAY: In which case, you will have to agree, then, with what Pat just said. I mean, I don't know how this is even a question.

DR. LANGMUIR: Could I ask that all the speakers identify themselves?

DR. TRIAY: Oh, I'm sorry. I'm Inés Triay from Los Alamos National Laboratory.

Let me just say something very quickly. When one talks about the geochemistry being complicated, the chemistry, the actual chemical reactions of radionuclides with minerals in the tuff are actually, in my view, less complicated than to actually get a flow model of the mountain.

You are going to have to get a flow model of the mountain one way or the other. That is the hydrologic information that you need. To actually say that to get sorption data is more complicated and, for that reason, you're not going to do it, is rather foolish, because every single program in the world is actually using sorption data to create a better case for radionuclide retardation, and the reason is that chemistry can actually be studied in a laboratory and extrapolated to any conditions whatsoever. They are not dependent on space, where are you? It doesn't matter. A chemical reaction does not depend on where in the mountain you are, or what is the hydrologic flow, or what is the water velocity. None of those things are important.

So, I have to disagree with the major statement here, the actual chemistry is extremely complicated to get to. It's not only not complicated, it's easier than most of the parameters that you can rely on.

MR. BOWMAN: Just to follow up on that just a bit. I'm Rob Bowman, consultant to the Board. I don't want to belabor this more and take up all the time, but there's something, for instance, known as the solids concentration effect, that's well-known in pesticide sorption and metal sorption in soils, whereby, if you change the soil to solution ratio, you get different sorption coefficients, so I don't think it's quite as straightforward as saying if you use the one gram of tuff and 20 mls of solution you get a K_d that's going to apply to a situation where you have 100 grams of tuff for every ml of solution.

DR. TRIAY: On the other hand, you can study sorption as a function of concentration, and I showed you those charts today.

Essentially, what happens when you increase the concentration of the radionuclide, like we are doing in those experiments, if you get a conservative K_d--because you have a lot of radionuclide for the amount of solid phase that you have--it's not going to go the other way.

Now, competition is something that is well understood, also, so the fact that isotherms are non-linear does not mean that if you use one gram of tuff and 20 ml of solution, expecting to find a different ratio in the field, the actual K_d that you get when you have one gram of tuff and 20 ml of solution should be conservative, and we have shown that today, as well as many other times during these meetings.

DR. LANGMUIR: I had a question on a little different subject here, but Ralston Barnard discussed performance assessment, and identified transport, and we had a later discussion of the effect of temperature in thermal loading by Robert Andrews, and I was a little concerned that it didn't appear they had the same script of assumed thermodynamic data, sorption coefficients, as inputs to their models. Ralston did not use the effective temperature on sorption or solubility that I could see, and yet, Bob Andrews did, and it bothers me that this is all in the same program, that we're not using the same information as a basis for our modeling.

Could you speak to that, either of you, or both of you?

DR. ANDREWS: One of the objectives of the TSPA was to include as many thermal dependents--of this last iteration--to include as many of the thermal dependencies as we felt reasonable.

Now, one could always say that we have these range of, in this particular instance, solubilities which happens to be geochemically-related and thermally-related, and just sample off that whole distribution, irrespective of the correlation that it relates to, geochemistry or temperature.

We chose, in contrast to doing that, which is just sample of a wide range of very uncertain, in this particular case, solubilities, we chose to try to put in a functional dependence to, in this particular case, pH, carbonate, and temperature, and model it that way, and then the sensitivity is to pH, carbonate, and temperature, not to just that particular parameter.

Now, as I alluded to, perhaps the functional relationship has not--and Drew, I think, pointed out that functional dependency may not be strongly thermodynamically based, but it's a kind of a fit to raw information, which can be questioned, of course, and that's, you know, the data that they're generating now, is to try to better define those things.

DR. BARNARD: Ralston Barnard from Sandia Labs. Everybody hear me?

In contrast to what Bob did, we used distributions of values for sorption and solubility. However, on the advice of the folks from Los Alamos, we did not include a temperature dependent in that, which would have given us a third dimension, you can think of, in the PDF that we used.

