UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FALL 2002 BOARD MEETING

September 10, 2002

Alexis Park Hotel
375 E. Harmon Avenue
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen
Dr. Thure Cerling
Dr. Norman Christensen, Afternoon Session Chair
Dr. Michael Corradini, Chair, NWTRB
Dr. Paul P. Craig, Morning Session Chair
Dr. David Duquette
Dr. Debra S. Knopman
Dr. Ronald Latanision
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek

SENIOR PROFESSIONAL STAFF

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

CONSULTANTS

Dr. Jared L. Cohon
Dr. Donald Runnells
Dr. Alberto Sagüés
Dr. Jeffrey J. Wong

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Karyn Severson, Director, External Affairs
Linda Hiatt, Management Analyst
Linda Coultry, Management Assistant
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P R O C E E D I N G S

CORRADINI: Let's all sit down and begin. Thank you.

Good morning. My name is Mike Corradini. I'm Chair of the Nuclear Waste Technical Review Board, and it's a pleasure to welcome you all to the third meeting of the Board this year.

Before we begin the meeting, it seems important to me at lease to pause a moment to acknowledge the tragedy of our nation experienced a year ago tomorrow. Many of you who are here today were attending a Board meeting in Las Vegas last September 11th. While we're together again near the anniversary, let's join in a minute of silent in remembrance of all who were touched by those tragic events.

(Moment of silence.)

CORRADINI: Thank you. I'm a new face to many of you, as are four of the other new Board members you'll see sitting at the table. A lot has been happening with respect to Yucca Mountain since the last Board meeting in Washington in May, and I'll have more to say about that after I introduce the Board, its continuing members, the new members, and its departing members.

Let me first give you a brief background of the

Board itself. Our Board was created in 1987 amendments to the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of the activities of the Department of Energy related to the disposal of commercial spent nuclear fuel, and defense high level nuclear waste.

The Board is required to report its findings and recommendations twice a year to Congress and to the Secretary of Energy. The President appoints Board members from a list of nominees submitted by the National Academy of Sciences. The Board is, by law and design, a multi-disciplinary group composed of eleven members with expertise covering a wide range of disciplines.

Let me now introduce the members of the Board to you. As I introduce the Board members, I'd like each one of them, ask them to stand briefly and be identified. Let me remind you also that we all serve in a part-time capacity. In my case, I'm Chairman of the Department of Engineering and Physics at the University of Wisconsin, Madison. My areas of expertise relate to nuclear and industrial safety, with emphasis on subjects such as multi-phase flow and heat transfer and mass transfer.

First, I'd like to introduce the continuing members of the Board, who I am sure many of you know. Dan Bullen is an Associate Professor of Mechanical Engineering at Iowa

State University. His areas of expertise include performance assessment, modelling and materials science. Dan chairs both our Panel on Performance Assessment and the Panel on the Repository.

Norm Christensen is a Professor of Ecology and former Dean of the Nicholas School of Environment at Duke University. His areas of expertise include biology, ecology and ecosystem management. Norm chairs the Board's Panel on Waste Management System.

Paul Craig is Professor Emeritus of Engineering at the University of California at Davis, and he's a member of the University's graduate group in ecology. His areas of expertise include energy policy issues associated with global environmental change.

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

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

And Richard Parizek is a Professor of Geology and Geoenvironmental Engineering at Penn State University. He's also President of Richard Parizek and Associates, Consulting Hydrogeologists and Environmental Geologists. His areas of expertise include hydrogeology and environmental geology.

I'd now like to introduce the new Board members. Mark Abkowitz is Professor of Civil Engineering and Management Technology at Vanderbilt University in Nashville, and he's Director of the Vanderbilt Center for Environmental Management Studies. His expertise is in the areas of transportation, risk management, and risk assessment.

Thure Cerling is a Distinguished Professor of Geology and Geophysics and Distinguished Professor of Biology at the University of Utah in Salt Lake City. He is a geochemist with particular expertise in applying geochemistry to a wide range of geologic, climatological, and anthropological studies.

David Duquette is Department Head and Professor of Materials Engineering at Rensselaer Polytechnic Institute in Troy, New York. His expertise is in physical, chemical, and mechanical properties of metals and alloys, with special emphasis on environmental interactions.

And Ron Latanision is a Professor of Materials Science, Professor of Nuclear Engineering and Director of the H.H. Ulig Corrosions Laboratory at MIT. His areas of expertise include materials processing, corrosion of metals, and other materials in different aqueous environments. Ron is also Founder and Chairman of the MIT Council on Primary and Secondary Education.

I'm sure all of you will get to know me and the other Board members during the course of the time, and we look forward to meeting all of you. In fact, I think, an extemporaneous note, there's a lot of you, so I think it will take a bit of time.

I would now like to introduce the departing Board members, and in doing so, say a few words about the role of the Board. These departing members, those that continue to serve, and John Arendt, who passed away last April, have played an invaluable role in the site recommendation process that has just ended. They did exactly what Congress requested, that is, comment on the scientific and technical validity of the Department of Energy's nuclear waste disposal program.

It's no secret that different players, both national and local, were not always completely pleased with what the Board said. But the Board's purpose is to objectively evaluate the technical basis of that program, and that is what the Board did, and we hope what it will continue to do in the future. I believe the nation owes a great deal to these individuals and their contributions over the past few years. So, at this point, what I'd like to do is request the departing members to briefly stand as I mention their names, and afford them an opportunity to say a few words, if they so choose. Outgoing Board Chairman Jared Cohon will then wrap up with some of his own remarks.

Donald Runnells is a Professor Emeritus in the Department of Geological Sciences at the University of Colorado at Boulder. He is also a technical consultant at Shepherd Miller Environmental and Engineering Consultants. His area of expertise includes geochemistry, hydrochemistry, and mineral deposits.

Don, anything you would like to say?

RUNNELLS: Just very, very briefly. I must thank the staff of the Board. They are, as Jerry Cohon often says, in all of their splendor along this wall. They have made my job, and the jobs of the other members of the Board, much easier than it would be otherwise. They work hard. An excellent staff, and I thank them very much.

I also want to say that the Board recognizes the very hard work of the technical people of the DOE and their contractors. We may not always agree with the direction or the results, but I want the folks in the trenches who are doing the technical work to know that we recognize their commitment, their devotion, and their hard work.

Thank you very much.

CORRADINI: Thank you, Don.

Alberto Sagüés is a 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.

Alberto, is there anything you want to say?

SAGÜÉS: Yes, indeed. Thank you. First, I want to echo somewhat the comments of Don. It's been an honor and a privilege to have served the nation as a Board member over the last five and a half years.

During that time, I was encouraged by the caliber and the dedication of the Yucca Mountain project scientists and engineers who address the unprecedented challenge of planning for a system that concerns public safety for many future generations. I'm very glad to feel that I, among others, had a part in making a possible contribution as a reviewer, in particular, on the importance of basic science and extrapolated materials behavior into the far future.

Of course, I want to express my great pleasure to have worked with such a wonderful group of colleagues, Board members, and staff. Thank you very much.

CORRADINI: Thank you, Alberto.

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

Jeff, I think you have some words for us.

WONG: Thank you. Yeah, like my colleagues, I'd like to say that the descriptors I have for the Board staff are brilliant, efficient, the best. I'd like to thank them for all the support they've given, and it's been a pleasure.

To the DOE, I want to thank all of them for all of their efforts in trying to explain their activities, and in particular, I'd like to thank Claudia Newberry, who's been a face that we've always been--or at least myself has always been in contact with.

I'd like to thank Steve Frishman and Englebrecht Von Tiesenhausen. They, in particular, held me to not only ask some of the real questions, but always looked for some of the real answers in the time that I've been here.

