UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER 2002 BOARD MEETING

January 29, 2002

Bob Ruud Community Center
150 North Highway 160
Pahrump, Nevada

NWTRB BOARD MEMBERS PRESENT

Mr. John W. Arendt
Dr. Daniel B. Bullen
Dr. Norman Christensen
Dr. Jared L. Cohon, Chair, NWTRB
Dr. Paul P. Craig, Morning Session Chair
Dr. Debra S. Knopman
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek
Dr. Donald Runnells, Session Chair
Dr. Alberto A. Sagüés
Dr. Jeffrey J. Wong, Afternoon Session Chair

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Linda Hiatt, Management Analyst
Linda Coultry, Staff Assistant

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P R O C E E D I N G S

8:30 a.m.

COHON: Good morning. Thank you all for being here for this first meeting of the Nuclear Waste Technical Review Board of the year 2002. And it is a very important meeting, because this is an important time in the history of the Yucca Mountain Program, for, as you all know, I am sure, the Secretary has announced his intention to recommend the site to the President.

My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board. Our Board meets three to four times a year, usually in Nevada, and at least one of those meetings in Nye County. And we were taking a poll before. We believe this is the third time we've met in Pahrump in recent years, and we're very pleased to be back here.

Many of you have come from quite a long way to be with us today, and we really appreciate that, and I want to extend a special welcome to Commissioner Jeff Taguchi of Nye County, who will say a few words of welcome after my remarks.

The Nuclear Waste Technical Review Board was created in the 1987 amendments to the Nuclear Waste Policy

Act. Congress established our Board as an independent federal agency to evaluate the technical and scientific validity of activities of the Secretary of DOE related to nuclear waste disposal. We are required to report our findings and recommendations at least twice a year to Congress and to the Secretary.

The President appoints Board members from a list of nominees submitted by the National Academy of Sciences, and this is as specified in the 1987 law which created us. The Board is, by design and by statute, a highly multi-disciplinary group with areas of expertise covering a full range of issues related to nuclear waste management.

I'd like now to introduce you to the members of the Board. And as I do so, let me remind you that we all serve in a part-time capacity. In my case, I am President of Carnegie-Mellon University in Pittsburgh, Pennsylvania. My technical expertise is in environmental and water resources systems analysis.

John Arendt is senior consultant and founder of John W. Arendt Associates, Inc. His areas of expertise are nuclear materials, facilities, quality assurance and control, and inspection. John chairs our Panel on Waste Management Systems.

Daniel Bullen is Associate Professor of Mechanical Engineering at Iowa State University. His areas of expertise include performance assessment modeling and materials science. Dan chairs both our Panel on Performance Assessment and the Panel on the Repository.

Norman Christensen is Professor of Ecology and former Dean of the Nicholas School of the Environment at Duke University. His areas of expertise include biology, ecology, and ecosystem management.

Paul Craig is Professor Emeritus of Engineering at the University of California Davis, and is a member of the University's Graduate Group in Ecology. His areas of expertise include energy policy issues, especially those associated with global environmental change.

Debra Knopman is Associate Director at RAND Science and Technology located in Arlington, Virginia. Her areas of expertise include hydrology, environmental and natural resources policy, systems analysis, and public administration. Debra chairs the Board's Panel on Site Characterization.

Priscilla Nelson is Director of the Division of Civil and Mechanical Systems in the Directorate for Engineering at the National Science Foundation. Her areas of expertise include rock engineering and underground construction.

Richard Parizek is Professor of Geology and Geoenvironmental Engineering at Pennsylvania State

University. He's also President of Richard R. Parizek and Associates, consulting hydrogeologists and environmental geologists. His areas of expertise include hydrogeology and environmental geology.

Donald Runnells is Professor Emeritus in the Department of Geological Sciences at the University of Colorado. He also is a technical consultant to Shepherd Miller, Inc., environmental and engineering consultants. His areas of expertise include geochemistry, hydrochemistry, and mineral deposits.

Alberto Sagüés is Distinguished University Professor in the Department of Civil and Environmental Engineering at the University of South Florida. His areas of expertise include corrosion and materials engineering, physical metallurgy, and scientific instrumentation.

Jeffrey Wong is Deputy Director for Science, Pollution Prevention and Technology in the Department of Toxic Substances Control of the California Environmental Protection Agency. His areas of expertise include risk assessment, toxicology, and hazardous materials management. Jeff chairs our Panel on Environment, Regulations and Quality Assurance.

That's our Board.

Our Staff, you're not looking that great this morning, Staff. Generally, I'm moved to comment on their either satorial splendor or something. But there's just no way to paper this one over. You don't look very good. But they're all here, and as Bill just said, they have had a busy month getting out our award letter. Sitting at the end here closest to me is Bill Barnard, the Executive Director of the Board.

Let me turn now to a brief overview of what is a very ambitious agenda that we have planned for today and tomorrow. First, this morning, Steve Frishman will be standing in for Bob Loux, who unfortunately is snowed in in the Carson/Reno area. I was there yesterday. I guess I made it out on like the last plane, or something like that. He will give us some views on behalf of the State related to the potential siting of a potential repository--just one potential is enough--potential siting of a repository at Yucca Mountain.

After Steve, Lake Barrett, Acting Director of the OCRWM, will give a general update on program activities. And the morning presentations will conclude with a series of talks about OCRWM's scientific programs, including a Project Update by Yucca Mountain Project Manager, Russ Dyer, a presentation on fluid inclusions by Drew Coleman, and a scientific update by Mark Peters.

In the afternoon, we will have a special session on Yucca Mountain Hydrogeologic Investigations, including presentations on regional and site scale saturated zone modeling by Frank D'Agnese from the U.S. Geological Survey, and Al Eddebbarh and George Zyvoloski from Los Alamos.

Bo Bodvarsson from Lawrence Berkeley Lab with give a presentation describing new unsaturated zone modeling investigations, and we'll conclude that session with a talk by Dave Cox on results from recent Nye County well testing.

Also in the afternoon, we will have a series of presentations by representatives of groups that have commented in the on the technical basis of Yucca Mountain science. To start that session, Bill Alley, Chief of the Office of Ground Water of the USGS, will discuss a letter that the survey sent to DOE Undersecretary, Robert Card, last year.

That will be followed by a presentation of the Clark County review of the DOE's Total System Performance Assessment, which is the main analytic tool that DOE uses to evaluate potential performance of a repository at Yucca Mountain. That presentation will be given by John Bartlett of Sandy Cohen and Associates. As many of you know, from 1990 to 1993, Dr. Bartlett served as Director of OCRWM.

Concluding the afternoon's presentations, John Garrick will present the findings of the Nuclear Regulatory Commission's Advisory Committee on Nuclear Waste. Dr. Garrick was appointed to the ACNW in 1994 and served as Chair for four years.

On Wednesday morning, we are privileged to have Tonis Papp, Chairman of the International Review Team that evaluated DOE's TSPA. Dr. Papp traveled here from Sweden, and we appreciate greatly his extra effort to get here.

A discussion of regulatory considerations and developments will complete our agenda for this meeting. We will begin with a description of the legal requirements contained in NRC's licensing regulation, 10 CFR 63. Those requirements will be summarized by Tim McCartin. And Jerry McNeish from the Yucca Mountain Project team will summarize the TSPA supporting the Site Suitability Evaluation and the Final Environmental Impact Statement.

Then Peter Swift will present a report on uncertainty analysis and the strategies that the DOE might use to address those uncertainties. Finally, the meeting will conclude with a presentation on the methods and findings of the NRC Sufficiency Review, which will be presented by Bill Reamer, Chief of the High-Level Waste Branch at NRC.

Let me turn now to a letter report that the Board sent last week to Speaker of the House, Dennis Hastert, President Pro Tempore of the Senate, Robert Byrd, and Secretary, Spencer Abraham. Copies of the letter are available in the back in the corner, and we hope you'll take it, read it carefully, and draw your own views about what we're saying.

This is an important letter. In it, the Board presented its views on the technical basis of OCRWM's performance estimates for a potential repository at Yucca Mountain. And I want to take a moment to summarize for you the key findings and observations that we make in that letter.

In evaluating the DOE's technical and scientific work related to individual natural and engineered components of the proposed repository system, the Board found varying degrees of strength and weakness. And I want to emphasize we're talking here about the technical and scientific work that undergirds the performance estimates that DOE has prepared. And we found varying degrees of strength and weakness. This kind of variability is not surprising, given that the Yucca Mountain Project is a first-of-a-kind, and very complex undertaking in many respects.

When the DOE's technical and scientific work is taken as a whole, the Board's view is that the technical basis for the DOE's repository performance estimates is weak to moderate at this time.

The Board makes no judgment in its letter on the question of whether the Yucca Mountain site should be recommended or approved for repository development. Those judgments, which involve a number of public policy considerations, as well as an assessment of how much technical certainty is necessary at various decision points, go beyond the Board's congressionally established mandate. It's very important that you understand this.

The DOE has produced estimates of repository performance using Total System Performance Assessment, a complicated model which relies on mathematical representations of and data on several physical and chemical phenomenon.

Uncertainties due to gaps in data and basic understanding result in the Board having limited confidence in current performance estimates that are the products of this performance assessment model. This is not an assessment of the Board's confidence in the Yucca Mountain site. The focus is on TSPA and performance estimates. At this point, no individual technical or scientific factor has been identified that would eliminate Yucca Mountain from consideration as the site of a permanent repository.

Over the last several years, the Board has made several recommendations that we believe could increase confidence in the DOE's projections of repository performance. For example, the Board believes continued scientific investigation could increase basic understanding of the potential behavior of the proposed repository system, and, as our letter indicates, if the site recommendation is approved, the Board strongly recommends that these investigations be pursued with vigor.

Confidence in waste package and repository performance potentially could increase if the DOE adopts a low-temperature repository design. Furthermore, the Board has recommended that the DOE identify, quantify, and communicate clearly the extent of the uncertainty associated with its performance estimates.

The Board has also recommended that the DOE use other lines of evidence and argument to supplement the results of its performance assessment. Moreover, the DOE could strength its arguments concerning how multiple barriers in its proposed repository system provide "defense-in-depth." The DOE has made progress in each of these areas that I've mentioned, but more work is needed, in the Board's view.

In its letter, the Board acknowledges that eliminating all uncertainty associated with estimates of repository performance would never be possible at any repository site, including, obviously, Yucca Mountain. Policy makers, not the Board, policy makers will decide how much scientific uncertainty is acceptable at the time various decisions are made on site recommendation or repository development. The Board hopes, of course, that the information that we presented in the letter, and the attachments, will be useful to policy makers as they make these most important decisions.

Again, we encourage you to take a copy of the letter, and to study it, as us questions during breaks, tomorrow morning, which I'll say more about in a moment. We'd be happy to give you our responses. We want you to understand what we're saying.

Let me close my remarks by talking a bit about public participation, which is something that's very important to the Board. We've provided three opportunities for public comment during this meeting. There is a brief 15 minute comment period around noon, or 12:15 today. It's on the agenda. I don't remember the exact time. And I'm going to hold that to 15 minutes. We're reserving that, and I hope you'll respect it, as a time for those to speak who cannot be here at either of the other two comment periods at the end of today's session and at the end of tomorrow's session. Those sessions can be more or less open ended. No one wants to be here all day and all night, but we don't have to watch the clock so carefully as we will have to watch it today at noon. So, please be respectful of that.

To sign up to make a public comment, please see either Linda Hiatt or Linda Coultry--Lindas, would you raise your hands--sitting at that table. They'll be happy to assist you.

As always, we reserve our, or I reserve the right to limit time specifically so we can stay on schedule. But, again, I'll be much more liberal about it at the end of today and at the end of our session tomorrow.

Let me also remind you that we always welcome written comments for the record, either to supplement your oral comments or as your only form of comment. It's especially useful doing it this way when your comments are lengthy, and time will not allow them to be presented orally.

We'll have an opportunity tomorrow morning at 7:30, before the meeting convenes at 8:30, to have an informal discussion over breakfast in this room. So, please join us. Board members will be here, and it's a chance just to talk one on one about issues you've heard today, or about anything at all. We'll be happy if you'll come.

Finally, let me offer our usual disclaimer so that everybody is clear on the conduct of our meeting and what you're hearing and the significance of it. Our meetings are spontaneous by design. Ignore the fact that I've read much of what I've said. It's the last time during the meeting that you'll see anything scripted by us. And those of you who have attended our meetings in the past know that the members, and especially this group of members of the Board, don't hesitate to speak their minds. But let me emphasize that when they do, that's precisely what they are doing. They're speaking their minds. They're not speaking on behalf of the Board.

When we are articulating a Board position, we'll let you know. Otherwise, it's that individual Board's comments, views. We're happy to hear them, but they do not necessarily reflect the position of the Board.

With that, again, welcome to our meeting. Thank you for having us here in Pahrump, and I'm pleased to introduce to you Commissioner Taguchi.

TAGUCHI: Good morning. I think I'll dispense with the formalities again. I was politely chastised as I walked in here because those of you who remember last year, I commented on those who wore ties. And, again, I at this particular point, made reference to the fact that yes, I am wearing one, so I will function in the same capacity as last year. And those of you who prefer to remove your tie, may do so at your own leisure. That's kind of one of those things you get caught in your own trap. I didn't expect anybody to remember that.

Truthfully, I was in Washington, D.C. a few months ago, and somebody commented on that issue. And I find that rather amusing that someone would remember something like that. All right, I've dispensed with the formalities.

As Chairman of the Nye County Board of Commissioners, I once again welcome the Board members and Staff to Nye County. As the host county for the potential

Yucca Mountain repository, we've always appreciated the Board's commitment to meet once a year among the people who will be most directly and permanently affected by any decision to the site repository here.

We feel, actually I feel that our speech writers do a pretty good job at conveying Nye County's message, and also add a little bit of intellectual promise to the speech giver. So, this morning, what I'm going to do is I think those of you who are old enough, I think I'm going to pull a Barry Goldwater on them. If you remember Senator Goldwater, certain eccentricities, his staff didn't know what he was going to say, and would always caution him over his remarks. So, I will tell my staff that the intent of the message will still be there. That's one of those eccentricities I have, and they're well aware that I change words around.

What's funny is is that during one speech in Washington, D.C., I just kind of augmented the speech, and those augmented quotes ended up in the Washington Post and the Las Vegas Review Journal. Funny.

But anyway, let's face some facts. You know, the complicated social and scientific issues affecting our communities need to be examined very carefully. Yucca Mountain is going to have an effect on the local communities, and these issues need to be addressed, as well as the site itself. These effects will be cumulative as time progresses as the population of this county grows. And since you were here last, the population has probably increased roughly 6 to 8 per cent. So, you're looking at a different Nye County than you were when you were here in Amargosa last year.

New economic endeavors associated and disassociated with the potential future repository are going to be of critical concern for all affected parties. And the need for critical review on all of these issues is of paramount importance in my purview.

This Board, the Nuclear Waste Technical Review Board, and Nye County, the State of Nevada and others must have continued oversight of the DOE program at Yucca Mountain. In other words, no sunset clauses. The Secretary's announcement has provided Nevada Bell with more phone traffic than a Los Angeles freeway at rush hour, and with the President's looming approval of the site, magnifies the importance of the discussions you will have over the next two days.

Any discussion concerning the letter that Jared has read to the Secretary and Congress of January 24th is of particular interest to my staff and me, because we are looking forward to hearing some of those issues presented in the format that you have outlayed here.

Nye County has appreciated the opportunity to share our scientific data with you. As you know, our independent science investigation program is conducting the Early Warning Drilling Project and the Alluvial Tracer Complex study, and Dr. Dave Cox will bring you an update on our most recent well testing work. And Dr. Parviz Montazer would like to share with you his ongoing work on an alternative conceptual design for a ventilated repository on an informal basis during one of the public comment periods.

Nye County has remained neutral in its positions concerning the facility, but Nye County's commitment to its residents has revolved around three specific issues: the health and safety of all Nye County residents, the method and mode of transportation of waste package, and the economic structures that are needed to support such a project.

Your discussion this week will send a message to the citizens of Nye County and its residents, the State of Nevada, and to this country. And, so, what kind of message will that be? That's what we're looking forward to hearing.

Again, on behalf of the Board of Nye County Commissioners, welcome to Nye County, to Pahrump. We hope you enjoy our hospitality here and our facilities. I'm looking forward to hearing what you have to say this morning, and tomorrow. Thank you very much.

COHON: Thank you, Commissioner Taguchi. Thank you very much.

As I said in my opening remarks, Steve Frishman will stand in for Box Loux. Bob is from the Nuclear Waste Project Agency. Steve, please, you're on.

FRISHMAN: Thank you. For the record, I'm Steve Frishman. I'm representing Bob Loux, who is Director of the Nevada Agency for Nuclear Projects. Bob has asked me to convey his apologies for not being here, but he is having a very difficult time even getting out of Carson Valley with the snowstorm late yesterday afternoon, and on a plane later.

We're at a point now where the meaning of the Board has become really a focus in this Program. I view your role as informing an extremely important policy decision, and I believe that your letter report has fulfilled that requirement.

The Governor has responded to the Secretary of Energy on his letter of intent to recommend the site. The Governor is particularly disturbed about the fact that it had little to do with site suitability. It had much more to do with other issues, all relating to security in one way or another, and there has never been an evaluation of the Yucca Mountain Project versus an issue of national security or energy security. So, we're in a situation where we have to question whether the perceived need on the parts of some people is a justification for any compromise in safety. And we believe that that is not the case, that Yucca Mountain site suitability has been an issue since the writing of the

Nuclear Waste Policy Act, and suitability has been a word that has been bantered around for many years, and its meaning has become prescribed by the Yucca Mountain site.

So, we're in a situation now where the Department of Energy and the Secretary of Energy are claiming that a site is suitable based on a notion that the site itself helped to invent. Up until about 1995, site suitability meant are the characteristics of the site such that we can achieve geologic isolation of high-level radioactive waste.

Since about 1995, suitability has been can we invent a system that compensates for the fact that the site can't meet that requirement. So, we're in a situation now where the Board's information to policy makers is very important, because the policy makers back in 1982 laid out a policy for geologic isolation of waste, and now the Secretary of Energy is in the position of trying to make a decision on a different policy. That different policy being can we engineer a system that will isolate waste long enough to meet an artificial regulatory compliance period. And, yes, maybe it can be engineered, but that's not what the policy required.

So, I think it's very important that you in your letter have talked about the natural barrier, and talked about the information that is lacking, the information that is uncertain, some that can never be any more certain than it is, and also, in a way, directed the Department to go back and look and define the natural barrier as well as it can, so that we can all then understand whether we are dealing with a repository that meets the existing policy requirement, or whether we're dealing with a federal or national decision that meets the capability of Yucca Mountain, and, by the way, is somewhat attuned to someone's perception of the need to have Yucca Mountain because there isn't anything else on the list.

So, we take this situation extremely seriously. We are gratified that the Board has met our expectations in terms of looking at the technical validity of the Department's work, and we're going to do our utmost to make sure that the policy of the nation is upheld. And as you all know, we're going to be doing that both through our somewhat unique methods of persuasion that we have been involved for all over the world probably, but also through the courts.

And one of the cases that we're going to be making in court is that the Project, as it is apparently going to be recommended--it seems pretty clear that the Secretary made up his mind even before he came to Yucca Mountain for an hour and a half and kicked the tires--it's pretty clear that the Secretary is going to make the recommendation, and what we're going to do, among other things, and it's already in progress, as you all know, is we're going to challenge whether that recommendation decision is in tune with the national policy. And we believe that in a fair test, that it will be found to not be in tune with the national policy, and if this nation wants a policy that is dictated by the capabilities of Yucca Mountain, then the Congress needs to make that decision in an open and proactive way, rather than in a default.

So, I guess that message is clear, and when the recommendation is made, because we believe it probably will be, then you'll see that we're going to be turning literally everything that we have to trying to keep this nation from making a mistake that, first of all, is permanent, second, sets an example to the rest of the world that this nation cares more about its interests in satisfying economic needs, satisfying perceptual needs, than it does in satisfying the basic premises of democracy.

So, that's where we are. Thank you.

COHON: Thank you, Steve. Questions from Board members?

Steve, I have a question that's sort of a technical policy/legal question. This key point that the State is going to be pursuing about whether a site recommendation based on what's known as consistent with policy, is that--let me make a statement, and then ask you if it's right.

That seems to be based on the old siting guidelines, and the argument then is that the new siting guidelines are not appropriate. Or are you saying even under the new siting guidelines, you don't believe the recommendation is justified?

FRISHMAN: We're saying under the Nuclear Waste Policy Act, the recommendation is not justified, because the Nuclear Waste Policy Act made it very clear that when you are looking to geologic isolation of waste, that the geology, and as it encompasses everything, is primary, and the Act used the word primary.

COHON: So, you don't need to argue that the new siting guidelines are inconsistent with that Act to make that point?

FRISHMAN: We argue that as well.

COHON: Okay. But that's sort of a parallel argument in support of your first one, but the first one doesn't rely on the second; is that correct?

FRISHMAN: The first does not rely on the second. We read the Act, and the Act laid out what was the intent of Congress and what was the intent of many of us who were involved with states and other parties in the evolution of thoughts that led to the Nuclear Waste Policy Act of 1982.

COHON: I understand.

FRISHMAN: The guidelines are a result of the requirements of Section 112(a) of the Nuclear Waste Policy Act. So, we have a policy argument here, and we also have an implementation argument, which is the 960 versus 963 guidelines.

COHON: Debra Knopman?

KNOPMAN: Knopman, Board.

Steve, could you comment about the State's position on the technical scope of the Environmental Impact Statement? And does that come into play here?

FRISHMAN: That will come into play, and we have seen, and, well, you've obviously seen our written comments on the Draft Environmental Impact Statement, and we find it to have fatal flaws. And in case there's any doubt, if the final comes out looking anything like the draft in terms of the fatal flaws that we pointed out, that will be the subject of another lawsuit. And the technical basis of it is, in many ways, already obsolete. What is described as the proposed action really isn't the proposed action anymore in terms of even a first level of detail.

The no action alternative is a hoax, because it doesn't represent an action that any responsible person or government would ever undertake. And it will be challenged on that basis, and the technical content of it, as it describes a repository, was only the repository de jure. It isn't anything like what we're thinking about in terms of evaluating the latest information as you were sort of forced into doing, waiting until November to make a statement in January.

So, the Draft Environmental Impact Statement didn't really describe the project that we're even thinking about today, and probably doesn't describe the project that we'd be thinking about a week from now.

So, one of the things that we've been looking at, and we have asked the question of the Department, we have an answer from the Department, regarding what is the meaning of this final Environmental Impact Statement when it comes out. And we have a statement from a representative of the Department that the final Environmental Impact Statement will not even be accompanied by a record of decision, which means that it is not a final Environmental Impact Statement. The National Environmental Policy Act lays out that the record of decision is the legal document, the final impact statement is incorporated into that.

But for some reason, the Department has made a case to us that the Secretary's decision to recommend the site is not a final decision. Well, this is bogus. Read the Nuclear Waste Policy Act. And the final Environmental Impact Statement is a key piece of the Secretary's decision, and we're going to require that the Secretary have a final Environmental Impact Statement that in fact describes what he is recommending, rather than what was the, as I said before, repository de jure at the time that the draft was written.

COHON: Priscilla Nelson?

NELSON: Steve, from your perspective, does the State reject the Department's argument for geologic isolation as being demonstrated, or does the State reject Yucca Mountain as a site capable of doing geologic isolation? Can you help me to understand and separate those issues?

FRISHMAN: We reject the site, because it is incapable of meeting the requirements of geologic isolation.

NELSON: So, you reject the site and, therefore, DOE's characterization of the site as one offering geologic isolation would not be possible?

FRISHMAN: DOE is offering a platform for engineered isolation, and that's essentially what Yucca Mountain is. And, so, we reject Yucca Mountain as a site because it does not meet the needs for geologic isolation. It's just a place to put a metal container.

COHON: Thank you very much, Steve.

FRISHMAN: Thank you.

COHON: We'll now hear from Lake Barrett, the Acting Director of OCRWM. Lake?

BARRETT: Thank you, Jared. Good morning, members of the Board. I have to admit I'm the one that spoke to Jeff about his tie this morning, because I will tell you that we at DOE, when Nye County speaks, we listen and we do remember.

I appreciate the opportunity to update you on the events since we last spoke to you in September. Many things have happened, but the most significant one occurred on January 10th when Secretary Abraham notified the Governor and the State Legislature of Nevada of his intention to recommend the Yucca Mountain site to the President for development as the nation's first geologic repository for spent fuel and high-level radioactive waste.

If the President decides to recommend the site, the State of Nevada will have the opportunity to disapprove the recommendation, meaning that Congress will ultimately have responsibility for designating the site for development, the next stages, or determining another unknown societal course of action for the responsible management of this nation's spent nuclear fuel and high-level waste.

The Secretary's notification comes after an extensive process of review and consideration of the body of scientific information that we have collected and analyzed during the 20 plus years of site characterization. As recognized by the Board in your letter, it is a matter of policy as to whether to proceed with site recommendation while the remaining uncertainties in the estimates of the repository performance are further evaluated.

We agree with you that eliminating all uncertainties would never be possible for any repository site. The Secretary, after his considerable personal review, believes that the science is sound and the site is technically suitable, and should continue into the site designation process under law.

The Secretary also cited compelling national interests to complete the siting process and move forward to determine if this will be a suitable site. Those interests include the importance of a repository in our national security, the secure disposal of nuclear waste, our energy security, and our efforts to protect the environment throughout this nation.

We agree with the statement of the Board that "no individual technical or scientific factor has been identified that would automatically eliminate Yucca Mountain from consideration." We also agree that our technical work is not finished and the ongoing course of research is appropriate to ensure the safety for the citizens of Nevada and the nation. This research, as contemplated by the Secretary and also by you, should reduce the uncertainties and increase the confidence in the long-term projections of repository performance.

If Yucca Mountain is designated as the repository site, such research would last throughout the construction, operating and monitoring periods, as much as 100 to 300 years after its opening.

If the repository development process moves forward, we will continue to evaluate issues that the

Department, the Board and the NRC identify. We specifically agree on the recommendation in the latest letter to continue a well-integrated scientific investigation to increase our fundamental understanding of the potential behavior of the repository system.

We will be continuing to investigate the performance analyses sensitivities and uncertainty impacts associated with our future design and operating mode decisions. We understand your issues associated with our technical program basis, and our work plans prioritize the actions to address the key uncertainties based on performance risk, and we believe these efforts will adequately address the issues in your letter.

Our goal is to develop a flexible repository design that can evolve with advancements in understanding and analytical capabilities inherent with a multi-decade program. Accordingly, we are explicitly preserving the ability to select, from a broad thermal range, a design for repository licensing and initial operations. We are continuing to develop a flexible design concept that would have sufficient technical basis for a license application.

We recognize that maintaining this flexibility will require further testing and analytical efforts for the lower end of the thermal range. In order to prepare for licensing, we are expanding our work related to uncertainties. These particular areas will include:

The continuing theoretical and experimental program on waste package passive film corrosion, to better understand the underlying fundamental scientific processes.

The continued review and modification of the Performance Confirmation Plan to address performance uncertainties far, far into the future.

Continued modeling activities to further incorporate multiple lines of evidence for processes that affect long-term performance.

Performance of additional uncertainty and sensitivity analyses to better understand the major contributors to long-term performance.

And continued review and validation of the parameter ranges and features and events and processes screening to ensure additional insight into total system performance.

These analyses will be used to supplement information on a lower-temperature operating mode, and the updated results from the testing programs will be used to expand the technical basis for the lower-temperature end of the flexible design.

Our ability to perform the desired technical and scientific work continues to be constrained by funding. While the President has supported increased Program funding, we rely on Congress to make the final decisions to fund the important research called for by ourselves, the Board and the Nuclear Regulatory Commission.

This year, Congress appropriated $375 million, a significant shortfall of $70 million from the President's request for this year. Of this funding, nearly $300 million will be used for the Yucca Mountain project testing, evaluation, and license application development activities. A small amount, approximately $4 million, is earmarked to initiate transportation planning and preparation for that endeavor, should it occur.

Next Monday, I will be able to share with you details of the President's 2003 budget request for this Program. At this moment, all I can say is the Secretary and the Administration will strongly support a continuing comprehensive scientific and technical program to ensure public health and safety for the citizens of Nevada and this nation.

Last year, in response to repeated funding shortfalls over the past several years, and especially this year, and in anticipation of the situation in the future, we began a process of evaluating and identifying the scope and schedule impacts on the body of additional work to support a license application. Our management operating contractor, Bechtel-SAIC Corporation, is developing a revised baseline that will include the work supporting a submittal for a license application.

The revised work plan and schedule will focus the project on the work needed to meet our goal of submitting the potential license application to the Nuclear Regulatory Commission in the 2004 time frame, and sustaining our potential ability to receive material from sites around this nation at a facility in 2010. The revised baseline for developing the Yucca Mountain facility is a careful balance of the technical, legal, institutional, managerial, and fiscal constraints on a complex program of this size.

We are also currently awaiting the National Research Council's report on the design and operational strategies associated with the concept of a staged geologic repository facility. We expect the report to be completed later this spring. Thus far, stepwise development for a geologic repository facility, with the design and operational flexibility and reversibility, coupled with a continuous learning feedback loop, has shown promise that could be extremely important for maintaining confidence for this first-of-a-kind program.

We are also awaiting the confirmation of Dr. Margaret Chu. She has been nominated by the President to be the director of this Program. I would admit many of you may know her from her scientific work at Sandia National

Laboratories. It is our hope that her extensive talents and energies will be available to this Program soon.

In closing, we have reached a key decision milestone point after more than 20 years of study. I am extremely proud of the work of the thousands of scientists, engineers and experts have performed over the site characterization phase of this Program. If this Program is allowed to continue, I am confident this team will serve the citizens of Amargosa Valley, Nye County, the State of Nevada, the United States of America, and the global community as a whole very, very well.

I also believe that continued constructive views of this Board has made our technical program stronger than it was, and you have been an asset to this Program in your comments over many years. I would also like to extend gratitude to you, the Board members, and your staff for many years of dedicated, exceptional work. It has been a pleasure to work with you on what I believe is a significant first-of-a-kind endeavor that is addressing a very, very important worldwide societal need and responsible management of this material.

I thank you for your contributions, and I would address any questions you may have for me.

COHON: Thank you, Lake. Questions? Dan Bullen?

BULLEN: Bullen, Board.

Lake, you mentioned some of the funding constraints associated with sort of the change. If there's a recommendation and if you proceed to license application, how are you going to balance that big emphasis on changing to the we've got to get the license application in versus we have to continue the baseline science and the baseline fundamental development, as outlined both in our letter--and, by the way, thank you for the kind words about the continuation of the scientific work, because I think that's very important. But I just wondered how do you do that balance now that the emphasis would shift toward license application, and that's more engineering as opposed to science? Could you comment on that?

BARRETT: Well, we don't see them as separate. They're going to be integrated together, integrated science. The natural science, as any engineering, work together in an integrated system. So, you can't just do one and not do the other. Yes, there will be more of a shift to bring along more of the engineering that we've had to defer over the last several years, but pre-closure engineering we need to accelerate.

But we are also going to continue a very substantial scientific program as well to address the Nuclear Regulatory Commission key technical issues that you've heard about. We need to continue work there. But it will be more focused work on the safety case for a license application. But it will be a balanced program. You cannot just do all engineering, you can't do all natural sciences. It's difficult.

I am very pleased with the support we've gotten so far within the Administration. The numbers that will be announced next week I think will show that. But it's premature for that. We did put our report out last summer, alternate means of financing and managing the Program for the future to Congress, which talks about freeing the rate payer funds that are paid into the government treasury, you know, for use in this program. If we can work that out, you know, within the Congress, if the site, of course, is approved. I believe there will be sufficient funds to do a job that we can all be proud of on an integrated science program.

BULLEN: Bullen, Board.

Along those lines, just one more quick question, and that was you did mention that last year's budget had sort of a very small amount of money for transportation, and I guess you can't tip your hat yet at what next year's budget might have. But transportation is an issue that's very important to the people in this county, and so I just wonder if you might want to comment on the types of studies or types of information that you'd need for transportation. Or is that just a nationwide issue?

BARRETT: It's very important within Nye County. It's very important in the State of Nevada. And it's also a very important issue nationwide. And it's also a worldwide issue as well. It's not well understood that today in Europe, as much fuel as is being moved in Europe today, as will be moved when this program is running in full capacity ten years from now. So, it is being done, and it's being done successfully, you know, within the industry.

What our plan would be is to basically use private industry and the industries that exist and build on that. We have a draft and request for proposal on our website which lays out our basic business plan to do that. What we are presently looking at is how we can best modify that and improve that with the experiences we've had in the last five years with that, and a better integration basically of the states and local and public safety aspects into the national program. And also the siting, once the siting is decided under the Act, then routing within the State of Nevada will be an issue that we basically would want to engage Nevadans to basically primarily say what would be the best situation for routing intra Nevada.

BULLEN: Okay, thank you.

COHON: John Arendt?

ARENDT: Arendt, Board.

I have two questions. The first is when you speak of sound science, what do you mean by sound science? And, secondly, what is the status of Dr. Chu's confirmation?

BARRETT: The last one is easy. She was nominated by the President. She cleared the Energy Committee. She now awaits floor action by the Senate. This is a time-honored tradition back to the time of George Washington to basically torture nominees, no matter what they are, kind of thing. So, we just have to wait until the 100 Senators decide it's okay and/or a decision is made there in the political room.

Regarding sound science, we have sufficient information scientifically, sound science, for the step we're about to take. We are not in a situation today where we are sealing a repository up and walking away in an irreversible situation. We're nowhere near that. We are at a situation now, we believe, there is sufficiently sound science to make a site designation to go to the next step, which is a political process. The Governor has the right to disapprove the site, or the State Legislature, and it may be disapproved. It's a political decision that will be made, but you need to have sufficient science to start that process.

Then there's another step for license application. We have scientific work to do for a sufficient license application. So, it is not sound enough today for a license application, but we believe we can be tomorrow.

Then for receipt of material, nominally 2010, there will be another demonstration, and the science will be sounder yet. And then in the monitoring period, you know, have sufficiently sound to receive it, and to go to the next steps.

So, it's sufficient information for each step of the process, because this is a staged process. We believe that it's sufficiently sound for this step after, you know, almost $4 billion of study and 20 years. Others may have a different opinion. The Board, I think you've spoken very clearly in your report how you saw it, and there is never zero uncertainties. So, how certain must it be? How much uncertainty can you tolerate is basically a call, and then review of the Secretary, after his review of this, does he believe it's sufficient at this step?

COHON: Debra Knopman?

KNOPMAN: Knopman, Board.

Lake, in your statement as you give the list of research areas that you intend to pursue, I believe I heard you say something about further understanding of sub-systems behavior. Is that correct?

BARRETT: Yes.

KNOPMAN: Does that mean that you're going to do the what we've called the "one on" analysis now?

BARRETT: We are looking at that, and we haven't gotten the results from Bechtel, as they are struggling and balancing this with our existing funds that we have in 2002. How much of that we're going to do right now, I don't know. At the end of the month, or in March, they're going to come in with their proposals. I don't know how much of that is going to be done now. We're going to be looking at that, you know, more as we go forward. I'm not sure, the jury is really not in yet on the balance.

KNOPMAN: Do you think it should be done?

BARRETT: Do I think it should be done?

KNOPMAN: Yes.

BARRETT: I don't know. I mean, I think there's value in doing it, and it's an issue of how much--if we had gotten our full budgets, we would have done it. Okay? I think EPRI has done some of the work, that you're well aware of, and I think there's value in doing more of that. So, I don't know if it's going to quite make the cut.

KNOPMAN: Maybe by way of explanation for the audience, what has been referred to as "one on" analysis, means that you look at the behavior of the system, adding barriers, adding engineering, adding different processes, one at a time, to gain insight into the workings of individual components, as opposed to looking at the whole complex system at once and one at a time, taking something away to try to understand what the value of that barrier might be. That's called, what I just described, "one off." The Board has recommended on various occasions a "one on," that is, starting with the system just as it is without the engineering, and gradually adding things one at a time to gain insight into sub-system behavior.

BARRETT: And there is value in that.

COHON: Jeff Wong?

WONG: Jeff Wong, Board.

Lake, as Steve Frishman earlier claimed, that the basis for the decision to move forward is inconsistent with the demands of the Nuclear Waste Policy Act, do you have any official or personal views, or responses to that particular claim?

BARRETT: Well, we're getting into legal challenges. We have multiple cases before the courts now. I'm an engineer. I am not a lawyer. And I would say that our counsel and the Department of Justice, as we've presented what we've done, are completely comfortable that we are complying with the law and the intent of the law as it is. And I'll leave it at that.

COHON: Priscilla Nelson?

NELSON: Sort of in followup, the question about the demonstration of geologic isolation as opposed to the preeminence of the waste package in terms of the outcome of TSPA, and as a tool for sensitivity analysis of importance, Steve's response indicated that, I interpreted it as the overwhelming predominance of the waste package in the TSPA and in the analysis does not satisfy the sense of some requirement for understanding the natural processes, independent from what the waste package is doing. And that sense of balance, I noticed in the string of activities, you had technical, legal and other things that you're kicking into now, the sense of the natural system wasn't there, unless you would include that in technical.