We felt that there were no data which allowed us to draw any conclusions, come up with any functional relationship for the change in solubility or sorption as a function of temperature, so this is one area that we felt that we would not be able to learn anything by doing a parameter variation on a--using a model that we had absolutely no confidence in whatsoever.

It gets to the point that I made in my talk, that if you don't understand the model, you shouldn't do a parameter variation study of whatever parameters you happen to include in there.

I hope that more or less addresses our rationale for what we did.

DR. LANGMUIR: I guess I'm closer to believing there really are numbers and guessible numbers that one could lean on with at least some confidence. There's a goal called EQ3/6 which is in the program that has temperature data on it for radionuclide solubilities. There's other data from the NEA databases on temperature effects on solubilities. I would think that should be all within this program.

For example, there's also some published data on sorption as a function of temperature, and analog data on elements that are like radionuclides, or could be considered analogs for them as a function of temperature. I would think all of that should be in the program or on the edge of the program being thought about at this point in these models.

Ardyth?

DR. SIMMONS: I'd like to respond to that.

Yes, indeed, there is information in the NEA database, there is information in EQ3/6, and the other sources that you've mentioned. All of that is available in the program.

However, something very important that didn't come across in today's discussions was that when the solubility data and the sorption data were provided to Total System Performance Assessment, there was an elicitation done, essentially--I don't want to call it expert judgment, because it was based solidly on available data that was both collected in the program, and then compared to what was available in these other databases that we just mentioned, and a table of values was provided by the scientists that are in this room, giving the best estimates of the data that we could really bank on, and it was there for PA to use in their calculations.

And, I think as Drew mentioned earlier today, at least the one performance assessment that was done by the M&O didn't make use of that whole body of data, only used certain selected parts of it, so we could--the investigators on the program provided data that they stood behind and that the project stands behind, but it was not used probably the most comprehensive way that it could have been, and we've commented on that, and, hopefully, we can do better for the next PA.

MR. APTED: This is a question, maybe, to Bob and to Ardyth, and anyone else who wants to jump in. One thing that was included in the M&O model was matrix diffusion. You didn't quite give us an idea of where the numbers are coming from, because I didn't see anywhere in the program where matrix diffusion is being investigated. It seems like it's potentially a very, what's the word, lucrative process that's going to knock down every radionuclide. Again, it's one of these processes which are not chemically-dependent, although I agree with Inés that, I mean, the chemistry is very important.

Matrix diffusion has the potential to decrease iodine, neptunium, all of these nuclides, so is there any work on matrix diffusion, and where did you get your values to your RIP TSPA? It seems like a missing topic at this meeting on transport, yet very potentially important.

DR. ANDREWS: Yeah. I mean, the matrix diffusion wasn't explicitly included as a particular coefficient with certain units, you know, as George had in his model that he presented earlier. It was the assumption that the matrix, whether it be by imbibition or diffusion, that the matrix transport was more significant that any fracture-initiated transport.

So, whether I talk about it from a transport perspective or from a flow perspective, the nuclides were advectively moving in the matrix, so it wasn't matrix diffusion, per se. I listed it up there as imbibition/diffusion, because when you get down at the detailed process level that LANL's working at, and George presented, then it is explicitly included, but in TSPAs, it's not in there explicitly as a parameter.

DR. TRIAY: Let me just say something. I mean, part of the Los Alamos work is looking at diffusion to get, actually, diffusion coefficients through the tuff matrix, and we have already published papers, essentially, the diffusion coefficients in the tuff for conservative tracers. It's about 10^{-6} cm^2/second, and for anions that are excluded in terms of size and charge, but, mainly, you can explain it in terms of just size exclusion.

For instance, for pertechnetate, you go down to about 10^{-7} cm^2/second. George has this data, and Bruce Robinson, and they use it in their models.

DR. BARNARD: Mick, diffusion is also part of the TOSPA model, and so it isn't a major contributor to fast path flow, which is sort of what people are most interested in, in seeing what kind of releases you get, so it didn't seem worthy of talking about, but it was there.