To the new Board members, I'd like to offer my congratulations once again. I hope you get to have some of those fine cookies from Sally Devlin and her friends in Pahrump. And also want to point out to you, according to Bo Bodvarsson, the worst questions, all of them, are very good questions. It's been an honor, and I thank you.

CORRADINI: Thanks, Jeff.

And, finally, there's one departing Board member who I have not yet introduced, and that is Jerry Cohon, who has been Board chairman since January 1997. As a scientist, Jerry brought to the Board expertise in environmental and water resources systems analysis. As Board Chairman, he led the Board through its many deliberations, interacted with the groups, such as the Congress, the DOE, the NRC, and the State of Nevada, the affected counties, as well as the general public. He did most of this while serving as President of Carnegie-Mellon University, a major American educational institution.

Staff members told me that when Jerry went from being Dean at Yale to President at Carnegie-Mellon, many of them wondered how long he would remain on the Board, given the work load associated with his new job. The pessimists talked about a month or two. The optimists talked about a year or so. His tenure lasted five and one-half years, a true testament, I think, to his abilities.

Jerry, thank you for your leadership during this critical time in the Board's history. I'm sure that in your new off-duty assignment, as I am told, as member of the President's Homeland Security Advisory Council, you will continue to serve our country in this critical area with the same wisdom you displayed on the NWTRB.

I should say again parenthetically, since a lot of these remarks were provided to me, I've only been on the job, so to speak, in my part-time capacity for about a month, and Jerry has been excellent. We have had a number of conversations on the phone. I thank him personally for that.

Jerry?

COHON: Thank you very much for those kind remarks. I wasn't aware of what Mike just told you all about what the money was saying about my longevity in this position. And had I known, I could have offset some of my blackjack losses over these many years.

Thank you, too, Mike, for giving me time during what is a very, very full agenda today, and I'll try to be to the point, recognizing that a former chairman is about as valuable to the current Board as yesterday's newspaper.

And while I'm on thank you's, I want to join my colleagues, the other outgoing members, in taking the opportunity to thank all of the many people who have had such an impact on this Board and on me personally during my time as chairman, and people who have made my time as chairman such a rewarding experience. I want to thank the other Board members with whom it truly has been a privilege to serve, the Board's staff--a remarkably talented and technically deep group--and especially Bill Barnard, who has done a superb job of managing the Board's affairs. And I have to say more than anybody, he's the one who made it possible for a university president to serve as chairman for five years.

My thanks to all with whom the Board has interacted over the last five years, and especially the DOE and its contractors. I want to again join my colleague outgoing members in saying you are very strong professionals and very good people, and I believe you work very hard to do the right thing.

In the remainder of my remarks, I want to touch on three themes: what the Board has done over the last five years during my chairmanship, how it did it, and a little bit on what it might do in the future.

Mike's characterization of the Board I think was just right: to provide an independent and objective evaluation of the science and technology in DOE's plans for Yucca Mountain. And we've this very well. That we had everybody made at us at one time or another, and sometimes everyone at once, may be the best measure of our success.

The Board's January 24th letter report commenting on the Secretary's site recommendation was the culmination of years of effort by the Board. That report was important, it was hard to do right, but it was very well done and received the attention that it deserved during the Congressional deliberations. But, in fact, and I want to emphasize this, it was only the culmination. It is by no means the totality of all that the Board has contributed to the Nation's nuclear waste program.

I have to say this Board understandably has been reticent about celebrating its accomplishments and contributions. We are, after all, a bunch of scientists and also engineers who are naturally suspicious of self-promotion. And I respect that, but as a former member, I don't feel so constrained, so I'm going to do some celebrating. I'm not going to try to be exhaustive. I just want to point out a few Board contributions that I think were especially important.

First, there is the ECRB, which was called the cross-drift when it was being excavated and, before that, when the idea was still being resisted by the DOE, it was called the "Board's Tunnel." "Our Tunnel" has yielded crucial information about the proposed repository block and I feel completely confident predicting there will be even more to come, and I think much more.

Secondly, as disappointed as I was personally in the Secretary's treatment of uncertainty in his site recommendation, the DOE in fact is much further along on this issue than they were just two years ago. Though I tip my hat to DOE for the good work they've done on this, the Board played a big role in pushing them to do it, and I hope the Board will continue to do so.

The third one is a subtle point, but I believe that the Board's insistence that suitability, which is an admittedly poorly defined term, nevertheless, suitability was different from licensability, and pushing that represented I think a very important contribution to the process. By insisting on this distinction and arguing exhaustively about its meaning among ourselves, we helped to preserve the site recommendation as the important social and policy decision that it was.

Fourth. Proving something not to be true is the hardest thing to do in science, and a decidedly unglamorous undertaking. Yet, the Board did not shy away from the challenge presented by the hypothesis of geothermal upwelling. I think we did a very effective job in marshalling limited resources and helping to spawn reviews of what was a very complicated and controversial issue.

Fifth. Only time will tell if the DOE ultimately settles on a hot or a cold design. But without a doubt, if it were not for the Board, the hot design would have gone forward unchallenged. Whichever repository design is ultimately selected, it will be better understood and probably more robust and resilient than it would have been without our aggressive questioning.

And, finally, on my short list, the last one I want to mention is one that's particularly important to me. I am proud of the Board's openness and the care with which we manage the public's role in our activities. Most notable has been the public forum that we have provided at our meetings. We have done that whenever and wherever we've met, in Washington, here in Las Vegas, and, importantly, in the towns closer to Yucca Mountain. And, wherever we've gone, we've brought along DOE and the other major players with us.

There's more that we've done, much more, in fact, but let me turn now to my second theme, which is how we accomplished all of these things, and the many more that I haven't mentioned. There are many ingredients of the Board's success: the outstanding quality of its members, the superb staff, the hard work and the care applied to the tasks at hand. These have all been crucial, but I want to focus on the Board's integrity. Created, as we were, to provide independent and objective advice, our integrity as a Board has been essential to our effectiveness.

Now, I have to admit that integrity comes in many forms. It includes having members free of conflicts, both real and perceived, and this is something that we've been very careful about.

But integrity also means intellectual honesty. And on this score, I am in awe of what we were able to do. For a moment, I ask you all to put yourselves in our place. Here we were, eleven people from completely different backgrounds, differences in training, experience, world view, charged with producing a single position on highly complicated issues filled with uncertainty. Producing these positions in a way that took account of and respected everyone's views, while still having something worthwhile to say, is not easy. I challenge you to try it. But, we did it, and I believe the key to this was the intellectual honesty each of us brought to the task and the resulting trust in each other that developed over the years.

Turning now to my last theme, the future of the Board, I defer to Mike and the Board's current members on the important question of the Board's role after the site recommendation. I can say with confidence, however, that there will be no less need for the Board's excellent, intellectually honest and independent review of the nuclear waste program than there has been in the past. Indeed, it makes sense to me that the closer we get to repository construction and operation, the more important are sound science and credible science and sound engineering and credible engineering.

Achieving soundness and credibility will require a solid conceptual understanding of the repository system and supporting data and supporting analyses. Furthermore, understanding uncertainties, communicating them fully and developing plans for dealing with them are crucial. All of these issues are on the Board's beat, and I expect that, as important as the Board has been up to now, it will be even more so in the future.

I think we all agree that Yucca Mountain is still a highly complicated and technically challenging program and will remain so. If the Board continues to perform as it has in the past--and I'm sure it will--the Nation will be well served. And, you can't ask for more than that.

Thank you very much.

(Whereupon, Dr. Cohon received a standing ovation.)

CORRADINI: Thank you, Jerry.

Before we go on to today's agenda, let me describe where the Nation finds itself with respect to the proposed repository for nuclear waste.