This question is one which continues, and it really is very important to the State. With the focus directly on LA, though, and with the TSPA as the tool and the waste package being the predominant entity in providing isolation during the regulatory period, it's going to take--natural systems may well take a back seat. When you say balance, how are you going to achieve balance regarding this in the Project?

BARRETT: Very difficult to do. First of all, technical to me, the way we look at it in the Program, is a combination of both the engineered and the natural sciences. So technical covers both of those. It's not just, you know, engineering, science and engineering. So, we constantly are balancing the work we're doing in both of those to try to get a balance, and they will see-saw a little bit as we go along. We must demonstrate regulatory compliance. The waste package is an important part of it. The natural system is as well. We can't have all our eggs in one basket or the other, and we try to have the balance.

Initially, this Program back in the Eighties and the early Nineties, it was 80, 90 per cent on the natural sciences, as I think it should have been that. And then we basically tried to shift, we've added to it the best available technology, talking about after the '92 Act and EPA standard, the best available technologies.

What we're looking for is to build the best system that we could at Yucca Mountain, and the Board was part of that back in the early Nineties, where the Board recommended, I don't know if any of you members were there at that time, but basically looking at the more robust waste package. And we started to do that as well, and we now in our projections, although they are estimates, we're coming in several orders of magnitude below the regulatory standards.

But we're not just comfortable with that. We still want to continue to look at the uncertainties, and we will continue to work in the license application on both natural and the engineered side, and the work that Bechtel is now doing is they're re-balancing the scope of the LA to have the right balance of natural science and engineering. But there is more of a shift as we're adding more of the engineering in now, but we are keeping a very strong top on the natural.

But it does turn out to be a judgment.

One of the things in the next meeting, we'll have Bechtel--done that, and I think you will be quite impressed with some of the work that Bechtel has done, sort of looking at the various inputs in as we're trying to basically make the management decisions about how much money goes to unsaturated versus saturated versus stable film versus manufacturing capabilities--with the waste packages, to try to balance that program out.

NELSON: Nelson, Board.

Just in followup, I guess maybe the focus comes down to the soundness of the natural science, and the soundness of the engineered barrier science and engineering. When the TSPA, as it's constructed now, is the tool and the waste package is there, it's very difficult to, with clarity, view the soundness of the natural science as it impacts on geologic isolation. So, the offer that you just indicated, that Bechtel would come and show us that this is important to them, and that they're working to achieve a balance there, is important, and I appreciate that, but the Board has asked and I think the international review panel has also asked for this idea of an understanding of the natural system separate from the waste packages being really a fundamental underpinning of that soundness of science appreciation.

BARRETT: The "one on" that Debra was referring to actually do that, and we've talked quite a bit about that. And it's not our intention to just mask the natural with a very good waste package. That's not what we want. That's not what we want to do.

NELSON: But I must admit, honestly--Priscilla Nelson talking, Board--that the number of times that something has seemed important and it doesn't show up as important in the TSPA sensitivity analysis, is a source of continuing wondering for me in some areas. I appreciate it.

COHON: David Diodato?

DIODATO: Diodato, Staff.

Lake, you talked about the idea of delivering the license application to the Nuclear Regulatory Commission in 2004, in that time frame, and there are other agreements I guess with the NRC at this time to come to closure on some of these key technical issues, 290-some key technical issues. Is the schedule for LA consistent with achieving closure on all those issues at this time, I mean, the agreements as they stand now?

BARRETT: This is a key part of the license application scheduling that Bechtel is doing, is to address all of those key technical issues, as we said we would. The details of that, and the balance of that, we're working that all out for the balance for the rest of '02 and '03. It also depends very much on how successful we are with our '03 budget presentation. So, yes, that will be in, and the key technical issues for the NRC is a very critical driver in the scheduling.

COHON: Richard Parizek?

PARIZEK: Parizek, Board.

Just a point of clarification. There's obviously studies that could continue in the engineering, and studies could continue in the natural science area. On the other hand, there's a question of what reliance do you put on the natural barrier. You're not saying that you could put the waste anywhere, given that robust waste package? We've often heard that statement by various people. You're saying there is credit in the mountain, and your program reflects that, and you wouldn't necessarily agree with Steve Frishman's view that there is really--not doing you any favors in that mountain.

BARRETT: Absolutely. They have to go together.

PARIZEK: And to that extent--

BARRETT: You cannot rely on one.

PARIZEK: To that extent, you'll continue the natural science investigations that underpin that conclusion?

BARRETT: Yes, absolutely. The natural science is an important part of the program, and will remain so.

COHON: Lake, I have a statement, and then a question.

The statement builds on some questioning by two of the Board members before, and you can respond to it, but it's not necessary to do so. And that's the concern that I have, and I think it's reflected among most of the Board members, that as the Program shifts post-SR in its focus, that the effect of that will be to concentrate the science program in a way that is very much driven by what's needed for LA, and then presumably after that, performance confirmation.

And I'm not questioning whether that's the right strategy or not, but the concern is, and one of the things that one cannot, should not forget about is in looking at such a complicated system that has to perform over such a long period of time, there are potential surprises, so-called unknown unknowns, things you can't fathom right now. And the more focused you are in your investigations, in my view, the less likely you are to detect potential surprises when you want to detect them, before they matter.

So, that's--I'm not sure what one does about that, other than one possibility is to make sure that all that DOE does is subject to very rigorous and intensive review from outside from all sorts of different quarters, people with maybe ideas that are totally out to lunch, you might think, but they can be very productive and creative.

The question. In your statement you made reference several times to things in our letter with which the DOE agrees. Are there things with which you disagree in our letter?

BARRETT: As a judgment issue, I disagree with the weak to moderate view. That's my opinion. I understand your opinion on that. But I don't think it is weak. That's my judgment on the science. Basically, I think as you go through on the strengths and the weaknesses, I would, again a judgment call, but I'm perfectly satisfied and I think you did an outstanding job in your report. I think there could have been a little more discussion on the strengths versus the weaknesses. But nonetheless, I'm sure the State will tell you the weaknesses needed more work versus the strengths. But overall, I think your report was very fair and very well done.

COHON: Other questions for Lake? Don Runnells?

RUNNELLS: Runnells, Board.

Lake, this question that keeps coming up about the role of the natural system, it seems to me that in the Repository Safety Strategy Report, there was a graph that essentially was a "one off" graph for the natural system that showed that its role in reducing the doses was something like six or eight or ten orders of magnitude. That's something we don't often hear about. It seems to me that that would be something that would be something that would be a concrete kind of response to the question does the natural system play any important role. Am I correct in recalling that? And it is of that large a role, isn't it? I mean, it's many, many orders of magnitude.

BARRETT: Yes. The natural system at Yucca Mountain is a very good system, despite the report that you might hear about. You know, what we're trying to do is to have a system that provides a very good margin of safety, you know, for the entire system. So, we are going to look at the--we are looking, we have looked a lot at the nature, and there is a lot of contribution from the natural system.

When we had the first big budget cuts, when the budget got cut in half in '95, we had to make a lot of very hard choices as to what we'd do with what resources we had, as we did that 800 person layoff back then. And some people argued the time is basically to stop the natural science work and just go to the waste package and the titanium drip shields, and that sort of thing, and we chose not to do that, because we felt that was going too far with not a balanced program. And we basically struggled with that.

But nonetheless, there is a lot of evidence, and we have not gone out and, as we say, spotlighted the natural aspects of it, because it gets more into a presentational part as it does to the fundamental science. And the TSPA number, we're not satisfied at all if the number comes out to be 2 per cent less than what the regulatory standard is. That doesn't mean you're home free at all. I mean, the whole defense-in-depth concept, you know, alternate lines of evidence are going to be necessary and required in the licensing process.

So, the black box TSPA is not the end-all, and we try to keep a proper balance that TSPA is necessary, but insufficient, you know, to successfully finish this. And when we get into the presentational aspects, I would like to be able to have clear presentational materials to counter charges that, you know, you could put the waste package in the--you know, Yucca Mountain leaks and Yucca Mountain is a bad site, et cetera, and yet we have not spent resources really in the presentational aspects of it, and in many quarters, it would be helpful to have it, and we don't have it as crisply as we would like it. And the "one on" might do that. That's why we are thinking about it.

COHON: I want to just pick up on your last comment about TSPA being necessary but not sufficient--my words, not yours. But that was the thrust of it, something with which the Board of course strongly agrees. And echoing some comments earlier when people made reference to the international review group of TSPA, also I think there was an ACNW committee, one of the things that comes out of there is the importance of understanding the repository system as a system, and not just demonstrating compliance. The latter does not necessarily imply the former.

And this idea of understanding the repository as a system really integrates and brings together Priscilla's concerns especially, but not just her's, about the natural system. You just heard from Don as well, and others, and my issue also about the unknown unknowns, anticipating surprises. The better and deeper, the richer the fundamental understanding, the better positioned the Program is to anticipate issues like that.

BARRETT: A few years ago when we started with the monitored geological repository, we changed the name and we ended in a substantial monitoring--it was that in mind, to allow more time for science to look at these things so we could have more confidence in our--see them go forward. So as part of our plan in making this reversible stepwise was to bring that component in, because we don't, you know, on the issue that technological arrogance that, you know, you know all the answers and you're going to do this, that's not here, and we're not there. But we think we have sufficient science for the step that we are at, you know, in the scheme of things in this nation.

COHON: Let me observe that if all goes as you plan, this is likely your last appearance before the Board in your current capacity. I have to, as the Chairman of a Board that is fiercely independent of DOE, I have to be measured in what I say at this moment, but indeed I think your appreciation of what the Board is and the fact that it is independent of DOE and must remain so is one of the most noteworthy things I think that you've contributed to the Program, from our point of view. So, that's probably as much as I should say, but on behalf of the Board, we thank you for all that you've done, and we congratulate you.

BARRETT: And I thank the Board.

COHON: Thank you. To show you what a caring chairman I can be, you have an extra minute by that clock. We have a 16 minute break, until five after 10:00.

(Whereupon, a recess was taken.)

COHON: Please take your seats. We're about to reconvene. For Board members benefit, I want you to know that this publication at your places is given to you compliments of Sally Devlin. And though I don't believe she's a shareholder in Saddle West, she also wants us to know about the two for one lunch special at Saddle West today. Apparently, there are coupons just outside the door there for those who want to take advantage of it.

Our next session will be chaired by Board member Paul Craig. Paul, you're on?

CRAIG: Russ, you're on. And the procedure is that we are required by law to end at 12 o'clock, Jerry's law, for public comment, and we will do that. So, you are scheduled for 30 minutes, 20 minutes of talk, and I will warn you when you've got five minutes to go.

DYER: Fair enough, Dr. Craig.

Okay, let's go ahead and get started. Next slide, please. I'm going to set the stage for some of the scientific and technical talks that will follow, but I'm going to also talk a little bit about some other things and Project status. I'll cover some of our recent accomplishments, Project path forward, touch on a technical issue, and then talk about some of the evolution of the Project that lies before us.

Recent accomplishments. Of course, as has been alluded to several times today, our mission, the Yucca Mountain Site Characterization Project mission was to provide a technical basis for the national decision regarding the development of a repository at Yucca Mountain. That has been provided.

To echo Lake's comments, we absolutely appreciate the Board's participation and contributions, particularly those instances where the Board's insights and observations helped us to develop a more robust technical basis.

This is a busy diagram. It's probably better in the handout. This is the document hierarchy that we've talked about over the years, with supporting documentation down at the bottom, the process model reports, and below those, the analysis and modeling reports, and below those, all of the data reports, the Draft Environmental Impact Statement, some of the documents that came out in the spring and summer. And then at the top, or near the top of the pyramid here, there are a couple of things, the NRC sufficiency comments of course are in place. We've completed the fee adequacy and TSLCC. Other documents are part of the decision basis for the potential Secretarial recommendation, and the potential Presidential recommendation. So, this document hierarchy has been filled in over time.

What lies before the Project here? Well, we will continue to support the SR process until the final determination, either affirmative or negative, on site designation. Should the site be designated, the Project is planning to prepare and submit a license application.

We have some major work activities that will lead to development of a license application. These include addressing the 293 agreement items reached between the Department of Energy and the Nuclear Regulatory Commission on the NRC's Key Technical Issues, which are mapable into the process of modeling report organizational scheme that we use. Continuing pre-licensing interactions with the NRC, and also continuing technical meetings with the Nuclear Waste Technical Review Board. We have scientific activities underway and that are planned that we will continue to address uncertainties, and we'll be doing more work in the design arena.

As Lake said, science will continue at Yucca Mountain. We have some tests that have been going on for a long time, the drift scale test, for instance, which we turned the power off to in January. We will continue to monitor that test through its cool-down period for the next four years. In the test evaluation arena, there are other hydrologic and long-term thermal tests that we'll be looking at. Materials testing and evaluation, there are tests that are ongoing, and there are some that are in the "Q" that we hope to initiate soon.

Site and regional environmental monitoring has been going on for a very long time, and we will, of course, continue that, and continuous improvement of models and analysis, and I think this is kind of the heart of a couple of the questions a little bit earlier. I'll call it the technical program, the science and technical program must be robust enough to continually challenge the basis for the models that are used, either at the process level model, or its roll-up into a TSPA.

Obviously, if something is not addressed in a TSPA, then it is absolutely insensitive to the TSPA. So, what are the critical things that need to be in the Total System Performance Analysis? And part of the Program has to be based on a philosophy of continually challenging the adequacy of the sub-models and the total systems model.

Engineering activities will advance. Detailed surface, subsurface and waste package designs will evolve. We'll be looking a construction and fabrication techniques, operational concepts and methods, and looking at quality control and safety processes. And I'll talk a little bit more about this toward the end of my presentation.

An example of a technical issue that arose in the spring that was addressed successfully, and I'm just going to hit the highlights of it here, Mark Peters will talk about it a little bit more, what happened was that we took water samples from the drift scale test in superheated, greater than 140 degrees centigrade zones, and those water samples showed relatively high fluoride concentrations, and a low pH.

This could have considerable impact on waste package performance, because the fluoride could be deleterious to waste package and drip shield materials performance. A hypothesis was that the source was either Viton borehole packers or Teflon tubing, or potentially the host rock itself. If it was the host rock itself, then this obviously has some very strong implications on waste package performance.

We responded rapidly. The Thermal Test Team put together a strategy to identify the source of the fluoride, and within a few days, had a proposed strategy which was approved through the system. And what that strategy essentially focused on was looking at boreholes that did not have the suspect introduced materials, characterizing the waters out of those boreholes, and to determine whether we were seeing the fluoride in that water vapor.

We collected the water samples, and then we took some of the Viton and Teflon packers and tubing and put those, some that were already somewhat degraded, put those into those boreholes, the pristine boreholes, where we had taken water samples, and then observes what happened there.

The results of the tests were that the fluoride concentrations and low pH were detected only after the introduction of suspect materials. This leads us to conclude that this phenomena is associated with the materials. It is not a result of some kind of geochemical process between the steam and the rock. The source of the fluoride is de-gassing of the hydrogen fluoride or leaching of fluoride at high test temperatures.

There's a couple of things I'd like to kind of point to, and this is an example of the kind of environment we want to have, and that is that an issue was raised, it was addressed by management, it was floated up to top management very quickly. We paid attention to it. We put resources on it. And we tried to resolve this uncertainty, an unknown unknown, if you will, that had popped up, tried to determined what was the cause of it, what it really meant.

The technical concern was quickly and effectively resolved by investigators from the Thermal Test Team. The results have led to an improved understanding of the experimental environment and they removed the concerns raised by the initial fluoride detection. They've also provided a lesson learned, reinforced some of our earlier constraints that we put on materials selection for the repository environment. If you're not very careful about what you introduce into the repository environment, you can change the environment in ways that you perhaps did not think of.

The next topic I would like to briefly touch on is an evaluation of thermal operating modes. This is a report that was just finished last week. I hope it was distributed to the Board. This is a snapshot in time evaluation. This is what we promised in our letter back in May, an integrated look at pros and cons of high temperature versus low temperature thermal operating modes. It draws on a lot of existing information, the Supplemental Science and Performance Analysis, the Preliminary Preclosure Safety Assessment, and some other previous work.

What we're looking at is the suite of uncertainties and risks that one needs to look at, not just the postclosure performance question, but preclosure safety and performance, costs, constructability, some of the other questions, and trying to get an understanding of is there any one approach, whether it's high or low, that based on our state of knowledge now, is absolutely preferable.

The results of the integrated evaluation, and as I said, this is probably the first of a series that will occur, either operating mode is likely to comply with applicable regulations and standards. The uncertainties associated with the lower temperature mode appear to be fewer, certainly in the postclosure performance arena. The costs of a higher temperature mode are lower. Construction and operational safety appears to be a little better in the higher temperature mode. But this is based on our state of knowledge at this time.

In related work, work is ongoing to enhance the flexible design to get a design that can truly be operated either at a higher temperature operational mode or a lower temperature operational mode. Design evaluation study will be completed to support the license application.

We have scientific analyses ongoing to improve the technical basis for the waste package. Right now, the target for what is considered a low temperature goal is 85 degrees C. We would like to develop a better basis as to whether that's 82 or 91 or exactly what that might be. We will complete additional analysis in conjunction with the in-drift design development, and we're pursuing further development of in-drift ventilation models.

Now, as we move from one phase of the Project to another, from the site characterization focus of the Project into a licensing focus, there are things that are expected to occur in an NRC dominated environment that are a little different from the research and development environment that we've experienced for almost two decades.

There are expectations of a license applicant that differ from the environment that is pervasive in a collegial scientific research environment. Discipline is one of the main things that is expected in an NRC licensing environment. And these are some of the things that are expected in an NRC environment, some of which you--strict and literal procedural compliance, that's a discipline issue. Attention to detail. But there are some others that are not inconsistent with a good research environment also. Commitment to excellence, an inherent questioning attitude, continuous improvement, teamwork, collaboration and communication, honest objective self-assessment, regular and critical reviews of work, internal and external reviews of work. So, yes, there are some changes we need to make, but I think we are well poised to move into that new environment.

In the coming months, we'll take several important steps toward defining our evolving mission. We have a large strategic planning effort going on that Lake alluded to.

We'll be completing detailed multi-year work plans, trying to sort out what's the most important thing to do during this balancing that we were talking about, because we are living in a realm of limited resources, large, but limited resources. We'll be working with stakeholders and oversight bodies, including the Board, to clearly communicate our plans and objectives, and to seek your input and feedback.

We've provided the basis for the national decision, and we'll see how that plays out over the coming months. We plan to develop and submit a license application should the site be designation. The site designation action lies ahead of us still.

Work activities will include continuing technical advances in science and engineering. And we are in the process of implementing cultural changes needed to make this transition from site characterization into the licensing focus, not dominated, but focused organization.

With that, Dr. Craig, I think I'm available for questions here.

CRAIG: Thank you, Russ. You're definitely way ahead of scheduling, and that's good, because we're going to have a lot of conversation. All right, Norm and Don, but let me ask a couple first of all, and Richard and Jerry.

DYER: And Dan.

CRAIG: And Dan.

DYER: I should have talked slower, obviously.

CRAIG: This kind of interaction is definitely the way to go. I'd like to ask you to say something about your perception of how the Board and you folks might interact in this next phase. What kind of changes do you see as desirable in the next phase of the operation in terms of relations between the Board and the DOE?

DYER: I guess I hadn't really thought that any major change in the structure approach was necessary. I think the technically focused reviews and candid feedback and very, very valuable for us. That's where we get a lot of very valuable information.

CRAIG: We continue as normal, as we have in the past. Good. Thank you, that's very helpful.

Norm?

CHRISTENSEN: Christensen, Board.

Russ, I think the two things that you emphasized here, one of them is the transition into the licensing mode, and the kinds of cultural changes that are occurring, also the emphasis on the, let's call it the refinement of a flexible approach. It strikes me that these two things at least potentially come into conflict, in that many of the expectations of the NRC are going to require increasingly, if you will, a stationary target to shoot at. And the issue of, for example, the KTIs and how--I guess the question I'm getting to is do you perceive some conflict, as the program wishes to move forward with a flexible design, in meeting the expectations of the so-called nuclear culture of the NRC and its expectations to be able to really pin down the features of the design?

DYER: Not necessarily. We can take the flexible design forward and make a rational informed decision at some point in the future, and if we care to pursue a point design in the licensing phase, we can have a basis for that decision. That does not mean that we are precluded from continuing to examine ways that the system might be made better.

CHRISTENSEN: So, the issues, for example the KTIs that may come up, let's say, with a design that might be significantly cooler than the design that's currently being considered, you feel like there is the flexibility in the licensing process that will occur over, let's say, the next four years that will allow that kind of flexibility?

DYER: Yes, I think so. Now, we're talking about 293 agreements which are predicated on some working assumption going forward. If the basis for that changes, if we were to decide to go to, say, a lower temperature operating mode, we'd probably have to revisit and perhaps renegotiate some of those agreements. They'd have to be re-couched in terms that are applicable to the new situation, whatever that might be.

CHRISTENSEN: Okay.

CRAIG: Don?

RUNNELLS: Runnells, Board.

A couple of questions that are related to each other. One is first you haven't talked about the schedule, other than to mention if the site is designated, the license application becomes, you know, the dominating feature, and that's 2004.

Prior to that, what would the next major documents be that will be produced? What will we see next, let's say, in terms of major documents?

DYER: I guess I would expect to see a couple of things come out. As the design documents mature and become available, those would be available, I think, and I suspect those would come out not as some huge design, but there will probably be periodic design reviews that we'll go through at certain stages along the way.

RUNNELLS: And are those prior to license application? Those are prior to 2004?

DYER: Yes, some will be. I mean, we'll look at them internally and make sure that we stand behind them before we wrap them into a license application.

RUNNELLS: What else in terms of major comprehensive sort of summary documents? Are any of those scheduled?

DYER: That's a little unclear right now. That's one of the things that the planning process is laying out, is what are the major internal milestones and documents that we need to produce, just like the document hierarchy that we put together. I'm sure that there will be revisions of the AMRs and the PMRs. There may be some systems level look at all of those, but exactly what that is and the timing is not clear yet.

RUNNELLS: My second question is in your previous slide, Page 14, Slide 14, you mention complete detailed multi-year work plans. You probably know that one of the things the Board has criticized the DOE for, perhaps not publicly, is the Board doesn't see planning documents. We don't see the design of experiments. We see sort of the end product. Will the Board have a chance to have input into the DOE's multi-year work plans?

DYER: I guess I would--

RUNNELLS: Criticize them, if you like?

DYER: I would say yes, and I would say that the letter you just sent is already providing input into those work plans.

RUNNELLS: I would encourage that, because that is a thing that the Board has worried about, is sort of seeing the end result and not having a lot of input into, or not having a chance to have input into the design or to comment on design experiments as much as we would like.

DYER: That might be an area that we might want to pursue, if the Board is interested in getting into that.

CRAIG: Richard Parizek?

PARIZEK: Parizek, Board.

On discussion of 2004, I think there was mention of the fact--what's the relationship between, let's say, a site recommendation and the need for an LA within a fixed time period? Is there some slippage in there? I thought when one decision was made, you really had a short fuse when you had to go with the LA submission.

DYER: I guess you can look at that two ways. I mean, the Nuclear Waste Policy Act has a linkage in there, but appropriations language for the last several years has told us very explicitly to focus on the site recommendation, and let the license application slide. Now, which of those has primacy, I don't know. But, the most recent instructions we got from Congress were to focus on the site recommendation.

PARIZEK: So, there is--you would have a license application within a year or two years of that decision, it's not required?

DYER: No. But as prudent managers, you'd like to do it as quickly as you can put together a quality license application, a successful license application.

PARIZEK: What if you went to, say, a cooler temperature operating mode, you want an implication that you might save some time on the KTI concerns that you have with the high temperature operating mode. But wouldn't that maybe kick in other new KTIs that a low temperature operating mode requires? And I don't know whether you've had much discussion about this with NRC anticipating there may be other difficulties that you have to deal with.

DYER: Well, that's exactly right. In fact, Chairman Mazur made those comments somewhat earlier last year.

CRAIG: Jerry?

COHON: Cohon, Board.

Norm covered already my major issue, but I want to review it again because it bears repeating. I'm personally very concerned about this transition in culture that you're embarking on. It's understandable why you would be doing it. On the other hand, I think that it's expecting a great deal of the program to be able to shift to this LA dominated culture and still maintaining the kind of research program that the Board feels is very important. It's hard to do, period, but I think it's especially hard to do under schedule and budget constraints that you know you're facing already.

Lake make reference to a National Research Council report on staged repository development that we're all expecting in the spring. Did DOE commission that report?

DYER: Yes, we did.

COHON: Have you anticipated what they might be saying, and has that been factored into your plans over the next few years?

DYER: Not explicitly.

COHON: When it does come out, let me ask anyhow for the record, even though it's a completely gratuitous question, when it is issued, will you factor that into your planning?

DYER: It depends what happens. I mean, if the recommendation, say, of the National Academy recommends a change in national policy or approach, that may take some statute. Certainly we would respond to that. If there are things that are within our authority, yes, I think we would look at those things that are within our authority, and try to accommodate the things that make sense.

COHON: Thank you.

CRAIG: Okay, I have Dan Bullen, Alberto, and Priscilla. Anybody else? All right, Dan?

BULLEN: Bullen, Board.

Could we go to Figure Number 4, please? It's your pyramid, I think, of all the document hierarchy. And as much as I hate people who use their position in a gratuitous manner, I want to ask a question with respect to access to these documents.

Most of these had been web based before the 911 incident.

DYER: Right.

BULLEN: And for the reasons of security, they have been taken off. But they are all available on the public reading room. And, so, I guess the question, since I'm such a lazy researcher, and it's a whole lot easier to go to web and search those PDF files, do you think they'll ever come back on the web? And, if so, particularly with the modifications of AMRs and PMRs? I just want to go on the record as one Board member, not the whole Board itself, that sure would like to see them back on the web as an easy access.

DYER: So would we.

BULLEN: Okay. You don't even have to comment on that one. Now, can we just go to 12? I would like to actually make a comment. I'm very pleased that your scientific analysis for ongoing improvement in the technical basis for the waste package is there, but I'd kind of like to point out maybe something that we said in our letter under waste packages. We're concerned about the extrapolation and the performance of C-22, Alloy 22, in the higher temperature regimes. And you evaluate the current technical basis for that 85 degrees C. I might want to point out that we cited, and I'll quote it here, "The theoretical basis for making such long-term extrapolations of corrosion resistance for Alloy 22 is still very limited. In addition, data on aqueous corrosion for Alloy 22 above 120 C under conditions relevant to Yucca Mountain are essentially nonexistent, creating a serious data gap."

Are there plans to address that data gap? And I just wanted to sort of highlight that in the transcripts of this meeting?

DYER: I'll say yes. I know that there is talk about not just continuation of some of the materials tests, but also bringing some new tests on line.

BULLEN: Thank you.

CRAIG: Alberto Sagüés?

SAGÜÉS: Yes, speaking here as a Board member, I just want to talk a little bit maybe on a point that Jerry mentioned just a moment ago, and this has to do with the so-called culture evolution concept that was introduced here. Again, speaking as an individual, the words may be alarm or dismay come to mind when something like this is taken in these terms. Maybe the words regimented science, if we're going to be talking about science, and so on. I think that this is a problem, of course, in that this is a very much one of a kind, unprecedented kind of project. This is not designed in a plant or the reactor of a system following a tradition that has been established over a certain amount of time. We're talking about doing something totally unique.

And the problem when I see this particular statement is that this may be moving in the direction of something exclusionary, but do away with the exploratory kind of research that looks for elements that are quite unknown, things that may come up that no one had thought about, and so on, and instead of that, spending time testing to verify that certain parameters have been measured right. I assume that that is not the intention, but it certainly could be interpreted in that fashion when looking at it.

DYER: That is not what that means.

SAGÜÉS: And I would like to hear you amplify on that.

DYER: Okay. You've got to have one part of your program that's focused on licensing, and there needs to be a clear traceable documented trail that lays the basis for why you're making whatever argument you're making.

Now, there can be another program going on simultaneously which is looking at challenging, if you will, the models that you're using. The idea of continuous improvement in here I think is consistent with that. You should never be satisfied necessarily with where you are, but looking to make things better. And I do not see an inconsistency between those.

SAGÜÉS: So, then what I interpret, and this is what I certainly would like to see if you wanted to clarify that, is that indeed we're talking about a sort of parallel path, if you will, a continuation of research that has an exploratory nature, together with activities that are going to develop parameters properly certified for a license kind of purpose.

DYER: Yes, but I'll take that a little further. Even, let's say, the exploratory science arena needs to have a level of discipline associated with it. Now, that discipline can be pretty much measured by what good science would do. I mean, you would take good, accurate measurements, you would need to make sure that you calibrate the equipment that you're using, that you keep records for that, that the work you're doing is repeatable, that your inputs are documented some way, whether it be a communication or maybe it's a telephone call from a co-worker, but keeping that documentation chained together is one of the things that we talk about in attention to detail. And I don't necessarily see an inconsistency there, and I think that you can do good science in an environment like this.

Now, the NRC licensing environment, kind of the paradigm that has been thrown out, is that that is applicable to a mature industry, an operating nuclear power plant, and there are certain expectations on the part of NRC for that environment. That, like it or not, that's the standard that has been set for us. Now, maybe over time, if that doesn't make sense, maybe there can be some adjustment to those expectations. But that's not our unilateral call to make.

SAGÜÉS: I would like to ask one more question, if I may.

CRAIG: You're cutting into Priscilla's chance here.

SAGÜÉS: Okay. Well, I can defer to her then.

CRAIG: We really are running out of time.

SAGÜÉS: Okay.

CRAIG: Priscilla, a fast one?

NELSON: Okay. Nelson, Board.

Russ, what I'm trying to investigate here is this. The Project has developed a strategy which really is to exercise the high temperature design that's existed, with the spacing of the drifts, to really understand whether it's possible to develop, and what kind of low temperature operating mode underground. The questions about high temperature operation regarding corrosion that Dan Bullen brought up raise the prospect that there could be unknown unknowns that appear in non-linear responses, things like this.

The question about hydrologic and thermohydrologic independence of drifts in this design is an assertion which would be difficult actually to validate in this time framework. The model for humidity and ventilation for heat removal is one which I don't understand how the Project plans to go about validating. And I think the question about validation of models in general and input properties, including thermal conductivity, are things that are going to take time.

The report that you referred to mentions all sorts of issues relating to natural--coupled processes, with water around the underground opening. We've got a year 2002 where there was not full budgeting, and a 2004 time for LA. You've got not very much budget, and maybe next year will be a good year, but not very much time. Realistically, I really don't understand how you're going to be able to develop a viable low temperature, for example, or even thoroughly develop the high temperature operating mode in this time frame with this budget, but particularly the adding on of the low temperature operation.

So, maybe that was moire of a statement, but it just seems impossible in a two year period to do all the things that really are indicated to do. So, does there have to be a prioritization that you're going to go through pretty quickly here?

DYER: Well, yes, there will have to be a prioritization. But I guess I would disagree that everything has to be done within two years. We need to have a plan to get information at appropriate times along the process, but some of these tests are going to be very long term. They may a decade long test. The key will be getting the most important tests fielded reasonably soon, and then observing them for a period of time, and then taking the observations and the information back into the decision process.

CRAIG: I have to break in at this point, because we're running out of time. This is a good conversation. Pursue it off line, please.

DYER: Okay. I apologize for speaking so quickly.

CRAIG: Fluid inclusions have been at the core of one of the most interesting of the scientific issues that we've heard about. We've heard a lot about fluid inclusions and their consequences. Today, we're getting an update from Drew Coleman.

COLEMAN: My name is Drew Coleman, and the purpose of my talk is to give the DOE perspective on that recent fluid inclusion report.

I've got a brief recent history slide here. In 1996, the State of Nevada scientists reported that elevated temperature fluid inclusions were in calcite and were evidence of deposition from upwelling hydrothermal fluids. The Board reviewed the State's work and recommended additional studies to assess the State's fluid inclusion observations.

The DOE funded a joint study with scientists from the State of Nevada, University of Nevada Las Vegas, and the USGS as participants.

The objectives of the study were to determine whether two-phase fluid inclusion assemblages (FIAs) indicating elevated temperatures are present in the host rock, and they were. Determine the spatial distribution of the elevated temperature fluid inclusion assemblages, and they were found pretty much throughout the ESF and the cross-drift. And measure the range of fluid inclusion temperatures which were reported from 35 to 85 degrees Centigrade.

And, finally, and most important I think, to establish a temporal framework of fluid inclusion formation by defining a paragenetic sequence and geochronology of secondary minerals containing fluid inclusions.

I have to be a little sensitive on this slide. I talked to Susan Lynch, and, you know, the opinions or the work of scientists doesn't always represent the position of their manager, so it's actually the State's scientists' conceptual model implications. And I think the key point here is the proposed model implies that the vadose zone is occasionally subjected to an upward flux of heat and gas-charged fluid, upwelling waters hypothesis.

And the reference is the Scientific Status of the Lingering "Upwelling Water" Controversy in Light of the Joint UNLV/USGS/State of Nevada Research Project that was given to the Board in May.

The USGS concluded that secondary minerals and associated fluid inclusion assemblages are consistent with vadose zone formation. There's no evidence of supporting flooding of the unsaturated zone. The extremely sparse and heterogeneous distribution of the deposits is specifically inconsistent with flooding.

And, finally, Paces, et al. conclude, "The physical and isotopic data from calcite and opal indicate they formed from solutions of meteoric origin percolating through a limited network of connected fracture pathways in the unsaturated zone rather than by inundation from ascending groundwater originating in the saturated zone."

The UNLV conclusions were consistent. They concluded, "The results from this study are not consistent with models requiring formation of secondary minerals in a saturated environment at Yucca Mountain.

Results, furthermore provide no evidence for the former presence of upwelling hydrothermal fluids.

Alternatively, the results are consistent with infiltration of a cooling off tuff sequence by descending meteoric water."

And, finally, "This study demonstrates that the hypothesis of geologically recent upwelling hydrothermal fluids is untenable and should not disqualify Yucca Mountain as a potential nuclear waste storage site."

Currently, the UNLV group has submitted a manuscript to Geochemica and Cosmochemica entitled Thermochronological Evolution of Calcite Formation at the Potential Yucca Mountain Repository Site, with Part 1 being a Secondary Mineral Paragenesis and Geochemistry by Wilson and Cline, and Part 2 being Fluid Inclusion Analyses and Uranium

Lead Dating.

The GS recently released their Ages and Origins report from the Water Resources Investigation Division.

I talked to Susan Lynch again of the State of Nevada, and the State is withholding their final conclusions until they can review the Cline work. It's currently just in house and submitted to Gosmochemica and Geochemica. The DOE concludes that the data and interpretations by both DOE and UNLV scientists confirms that the conceptual model of descending percolation is accurate. DOE may continue to examine secondary minerals in conjunction with other studies.

And, finally, DOE concludes through this study, in conjunction with previous work, that upwelling waters or seismic pumping hypotheses for the origin of secondary mineralization at the Yucca Mountain site have been adequately addressed.

And that's the last slide, I believe.

CRAIG: Thank you very much, Drew. Questions?

COLEMAN: I guess I'll take questions, try to answer questions.

CRAIG: Pardon?

COLEMAN: I'll try to answer questions.

CRAIG: Okay. Debra? Others? Debra Knopman?

KNOPMAN: Knopman, Board.

Drew, did you mean maybe discounted, or

discontinued, on this last slide here?

COLEMAN: Well, I guess it's worded a little awkwardly, I agree, but what I'm trying to say is that we don't think any additional field work will be necessary. We have the data in hand to continue to address the alternative conceptual model in our future documents.

KNOPMAN: And just for the record, could you explain what your role has been in this process?

COLEMAN: I was functional monitor for the cooperative agreement task under which Jean Cline operated, and the participants.

KNOPMAN: And could you also just describe sort of the foundation of these studies was in terms of a common base of data gathering and analysis methods, that each group that then independently drew their own conclusions was working from the same data base? I just want that to be clear.

COLEMAN: Yes. Mostly the Cline study, they took 155 samples throughout the ESF and cross-drift, and they plan to cut five thick sections, and keep two and give the middle one to the State, and then give the other two to the USGS, and that process went somewhat slow. I'm not sure I ever saw any data presented by the State on the actual UNLV samples, but I know they had some in hand. The GS is finishing out their set of the samples, and they have a report due at the end of the fiscal year this year.

KNOPMAN: But I just want to be clear that there is not an ongoing scientific disagreement among the parties here about the methods of data collection, and the analysis of the samples, that the disagreements, as they still exist, relate to the interpretation of data that everyone has brought into in terms of their intrinsic value.

COLEMAN: I would agree with that. I haven't seen any evidence that anyone disputes any of the data collected on stable isotopes or fluid inclusion work. Mostly, the disagreements between the USGS and UNLV on the one hand, and the State on the other revolved around the interpretation of the data.

CRAIG: Leon Reiter?