DR. LANGMUIR: Can I ask if our consultants have any specific questions they'd like to ask of speakers of the day?

MR. RIMSTIDT: I had some concerns about the definition of K_d, as was presented here today, in terms of amount of material adsorbed per mass of rock sample.

My understanding adsorption phenomena suggests that K_d should always be reported in terms of surface area. Can someone explain to me why the units seem unusual?

DR. TRIAY: I actually reported the K_ds in both ways. I reported a K_d which was amount of material per gram of solid, divided by amount of material per milliliter of solution, and that is why the units are milliliters per gram, but I also had a table with all of those K_ds converted to Kas, what I called Ka, where I actually divided the K_d by the surface area, and I reported the Ka in meters, which is what the Europeans normally do.

I mean, you can report it in any combination of that unit, but I chose to report it in meters, so...

MR. RIMSTIDT: And can I follow that up? The K_ds are used in a variety of models, and my understanding of the way they're put into the models is in terms of amount adsorbed per gram of rock, and yet, now they're talking about the internal surface area of these materials.

Now, who's measured the internal surface area of these samples?

DR. TRIAY: You mean surface area of the samples? Let me say a couple of things.

In the view graphs that you have, the surface area of these tuff materials are reported. The good news is that for things that are zeolitic, the surface area is not varying very much at all. I mean, you're talking about all the data that you have there shows that it varies from 20 to 25 m^2/g, which is truly, I mean, that's a perfectly narrow range if you want to extrapolate that.

The other thing that I wanted to point out was that for a lot of the radionuclides, like, for instance, things that are cations, like cesium, surface area doesn't really matter. Essentially, what you're looking at there is an ion coming out of solution and a cation coming in from the solution phase into the actual sites of the exchange here. So, in that case, these sorption coefficients are not going to be sensitive to surface area.

In the case of the actinides, I showed that they were, but the good news is that, based on a rock type, you can say in a narrow range what the surface area that you are going to be expected is.

MR. RIMSTIDT: How do you measure the internal surface area of materials like that?

DR. TRIAY: All I said was that I measured the surface area based on BET analysis.

MR. RIMSTIDT: But that's the external surface area of the grains, not the surface area of the pores inside of the grains.

DR. TRIAY: That is correct. That is why I continued to say, for the neptunium, in my talk, I said that most likely than not, what you are observing is either cation exchange, in which case, surface area should make no difference, and, in fact, we have two papers, a paper and a report out saying, as a function of size of the tuff, and the different surface areas as a function of size of the material used for sorption, we get the same sorption coefficients, because it's a matter of the actual crystallite, not of any other process.

For neptunium, which is what I was addressing today, it looks like it is a combination of cation exchange and surface complexation. The surface complexation appears to only occur at the surface, in which case the internal sites don't seem to make any difference because, if not, I would not have been able to predict the way I did, giving reasonable predictions.

MR. RIMSTIDT: I'm afraid I'm still confused on this issue, because you're being very specific in your answer, but my question is very general, and that question is that when you do sorption experiments, you're sorbing ions to the outside of mineral grains.

DR. TRIAY: Not always.

MR. RIMSTIDT: Okay, but that's the general concept, and you, then--

DR. TRIAY: Not always.

MR. RIMSTIDT: --measure the external surface area, but the fluid is flowing through--in the models, we're assuming the fluid will flow through fractures and pores that are inside of the grains, and you're not making a distinction between the surfaces on the outside and the surfaces on the inside.

DR. TRIAY: Okay. If that is the question, then it's
not a matter of the actual reaction. You're talking about if I actually take the tuff, which is a solid piece of tuff, and I crush it, right?

MR. RIMSTIDT: Right.

DR. TRIAY: And then I get a sorption coefficient. Am I able, with that sorption coefficient, to predict what happens when I actually elude a radionuclide solution through solid tuff or fractured tuff.

That was the intent of the second talk. Jim Conca addressed that issue, and, actually, we have papers on saturated systems. His system happened to be unsaturated, on taking a solid tuff column and applying a batch sorption coefficient to predict what would happen when you actually flow selenium through a solid tuff column.