As you no doubt all know, earlier this year, Secretary of Energy, Spencer Abraham, and then President Bush, both recommended the Yucca Mountain site as the site for a permanent high-level waste repository. Governor Kenny Guinn of Nevada then notified Congress of the State's disapproval of the President's recommendation, and on July 9th, the Congress overturned the Governor's veto, and on July 23rd, the President signed a joint resolution of Congress recognizing this.

The formal site recommendation process now is over, and Yucca Mountain has been officially designated, authorizing the Department of Energy to submit an application to the U.S. NRC to obtain a license to construct a repository at the site.

Undoubtedly, this is an important juncture in the life of the Yucca Mountain project. We're very interested in how the DOE will meet this new challenge, and continue to address the ongoing technical and scientific issues that the Board is concerned about.

Let me now turn to our meeting agenda. I'll try to be brief, because we're already slipping a bit past our time, because we have a very ambitious agenda and a lot of ground to cover today. Right after I'm done, we'll start with Bob Loux, Executive Director of the State of Nevada Agency for Nuclear Projects, who will present the views of the State related to the proposed repository at Yucca Mountain. Mr. Loux is responsible for the staffing, organization and direction of the State Nuclear Projects Office.

We will then have an overview of the current activities of the Office of Civilian Radioactive Waste Management by Margaret Chu, the Director of the Office. The Board first met Dr. Chu at the May 2002 meeting in Washington. At that time, she discussed some of the long-term goals that she has for the office and the program. We're looking forward to hearing some of the details associated with these goals.

Following Dr. Chu's presentation, Don Horton, Deputy Project Manager of the Yucca Mountain Site

Characterization Office will present an overview of the project activities, including long-range plans and project priorities for science and engineering activities. The Yucca Mountain Site Characterization Office is responsible for the study of Yucca Mountain as a potential repository site.

We'll then take a short break, and Board Member Paul Craig will chair a session extending to the mid-afternoon that will cover a number of scientific and engineering topics, including ongoing and planned testing, igneous consequences, thermal issues, corrosion, design, and a report of Inyo County, California on their hydrologic studies. Paul will review for you the agenda for his session in greater detail later this morning.

At mid-afternoon, after a break, Board Member Norm Christensen will chair the final session of the day, and as he will explain, that session will be devoted to comparing different performance assessments and barrier analyses of the proposed Yucca Mountain repository. And that bring us to the close of this very ambitious one day agenda.

So, let me just say a few more words about public comment and the ground rules of our meeting. We have scheduled our public comment period at the end of the meeting in the late afternoon. Those wanting to comment should sign the public comment register at the check-in table in the back where Ms. Linda Hyatt and Linda Coultry are seated, and they will be happy to assist you.

If someone wants to comment and absolutely cannot stay until the comment period at the end of the meeting, please let us know and we will try to accommodate you at the close of the morning session. I understand a couple of people have already talked with Linda Hyatt.

Let me point out, and I'll remind you again later, that depending on the number of people who sign up for comment, we may have to limit the length of time you have to make your comments during the comment period.

As always, we welcome written comments to the Board for the record. Those of you who prefer not to make oral comments or ask questions during the meeting may choose written route at any time. We especially encourage written comments where they're more extensive, and our meeting time may not allow them to be spoken orally.

And then, finally, I have to offer one usual disclaimer for the record so that everybody is clear on the conduct of our meeting, and what you're hearing and the significance of what you're hearing. Our meetings are spontaneous by design. In fact, as an aside, this might be the most choreographed part of my life. I've never had such a long speech. Those of you who have attended our meetings before, know that the Board members do not hesitate to speak their minds, and I think the new Board members will be of similar vein, but I have to emphasize that that's precisely what they're doing when they're speaking. They're speaking on behalf of themselves, not on behalf of the Board. When we are articulating a Board position, we'll be sure to let you know. You can find the final Board position in our written letters and reports, which can be accessed through the Board's website.

So, with that, we can begin. Let me start by introducing Bob Loux, Executive Director of the State of Nevada's Agency for Nuclear Projects. I was looking for Bob. He will present his views of the State related to the proposed nuclear waste repository.

LOUX: Thank you very much. I will be very brief this morning. I know you have a full agenda, and a lot of things to cover.

I want to take the opportunity to welcome all of you to Nevada officially on behalf of the State and the Governor and, in particular, welcome the new Board members. We look forward to working with you. I know that you believe and know there's a number of challenges ahead that we'll all be working on together. I wish you the best, and want to offer any way that we can be helpful or be of service, please call on us and let us know.

I in particular want to thank and congratulate the outgoing Board members for their long and hard work in this arena. As the past Chairman has indicated, it's not an easy arena. It's full of a lot of controversy, a lot of delicate subjects. And I think the Board has handled themselves, in particular, very well in dealing with these.

We may not have always agreed on all of the issues, but I think that one thing that we, and I know the public, could count on is the Board's objectivity, the Board's openness and willing to hear not only from people like the State of Nevada and the affected counties, but in particular the public, and I think everyone really appreciates the hard work that you've lent to the project.

In particular, the integrity of Board members and the willingness to again be a part of this process is very important and something that I think we're going to look forward to as a new group.

I want to mention in particular something the outgoing Chairman mentioned, and I think that's the issue that has to do with conflicts, and I know that's of great concern not only to Nevada, but a number of other parties, and it really is incumbent on the Board to take a look at those issues hard if, in fact, public confidence and confidence in the process of your reports and your activities are a meaningful thing, which I believe they are. We already have a particularly example even recently of a conflict that I think is of some concern. The Chairman of the Igneous Peer

Review Group has apparently indicated publicly that he's going to go now and work for the Department shortly after his tenure in chairing this, which in itself creates at least the perception of some conflict relative to their activities, and, of course, is great concern to us.

But having said that, let me indicate that as the project now moves from more of a political environment to the legal and regulatory arena, the Board's work is even more important than it has been in the past.

Let me close by just letting you know that the State of Nevada, and I want to talk about this just very briefly, is currently involved, if the federal government in at least four particular cases that are all in the U.S. Court of Appeals in Washington, D.C., we are challenging the Environmental Protection Agency's rules for Yucca Mountain, the period of performance, and what we think is a gerrymandered control area. We're also challenging the NRC's licensing regulation as not being in conformance with the law that requires any repository to have as its primary barrier for isolation, the geologic structure.

We're also challenging the recommendation of the President and the Secretary and the guidelines or siting regulations that DOE has used. We believe those regulations are faulty in that they do not comply with the law and are aimed at moving from a geologic barrier to one of more of an engineered barrier, something we don't think the law allows. And, consequently, the actions by the President and the Secretary are defective as well.

And, lastly, we're challenging the Department of Energy's environmental impact statement on a number of grounds, both procedural--for example, there's still no record of decision from that particular document that should have been on the table as a part of the recommendation process--a number of other substantive issues that are contained in that document as well.

The first one of these cases is going to be heard in February in the Court of Appeals. We anticipate probably getting an initial decision out of the court on the first case probably a year from right about now, and the others following thereafter.

The only other real activity that is taking place in that arena is that the government has moved to dismiss all four of these cases, and some cases as being moot as a result of the President's signing of the resolution, and more recently, the court has denied the Justice Department their motion in all four of those cases. So, each one of these cases will be heard on the merits eventually before the Court of Appeals.

With that, Mr. Chairman, let me thank you again for the time. I know you've got a big agenda. And, again, thank the outgoing members. We really have appreciated your work. And I'll let you get on with your business today.

Thank you very much.

CORRADINI: Questions for Bob?

BULLEN: Bullen, Board. Actually, Bob, I had a quick question for you. I thought that at least two or three of these cases had been consolidated and were going to go to the Circuit Court of Appeals in D.C. So, have they consolidated a couple or three of those cases from the State, or is that not true?