REITER: Leon Reiter, Staff.

Drew, we had a meeting in May where the various parties presented their views, and it seems that USGS and UNLV and Bob Bodner was a consultant to UNLV, a former consultant to the Board, all seemed to agree that the hypothesis of upwelling, we really couldn't find evidence for that. But people like Bob Bodner raised a number of interesting issues that arose that were questions, and I wanted to ask you, or I want to sort of list those questions, and I wonder what you guys are planning to do about this.

There were some questions about what's the source of salinity in the fluids. Another question was what was the source of the magnesium in the enriched layer that was found, and there was also questioning about the matching of the fluid inclusion data with the model of this cooling off of a magna body. In fact, I've heard somewhere that some people from the Center for Nuclear Waste Regulatory Analysis are also looking at that to try to figure out what's going on, and they say, although they may not have, from what I understand, made out implications for upwelling, implications for other models that the DOE is looking at. Are you going to be addressing these kinds of issues?

COLEMAN: Well, Joe Whalen is still looking at the fluid inclusions at the USGS in Denver, and I'm talking with him regularly. So, that work is ongoing. Brian Marshall is still modeling the fluid inclusion temperatures, and I had some discussions with him recently. And, so, that effort is ongoing, among other things. Everything is being looked at under the Plan B replanning effort, and I'm hesitant to make any statements that are too bold. But, yeah, we're going to continue to look into those kinds of questions, at least finish out the Whalen portion of the fluid inclusion studies, and continue with the Brian Marshall modeling of the fluid inclusion temperatures.

CRAIG: Other questions? Drew, thank you very much.

COLEMAN: Thank you for the opportunity to address the Board.

CRAIG: We now move to one of our old standby regulars, Mark Peters. Good old Mark Peters, who has appeared before us many times to cover massive amounts of material on the scientific program.

Mark, you've got a full hour, including the questions. You've got 40 minutes to talk, and I'll warn you when you've got five minutes to go.

PETERS: Can everybody hear me okay? Thanks, Paul.

COHON: Could I ask a question? How did you lose all that weight? You look terrific.

PETERS: It's this project. No, it's on purpose. My wife said you've got to lose some weight, so I went and lost some weight.

And thanks for the introduction, Paul. This is, again, a similar presentation to what you've seen from me several times now. There is a lot of material. I will say there is a lot in the backup. I moved some to backup, given the limited time. And I also have an advantage that Bo and Al are going to talk this afternoon. So, I might be able to gloss over some of the UZ and SZ relatively quickly.

I'm going to try to go through it. I apologize for the length, but I did want to give you a feel for all the things that are still going on in the testing area.

So, the same objectives that I've had in previous meetings, just to provide you a status on the data collection and testing program in both the natural and engineered barrier areas in support of the models and also the design.

I'll start, as always, with the unsaturated zone, elaborate a little bit on the drift scale tests, about the fluoride measurements that Russ alluded to in his presentation, an update on Chlorine 36 validation. Then some slides on two tests in the cross-drift, Alcove 8 and the bulkhead experiments. Then an update on the status of where we are with data collection for the Busted Butte test. Finally, a very quick status on the alluvial testing complex in the saturated zone.

Nye County will be presenting, I believe it's later today, and they'll talk a lot more about their program, and of course this is all being done in cooperation with the Nye County program.

Moving into the engineered barrier, an update on the thermal conductivity measurements that we're doing, primarily in the field, a very quick update, something I have not talked to the Board about before is investigations that we have ongoing in the rock properties area, and also a very quick status on the natural convection tests at the Atlas facility. Finally, a set of slides on waste package materials investigations at Livermore, as well as General Electric. And then two quick slides on Argonne work in waste form area.

I'm going to try to sprinkle in credit where credit is due. I'm presenting a lot of people's work here. I've done none of this work myself. I'm trying to give you an overview of what all these great scientists have done on the Yucca Mountain Project.

Starting with the ESF, I'm going to talk mainly about the drift scale test, and also about Chlorine 36 validation. Remember, there we're collecting samples from the Sundance Fault that crosses the ESF right in this area here, and the Drillhole Wash structure that crosses just upstream of the cross-drift.

I won't dwell on the details here. This is a diagram you've seen before. Here is the ESF, and then the cross-drift with the alcoves located with the potential repository block here to the west.

Starting with the drift scale test, I think it's old hat, and we're out there evaluating thermally coupled processes in the rock in the middle non-lithophysal unit of the Topopah Spring. This is just a diagram that shows the scaled back test with the wing heaters and the boreholes both above and below the heated drift.

Russ I believe mentioned in his presentation that we've started the cooling phase of the test. That's the main point of this slide. Here, it's time and days versus total power shown in the blue, and drift wall temperature shown in the red. January 14th, a couple Mondays back, we did turn off the heaters. It's not a controlled cooling where we're turning back the heat. We actually just flipped the power off. So, we're right now in a natural cooling phase.

Back up one second. I'm not sure what just happened when the fluoride slide comes up. So, at any rate, again we're seeing drops in temperature that we would have expected, very rapid drops in temperature early on, and it will, of course, level off as we approach a steady state. That's the basic gist. What's shown in the power here is just the various increments when we turn back power as we were maintaining the temperature at 200 C. at the drift wall.

This right here is 200 C. I apologize for that. That fell off of the graphics. This is 200 Celsius right here. Sorry about that. It might actually be on the same scale. I think we might have fitted it to the same scale. I was out there the next day, and it was down on the canister, it was down a good 15 degrees in the first day. I did not call out there today to see how far it is down now, but I would say 30, 35 degrees C. it's gone down on the canister. The rock is cooling much slower, of course, because of the thermal conductivity of the rock.

There's a whole set of slides in the backup that talk about the predictions that we had had for the heating phase, the bullets for the different processes. Bo is going to allude to some of that in his presentation, so I won't dwell on that. But they're back there. There's about five or six slides that talk about the predictions.

Moving into fluoride, Russ already gave us a good introduction. In the spring and summer time frame we collected water samples from some of the hydrologic boreholes from the superheated areas, above 140 C. And, so, these were samples that condensed from steam, and when we took them to the laboratory, well, first of all, we were measuring pH in the field, and the pHs were very low, down in the 3, 3.5 range, much lower than what we were getting from water samples that were taken at sub-boiling temperatures. We took them to the laboratory and did analysis and saw really high fluorine concentrations. That caused us to really think real hard what's causing this, obviously. These were anomalous readings. We had not seen those before in the drift scale test, any of our coupled process testing.

So, we went, and as Russ alluded to, we followed a process where we put together a strategy to test basically two hypotheses, one, the fluoride was coming from material we had introduced to the test, namely the Viton for the packers, or the Teflon for the sampling tubes, or the host rock itself where the fluoride primarily is contained within the fractures.

So, we did a field test. We went out and we said, okay, if it's coming from the introduced materials, let's take a hole that has not had any introduced material in it, sample some steam, then go into that same hole, introduce some Viton and some Teflon, and then resample. This is summarizing those results. This is just time elapsed from the first sample collected versus measured values in either ppm or pH. So, pH is shown in the triangles, whereas fluorine in ppm is shown in the--excuse me--in diamonds, and these are shown in triangles.

So, here we're collecting samples, coming along at basically no fluoride in the water, pH is up around 5, 5.5, which we would expect, introduced the Viton and Teflon into the boreholes and saw the dramatic rise in fluorine concentration with precipitous decline in pH.

There's additional samples that aren't plotted here that we just analyzed that continue to pick this trend up. The longer it was in, the higher the fluorine went, the lower the pH went. So, the results of this field test have really allowed us to confirm the hypothesis that the source of the fluoride was introduced by the fluoroelastomers or the introduced materials from the Viton or the Teflon.

We've also got a laboratory testing program that we started in parallel with the field experiments to address some of the more detailed questions. And there's some preliminary results from that as well where we're doing autoclave experiments where we've got steam, water and steam, with the water and steam with the introduced materials, as well, to confirm in fact what we're seeing in the field test.

There's been a high temperature reaction chamber test set up at Berkeley, as well as similar experiments at Livermore in an autoclave, and they show the same systematics. If you introduce the Viton, you get really high fluoride concentration and very low pHs, much like you see in the Livermore experiments. These were initiated in parallel prior to the results of the field experiments, because we weren't exactly sure what kind of definitive results we'd get out of the field. We were very, very pleased with the results from the field. But we'll continue these through fruition.

Moving to Chlorine 36 validation, here again we're validating the occurrence of "bomb-pulse" Chlorine 36 at two locations in the ESF. I pointed out the Sundance Fault that crosses the ESF down near Alcove 6, and the Drillhole Wash that crosses the ESF just upstream of the cross-drift breaking out.

By way of an update, the last meeting, I told you about us using common crushing and passive leaching techniques for all the analysis of the validation samples from here forward. The USGS has leached, they've resampled validation core. We're now off of the reference sample that we were doing the leaching experiments on, and we're back to unknown, the validation core. They've been doing the leaching of crushed core providing that leachate to both Livermore and Los Alamos.

Los Alamos has also continued some leaching testing on some of the ESF samples to compliment what we're doing with the validation with the validation samples, and the data that we have to date from this new batch of unknowns that we just analyzed, it's on the order of 24 or 25 samples from the Sundance Fault, again, leached passively for an hour and then analyzed, and the good news we feel is that when Livermore and Los Alamos take those leachates and do the analyses, they're getting the same answer.

Whereas, if you remember, in the past, I've been up here telling you that we've had these discrepancies between the two laboratories and datasets, and that's why we went through the whole process of leaching tests, et cetera, et cetera. We feel like we've worked our way through that, but I'll talk about the fact that we have yet to see "bomb-pulse" in these validation samples.

This is just a couple plots that show that this next set of validation samples, Livermore results plotted on the Y, Los Alamos on the X, this is just a one to one line showing the error bars. Remember, the early results, Livermore results for these validation samples were down in the 50 to 100 times 10 to the minus 15. This is reported in ratios of times 10 to the minus 15 of Chlorine 36 to total Chloride. Whereas, Los Alamos was getting numbers up in this range. So, there was a pretty big different. We think now we've solved that problem using the common leaching and processing techniques.

The next plot just shows the same samples. Here, we're just talking Chloride concentration rather than Chlorine 36 to total Chloride.

I put this diagram in because I find it useful to talk through the complexity of the Chloride, the systematics in the Yucca Mountain rocks. What we've got here is Chlorine 36 to total Chloride ratio times 10 to the minus 15, versus increasing leaching time. What I'm trying to get at here is there's different reservoirs of Chloride in the rock, and leaching time is going to have a significant effect on what answer you get.

Early on, this conceptual model would suggest that early on with short leaching times, that's when you're going to exploit the "bomb-pulse" component.

As you continue to leach, you will start to leach some of the matrix, more of the matrix component, some of the accessible pores, causing the ratio to decrease. The reason there's differences in times here is because, as you know, the Chlorine 36 production rate varies with time, so you'd expect there would be some variability in this, depending upon the age of the water.

These lines separate, because if you go to a very aggressive leach, you could start to pick up rock chloride, which is dead chloride, which would cause the ratio to go down pretty dramatically. Whereas, if you continue a passive leach, this conceptual model would suggest that you would start to leach salts that are greater than 10,000 years old that could cause the ratio to go up.

Again, I'm not trying to say that this explains everything we see. But I find it useful to help us to think through why we're still seeing these differences in systematics.

I should also mention that when you talk about the early June Fabryka-Martin data, we were looking at leaching times on the order of 24 to 48 hours, and she was still seeing evidence of "bomb-pulse." So, that's somewhat of an inconsistency with the way I just explained that, and I realize that. But, again, this doesn't explain everything. It's just I find it useful on how to think through the systematics. But we're still thinking through this.

Go back one second. I should also mention the Cathay leaching times were down in here, but remember that Mark Cathay did more of an active leach. He tumbled the samples.

So, the final set of bullets on Chlorine 36, I already alluded to this. We've looked at the next set of validation samples. We think we've solved the discrepancy between the two lab data sets, but we have yet to find "bomb-pulse" Chlorine 36 in the validation samples. Remember, those were drilled from boreholes, whereas the early June Fabryka-Martin data was taken from samples from the total walls. So, it could be that there are still differences due to the sampling. We're investigating that.

One of the things that we are going to do now is we have core from Niche 1, which is a niche located just off the ESF right near the Sundance Fault. June Fabryka-Martin did look at core here, not samples taken from the total wall, but core, and she saw evidence of "bomb-pulse" in a high percentage of those samples. So, part of our path forward will be to go back to those cores, reprocess some of those samples, and see if Livermore and Los Alamos, using common processing and leaching techniques, can in fact find "bomb-pulse" in those samples.

So, I think it's still a status report. We're still working through some of the issues.

Moving into the ECRB, this is a diagram you've seen before, the cross-drift showing the contacts for the different sub-units of the Topopah Spring as you go down the cross-drift, with the middle non-lith exposed in this area--

excuse me--the lower lith exposed over a large section of the tunnel, the Solitario Canyon Fault right here. North is in this direction. It shows the locations of the test alcoves. The regular font black, are the existing test facilities, with the Italics blue are facilities that are in the multi-year plan for the out years.

I'm going to talk today about results from the cross-over alcove, the drift to drift test between the cross-drift and the ESF, and also tell about the bulkhead experiment which is going on in this back half of the cross-drift.

One of the things that I'll clarify a little more when I get to the bulkhead investigation, notice there's another bulkhead here now. When I talked to the Board in September, we were talking about taking this first bulkhead and moving it down tunnel. Since that time, and I'll talk about why we've reevaluated that and we've kept this first bulkhead at the same place, and added a fourth bulkhead here.

So, starting with Alcove 8, Niche 3, the cross-over alcove, I'll mix those back and forth, here we're looking at flow and seepage processes in the potential repository horizon rocks at the scales of tens of meters.

This is just a schematic diagram that you've seen before showing the layout of Alcove 8, Niche 3 below. This distance here is on the order of 18 meters. There's boreholes drilled down as well as up for monitoring the travel of the wetting front, the moisture front. And as you remember, we've got an infiltration plot in the floor of Alcove 8, and we're collecting seepage in Niche 3 below.

This is a picture from the back of Alcove 8 looking out toward the opening. Remember that the infiltration experiment is right now concentrated on a fault that's exposed to the floor of Alcove 8, and also exposed down in Niche 3. So, we've got four chambers that are hard to see in this picture where we're infiltrating with infiltration permeameters, putting in a constant head and looking at how the fault takes the water, and how much seeps into the opening below.

These are just some bullets on the status, uninterrupted ponded infiltration since March, over 60,000 liters applied. How much the fault's intake rates along the fault, it's decreased from about 250 liters per day, down to like 170 liters per day earlier this month.

We did a test where we were just infiltrating water with 10 ppm Lithium Bromide. In October, we introduced the pulse of tracer that had a higher concentration of the Lithium Bromide, as well as 25 ppm of polyfluorobenzoic acid. And we're again collecting water in Niche 3, quantifying that, and also now doing the tracer analysis.

Bo is going to talk about this as well, so I will not dwell on it. This is some of the information from the tracer recovery in Niche 3. Time versus normalized concentration for all three, the Lithium Bromide and the fluorobenzoic acid. Important points here, Bromide is acting as a conservative. The fluorobenzoic breaks through prior to the Bromide. That's being interpreted as the effect of matrix diffusion. That's why we had Bromide, Lithium Bromide, and PFBA in there, was to look at the effects of matrix diffusion, and we're in fact seeing systematics that are consistent with our conceptual understanding of that. And, again, Bo will probably expand on that some when he gets up here.

This is just a picture of the collection trays in Niche 3 where we quantify the water. And there's backup on Alcove 8, Niche 3 that show the time history of infiltration versus collection. I just didn't have time to go through that.

Again, the bulkhead investigations, we've got the back 918 meters of the cross-drift isolated from ventilation. We're looking for rewetting and monitoring for liquid water. This bullet, read this as monitor for free liquid water from either dripping or condensation from the vapor phase.

This test has been going on for over two years. We had a bulkhead entry just after the last Board meeting. We went in on October 1st. I'll talk some about what we saw when we went in there.

I've already alluded to some of this. This is a picture looking down the cross-drift past probably the first bulkhead. I don't want to confuse you here, but I mentioned that there's now four bulkheads. In a slide that's coming up here, I'm still talking about three. But I'll clarify that as we go along.

The three bulkhead doors were opened on October 1. We've now put a fourth bulkhead in at 22+01. The last three were sealed in November. We sealed the one at 17+63, the first bulkhead, in December, and now the test is now back to no ventilation monitoring. We did a lot of enhancement to instrumentation inside the drift this time around. We added cameras, which are very useful because we've got them focused on areas that were showing evidence of liquid water, looking at the drip clause, looking at other areas, to see how the wetting is occurring realtime. So, we can sit down in downtown Las Vegas, move the cameras around. It's an interesting system.

But I did mention at the previous meeting that I said that we were going to take this first bulkhead and move it down. After we saw what we saw when we went in in October, there were some wet areas that were developing down in this part of the tunnel that weren't well developed in the previous entry that were getting more well developed, and they were really raising some questions about what we were seeing in this part of the tunnel, whereas before most of the phenomena had been occurring down at the back end. So, we looked at that, and also through conversations with the NRC, we made a decision to keep the configuration as is, but add a fourth bulkhead.

This is just a picture, I'll explain it, it probably doesn't mean much. This is paint, green spray paint on the wall of the rock, and this is rock around it. It's a little dark. It might show up a little better in your hard copy. But what this is is this is water droplets that collected on the spray painted part, but didn't collect on the rock. We saw a lot of evidence where there were shotcreted sections. The water was collecting on the shotcrete, but not on the rock next to it.

But what we saw when we went in is there was alternating dry and wet areas. So, that's not immediately straightforward to explain in terms of condensation within the drift. Why would it be alternating? So, we're looking at that in the context of what's going on also with the surface geology, how it ties to the infiltration map. I think Bo can probably expand on what he thinks it all means. But I think the bottom line is you need to continue the test to answer some of these questions. It's telling us something about what's going on in the drift as well.

But, again, it was dry just before the first bulkhead, and wet through another 200 meter section. I can go through this in the questions if you're interested in the details of where it was wet and dry. But in the wet sections, the dampness was more pronounced on the upper parts of the drift walls.

Again, down by the Solitario Canyon, it was relatively dry, and back behind the third bulkhead, and here I'm talking about first, second and third in the past, I haven't added in the fourth, if that's clear, so this first is 17+63, the second is 25+03, and third here is the one just behind the TBM.

Remember, the TBM was on and powered through a lot of this test, and that was probably causing some complicating factors for us. The TBM is now off. We've turned it off to hope to isolate that as a variable.

These are some pictures from the October entry. I think you can probably see drops on the utility lines here, also water collecting on the conveyor belt. There were droplets on the conveyor belt, whereas, the underside of the conveyor belt tended to not have droplets, but this is where it had puddled and was running.

NELSON: What's the date on that?

PETERS: What's the date? October 1st. This was October 1st. What we did on October 1st is we went in without ventilation. We opened the doors and didn't ventilate because the minute you ventilate, you start to lose a lot of this evidence. So, we went in with supplied air, a couple of the scientists went in with supplied air to try to get some of these observations documented prior to it all drying out. Although, the dryout still leaves salt residue and rust spots and things, you can still get meaningful information.

The next slide is another picture from October 1st. Here, what this is trying to convey is water droplets on the mesh and on the shotcrete versus the rock next to it, which does not have any drops, and then water collecting on the underneath of the vent line. So, this is the kind of moisture that we're seeing inside there when we go in in that initial entry, similar character to what we saw in the previous entries.

NELSON: Do you think that the rock is not wet because it's absorbing the moisture?

PETERS: The question was do I think the rock is not wet because it's absorbing the moisture. There's people in the audience who could probably address that better. I think it has to do with the temperature of the wall. And it's interesting, it could have to do with the temperature of the wall and the fact that it's shotcrete, so it's different thermal properties of the wall, and the spray paint would do the same, or it could be that the spray paint and the shotcrete are in fact causing--I think it's either one. It could be not absorbing the water. The bottom line is you could be right, or it could have an effect on the temperature at the wall itself that could be causing it to condense there, a cold spot where it condenses there, and not on the rock.

But the bottom line is there's an observation that where we see paint for shotcrete, there's water, and not on the rock itself.

This is just, I don't expect you to memorize this, other than this is temperature and relative humidity versus time for the different stations that we have in that test area. This is when we close the first three doors, and here's where we closed the last door, just that the temperature gradients that we were seeing early on, here we're looking at probably a degree or two temperature difference, whereas when the TBM was on, we were more like three or four degrees. The temperature gradient exists, but it's less, and also the relative humidity, as you'd expect, pretty much goes straight up towards 100 per cent as soon as we close the doors.

So, this test is, again, a shut up, it's shut up, the doors closed since just before Christmas.

Moving to the unsaturated zone below the repository horizon to the Busted Butte test, we've talked about the objectives of this test many times, looking at heterogeneities on flow and transport, looking at fracture/matrix interaction, colloid migration in the UZ, scaling of laboratory sorption data to the field scale, and of course looking at overall scaling issues.

A diagram of the injection face at Busted Butte showing the two planes of injection holes, here in the Topopah Spring vitrophere unit, and here in the Calico Hills unit, showing the overcores that we've completed. You've seen this diagram before. The overcores that we've completed on some of the injection holes, trying to get a feel for how far the reactive tracers have travelled. The concern is that it's broke through to the collection plane, but we're trying here to get information on the reactive tracer.

We also did a mineback that I also showed you last meeting. Here's the Phase 2 block with the injection holes and the collection holes coming in off of this face, showing the orientation of the mineback, drove it this way, and then mined successive faces, stopped at basically each one of these planes of injection holes, and took a set of auger samples. Again, we imaged the face, because we were looking for the fluorescein dye, and also took auger samples for quantitative analysis of where the tracers had gone.

So, the next couple slides are just results of some of the analysis of the rock samples that we took. Here, we're looking at the overcore of Borehole 20. This was, again, in the Topopah Spring vitrophere unit. It's a high injection rate hole, 50 milliliters per hour. What you're looking at if you squint is distance from the actual injection hole down. This is in centimeters, so 50 centimeters here, normalized concentration of four different materials, Cobalt, Lithium, Nickel and Fluorobenzoic acid.

The Lithium is relatively flat, because it broke through in the collection pad, so the front is well below this depth in the system, whereas we're still seeing evidence of sorption of the Cobalt and the Nickel, and the Fluorobenzoic is acting conservative, as you would expect.

These profiles are consistent with the KDs that we have for the Calico Hills and for the Topopah Spring for Nickel and Cobalt. So, that's one example of the sorts of data that we're collecting from the overcores.

The next slide will show here, we're taking a face--go back to the slide of the mineback. What this is is there was a face exposed right here, right along the plane of Borehole 20. We did a set of hand augers where we drilled hand augers into the face, took samples, and what you're going to see is a series of plots that show the concentrations of those same four elements as a function of distance from Borehole 20.

So, Borehole 20 would again be oriented like this. So, the face is right at Borehole 20, so you've got A, which would be taken from 0 to 10 centimeters. There's typos here. This should be B, 10 to 20 centimeters, and C, 20 to 30 centimeters. So, they're samples that were a set of samples, and averaged over these intervals of the auger hole, again showing a very similar relationship. The spike here right at the injection hole is because that's where the injector was. You see the decrease, you see the PFBA acting conservatively. There's a sense absorption of the Lithium in this dataset, whereas the Cobalt and Nickel are still acting as reactor tracers, consistent with the KDs for this rock type.

So, this is the kind of data that we're getting out of this test. This is ongoing data collection and analysis that's being used to model the test results.

We're also looking at colloids at Busted Butte. I'm going to talk today mainly about some lab block experiments that we're doing with colloid transport. We're looking at Lithium Bromide and colloid imbibition into the matrix, and comparing it to our colloid transport models that we use in the site scale model and, again, trying to get at more controlled lab scale experiments to help us interpret the results of the colloids in the field scale experiment at Busted Butte.

This is the results of one of those block experiments. Here, what you see plotted is time versus cumulative mass balance. This is basically colloids collected for the experimental data, which is shown in the solid line, and three different simulations.

What's being varied here in the simulations is the coordination number. The pore structure of the rock is being varied, as well as the size of the colloids.

So, without getting into the gory details, you can see that in varying the parameters on the pore structure, as well as the size of the colloids, we can match the experiments with certain assumptions about those two parameters. This kind of modeling and fitting is being used again to then interpret the results from the field scale experiment.

Moving into the saturated zone, I won't dwell on this because of time, but we're collecting site-scale data in cooperation with the Nye County program in support of the saturated zone model.

I'll talk briefly about status of the alluvial testing complex. Again, the cornerstone of that test is 19D here, south of Yucca Mountain. We've since drilled, Nye County has since drilled two new wells to the north and to the east. This is just a status. I've talked before about the single hole hydraulic and tracer tests that we did in

19D.

Again, Nye County has now drilled two new boreholes. We now have a triangular testing complex. The new boreholes are being used for monitoring for the hydraulic testing and injection wells for the tracer testing. And we've done some scoping cross-hole hydraulic tests just in December, spilled over into this month, and we're preparing to initiate the cross-hole tracer testing.

So, we've gone through the natural system. Let's talk a little bit about engineered barrier, acknowledging that thermal properties investigation supports the coupled process models, as well, which I consider more of a natural barrier model. But, again, these investigations are field laboratory based. They support the coupled process models, the EBS models, and design, and we do have a geostatistics initiative in place to try to evaluate the variability and uncertainty in this important parameter.

I repeat this slide just because to remind you that the field tests that I'm going to talk about are located in the lower lith and the cross-drift. They're located in this area right in here. There's three different locations, all within this part of the lower lithophysal.

The layout of Tests 1 and 2 is in the backup. So, I'm not going to go through that.

We've done different scales of experiments. The first test was a single heater with a single instrumentation borehole drilled in like an "X" fashion. Whereas, the second test was a larger test, three heaters and three instrumentation boreholes to try to perturb a larger volume of rock.

The first test is finished. The second test is in Stage 1 of heating, when we've got a third test that has a single heater with boreholes above and below. That's to more look at any influence of convective effects. That test equipment is being installed, and we're about to start that test here this winter.

The results from the first test, you saw this diagram, at least a preliminary nature of this diagram, in the last meeting. Thermal conductivity and thermal diffusivity versus time for this two-hole test, the first test. What they've done here is they've taken conduction only model, and fit the temperature profile, and come out with thermal conductivity and thermal diffusivity.

They've also looked at the same results using NOFT to try to account for the convective effects, and come up with a similar answer. But the thermal conductivities are consistent with the kind of ranges that we assumed in SSPA for the lower lith thermal conductivity values. Again, these are field scale experiments, so we're trying to get the influence of the lithophysal porosity as much as possible.

The next slide is a set of predictions and field data for the second test, the larger test, the six hole test. The bottom line is that when we go through and look at it with the conduction only model, we come up with very similar thermal conductivities to what we got for the smaller scale test, and consistent with what our assumptions are in the SSPA range that we used.

We are doing a laboratory program where we're taking matrix samples, analyzing conductivity, thermal properties in those samples. It's obvious to you I know that the conductivity is a function of a lot of different properties, the porosity and the saturation, the temperature and temperature gradient, of course the lithophysal porosity. The field scale experiments we're hoping will help us address this issue.

The status is we're looking at different techniques for measuring thermal conductivity in the lab. I won't go into the details of the different techniques, but the guarded heat flow meter technique was the technique that would have been used on previous samples in the past in the Project. There's some concerns about there being convective effects influencing that technique, so we're testing independent techniques to ensure that we've got that question answered.

We've got thermal conductivities within the range of 1 to 2. I don't have--this is very preliminary information. Hopefully, next meeting, I can show you some plots that show how it varies with temperature saturation. But the variability is what we would expect, given these kinds of differences in rock properties.

These are going to go real fast. I've got pictures in the backup. We are undertaking investigation in the field, collecting samples, large cores, as well as doing some slot tests to investigate rock properties. Here, I'm talking about mechanical properties. Again, the large diameter coring is ongoing. The laboratory measurements are ongoing. I've got some pictures in the backup that show the kind of scale that we're looking at in terms of samples.

Also, no results yet on the natural convection tests. Here, we've got two tests set up at Atlas at 25 per cent scale and 44 per cent scale, where we're looking at convective effects within a mock drift with electrical heaters. Here, we're looking at validating the natural convection models and also evaluating the potential for cold traps.

There's pictures of the construction aspects of that in the backup. But, again, no results. These tests were just turned on earlier this month.

Moving into the waste package, and I switched--go ahead to the next one, John. The next one in your package has been moved back a couple. So, I jumped ahead to Page 45.

44 got moved back a couple.

I'm going to try to give you a picture of some of the things that are going on in this area. I can't do it justice in the two minutes that I have. But we continue, all the programs that you've heard about in the past, we continue to investigate at Livermore as well as at some of the other subcontractors.

What about Alloy 22? We're doing some electrical chemical testing, short-term testing, using various methods. The examples I'm going to show you are polarization resistance methods on prismatic samples, freshly polished. The results I'm going to show you are going to be from simulated acidic water. As you all well know, we've got several different water compositions that we're looking at in the testing program. The results that I'm going to show you are going to be corrosion rate as a function of temperature, and the bullet here just reminds me to tell you that these experiments were repeated at each temperature range.

The next diagram shows the results of these tests. These are Livermore tests. Corrosion rate for Alloy 22 samples, again in deaerated simulated acidic water, versus temperature, shown here. And then on this plot over here, showing the activation energy for that corrosion rate, a relatively low activation energy came out of the results of these experiments. But this is getting at the effect of temperature on corrosion rate. That's been discussed a lot in the context of the SSPA, and it was also discussed a little bit earlier today. So, this work continues.

What about the effect of environment. Here, these are open circuit potential measurement, again, on Alloy 22. This is fresh Alloy 22, corrosion potential in volts versus time, relatively long-term exposure, nine months of exposure in different environments, here, acidic water, simulated concentrated water, and dilute water.

The main point here, this is constant temperature showing the effect of pH. The pH range up here is on the order of I believe 3 to 3 1/2, whereas, the pH range for these waters down here is more on the order of, I can't remember exactly, 9, 10, 11, relatively basic. So, it shows the effect of pH, but also shows that we get up to this 300 millivolt range, and it tends to flatten out. These are relatively long-term experiments, not 10,000 years, but trying to get at these longer term experiments to help us address the change in corrosion potential with time.

The next plot, these data points are straight data points. They're not real data, so you can put an "X" through those. There was a curve there that shouldn't have been there. But what this is showing, this is actually data from General Electric. This is showing the effects of trace elements on open circuit potentials. This is very preliminary data, but I wanted to put it in to point out that we are doing--looking at the effect of trace elements like lead on corrosion potential in the different materials.

You can see the effect of lead at least in these preliminary results as relatively minor on the corrosion potential.

What about passive film? We're looking at the stability of the passive film layer, also what's the makeup of the passive film layer. The next couple slides are going to focus on the makeup of the passive film layer. Here, we've taken samples, applied potentials, samples that were exposed to 95 degrees C. basic saturated water, and applied potentials to those samples, and then measured the films to see what the concentrations of various key elements were in those films.

Talking to the folks at Livermore, this is Livermore data, some of these values that are going up, they think they may not have yet reached steady state, and that they will eventually flatten off. But this is giving important information on what the makeup of the passive film layer is, which then translates into our models for passive film stability.

This is, again, very preliminary ongoing work, just to give you a feel for the kinds of data we're collecting.

We've also taken some of the samples and not exposed them to water. We're actually just putting them in a furnace. We've taken a mill surface Alloy 22, taken the mill surface, put it in an oven at 400, 550 C., and looked what happened to the film. This is just a picture, a TEM photo of that film, as well as a traverse using the EDS spectra probably on the SEM, showing the concentrations of chromium and nickel. This is base metal here, and then there's two different layers, a chromium rich layer and a nickel rich layer. And on looking at the thermometer on how that evolves with heating and air, here trying to look at the effects of the dryout period when there's no water present.

Two more slides on waste form focused on colloids. This is data from Argonne National Laboratory. Here, we're looking at the generation of colloids from commercial spent nuclear fuel. These are just dynamic light scattering measurements showing the size of the colloids coming off of spent fuel as a function of time, and showing that the colloids concentration decrease with time.

Also, they're taking those colloids and characterizing them in great detail. This is a TEM image of one of those colloids with some very preliminary results suggesting that the composition of that is made up of iron, silica and maybe some uranium.

Talking to the Argonne folks, they have yet to characterize the phase. They're still in the process with a lot of these colloids of looking at just the elemental concentrations.

Finally, also, the glass waste form. Here, you're looking at clay colloids that form from the alteration of the glass waste form, and this is just an example of some tests that they're doing to look at the colloid formation, size of colloids that are formed, and how they agglomerate and eventually fall out of solution as a function of, in this particular case, sodium chloride. These are the kinds of tests that are going on at Argonne and PMML to address these issues.

Finally, a very quick, hopefully not too quick, tour through the testing program. I tried to cover pieces of everything to give you a feel for what we're doing. Again, in the ESF, cross-drift laboratories, we feel this testing program is important. It continues to confirm our technical basis for addressing uncertainties, and hopefully providing additional confidence in our models.

So, take a breath. That's it.

CRAIG: Thank you, Mark. As always, that was a lot.

Richard has his hand up. Richard, Debra, Priscilla and Alberto. Richard?

PARIZEK: Parizek, Board.

On Page 8, you had one fluoride value at about 130 days, which was non-zero. Is that a measurement error?

That's the second triangle on the bottom, it's really almost on zero.

PETERS: The measurement, I believe they're using--I'm not sure what technique they're using, Dick, but it's probably on the order of a tenth of a ppm, a couple tenths of a ppm.

PARIZEK: But nothing serious from the point of view of a pH problem?

PETERS: No.

PARIZEK: I mean, within acceptable--

PETERS: Right. I mean, it's probably at the most like that.

PARIZEK: Then the question of when the TBM was turned off, do you have a date on that?

PETERS: Yes, we lost power, I told you last meeting and I'll have to test my memory, we lost power because of an electrical failure, let me get my dates right, last spring. I can get you the exact month. I just can't pull it off my head.

PARIZEK: It would be helpful to know the timing.

PETERS: Yes, I think it was like April, or so.

PARIZEK: And it's been off since that time?

PETERS: Yes. We did turn it on while the bulkheads were open briefly to do a maintenance program, and then we turned it right back off.

PARIZEK: Okay. Another question. When you go in now and just open up pre-ventilation, do you have the molds and all of the things growing that you were worried about?

PETERS: Yes.

PARIZEK: So, you go in in protective suits?

PETERS: Yes, the same way you all went in all dressed up in green suits, yes.

PARIZEK: And the molds, no one has discussed what they are, or identified them, or done anything with them? The question is relevant maybe from the environment that you create by opening the door, or from just the humidity, and light that you had. It's probably introduced. It's not in the rock? Or do you have things creeping out of the rock? Sally Devlin's bugs.

PETERS: Well, first of all, we collected it when we saw like one of the first entries, and it was analyzed and it's mainly penicillin, for those who are interested in molds. It tends to grow on the railroad ties, the wood ties, and where there was debris left behind. It doesn't appear to grow on the rock.

PARIZEK: This is what the Canadian block experiments were showing, that you actually had a reducing environment inside a piece of the Calico Hills, I think?

PETERS: Yes, they had reducing conditions in the saturated experimented ACL, and they were hypothesizing that

that might be because of microbial growths.

PARIZEK: Yes, and that's again just handling the block that introduces it, or whether it's native to the rock formation is not yet known; right?

PETERS: Right. All I can say is qualitatively when you look at it, it tends to grow. Where it grows is on the materials that are introduced into the tunnel.

PARIZEK: Now, on the tracer experiments with a drilled back or mined back checks on it, how do the numbers of travel time agree, again, with the Canadian experiments? They had both the non-saturated experiment as well as the saturated experiment, and you have some other numbers which you got in terms of the forced experiments by injecting fluids, and seeing that they did break through or they didn't break through at a given reference depth. Are there similar numbers involved?

PETERS: Yes, is the answer, I mean because the ACL experiments are telling us the same thing. The experimental determined sorption coefficients are consistent with what we're seeing in the block experiment. So, I'd say indirectly, yes. I'm not sure if the scientist, I can find out, but I'm not sure if the scientist has done a one, you know, compared it directly. But they're giving us the same bottom line answer, that it's consistent with the lab data.

PARIZEK: I'll pass.

CRAIG: Okay. Debra?

KNOPMAN: Mark, two questions. One, while we're on this, I want to understand a little bit more about the thought process. I mean, it seems to me, as Russ described it, and you, it is a good news and potentially bad news story as to what went on here, because for one, I guess I'm surprised that there wasn't in place already some check on materials for testing purposes. And I'd be curious to know if the manufacturer of Viton had said don't use over 100 degrees C., for example. And if that did happen, did someone just not read a label, or what? What was the case?

PETERS: Okay, I'll take that one first. We've got to go back to the '96, '97 time frame when we put the stuff down whole. Let me back up. We do have an analysis program as part of this test to look at the introduced materials. Did we have one, did we do this analysis for Viton? No. I go back to the '96, '97 time frame. If you read the literature on this materials, it says stable to 200 C. And that's what we looked at back then and said, okay, well this is going into hydrologic boreholes that weren't originally intended to sample water anyway, that we've evolved into them using those pack rolls as that.