The answer, at the moment, appears that these batch sorption coefficients are capable of making that prediction. That's the data that he showed.

DR. DOMENICO: Inés, I find that hard to believe. Your specific surface on your crushed samples, certainly we would expect them to be more sorptive, more reactive than going through. I never heard of any experiment where the finer-grained the material, the more reactive the system.

DR. TRIAY: Let me say, I guess, what I said again. For cation exchange mechanisms, what we're saying is--and there are two papers on that issue, one on the high-level waste management proceedings, and another one that is a report by Rogers and Meijer--they actually did experiments as a function of particle size.

What was the range, Arend?

DR. MEIJER: I think it went all the way from something like 50 microns, maybe 20 microns, all the way up to, oh, 5,000 microns.

DR. TRIAY: Did you see a difference in the K_d based on those different sizes?

DR. MEIJER: No. The K_ds were the same for all the different fractions that we used. In other words, we used-- Arend Meijer is my name.

We used one fraction that might have been from 20 microns to 50 microns, another fraction from 50 to 100, another fraction from 100 to 200, another, you know, et cetera, did separate batch experiments on all of those fractions, using the same mass of material in each one, got the same K_d each time.

The reason is that the grain size of these rocks is so fine that even at 20 microns, you have, you know, probably hundreds of individual grains in one little, or individual crystals in one little grain at 20 microns, so the surface area that you add by crushing the rock is minor, is almost minuscule compared to the inherent surface area of the crystalline material in the rock because the stuff is so fine-grained.

DR. DOMENICO: Can I follow up on that with one other question on that, and then I'll keep quiet.

You know, you have K_d as a linear absorption isotherm, and I want to know just how you view the uncertainties in this. Obviously, your isotherm was not linear. As a matter of fact, you had several linear isotherms described by one point, I believe; is that correct?

DR. TRIAY: Wait a second. First off--

DR. DOMENICO: The slope of a linear absorption isotherm is K_d.

DR. TRIAY: Absolutely. So, essentially, what you saw on those view graphs is K_d as a function of initial concentration, and the fact that the K_d was constant.

DR. DOMENICO: I saw the curve.

MR. BOWMAN: Actually, most of your curves--this is Bowman--showed that the K_d started high at low concentrations, and then decreased to a constant value at higher concentrations.

DR. TRIAY: No. That was only for the clinoptilolite and the zeolitic tuff. The rest of them were essentially zero, no matter what the concentration, but let's stay with that issue, coming back to what Rob was talking about before.

That, essentially--what that tells you is that using a 20 ml to one gram ratio is a conservative thing to do, because, essentially, what you're saying is that at higher--when you have the total concentration in the solution phase higher, that when you have less concentration in the solution phase, you essentially get a lower K_d. So, that should be a very conservative number to use if you actually do it at 20 ml to one gram.

DR. DOMENICO: So you feel those are safe-sided numbers?

DR. TRIAY: Well, I mean, based on what Rob just said that he saw in the data, which I agree totally, for zeolitic, and for clinoptilolite itself, there's no question that that has to be a conservative number.

MR. BOWMAN: That's not the same as changing the solid to liquid ratio, in terms of effect on K_d.

DR. TRIAY: As a matter of fact, we have data showing that that is exactly the same, but I would like to know how do you think that that is not the same?

MR. BOWMAN: Most of the data in the literature shows that as you increase the ratio of solutions to solid, the K_d goes up.

DR. DOMENICO: That's correct.

MR. BOWMAN: Which would be the reverse of what you're saying of your numbers being conservative. Now, whether it's a big enough factor, a factor of two, a factor of ten, that's a question.

DR. TRIAY: If that happens, it's normally almost an artifact of how you separate the phases. There's no--I'm sorry, but, I mean, just from the point of view of physical chemistry, right, I mean, there's very little explanations that you can come up with to actually justify the statement that you've made.

MR. BOWMAN: Nonetheless, that's the observation.