LOUX: They've actually consolidated two cases. First, we separately had sued the President and the Secretary as a separate cause of action. That has been consolidated with our challenge to the Department of Energy's siting regulations. And more recently, the environmental impact statement challenge has been now consolidated with the guidelines and the recommendation case as well.

So, in actuality, there's only three cases. There's one against the Department of Energy on those three counts, plus the NRC case and the EPA case.

BULLEN: Bullen, Board again. Does the timing of the cases pose any problem for the State, or is it basically the schedule is to your liking?

LOUX: It is to our liking. We'd like to see decisions early out of the court. We'd like to see those issues resolved. From our perspective, if any of the Department of Energy's actions are going to be overturned, or the other federal agencies, it's better to know that now than after things are well down the road.

However, in our view, most of these cases, if not all of them, are probably fatal to the project in our view. For example, the guideline recommendation EIS case, if we're correct, we've caused the whole process of recommendation to have to be done over again, if it can be.

So, you're correct. The schedule is a little accelerated. Again, February and March are dates that are coming up for much of these cases, and we're happy with the schedule.

BULLEN: Thank you.

CORRADINI: Other questions?

(No response.)

LOUX: Thank you again, Mr. Chairman.

CORRADINI: Thank you, Bob.

Okay, next on our agenda is Dr. Margaret Chu, Director of the Office of Radioactive Waste Management at the Department of Energy, and her talk will be about overview of programmatic developments.

Margaret?

CHU: I first want to thank all the Board members for their willingness to take on this important and challenging job in overseeing the technical work of the Yucca Mountain program. And this program not only takes a vital role for this Nation, it's also a very, very important at a global level, and your contribution has been invaluable to us and to the program over the years, and I thank you, all of you.

And then I also want to say special thanks to the departing Board members for their countless hours of hard work in giving us guidance over the years. Under the leadership of Dr. Cohen, you have played a pivotal role in the direction of the program, and I sincerely thank all of you.

And then I also want to welcome and thank the new members for their willingness to continue on the relay--I call it a relay--in providing us guidance and oversight in the program. And I really look forward to many years of fruitful interactions with the Board.

Now, first let me give you a very high level summary of the status of where we are in the Yucca Mountain program. You all know since last May's meeting, many things happened. Both the House and the Senate have voted for Yucca Mountain to go forward, and the President signed the Congressionally approved resolution of repository siting approval on July 23rd. Actually, I attended the signing ceremony at the White House. It was quite exciting.

And then that approval allows the Department of

Energy to move forward to develop a license application to be submitted to the Nuclear Regulatory Commission.

So, we have turned the corner and then we have embarked on the next phase of the program. There are two big milestones down the road. Our schedule is to submit a license application in December of 2004, and then our schedule is to receive waste December of 2010. And we need a budget and the resources to make it happen. And in the past, the transportation program has been lagging behind, and so one of the key activities now is to ramp up the transportation planning activities. And another high priority item for me personally is to continue the science and technology program, and I'll talk a little bit more about that.

And as part of the necessary elements of the program, I have also recently made a commitment to the NRC that I'm initiating a management improvement initiative for the program. And the purpose of that is to line up the troops in the program so we can be ready for the license application and the steps beyond that.

And then like I said, I really look forward to continued input from the Nuclear Waste Technical Review Board, and it will be very important for us in the future.

Now, let me talk about the status of the program in terms of the two big milestones. One is the December 2004 license submittal milestone. There are three critical elements to get there. The first is we have to complete a high quality license application, and the document itself contains information on the site, information on preliminary design, and then we have to submit a pre-closure safety analysis, we have to submit a post-closure total system performance assessment analysis, and then we have to talk about how we plan to do continuing testing and evaluation and a performance confirmation program. So, these are the key elements in the license application document itself.

Along with that, it's also an NRC requirement that six months before we submit our license application, that means in June 2004, we have to put all the relevant information into what's called Licensing Support Network. This is an electronically web based database. And it needs to be certified by NRC six months before our submittal. So, this is a pretty big job.

And then on top of that, we have to ensure we have an effective Quality Assurance program. That means we have to show everything we said in the license application we actually have objective evidence, it's correct and it's all documented.

Now, given these are the key elements, what are the challenges to deliver that? The first challenge is it's the first of its kind. It's never been done before. So, what are the expectations in the license application, how are they going to be reviewed, what does it all mean? I don't think anybody knows at a detailed level. So, that's quite a challenge for us at a programmatic level.

And the second thing is the cultural sea-change for our program from a scientific investigation culture to an NRC regulated QA environment. And the third is we need to have timely resolution of NRC's key technical issues and other open items.

And then I have been talking to my program folks. I say submitting the license application is a challenge, and what will probably be even more challenging to us is during the review period after the submittal of the license application. How do we defend it? How do we show credibility? How do we show NRC and basically the whole world that this is a good program and it's a defensible license application?

Now, how do I deal with those challenges? My strategy right now is we will acquire adequate resources for a quality license application submittal. So, that means I'll have the best QA practices. I will have the right expertise looking at this, the right expertise, and then the strategy to deal with technical issues. And then also, I'll have the right expertise, have people that have the NRC licensing experience to help us in the next phase.

One of my challenges is the time is very short. So, what I'm hoping is through this management improvement initiative, to make the program more efficient, and so I built some schedule contingencies in the next two years. And then the third thing of my strategy is to clarify NRC's expectations to interactions early and often. Since it's the first of its kind, we need a lot of that. So, that's license application itself.

So, the next big milestone is the waste acceptance at 2010. One of the critical things that needs to happen is we have to receive construction authorization from NRC in 2007. The reason I say 2007 is after the license application, there's 18 months of NRC review, and then there's 18 months of Board hearings. So, 18 plus 18 is three years. So, if everything goes well, then theoretically we'll get construction authorization in 2007.

And then after that, it means that we need to construct a repository by 2010. So, it's a very tight schedule. And in the meantime, we have to develop the whole transportation capability to support the 2010 waste acceptance. That means we have to acquire all the casks, the operations readiness, interactions, and then we have to identify all the shipping routes, and then we have to be ready for emergency responses.

Now, what are my challenges to get there for waste received in 2010? First, as I already talked about, is we need to have timely and effective response to highly rigorous license review after the submittal of the license application. And since the transportation activities have been deferred for years, we need to develop and implement a national transportation program with a very tight schedule.

And then the third thing is the reality. We have to ask for large funding for the construction of the licensed facility. And given the current budget caps and system, it's very difficulty to have a huge increase in budget requests.

And then the fourth thing is, again is reality, we have a lot of ongoing litigation that potentially can delay us.

Now, how do I overcome those challenges? The first thing is acquire resources and systems to support license application, and then be ready for the review period so we can get construction authorization in 2007.

The second thing is we will consider a phased construction approach for the repository design and construction, and the benefits are three-fold. First, we can start receiving before it's a totally complete whole subsurface and surface facility. The second thing is to help us in the budget request so I don't have to request a humongous amount of money at one time. And the third thing is we can take advantage of the lessons learned as we go along if we use a phased approach. So, we can improve our operations and our systems as we learn more and more as we go along.

And then the third thing is the science and technology program. That, if I do it right, I believe will increase the credibility of the safety of the repository. It will give us a chance to continue on with reducing uncertainty of the repository system. Also, it will give me a chance to be more cost effective, and look at technology advances as we go along.

And then the fourth thing is to jump-start the transportation program. What we are doing right now is we start on some policy statements on how to do the emergency response right now. That's a requirement of the Nuclear Waste Policy Act. Also, we are in the process of initiating some acquisition of long-lead specialty casks. Let me clarify that. DOE has very heterogeneous waste, and so there are some cask designs that have not been done, and nobody is manufacturing them. I'm not talking about the commercial side. I'm talking about the DM waste. And, so, these take years, and so we're going to start the specialty cask acquisition process so we'll be in time for the waste received in 2010.