So, I mean, you've got a valid criticism, but if you look at the literature, it suggests that it was stable to that temperature range. But we went back to look at the literature when this developed and it also says that it can start to de-gas at lower temperatures. So, in one way, as I look back, having been part of the original testing, I'm critical of myself, because we probably missed this one to some extent.

KNOPMAN: I mean, just closing the loop here has I think fairly strong implications for performance confirmation.

PETERS: That's correct.

KNOPMAN: That virtually everything that's going in there, well, depending on what thermal operating mode you're in, will have a big effect on the equipment, the instrumentation, the longevity of the instrumentation, the confounding factors, none of which I've seen addressed.

PETERS: Well, could I just say one thing, though? I would like to underscore what Russ said, though. I think the success part of this is the way we responded to it, because I mean it was discovered, and we went out and very quickly addressed the issue. And I personally think that should be congratulated.

KNOPMAN: I agree, and I'll congratulate you.

PETERS: Thank you. That wasn't why I said it, of course.

KNOPMAN: That's okay. You did respond well. I mean, I think that's true.

Let me also just on this point, you're sampling superheated waters at 140 degrees C. To what extent was it a surprise that you had as much superheated water to sample? After all, the whole premise of this high temperature operating mode is that you're driving off your liquid water. So, tell us a little bit about what you're finding, in fact, in terms of presence of superheated liquid water.

PETERS: I think I might have confused you. It was steam.

KNOPMAN: It was all steam?

PETERS: Yes.

KNOPMAN: Okay.

PETERS: That's my fault because of the words. It was steam that was condensed in the sample tube as we pumped.

KNOPMAN: Okay. Nevertheless, you still have water?

PETERS: There is steam, yeah, vapor, water vapor in the system. That was expected. I mean, I'm not sure what else to say.

KNOPMAN: Wouldn't you have expected by now that a lot of that would have been gassed off, driven off?

PETERS: Bo can probably address that better than me. But there is a significant amount of water vapor in the air mass, even above boiling. I mean, I'd go to the heated drift, and the relative humidity in the heated drift is still, back in the heating phase, was still on the order of 2 to 3 per cent, which suggests there was a lot of water vapor in the air.

KNOPMAN: Okay. And, finally, just a clarification on Slide 46 when you're talking about the electrochemical testing of Alloy 22, and you talk about testing temperatures, and it stops at 90 degrees C., and I don't understand why you're not testing above 90 degrees C.

PETERS: This particular data does. Right now in the program--

KNOPMAN: Wait, I'm sorry. I guess it was your 45.

PETERS: Oh, yeah, that's the GE data.

KNOPMAN: That second to the last bullet.

PETERS: Yeah, this particular program only went--we've only gone up to 90 C. We have ongoing a plan to go to 120, and then the next question will be, well, what about even higher. That's being evaluated. That's in the plan that's being evaluated within the context of all the planning that you've heard about this morning. So, we're not ignoring the fact that we've got to go to a higher temperature range, is I guess the message.

CRAIG: Dan Bullen?

BULLEN: Pass.

CRAIG: Let the record show that we have had a first. Priscilla?

NELSON: Nelson, Board.

Other than the followup of what are you waiting for, let me ask you what are you doing in the field project to be prepared to validate your evolving ventilation and humidity models for the underground tunnels?

PETERS: We did the ventilation tests, the Phase 1 and 2 ventilation tests, at the Atlas facility that are complete.

NELSON: But there's nothing underground or on site?

PETERS: Right. Right now, nothing underground. The program to address that aspect is focused on the Atlas testing.

CRAIG: Alberto?

SAGÜÉS: The one just a second ago, right there, I want to emphasize a couple of things you apparently are aware of.

First of all, the corrosion rates indicate that they are at about an order of magnitude, or almost two orders of magnitude greater than the corrosion rates which are in the long-term experiments, and there you have about 1 micrometer per year, and in the long-term, you get about .05 micrometers per year, or so, which indicates that those tests are done with either extremely young specimens, very short time tests, and over there, they have obtained an activation energy base of about a fraction of the activation energy that was used for the SSPA study. And the introduction of temperature and the corrosion rates have a tremendous impact on the very long-term performance, and so on.

So, what I want to indicate, and I think you agree, you have a long, long way to go yet before you get data that are going to be usable for the kind of purposes that you need, namely, in order to get a credible estimation of what would be the long-term temperature dependence of the corrosion rate. Is that right?

PETERS: Well, you want me to comment on that?

SAGÜÉS: Yeah. The question is do you agree that this is just barely just beginning to--

PETERS: Yeah. Well, I mean, I don't know if I'd say barely. I think you've heard it from a lot of the previous speakers that we've got--we're going to have to have a continuing testing program, particularly in this area, to address the issues. I mean, some of these tests--some of these we've just started in the last year, some of these short-term tests. So, yes, there's more to do. Are we going to do it? It will be part of the prioritization to do the right thing. But this is certainly a key part of the program.

I'm not going to presuppose. You heard a lot of talk about license application versus continuing measurements, you know. That's all going to have to be factored in, with budget realities, et cetera. But, yes, there's a long way to go in this area to be able to defend the waste package long term, but we've got testing now, and we've got all the monitoring period to continue this testing.

It could be a very long time. So, I'm not personally worried about that. I think we've got time to continue to address that issue.

SAGÜÉS: Because this is indeed crucial.

PETERS: Yes, it is.

SAGÜÉS: To be able to--between, let's say, high temperature versus a low temperature operating mode, because that is at the center of that prediction; right?

PETERS: Absolutely. But I'll bring something up that you didn't bring up that I thought you were going to bring up. This activation energy is different than the one that--

SAGÜÉS: Yes.

PETERS: It's much smaller.

SAGÜÉS: Three to four times less.

PETERS: Yes, it's much less. So, that by itself tells you, okay, we've learned the temperature dependence is less than we assumed in SSPA if you take this at face value.

SAGÜÉS: Well, in these tests, it is. But now the question is are test tests the good ones, or are the smaller tests in Virginia the good ones, you know, which means simply that--

PETERS: Yes. Well, there's a matrix that we have to work through to get to all those answers.

COHON: Cohon, Board.

You don't have to be a corrosion scientist, though, and I'm not--

PETERS: Me neither.

COHON: Right. But he is. --to realize that having no data in the temperature ranges that are likely to obtain for something like 1500 or 3000 years in your scenario is not a good thing. And, I mean, no data, not a little data, no data.

PETERS: Above the 120 range.

COHON: Right.

PETERS: This is just one example, of course, in the test. We've gone to higher temperatures.

COHON: But where you've got me is when I go above 120, which is, I don't know, is it 2000 years? But, anyhow, hundreds of years.

PETERS: Yes, hundreds of years.

CRAIG: Okay. So, we're concluding then the observation that there will be no surprises in this area?

PETERS: In waste packages.

CRAIG: In waste packages, because you can't afford to have any.

PETERS: Well, if I said that, I didn't mean it.

CRAIG: No, I said that. You definitely did not say that.

PETERS: I personally feel that the mountain, the mountain is a good place.

CRAIG: Okay. Don Runnells, last comment?

RUNNELLS: Runnells, Board.

Mark, we see bits and pieces of information on colloids. It's hard for me to put them together, and I know it's fairly early in the program of studying the colloids. But at this point in time, can you summarize for us what your knowledge is, what your feeling is about the potential importance of colloids?

PETERS: First, let me summarize what I think the state of the program is. I think in the generation of colloids from the waste form, I would say it's a more mature program relative to some of the other areas. They've got a better handle on colloid formation from the waste forms. Whereas, the transport aspects through the UZ and the SZ is less mature, and the colloid model for the UZ is new, really just developed during the SR time frame. So, I think we've got a lot to learn. How well do we understand it? That was really your question?

RUNNELLS: That was part of the question, sure.

PETERS: I mean, I think we've got--I'd almost punt that to Bo, because it's a hard question, plus he's the guy who's the UZ modeler, who can speak to that. And then I'd punt the other part to Al, because they've got to defend the UZ and SZ models and how the colloid aspects are incorporated into their models. So, if I can, taking speaker privilege, I might punt that to them.

RUNNELLS: On a scale from zero per cent to 100 per cent, where are you in your knowledge base of colloids?

PETERS: We're not zero, and we're not 100. I want to say we're above 50. But in the UZ and SZ, we don't have-- we're just now developing the field data to be able to even validate those models. I mean, C-wells have colloid data. We're going to do more in the alluvial testing complex. Catch me two years from now, and I think in SZ, I'll be much higher confidence. In the UZ, Busted Butte is maybe going to give us some information. But the UZ, I'd say probably lower confidence than the SZ.

CRAIG: Mark, thank you very, very much. It's been a good session, and we now call this session to a close and move to the public comment period.

COHON: Thank you very much, Paul. Thank you for chairing that session.

Seven people signed up to speak at this public comment period. I'm going to call your names, and when I'm done calling your name, I'm going to ask you a question about your schedule. So, please listen up.

Dennis Bechtel, Andrew Onell or Oneil, Jacob Pazz, Sally Devlin, Grant Hudlow, Bob Williams and Atef Elzeftani.

WILLIAMS: I thought I was signing up for 5:30.

COHON: Good. Thanks, Bob. That helps.

Of the people whose names I just called--Grant is not here? It's getting easier all the time. Well, let me still ask. Of the people whose names I just called, are any of you not going to be here either this afternoon, during this afternoon's public comment period, or tomorrow's?

Okay, I'm going to give you two preference. Are you mr. Bechtel? Presumably no relation?

BECHTEL: No relation.

COHON: Okay. This is Dennis Bechtel. Please state your name again.

BECHTEL: Dennis Bechtel. Unfortunately, no relation.

First, I'd like to commend the Board for holding meetings like this in Nevada, and I'd like to commend the Board for what I feel is very important oversight to the citizens of Nevada and to citizens throughout the country. And, also, I feel your reports are for technical, but reports on very complex issues, are very readable, and as readable as a lot of the topics can be, I guess. So I hope that will continue, and I'm sure it will continue.

COHON: Thank you.

BECHTEL: I was concerned in reading the letter report, you listed a lot of strengths and weaknesses in the Program, and I think at this stage where we're nearing site, potential site recommendation, that's of concern to me. It's not just in the issue of national environment, it's also in the issue, as the last questioning pointed out, in the engineered barrier system. So, I think that gives me pause because we're entering into a very important part of the program, and there seems to be many questions still hanging out there.

I think the police makers are probably going to kind of key on the first three pages of your letter, and maybe less on the background material, and you, quite appropriately, point out weaknesses there. But I think you sort of let DOE off the hook on a couple of areas that causes me some concern. You indicate that there's really basically no scientific or technical issues that would necessarily disqualify the site. But I would point out that given the fact that there's so many maybe inadequacies of the data, you could also say that there's really no certainties about the site as well.

So, I think folks may glop onto that as maybe an unrealistic view of maybe the suitability of the site. And I guess the other point is you indicate that all sites are going to have problems. Well, that's true. But, I mean, there's probably varying degrees of problems, you know, and there could be better sites, perhaps even in Nevada.

So, I hope that if you get to the point where you're actually testifying in front of Congress, that that is pointed out as well, you know, if you feel that's an accurate statement.

The other thing, just in viewing the program today, there were a couple of things that kind of struck me. One is the questions about, you know, whether in fact you've actually got, you know, the reports that you actually need to make decisions. And that kind of blows my mind, in a way. I don't know if that affects your decision. I suppose it could go either way. If you had more information, you may be less uncertain about things. But I'm hoping you're able to get all the reports that you need to be able to do the work you need to do.

COHON: Let me respond to that right now. Indeed, the Board has access to all information it feels it needs. DOE is also forthcoming in providing us reports, even in draft form. What Dr. Runnells was referring to particularly was the work plans that they develop, which we also have access to, but we generally don't see those as they're being formed, but rather after the fact. And his point was we might have some useful input even before they're completely formed, and that's not something the Board has done in the past. But in terms of reports and results, it's completely available to us.

BECHTEL: Okay. And I guess the other point that came up today was the cultural evolution issue. I guess I would like to think, maybe naively, that that was part of the program, you know, before, attention to detail and all this other stuff, and it doesn't seem like something that would, you know, necessarily we're going to salute and we're going to get in with the NRC and suddenly we're going to, you know, change courses. That's good to see, but hopefully that's throughout the program.

The other part, other concerns I have are Dr. Bullen brought up the issue of transportation, which I think, as you indicated, is very important to folks in Nevada. And, you know, the fact that at one time, there were actually members on the Board that actually looked at transportation issues, and I would hope should this project proceed on, that I could see a role for that, a technical role for the committee in actually looking at that, because there's a lot of unresolved issues in that part.

And, I guess lastly, I also am concerned about a lot of the material not being available on the web right now. I'm all for national security, but I think it's important, particularly for the public who may be residing far afield, that they really need to have this information to be able to potentially make decisions.

And, finally, you know, the final EIS is not out yet, and of course we're talking about a potential recommendation to the President, you know, soon, or to Congress, and I think there's a lot of--the public devoted a lot of time to reviewing the draft document, and there's a lot of important issues embedded in that document that have not been, you know, we don't know how they're going to be resolved.

So, I would urge DOE to release that document as soon as possible, because there's a number of concerned citizens, you know, throughout the United States that would like to find out how they're going to attend to those issues.

So, thank you.

COHON: Thank you, Mr. Bechtel.

Let me just point out there are indeed still members of the Board very interested in transportation issues relating to nuclear waste, not only interested, but have expertise in it. And we stay current and informed on those issues, and we are quite prepared to get involved and take them on. Thank you.

Now, Mr. Elzeftani, since you will be leaving-- where did you go? There you are. Please, and if you'd state your name again for the record, since I'm sure I didn't do a very good job in pronouncing it, it would be appreciated.

ELZEFTANI: With this Aladdin and all these other things, probably the American people started to get familiar with these crazy names. So, I was born and raised in Alexandria, Egypt more than 50 years ago. My name is Atef Elzeftani, simple. Too many letters. Somebody called me Mr. Alphabet, but that's fine. Technically speaking, I'm a hydrogeologist with--finally, I got my Ph.D. from Alexandria in 1989, approved after Nassir kicked me out from Egypt because I was talking about the civil rights back then.

But, anyway, I got my second Ph.D. in physics from the University of Florida back in 1974. I got involved with the Chester C, some of you might have heard the name. He was the department chairman over at the University of Illinois, and he got me involved into this nuclear waste situation, because he was a member of the ACRS of the Nuclear Regulatory Commission. Well, that's really the short story.

I always wanted to stay in a dry climate, so I left Illinois with my wife. We came to Sin City, as they called it back then, which is Las Vegas, Nevada. Now, as I was driving this morning from Las Vegas, it dawned on me that, boy, Las Vegas about 25 years ago, it looked like Pahrump Valley. I haven't been here for about maybe two or three years. But some of you will drive, you will see the immense part of the valley when you go back to Las Vegas, and you'll find out that houses--now, it's all over the place, pollution problems, air pollution, traffic, and all these other things.

Now, when I moved out here, I really didn't, after I became a citizen in 1974, I had no idea about the Native American, who used to live here some time ago. Don't take me wrong, please. So, I was asked one time, well, the Congress is considering the six sections for the Paiute tribe, where should we put them. Back then, Howard Cannon and the other guys. So, anyway, the Congress gave the Las Vegas Paiute tribe a piece of land, which is on 95 as you drive from here to the Nevada Test Site. That brings me to why I'm here.

I was planning to come just to see what's going on, but the tribal chairman said, well, get in your car and go over there to that meeting and tell them the following. They had a tribal court. That's why I was late. Anyway, so I'm here on their behalf as a sovereign nation of the United States.

Some of you may not know that, but our 650 or so federally recognized tribes, Native American tribes, they have their own sovereignty more or less equal to the state sovereignty, and the story is so long. So, their unofficial position now is that Yucca Mountain is not good for the tribe. And I was asked all these other questions by the seven members of the tribal members about the technical part.

Now, to go back to the technical part, back when I worked for the NRC and the Waste Management for about three or four years, 10 CFR 60, and Dan Fehringer and all this group, we kept wrestling with the Nevada Test Site, and the unsaturated zone. One thing I did realize as an unsaturated zone, or call it unsaturated zone hydrogeologist, I said fracture flow is going to be prominent.

Some of you members who are no longer here didn't believe that back then when you guys got together by the Act of Congress. It took the DOE, what, 10, 15 years to realize that there is such a thing called maybe a fracture flow, and you need to consider it, not this .0001 millimeter of recharge.

And then my other concern, and concern of the tribe, is you can't model the site. Maybe you can build a permit, but you have to show them that it's going to last for 5,000 years. Nobody knew until you really live it and you see it, as I saw it 35 years ago.

Now, what I'm saying is as the technical people as you are, there's two things. Just about a week ago, it dawned on me that this little--in Alexandria, I grew up with it for 21 years before I left, and it dawned on me when they were talking about the contest of silting, that this is really the head of a clay. That's the literal translation of the word. And here it is. I'm 53 years old, born and raised in Alexandria, and it finally dawned on me why they called that area that name.

Now, I'm on the--infinity, plus infinity is Albert Einstein. Kept thinking the specific heat of the diamond for you ladies are lower than everything else. Five, six years later, he proved that this is because the quantum theory.

Now, we can argue about the technical things. The DOE has spent a tremendous amount of money in the technical aspect, and I don't have any problem with that. If I am the president of the United States, I would say scrap it. Everybody got a good job. We'll finish it.

Now, here's one last thing. They are, in a sense, the council is outraged with regard to the visit of the Department of Energy secretary comes down here, goes through the tunnel. A day later, or two days later, he calls the governor and says we're going to recommend the site.

Now, we can argue about the technical issues for a long, long, long time. But I feel that we will never be able to put our hands around it 100 per cent, or 90 per cent even. with the performance assessment and modeling, and all the technical data that has been generated for that time, and I'm very familiar with it.

Now, the official position is, number one, we would like to see the tribe, or the Native American tribe people, get on the mailing list for this Nuclear Waste Technical Review Board. That's number one.

Number two, somehow, somewhere, but I did fight with the NRC and the NRC chairman came here and met the chairman of the tribe, and some of the other commissioners, I'd like to suggest on their behalf that either some of you members of the committee or the chairman of the committee stop by sometime for a private visit. We will lunch you and things like that. But you need to get the word from the horse's mouth. These are two imperative points I'd like to make after all that story.

So, best wishes for you. I've been delighted to see a lot of technical things happening, and all kinds of things like that. I know we're getting gray hair like me and losing hair, and all that. So, keep at it, and hopefully we will reach another agreement.

One other point after--also, the last point is transportation issues. We've seen the unimaginable. I mean, I personally had nightmares for a month. I haven't lost anybody there, and I haven't lost anything, but I woke up many, many times dreaming of what I saw. That's unbelievable.

Now, the scenario that it comes so close to us is what are we going to do with the transportation. Glenn Seborg, when I met him for the first time and the last time in 1986, said the Congress needs--you know who Glenn Seborg is, he's passed away now--he said, when I asked him about that question back in Berkeley, he said the Congress needs to change the law, reprocessing and using, well, we call it waste, but it's not waste, and I think if that goes into the political arena, then something might change. We might be out of a job, all of us, but maybe that's an opportunity.

Thank you very much, and I appreciate it. I'm sorry if I'm not going to be here late afternoon. I appreciate it.

COHON: Thank you, Dr. Elzeftani. And I did not take that hair comment personally.

Dr. Elzeftani, would you give the mailing information to one of the Lindas sitting over there, so we keep the people on the mailing list?

Dr. Pazz and Mrs. Devlin, if you wouldn't mind, and if you'll still be around, could we invite you to comment later? Thank you, Dr. Pazz. And, Sally, thanks. I appreciate your accommodating our schedule.

We'll take a break now until 1:30. Have a nice lunch, and my thanks again to all the speakers.

(Whereupon, the lunch recess was taken.)

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

BULLEN: Thank you, Chairman Cohon. Since Jerry did such a very nice job of outlining the entire meeting this morning, we can dispense with any introductions of the next session and we'll just move right into the presentation which is on regional saturated zone model update by Frank D'Agnese.

Frank?

D'AGNESE: Thank you.

I was asked to give sort of an overview and an update on the regional saturated zone modeling. When I went back to my files, I realized that the last time I had done something like this was January of '97. So, a lot has happened since January of '97. So, I have 20 minutes to review five years.

Just to give you an update or, at least, a historical picture, this is where we were five years ago. In 1997, we published a Water Resource Investigation Report, 96-4300, which described the 3-layer, steady-state, MODFLOWP based regional groundwater flow model of the Death Valley region or the regional groundwater flow system on which Yucca Mountain sits. We also published early in '98 a report that described these simulated effects of past and future climate changes on that regional groundwater flow system. Around that same time, the Nevada Test Site underground testing area's program or project also released a 15-layer, steady-state, MODFLOW model of roughly the same regional area.

This shows the boundaries of those two models; in black, the boundary of the regional 3-layer model developed by the Yucca Mountain Project, and then in orange, what is called the Nevada Test Site regional model boundary.

So, if we go on to the next slide, please? As a result of these two models being released roughly around the same time, these different groups within DOE, the Yucca Mountain site characterization office and then other groups within DOE Nevada Test Site, the underground testing area's program, defense programs, and hydrology resources management program, approached the USGS and asked the USGS if we would embark on a study of synthesizing these databases, these geologic models, these 3D geologic models, and these groundwater flow models for the purposes of satisfying the needs of these four different DOE programs.

Go on to the next slide, please? The short-term goals conducted between the years of '99 and 2001 which have just been completed this past year was combine the DOE models and the datasets, characterize 3D flow paths, calibrate a steady-state model, estimate the flux magnitudes, determine the potential effects of actual geologic structure to include explicitly geologic structures into this regional model, and improve upon the sensitivity and uncertainty analyses that were developed in the previous two regional models. That has since been completed and we'll show you where we are right now.

Long-term, fiscal years 2002 to 2004, develop a model that would potentially evaluate things like pumping impacts, be appropriate for providing a technical basis for water appropriations, be able to be used for designing effective groundwater monitoring network, and ultimately be used not only by DOE, but other stakeholders in the region within the groundwater basin as a groundwater management tool for the Death Valley groundwater basin.

Go on to the next slide, please? At the time, we were concerned with what we called recent program reviews and findings and this is five years old now, but I just want to remind you what types of things we included or were asked to include. Information from the saturated zone expert elicitation which was conducted in the '97-'98 time frame, external peer reviews that were conducted on the UGTA program, comparison of the two models, concerns by the NWTRB on data south of Yucca Mountain, the Nye County early warning system that was coming on line, underground testing areas, corrective action unit studies that were being conducted.

So, to have an investigation that synthesizes the existing data, but also includes this data as it's coming on line and we've done that.

Next slide, please? So, what the USGS offered to DOE was that this effort would have five components; an integrated modeling database, live interactive database, a comprehensive geologic interpretation which would update the geologic conceptual model through the 1990s, a 3D hydrogeologic framework model synthesizing the two existing geologic models and improving upon that with this more comprehensive geologic interpretation, a regional hydrologic conceptual model. The issue here is to reduce the uncertainty that exists in the various components like groundwater discharge, groundwater recharge, those types of things. And then, ultimately, a calibrated flow model. Each one of these components, we suggested, needs to be independently documented, clear QA, have clearly assessed levels of uncertainty, and also describe alternative likely hypotheses for conceptual models.

Next slide, please? And then, also take into consideration other stakeholders in the basin in the region like Department of Defense, Nye County, Fish & Wildlife, Park Service, that sort of thing.

Next slide, please? So, there are really five major activities that are based on those five major components. In Work Package 1, the regional database, here, we're integrating data, not just point data, well information, water levels, lithologic logs, geophysical logs, but also spatial GIS data so we can conduct analyses, share that data and ideas, and also use that as inputs to the models. A comprehensive geologic interpretation, this is synthesize geologic maps, tectonic maps, cross-sections, and geophysics. These have since been published. This cross-sections, the geophysics, the geologic maps, and tectonic maps are about to be published. An improved 3D hydrogeologic framework model, first, the synthesis of the two existing geologic models from the underground testing areas and the Yucca Mountain Project and then ultimately a synthesis of this new, improved geologic interpretation into that framework model. Reduced uncertainty on evapotranspiration, recharge, water use, and hydraulic properties, and then ultimately a steady-state groundwater flow model, and down the line a transient groundwater flow model.

Next slide, please? This is the boundaries now of what we call the Death Valley regional flow system model. It includes all of the areas that were modeled by the Yucca Mountain Project 3-layer model and the 15-layer underground testing area's model and it also includes the west side of Death Valley. So, it includes the entire, what is considered, the groundwater basin of the Death Valley region.

Next slide, please? So, to update you on the short-term goals that have been achieved, we have delivered a site saturated zone model with updates to the site saturated zone modeling group. We had updates to them mid-year fiscal year last year, late in fiscal year 2001, and again early this year. This model includes a synthesize of all the regional hydrogeologic data, point data, that exists in the basin to this point. It includes a hard-merge, what we call a hard-merge, of the geologic model from the Yucca Mountain Project and the underground testing areas geologic models. It has significantly more hydrogeologic units, true hydrogeologic or hydrostratigraphic units, and it also includes faults, hydrogeologic structures explicitly in not only the framework model, but the flow model. We have what we would consider an improved or quantified uncertainty in the discharge and water levels that was independently documented, particularly the discharge, in another report. And, the model is not just three layers now, it's 15 layers, 15 flow model layers.

Next slide, please? Some of the important things here with this updated steady-state model is a significantly more quantified sensitivity analysis/uncertainty analysis. And, this is just an example of the type of output that we would get, what we call parameters of composite scaled sensitivities. This is a measure of the relative sensitivity of defined parameters relative to other parameters. So, right here, a parameter by the name of K211HZONE8 is significantly more sensitive than some of these other parameters here farther down the line. So, it's a measure of which parameters are important based on the observations that are being used to constrain our groundwater flow model. The constraints would be water level observations, hydraulic heads, and groundwater discharge or flows.

Next slide? Also, we have a dimensionless scaled sensitivity. This tell us for a given parameter--we'll just call this the red parameter for now. We can tell which observations contribute more information to the estimated parameter value of a given parameter. So, we can actually say, well, if we want to reduce uncertainty in an estimated parameter value, perhaps then what we should do is find these three or four or five observations and reduce the uncertainty in those measured observations and that would further constrain our model and give us a better estimate of those values. So, it's a better way of determining which parameters are controlling our predictions.

Next slide? This is the last slide. Long-term, our goals are to incorporate this new comprehensive geologic interpretation. In addition to the regional hydrogeologic units, there are the local hydrogeologic units that are consistent with the site saturated zone model. This would add even more hydrogeologic units to our regional framework and flow model. We would have improved hydrogeologic database. This would include all of the recent data coming out of Nye County, as well as the underground testing areas program being conducted on the Nevada Test Site. This next version of the model would be a combined steady-state and transient simulation again with uncertainty and sensitivity analyses. And, ultimately, the final report would have all the available data available along with the framework and the flow model available through the Internet.

Thank you.

BULLEN: Thank you, Frank. Actually, the best laid plans of staff lay out an agenda that basically says that we have discussion on this in about a half hour or so or maybe almost an hour. But, since you got done early, what I'll do is take Chairman's prerogative here and ask if could have a few questions now and then I'll cut it off at the time frame and we'll go on to the next presentation.

Do I have questions from the Board? Don Runnells to start with? And, I want to remind the Board Members to speak into their microphones so that we can get it transcribed and everybody can hear us. So, Dr. Runnells, it's all yours.

RUNNELLS: Runnells, Board. Frank, on the way down here, I was reading the final report of the International

Peer Review Committee on TSPA. They are very critical of the USGS saturated zone modeling effort. Can you comment on that? I know Debra pointed out to me that their review is based largely on the 1996 reports, but incorporating that into your answer, do you feel that you have addressed the criticism, the specific criticisms, that the peer review panel have on the USGS SZ model?

D'AGNESE: Yeah. I think I can address that. And, yes, that panel review was of the 1997 report, and therefore, the 1996 model. I think that the criticisms that the panel have there are very similar to the same sorts of criticisms or comments we got out of the saturated zone expert elicitation that was done many years back. They're also many of the same sorts of criticisms that we, ourselves, documented in the report, in the 1997 report.

I was also reading the comments as my colleague was driving to Pahrump. So, I would kind of group the comments from that panel into four different types of criticisms. One would be comments that were sort of misinterpreted in the report. In other words, we actually noted those as limitations in the 1997 report and perhaps the panel just didn't really catch on to that information or we didn't make it clear enough in the report that we acknowledge those as limitations in the '97 report.

There were criticisms in the International review that have already been addressed now in the short-term goals. One example is just more detail in the hydrogeologic layers, what they called under-parameterized. The technology has changed significantly in 10 years and we went from 20 defined or 25 defined parameters and nine estimated parameters to something like 200 defined and 35 or 45 estimated parameters in these current models; so, significantly improved. So, those are addressed.

The third category that I would have describing the International Peer Review is criticisms that are not addressed yet, but they're slated to be addressed in the upcoming model. I've written down--an example is including this new comprehensive geologic interpretation. They called for including a lot of the available geophysical data which we agreed and we said that in the '97 report and we're getting that in now. And, also, one of the recommendations was that you should consider recharge in the femoral streams. That is now being brought into the model as we speak. So, that would be the third kind.

The fourth kind would be those types of details that we've also recognized as limitations to previous models. We've discussed that with DOE and other stakeholders. We recognize that they're needed. But, because of time and fiscal constraints, they have not yet been included and they're not currently planned to be incorporated into this current version.

So, that's kind of how I would break those down.

RUNNELLS: Okay, thank you. Just a quick followup on it. Thanks for your answer. That clarifies a lot for me. I guess, I'm puzzled as to why the International Peer Review Panel in a report dated last month was reviewing 1996 models. Why wasn't there better communication between someone, USGS and DOE and DOE and the peer review panel, so that they would be reviewing things that are more current than 1996? The cover letter is this month. It's January of 2002.

D'AGNESE: Yeah. Yeah. All of the components leading up to the short-term goals that's being delivered as a result of the 2001, the final report, I've got it on CD with me, but that final report has not yet received USGS director's approval. So, that's why that hasn't been. As to why there couldn't have been a little more communication with the International Panel on the products that are coming out now and some of the more recent publications that are synthesizing this data, I don't think I'd be able to answer that.

BULLEN: Jeff Wong and then Debra Knopman?

WONG: Jeff Wong, Board. I just have two clarifying questions. So, none of this latest thinking has been incorporated into the models that are used currently to support DSR?

D'AGNESE: That's correct. What was used by the regional input to SR was from the '97. As to how this latest stuff is being incorporated into the site model, I'd let Al Edderbbarh and George Zyvoloski talk about in the next presentation.

WONG: Do you think that any of this new thinking would change conclusions about performance?

D'AGNESE: I guess, I don't want to pass the buck to George and Al, but they've been using some of these latest results. So, they may be able to tell you how this is changing their results.

WONG: Okay. The last question I have is on Slide 12. You said that these are the parameters that are more sensitive and I don't--they're more sensitive to what or are these the parameters that are the most sensitive in terms of changing the result of your model?

D'AGNESE: This is just an example slide. So, I'm not going to say that these are specifically the ones that are in the final model. What we're describing here, it's a relative sense. What we do is we go through and we calculate for every given parameter the contribution of a given observation to help constrain the estimated parameter value. So, for example, this one far on the left, that particular parameter and the value that's estimated for that parameter, what that's saying is there's a lot of observations, whether they be heads or flows in the groundwater flow model that are constraining that as opposed to this. So, for example, this is HFB of Death Valley. That's the Death Valley Fault. And, what we're seeing is that there's not very much water level constraints or groundwater discharge constraints that are helping constrain the hydraulic conductivity of that particular valley. So, it's relative. What that tells us while we're calibrating is we're probably never--or, at least, given the current dataset, we don't have enough information to tell us much about that. Perhaps, it would be best to come up with a best estimate.

As opposed to these on this far end, we have a lot of information in our dataset that constrains the parameter values that we're going to get at. What that translates to is when we're making a prediction, we can also--we could do a similar thing. We make a prediction like potential advective transport from the facility. The question is which parameters are controlling that particular prediction and then how much information do we have about those particular parameters. So, if we have a parameter that is really important to our advective transport prediction, but it's somewhere down on this end, that's not very good. We want to know more about that parameter as opposed to predictions that are constrained by parameters that we have a lot of information about. So, it tells us then, well, what do we do? Do we go out and get more information, that sort of thing.

WONG: Thank you.

BULLEN: Debra Knopman and then Richard Parizek?

KNOPMAN: Frank, just to clarify, you haven't shown us any results. Is that because it's all in the next presentation or you're not--how come we're not seeing any outputs?

D'AGNESE: What it came down to was a time constraint. I was trying to give an overview of what we've done in the last five years. We have that information. Again, the report is close to release, that sort of thing.

KNOPMAN: Okay.

D'AGNESE: If you have specific questions, we can go through it.

KNOPMAN: Yeah. Well, let me just ask a specific question. You've got now a much more parameterized, more parameter intensive model, which means you need a lot more data to support the parameter estimates. Do you have off the top of your head a sense of how many data points you, in fact, have that you're using to estimate the model parameters?

D'AGNESE: Off the top of my head, I might have to point to my colleague in the back of the room, the number of flow observations. What we've done now is we've actually quantified groundwater discharge from every natural groundwater discharge site within the Death Valley region. So, we have a measurement and we have a coefficient of variation on how well we think we understand how much is discharging for every discharge point except for the Death Valley salt pan and we were able to use some recent estimates. So, that's rare that you would have a groundwater flow model where you've actually measured just about every discharge point and you're using that to constrain the model. As far as water level observations or heads, hydraulic heads, it's not so much that we've increased the number of values used, but we have actually done a much more methodical diagnosis of the quality of those measurements and quantified, you know, the target heads and the uncertainty that those are measured. As far as the number of heads that we now have within the region that we're using target heads, steady-state--

SPEAKER: 670 heads and about 50--

D'AGNESE: Right. So, that's 670 hydraulic heads and those are sort of average because we're looking at long-term averages. If we were looking at number of measurements, thousands, tens of thousands, 20,000, something like that, actual measurements within the region over the record.

KNOPMAN: And, total number of parameters now in the model?

D'AGNESE: We have--
SPEAKER: 223.
D'AGNESE: 223 defined parameters and those that were estimated using nonlinear aggression, 30 something--34 or 35 estimated parameters.
BULLEN: Richard Parizek?
PARIZEK: Parizek, Board. Frank, how did you handle faults? Can you maybe elaborate on specifically how the faults are being treated in the model?
D'AGNESE: Right, right. In the geologic model, in the three-dimensional geologic model, the faults are included essentially to create the tops of hydrogeologic units. So, there's an offset. There's a discontinuity in a unit and then it's offset. And, that offset is delineated by a particular fault. But, clearly, when we put that into a groundwater flow model, what we're trying to do is not just show the offset, but also that fault has a width and it has properties. And so, what we've done is we've used a package within the MODFLOW package called the horizontal flow barrier package where you specify the location of a fault in between a model cell. You specify the width; so, some idea of what the width is. And, in many cases, we extended that fault through the entire section of the flow model. That's where we come up with these HFB or horizontal flow barrier parameters. We then started out by giving them some kind of

a conductance value, relatively no impact or barrier to flow and then changing them to extremely high barriers to flow. Then, also, we went in there and actually calculated a sensitivity to determine whether or not these things are actually significantly affecting the results of the model or not really affecting the results of the model.

PARIZEK: So, a lot of the hydraulic properties of the fault zones are arrived at indirectly, more or less--

D'AGNESE: Absolutely.

PARIZEK: --through really calibration process?

D'AGNESE: That's exactly--

PARIZEK: And, not any new field data specifically on these faults.

D'AGNESE: That's right.

PARIZEK: I guess from a transport point of view and a site-scale model, it becomes a bit of new need, I mean, to talk about the role of the faults and their--

D'AGNESE: Right. And, actually--

PARIZEK: --properties.

D'AGNESE: Again, I'm not going to put words in my colleagues' mouths, but I'm sure they're going to have some kind of a discussion about how they're handling these discontinuities, as well.

PARIZEK: And, you show a time frame for the non-steady model updates as 2004. That's about the LA time frame. The odds are those findings won't be available in time for LA space, very likely, in view of the time it takes to get this out, get a peer review, and accepted internally. So, it's very possible that a transient model that has maybe improved predicted capability may not be used?

D'AGNESE: That's correct. That's correct.

PARIZEK: So, the cutoff for new data that would go into your model for 2004. So, if Nye County continues drilling, when do you stop putting data in?

D'AGNESE: Well, the nice thing about it was we've gotten the process for moving the data to the geologic model to the flow model so much improved that even with this latest model, we were able to continue to add water level observations, those types of things, into the model up until just about to the very end of the modeling process. So, if we continue in that vein, we should be able to continue to update our database, our framework, and our flow model almost through to the end of the modeling process. At some point, obviously, in late 2003, 2004, we'd have to cut it off because we'd have to start to move the report through the review process.