DR. TRIAY: That's the observation, and it has to be--and the reason it's so small is because it has to be based on artifact, not on an actual chemical potential.

DR. LANGMUIR: Maybe we should get Jim Conca into this in the sense that his pore studies are giving you a tremendous solid/liquid ratio, much, much higher than you ever see in a batch test, and if, in fact, the K_ds are the same, this is proof that it's valid to extrapolate the batch work to the tuff.

DR. CONCA: This is Jim Conca, WSU.

I actually, I mean, I want to be conservative and say I agree with that. It may just be for these kind of tuffs, where they have very high surface areas and, in fact, changing the water/rock ratio doesn't seem to change very much.

Now, maybe if it was, you know, unweathered basalt, you know, you might get very different results, but for these tuffs, that appears to be the case.

MR. BOWMAN: Also--this is Bowman again--I think it'll be interesting when you do some of the more strongly sorbing solids, because those will be a much more robust test than selenium, which only has a K_d of--or a retardation factor of two or something.

DR. TRIAY: And, Rob, those have been done, those experiments, those type experiments. We just happened to show unsaturated data, because that was a particular concern of the Board in the past. However, we have done this with saturated solid rock columns, and we have used the batch sorption K_ds for cesium, which is an extremely high number in zeolitic tuffs, and we have been able to predict what happens in the solid rock columns very well.

DR. MEIJER: This is Arend Meijer one more time here.

On the same issue, we have done experiments at different water/rock ratios, and, basically, we see decreasing K_ds with increasing, well, if you call it--decreasing water/rock ratio, then, which is opposite of what you're saying.

I've seen what you're saying in the data on organic sorption and a few other things, and from a physical chemical point of view, I've never really understood it, but there are all sorts of possibilities. I mean, you could have phases being adsorbed, or you could have crystallization on the surface, I don't know, all kinds of strange things, but the fact of the matter is that we have done experiments with these rocks, and we see the effect that Inés mentioned, and so, we don't seem to have that problem in these rocks.

MR. APTED: At the risk of stirring the pot, it looks that the approach is to take the UFA and solid rock cores, and because of its capability of doing saturated and unsaturated sorption tests, to use that as some sort of leading bench mark. I mean, that's sort of a confirmation that we can use the saturated data.

So, the natural assumption, then, from that would be that there'll be no more column tests, and so on, that all future sorption would be done in the UFA?

DR. TRIAY: The only thing that I mentioned--and I told you exactly the only things that I thought needed to be done. Let me just review that so that you can comment on whether you think that's reasonable or not.

The only thing that I said, in the future, neptunium, my belief is that we need neptunium data just on clinoptilolite and calcite as a function of pH, ionic strength, and bicarbonate. Why? To be able to extrapolate to different groundwater conditions, and so that when you see a K_d, Rob and people like you, experts in sorption say, well, where did that come from? I mean, that's a fact that that's for, and now, how do you justify that number? That's all I said that needed to be done for neptunium.

The other thing that I said was that for selenium, we needed to identify what minerals dominate the sorption of selenium. Selenium is an anion, either selenite or selenate. What is doing the sorption that is being observed in zeolitic tuffs? We have to address that issue, because if I tell you that the K_d is 14, you're going to ask me, how come? But that's the only thing that I said that needed to be addressed.

The plutonium experiments, I said that would be addressed with column experiments, not with batch sorption experiments. We have already done batch sorption experiments, and for the uranium, all I said was, look at the literature that is out there and try to, from the literature, predict the number that is observed with tuff, and if you're successful, you're done. If you're not successful, you need to address that issue by doing more batch sorption experiments as a function of mineralogy in well-controlled conditions, rather than tuff and J-13, because that is just a matter of an observation, but if you actually need to understand the chemistry to defend the K_d, you need to do it in well-controlled systems.

MR. APTED: I guess my point is, isn't batch sorption a dated technique? I mean, isn't it preferable, for all future data collection, to be done at partial degrees of saturation in a UFA, be it in a hot cell or whatever?