And then we are looking at other fleet requirements and operational requirements, and also we'll be looking at institutional activities. I'll talk to that a little bit more later.

In addition to that, there's some across-the-board challenges. In the short term, the funding. We need adequate funding for near-term milestones and ability to manage some of the things we need to do now. The FY 03 budget request level is $525 million. The Senate mark was at $336 million, and the House Committee mark at $525 million. This is current. And the timetable for the House-Senate conference is still unclear. There are rumors that they're going to continue a resolution. And we have put in an amendment of $66 million to the Congress that has not been put in officially, so it's unclear what the outcome of the amendment will be in this process.

From a funding perspective in the long-term, I will look at life-cycle cost reduction and then also I have to start looking at near-term reprioritization based on the budget outcome.

And then I'll mention a few words about this management improvement initiative. That's a commitment I've made to the NRC. And there are five areas I'm working on. Based on years of assessments and audit of the program, one of the key issues being brought up over and over again is the roles, responsibilities, authority and accountability of the program have not been very clear. So, this is one of the first things that I'm embarking on, is clarify roles and responsibilities to make sure everybody knows what their job is supposed to be, how to do it efficiently.

The second thing is, and I already mentioned it, is the QA program. I'm trying to make it more efficient, and then I want to make sure QA is part of the everyday work rather than people looking at it as an audit function.

And then also I'm trying to, in the process, trying to streamline some of the procedures in the program. And then the fourth thing is the corrective action program, and then the safety-conscious work environment.

I do want to call to people's attention when NRC reviews the license application, they not only look at the license application, they look at the organization as a whole, whether it is ready to embark, for the next phase, whether it's ready to be licensed to go on with the construction and operation. So, all these things are important, the cultural thing is an important consideration for NRC. That's why I'm doing what I'm doing.

And then I've already started implementing the initiatives in August of 2002. It will probably go on for many, many months.

Next, I'm going to talk about the science and technology program. You're going to hear more later on from Steve Brocoum on some of the specifics. But I want to reiterate I'm committed to develop a sustained program. I want to emphasize that word sustained. I want to institutionalize this program. And the purpose of that is to increase confidence in repository performance by reducing uncertainty, and then enhance efficiencies and reduce life-cycle costs, improve existing technologies and develop new ones. And then I want to maintain the U.S. leadership in nuclear waste management.

What is the relationship of this program to licensing? It may enhance confidence during the license application review period. And the relevant information will be integrated into licensing-related activities as appropriate. And some of the activities may be implemented through the performance confirmation program. But it is separate from the core science of the license application.

The DOE task force lead by Steve Brocoum was created in April to start the planning. And in May, I said I was hoping I could give you a budget number, but given the budget situation, the initial funding for this program has not been determined. But I am committed to start a program in the science and technology. And then your input and review will be very valuable for the progress of this program.

I also want to let you know my plan right now is to formalize this into the organization sometime in October.

So, there will be a real organization in my chart.

Now, in summary, with the formal site designation of Yucca Mountain, a historical milestone was achieved, and one which could not have been achieved without the hard work and dedication of many people. And as we move forward, we look forward to the comments of the Board members to help DOE improve its technical basis. I'm hoping the science and technology program will be an important area for the Board members to review and comment on.

Thank you.

CORRADINI: Thank you, Margaret. Questions? Debra?

KNOPMAN: Knopman, Board. Thank you, Margaret for giving us this good overview.

I'd like to just ask a few questions so I better understand your notion of culture change and the reconciliation of a compliance culture, licensing culture, with your desire to have a science and technology program, not part of that licensing culture. I guess--and let me put a third point on the table that I think would also help me to maybe understand what you're saying.

You talk about the importance of improving credibility, not just for the NRC, but for I think you said for the world at large. And I'm just wondering how you bring these pieces together. One could say, or make an argument, that a compliance culture is actually not conducive to investigation and exploration for its own sake, or for improved understanding on its face. It's only in the context of what one needs to do for the regulator.

And I guess my concern here is that in defining culture changes, sort of an either/or proposition here, either we're in a site characterization investigative mode, or we're in a compliance culture and we're going to make this change, that you're maybe going to run into some difficulties in the signals you send internally and externally about the value placed on an investigative science and technology program that's actually designed to go beyond compliance in the sense of pushing for a higher level of credibility, looking for continuing to challenge the models, the technical basis.

CHU: Let me try to answer, but maybe not in the correct order. Let me first talk about the core science and the license application versus the science and technology.

The way I view it is license application has its technical basis, and this is what the core science is about, we continue to work on that technical basis, work around that technical basis. When concerns and technology come in is, for example, if we believe that the saturated zone, say we have taken the license application technical basis is very conservative, then the science and technology can come in and pick up some activity and say are there other credits there in the saturated zone that can supplement the license application technical work, make it more defensible and vis-a-vis additional information.

Or, there are certain things that we feel that may need some fundamental longer term understanding to support the technical basis for the license application. It's just simply not here yet. I think the science and technology program can pick that up and say let's go on with those fundamental understandings that will either beef or say, well, we have some issues there on our technical basis. Let's do good on this job, on the site, look at some significant--I want to emphasize that if there's significant fundamental issues that need to be looked at in the long term, that's where science and technology will come in.

I hope that kind of gives you a good feel on the difference between the two areas.

KNOPMAN: Yeah, that helps. My question really had to do with culture change as well. Just on your answer, as a quick follow-up, if I may, you defined short and long-term S&T needs. The short term, though, didn't sound to me like it was in a challenge mode. It was in sort of enhancement, what can we do beyond what we've got, and that is the segway into this culture change issue. Will the S&T group be, in a sense, either play devil's advocate or be the in-house what used to be called skunk works at I guess Lockheed way back when, to have a group of challengers internally--

CHU: I hope we will, and looking at all the issues internally, that's part of the goal. But let me address the culture. What I mean by culture change is, and this really has nothing to do with science and technology, it's, for example, the QA. There are a lot of people, because they're scientists by training, they are not used to this discipline of documenting things in a process oriented way. So, those things are very important to the license application, and we can have gaps in our documentation or process development or decision development process, and later on, we can stand up and justify what happened. Well, how did we get to where we are? So, that's what I mean by cultural change. People need to understand how they need to fold in some of this nuclear culture in their daily work so a license application is defensible.

Do you have another question? I thought you had three.

KNOPMAN: That's okay. I was just trying to--I raised the question about credibility. What's your measure there of wanting to improve the credibility of the program outside of what might be required, strictly speaking, in a license application?

CHU: To me, credibility is something that's hard to define. It's through interaction with the Board, interactions with the Academy of Science folks. Credibility means I'm going to encourage my technical folks, peer review publications. To me, credibility means communicate to the people, to the State folks, to the technical folks, to everybody. To me, that's what it means. And then we can address a lot of the issues by interactions with a variety of folks, and that's what I mean by increased credibility.

CORRADINI: Let's move on. I see Dan, Priscilla and Mark. Dan?

BULLEN: Bullen, Board. Actually, just three quick questions, but maybe in inverse order of the way you presented.

You talked about the transportation issues and the need to jump start the transportation system. But I guess the key question there arises in that how are you going to interface with the utilities that currently have the fuel. And in particular, there's a lot of utilities, or there are a lot of utilities, even in my home state, who are putting spent fuel into dry cask storage in containers that are considerably larger than are going to be emplaced in the mountain, greater than a 44BWR container or greater than a 21 PWR.