PARIZEK: Your inventory of water withdrawals included in the model in terms of, say, pumpage in Amargosa Farms, here in Pahrump, and elsewhere?

D'AGNESE: That was a pretty massive undertaking that's gone on and that's just being completed this fiscal year. So, it's a complete inventory of water use pumping within the Death Valley region over the entire historical record. That will be included as information that is input into the transient model.

PARIZEK: Okay. So, like here at Pahrump, if you pump water, you can take it out of the flow system, but if it goes back as sewage, therefore you've got to put something back. Do you put anything back?

D'AGNESE: We're--

PARIZEK: --say, in the--

D'AGNESE: We're working out the details of how we want to handle those type of complexities right now.

PARIZEK: Okay.

BULLEN: This is Chairman's prerogative again. I know Leon has a followup question and so does Debra, but what I'd like to do, Frank, is ask you to take a seat and we'll get Al and George up here to make their presentation and then we have 25 minutes for more questions and we'll continue at that time. I apologize to Leon and to Richard and to Debra, but we'll try and stay on schedule because our Chairman set such a great example this morning, both Paul and Chairman Cohon.

Our next presentation is site-scale saturated zone model update and integration of new regional and site-scale models by Al Edderbbarh and George Zyvoloski. I think it's a tag team, is that correct, or are you first, Al, or are you going to do it all?

EDDERBBARH: Well, I'm going to do it all and George is going to keep me honest.

BULLEN: He's going to keep you honest. Okay, great. Thanks, Al.

EDDERBBARH: Good afternoon.

What I'm going to share with you this afternoon is the evolution or development in the site-scale model. As Frank showed us before, the site-scale model is an area of the regional model and it's integrated somewhat with the regional model through flux, boundary conditions, and also through recharge and, hopefully, through the hydrogeology and the hydro framework model. The work that I'm presenting here is the efforts of a team of scientists from BSC, Los Alamos National Lab, Sandia National Lab, and the USGS, aside from the regional team which is providing the regional model.

This afternoon, I will talk about the new data and analyses that we have incorporated into the site-scale model since we last presented to you the status or the conditions of the site-scale model. Then, I will talk about the updates of the model and I'm going to concentrate mainly on the flow model and I'm also going to talk about the integration between the regional and site-scale saturated zone model. I will conclude with multiple line of evidence that we have been conducting aside from TSPA and also aside from the mechanics of building the site-scale model, calibrating the site-scale model, and running the analyses.

Next slide, please? As you know, the main area of the new data that we have incorporated into the site-scale model is the data that was collected in cooperation with Nye County. We will be talking about that a lot and also data from the ATC and the ATC testing, both the hydraulic and the tracer testing. And, also, data that was obtained by USGS and by Nye County, mainly aero-magnetic data and other geological mapping data. And, this slide here shows some of the existing and planned wells from Nye County and it also shows some areal plane for the cross-sections that have been developed using lithology data and--sorry about that.

Next slide, please. This is one of the Nye County geologic cross-sections that was developed in Denver by the USGS and 22S is already drilled and we have information from it that we use to develop this cross-section here. 20D is in the plans. So, once we put 20D in place and we get the lithology data from it, we will be able to see how we faired in this conceptualization. And, this cross-section here, particularly, is very important because it goes north to south along the inferred flow paths from the potential repository to the accessible environment. It's also important because it's helping us reduce the uncertainty in the transition zone, that transition where the water table goes from being in the volcanic tuff to being in the alluvium. And, it's very important from a transport point of view because the conceptual model for transporting the alluvium is different from that in the volcanic tuffs and also because the alluvium has more potential for suction, and therefore, delaying the transport of radionuclide into the accessible environment.

Next slide, please? Again, this map was obtained from the Nye County work site. This is their program. It shows the existing wells and also the planned wells. We have been working very effectively with Nye County, giving them feedback on what kind of information we're getting from the models in terms of where well point could get more buck for the money. This area here, if you recall from the expert elicitation panel, was called the Data Hole and I think we presented it before you like three or four years ago and the question was the Data Hole. And, now, thanks to the efforts and cooperation with Nye County, the USGS, and DOE, this Data Hole has been filled.

Next slide, please? Again, I think you have seen this before, but basically the flow model that we are using is a 3-D model that extends 30 kilometers east to west, 45 kilometers north to south, and is 2750 meters thick, and the grid resolution contains 19 hydrogeologic units. I mean, these are the different units that are characterized in the model. Now, the layers are more than that because sometimes we have more than one model layer in one stratigraphic unit and the model layers are very thin at the top of the model, 10 meters at the top and they are as wide as 500 meters down at the bottom of the model. We use water level measurement in wells for calibration purpose. We also use hydrochemistry data to guide the calibration efforts and to kind of support the flow path generated by the model. And, we also use a very important feature of the site which is an upward gradient from the lime carbonates into the surficial aquifers which are of our concerns in terms of transports. That's a very important feature because that upward gradient tends to keep flow paths generated or emanating from Yucca Mountain at the water table surface. We also use a range of measured permeabilities both from cross-holes like the C-well testing and now the alluvial testing complex and also from single wells all over the sites.

Next slide, please? Okay. For the numerical model, as we have discussed before, the boundary conditions are specified heads and these heads are extracted from the regional potentiometric surface that's used in the regional model because it has a lot more data and it has a lot more control of, you know, flows and recharge and what have you. Then, we use the specified flux on the top of the model and we obtain recharge from three sources again. The UZ site-scale model for the footprint of the repository and recharge from the regional model everywhere else, with the exception of Fortymile Wash because Fortymile Wash, as one of the Board Members pointed out earlier, Fortymile Wash has some ephemeral recharge, and since it is along the flow paths, we wanted to capture that. So, a special study was done to estimate the recharge from Fortymile Wash and it was used as direct input into the model. Now, as far as the water budget, the regional fluxes are used as calibration targets meaning that we tell our automated inversion calibration routines we want the fluxes to match that, just like we are asking it for--to match the water levels. So, it's a calibration target, it's not the direct input; rather, parameters that guide the calibration exercise. We used steady-state model. There is no change in the storage, and so far, we have been very lucky in that we have been able to preserve mass balance and the mass balance error is very negligible.

Next slide, please? Since the TSPA/SR, we have embarked on a series of sensitivity analyses to evaluate other conceptual models; i.e. the conceptual models of the large hydraulic gradient in TSPA/SR. We used the water level north of Yucca Mountain as a large hydraulic gradient, large hydraulic head. In another conceptual model that's documented in the recent revision of the water level AMR, we are presenting a different--an affirmative conceptualization that's also likely to occur and it is those water levels are perched waters. We also removed the east-west barrier and replaced it with some thermal alteration rock scenario and the result of the sensitivity analyses are summarized in this flow path comparison. On your left here in red is what we used in TSPA/SR, and on the right here in blue is the newer model that we have used in the expected case analysis that reflect other conceptualizations including the large hydraulic gradient, including Solitario Canyon, including anisotropy, and what have you. And, what we need to conclude from those two flow path figures is that in the blue here, the flow paths are longer meaning transport time will be longer, and also since the blue path lines go to the east and back to the south, the flow paths linked in the alluvium is much longer than what was used in the TSPA/SR. So, so far, all our analyses and studies are kind of confidence building multiple lines of evidence telling us that what we have used in TSPA/SR is conservative.

Next slide, please? This slide did show us the different region and what kind of anisotropy we're using. In terms of horizontal to vertical, we're using a 1 to 10 ratio. In the areal plane we're using a 5 to 1. I think, Dick, you asked before about the faults. A lot of the faults are mapped explicitly in the site-scale model. They are mapped in the hydrogeologic framework model. We have hydraulic conductivities that are much higher than what's surrounding them. Even within a fault, the hydraulic conductivity along the fault is like five times and sometimes 20 times larger than across the fault.

Next slide, please? Again, the new data that we were able to use in the calibration validation activities are a new hydrogeology from the Nye County data, from aero-magnetic surveys, from geological mapping, water level data from Nye County wells, and we also have been calibrating to study the impact on the grid size on calibration and those are ongoing studies.

Next slide, please? The integration of the site and regional models. Now, as Frank presented, the regional model has evolved a lot since the 1997 model which was used for the SR and so did the site-scale model. So, both site and regional models continue to evolve and the most recent regional model flow is in review, and as Frank had presented, it differs quite a bit from the one we used in SR and that's why the International Peer Review Team reviewed the 1997 model because it takes a long time to carry the whole process. At the time when we were building our site-scale model, the only thing available to us was the 1997 model. So, it took time, you know, to develop the site-scale model, to calibrate it, generate flow field, feed it to TSPA, do a TSPA analyses, and do the documentation. This is the whole process.

Next slide, please? The plans to integrate the two models as we have them right now is to use the same hydrostratigraphic framework model. Before, we had two parallel efforts; one to feed the site-scale model with the hydrogeologic framework model and the other one was to feed the regional model. Right now, that effort is combined to one and we'll be able to just extract the site-scale hydro framework model from the regional scale model. That will insure a certain degree of consistency in terms of hydrogeology used for both models. Both models will use the same zonation within the site-scale model to subdivide hydrostratigraphic unit for parameter estimation. Now, grant you, the site-scale model has a better resolution. So, we'll have more subdivision within the site-scale model than you will have in the regional model. We will use the same numeric grids that will coincide; i.e. the regional model grid is 1500 by 1500, the site-scale model is 500 by 500 meters and may be smaller. And, what we have here is that within a regional model grid, we will have nine site grid that coincides with the boundary. So, we don't have overlapping between grids. We will use the same depth of extent, whether that's going to be 2000 meters or 2750 meters. And, we will use consistent hydraulic properties. The regional model used permeability for calibration because we don't take into account the temperature issue we do in the site scale model. That's why we use the hydraulic conductivity. The regional model used hydraulic conductivity and we used permeability and we will make sure that the two are consistent. And, we will be using consistent boundary fluxes from the regional model.

Next slide, please? Now, I go on to the multiple lines of evidence. When I was preparing this presentation here, I was a little bit influenced by the International Peer Review Team comments. One of the comments that they came up with is the differences between single-hole permeability data and cross-hole permeability data and I will talk to that. And, I also wanted presented here some groundwater carbon age analyses that we have done to support a UZ/SZ transport time analysis that was done independent of SR and independent of TSPA.

Next slide, please? The issue here with the evaluation of single and cross-hole permeability data is that single hole permeability data indicated that the permeability of any material decreases with depth and that's consistent with intuition as you have more burden as you go down with depth. However, in contrast to that, the result from the C-well cross-hole testing indicated that to the contrary of what we observed with single permeability data, the permeability increases with depth. So, that was a point that was identified by the International Review Team that reviewed the TSPA.

Next slide, please? So, the answer to that is the cross-hole test permeability of the C-well increases with the proximity of test location to Midway Valley Fault. I mean, that contradiction was able to point us out to a very important feature of the site, the importance of faults in terms of hydraulics and transport. Right now, we are proceeding with a high-resolution numerical simulation of the C-well cross-hole tests to determine the permeability of the faulted and the unfaulted rocks. So, we'll be able, you know, to gain that understanding in terms of what contribution the faults and what contribution the rock do in terms of the hydraulic properties and transport properties.

Next slide, please? This slide just shows the combined UZ/SZ air and water permeability data. And, as you see here, the logarithm of the permeability here decreases with depth.

Next slide, please? This shows the single-hole test, the cross-hole test, and also the model calibration points. And, George is taking this into consideration. George is the nuts and bolts of the flow modeling. So, he is taking this insight here into consideration.

Next slide, please? Now, I will talk about the use of Carbon-14 dating to corroborate results from an analysis that we completed to estimate transport time in the UZ and the SZ from the potential repository horizon all the way to the accessible environment independent of TSPA. The result of that analysis--and that analysis is documented in the Twiller (phonetic), the White Paper that was completed a few months ago. And, it's also documented in the UZ Expected Case White Paper which is available now. The result of that analysis is corrected groundwater C-14 ages are 11,000 to 17,000 years. The uncorrected ages are about 12,000 to 18,000 years. And, this corrected groundwater carbon age are consistent with the combined UZ/SZ unretarded advective transport, if you make one more correction and there is a correction from the ground surface to the potential repository horizon.

Next slide, please? We were asked in the middle of the presentation to put a slide or two on the data and analysis that we incorporated into the SZ Expected Case White Paper, and which some of it was also documented in the Twiller White Paper. For the Expected Case White Paper, SZ White Paper, used the most recent stratigraphy and hydrochemistry from the Nye County wells. We also used the most up-to-date data from the hydraulic testing that was completed at the ATC, and at the time of completing the White

Paper, only single-hole testing was completed. So, we incorporated hydraulic and tracer testing data from the ATC into our understanding that went into the SZ Expected Case White Paper. We also took the benefit of what we learned from the calibration of the different conceptualizations of the large hydraulic gradient into that White Paper and we also did analyses for the new compliance boundary which is only 18 kilometers as opposed to the 20 kilometers that was done in the TSPA/SR.

Next slide, please? Bo later on is going to talk about the UZ part of it. But, what we have here in this figure is a figure that's documented in the SZ Expected Case White Paper and the figure here shows the transport time breakthrough curves for the UZ and SZ separate. This is the SZ in black, this is the UZ, and in red is the combination of the UZ and SZ.

Next slide, please? In summary, we believe that a scientific model of the saturated zone flow and transport at Yucca Mountain has been developed. That model was calibrated to hydrogeologic data and hydrochemical data, wide level data. Some testing of transport conceptual model has been completed; that is, the C-well data have provided us with insight on the conceptual transport in the volcanic tuffs. The ATC is giving us insights on the transport in the alluvium. Nye County data are being incorporated as it becomes available and what data didn't make it into the model will be used for validation of the model. And, data collected since completion of model supporting TSPA for site recommendation are consistent with the bases used for this model. And, we call your attention to the two flow paths that we showed, the one we used for TSPA/SR and the one that reflect the new data and new analyses.

Next slide, please? As I said before, we have been using the model to guide data collection activities. We have been suggesting to Nye County locations where we can get more out of the holes and they have been very cooperative in that aspect. Data are designed to reduce uncertainties, relax conservative assumptions, and further validate the conceptual models and the numerical models and the results of the models which are fed to TSPA.

Efforts, as Frank has mentioned earlier on and as I did a few slides ago, efforts continue to improve the consistencies between the site-scale and the regional scale models. And, some of these efforts have used unified hydrostratigraphic hydro framework models and also to have consistency in terms of vertical extent and in terms of gridding.

That's all I have. I know that Mark Peters earlier this morning had reflected the question on colloid to me. With your permission, I can answer that or I can just wait for other questions.

BULLEN: Dr. Edderbbarh, why don't you go right ahead and answer the colloid question now and then we'll take questions from the panel. Go ahead?

EDDERBBARH: Can you remind me of the question? I think the question was in terms of colloid and the--go ahead, please? I better let you phrase your own question.

RUNNELLS: Runnells, Board. I probably can't remember it. Do you want me to--

BULLEN: Do you want me to give you a little time, Don? I have a couple of comments to make and I'll let you think about that.

RUNNELLS: Okay. Well, the last part of the question was on a scale from zero to 100 percent. How much do you think we know about colloid--transport?

EDDERBBARH: I guess, I shouldn't have brought it up. But, anyway, I can answer for the SZ. The current model that we have use reversible and reversible kinetics in terms of colloidal transport. The uncertainty in the model is very broad right now. But, for the volcanic tuffs, we have data from the C-well testing that helped us constrain the range. For the alluvium, so far in TSPA/SR, we went with theoretical conceptualization and now we have data from the ATC that's helping us verify the conceptualization and also helping us constrain that range of uncertainty. We also have been using real data from the NTS because they have the same process and they have real data that we are using.

BULLEN: Thank you, Dr. Edderbbarh. Just to show you that we non-hydrologists actually pay attention to your presentations, I wanted to point out that the last time you spoke to us, you had a great 3D visualization with the particle tracker that was a FEHM model for TSPA/SR. I was looking forward to that and I guess you need more budget money so you can do that for us next time.

EDDERBBARH: That's right. That's correct.

BULLEN: What I'd like to do now is ask Frank to come back up to the podium, if that's okay. And, I would like to go back to Leon and then Debra Knopman and then we'll follow on with questions. So, it's going to be a couple of questions for Frank, and then if you'll just stay there, Dr. Edderbbarh, that would be great.

EDDERBBARH: I will stay here.

BULLEN: So, Leon, did you have a question, Leon Reiter, from Staff?

REITER: Leon Reiter, Staff. Al, I'm having trouble understanding the Carbon-14 argument. I looked at your chart in the back here and these represent samples that you took in the saturated zone, is that correct?

EDDERBBARH: Well, yeah. And, Zell, please, help me out here if I say anything wrong. I think the samples were taken in the saturated zone samples and also UZ samples.

REITER: Okay. Is the last one Nye County Well 2-D? Is that one of the Nye County wells?

EDDERBBARH: Right. That's 2-D.

REITER: Well, just maybe a quick question. It seems to be kind of odd that the Nye County which looks to be the furthest wells, the youngest water, another question is that if we're looking at saturated zone, if I remember correctly, a lot of recharge is occurring up in Tiva Mountain area which is a lot longer flow path and the geology, the unsaturated part of the geology, may be different than that in Yucca Mountain. I'm just wondering how you get all these things together and calculate what this means for the travel time from the repository to the accessible environment?

EDDERBBARH: Let me add something that I failed to mention. Is that we don't just use the Carbon-14 samples or analysis. We also look into the uranium-238, 234, and other constituents to determine the signature of Yucca Mountain. So, that's what we are tracking. So, that's probably why you see that 2-D has younger water than upstream because we're tracking the flow that may have originated from under Yucca Mountain using the uranium ratio and using other constituents.

Zell, do you want to add to that? Zell, do you want to add to this?

PETERMAN: This is Zell Peterman, USGS. The raw numbers, the raw C-14 analyses were generated by the USGS, and then these are corrections, model corrections, I would guess. I haven't seen this particular version, but probably using FREAK-C (phonetic) or something like that.

EDDERBBARH: That's right. That's right.

PETERMAN: So, I don't know what more I can add. We also have a program to--these are all based upon dissolved inorganic carbon. We have another effort directed at separating the organic carbon and doing direct dating on that. That's being done at the Desert Research Institute by Dr. Jim Thomas and we have just a few analyses, so far, and they don't differ all that much from the uncorrected or corrected values. So, if I were to make a guess, I would say, you know, they're all going to come in about the same within a few thousand years. With regard to the younger age for EWDP 2D, you know, that is in or close to Fortymile Wash, I believe, and there is younger recharge in there. So, we're probably seeing mixed ages. I guess I can't say much more than that.

BULLEN: Thank you, Zell.

PETERMAN: Okay.

BULLEN: Debra Knopman and then Priscilla Nelson?

KNOPMAN: Knopman, Board. This is actually for both Frank and Al and it has to do with the characterization of uncertainty in your model results. Just looking at Slide 20 just for a takeoff point, this slide doesn't tell us anything about uncertainty, of course. It tells us something about the spread, the dispersion characteristics within both the unsaturated zone and the saturated zone.

D'AGNESE: That's correct.

KNOPMAN: What can you tell us about how uncertainty would affect both the location and the spread in those breakthrough curves? Starting with Frank's model because he's feeding uncertainty into your site-scale model, the question is how much are you feeding in there from your values and, Al, how does that propagate through your model?

D'AGNESE: This is my first time of actually seeing that curve. So, I don't know if I could comment on it. Let me just talk about three different things that we calculate uncertainty for in the regional model. What we're concerned with is the location, the extent, and the hydraulic conductivity or hydraulic values of these hydrogeologic units in which this water moves through and then these materials move through. Inherent in the method that we use, the inverse (inaudible) regression method that we use inherent in MODFLOW 2000, we are specifically characterizing the uncertainty in the value, the estimated parameter value. I showed that slide that showed the really sensitive parameter values. We have a very sensitive parameter. The hydraulic conductivity, for example, that's estimated for that parameter. If it's highly sensitive then the range of possible values are very small. If we have a very insensitive parameter, the hydraulic conductivity that could potentially be estimated, the range is extremely large. So, that would affect then what gets passed on to--that affects the flow, the flux, the potential range of flows that Al and his group would extract and use as a constraint in their site model. So, I would pass that onto Al.

The other thing, though, that we have a difficult time characterizing is the uncertainty and the location and extent of these hydrogeologic units and then we have to do a manual change, evaluation of conceptual models, one after the other.

KNOPMAN: Well, give us some ballpark estimates of how your predicted head values change at some--you can pick a location or locations within your model as a result of the parameter uncertainty. Never mind model uncertainty; let's just talk about parameter uncertainty.

D'AGNESE: If we're concerned with a prediction--and my understanding is that since we're discussing Yucca Mountain, the prediction that we're concerned with is the flux from the regional model into the domain of the site model. Luckily, we've done a lot of characterization in the area of a site saturated zone flow and transport model. So, as a result, the regional model is well constrained in that area. We have a lot of head data in the Amargosa Valley, relatively the Nevada Test Site, Yucca Mountain and constrains well those parameters that control flow into the site saturated zone model. The most sensitive parameters in the regional model are the parameters which also control the prediction which we pass to Al. I don't have the exact numbers, but that is available.

KNOPMAN: I'd like to know what range then within the flux, how much--how that's bounded or constrained, Al?

EDDERBBARH: Let me just talk a little bit about the saturated zone part of this breakthrough curve and how we arrived at it. From the TSPA/SR sensitivity analyses which were conducted, the saturated zone specific discharge was one of the most sensitive parameters in TSPA/SR. For TSPA/SR, we had the range on it that was elicited from an expert panel and it was 10 times and .1. That was a very broad range. Then, we went back to the drawing board and used the new data, new analyses, and looked more into the role of faults and looked into analyzing the permeabilities from the hydraulic testing. We looked at the fluxes from the regional model and we were able to reduce that range to one-third and multiplied by 3 for the SSPA. That's what we presented in the SSPA. These are the ranges--uncertainties that we are dealing with right now.

Now, the specific discharge here is the main driver here. And, this breakthrough curves also incorporate in it matrix diffusion. But, for matrix diffusion, we use the--what they call the envelope, the upper limit of the envelope after--I mean, we used 20 meters spacing. If you make it 50 or 100, you still have the same breakthrough. But, if you advance it, you make it 10, your performance improves quite a bit.

BULLEN: Priscilla Nelson and then Richard?

NELSON: Let me just ask two questions. One is I would have thought the issue about single and cross-hole permeability differences might be a reflection of anisotropy or scale effects rather than proximity to a fault. I mean, having to be in proximity to a fault. Would there not be an anisotropy effect and a scale effect in between the two kinds of tests?

EDDERBBARH: Well, you're right because the single-hole test only queries or questions, you know, a very small radius of influence as opposed to cross-hole testing which will bring the scale effect. But, I think the issue that the International Peer Review Team brought up is conventional wisdom used in the scientific community that permeability will decrease with depth because of the overburden. And, I think, it makes sense if results from the single well tests, the permeability decrease with depth--

NELSON: That's moderated by the lithology--

EDDERBBARH: That's right.

NELSON: --properties--

EDDERBBARH: That's right, yes.

NELSON: So, it's just a gross rule of thumb. Okay. Let me ask you about two other things. You said you used the hydrochemical data to advise you on flow paths. It seems like it could also tell you some things about mixing and dilution. And, we've also heard in the past from Linda Lehman about temperature and temperature measurements. And, it seems that that is an independent set of measurements that could be used to test your model. Do you have plans to use any of these other alternative ways to really test what the regional, for example, model is telling you and then forming the site-scale model?

EDDERBBARH: Yeah, we are using temperature data to validate in the validation exercises. We will not be using it in terms of calibration or construction of the model, but we are using it for validation purposes.

NELSON: Nelson, Board. In what time frame will you be doing that validation study?

EDDERBBARH: The validation is for the LA. That's what we're planning for is to validate our current model in time to support LA, license application.

BULLEN: Richard Parizek?

PARIZEK: Parizek, Board. For either of the speakers, do you have any independent velocity data to use for calibration purposes? You have calibration targets, but are there any velocity data anywhere? If you go to the test site or elsewhere where somebody may have tracer experiments that can run long enough you can find arrival time or from any weapon tests?

EDDERBBARH: Actually, we're documenting that in the in situ AMR and Bill Reimis (phonetic) from Los Alamos has conducted tracer testing data and he was able to back up velocity values that we're going to be using. He's documenting that in the in situ AMR and we're going to be using that in the validation process.

And, also, to answer your question about anisotropy, we have taken a fresh look at the C-well data in the KTI agreement and came up with an analysis that was done at Sandia for anisotropy, you know, from this data and that's also documented in the in situ AMR and the results from that will be used to guide us in this validations exercise in terms of validating the results of why multiple well testing permeability increases with depth as opposed to the single one which decreases with depth.

PARIZEK: Parizek, Board. In terms of flow interval spacing, I would think this figure would be driven, in part, by flow interval spacing assumptions. Is there any new work underway to deal with that as a better defined value?

EDDERBBARH: Our efforts right now is to improve--well, I mean, I shouldn't say that. As I said before, we're at the upper limit of the envelope. We're using the maximum spacing which is 21 meters. If you increase it to 50 or 100, the breakthrough curve stays the same. And, we derived those spacing from old flow meter surveys that had very poor resolutions, and moreover, the 20 meter spacing is biased by data from older wells that had questionable stability. I mean, if we use just the C-well data which was obtained more recently, that mean will shrink and performance will improve. But, I think the objective of our work right now is just to show that what we have documented in TSPA/SR was conservative, not--I mean, if we find something that was not conservative, we had to go back and use that, but right now, I think part of the confidence building is we use 20 meters spacing in SR and then we come to illustrate that it's 10. That's good because it's going to improve performance. So, we have nothing else to do, you know, except show that as a multiple line of evidence.

PARIZEK: The Board did ask for independent lines of evidence beyond total system performance assessment type argument and you gave us the Carbon-14 example as independent of the model simulations. And, so long as we can believe the Carbon-14 data and Zell would like us to feel good about that because you're always struggling with these corrections you have to make, that's a powerful argument, is it not, that you have Carbon-14 ages that are not out of line with what you're model was forecasting?

EDDERBBARH: That's correct.

PARIZEK: But, isn't it also true that any new Nye wells that are drilled, you immediately can throw the data in your model and prove your model, frankly, do the same. But, when you drill a new site and do some new testing, that's also independent testing of your model?

EDDERBBARH: It is.

PARIZEK: And, you could look at it that way from any new holes that go in to see how far off you might have been in terms of what you assumed about that part of the flow domain.

EDDERBBARH: That's very correct. And, basically, before we had the ATC, the conceptualization for transport in the alluvium was thought to be somewhat of a dual medium with less matrix diffusion than in the volcanic tuffs. It turned out to be a single continuum. So, that's validation, you know, of the conceptualization that we incorporated into the model.

PARIZEK: And, one more point. On Page 8, that's your two alternative flow paths that you showed or, at least, the plume that you might get from Yucca Mountain example, you're showing again a southeasterly and southerly direction of flow. There was also some chemical data, I think, from one or two wells that sort of support the need for flow that way. Can you refresh our memory as to where those points came from?

EDDERBBARH: Maybe Zell can answer that because I think it was pointed to us by Jay Paces. That when we were talking about the more direct flow north to south, I remember Jay Paces from USGS say that cannot be collaborated with the hydrochemistry data. The hydrochemistry data indicated real strong component of flow west to east.

Zell?

PETERMAN: This is Zell Peterman, USGS. The problem with the blue flow path is there aren't any wells until you get clear over to the middle of Jackass Flat to the water table. So, there's no data there to verify that excursion east of Fortymile Wash from a hydrochemical standpoint. Now, if you'll overall look at the hydrochemistry, you generally see a broad plume of low chloride water coming more or less straight south from Yucca Mountain. You see the same thing if you look at sodium or sulfate or anything that's conservative or semi-conservative. But, the resolution of the hydrochemistry is not equivalent to what you're seeing up there on those slides. I mean, the well spacing just isn't there. Now, with increasing number of Nye County wells, that's going to improve along Highway 95, no doubt. But, that's kind of where we are at the moment.

PARIZEK: Zell or anyone else, I thought there was a well that suggested that that bend toward Busted Butte, I guess, needs to be there because of a well that had kind of a unique chemical signature to it just about where the bend occurs, somewhere right up in there. I can't find the data and I just knew that I heard--

EDDERBBARH: --and J-13 and J-11--

PETERMAN: I don't recall that. You know, J-12, J-13, JF-3, they're all very similar in composition. It must be maybe one of the WT wells, I'm not sure. It's not coming to my memory. Oh, okay. There's another dataset that Al mentioned and that's the uranium isotopes, the U-234/238 ratio and what those show is a very strong anomaly of elevated ratios, more or less, right over Yucca Mountain and they do change then towards Fortymile Wash. Now, J-13 has that higher ratio, whereas J-12 doesn't. The problem is J-13 has been a supply well for, what, 25 years or so and has had a lot of water pumped out and it could be pulling water in laterally. So, it's hard to know whether that's a good indicator of the natural system.

PARIZEK: Just looking for another line of evidence to support that interpretation is you've got it different ways, but I thought there was, at least, some--

EDDERBBARH: Yeah, I think, Dr. Parizek, what you saw is the uranium data that we had documented in the Twiller paper.

BULLEN: Bullen, Board. I'm going to try and keep us on schedule. Is that okay, Zell?

PETERMAN: That's fine.

BULLEN: And, I'll let you guys carry this on off line.

I do have one question that I want to ask before Dr. Edderbbarh leaves. This is a question from the audience, maybe more in the line of clarification of your calculations. The question is what is the nature of the flow and the boundary condition between the tuff and the carbonate aquifer? The question is basically asking is there upwelling or is there down flow? If you could just tell us in your calculations what is that nature?

EDDERBBARH: Well, the water level data evidence shows an upward flow and that's produced--I mean, that upward flow is produced in the site-scale model. So, basically, there is an upward flow from the carbonate into the overlaying alluvium and tuffs.

BULLEN: Okay. Thank you very much. I want to thank both of you for putting up with our questions. You'll see that the questions always expand to match the time.

Our next presenter must have made somebody mad because this is the shortest presentation I've ever seen by Bo Bodvarsson here. But, Bo is going to give us a 15 minute presentation on unsaturated zone flow and transport model update. So, we're focusing down and now we're to the UZ.

Bo?

BODVARSSON: Good afternoon. I'm here to talk about update on the UZ flow and transport and coupled processes model. When they told me to give this brief talk, I put together a list of topics that David then wrote with me and chose a bunch of other topics that I don't know a heck of a lot about. So, I'm going to try anyway. It was all his fault.

I want to talk about the role of process modeling really quickly, some of the issues we considered in this presentation, how we are dealing with these issues, and then concluding remarks.

As all of you probably know, there are many purposes for process modeling. It's to understand processes, for test design, data analysis and site characterization, to make predictions over the long-term, do sensitivity analysis, and then, of course, if the model is valuable, we abstract it and put it in a total system performance assessment. You have site-scale models and you have drift-scale models and we have smaller scale laboratory models.

Next one, please? The issues I'm going to consider in this talk are based on consideration of data. I'm going to talk a little bit about the moisture condensation in the

ECRB. This is very important for our model. I'm going to talk about model validation issues with regard to radon and how we use that to validate properties, some issues about the seepage testing that is going on in the lower lithophysal, matrix diffusion in Alcove 8/Niche 3. Then, I'll go into radionuclide transport issues, DCPT/FEHM issue, and transport below the drifts. Then, I'm going to briefly mention coupled processes issues, the drift-scale test, and THC effects on fracture sealing.

So, to start, ECRB moisture condensation, this is--am I in your way or are you okay? Okay. This is, of course, a fascinating topic for all of us. This is a very important test that is very close to the heart of the NRC regulator. It's very important for the project and is, therefore, very important for us to understand why are we getting water in the ECRB? What does it mean in terms of the test because we must eventually decide when we should stop that test and use perhaps the tunnel for testing purposes, as well as understand processes. Is it seepage, is it condensation, can we expect this to happen in emplacement drifts? That's all very important.

We have collected some information that Mark Peters mentioned. It's very important information and most important for us in the UZ are temperature changes in boreholes and within the drifts because that gives us indications about how much the rock took heat from the ventilation. Also, very important, the relative humidity increases in the drift. And, the third, almost most important thing, is the degree of dryout of the drifts, the moisture tension as a function of distance from the drifts. All of these factors are clues that must help us explain what is causing the condensation.

Our current theory and my favorite theory is the following. It's condensation, it's not seepage for the following reasons. The canisters we have measured, so far, indicate very, very low chlorides and low, low concentrations of silica, but there are more testing ongoing with regard to the chemistry. And, as you know, the water that's formed here on the paint indicate that it is not sucked through the rock, as Priscilla mentioned in one of her questions.

The hypothesis for the reasons for this is the following. We need a temperature gradient and we need flow from a hot region to cold region. That causes condensation. Every single borehole I've seen from geothermal system--I have worked a lot in geothermal systems--has internal flows in the boreholes. Why does it have internal flows? Because you have rocks of finite permeability. You stick a hole of infinite permeability in it. The density of the fluids in the hole are different from the density in the fluid outside the hole. Therefore, you are always going to get flow from one area to another area within the borehole. You see this clearly in temperature measurements of geothermal wells.

I think the same thing is going on in the ECRB. You have a medium where the gas phase is a dominant pressure phase. You intercept that medium with an infinite permeability drift. There are pressure variation laterally simply because there are density differences because there are temperature differences. We then take out and mine this drift, we ventilate it, and the temperature of ventilation is a few degrees above the ambient temperature at that location. You, therefore, create higher temperatures where you ventilate the most and lower temperature further away. It's further complicated by the tunnel boring machine where we have still increasing temperature there. So, you have a high temperature to low to a high temperature here. Then, we close off these bulkheads. What happens? You get infinite flow in the drift just like you would from a borehole. You have air coming in in one location and going out at another location. Air carries water with it and when it cools down, the water condenses. So, I think this is our current theory and this is what we are using to model this phenomena.

We have already matched the ventilation effects on the moisture tension which is shown here. These are model results versus actual data. We are in the process of matching the temperature history with time and then that, of course, I hope we can show condensation of water in appropriate places in the ECRB.

Now, why is this important? This is important for several other reasons. If we are able to explain it with this explanation, number one, we understand the process, number two, we can then go back and say how likely is this to happen when we actually put the emplacement drifts in because we know the temperatures in the system and we know then how much water we expect to accumulate over hundreds of thousands of years if the model is correct and the hypothesis is correct. Sorry it took so long, a long-winded explanation.

Next slide? Another one which I think has been very successful is radon data and pressure data from the tunnel. We measure radon concentration because we want to be safe. Mark Tinan has been sending me e-mail daily for about five years to look at this dataset--no, I'm kidding. He has encouraged me to look at this dataset and I have suddenly been interested in it and decided to look at it. This is a flow of radon in the main drift over a kilometer or so, one kilometer to one and a half kilometers. The barometric pressure in the drift is the same as the barometric pressure on the surface because, of course, the high permeability of the drifts. But, the barometric pressure in the rock is much less because of attenuation in rock. This causes pumping effects because the signal pressure in the drift will variate a lot more than in the rock. So, sometimes you have radon coming in, sometimes you have radon coming out, depending on the ventilation rate, and depending on the air pressures. This is an ideal dataset to validate large-scale permeabilities over kilometers.

So, what we did was we calibrated for 10 days and you can see the air pressure in the model just right on top of the dataset, very good match, and match of radon is also pretty reasonably good, I think, given the quality of the data and quality of the assumptions we use. Then, we predict in the next 10 days and you can see the predictions are also quite good. This gives us quite a confidence in the parameter values using an optimization function which is part of fracture porosity versus permeability. You see on your scale, we determined the permeability extremely accurately as 11.1, 11.2 versus--this is a log scale. So, it's basically 10 to the minus 11 meters squared 10 darcies which is really similar to what we measure from large-scale pneumatics. A good validation, a large-scale validation of permeability.

The porosity is much less constrained. You see the minimum band here even though this is the minimum here. The scale is much larger, and therefore, we are not able to constrain the fractue porosity, as well. But, with appropriate tests which could be done at a very low cost, we might be able to do this better, but again this is a very reasonable large-scale validation test, I think.

Next one? Seepage/evaporation analysis, a lot of concern has been with evaporation processes, how much does it affect seepage, how much does it affect seepage threshold, how much does it affect the whole phenomena of seepage? We are doing systematic testing, as well as testing in Niche 5. We do a very detailed evaluation of the moisture front that it comes through in the sealing on the niches. We sketch out the fracture systems and we do a time series analysis of evaporation processes occurring there, as well as we put pans when we do the test to look at the global evaporation phenomena. The conclusions we have so far from this study is that evaporation does not account for the difference, at all, and this validates the threshold concept we have talked about for a long time. It's significance or the suggestion to do this was a very good suggestion. Our lower lithophysal tuff has better seepage characteristics than the middle nonlithophysal and I said that before because of the small fracture characteristics.