DR. CONCA: May I address that? Actually, to some degree, yes and no. I mean, I've actually--my confidence in batch tests has actually increased with all of this work here, and, in fact, as easy as the UFA is relative to normal flow experiments and column experiments, it's still, you know, setting up K_d batch experiments is still very efficient and very quick, and I would use the UFA to look at specific questions in this system, and once you've answered those questions, then you do a bunch of batch, you know, K_d experiments for that system, and then you move over here and you look at this system, and, you know, you want to use it judicially because, in fact, thinking of something with a K_d of, say--I'm sorry--a retardation factor of, say, 40, and you have a .1 ml/h flow rate, and you have to--

DR. TRIAY: Not in your lifetime.

DR. CONCA: Now you're looking at 4,000 hours of run, and even in the UFA, 4,000 hours, not 4,000 years, but, still, it's 4,000 hours, so you're not, you know, you don't want to use it for everything. I would think you want to use it, you know, judiciously.

DR. LANGMUIR: I promised to ask Inés a question, so I'll ask it.

My sense is--Mick was concerned about colloids as a potential basis for moving radionuclides out of the waste package. I have been doubtful for some time that they're going to be an issue at all. Inés has some ideas, I think, and some suggestions on what might be done to put to closure colloids as a potential mechanism of release of radionuclides, and I'd like to hear what she has to say about that.

DR. TRIAY: Well, as I said during the talk, I think that the first issue is to actually address colloidal stability. The reason I say that is because, first off, there is data in the literature, so almost all that you would have to do is to actually look at the colloid concentrations in the groundwaters at the site. If that actually agrees; in other words, if you can place that point on the stability diagrams that are available in the literature, you actually have a very good case that you actually can bound the total amount of colloids in those groundwaters.

Now, a lot of people are concerned, because they say, well, how about groundwaters that are not there now? In other words, you have a system there now, and most likely than not, the colloidal concentration is going to be very low.

Well, my answer to that is, again, I come to the stability diagrams, because the stability diagrams, actually, again, that is chemistry. You actually are compressing the double layer, and, for that reason, you aggregate the colloids and they get big and they settle out. If you can actually make that case for whether the colloids are near-field colloids or far-field colloids or whatever they are, based on the chemistry of those colloids, if they are clays, iron oxides, whatever it is, you actually are done. I mean, that's the end of the discussion.

DR. LANGMUIR: I suggested last year at the colloid workshop that there should be some simple experiments done in unsaturated tuff with colloidal solutions to see if they go-- and under thermal conditions, and I didn't see those as specific experiments that you had going.

Are you planning them, and, if so, when are they going to happen?

DR. TRIAY: We are actually setting up those experiments. The reason that we're having trouble with it--

and I think that we have resolved this issue--is that the best way to actually address colloid stability is by counting particles. Well, we actually had in our laboratories an ultra correlation photospectroscopy technique. That is not good enough. That actually gives results that are full of artifacts.

You need to do it by counting particles, and we actually set up a correlation with another group in our laboratory, in Los Alamos National Laboratory, that has this capability, and those results should come out very soon.

DR. DOMENICO: A question to Bob and Rally, perhaps. Something confused me.

A few meetings ago, Bob, I think Jim Duguid presented some results where neptunium was a real bad actor, neptunium's a daughter product, and you guys got Pickford's model, which handles chains. I don't believe the Sandia model handles chains. Correct me if I'm wrong. If I'm right, how did they get neptunium in there, and neptunium seems to have disappeared.

Now, as a major factor, do we have some model-dependent results? I believe there's two different models involved. Is that not right?

DR. ANDREWS: Yeah. I mean, that is true--Bob Andrews from the M&O--and in our case, neptunium is a daughter and is allowed to ingrow, and everything is allowed to ingrow as it should. In Sandia's case, they pre-calculated the neptunium ingrowth and put that into the source term directly.

Now, neptunium, to answer your second part of your question, is for longer time periods, at the larger times, neptunium still is a bad actor in terms individual doses.

DR. DOMENICO: With both models?

DR. ANDREWS: Yes.