And, so, have you talked about interfacing with the utilities in trying to coordinate the efforts so that you're not going to end up with multi-purpose containers that were purchased by the utilities for storage and transport, but are not going to be suitable for disposal, and then actually maybe generate another low-level waste stream because you're going to have to deal with those, too? And I realize you have litigation, so I'll let you--

CHU: First, you know, I want to remind you, Dan, that there are ongoing litigations with the utilities. So, a certain part of us, our hands are tied in that.

And, also, the existing contracts with a lot of the utilities on how to receive their waste, it's already in the contract. However, after saying that, this is a part of our plan in the transportation, and I forgot to mention that our plan in the transportation program, is early next year, we will publish a national transportation plan. That will be really our program plan for the whole transportation program.

Some of it may be real plans. Others, you know, may be plans of plans, you know, and some will have policies in there, sometimes just putting down criteria, how we plan to proceed, and it will cover the whole spectrum. And then the interface with utilities, I hope will be one thing in that plan. I really can't answer you right now. This is something we'll continue to work on.

BULLEN: Thank you. Bullen, Board. Just a quick follow-up on your phased construction approach, and maybe we'll hear about this later on in the presentations today.

But when you finally put a license application in to the Nuclear Regulatory Commission, you'll have an application for the full-blown design, but will you have a defined minimum requirement of, say, surface and underground facilities that will be necessary for this phased operation? And how will that be defined, and have you already set that out?

CHU: We're still working on that. My understanding is NRC's rule has certain requirements in there of what the surface storage means. I believe it's as long as it's part of the operation, logistics, you can store some on the surface. And then all the surface design will have to give enough detail so NRC can review it and have confidence to say yes, it's safe, you know what you're doing, you're ready to do that. So, we're continuing to work on that.

BULLEN: Okay. One last quick question.

In your long-term science program, you said you wanted to institutionalize it so that the funding would be stable. What plans do you have for that institutionalization so that, I mean, from year to year, you're always going to have the ability to have that chunk of money that you can continue the program, despite the fluctuations of Congressional appropriations?

CHU: You know, I don't have an org chart that has that program in there, you know, occupy a box. That's what I mean by formalize it. And then I will--the item in my budget request every year, and it's in my preliminary '04 budget request right now. So, that's what I mean by formalize it.

BULLEN: Thank you.

CHU: And then after a few years, if I continue to get the funding, it will get formalized and become a usual item in the budget request.

CORRADINI: Priscilla?

NELSON: Nelson, Board.

Help me to understand how you see the priorities evolving, because from my perspective, and it may be a bit suspicious or somewhat jaded from trying to maintain priorities, I could see the issue of a very tight budget and budget caps established by the organization or by Congress, fighting for priorities for budget. There's this thing called performance confirmation, which is a part of the license application. From the science and technology perspective, it seems very clear that getting something into PC as opposed to not getting it into PC may actually result in different priorities for funding, and different security on the work being done. So, the choice of what actually is considered part of performance confirmation and, therefore, part of license application, as opposed to what's outside and in the science and technology budget becomes an important issue on priorities, I would imagine.

So, that issue about what's in PC and what's not becomes important I think for Board understanding of where the project is going. And that seems to be taking place over the next 18 months. You would expect that to be fully defined in that period of time; is that correct?

CHU: Yes. I think we have a PC talk later. The performance confirmation program uses a risk based, performance based approach. That's how they're going to prioritize what needs to be in the performance confirmation program. And then my hope is with the science and technology program, we'll work closely with them to make sure they have this risk based, performance based approach that will be in the license application. And then how we fold into it, I can't tell you right now, but we will work at it and maybe they may be a little bit more compliance focused and we may be a little bit broader based. I know I'm not quite answering your question, but these are some of the things that we need to work on.

NELSON: I think that's true, and that's of great interest to the Board, is understanding this.

CHU: Okay.

NELSON: Because the decisions about what are considered part of PC and what are not have different vulnerabilities in a tight budget world, and we really will want to focus on that.

CHU: Okay, thank you.

CORRADINI: We have one more question. Mark?

ABKOWITZ: Abkowitz with the Board.

Margaret, up until now, the Board has been focusing predominantly on issues around the repository site itself, and I see that there's still a lot of important work to be done, and I see the science and technology program as an instrument where some of that activity might continue and evolve.

From my vantage point where I'm showing a new paradigm where there's an entire waste management system that needs to be addressed in terms of how we get these wastes from their current locations safely destined for the repository site, and I would like to believe that in your comments about the science and technology program being an important area for Board review and comment, that that also extends over to all aspects of the waste management system, which may include science and technology, but may also include other methods and practices that are considered, you know, to be proactive and on the cutting edge. So, I'd like to at least hear your personal comment on that.

The other question I have for you is that as part of your management improvement initiatives, does that explicitly include a cross-polarization within DOT between various programs? For example, to what extent will there be interactions between your program and the WIPP program, so that we might actually learn from experiences, which not only will put us farther along, but also perhaps in a most cost effective manner? Thank you.

CHU: The management improvement initiative, at this point, it's very internally focused, and we did not include like cross-polarization with WIPP and other things. But that's a very interesting idea. We will take that idea and think through how to incorporate those lessons learned.

And then I want to come back and answer your science and technology on the whole waste management activities. Yes, it is of great interest to us. So, we are looking at, and we do want to look at the whole waste management cycle, and that's why I also emphasize the life cycle cost of the whole system, so it's really from generation to disposal.

Actually, within the program, actually it's outside the science and technology program, one of the things we're looking at is looking at some of the environmental clean-up waste, and looking at their waste residuals, and then try to evaluate their waste acceptance criteria into Yucca Mountain and say are we optimizing that. So, yes, to answer your question, yes.

But, of course, the reality is what is our budget going to be like. So, we have to prioritize, and then figure out what is the best way to go forward. But I'm looking forward to comments from the Board members.

CORRADINI: Thank you. Thank you, Margaret. We'll have to move on.

CHU: Thank you very much.

CORRADINI: Our next speaker is Donald Horton. He's Deputy Project Manager of the Yucca Mountain Site Characterization Office, and he will be speaking on the Yucca Mountain project plans. Don?

HORTON: Thank you. I'd like to thank those Board members that are departing after today, and thank them for their interactions with all of us here at the project, and also welcome the new members and look forward to the path forward on many more interactions as we continue in the program.

Dr. Dyer regrets that he can't be here this morning. He's off in Moscow giving a presentation over there, and he's sorry that he couldn't make it here this morning.

What I'd like to do is to address a few of the areas that we have on the project as our priorities in the coming years and immediate future. We're going to have much more detailed presentations later on this morning and this afternoon, but I want to provide a highlight of those activities. They are the Yucca Mountain status, the major technical activity streams, such as the repository safety prior to permanent closure and after permanent closure, the repository safety case, and the summary.

As you know, the site recommendation and selection was passed this year. Our next major milestone, as has been discussed previously, is our license application in December 2004.

As far as the project status, our highest priority is protecting the health and safety of the workers and the public, and protecting the environment. We're going to have to instill a safety conscious culture across the project. And what I mean about a safety conscious culture is not only industrial safety and nuclear safety, but we want any employee to not only have the right, but deserves the option of raising any question or any issue on the project, and getting a timely, satisfactory resolution to that. This is not only in the QA area, but any area on the program.

We're going to be developing a license application that successfully meets the Nuclear Regulatory Commission requirements. We plan to submit the license application to the NRC, as previously stated, in December of '04, and we're currently working on sections of the LA.

The focus of the technical work is on engineering and design, performance assessment, scientific activities, and continuing testing and performance confirmation. All these topics will be covered this morning and this afternoon.

The major technical activity streams include the repository safety prior to the permanent closure, which includes the engineering and design, the design evolution, the preclosure safety analysis. The repository safety after permanent closure includes the TSPA assessment for the LA.