Next one? Alcove 8/Niche 3, Mark mentioned this dataset before. So, I'm not going to spend a lot of time on this. What I want to emphasize is that this is a very important test for two reasons. It allows for the 10 meter to 20 meter scale to validate our seepage models and it also helps us now finally to get very, very consistent data on matrix diffusion. That the bigger molecules go through much faster because of the filtration of going into the fine matrix. That's why this is much quicker than the lower sized molecule and conservative molecules.

Next one? This is another one David asked for. We had mentioned this before, I think, the difference in transport models. It's our belief that the current dual-porosity FEHM model is conservative with respect to transport in the unsaturated zone, and that if we use a dual-permeability model, then you should get considerably more performance out of this. What we show here is a transport model T2R3D and here is the conservative model used in PA. So, we can get more performance, we think, by using a different formulation in our approach.

Next slide, please, and we're almost done. However, this again shows the conservatism here in the PA. We have breakthrough curves from the repository to the water table of something like 10 years which is very conservative. Whereas, it could be like Al showed with the travel path going over thousands of years. The other thing I wanted to show was the results I recently saw from TSPA. This is SSPA results and it kind of is nice because it's hard for me--I almost never see anything that makes a difference in the natural system when you have a waste package. You have such a great waste package that lasts hundreds of thousands of years. So, it's sometimes nice to see something that makes an impact and I think this does based on these results.

The approach they took in TSPA to mimic this dry area under the drifts was basically just to put the radionuclides into the matrix flux and not into the fractures. Now, it basically says if there's no seepage into the drifts, there is no water to carry any radionuclides, and therefore, it should be a diffuse mechanism going down through the rocks underneath it. It doesn't take into account the dry area, but it gives you significant performance, as you see here, surprisingly large performance. If you take just the delta from TSPA/SR, you get about 10,000 years gain out of this thing, but if you look at the mean 95 percentile, the medium and the 5 percent which is way out of the curve, according to the TSPA, there could be significant performance assessment just by putting stuff in the matrix if we can verify it without having to verify the shadow concept. So this, to me, is kind of interesting.

Next one? Finally, on to coupled processes and again the drift-scale test was turned off, as all of you heard, and the drift-scale test team consisting of members from various labs made predictions about the cooling phase that is going to give us more information about coupled processes. It will be very interesting for us now to follow and see how well our models that have been calibrated for four years against heating can reflect the cooling of that specific test.

The final one is the one on thermal hydrological-chemical issues. This was something we spent quite a lot of time on also in the SSPA and recent reports. We looked at high and low temperature case with the THC models and we found based on the various rock assemblages, we found no extreme values of pH or salinity, certainly not anything that resembled the fluoride and the pH resulting from the fluoride that was observed. We think that based on a lot of modeling studies--and this has been extensively communicated with the Board that there's a low probability of seepage within the thermal period for various reasons, as mentioned in the report. And, low temperature has less thermal-hydrological uncertainties and higher probability of seepage. And, the issue, we have talked many times in the Board, the sealing based on laboratory experiments, is still somewhat of an issue.

Next one? So, to conclude, the approach used in all areas, not only UZ, but in SZ, waste packages, and everywhere is to have a very close relationship between model prediction, model verification, test designs, and then predictions over tens of thousands of years. And, that has been critical to our success. We have identified a possible hypotheses for the water that we believe is condensing in the

ECRB and we are hoping to verify this with the model that is currently under development. The radon data has proved very nice in validating the large-scale permeability over kilometers, as well as some indication of fracture porosity. We looked at seepage with respect to evaporation and we think matrix diffusion is important from the testing and the modeling and this can help us delay transport through the UZ. And, finally, we will continue to evaluate coupled processes with the drift-scale tests.

And, that concludes my talk.

BULLEN: Thank you, Dr. Bodvarsson. You just kept us right on schedule, too. I think you went 45 seconds too long.

BODVARSSON: I didn't want to disappoint you, 15 minutes.

BULLEN: Okay. Questions from the Board? Dr. Nelson followed by Jerry Cohon?

NELSON: Just a quick one, Bo. Nelson, Board. In all of your discussions about the near-field environment and how it's working, what guidance would you give the project about the need to avoid any section of excavated tunnel from a place of waste package placement because of the presence of fractures, other than something like a capable fault? Do you understand the question?

BODVARSSON: Yeah, yeah, I understand the question.

NELSON: Is there a reason to avoid putting packages somewhere or is there no reason, at all, to avoid putting packages?

BODVARSSON: That's a very good question. I think that the data we have to answer that question are the following. We have the Southwest Research Institute data that actually have very big blocks and they have big fractures and they actually got seepage into the boreholes. That's the extreme. Then, we have other numerical studies we have done, as well as the drift-scale test studies. I think there is every indication that in the lower lithophysal when you have the small fractures present with large surface areas with the rock matrix that the capillary pressure effect will help equilibrate any pulses that want to go through. I'm more concerned with the middle nonlithophysal where you have larger, sparser fractures and faults. So, I would say, in addition to very large-scale faults, that you might have huge permeabilities that may focus flow. That perhaps with some heavily fractured areas in the middle nonlithophysal, you might well look at that in terms of candidates for what you're talking about.

NELSON: Nelson, Board. In the lith, you would see no reason to modify emplacement on the basis of any observations made during the excavation?

BODVARSSON: No, not from my thinking process over the last few seconds.

BULLEN: Jerry Cohon?

COHON: Can we go to Slide 10, please? I didn't understand what is different in terms of the inputs in this run compared to the base case.

BODVARSSON: Yeah. In TSPA, we developed three-dimensional flow fields. That's done with the large-scale 3D flow model. That has everywhere in the system of flow in the fractures and a flow in the matrix, everywhere. Okay? It used to be that we ignored the fact that we had a drift and that--the fact that we had a drift and we have--

COHON: Okay. So, this one includes the idea of the drift shadow?

BODVARSSON: No, not the--let me just finish two more sentences.

COHON: Oh, okay. Sorry.

BODVARSSON: So, what we used to do then was just to simply throw the radionuclides straight from the drift into the fracture flowing fracture system which, of course, is occurring outside here.

COHON: Okay.

BODVARSSON: But, now, what we do, we don't take credit for the fact this is actually drier, but we take credit for the fact--this is very important--is that if there is no seepage here into this drift, there's no water in the drift, therefore the waste sitting at the bottom here must think by itself where can I go and the fracture saturations are so small, less than 5 percent, general, but the matrix saturation is 80 to 90 percent, diffusion is a process that follows water and since there's lots more water in the matrix, the radionuclides have to go into the matrix. You see what I'm saying?

COHON: Okay, yeah. So, it's all predicated though on the correctness of the seepage representation?

BODVARSSON: That's exactly right, absolutely.

COHON: Okay. Which leads me to what I'm sure is a simple minded question, but going back to your condensation argument, your argument for condensation that's being observed--

BODVARSSON: Yeah.

COHON: If I followed you and I may not have, it sounded like you were saying whatever moisture we're seeing is actually coming out of the rock. It's being transported by air out of the rock?

BODVARSSON: Yeah.

COHON: Okay. Now, just by conservation of mass, what implications does that have for drift shadow, for threshold-- if you're going to argue that--

BODVARSSON: Absolutely, I understand exactly what you're saying.

COHON: Okay.

BODVARSSON: If you generate water within the drifts, you're not going to have any more drift shadow. That's what you're saying, right?

COHON: Yeah.

BODVARSSON: Well, that's exactly a good point. The answer is this. When we ventilate, we disturb the system, we create temperature gradients that are substantial, up to 3 or 5 degrees in that area, and that artificially made temperature gradient causes the condensation based on this hypothesis. Okay? Now, in the real system, ambient temperatures, you have much less changes in temperatures than we have from the ventilation system, and therefore, you may expect much less condensation, if any, but we need to verify that with the model calculations.

COHON: Yeah. No, it's not that I'm worried about condensation. It's if you're going to make that kind of process argument, physical process argument, for why there is condensation, what does it have to say about the defensibility of the drift shadow? That's my point.

BODVARSSON: Yeah. And, my answer was--

COHON: And, I think you've got some work to reconcile these things, don't you?

BODVARSSON: Yeah. And, my answer was that we introduce artificially the water--

COHON: No, I got that, okay.

BODVARSSON: And, maybe when you have emplacement drift, you're not going to introduce that artificially and maybe it will be little or low condensation, and therefore, the concepts are still reconciled. But, you must verify that, obviously.

COHON: Okay, thanks.

BULLEN: Debra Knopman?

KNOPMAN: Knopman, Board. This question actually follows up on Jerry's. This barometric pumping mechanism that you think is a possible explanation, plausible explanation, for the condensation, let's see if we can take it one more step. You tell me if this is right or not. We stop ventilation, we seal up the repository, we have drip shields in there, we have lots of differential heating as you go along a drift. So, you've got an incredible amount of air instability as this barometric pumping is going on up and down the drift in lots of different ways bringing in, drawing in quite a bit of moisture in the process that's going to probably condense somewhere in the drift, but we don't know where. So, you're bringing--that mechanism seems to me to be now your vehicle for bringing more moisture into the sealed drifts that could get--then, it starts bringing into question what you've got in terms of condensation under your drip shields. You can have different temperatures between the waste package and the drip shield. I don't know about the temperature differential and the gradients with your invert material. I don't know what's going on there. What do you think?

BODVARSSON: Well, I think I explained myself very poorly. So, that's my first thing. The radon is due to barometric pumping. The condensation based on this hypothesis--and I'm just saying this is a hypothesis--is not based on pumping. It's simply based on the fact that we artificially created a temperature gradient from an inlet during the ventilation process because the average temperature of ventilation is higher. So, I had a temperature gradient like that. Okay? Say, 5 degrees--3 degrees, 5 degrees. Temperature gradients and infinite permeability create different pressures in different areas. Those different pressures may create air coming from the rock continuously, not barometric, although it's affected by barometric pulses. But, generally, it might be continuous for quite a while and then go out and condense over here because it loses the temperature right there.

With respect to what is called the cold trap or differential waste packages, my hypothesis with that is that that--I haven't looked at this in deatail, but that will probably not occur except very late in the cooling cycle. And, let me tell you why. In the drifts, you have much higher temperatures. Therefore, air pressures have to be higher than in the rock because of pb equal to nrt (phonetic), the old good law. And, if the air pressures are higher there, if I have a hot canister here, I have a pressure, cold one here, I have pressure, infinite permeability pressures equilibrate so that the cold and hot won't matter. The air pressure will still be much higher than the rock. Therefore, the air flow will always be into the rock or out laterally. So, you may have condensation laterally and not within this cold trap areas. It's just my thinking.

However, at the end of the cooling cycle when you're almost close to ambient, therefore the pressure difference don't dominate any more. The temperature difference dominate and then you might have it.

BULLEN: Bullen, Board. Thank you very much, Bo.

BODVARSSON: Thanks.

BULLEN: Let me just state for the record that the next time Bo gets 20 minutes. Okay? So, he can take that much.

Our final presentation before the break is an update on recent Nye County well testing activities by Dave Cox from Questa Engineering. Dave?

COX: This again is one of these presentations that's a compendium of information generated by a whole lot of people. Other folks I want to recognize include Dale Hammermeister, of course, the on-site rep for Nye County, Jamie Walker and Ray Nadowny (phonetic) who both have been involved in the testing and data acquisition for these tests, and Scott Stinson who assisted with actually running some of the tests on the interpretation.

Next slide? We have three different wells that we want to present information on today in three different areas, in particular; the 7S, 7SC area, the 3D and 3S area, and then over in the ATC.

Next slide? These tests were done within the last year. 7SC and 7S test were in March, the 3S, 3D were in April, and then IM1, IM2 were tested in October.

Next slide? So, first, let's talk about 7SC. We ran a pump/spinner test in four zones opened in that well. Most of the flow came from the upper two zones which had a higher head than the other zones. So, in this case, we actually had higher heads in the shallower zones than lower, one of the rare cases in the Nye County wells where that's happened. The 48-hour pump test, here, you have some results close to 2,000 square feet per day for transmissivity and about 2.2 darcy. The permeability near the well was damaged because of grouting that had to be put in to hold the well. And so, the way we got the analysis here is we get the permeability outside the damaged region from the interference over to Well 7S there. So, that came on the interference response and again we hit the same transmissivity, but that's how we know that the permeability was reduced by about a factor of 40 times right near Well 7SC.

Next slide, please? You can see here the stair steps here and here and there's actually a couple of little breaks. These are caused by movement of lost circulation material in and out of the well like that. And so, they're kind of plugging off parts of the screen during the test. So, on this particular case, what we did was we matched the recovery period to tell us permeability.

Next slide? And, you can see here that you have several things showing up here. This is a log/log plot like I've shown a few other times before and I'm not sure whether to this group, but at Devil's Hole and places like that. They're commonly used in petroleum industry. What we do is we plot the log of the change in head versus log recovery time or log of producing time. The early time unit slope is giving us a wellbore storage or near wellbore effect. In an ideal case where we have homogeneous properties, this derivative curve which is the grain curve here will come up, reach a peak, start down, and then stabilize. That stabilized portion on the derivative is where we would normally draw a straight line on a semi-log plot. So, that would be the Cooper type of analysis on that. Here, instead of getting stabilization now, it keeps heading down and that's because we're being fed by more water coming in from outside this damaged region. We also have this bump right here in the derivative corresponding to the bump in the head change there and that's where the head and well finally drop below the head of that third zone. And so, we're seeing the effects of different head levels in the different zones there. So, a very complex test analysis. The bottom line is the very steep derivative coming way down like that is an indication of that near wellbore grouting that interfered with the ability of the formation to produce water.

Next slide? Now, if we move to 3S, April, we tested that.

Next slide? We had a 24-hour pump test there at 41 gpm. So, again, a relatively low rate. Once again, we're getting impaired permeability because of the grouting. During previous operations this test, but after an earlier test on 3D, the well began to flow air out one of the shallow holes. So, they had to grout it off to maintain integrity of the wellbores. So, that ended up actually causing a damaged region that extended around both the 3S well and the 3D well. So, because of that then, we have a larger damaged region. We had an original test in 1999 on 3D that indicated about 14 darcy. Now, we're down at about .17 darcy. So, obviously, grout helps to plug off permeability, as we all know. That was not the intent, but operationally it had to be done. So, we now have the interference response where we modeled this recognizing that we have the inner region that's damaged and an outer region that still has normal formation properties.

Next slide? So, on this one, this is the 3S response. Here, with basically a single aquifer unit being open during the test and with this support from outside, we see the derivative turning and heading all the way down like that. This is a classic indication of pressure support. We can't tell whether it's coming laterally or vertically. Leaky aquifer has a very similar type response, but in this case with a combination of the well history with the other information, we know that we're seeing this from outside laterally.

Next slide, please? Now, let's move to the ATC testing in October of last year. The ATC well layout, we have 19D which is sort of the cornerstone of the ATC, 19IM1 is about 20 meters north, and 19IM2 is about 20 meters east of IM1.

Next slide? The completed intervals, we have the alluvial intervals up at the top in 19D, then a couple of tuff zones, and tertiary sediments on the bottom. The IM1 and IM2 are basically completed in Zones 1 through 5, very similar to the 19D. Zone 4 here is the one that's likely going to be used for the tracer test.

Next slide? So, we originally had back two years ago, we had some testing on 19D prior to the drilling of IM1 and IM2. So, we had those tests which indicated the permeability of about 2 darcy. Now, we have these two monitor wells that have been put in and we were in pump tests in those while we were measuring the heads in the offset wells. So, we also have the interference effects.

Next slide? The spinner and pump/spinner tests indicated that Zone 1 and 2 contributed very little. They're the shallowest zones. Zone 3 provided most of the flow, but it's a very thick interval which makes it harder to do tracer testing. So, that's why most of the effort has been focused on Zone 4. Zone 5 is in the tuff and there was a fracture in the tuff that contributed most of the flow at about 955 feet. And, Zone 6 and 7 did not contribute much. So, that pump test on 19D, what we found, a total of about 4,000 square feet per day, transmissivity 2.3 darcies, average permeability over the whole open screened interval in Screens 1 through 5.

Now, the other thing that's interesting here is we could see multiple flow barriers at a distance from the well indicating we have some kind of a channel approximately 1400 feet wide. Now, that distance is not well-defined or well-determined because we know we have multiple layers here and we're getting some effect from that and we don't know the effect of compressibility or storage of each of those layers independently yet. So, because of that, think of that as 1,000 feet plus or minus, 1400 feet plus or minus.

Next slide? So, what we see here again on this derivative type analysis on log/log plot, here, the derivative comes up and we reach stabilization. So, that's telling us the permeability away from the well being about 2 darcy or 2.3 darcy. This increase in the derivative after that point in time is a sign of these boundaries or flow barriers at some distance from the well. We're seeing a couple of them out there. If they're only a single boundary, what would happen is this would come up and stabilize about a factor of 2 higher than what it is for the flat period there. So, the fact that we're seeing continued increase over a substantial period of time says we're seeing flow being channelized here between barriers.

Next slide? Well, in IM1 and IM2, we did separate tests of each of those. So, we're pumping IM1, monitoring IM2 in 19D. Likewise, we then came back and pumped IM2 while we were monitoring IM1 and 19D. Preliminary results, 2.1 and 2.3 darcy; so, same permeability, same transmissivity. As well, we do see the effects of the barriers there. Now, the interference response, there's definite interference response. There's indication of anisotropy there. We just haven't had time to complete the analysis of that yet. But, we are looking--the key one there is 19D because it's at a different angle from IM2 than it is from IM1. 19P, the response there is very muted and it looks like there's a flow barrier between that. It's only a very shallow hole in the rest of the productive interval there.

Next slide? So, here, we have the same type of derivative plot for the IM1 test. Again, you can see the effect of these flow barriers out here. A good stabilized derivative time giving us good value of permeability at about 2.1 darcy.

Next slide? Here is the IM2 test results. Once again, derivative climbing indicating flow is being channeled here.

Next slide? So, in summary, these test results indicate permeability of about 2 darcy or more around 7S and 3S, but low permeability immediately around the well because of the grouting operations or because of loss circulation material. 19IM1 and 19IM2 testing basically have confirmed what we've known already from the 19D testing. We do see definite indications of multiple flow barriers and we see definitive interference between the different wells here. So, all these are very positive factors that indicate that the ATC here should be suitable for tracer testing.

Next slide? We've learned a lot of lessons. First, well testing again has demonstrated its usefulness at characterizing the system and evaluating the artifacts introduced during drilling and completion. We've changed our drilling procedures to put shallow wells a little further away from deep wells so we don't run into these problems again.

Next slide? We did get much better completions on IM1 and IM2 than we had in 19D. So, we saw no evidence of the progressive plugging. We used larger screen openings, got better gravel packs reduced the need for LCM, and it looks like we got much better zonal isolation in IM1 and IM2. Now, the skin factors that we saw there, if anyone noticed those written down in the type curve analysis, those apparently relate primarily to the multiple layers being present, not to additional damage. It's rather an artifact of multiple layers and we saw no signs of screen plugging.

Next slide? Okay. Now, I will give you a quick update on activities for Nye County coming up here. So, you can see here the red wells are the Phase 1 drill holes, Phase 2 are the light blue, Phase 3 are wells completed up through January here, and then we have additional wells to be completed in the next couple of months.

Next slide? We'll move on here and go to progress. So, we've had the four exploratory boreholes, four multiple screen monitor wells that are now completed, and the three piezometers.

Next slide? We have obtained core during the drilling and completion of these alluvial wells. We've got core from the alluvial pathway there now where it looks like the transport will go from Yucca Mountain. It's suitable for both hydraulic and geochemistry testing and about half the core was provided or made available to DOE and the Yucca Mountain Project. Location here, we've got it at 10P and 22PA.

Next slide? So, this is just a slide showing you what the core looks like. You know, we have pulled it out. We've got core barrels and so on.

Next slide? Work to be done. Right now, we're planning on cleaning out and testing existing holes. 2DB is a well that Nye County drilled a little while back. We've got several hundred feet of fill in. So, we want to clean that out and then pack off which is says "pacer off", but it's really pack off and test the paleozoic section down there and collect aquifer tests and water chemistry data. If we have enough time and money, we may try and test the shallower tertiary sediments there, too. The Felderhoff is an old oil field test. It was plugged many years ago. The idea here, if we have sufficient time and money, would be to draw out the plugs and try and complete it, screen off the paleozoics from 2300 to 2500 feet. This is going to be a fairly difficult one. I'm not sure whether we'll get to that this year or not.

Next slide? Okay. The other work to be completed, 22PB, 23P, and air in 3D to clean that out and get a deeper completion on that which will also give us some samples and information on hydraulic gradient and water chemistry there.

Next slide? Future phases, the DOE Cooperative Agreement and Funding is being arranged and you'll have to direct any questions on that to Dale. I can't answer those. And, the plans for the next five years are being developed. These will be presented at the May TRB meeting.

Thank you.

BULLEN: Thank you, Dave.

Questions from the Board? Dr. Nelson?

NELSON: Nelson, Board. Are you taking thermal data, as well?

COX: Yes, we are, but we--the thermal data is actually showing us some things, too, in terms of where the flow is going between different zones and such, but we haven't really had time to analyze all that.

BULLEN: Other questions from the Board?

(No response.)

BULLEN: Questions from Staff?

(No response.)

BULLEN: Wow.

COX: Okay. We have one more thing to say here.

BULLEN: Go ahead, Dave? That's fine.

COX: We do have copies of the 19D report, well test report, and the 3S/3D test report in the back there. We didn't bring copies for everyone, but for those folks who are interested, it's highly technical, but it goes into much more detail on this type curve analysis and so on.

BULLEN: Right. Thank you. We have a couple more questions before you go. You know, we always expand to meet the time.

COX: That's fine.

BULLEN: Dr. Knopman, Board?

KNOPMAN: Yeah, I just can't stand the vacuum here. Knopman, Board. Dave, could you just sort of step back from everything you showed us, the detail, and give us a sense of what you think you're learning that you didn't know before the drilling program began and what you think the implications are in terms of characterizing the saturated zone and transport in it?

COX: Okay. Now, you recognize that these are kind of personal observations in response to a question off the cuff here. So, don't consider this an official Nye County position.

KNOPMAN: Don't consider my question an official Board concern.

COX: Thank you. Well, I felt I had to make that disclaimer. But, I think the key thing to me looking at things, one is that in most of these cases we are seeing heads that are higher in the deeper zones. So, we're seeing flow coming up for the most part. In the case of 7S there, what we're seeing, it's not really perched water, but it's water that's coming or has split into about three different zones and then a spilling at different points. So, it's water that's being kind of held up and that's why we have the upper zone having higher head. But, all the other wells, we're seeing higher heads in the deeper zones. So, I think that's key.

The second one is that for the most part we're seeing--on the other tests on other wells, we saw kind of 10 to 100 darcy. These, we're seeing things that are quite a bit tighter down into the average range of, say, two to 10 darcy. But, even then, averages are misleading. If we look at individual zones, we're probably talking--you know, some of them are tighter, but there are still a lot of things in the, say, 5 to 20 darcy range. So, relatively good permeability which says flow will happen fairly quickly.

In terms of the fractures, there are a lot of things that are highly influenced by fractures; as, for example, the fracture there in Screen 5 on 19D. So, we're seeing a lot of fracture flow. And then, finally, we're seeing a lot more barriers than I expected laterally. And, these barriers have to tend to channel flow and to basically speed things up. So, in a case like this where we're talking a zone that's 1,000 to 1500 feet wide, if you look at one of these maps, you know, that's a very narrow piece. What is says is flow has to channel through those and be deflected into it, or if it runs up against it, it's going to be deflected on the outside of that. So, these barriers that are there that extend, at least, thousands of feet from the well, I think, are an overprint and whether that's depositional or post-depositional, I don't know. But, it's an overprint on there that has to affect flow paths substantially.

BULLEN: Bullen, Board. As a followup to that, I guess, I want to ask the rhetorical question, are there surprises? Are these surprises in what you'd expect the flow field to look like or do you think that these are just the natural variabilities that you run into in nature and you'd expect to see this kind of behavior?

COX: Well, I'd have to say for me based on my past experience it is surprising, the degree of heterogeneity and the number of barriers that we're seeing. I don't normally see that. But, on the other hand, I normally work on oil fields and, you know, we have a whole lot more wells and so on. We do see barriers, but not nearly as often as we're seeing here.

BULLEN: Okay. Thank you, Dave.

Any other questions from the Board? Dr. Runnells?

RUNNELLS: Runnells, Board. Just a quickie. You've talked just about the hydrologic testing. Are you also doing geophysics, doing chemistry?

COX: Well, there has--I'll have to defer that to Dale. Dale?

RUNNELLS: With all the tests they have, I wondered about the ones that you just described.

BULLEN: With him taking so long to walk around, see, that way, we'll use up the rest of the time that--his walking will expand to fill the time available here.

HAMMERMEISTER: Yeah, this is Dale Hammermeister, Nye County. Yes, we do geophysics on boreholes and we also do water quality data. We have not published any reports and we're working on the analysis. However, Dave has published several reports on his aquifer tests.

RUNNELLS: Runnells, Board. Are you measuring oxidation reduction potentials in these new recent wells?

HAMMERMEISTER: Nye County isn't, but I believe Los Alamos or the USGS are measuring oxidation reduction potentials. They can answer that question.

EDDERBBARH: That's correct.

HAMMERMEISTER: At the same time that we sample wells, the USGS in Los Alamos and actually UNLV also sample the wells.

SPEAKER: That was Al that commented. The USGS in Los Alamos are measuring oxidation reduction potentials. Can you tell us if they're reducing or oxidizing?

(Pause.)

EDDERBBARH: I don't think I have an absolute answer on that because it varies with that--I mean, the samples, whether it's--you know, I mean, some samples oxidizing and, you know, other depths of reducing and also with location. Aaron Meier is the scientist who does the data collection and measurements. If you want, we can get you, you know, complete pictures on all the wells. We can maybe communicate that to the Board if you are interested.

BULLEN: Thank you. That was Al Edderbbarh.

Well, I'm going to take the Chairman's prerogative now and give you three whole extra minutes instead of just one extra minute today. I want to warn you that you have to be back here at 4:00 o'clock because the next session Chairman is even meaner than I am. So, we'll reconvene at 4:00 o'clock.

I want to thank all the speakers for the presentations the first part of this afternoon. Thank you.

(Whereupon, a brief recess was taken.)

COHON: If you will take your seats and take your conversations outside, if you're going to continue them. Thank you.

This last session of the day which focuses on a series of reviews done by external organizations will be chaired by Board Member Jeffrey Wong. Jeffrey?

WONG: Thank you, Dr. Cohon.

Okay. Again, as Jerry said, this last session is on external reviews, and the very famous board member whose initials are D. B. wanted me to be more poetic than himself in introducing the session, so I'll say that there are many contributors to the crucible scientific debate, and hopefully, from this crucible, the best understood performance estimate will flow. And with--you like that, Jerry?

And with that we have four speakers and our first speaker will be Dr. Bill Alley who is with the USGS in Ruston, Virginia, where he is the Chief of the Office of Groundwater. Dr. Alley?

ALLEY: Thank you. It's not often that one gets to give a presentation on a letter. But I feel a little better because I was talking to somebody during the break and they said that they had survived giving a presentation on a memo. So if they can do that, then I can do this.

Basically what I'd like to do, there's copies of the letter at the back of the room, for you that are interested. What we did is, we--the U. S. Geological Survey has played an active role in studying nuclear waste disposal for a long time now. We've been investigating the Yucca Mountain Nevada Test Site region, if you will, geology and hydrology since the 1950s. And actually, on a number of occasions over that period of time we have commented on various aspects of nuclear waste disposal.

Perhaps the most recent comments were made at the time of the viability assessment which in 1999 we published Circular ll84 that summarized the comments of a review team that we put together. We put together a team of people who are subject matter experts, external to the projects within USGS at the time. So recently as part of the federal register and as part of the sight recommendation decision, we were asked to give our point of view once again.

I should emphasize that the point of view that I'm presenting is based on essentially forming over a relatively short period of time a team of experts both external to the Yucca Mountain project as well as those who were doing it on a day to day basis to try to elicit our overall opinion of the current state of affairs relative to site suitability. I should also state that any comments that we have relative to that are solely within the bounds of our expertise and our science and limited to our science issues, and we are, as an agency, obviously neutral on all other issues that are outside the bounds.

The USGS views Yucca Mountain as a potential repository from a scientific point of view as opposed to an engineering point of view, if you will. It's an immense undertaking. Many times today I've heard the words "first of a kind". And it needs to be implemented in a staged manner with recognition of the uncertainties and the limits of production.

I'll review the Secretary of Energy's decision to recommend a site as one step in this continuing step-wise decision making process, and so our information in perspective in the letter that we provided was solely related to this particular step. Just to summarize some of our general conclusions at this point, are, one, is that geologic disposition is the only long-term approach to high level waste at the present time.

Second of all, on balance, again, at the present time, the site attributes are positive and we do not see any fatal flaw, if you will, relative to the earth science issues related to Yucca Mountain as a site for nuclear waste disposal.

Thirdly, that we view and have long held that retrievability is an important aspect of geologic disposition, and most importantly is one that's achievable at Yucca Mountain by the nature of the fact that you have this very thick unsaturated zone in an arid climate.

And finally, and I'll mention these later on.

There are several aspects of the site characteristics that suggest some key design considerations. A number of these you've heard about today in the course of the discussions.

Just a few statements about some of the positive attributes are assets of the site, the air, climate, low rate of infiltration. Again, the thick unsaturated zone, the lack of economic mineral or energy deposits, the ease of excavating stable tunnels, the natural path of ventilation to the mountain, and the presence of zeolites and other--particularly zeolites, retard the movement of certain radionuclides.

There are also characteristics, as you well know, that potentially may degrade repository performance, and that consequently deserve scrutiny. If the President designates Yucca Mountain these attributes may and often will require additional study and monitoring, and I'll mention four of them right here, the four key ones that we talked about. One is that during the pre-closure period critical surface facilities must be designed using state of the art engineering practice to accommodate the potential for earthquakes. Whereas, the engineering design is outside the scope of USGS studies, USGS has confidence in the probablistic earthquake has an analysis upon which designs will be based.

The second is that the potential for future volcanic activity has been extensively studied because of the presence of nearby volcanic features that are much younger than Yucca Mountain. The U. S. concurs with expert panels that the probability of repository piercing eruption, including surface eruption is on the order of 1.6 times 10 to the minus eighth per year, or on the odds that's something like 16 in a billion.

Thirdly, and one which has been a focus of much discussion today, is that there is a deep potable aquifer beneath the site which is an important resource, very valuable resource for the region, both from a human and natural environment perspective. We believe that the arid, the site characteristics of an arid climate coupled with the hydrologic characteristics of the unsaturated zone as has been studied extensively, will help result in limited contact to the water waste. Clearly, this is a matter that should continue to be evaluated.

And fourthly, future climate changes are errantly uncertain and can result in either positive or negative effects on potential, on the proposed repository. Their plausible limits on future climate are based on records of climate change over the past million years. If one looks at those, one essentially has an expected range that could be significant cooler periods with double today's precipitation. It's likely that the climate at Yucca

Mountain in the next 1000 years will be intermediate between the two extremes. It's probably semi-arid at times. Clearly if one looks at the science, climate change today it has evolved. It has even evolved since we wrote this letter in late last year, So it's another area that requires continued scrutiny in terms of the effects of possible climate change.

We recognize that it is desirable to continue to improve knowledge of the site to reduce uncertainty, apply newer science concepts and support refinements in repository design.

With respect to the design considerations we believe that the temperature of the rock should be kept below the boiling point at all locations to reduce the impacts on the natural assets of the repository system and also importantly to reduce uncertainties in predicting the repository system behavior. And we've heard a lot about that today.

Second, the forced and natural ventilation should be used to cool and dry the surrounding rock and thus improve repository performance, again minimizing seepage into the drifts. And seepage in some fraction of infiltration and percolation through the mountain is a key to the value of the natural system in containing the waste, and ventilation can have a major effect on seepage.

And third, again to emphasize the period of retrievability, and monitoring as necessary to preserve options of future generations. Certainly, the limitations of quantitator prediction over such long time periods need to be recognized and reenforces its need for retrievability and monitoring.

It also, as we've heard today on a couple of occasions in fact, emphasizes the importance of multiple lines of evidence, in addition to the TSP analysis. In particular, the two that we point to are studies of both natural and human analogues, the preservation of packrat middens for tens of thousands of years, the preservation of ice age painting in caves, and other types of evidence. It's important to illustrate the potential for essentially the design and operator repository under ground at Yucca Mountain.

And secondly, to point out the importance of geochemical studies of calcite and opal (phonetic) at Yucca Mountain, which have shown unequivocal evidence that the water table has been below the repository level for millions of years. And that the effects of past climactic shifts were greatly attenuated at the proposed repository depth.

Again, basically our comments are based on our long history of working at the mountain. We feel that the strength of our comments is our foundation on our long history of scientific work in the area and ability to stand back and take a broad science-based overview of the earth science aspects, and the preponderance of evidence to date.

We recognize that the weakness of our review is that we have not undertaken a detailed review of all current documents and obviously that's something left to others to do and is an overwhelming task.

So in conclusion, I think we, on balance, feel that at this particular step in the process, in a stage-wise process, we feel that the characteristics of the site are such that one should continue forward. We recognize that there is still continuing work to be done, and that it is, in essence, a first of a kind, a large scale scientific experiment. And so it does not ever come to a completion. Completion is a point where you can say, oh, thank goodness. We did all the work, now we can go home and everything will be fine. So I think that's a general summary of what's contained in the letter. Again, there are copies in the back of the room and I'd be happy to take any questions you might have.

WONG: Okay, thank you. Questions from the Board? Dr. Parizek?

PARIZEK: Parizek, Board. Some have said that the U. S. Geological Survey is really the godfather or the grandfather of the Yucca Mountain project. I don't know, you know, if you would agree with that, but I mean this survey made early recommendations about that area. And you know, as a parent, you like to see the best in your children, you know, they may be miserable, nasty and anxious, but you don't want to pay too much attention to that because you really want to see good things about the site. To what extent do you see good things about the site that may be clouding the bad things about the site? I mean can you--you gave us a list of the pros and cons, but could you kind of clarify these in, you know, in hindsight, after some years of working in the desert. And also the test site, because obviously you've made observations over the years, about the test site, or groups have, and you're bringing that experience into play and so on. So we just want to carry this further because some of what you've said is not really rigorous mathematical TSPA analysis numbers of something, right? Which people have to deal with. You're sort of giving opinions, a sort of professional opinion, a sort of--the whole organization of U. S. Geological Survey's feeling about it, right? So that's sort of harder thing to quantify, you know, in terms of testimony before governmental parties and so on. So that's your opinion, somebody else has another opinion. But it's more than just kind of a casual opinion. It's based on years of integrative experience of many people. Isn't it? Or--

ALLEY: Yes. I would say--

PARIZEK: Like naughty children and you don't want to see anything bad about it.

ALLEY: It is true that the USGS was heavily involved in the initial selection of the site and many of the opinions that we have presented in our letter are long-standing opinions over a couple decades or more in some cases. The retrieveability, the monitoring, and so forth.

A couple points: One is we tried to bring as many people to the table as we could to hear from all sides within the U. S. Geological Survey, and I can assure you it's not a uniform body of thought internally. In fact, we have plenty of what some people would call renegade scientists located within the survey. In fact, I worry a little bit about hearing what I hear things like a more disciplined approach to science. I worry about not letting those renegade scientists come in and have their opinions, which sometimes play out to be quite correct.

So we recognize that we have long-standing opinions here. We pride ourselves--we have two assets for the organization. We are not involved in managing anything. We couldn't manage anything, really. So we realize we have nothing to fall back on. So in that sense the only things we have are the talents and capabilities of our people and our own unbiasness. So we pride ourselves in our unbiased character. So we continually ask ourselves questions. I continually ask the group questions, do we really still support the low temperature designs just because we are obstinate and that was our idea in the first place, and we're not really willing to give up on that idea, or have we just--are we sticking to our guns and we just haven't seen the evidence that we feel a better design is possible through high temperature as a result. And the honest answer I got back from people strongly feel within the survey is that, no, we feel like, you know, we continually are open to the idea, but we just, you know, we still believe in the repository design that it should be the low temperature. So there is no such thing as a completely unbiased--when you have some stake in it, a scientific stake in it, but I think I can say that the perspectives we have are pretty close to that, as close as we can make it to an unbiased statement, and not getting too attached to any particular children.

WONG: Dr. Reiter?

REITER: Leon Reiter, Staff. You mentioned some of the history and documents. I notice that in your letter you mentioned Circular 903. I guess which is one of the central documents in the unsaturated zone. And I want to, there was a quote in there, I want to know whether you still hold to. And the quote is as follows: "It is difficult to conceive of any geologic surprises that could present serious problems with the unsaturated zone." And I wonder if you people still believe that or if you follow the maxim of Wendall Worth (phonetic) who said that one is most comprehended site before one begins detailed investigation.

ALLEY: Yeah, I would say that we would not stand behind that statement at this point in time. I don't know what year that was written, but obviously we've learned a lot more about the unsaturated zone and a lot more about the transport of contaminates within the unsaturated zone, and so I would say there are plenty of surprises.

WONG: Dr. Knopman.