DR. DOMENICO: I thought it was an overwhelming problem the time Jim Duguid presented it to us a few meetings ago.

DR. ANDREWS: They are very high individual doses from neptunium being driven by the solubility values, which are very high in the oxidizing environment, driven by the alteration dissolution rate of the spent fuel itself, and driven by the very low dilution in the saturated zone. All three of those things tend to increase the doses.

DR. DOMENICO: Are Sandia's results reproducible by your model, or vice versa; do you know?

DR. ANDREWS: Yeah. I mean, we did that using the TSPA-91, you know, essentially using exactly the same databases, if you will, and came up with virtually identical results.

Even in this iteration, the bottom line results are not that dissimilar, although a lot of detailed assumptions, you know, buried in there, you know, thermal dependencies or not, pH dependencies or not, are different, but the bottom line results, if you compare the CCDFs of peak dose, for example, or even CCDFs of cumulative release, are factors of half a log off, so factors of three off, and in this system, that's not significant.

Rally, I don't know if you want to--

DR. BARNARD: Oh, I just wanted to say that the million-year drinking water dose, 80 per cent of the contribution was from neptunium for both our aqueous models, and that, to echo what Bob said, the factors of importance are the lack of dilution in an unsaturated environment, and the release rate from the source term. If we had the neptunium coming out at a very slow, consistent rate, you'd get much lower doses, but when it comes booming out as a result of the modeling assumptions we made, then you'd get high doses.

DR. LANGMUIR: I'm going to ask a large question of several people, and maybe this will be the last question. I think everybody's tired enough to wrap it up here, but, specifically, we heard talks today from Ed Kwicklis, Arend Meijer, and George Zyvoloski, all of whom either modeled or discussed measurements of ages of waters in the mountain, and we had tritium data, we had Chlorine-36 data, and C-14 data, and it was clear that that data was not always consistent. It required some rather imaginative explanations of how you got some dates over and above others, when they weren't consistent with the directions you thought you were moving in the mountain.

I guess I'd like to ask all three of you how you feel about the apparent inconsistencies, and how close you think you are to understanding how the water has gotten there in the mountain and where it's coming from, and how the fluids are moving under current conditions.

Incidentally, also, apparently, the--the wrong word's "apparently" in that context, but apparent C-14 dates were being used, I think, by some of you, which is difficult to interpret. When you don't know the absolute age of the C-14, it's tough to relate these ages to each other among the isotopes.

Could I have some comments from any of the three of you, or all of you, what you think about the ages in the mountain?

DR. MEIJER: This is Arend Meijer. I could start, if you like.

It's true that, certainly, the Carbon-14 ages probably aren't ages, at least in my view. The Chlorine-36 ages are closer to being ages, except that there are problems with dead chloride in the rock that has to be corrected for. The tritium ages, they're so young, all you're seeing there is the evidence of bomb pulse at some depth, so I don't think there's that much problem with the tritium.

Now, back to the Carbon-14, my bias, I guess it is, is that the Carbon-14 is basically controlled by gas flow through the mountain, so that Carbon-14 ages of the water are almost nonsensical, and what you're really seeing is the residence time of Carbon-14 in the gas phase that this water's in contact with over the long time frame; that is, the gas phase changes, you know, with a given volume element of water.

So, the Carbon-14 elements, or Carbon-14 ages are giving you residence times of something other than what the Chloride-36 ages are giving you. I think the Chloride-36 ages are giving you residence times of the water, once you get the bugs worked out of the correction procedure.

It's also notable that the uranium disequilibria ages that have been obtained on carbonate minerals and fractures, and even in the matrix in some samples from cores in Yucca Mountain tend to corroborate the Chloride-36 ages, or Chlorine-36 ages, and uranium wouldn't be a gas phase isotope, either. So, here you have two water phase isotopes, or isotope systems that give you, essentially, the same result, and so then, it's my position that those are probably close to real ages of the water, or residence times of the water; whereas, the Carbon-14 is giving you something that reflects the gas phase.