On the engineering and design, the preliminary design at LA submittal will emphasize the systems important to safety. This includes the concept of operations that will be included in LA and provides a basis for the preclosure safety analysis.

The design is going to evolve, and the level of detail will increase as DOE learns more and adjusts to the changes in our understanding of the system.

Progress toward completion of the preliminary design will be tracked through interim design reviews. And these interim design reviews will be conducted by DOE at a high level to see the status and the integration and the adequacy of the design. It's not the substitute for the independent design review that's required by the regulations. There will be numerous of these design overviews, and we welcome the participation of Board members, as we did in one of the other design reviews that we conducted, along with the Nuclear Regulatory Commission, on-site reps., and others.

As you can see by the chart on the bottom of this slide, the preliminary design and the conceptual design are where we're at right now. The detailed design will progress as we continue on in the project. We will have some of the detailed design completed at LA submittal. We'll have another major portion continued through construction authorization, and it will continue on after the receive and possess.

The preliminary design that will support the LA will consist of additional detail and refinements to the design concept for the SR. The final decisions and approvals of the LA design have not been made at this time.

The LA design is expected to fall within the bounds established for the flexible design concept described in the SR and the Environmental Impact Statement. And I think that we're going to have Dr. Boyle discuss this more at length this afternoon.

The environmental impact analyses are part of the evaluation and the selection process for any significant design changes.

The PSA is a quantitative analysis of the potential events during operations and their consequences, or doses to the workers and/or the public. They start with the descriptions of the site and the design. They identify the potential events and their probabilities of occurrence. They assess the adequacy of the facilities to perform as intended, identify any limits on design or operations, and describe the means to mitigate or prevent the accidents.

PSA iterates with design to achieve preclosure performance objectives. It provides a mechanism to integrate the design concepts and evaluate the performance of these concepts.

The major elements of the TSPA-LA are to incorporate the new scientific data and information that we obtain as we go along, to qualify and validate the supplemental science and performance analyses, the SSPA, and the final environmental impact statement models.

It will also address the NRC Key Technical Issue Agreements. It's also intended to improve the treatment of features, events, and processes, and the scenario analyses. Performance of the TSPA licensing compliance analysis will evaluate the dose-based performance objectives and demonstrate multiple barriers that we have on the program.

On the TSPA, some of the milestones that we have are the TSPA Methods and Approach Document is due this month. The process Model and Abstraction Analysis Modeling Reports, the AMRs, are due in June of '03. The FEPs for the LA are due in October of '03, with the TSPA Model AMR due in December of '03, and the TSPA-LA Analysis Report is due in May of '04.

The repository safety case, or as we refer to it as the licensing bases, is a set of logic, analyses and calculations, including the quantitative and qualitative supporting information, that show the repository would meet the performance objectives.

The DOE safety case will be documented in the licensing bases for the LA. In addition to the quantitative safety analysis, the preclosure licensing basis will include a design margin and defense-in-depth similar to the commercial reactor precedent and experience.

In addition to the quantitative performance assessment results, the postclosure licensing bases will include multiple lines of evidence, multiple natural and engineered barriers, natural and manmade analogues, and continued testing and evaluation.

DOE is considering the merits of preparing a separate document to communicate with decision makers and the public. This document will be prepared, as I understand it, right now, it's being considered to be prepared by the Science and Technology Group.

In summary, DOE has developed plans and schedules to submit a license application to the NRC in December of 2004. The focus of the major technical activity streams is engineering and design, performance assessment, and continuing testing and performance confirmation.

Progress towards completion of the preliminary design will be tracked through the interim design reviews, as I stated, with participation hopefully of Board members and staff. PSA will be developed iteratively with the design. TSPA emphasis will be on enhancing confidence and adequately representing uncertainty.

Continued science, testing and performance confirmation will be managed in an integrated manner. DOE's safety case or licensing basis will be documented for the LA.

And the last chart just shows some of the schedule that we have laid out, which I discussed earlier as far as the Safety Analysis Report Chapter 1, with the interim dates that we have for completion. And then Chapter 2 is the repository safety after permanent closure, with the dates for specific deliverables, leading to the initial licensing support network certification in June of 2004, and the license application submittal in December of 2004.

We're going to be getting into much detail on these subjects, as I said, this afternoon and later this morning. You discussed performance confirmation earlier with Margaret. One of the suggestions that some of our staff had earlier this week was that perhaps this would be a good topic for a separate meeting with the Board members in the immediate future. So, we're looking at that right now.

CORRADINI: Thank you, Don.

HORTON: Thank you.

CORRADINI: Questions? Dan, then Debra.

BULLEN: Bullen, Board. Actually, I'm glad you have this timetable up here, because one of the questions with respect to design deals with the selection of the operating mode between high temperature and low temperature, and I just wondered when will that decision be made in this time frame?

HORTON: The decision on the design right now I think has been made. We're going in with the SR design. However, this allows a flexible operation, and Dr. Boyle is going to discuss that later on today. But it will allow us to operate at a flexible temperature.

BULLEN: I'll maybe defer to Bill's presentation this afternoon.

HORTON: Okay.

BULLEN: My only other follow-on question is to what extent, when you talk about your repository safety case, is this case not dependent on TSPA? What are the other arguments or discussions or analogues, or whatever you bring into the case? Because a lot of it is deciding how to set the dose limits and calculate it, and TSPA sort of drives that. But to what extent is it not dependent on TSPA?

HORTON: I'm going to have to defer that to Peter Swift or Dr. Boyle to give us that latitude on that.

BULLEN: A preview of coming attractions for this afternoon. Thank you.

CORRADINI: Debra?

KNOPMAN: Just a clarification. You say twice in your slides DOE's safety case will be documented as the licensing basis for the LA. Does that mean that the safety case is the identical document to the NRC's required safety analysis report, or are there two separate documents?

HORTON: The safety case and our licensing analysis will be a part of the LA, and they are synonymous. The Board uses the safety case. We in DOE are now using the licensing analysis. But they are synonymous.

KNOPMAN: Okay.

HORTON: Does that help?

KNOPMAN: I hope so.

CORRADINI: Other questions? Priscilla?

NELSON: Nelson, Board. I note on Slide 12, DOE is considering the merits of preparing a separate document, and I personally would strongly encourage that as a basic way to communicate the decision to the public.

I have a question just overall. You've had a tremendous background in quality assurance and in the industry in general, and in putting this package together, which is the license application package, it is made to look a lot like other projects in many respects, in terms of deliverables, the kinds of parts that are there. I'm just wondering is there any particular aspect of this project that you find very different, and perhaps difficult to package, as other projects you've been involved with?

HORTON: Well, the other projects that I've been involved in have not had as near the degree of science that this project has. Up until this point, a majority of our work has been in the science and testing area versus when I've been on other projects, and all that's been done and you start in the design and the construction area, and start immediately more in the compliance versus thinking outside the box, so to speak.

NELSON: Well, that's looking backwards. Just looking forward to the pall mall movement to LA, it's packaged to look like other projects.

HORTON: Yes.

NELSON: Is there anything that's very difficult to package?

HORTON: To date, I haven't seen anything that will prevent us from packaging it in something similar to what I've been accustomed to in the past. I don't think that we'll have any problems doing that.

CORRADINI: No other questions. Thank you, Don.

HORTON: Thank you.

CORRADINI: We're going to take a break for 15 minutes, and Dr. Paul Craig will begin the next session.

(Whereupon, a brief recess was taken.)

CRAIG: I'm Paul Craig, and I'm a member of the Board. I'm going to chair the session this morning. We have a wide range of science and engineering design issues, and that will extend through the lunch break, into the middle of the afternoon. We're going to start out with a science and engineering update by Mark Peters of Los Alamos, the Science and Technology Manager for Bechtel SAIC. We've heard (inaudible) and missed him at the main meeting. He's got a lot of ground to cover, and we're going to hear about lots of progress or lack of progress in many areas in resolving the Chlorine 36 controversy.