KNOPMAN: Knopman, Board. Bill, you started off by saying that the USGS expertise is in earth science and that you try to confine yourself to that. Yet, throughout the letter in the supporting document there is reference to and discussion about, and judgements on, engineering design.

ALLEY: Um-hum.

KNOPMAN: And I find that interesting. It seems to me, and you can tell me if this is a fair or unfair characterization that what you've recognized as you were putting this letter together is that design and characterization of the natural system are very closely intertwined. And therefore you almost couldn't avoid talking about design matters even though it's outside of the expertise and outside of the study that has been conducted by the survey.

ALLEY: Right. Let me take the three design aspects and sort of illustrate that. The first one is a cool repository, and there, one could argue, I mean there's plenty of arguments relative to what might happen to the canisters and the engineering structures and the chemistry thereof. But there are many earth science aspects that one has to think about in terms of the temperature of the repository, just in terms of the effect of high temperature on the rock. The expansion from temperature on the rock, the multi-phase aspects of the chemistry, the complicated chemistry, geochemistry, that one has at higher temperatures. Possible dehydration of minerals, and the question of where does the water go after whatever period of time it is and it finally cools down and starts to condense. Those are all earth science issues, but they interplay with that design aspect.

Relative to the retrievability and monitoring, I think that's very much recognizing uncertainty in our earth science. One can just simply argue for that purely on the uncertainty that one has about the geologic aspects of the repository, so again that's an earth science engineering design aspect, if you will.

And the third aspect, which is the ventilation, again, is very much related to seepage into the tunnels and the--again a fundamental earth science aspect, perhaps the most fundamental earth science aspect. So we only commented on the design aspects as they relate to earth science issues.

KNOPMAN: So in saying that you find Yucca Mountain a suitable site, which the letter does say, it's a conditional statement? It's conditioned on your view of design?

ALLEY: Yes, I would say so. It would be very important. I think that further understanding the conceptual framework for movement of moisture through the unsaturated zone, the whole issue of past pathways is still out there and being discussed in a relationship of faults to rapid movement through the mountain.

And then there are some areas which I think could build confidence in terms of the mountain that really haven't been probably taken as much advantage of as possible. I think that characterizing the unsaturated zone from the repository to the water table is an area where we could build more confidence and reduce uncertainty relative to essentially what happens when the waste--inevitably some of it will leak out of the bottom and move downward, and there really is not that much known about what is going to happen in that zone. So I would say, you know, again thinking towards monitoring, trying to further the conceptualization of the unsaturated zone, and looking at the data sets that we already have and making sure we don't too hastily abandon those, when all that infrastructure and knowledge is built into them.

MR. WONG: Dr. Bullen?

BULLEN: Bullen, Board. Sort of along the lines of what Dr. Parizek said, but you have, or your organization has the history of a long--the benefit of a long history with the site, and you have, you know, developed essentially a number of points that you think are attributes. Specifically I'm interested in are there any data sets--as we go through the transition to, or the potential transition to a more licensing focus, and you know, you talked about the people who think outside the box they may be a little bit repressed in this, is there any data or critical data sets that you think might be important to pursue, and how would you rank them? I mean right now the Board has always strongly stated that we wanted to see the continuation of a good scientific program to support the long-term performance. What types of data sets, what type of information would you like to see continue to be developed from the USGS perspective?

ALLEY: Okay. First of all, I think it's important to understand that the importance of long-term data, so in other words many of the data sets that are being carried out today, it's important not to abandon those and move over here, because they've developed the knowledge that you can build on. So I think a very strong look at what the current data sets are and which of those should be continued, clearly that builds a case for a lot of thought being given now towards what is referred to as performance confirmation, or how does one monitor the site.

WONG: Dr. Craig.

CRAIG: Paul Craig, Board. There have been a number of concerns raised that the mountain would not do the necessary job of isolation in the absence of the canister. In fact we heard such a statement this morning from Steve Frishman. And then just before the break we saw some of these breakthrough curves that showed that a significant portion of the water would pass through both the UZ and the UZ at times less than 10,000 years, the regulatory time. And a significant fraction in 20 percent--20 percent or so, at times, much less. Is it the--less than 10,000 years. Is it the position of the Geological Survey that the mountain without the engineered canisters could provide the necessary isolation?

ALLEY: I think we haven't done the analysis to really come to that conclusion because there are so many--I guess you run the TSPA as a first cut at that without the canisters, but we haven't carried out that kind of analysis.

CRAIG: I'm trying to understand the basis for your confidence--

ALLEY: Right.

CRAIG: --that the system will perform, which I think was the essential element in your letter.

ALLEY: Right. I think our view is that the system will, that probably the natural system, the natural barrier is a good natural barrier. Playing that all out relative to the standards that have been set forward in terms of dosage and things like that is a very complicated detailed analysis that we have not gone through. And so we can't really make a blanket statement that we feel that the mountain will perform exactly as the regulations say it will.

CRAIG: In that case I guess I want to say I'm confused about what the basis is for the positive statement that's in the letter.

ALLEY: I think that the basis for the positive statement in the letter is that we view this as a step in a step-wise process. We see the mountain as a good natural barrier. And we see that there is continuing work that has to be done to monitor the performance of the mountain, and it's a stage-wise--there's no absolutes here. I think it's the basis of our letter and we're looking at this as a step-wise process, and if one looks at the current, where we are in time, right now, we would say that the--it seems like--and there's no fatal flaws that we can discern relative to the mountain performing as a repository.

WONG: Okay, do we have any further questions from the Board? Board staff? Okay, seeing none, thank you. Thank you, Bill.

Our next speaker is Dr. John W. Bartlett. Dr. Bartlett will give a presentation on the Clark County Review of the TSPA. He is with S. Cohen & Associates, and from 1990 to 1993 he was the Director of the DOE's Office of Civilian Radioactive Waste Management.

BARTLETT: I got religion this morning so I took off my back-east suit and tie. Thank you.

The prior discussion gives me an opportunity for some historical perspective. It happens that I was involved in preparing the first program plan, the first office in the Atomic Energy Commission that recognized disposal, The Division of Waste Management and Transportation, 1972. The Division sent us down to the Nevada Test Site to talk to the USGS about the potential for using the NCS. Very quickly, the USGS sniffed and said, "Well, we have 900 years of experience in characterizing this site, and for another 900 we'll let you know about the feasibility." And then they offered us the call there to the mountain. And that was the initial point of operation.

Also, not long after, there was a meeting held where representatives of the program, in essence, for the first time, really met with the geology community. And we said we would like you to predict things like frequency of seismic activity, different levels. And they said, you want what? At the time the idea of plate tectonics was just coming into broad acceptance. So things have come a long way, and actually over sort of a long time, but they--we're not in focus are we. We'll get it down a little bit. Sorry about this.

(Pause.)

Thank you. Well, things have come a long way, as you can tell when you think about some of that perspective. This, as it says, was an independent review, PSSE and it's supporting documents done for Clark County. And the key operative word here is independent. Clark County was scrupulous in letting us do our thing. So scrupulous in fact that when I talked about this at the ACNW meeting in November, Englebrecht observed that he'd never ever seen the slides. So it was totally independent at the time. And was totally.

The objective of this effort is taken here, this is right from the statement of work. Basically, the effort was to get substantively into what was done with regard to TSPA in particular, just the TSPA aspects of the performance of TSPA. For the PSSE specifically, and the documents that supported it.

The scope of our efforts was measured in feet of documents, and this does total about six feet when you pile them all up. And anybody who would like to take them out of my closet is welcome to do so. Little phrase here, AMRs and PMRs that were available. Thanks to the generosity of the libraries at TRB, I had access to virtually all of them. So we did review all of these documents to come up with the findings I'm going to tell you about today. And that's a lot of pages. For example, the TSPA for the SR and the supporting model and assumptions documents, just those three total about 5,000 pages.

Let me talk first about the characteristics of the documents and the relationship between the documents and the TSPA efforts, as it was reflected within those documents. I assert that there was substantive technical information that's concerned with the TSPA efforts. It's all there, pretty much. But it's limited in one document in particular and very difficult to trace throughout that suite of documents. This is what we found as we went through this effort. There was no single document that really pulled together the substantive content of the TSPA effort. And secondly, relationships between the models and the assumptions and the data that were used in the TSPA effort were not clearly evident throughout the documents as they were reported.

Thirdly, with respect to the characterization of the documents, it was hard to find information completely concerning a given topic in a given document. And I can illustrate this by the fact that when we did a review of the viability assessment, one of the things that I looked particularly closely at was the cladding performance, specifically because it is an expensive body of data, and you could, if there was enough information in the document, make a comparison between evidence that was available and the assertions and methods that were used in the documentation and thereby make a reasonable effort of conservatism, whether it was there or not, or whatever. With the VA you could do it. Everything you needed was in the VA, in a couple of supporting documents. And I could come up with an assessment of conservatism that I had some confidence in, comparing the data to the documentation.

In the case of these documents I found I couldn't do it. Kept getting referred from one document to another to another, and ultimately the substance proved to me, as far as I could determine, actually in the AMRs and PMRs, and specifically in the AMRs, right at the bottom of the chain, and so you had to trace through this to try and get an audit on any specific topic. And so I generalized that by saying that what happened, or appears to have happened, is that the traceability and continuity of information concerning TSPA was converted in this documentation to more what I called information accounting. As far as I could tell, referencing one thing to another, they never missed. The referencing, cross-referencing was always correct, but the ability to trace the information relevant to a topic was bound to be very difficult.

Now, there was a previous trade press report on this and the headlines said, "Documents are a mess." As if I said that. No, I didn't say that. The documents are written beautifully. But for purposes of trying to trace through this suite of documents on the TSPA topics, we found it to be very, very difficult. Somebody did a beautiful job of preparing the documents themselves, and I congratulate them.

Now, the findings with respect to the TSPA analyses. We found that many assumptions were extreme and seemed not to be related to data or realism in many cases. And it was very hard to trace the basis for the assumptions. They were just not there. And there was no rationale in many cases. This was particularly true of the TSPA-SR support documents. If they just stated what were the alternatives, why was this one selected, what effect does this have, etcetera, could not--I could not trace that throughout the documentations. And these assumptions, as I say, are apparently highly conservative, were non-conservative, but you really can't get a handle on them, which was the objective of this effort.

And I sort of ran over it, but the TSPA-SR, which is what many people have reviewed, such as the International Group, is quite different from the TSPA that supports the site suitability evaluation. The results are very different, methodologies are different, but the basis for the differences I had a very hard time finding, and in fact couldn't except for some major factors. Two things were apparent: The TSPA in support of the site suitability evaluation, preliminary, had assumptions concerning, or used a temperature-dependent corrosion model, and radically changed the assumptions concerning the solubility of neptunium. Two really key factors. Also an assumption that there were well failures that gave early package failures. Beyond that it was very, very difficult to find the basis for difference between the TSPA-SR and the TSPA supporting the PSSE.

As a result we found that the documentation doesn't provide a sound foundation for, particularly, the S-TSPA, which is, according to the documentation, the basis for the preliminary site recommendation. Not the TSPA-SR. So it's the S-TSPA that you really have to understand to understand the basis for where the program stood at the time. And by the way--

BULLEN: John, just a quick question here. Bullen, Board. We're not familiar with the S-TSPA. Is that the suitability TSPA you're referring to?

BARTLETT: Yes. PSSE, Supplemental--

BULLEN: Oh, supplemental TSPA?

BARTLETT: Yes, yes.

BULLEN; Yes. Okay, so that's SSPA. Okay.

BARTLETT: That's Volume II--

BULLEN: Of the PSSE file.

BARTLETT: Of the TSSA.

BULLEN: SSPA?

BARTLETT: Right, there's going to be a quiz in the morning.

BULLEN: Okay, I had seen it as STSPA, so--okay.

BARTLETT: Yeah. It sort of runs on.

BULLEN: Right.

BARTLETT: So this is the shorthand. But yes, the documentation, the supplemental that specifically supports the PSSE. And as I said, that's very different from the TSPA-SR. And as a result you wind up in a situation where it's very hard to find the foundation except what you found in fact was that there seemed to be a lot of extreme assumptions within that foundation.

The result of this in our findings is that, as it says here, you get the impression that the projections of performance are much more an artifact of the assumptions than they are realistic representation of the repository itself. You could have come up with any result depending on what assumptions you made. And they did not seem to be closely related to the specific technical information that was available. It would have been closer, I think, if the basis had been related to what EPA calls reasonable expectation. Very simply, take your best shot at what you do know and see how that comes out. But that didn't seem to be the basis for these.

The TSPAs did not use a specific repository design as their basis. And so the variations on the high temperature performance and the low temperature performance were presented in such a way that you could not interpret them realistically as a basis for comparison of those two conditions. And so we couldn't get a solid foundation for the suite of results, and again a foundation for the supplemental TSPA.

And of course, as we all know, as the repository design stands right now, the performance during he regulatory compliance period depends essentially solely on the Alloy 22 where the current data bases, by many people's thinking, very small and fragile, and the ultimate long-term performance is genuinely unknowable. Now, you can make some good projections or estimates of whether or not that film is going to stay stable, but becomes a probablistic assessment. But it is ultimately unknowable. And of course DOE's analyses found, and we all know, that most of the performance factors are temperature dependent, but the performance was found not to be temperature dependent.

There may be a reason for that in that the temperatures spike is relatively a short duration. And this gets to this next point, that the analyses imply within this framework of assumptions and the like, that either the high temperature has no apparent effect and the temperature dependence has no apparent effect that's lasting, or they have no persistent effects throughout the operation and the life time of the repository. But you can't tell from the analyses, as we were able to interpret the contents.

We're all familiar with the use of one-off analyses and the Board's suggestions of one-on analyses. I have a suggestion relative to that. Way back in 1988 the site characterization plan--a lot of you may still have been in school at the time--the basis for expectations of performance of repository at Yucca Mountain was that the mountain would be fantastic and the NRC's requirements for waste package life time were 300 to 1000 years. And I remember in a senate hearing giving perspective on that by saying that if you had placed the package during the battle of Hastings, it would still be intact. It's some idea what a thousand years was at the time.

Well, if you went back at this point and used the 1000-year package, which is a simple stainless package just to get the thing in a hole in the floor, and what we know about the mountain today, how would that come out? Going back to the basis of the SCP. I don't know. It's an interesting interpretation of this whole question of natural versus engineer barriers, and what the role and capacity of the natural barriers is.

Well, again, hot and cold repositories have not been evaluated in detail, and they pose of course different problems. If you have a hot one you may have significant coupled effects. They may be short in duration. They may be not lasting in duration. But they should be characterized, and that's a big unknown, as we all know. If you have a cold repository you may have to have a big footprint. You may have to know more about a larger piece of the geology in order to have a realistic assessment. Those kinds of details we didn't find in our reviews.

And then I think the last goes without saying. At the time that this was done there had not been comprehensive reviews. The IAEA/NEA team was under way. The waste package people were doing their thing--still don't have their final report, but there had not been this kind of comprehensive review which the elements of these analyses suggests should be done in order to have confidence that they represent the repository system, or at least you understand what was done with them to represent repository system.

So that's a brief summary of our effort, and there is a comprehensive report available if Clark County is willing to distribute it. I'd be glad to answer any questions.

WONG: Thank you, Dr. Bartlett. Questions from the Board? Dr. Parizek.

PARIZEK: Parizek, Board. You've given us a good look at the problems, and a lot of us who have reviewed these documents and all--

BARTLETT: It's not all new, obviously.

PARIZEK: Well, I mean struggling through the whole process, but your bottom line or the bottom line of your review may not be too clear, and I was--can you conclude from all of that that the site is not suitable, the geology is not suitable, the canister is not suitable, or is suitable? Or you're suspending judgement, just showing the trouble you had, trying to arrive at a conclusion?

BARTLETT: I would have to suspend judgement based on this information as it was presented. Trying to do a detailed technical audit, so to speak, of the technical work that was done to provide a TSPA as a basis for a finding.

The suite of documents with a lot more manpower than we had available could yield that information, but it would take a lot more. It is not very clear and crystalline in the information provided directly as a basis for the preliminary site suitability evaluation. The results are clear. Where they came from, how they came out of that enormous effort, we had just had a terrible time working out from this suite of documents.

PARIZEK: So your recommendation could be what then? To clean it up?

BARTLETT: Yes. Yes. To essentially do the kind of review that the IAEA/NEA did. But on the supplemental, or whatever it turns out to be, the actual TSPA methodology and assumptions that are used to support a recommendation should it be forthcoming to the President.

PARIZEK: And then a summary document, perhaps that integrates all of this--

BARTLETT: Yes.

PARIZEK: --is growing faster than you can--

BARTLETT: I think it's all there. You just can't find it very readily. As I said, I traced through this business with cladding performance because I had done it before. And I went to five different documents and I still couldn't pull it together the same way that I was able to do with the VA.

PARIZEK: Yeah. One last question. The number of people involved in the process, I mean to give an idea of the level of effort, I mean one person would die trying to do several--

BARTLETT: With the equivalent of one man-year, roughly.

PARIZEK: Yeah. I mean but a team of people from your-

BARTLETT: Several people, yes, reviewing the various elements with relatively expert knowledge.

WONG: Dr. Bullen?

BULLEN: Bullen, Board. Actually, you touched upon a couple of issues that the Board has mentioned previously. The first of which is traceability and the ability to take a look at the documentation and figure out where the data are that are drawn upon and the assumptions made. And secondly, that the issue of transparency or the ability of the project as a whole to not only sell it to the technical review board and to sell it to Congress or just sell it to the President. But to basically put together a presentation that's lucid and understandable by the general public. And I think I remember five years ago saying that my, at that time 13-year-old daughter, should be able to read this and understand it. Now, I guess the question that I have, in your overview document would it be helpful if a simple explanation of the uncertainties and the bounds of performance were presented, and comparing that performance with the regulatory standard and laying it out in a simple term? Do you think that would be sufficient, or what are your suggestions I guess would be the--for the type of presentation that would be understandable not only to the technical reader, but to the general public, because I think that's kind of the bend that you're looking for.

BARTLETT: Well, I think they are very different. And I would underline the fact that the documents are beautifully written in terms of what they present. The top level documents, these public, or semi-public documents, are clearly descriptive of what was done. What is not there is why it was done, and the traceability to the technical foundation for it. Ultimately the information is in that suite of documents is under the AMRs. That's really where it is. 2000 of the--somehow you've got to distill all this information if it's acceptable to Justice Fry (phonetic). Here is what we did in an attempt, in general terms, to say why. It would be different, and I don't think any of that, frankly, would be suitable to the Board. It's just missing that kind of detail. The kind of detail--we were trying to (inaudible) as a surrogate of the Board and found them very difficult.

BULLEN: Welcome to the club.

WONG: Dr. Craig?

CRAIG: Yeah. I actually heard you raise two types of questions. One is the one that we've been talking about for the last couple of questions, which has to do with the basic posture is the information is probably there, but it's very hard to get at. And that certainly is a problem. I think no one, I don't see how anybody could legitimately argue with that assertion. But there was another assertion that you made, and I wrote it down almost as a quotation. Many assumptions are extreme and are not related to data or to realism, and they are not explained or justified. That suggests that important ideas are not in fact in the documents, no matter how much re-writing you do. I'd like to hear your--that type of issue.

BARTLETT: That's, I think an astute observation. I cannot find why there were assumptions about--why there were assumptions concerning some of the factors, the performance factors. I couldn't find, you know, one man-year's effort of review. They could very well be done in the underlying technical documents which actually were unavailable for public review. And certainly, there is nothing on the web site now in that arena. But it wasn't those kinds of the bases of assumptions, some of the really critical ones. For example, in the supplemental TSPA there's just a brand new approach to cladding performance in comparison with the VA. Where that came from I simply couldn't find. There's assumptions--it was very simple in the VA. 1.25 percent is going to fail, bingo. That in itself had no basis in reality when you look at the fact that the data bank says .1 percent have historically failed. So here you have from .1 to 1.25 with no basis back in the VA and now one you can't find in the suite of documents that have come forth since the VA.

WONG: Dr. Sagüés?

SAGüéS: Thank you. You make a statement here in this summary of the principal findings and thus in the first transparency that the TSPA results for the unitary compliance period depends solely on Alloy 22 performance. Now, I think the project has made the argument that if you work with severe, with distress packages like with 300 centimeter square holes and so on, the performance still is--I mean it's degraded compared with what would happen if the packages were not distressed, but it's not so severely degraded that it would begin to get very close to not to be in compliance. So when you say it depends solely on Alloy 22 performance during that period is that (inaudible) or do you really mean--

BARTLETT: No. It's very nearly solely. In January, 1999, the month after the VA was published, at one of your meetings, DOE presented a bar chart version of the contributions of the principal performance features of the repository. And it was done sort of a perspective, and it was a log chart, so it's very hard to be precise. But you could estimate that, in essence, that chart showed that there were 903 elements of performance. 900 of them were the Alloy 22. And you could estimate that the UZ had .02, and the SC had .05, or something like that. But it was very, very small. And since then the design has evolved even more because now the Alloy 22 is on the outside. So if you use whatever the basis was then to extrapolate from 903 to whatever it is now, or the same sort of thing, you would find basically, especially under the TSPA-SR, that Alloy 22 is it. And the current strategy is to rely on that.

Now, that was modified in the supplemental. As I said with our limited manpower I could not trace the basis for the modification except to say there's going to be weld failures, or truss corrosion cracking or something.

SAGüéS: Okay, so but this statement didn't refer, maybe a little bit earlier to the TSPA/VA--

BARTLETT: Yes.

SAGÜéS: And one last thing. You say that in the same bullet here that depends solely on Alloy 22 performance for which the current database is small and fragile, and the long-term performance is unknowable. Now, unknowable is a very strong term. What do you mean by unknowable? That it could never be known, it is impossible--completely impossible to predict, but of course, you know, if we're talking about forecasting tens of thousands of years--

BARTLETT: Well, that's exactly the point. I think you've made the point yourself many times in these meetings about whether or not you can expect the film to be stable. The waste package task force or that expert group found three things that could go wrong. And they simply say, we don't know whether they will or not from either--any one of them or whatever. And I think that is not for the 10,000 years, that's not an inaccurate statement. It's unknowable. You can say with a very high probability, perhaps, if you've got a better database, that it's very likely that in fact it will perform as expected. But for 10,000 years?

SAGüéS: Sure. That goes to just about anything in the repository, right?

BARTLETT: Oh, absolutely. Absolutely.

SAGüéS: Yeah, but that's something that I guess the project has never questioned?

BARTLETT: No. One of the things for example, I noticed way back when there's--I mentioned tectonics. In that 10,000 year time frame, or what is it? I forget which time frame, but the thing, the entire repository, the entire structure will translate about a mile on the surface of the earth. And so are there differential translations in terms of depth and effects on formations? I mean these kinds of things I put in the category of unknowable. And relevant--

you can attach probabilities, but unknowable.

COHON: Could I just follow up one?

WONG: Go ahead, Dr. Cohon.

COHON: Just to follow up on both aspects of Alberto's question, starting with the latter, which is really I think a semantic issue. I don't think anybody disagrees with you, but--well, maybe. Of course, none of this is knowable in advance. But it's all knowable in retrospect. I mean it's knowable.

BARTLETT: Yeah, it is knowable. It's an issue of when you know it.

COHON: Right. Okay. On the first part, which I think is more important, your observation about the total reliance on the waste package, I think looking at it from the context of the supplemental TSPA, I think that maybe a more complete statement--it doesn't really challenge what you're saying, but a more complete statement would be the DOE estimates of performance for the waste package are so robust that it doesn't matter what else happens.

BARTLETT: That's one way of putting it.

COHON: However, I mean in the USCS discussion we had earlier, shows that, you know, if you put this stuff in with no package whatsoever, there would still be some delay in the waste appearing at the accessible environment, whether it would be in compliance--

BARTLETT: Compliance is another question. That's right.

COHON: Right. So compliance very much seems to be dependent on Alloy 22, but it's not the only--

BARTLETT: Yeah. DOE has built a marvelous margin to compliance with the present concept. I would estimate it's only a factor of a million. In reality--but your letter has a wonderful sentence in it about compliance ain't necessarily understanding what the system is doing. And yeah, it's a fantastic machine for compliance. No question about it.

WONG: Any further questions from the Board?

Thank you very much, Mr. Bartlett.

Our next speaker will be Dr. John Garrick who currently is the Chairman of the Advisory Committee on Nuclear Waste, or former Chairman of the Advisory Committee on Nuclear Waste, and their findings in terms of review of the PA. And I might add that I commend Dr. Garrick because he is still wearing his tie.

GARRICK: And I'll explain why. I packed the damn thing and I'm stubborn. And besides which, it's a better thing to hang the mike on. Thank you. Thank you very much.

I'm pleased to be here, but the only other time I've presented anything to the Board was shortly after it was formed and it was not in the context of being on the

Advisory Committee on Nuclear Waste. I was an independent, and I was brought in to talk about the subject of human intrusion. And I'm glad that that's not on the agenda today.

I'd like to acknowledge--I want to recognize Dr. Andy Campbell. We have an agreement. I'll make the presentation, he'll answer the questions. So I feel pretty relaxed.

What I would like to do is talk to you a little bit about what the committee did here. And I think I'm probably the second person that's here because of a letter. We wrote a letter that was reasonably critical of the TSPA-SR. You've heard a great deal about the TSPA-SR and what's right and wrong about it. And I'll try not to just repeat what has been said. But this was in the context of a much broader question that we were trying to address. And that was the question of the adequacy of the NRC's issue resolution process.

This is the process by which the NRC will make a decision as to whether or not sufficient information exists to enable them to docket a license application for Yucca Mountain. So that was the principal assignment that the committee took on. And the committee is a very small committee. There are only four of us. And so we adopted a vertical slice strategy. And the vertical slice that was assigned to me had to do with the TSPA-SR and the NRC's activities associated with performance assessment.

In the process we also, in order to assess our given opinion or our judgement about the capability of the NRC to reach a conclusion relative to sufficiency, we had to look at the DOE documents. And of course, that's a major, major, major task. As a result of our vertical slice effort we issued a number of reports. In fact there's a couple more that will be added that are added to this. And one I see circulating around here today on conservatism that just came out a week or so ago. But we issued a report on high level waste chemistry issues. One of the vertical slices was on that. We issued a letter on the issue resolution process itself. That was a fairly global challenge. And in a sense contained the performance assessment component. But because of its rather importance in the whole decision making process, we chose to issue a separate letter on the total system performance assessment site recommendation, and I was the lead member for that activity.

The conclusions that we came up with with respect to the resolution process are consistent with the NRC staff's sufficiency comments. That is to say we focused on some rather narrow issues, and even though they had some rather critical aspects to them, we did not find ourselves out of position with the Commission staff with respect to what they were saying about the progress that had been made in establishing sufficiency.

We focused on ways to improve the TSPA before the license application. The strategy that we attempted to take on the vertical slice was to see if we couldn't pretty much start with what we thought were the principal drivers of the risk and peel the onion back from that on the basis that, while there is still some debate going on, that there may be other radionuclides making a greater contribution than the three or four that have been identified, radionuclides such as maybe chlorine or maybe protactinium or one or two others, cesium perhaps. But if we can take the position that we're reasonably confident that the risk of this repository is going to be principally driven by neptunium, tecnicium, iodine, then we're--and colloids of plutonium, then it seemed to us that one of the things that would provide focus to the vertical slice would be to concentrate on those radionuclides and back our way into the analysis.

And the other thing that was very important in this was that our committee has been challenging the NRC and the NCR staff for many years to move more aggressively with respect to the risk informed regulatory practice. There is a great deal of talk, it's now time in the judgement of the committee to see how well we are able to walk that talk. And so given the assignment was mine, it's quite understandable that I would put a lot of attention on just how risk oriented, risk analysis oriented was a performance assessment.

We've heard a great deal about these other issues of transparency, traceability, and defensibility of the results, and I'll come back to those a little bit.

Now, one thing I should say is that the committee has been a very strong proponent of the use of probablistic performance assessment. Our total system performance assessment. But we have some conditions under which we are great believers in this. Now, my own personal thing, and I will not speak in behalf of the committee in that regard, is based on a much broader view of the development and application of risk assessment than with respect to the waste field. I've led a team that did the early large scope risk assessments on about half of the nuclear power plants in the U. S. and about 20 to 25 foreign reactors, and I think that, as much as anything else, had contributed to my optimism about the utility of this particular tool. I think the main thing that I liked about it, not being trained to be a risk analyst in the first place, I was trained in physics and nuclear engineering. I was in criticality and neutron transport to begin with. But what attracted me to this was a number of things. And a lot of those things have been confirmed by that experience base. But one of the things I liked most about it, it deals with the question of "so what". One activity you find if you serve on panels and committees and review boards is that it is very difficult to keep things organized, focused and converging. The risk assessment helps that process. But it requires some things. One of the things it requires is agreement on what the performance measures are.

What is it that the Nuclear Regulatory Commission, the Environmental Protection Agency, the agencies that are involved in this, what is it that they want to bank on to characterize the risk of their facility? Now, in this case it's pretty much prescribed to radiation standard and it's the likelihood of being able to comply with that standard in basically three areas. The overall risk associated with the repository, a stylized human intrusion assessment, and the ground water standard.

The other thing that we have to have for a risk assessment to have credibility, and much has been made of this already, is that the analyses models must be realistic and reasonable within the limits of the evidence. And the DOE themselves in the TSPA-SR make this assertion. So they are very much aware of the fact that the protocol, if you wish, for risk assessment is not that you build a probability density function around the bounding value or that you build a probability function around the conservative assumption and then propagate that and say you've calculated the risk, but rather to, as somebody said earlier, I guess it was John, give it your best shot. We want to know what the experts really think the risk is. And the reason we want that is we want a calibration. We want the best people that know how to do that to do it first and then give us a reference line against which the regulators, the public, or anybody else can be as conservative as they want to be. But at least now they've got something to be conservative against.

Results including uncertainties are quantified. Quantification is a big part of my interest in this discipline and what the committee has been talking about. I've been impressed with the use of the word evidence that I've seen in the NWTRB documents. We have used this word many times for a long time, and we like to characterize analysis as having--as there being two types: Evidence-based and assumption-based. And you much prefer an evidence-based analysis.

Now, here is what we found out during our vertical-slice. First, in the over-arching conclusion is that it's not a risk assessment. It's basically a compliance assessment. It is focused very much on the standards, but it's not telling us what the risk is. The modeling as we were able to determine in our rather abbreviated investigation of these--this massive amount of material, the assumptions were quite inconsistent. There were some assumptions that were clearly very conservative, some assumptions that were pretty realistic and some assumptions where there's chances they were non-conservative. And so it was a mix of conservative and non-conservative elements and that's a violation, if you wish, of why a quantitative risk assessment was invented. And there are many examples. For example, in working this out and consulting my colleagues in the area of coupled processed for example, we were able to find that these processes at the process level were treated quite independently, but somehow during the abstraction process they were combined. And we didn't know and couldn't quite figure out just how that combination took place.

With respect to the source term, we had lots of questions about the assumptions having to do with the in-package condition being a water saturated condition for all of the packages. And the impact that would have on the mobilization of the waste, when there is no evidence that would really support that kind of an assumption. The diffusivity transport model, it too contained a great number of assumptions and conditions that gave us some concern with respect to the realistic and reasonable approach. Such as the assumptions having to do with the liquid film and the assumptions having to do with the coefficients, the diffusion coefficients.

There were a number of other things. This business of clad failure. The unzipping of fuel cladding. Obviously, the team was not very basion, or I don't think they would have made the assumption they made about the fuel cladding unzipping, because there's thousands and thousands of assembly years of experience in storing this fuel. And so here was a case where an assumption replaced evidence that actually existed. And then the whole business that we've heard quite a bit about, and I could go on, on solubilities. In some cases the analysis was driven by solubilities that were assumed to be constant and then you would find reference in the document that the reason there was no uncertainty with the solubility is because it was assumed to be constant. Well, that's not risk assessment. So these are the kinds of things that we worried about. So we thought that the analysis was, for the most part very assumption-based. Some of the assumptions were very difficult to, in themselves, be rationalized with respect to their supporting evidence.

And this most important thing of the margin of safety not revealed, therefore was denied the reader. And then I think that while everything else I said here was clearly a committee kind of finding, I had been hounding on this issue for 10 years of a simplified model. And I think that when you talk about a situation where you have some 250 to 300 radionuclides of the fission product for variety, and several dozen radionuclides of the actontinite variety, and the analysis is pretty convincing that only a very few drive the risk, it seems to me that right off you have a wonderful opportunity for building some very nice physics-based, simplified models. And I think if they did that, the kind of things that John Bartlett talked about would be overcome.

So what was our conclusion? Well, conclusion is very simple. It's a very handsome piece of work in the context of looking at it from a point of view of being in compliance with a 10,000-year compliance period. But it does not answer the question, what is the risk? And I've heard a lot about people, including this Board, not wanting to rely only on the risk assessment as a basis for making a decision. Well, clearly, you can't rely only on a risk assessment. Decision making is based on three broad categories of attributes: Costs, risks and benefits. The risk is one of them. But on the other hand, if you are talking about risk and you are asking that additional analysis be done outside the risk assessment and that analysis turns out to influence the risk, then by definition it has to be part of the risk assessment. And this is an area where there seems to be a tremendous amount of confusion and miscommunication. I think the model complexity inhibits confidence in the results. We've said that. And I think the linkage between the assumption set and supporting evidence lacks the transparency that we are all looking for. So those were our fundamental conclusions.

Now, what I didn't present today was what we had to say about the NRC and their approach in the TPA world. But I assumed that the main interest here was DOE. So what's out recommendations? Well, of course, what you haven't done, we recommend be done. And most important is to implement the basic tenet of risk assessment. Realistic and reasonable results, scientific basis for quantifying margins of safety.

Now, the risk assessment business is going through a period of maturing and trying to find its way, but one of the ways it is finding is that when we talk about a risk assessment, particularly a quantitative risk assessment, we are talking about realism. And we are talking about reasonableness and we are talking about quantifying the uncertainties. I've always had the feeling that if there is one thing we should know, it's what we don't know. And it's sometimes very difficult for us to admit to that. But we need to do that. And especially on projects that have as much public impact as this one does.

We recommend that we improve the traceability between the evidence and the risk-informed results. We did the same thing that John Bartlett did. We tried to at each way in this backward thread that we were taking, find out what the assumptions were that were providing the boundary conditions for the analysis, and what the supporting evidence for that were as well.

I do still think that the abstraction of a simplified basic physics model would serve the project immensely. And the only reason I say this is not, again, out of an abstract thought about what we'd like to have, but it has been an enormous benefit in the reactor field. In the reactor field we have something we call very often a dominant sequence model. And these dominant sequence models now have been computerized and have been put in monitors in the plant and so that they now have a kind of a first order or zero order of proximation of what the condition of the plant is in terms of risk when a particular system is taken off line. It's something to think about. And of course, in my interpretation of what a risk assessment is, it's a structured set of scenarios. And if--now in the case of the facilities you end up with millions of scenarios, but it also turns out that a relatively and manageable few scenarios tend to dominate the risk. And if you somehow characterize those in the form of a model it's amazing what people will do with that model and what opportunities exist for communicating what this whole business, what otherwise looks like to be a very complex exercise is all about. So what's the follow up here? Well, we haven't done a review of the supplemental science and performance analysis. This is what I guess John Bartlett was calling the S-TSPA. But we have read it and we've looked at it and we have found that what we see there, we like, in large measure. And even without--before we wrote our letter, it was clear that this was well along the way and that the DOE had recognized some of the shortcomings of their TSPA and were working on it.

There's other documents. There is the updated letter report that we've heard about today. And most of the documents are giving us added confidence that the criticisms of our September 18th letter are being addressed.

So with that I think I will stop and ask for questions.

WONG: Questions from the Board. Dr. Cohon?

COHON: I'd like to ask you a question that I will admit up front I would refuse to answer.

GARRICK: Okay. I get a lot of those.

COHON: Maybe I'll get lucky. Based on your assessment of TSPA and the tactical basis that DOE has assembled, do you think they were ready to make a site recommendation?

GARRICK: You're right.

COHON: Well, you can refuse to answer it, too.

GARRICK: No, I don't refuse to answer it.

COHON: Good.

GARRICK: I think that what I'm talking about primarily is I'm measuring the TSPA as--in terms of what I see as a prescription for a rational risk assessment, assessment of the risks. And whether or not when they, if they did everything that the committee wanted them to do, how that would change things with respect to the site recommendation. I suspect in fact it may not change them qualitatively but quantitatively. But I think that some aspects of it would be changed dramatically, and that is the confidence that people have in the risk assessment. So I think the only finding that we feel is important right now is whether or not we have seen enough--and I'm not NRC. We're an independent advisory body. But let me characterize it that way--whether or not we think we can have enough information to file, to enable us to file a license application and we're reasonably optimistic about that.

WONG: Dr. Knopman?

KNOPMAN: Knopman, Board. Here is another one, John, you don't have to answer. The NRC staff has developed its own TSPA as we understand it.

GARRICK: Right.

KNOPMAN: Would you venture into some characterization of how much closer they come to a risk informed realistic assessment as compared to DOE's?