DR. LANGMUIR: Well, aren't you really, perhaps, then, looking at--when you look at the Chlorine-36 or the uranium isotopes, you're seeing a groundwater travel time or groundwater movement rate in the mountain, which is useful information, and when you look at the C-14, you're looking at CO_2, which is a gas, and, therefore, you're looking at a pneumatic rate, which is also important to us in terms of radionuclide transport, so they're different information, but they're relevant in different ways to the issue of release in isolation.

MR. KWICKLIS: I'd like to make a comment.

One is that Arend showed data from UZ-1, which showed that about the Calico Hills/Topopah Spring contact, the age of the gas, based on gas samples, was 25 per cent modern, or 23 per cent modern, or about 12,000 years old.

If that number is representative of the time it takes for gas to migrate from the ground surface to that contact, I don't understand how a gas that old could cause water samples at the same stratigraphic horizon to be 5,000 years or 1,000 years old, so I just don't see--I think the trends are all wrong here.

In answer to another part of your question, I think that, through modeling, we understand how you can get age depth inversions for some of these water samples, and that's if you have a focused point of recharge that hits some relatively impermeable layer, and that water spreads laterally along a horizon, you're very likely to drill from older water into younger water that has entered via some focused recharge mechanism.

And so, I'd also like to add, Arend showed some C-14 data from UZ-4 and 5, along with some tritium data, and I've spent a lot of time with that data, and I see the difference in the C-14 ages between UZ-4 and 5 as consistent with flow patterns that result from focused recharge occurring in the wash, and then spreading laterally along the low porosity, low permeability layer, and I see, when I look at that data, I see a very erratic tritium profile that is more suggestive of liquid flowing laterally along certain stratigraphic layers, and not with a broad diffusive-type of flow mechanism that I think barometric pumping would produce, so I'm not sold yet on this barometric pumping method.

DR. LANGMUIR: Do the age data help us to understand the origin of the perched water that's been sampled recently, the large amounts of it, or to understand the origin of the steep groundwater gradient that we know exists north of the site?

MR. KWICKLIS: Well, where we see perched water throughout a lot of the mountain, it appears to above this--either within or slightly above this very low porosity, low permeability basal vitrophere of the Topopah Spring, and we know from examining the core, that many of the fractures in that horizon are mineralized, clay-filled, because the vitrophere of being glassy is very reactive compared to already devitrified tuffs.

And, this combination of filled fractures and extremely low matrix porosities produces a condition that it's a very impermeable and restrictive layer relative to what's seen above it, and it's very similar. As long as the recharge rate is greater than the permeability of that layer, you're bound to have perched water, eventually, above it.

As I said, there are more complex explanations for some of that perched water that have been proposed, and those involve that, as water flows from--as the depth of the water steepens in the northern part of Yucca Mountain, as the water table has a steep gradient up there, there's some lateral flow along these low permeability layers that so, essentially, you have a condition where water is flowing from the saturated zone at a higher elevation, into the unsaturated zone at a lower elevation.

DR. LANGMUIR: Yeah. I understood those things. I was more interested in the age dates of those waters relative to other waters in the mountain.

MR. KWICKLIS: I haven't seen a whole lot of other groundwater ages, other than what I shared here in my presentation this morning. I'm aware of Gene Martin's data that shows a lot of very old ages, but I'm not aware of Al Yang's data showing ages beyond about 5,000 years.

MR. ZYVOLOSKI: Basically, my comments are included in the comments that have been already made. We used our results just to get qualitative agreement, and we haven't been doing that for very long, and so, I guess what I would say is that the old, young--excuse me--young, old, young kind of thing that you see with the Chlorine-36 can be explained, at least qualitatively, with stratigraphy, and with the non-linear nature of the relative permeabilities.

DR. LANGMUIR: I'm going to close it with that, and thank all of the speakers, and the consultants, and the audience for your presence and your efforts to make this a good session.

We start tomorrow with a session on transportation at eight-thirty in the morning in this room.

Thank you very much.

(Whereupon, at 6:30 p.m., the meeting was adjourned, to reconvene at 8:30 a.m. on July 13, 1994.)