GARRICK: Well, I think the short answer to that is that we've been pounding on them for a couple, three years and I think Tim McCartin is here in the room, and he probably is worried to death about what I'm going to say, but I think they clearly understand what we are talking about and the activities that they are engaged in and as they update the TPA, are certainly in the direction that we've been advising them on, so I'm encouraged by it. One thing you have to appreciate is that their approach to the TPA has to be different. Their approach is not so much to do with independent performance assessment, although that's part of it. Their approach is more to develop a model that will allow them to verify and review, and they've recognized that. And I think that as a result of that they are able to take some efficiencies that they wouldn't otherwise take if they were really trying to develop a competitive TSPA.

WONG: Dr. Bullen?

BULLEN: Bullen, Board. Actually, in continuing the line of embarrassing questions, I thought maybe I would ask you that, given that it's not a risk-informed TSPA, do you feel that it's an adequate compliance-based TSPA, and is that not necessary or sufficient for a site recommendation that we meet compliance so why should we not go forward?

GARRICK: Well, when I had a face-off with the Chairman of the NRC on this same subject, that's kind of the question he posed as well. And in the context of the regulations I think it's a reasonable compliance performance assessment at this stage. I think even there there's shortcomings. But at the same time, we have taken the opportunity to push the NRC a little bit on the basis that they are committed. They are committed to risk-informed regulatory practice. And where it's--we have not always been pleased with the progress, and we've not always been pleased with the staff's actions in that regard. And so this was an opportunity for us to communicate against something very specific as to what we mean by that.

WONG: Dr. Sagüés?

SAGÜÉS: It's getting to be late day--

GARRICK: Yes, it is.

SAGüéS: --but I enjoy very much the approach that you took for your presentation and then I was looking at your CV here and a little bit of your background.

GARRICK: Do you see a proper match-up there?

SAGüéS: Yeah. In about say 500 years or a thousand years or maybe 3000 years, there is not going to be an NRC, and there's not going to be a lot of the institutions that we are living with right now, and at the whole overview, the questions that you hear, the approach to the reports is heavily, heavily regulations oriented, and is heavily oriented towards the overall culture that exists around the regulatory agencies that are supposed to grant the permit, etcetera. Now, none of that is going to make any--is going to have any immediate relevance in the far future for which this repository is being contemplated. Now, do you think that maybe the overall approach is too much regulations oriented, too much institution oriented? Shouldn't it be viewed as an issue of public health or something like that instead of this, this very highly-focused view that we're using right now?

GARRICK: Well, I consider myself a systems person. And I like what you are leading to. If I had my way, there wouldn't be safety goals. There wouldn't be any of that. What there would be would be a very comprehensive Manhattan project, Apollo Project effort to quantify the various energy cycles, the hydrogen cycle, the uranium cycle, the fossil cycles. And to let the results of that analysis performed in the context of a decision analysis framework, speak for itself and the citizens vote accordingly. That's how I would do it if I had my way, because I have a feeling that if we really did that the right way, and recognized that energy is not something that you can solve in four and five--let's see, two, four and six year increments coinciding with the election intervals, but is something that has to be done a 50, 100-year horizon. And I think that's what is really missing. And so I think the broader issue of health and these--and this also happens to be one of the things I really like about the risk assessment technol--discipline. It is not a compliance thing. It's asking one very simple question, and that is, what is the risk? And my colleague and I formalized this a little bit in the first paper of the risk analysis journal in 1981 to put forth a definition of risk. And that's the three questions that are on this handout you had, namely, when you ask the question, what is risk, you're really asking three questions: What can go wrong, how likely is it and what are the consequences? And the what can go wrong component of the question is best answered by a series of scenarios, including a category that might characterize the scenarios you can't think of. You at least have to account for them.

So I'm very much a student and a believer in this process of elevating this as high as you can. I agree with you. I think that I'm involved in something called generation-four planning. This is the next generation of nuclear facilities, nuclear reactors. And I think some of the things that are being done there are a very creative, and they are finally realizing that this is a much broader issue than a nuclear reactor. And I'm hopeful that it will trigger some of the very thought processes that your question stimulates.

WONG: Okay, I think we're out of time. Thank you, Dr. Garrick.

GARRICK: Thank you.

WONG: I turn the meeting back to Chairman Cohon, and remind everybody that this session will continue tomorrow morning with a presentation by Dr. Tonis Papp.

COHON: Thank you. Thank you, Jeff, for your fine job of chairing the session.

We have eight people who have signed up to comment at this time. I want to just go down the list and seek confirmation. Parvis Montazer? Jacob Paz, are you still here? Oh, okay. He's busy. Sally Devlin, Bob Williams, Judy Triechel, Ruth Widenheimer. I saw her. That's it, it's seven, not eight. And then a name, I apologize, I can't read it. Ms. Widenheimer, I haven't called you up yet. I was just confirming you were--

WIDENHEIMER: Well, I was just going to say I have some children with me.

COHON: That's correct. All right. Well, we're ready. The best I can do with the last name is something like Miranda, Miran. It starts with an M. Who signed up to comment but they've not heard their name called. Anybody? Okay. We're down to six.

This will be the ground rules, okay? I'm not going to cut you off, but at five minutes--please listen up.

In five minutes I'm going to raise my hand. And then every minute after that I'll raise my hand. Just to let you know that I'm still here and that we all want to get home at a decent time. So with that, let me start with the first one up. Parvis. And if you could state your name again for the record.

MONTAZER: Can I use the--

COHON: Of course. Do we still have the portable mike out? You want to bring it back up?

MONTAZER: My name is Parvis Montazer. I'm reporting on behalf of my county. I just wanted to give you a quick progress report on the preliminary evaluation of a naturally ventilator repository, and again I want to emphasize that this is a progress report and everything I'm talking about is preliminary.

I was supposed to give a full presentation and unfortunately, because of health reasons, I lost about a month worth of work in September so we have a report that is prepared, a preliminary report. It's scheduled to be released in--next month, early next month, February. And the final report is scheduled to be released in May before the next NWTRB meeting, we hope we're going to have the opportunity to present a full presentation at that time.

Of course, we're planning prior to presentation to the NWTRB present our--my county's viewpoint and suggestions to DOE. We have not had that chance this go-around and mainly because most of my planning has been in the past two or three weeks, so we'll give the whole report and presentation by May for everybody's benefit.

Our objective of the permanently-ventilated repository has always been, since 1995, my county has been studying this concept. Then to provide a cool and dry repository. In this particular case we're hoping to provide a way of allowing safe closure of the repository. Previous ventilation, actually ventilated repository was considering and continues to open a repository, which was not very well accepted. The acreage requirement is going to be met by reducing the temperature and of course, because of all of that the uncertainty will be reduced significantly as everybody has talked about all day today.

The basic bottom line system is, there will be a number of relatively large diameter area and have meteor diameter intake shafts, or I'm sorry, the drifts. And these will be eventually or at some certain point in time depending on the design situation, will be filled with rubble. Whether they can be constructed with rubble, the mining techniques themselves, those are--we're leaving that kind of aside. Basically all of these at some point in time will be filled with rubble, and these red tubes here indicate that basically the waste emplacement boreholes where the waste is going to be. In this conceptual design the waste more or less is going to be isolated from the ventilation system. Therefore, we believe that it will provide a repository that can be closed as well as providing a temperature relief.

In a simple cross section in these will be the ventilation drifts so that it will be eventually filled. And these will be in this case, this 2.5 meter diameter, we've taken as an initial and it's mainly to increase the stability, but it's not cast in stone and other aspects of the DOE design may change that.

This is a little bit of 3-D conceptualization of the same thing. There are added help for removing heat from the canisters. These are the emplacement boreholes, the canisters will be--are very conductive. They are mostly metal, and therefore we can take advantage of that in using heat sinks, carry part of that heat to the ventilation system, and the ventilation system can be provided with additional heat sink to improve the heat transfer between the rock and the air screen that is going through these ventilation systems that are going to be eventually filled with rubble.

At a cross-section of the western part of Yucca Mountain just pictorially I wanted to show how the overall systems would work. This will be ventilation. Air will be coming through this rubble filled hose and will be distributed both east, in the east/west direction as well as north/south direction by these north-south drifts. And will be taken up by a shaft. Again, all of these are going to be filled eventually with rubble. And in order to increase the elevation difference, we're proposing to put a chimney up there basically on the west side. The important thing in this whole concept is not to penetrate the PTN, and for two reasons. Number one, PTN is a protective system for hydrologic system. Number two is that PTN is not a very good stable competent lock to support an open--even if it is filled with rubble and will affect the longevity of the natural ventilation system.

I want to give you just a simple example where I've used about 250 years of pre-closure ventilation, which in this case I'm assuming that it's going to remove most of the heat. We balanced this with the previous simulations and we're going to verify that in this process with a 3-D simulation, like I said. And this is basically what DOE has presented in the PVR, except that in this case I'm using one canister. Basically this is half the loading of the fully loaded system.

The results are, these are again preliminary results. The--I have a profile along a ventilation shaft, I'm sorry, ventilation drift and each one of these ventilations showed are in between the two waste emplacement boreholes. This is the ventilation, the temperature remains at about 20 degrees C. And in this particular case I'm putting 18 degree C air system through the ventilation system. And this is across the borehole. The important thing is that about 20 meters above and below the temperatures maintained after about 720, 725 years is-- remains at 35 degrees. I have not run this simulation past 725 year, because I don't believe it's going to be much different than this.

This is a result of the same simulation in the cross-section. Basically these hot spots are the waste emplacement boreholes, and the blue spots are the drifts. And again we're going from 200 years. I'm just showing you the 200, 500 and some are 25 years after the original, initial installation of the borehole emplacement system.

This basically in summary we have tentatively concluded that for the cases that are considered for 50 percent heat load applied after 250 years of pre-closure period, the host rock temperatures can be kept below 60 degrees. Actually this is at the repository level. The area requirement may be reduced significantly from DOE's. In this particular case I calculated about 500 acres requirements. If you remember the HTOM requires about 1100 acres, so this is less than half of the HTOM requirement.

And the only drawback in this is that we need ventilation drifts spaced about 30 meters apart. That means that we need about 50 of those ventilation drifts going in the east/west direction. That's a construction issue and it is not necessarily overwhelming considering what is already planned, and considered. And we're working towards answering some of the questions that have been risen as far as our assumptions, etcetera, are concerned. And these are basically whether direct natural ventilation of the waste emplacement before we close the repository, basically if that assumption is correct, meaning that can I keep the heat load to basically nothing during the first 250 years? And also we wanted to consider this as an alternative whether it's possible to indefinitely ventilate the waste emplacement boreholes. In the initial base line design we're planning after a certain period of time to close those waste emplacement boreholes and that's when the heat load starts going up.

Also we have not incorporated the fractures, the role of fractures in the initial '95, '96 ventilation work. The practice played a major role in this particular case that I have shown we are not considering yet and I think that is going to add to the removal of heat considerably. And also we have not considered additional north/south drifts and how they might affect the temperature removal.

That's all I have and thank you for your patience.

COHON: Thank you, Parvis. He demonstrated a very useful technique. It's called avoiding eye contact with the moderator of the public session. You were good.

SPEAKER: (Inaudible) frantically.

COHON: Well done, Parvis. Thank you. And we do have the mike? Very good. We have accompanying Mrs. Widenheimer a couple of young people for whom hanging around for another 45 minutes to be very inconvenient. So I'm going to call on Mrs. Widenheimer now, and her one companion, or two, depending on how many want to come up.

WIDENHEIMER: Well, we've lost one.

COHON: Okay.

WIDENHEIMER: Could you identify yourself again, Mrs. Widenheimer for the record.

WIDENHEIMER: Yes. My name is Ruth Widenheimer--

COHON: Wait. Hang on. You need a mike. You can come over here where I am or you could go back over there. You see that mike right there? Okay.

WIDENHEIMER: My name is Ruth Widenheimer, former school teacher, and therefore I thought the best thing we could do--you don't want to hear me. I happen to get run over by two skateboards when I left earlier, and I said to the two young lads here, would you like to come and talk to the board. They said yes, they would. They went home and they both wrote a speech, and I've got one there. Maybe you can give him a hand and he'll come up anyway. They wrote them out and here they are, and so I present, and by the way, they are now on television at 7:00 o'clock tonight presenting their views along with the other skateboard kids. That's Channel 41, our own television station, and I'd like you to listen to what they have to say. And they have not been coached. This is Will and Shawn. Go ahead.

SHELDON: My name is Will Sheldon. The lady asked us if we'd want to do something about it and I said yeah. So I went home and I wrote a speech, and I wrote, I think it's wrong what people want to do to Yucca Mountain. I need to put--they need to put this nuclear waste where no one lives for at least on a 100 mile radius. Pahrump is like Las Vegas when it was little. It has a lot of potential to grow, so if they decide to store nuclear waste in Yucca Mountain like planned, it will affect the town majorly. In my thoughts I think if it does get stored here, people will leave this town. The people in the community waste their money on stuff to keep them safe so if something were to happen at Yucca Mountain they'd be okay. But if they didn't have to worry about Yucca Mountain we could take the money and put it back into the community for stuff that we need. For example, the movie theater is gone and us kids don't have any skate park or any other recreations for us.

And I've got one more thing to add. If they put nuclear waste in Yucca Mountain that is a terrorist attack waiting to happen.

WIDENHEIMER: Thank you very much. I can't upstage the kids. They always kind of beat you out at the polls, so I had probably one or two questions to ask you or thoughts to deliver. I've gone to your meetings for about four or five years now and I've heard a lot of the same things, and a lot of the same uncertainties, and I think the whole thing is a question of humanity. And so I'd like to ask, seeing that I am 76 years old and I've lived through all of this, I've lived through the country's storage of all this nuclear waste. It was necessary. We won a few wars having it, etcetera. But we've come to the point now where in your own literature you say we have enough materials to store right now for 1-3/4 Yucca Mountains. And that's the truth. 1-3/4? What are you going to do with the other 3/4 of the load. I've already, if you take the tour and you talk to some of the tour drivers, they say, "Oh, we're looking at that site right over there", and they point to a place about two or three miles up the road northwest of the original Yucca Mountain between the two homes that could maybe again catch fire again some day with--or spew out the lava.

Anyway, the point is I am saying this only to say to you please try to think of another approach. Don't put all your eggs in one basket. This is a question of the survival of humanity, in my estimation, and the quality of life. All you have to do is watch some of the nature programs and you'll see how intertwined all the life is. And you'll look at it and you count your blessings that you're alive today and that you can suck another breath of air. Please think of other ways, put the money out there and say to kids, "Here's money. Come up with the ideas." If you think money will get people to talk.

Anyway, good luck in your venture and we thank you all for coming here. This is a very important task you've taken on and I'm sure it weighs heavily on you shoulders. Thank you.

COHON: Thank you. And thank you, Will, for writing and reading your statement. Jacob Paz. Dr. Paz. I believe we have a document from you, yeah?

PAZ: My name is Dr. Jacob Paz. I was born Israel, make atomic bombs--and then by myself explode them at the Nevada test site. So I presented myself self-employed. First of all I'd like to thanks to the Board for their good review and a comment which they make in their presentation. I have certain uncertainty which I'd like to share with you, maybe through repetition, but very short and to the point. Yucca Mountain, in my opinion, is not just a radioactive site. There is a very good potential, probability of it to become a wrecker site, a mixed waste site. This concern has been brought to the attention of EPA and NRC and I'll just very briefly review it. EPA, when I raised the question the Yucca Mountain site will become a resource recovery at site as result of canister which the department of Energy plans to store. Quote so quote, they gave the authority to regulate it to the state. However, if you have requisite the law required you're going to do visibility study, or remedial investigation. You have to locate to do it now. Later it's going to be too late.

Second, there is all of the lawyers. A very serious legal question is like if the Board would look into the matter potentially if it's a requisite very clear in the regulations state that you cannot have a requisite in a seismic active region, and or a hundred years of flood zone. Progressively it has become a requisite when it's closure and subsequently it will become a mixed waste site. Those issues need to be addressed very clearly. There's an issue here where the dilution, which issue by EPA is in compliance or not. It's not my point.

The other point which I want to mention is, first of all for after long time of debating was they will look at Yucca Mountains, they agreed that under consider to take the issue of complex mixtures. I will read only two quotes.

Fist of all it's paper by Shuzuki, study of mixed radiation has progress, but was this the risk of environmental accident or space radiation which is often composed of one or more two types. The action of mixed radiation must be further investigated. We don't have information this point of time.

Second, the most important part is human protection of human life and the environment, and for some reason of not the effects of heavy metals has not been fully addressed in the environmental and other documents. If you have risk assessments and using probablistic risk you need data. You don't have the data at this point of time on complex mixtures. I hope I will change some of the people's position later on when it's published and when we have the data. Other issue which is associated is the migration of the rock and soil data. Heavy metals, when EPA approached passed the bucket to NRC. NRC stated we don't have regulation. I'm not going to play. This type of force cannot be done. It has to be slow. Who is responsible is the question. I have the document. I will provide it.

Other issue which is extremely concern to me is the progression of the Nevada Test Site risk assessment into Yucca Mountain. There is no boundary. A very serious issue is in transportation. All the bridges, and many of them the infrastructure in these are corroded and they are potentially serious for accident.

Thank you. I just want to tell you I will also supply some of my comments to the NRC and so on. Thank you.

COHON: Thank you, Dr. Paz. Next, Sally Devlin.

DEVLIN: Thank you, Dr. Cohon and Board. And there are a few people that didn't get my report on transportation and I have it here, so--but I of course want to thank all of you for being the best Pahritzers (phonetic) in Pahrump. And it has been a very, very long meeting and we really welcome you. We're so glad to see you and I promise you no cookies that will kill you.

I had several things to address on this. I just have to take exception--

SPEAKER: (Inaudible) closer to the mike.

DEVLIN: Oh, I'm sorry. I just have to take exception with USGS and Mr. Card (phonetic). Is he here?

SPEAKER: No.

DEVLIN: Well, anyway, I want this to go back to him. And the reason I'm saying it, he said our land here is worthless. Now, what earth science worth his salt would say the land is worthless? Remember that Yucca Mountain is part of the Bullfrog Range, and if you lived in Pahrump when the mine was open, you would have had one heck of a party every time they finished a million troy ounces of gold, and they wined and dined us every year. And I went up for three years, so what is in Yucca Mountain. What's in the cores you took out of Yucca Mountain? How much gold is there, Russ? Our land is not worthless. Tell him I said so.

And I'm just delighted to meet John Bartlett and John Garrick. And the reason I say that is, obviously they've been in sales because they used the terminology that I used for over 30 years. Your costs, risks and benefits. Now, the question is can we afford this, to load it up for 36 years and the second repository for another 36 years or more? That's number one. And what about the DOD stuff? And you know I will never trust anybody with DOD classified stuff, Abe. Did you hear me again?

My canisters, and I have to get into my favorite topic, which are my bugs. And I can't wait to see the Congressional report. I remember when colloids were first introduced to this group. And of course the bugs right around that. And what fascinates me about the bugs, and I'm very disappointed, because I asked you for $3 million several times for the study of my bugs for Dr. Amy (phonetic) at UNLV. And these funds were not forthcoming. Now I'm going to ask you for triple that amount of money. And the reason is until you test for my bugs at all 103 sites, and every place else that the DOD is, which we don't believe, because they won't tell us what they are putting in our mountain. I am very, very curious to find out because they are just like the fungus we're finding every day. And my bugs are multiplying. What are the bugs going to do to the canister? Of course, they love nickel. And I've given you all kinds of things on it. And when I got into the bugs, it was because at Hanford, they were in the salt and they dug a well that was 4500 feet down and they found my bugs that didn't need oxygen. And it goes on and on, 7000 feet under the sea bugs that eat a thousand rats. All kind of fun. So this has got to be a national process where every single one of these sites is really investigated for the bugs.

And to get back to Hanford, as we all know, in the water holes that are holding all the rods, my bugs ate the rods. And that is why that company got $800 million dollars and a billion-dollar bonus. That was in the GAO report I brought you. So there's lots of stuff and what's Hanford going to do with their stuff? From what I understand, put it in dry storage.

Now, we're talking not one but two repositories, 140,000 metric tons, and I'm going to talk about transportation and money tomorrow because I'd love to ask Lake Barrett for a trillion dollars, because that's what it would take to provide the transportation canisters and so on. That's only a third of our gross national product. But I do want Dr. Amy to get some money and I do want the rest of these sites to get money for testing the bugs.

I really feel very concerned about the word retrieveability, because when I left you in September, Abe and I were sitting up at the two repositories playing gin rummy and old maid and so on. And since our government is only responsible for 100 years, I don't know if we'll run out of cards in 200 to 225 years. So you're getting a picture again of assumed uncertainties, my favorite thing. And I really feel as the public that it isn't right for you to have assumed uncertainties. It affects us very deeply to our hearts. We feel that there are other methods, transmutation, moxing, what have you that this waste could be put to, and I think the 9.7 billion, or whatever the numbers are that the rate payers have paid, I get into the Price Anderson and what the nuclear power plants are supposed to have in reserve for accidents.

And of course, we all have one other thing to add today. And that's terrorism, sabotage, and so on. And I don't think there's anything anyone here from the Governor's office, but we just went into the interim legislative committee on home security, and I used my toastmaster's word for the day, and that was xenophobic. And that's what I accuse the State of Nevada of being. And I said that you will not look at the State of Iowa, total virtual medicine. Wisconsin with total virtual schools, and so on and so forth. And therefore, I say this state needs educating. We are number one in two things: Sex and smoking. We're at the bottom of the barrel with nursing. We have 42 nurses versus every place else that has 720. So you see where Nevada stands and I think I have to change that thing and we've got to wake the governor up, and we're working on it. So again, I'm saying we need virtual hospitals, and Russ, I want you to go and see Mr. Ness (phonetic) and ask him for the hundred million again. And we'll form a committee and we'll get virtual medicine here. We have no medicine in Pahrump. So, please, everybody have a good, safe dinner, and enjoy it. And again, thank you so much for coming. We'll see you tomorrow.

COHON: Thank you, Sally. Thank you, Sally. Bob Williams.

WILLIAMS: I would like to use the podium as well.

COHON: By all means.

WILLIAMS: So I can look you all in the eyes.

I'm Bob Williams. I'm retired from EPRI eight years now. A lot has changed in those eight years, but a lot remains the same. I periodically ask myself why I'm here today. I think part of the reason is after five years of not missing a nuclear waste technical review board meeting, I'm addicted. I occasionally need that fix.

The other part of it is I really do give a damn.

So I'm here to give you some hopefully helpful advice. Hopefully, not offensive because I offer it in the spirit of being constructive.

I really got mad when I read your report on the web, your January 24th report. I compliment the staff for getting it on the web the same day it was issued, but there was some congressman who said, dammit, give me a one-handed scientist. I am tired of, on the one hand and on the other hand, from scientists. Your report struck me as too many both-handed comments. I'll get into that a little more later.

But there is no sense here that this is war; that anything has changed to change the way we approach nuclear waste disposal or that there is any more urgency than there was a year ago or ten years or 20 years ago.

One of the main underlying reasons as I thought about it for the last four days is that you fellows have mastered the art of Beltway-speak, or Washington-speak. You are so used to talking in code and talking in legalisms that I don't think some of your reports really communicate. Now, let me give you an example of what would be a plain statement. This is my basic and prior, based on watching this program. I think there is about a one percent chance of success in licensing in 10 years. I think there is perhaps a 10 percent chance of success in licensing in 20 years, with the current design as it is. Now that would be telling it like it is. You may have different perspectives. I think there is about a 90 percent chance with a vitrified waste form, particularly a low temperature vitrified waste form such as substantially purified. The term of art is partitioned waste such as might be produced at Savannah River, is being produced at Savannah River, might be produced at Hanford.

So I keep asking myself why did our carefully crafted process fail to work? Why did we fail to converge on a workable and licensable design? And I'll try to answer that rhetorically in just a moment.

My third point is I have some free and hopefully constructive advice for Steve Frishman. I think in the spirit of being plain-speaking, and Steve and I have known each other for 20 years, I think, at least, please don't hang your legal argument on this, Jeff (phonetic), for your argument.

Between 1975 and 1980 a number of different analyses were done that basically said you needed a reduction in the hazard, the ingestion hazard of waste on the order of 17 orders of magnitude. And the studies show that the geology would only accomplish 10 or 12 orders of magnitude. Those are published in the proceedings of the Tucson conference back in the early years. I might even still be able to find one in my files.

So the point is the congress was well informed that they needed a multi-barrier system and that the geosphere by itself was not adequate. So I'm confident that you can mount a legal attack that will tie us up for five or 10 years, but please do it over something important, not something that's such a bogus issue as that.

Now, the next part is that the reason you want the waste package to work for a while is that radioactive decay basically gets things down to where you only need 10 or 12 orders of magnitude of protection, and that can be accomplished by the geology.

Now, my fourth point relates to strategy. The strategy is flawed a dozen different ways. I will only highlight a couple or three of them. Earlier speakers have said we need a simple strategy and we need a simple explanation. I think most of the people in the room would say we have neither.

Now, as part of my method of speaking plainly, let's lay it out on the table like it really is. In some situations you have a course of action called A, which is perfectly viable, and a course of action called B, which is also viable. But a compromise in the middle, A-B, which is not viable.

Now, I got this insight from my work in mental health, volunteer work in mental health where somebody pointed out nobody in their right mind would structure a mental health system the way ours is structured. But then they thought it out, that it is the result of a terrible political compromise, that we do only the things that the parties could agree upon. So Nevada was what the parties could agree upon in 1987,

Thank you. I'm trying to accelerate.

The pro nuclear crowd had so much technical arrogance, so vituberous, that they figured, hey, we can license a site any place. The anti-nuclear were equally shrewd. They said go ahead and work to your heart's content. There's no way, with all the technical problems at Yucca Mountain you'll ever succeed. So somebody like the technical review board needs to stand up and say we are working on a particularly difficult site. We have political advantages that permitted us to go to work, but we have some other advantages that are becoming more and more evident.

Now, I ask again, why did the process run amuck with respect to the waste package? The concept was that it was too robust and easy to prove. In my view, the whole thing has gone awry. Any of you who would take the trouble and go back and read the licensing analysis for KBS-2 would immediately discover that there is a succinct easy-to-understand reason why the waste package will last a million years in the Swedish groundwater. For me with a chemistry background it's easiest to speak in terms of buffering the granite, then buffer the glass in such a way that it won't corrode. The groundwater is such an EH/PH regime, with copper, which is hot ice and statically pressed around the fuel, will not permit the fuel to be accessed.

Now, I know because I personally worked on part of the design of the multi-purpose canister that there are $46 billion dollars in the program for waste package, and that's before you add three more billion or something for the drip shields. So there's plenty of money to go to an oxide waste form. An oxide waste form is what doesn't get oxidized when you're in a oxidizing environment, like Yucca Mountain. Glasses are made out of metal oxides.

Now, where does this lead me? Why has the process run amuck on licensing? Well, the cultural change that's being talked about here is the least of our worries, in my humble opinion. My lesson learned from a life time of experience in the licensing arena is don't start with a design that you intend to change. You get 800 people working and you start making major changes, you'll get tied up in your socks. The NRC will never know what report, what drawing, what design they should be working to. The reason you do advance design and the reason you have a preliminary phase is so that you get a small group of people that can rapidly complete the iterations and then proceed to turn it over to the force of 800 or 1000.

So there is a major disaster that will result, first from that and second from the long time frame.

Now, as one example, there is a forgetting function that I happen to have insight into. It's this flooring in the teflon. EPRI got burned in a joint program with DOV in 1984-85 because teflon came out in a joint project we were running with Batelle (phonetic). We had 3/4 of a million in it, Batelle had 3/4 of a million in it. And the leaching of fuel was all screwed up by the flooring that came out of the teflon.

Now the MCC program which was a multi-laboratory program, now a Catholic University had a big role, also got burned by the flooring coming out of the teflon. Now, why didn't the peer review process pick this up? Well, it's just impossible over a long time frame, over a 10-year period for people not to make mistakes like that. So we're headed for disaster by embarking on a licensing program that's going to run over 10 or 20 years.

Time scale is too long to efficiently manage. Now, one of the things I think about is the third lesson that EPRI learned. My first contact with John Bartlett is he was my surrogate regulator. EPRI had a two-part contract beginning in 1979 that had SAIC, people like Larry

Richardson and Bob Bullen as the DOE design team, and John Bartlett and the Analytic Sciences Corporation as my pseudo regulator. I think the DOE needs to consider doing that so they get some straight-ahead stiffening of what the regulators are likely to say.

I'm getting very close to the end.

Now, I encourage each and every one of you, even if you have to do it individually, to get rid of the Beltway-speak, even if you have to write it on your personal stationary and draft a resignation letter from the TRB that goes with it. But in the course of that letter, I'm not asking anybody to fall on their spear or fall on their sword, If you do it right you have both the prestige and the forum to structure a vehicle for political compromise.

Now, I jump ahead to say that there is too much talk about risk analysis and not enough talk about decision analysis. Many of you who are experts here, and I don't know precisely who the decision analysis experts are, realize that. The political compromise in a nutshell is to say if we were to go over the defense waste repository at Yucca Mountain, we might be able to license it in five years. It would be the cold repository we've talked about. It would have a glass waste floor. Those of you who have insight into the process maybe knows that there's some problems there. You know, I'm tempted to write that. I used to have my hands on all the levers and could quote chapter and verse on virtually everything. You folks are in a better position that I am to do that.

Now, another concept that got lost in the shuffle is "compared to what?" The compromise that happened in the 1987 policy act was that we took out alternatives. So I would like to charge you folks with taking the bit in your teeth. You are this all--panel which is supposed to advise the President, advise the Secretary and compare the ease of licensing Yucca Mountain to some surrogate repository that's been--like one of the KBS designs. I bet you could talk the Swedes into doing that.

One thing I have to alert you to is the licensing criteria that cuts off at 10,000 years. There is no way that the licensing process, given that the rest of the world looks beyond 10,000 years, that this one can cut off at 10,000.

Now, why should there be a political compromise? Why would Steve negotiate with you? He's got you by the short hairs. Well, some unforeseen event may force progress. Somebody might come in that, you know, we talk about lying awake nights after 9-11. The thing I lay awake is thinking that 98 percent of the containerized cargo comes into the United States without inspection. Many ships come into the United States without any inspection. So a nuclear weapon could just as well have been at New York as well as airplanes crashing into the Trade Center.

Well, my concluding remark, again, is why didn't the process work? Why didn't we come up with a better design like a heat-seeking missile that hones in on the easily licensed, readily licensable solution? Well, the blame is not totally that of the Nuclear Waste Technical Review Board, but I think now is the time for you to not be bound so much by your charter, but to sit down and be plain spoken about what needs to be done. And I think the whole business of an override in Congress would go better if the DOE and the program had made some attempt, making a political compromise with the State of Nevada. In other words, go in and see if they would accept the idea of accepting the defense waste canisters which are so much more benign than the spent fuel. Part of the horse trading would be to go find a new site for a repository. If the earlier speaker is right, that we have enough waste for one and a half or two repositories, and I think we go with the DOE system, then that's the compromise. We then stand up to the public and say we've got--for true waste, we've got Yucca Mountain working for glass logs (phonetic), and we're about to have X, Y, Z working for spent fuel.

Thank you.

COHON: Thank you, Bob. And thank you for not putting the entire blame on--at the feet of the Nuclear Waste Technical Review Board. We appreciate that.

Judy Triechel?

TRIECHEL: This is Judy Triechel of Nevada Nuclear Waste Task Force, and this is really cool because I'm not really sure I'm going to see--oh, okay.

Okay, all I have are four view graphs, and they are just statements. This one comes out of a new set of information sheets that comes out of headquarters, and it's on the Energy.com web site instead of the YMP.gov, or Energy.Gov instead of YMP. And it says volcanism resulted in a low but calculable dose when considering how the low probability of a volcanic eruption. The likelihood of the repository being disrupted by igneous intrusion is extremely small, about one in 70 million per year. And the big deal here is the calculated peak dose would be less than one percent of the NRC and EPA radiation protection standards. And it's out of a section called commonly raised topics.

Here's the second one out of that same set of information sheets. Groundwater systems in the Las Vegas Valley, Pahrump and the Amargosa Valley are not connected. Yucca Mountain is located in the Death Valley hydrologic basin. The boundaries of the Death Valley Hydrologic Basin, in which the repository would be located, are defined and understood. Water in this basin does not flow into any rivers or oceans and is isolated from the aquifer systems of Las Vegas and Pahrump.

I don't think that those are dishonest, but I think they are very misleading. This leads you to believe that the water system is not around anyone. And when you couple that, the next two quotes, the last two view graphs are from a tour guide on a Yucca Mountain tour. Even if water carries radioactive waste away from the mountain, he said, the local watershed stops far before any residential area or waterway. And the same guide, as for earthquakes, they are primarily surface phenomena. Well, you wouldn't be seeing the fault lines down in the repository if in fact earthquakes were just primarily surface phenomena.

COHON: That's it?

TRIECHEL: No. I know you're weary. I know you're bleary, but that's not it. Those are the examples I want to use. That tour guide at Yucca Mountain took journalists and has taken many journalists, and the journalist that I got those from writes for Cox (phonetic) News, so there were articles printed all over the country with those statements. And they are very misleading. Now, the Board is charged with the technical validity of the scientific work, the technical validity of the work that DOE is doing, the reports that they put out. And those reports are supposed to, according to your charge, be defensible to the scientific community and understandable to the general public. And it seems to me that the first rule of technical validity is accuracy. And as far as the public is concerned, accuracy means honesty. And the examples in those statements are stated without any sort of uncertainty. They are just plain facts that this water never goes to anybody and that those basins don't have anything to do with anywhere that people are. And that's absolutely misleading. And those shouldn't be out there. And we met with two journalists who had been to the mountain and they'd been told to pull those fact sheets off of the web before they went so that they could read some stuff.

And earlier, I think when you, Dr. Cohon, were talking about the letter report that you had put out, you said that decisions will be made, policy makers will decide the acceptability of the amount of uncertainty. But how do they do that? Policy makers do not read these reports. They don't do as John Bartlett did. They don't do as John Garrick did. They take a tour, they listen to a tour guide and they read some information sheets. And that's why this country--I've been getting e-mail messages and phone calls from people all over the country. I guess the nuke industry is out there on a rant, and submitting editorials that are coming in with this absolute certainty, Yucca Mountain is completely perfect for nuclear waste. There's nobody around, there's no water, nothing happens, and so it appears that this kind of information is going out to people. And there are only two kinds of information or those sorts of fact sheets, and a trip to Yucca Mountain with somebody who is telling them this stuff, it comes out wrong.

One of the things that Steve was alluding to when he spoke earlier and I heard it again during the day was that Yucca Mountain becomes safe because it's so needed. And Bob Williams was kind of talking that way a little bit. But that's not true. It doesn't matter how much you need or you want to have a place for nuclear waste. It doesn't make Yucca Mountain get any better.

There were also two more statements that were in an article recently by a person who formerly worked for the NRC and seems to have come to his senses. But he said the unknowable can be stated with certainty. That's what we saw in these things. These are very uncertain things, and they are being stated with absolute certainty. And this is being sold as a chain that's as strong as its strongest link, which of course is the canister. But I just really think that within your charge you can direct the DOE to be accurate And I think they need to be accurate when it comes to the people who do not read the reports and who only rely on the stuff that they see. And it's out there.

Thank you.

COHON: Thank you, Judy. Is there anybody else who cares to comment at this time?

(No response.)

COHON: Let me close the meeting with two sort of partial responses or reminders about what the Board is and what the Board isn't. And I'm reacting in particular to some of the things that Bob Williams said and one thing that Judy just said.

The Board has a congressional mandate. Like it or not, it is what it is. And it's very clear as to what it is. And the line that separates what the Board can do and should do from what it shouldn't, which is to say policy, is a very clearly bright--clearly drawn bright line. And the Board is well aware of it.

The other issue is I think that Judy, the Board feels like it has played a role of insisting or strongly encouraging the DOE to be accurate and to be comprehensive. For us, again, respecting that line that separates the technical from the policy, for us the focus has been on strongly conveying the importance of quantifying uncertainty, and conveying it in a meaningful way. That has always been part of our statement. We don't just say quantify. We say convey it in a meaningful way.

Our focus has been on national decision makers, but you raise a good point about how--well, I'm inferring a lot from what you said. There are decision makers everywhere. There are people who influence opinions everywhere. And the input that they receive is also very important, so your point is well taken.

We will adjourn for the evening. Now, let me remind you that at 7:30 in this room, one hour before the start of the formal meeting, we will serve up breakfast. And the Board, and you are more than welcome to join us in that informal setting. My thanks to everybody for their participation today.

(Whereupon, at 6:35 p.m., the meeting was adjourned.)

REPORTER'S CERTIFICATE

MEETING NAME: NUCLEAR WASTE TECHNICAL REVIEW BOARD
WINTER 2002 BOARD MEETING
LOCATION: PAHRUMP, NEVADA
DATE: JANUARY 29, 2002

This is to certify that the above transcript is a true and accurate record of the aforementioned meeting which was electronically recorded and transcribed under my direct supervision.

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