Then comes Steve Brocoum, an OCRWM senior policy advisor, who's going to talk about the DOE's proposed science and technology program, and this will be a more detailed discussion of some of the things presented earlier by Margaret Chu.

Finally, before lunch, we're going to hear about the Inyo, California Regional Ground Water Monitoring program. Inyo County is one of the affected counties, and the speakers will be Andrew Remus, Project Coordinator, and mike King of the Hydrodynamics Group.

After lunch, we're going to hear from Bob Budnitz and Frank Spera about the Igneous Consequences Peer Review Panel and about Bob's new job. Then Bill Boyle is Acting Deputy Assistant Manager of the DOE Office of Licensing and

Regulatory Compliance, who will talk about postclosure thermal conditions, how hot should it get, and DOE's insights about the controversy of whether there should be a high temperature or a low temperature design, HTON or LTON.

Then comes Gerry Gordon, Senior Staff Scientist with BSC/Framatome, who will talk about corrosion testing. And the final talk of this session will be Gordon Pedersen, Deputy Manager of BSC repository design project.

That's a lot of terrain to cover, and we're going to hold you to a tight schedule. I have my little timer here, so speakers, when you here this noise, that will typically mean that you've got five minutes. Actually, this group is well trained, and Mark Peters in particular is really good covering a lot of information in a very, very short time.

Mark is a Geophysical Science Ph.D. from the University of Chicago, and as I noted, the Science and Technology Manager. Mark, where are you?

PETERS: I'm going to wander, if that's okay with you all. I have 70 slides to do in 35 minutes. I actually have 40 slides. A lot of the information is in back of the slide. So, we can cover it in questions, or you can get me at the break.

What I'm going to focus on is what you all have become used to seeing from me, and in particular, the science program. I'm going to be focusing on the testing program. I'm going to try to cover status of the entire program, with the exception of the waste package materials and the EBS environment area, which Gerry Gordon will touch on this afternoon.

The same objective that we've had in previous presentations, to provide the status on the data collection and testing program in support of both the models in support of performance assessment as well as the design. That part of the discussion you all had with Margaret is up here. This is the status of the ongoing program. I think Margaret referred to it as the core programs. This is the ongoing program that will support the technical basis for the license application submittal in 2004.

I'm going to walk through the system, much like I have in the past, starting with the unsaturated zone, talk about the ESF studies, the drift scale test, the long awaited slide on Chlorine 36 validation, then talk about the cross drift studies.

I'll walk through the status of a lot of these tests, a lot of it you saw in the field yesterday, a brief overview of what's going on with the Busted Butte blocks that were taken out, and they're being studied by the Canadian program, AECL, up in Ontario, and then a couple brief slides on saturated zone updates. And the second half, saturated zone, and the rest of it, we didn't focus on too much yesterday, so this might be some new information for the new Board members.

And then what I've got as engineered barrier, although they're tied into natural barriers as well, thermal properties investigations, rock properties, mechanical rock properties investigations, and then a couple slides on the Atlas Test, natural conduction test, and the breached drip shield experiments that we did over there. Finally, a couple slides on waste form investigations going on at Argonne.

And then I also included a slide, as well as one of the backup, on some of the details of the measurements that we made associated with the June earthquake, and then wrap-up.

This is a diagram of the exploratory studies facility, going in at the north portal and coming out the south portal, with a schematic of the proposed repository block. This is consistent with the site recommendation. Just to give you an idea, we're going to again focus and I'm going to talk primarily today about just results from the drift scale test at Alcove 5 and also the Chlorine 36 validation work which is looking at samples from Niche 1 down here near Alcove 6, as well as samples from the Sundance Fault as exposed in the SF.

First, the drift scale test, layout of the drift scale test, a design this entire audience has seen many times. The observation drift, the connecting drift, this section here is the actual heated tunnel. It was heated with nine large canisters with electrical heaters on the inside, as well as wing heaters in the walls. This test is now in the cooling phase. The cooling phase started in mid-January of this calendar year. Still plan for a four year cooling phase.

And then the other colors are simply showing the boreholes that are used to monitor temperature, mechanical movement of the rock, as well as moisture redistribution, and also sampling water and gas for chemical analysis.

This diagram gives you all an update on the temperature along one representative thermal couple. This is a thermal couple into the drift wall. About halfway down the heated drift in the ceiling, shows the time history for temperature on the right, and power on the left. It shows the cooling phase. You can see up around 200 C., the peak temperature, that's where we maintained it. We got there after about two and a half years of heating, maintained that to the four years, flipped the power off, went down to zero power, and we're now in the natural cooling phase, and you can see we're around the 120 degrees C. range along the drift wall. And this just shows how we stepped down the power to maintain that 200 C., and then went to zero power.

This is a plot of that same thermal couple. It's a little confusing on the Y axis. This is approximately 200 C., so this is just showing the difference from 200 C. as we enter the cooling phase. It's showing a measurement in green here, so again, we're down around 120 C. of that representative thermal couple.

It also shows the simulations that we would expect for that location for that thermal couple along the drift wall in terms of our simulation using TOUGH-2. You can see that there's a discrepancy between the simulations and the measurements. That's actually we think telling us something about how we're handling the predictions in terms of what's going on right at the drift wall, and how we're handling the radiation in the drift with TOUGH-2 simulations. Remember, this test was intended to be a test of the rock, not of inside the drift. But we've been learning some things about what's going on, which is allowing us to refine our models for drift wall temperatures.

Before I leave the drift scale test, we continue to collect data as well on moisture, the rewetting process, and also sampling water and gas. There's a little bit more information in the backup on that.

Chlorine 36. The Board is very familiar with this work. We had back in the mid Nineties, we had discovered elevated levels of Chlorine 36 primarily along structural features in the northern part of the ESF that were considered bomb-pulse signatures. We went back in to validate those occurrences by drilling systematic holes across two features, the Sundance Fault and Drillhole Wash Fault, and had an independent group, USGS and Lawrence Livermore team, do independent analysis. And you're familiar with the history. We were getting varying degrees of discrepancy between the two organizations in terms of analysis of the data.

When I talked to you in January, we thought we were converging. We thought a lot of it had to do with how we were preparing the samples in terms of crushing and leaching in the experiments. We were converging and starting to get the right answer, so then we said okay, but we haven't found bomb pulse yet. Both organizations have yet to discover elevated levels that would be deemed bomb pulse. So, we went into the one place where the previous investigators at Los Alamos had found bomb pulse in core. Remember, the original dataset was taken from blocks taken from the walls. So, we went back in, got some core from Niche 1, said okay, here we go, we're going to get the same answer. We went the other direction.

So, we're back to where we've analyzed core. The two parties, Los Alamos continues to reproduce their data internally, I would say. They found elevated levels of Chlorine 36. Bomb pulse is defined roughly anything above

1200 times 10 to the minus 15 is considered bomb pulse. So, you can see Los Alamos found elevated levels of Chlorine 36 to Chloride ratio, whereas, the U.S. Geological Survey continues to find levels that are considered more like background.

Both the blanks internal to the organizations checked out okay in these measurements. So, we're in the process of trying to figure out what's exactly going on with this dataset, and then plot out a path forward.

One of the things that we're doing as we speak at the U.S. Geological Survey is they're conducting a series of crushing blanks. We did a lot of work looking at the leaching process, whether you shake it or whether you just let it passively leach, and we feel like that's been established as not a problem. Now we're going to look in more detail at the crushing.

We're also going to write up a report. We've done a lot of work here, and we continue to delay the report waiting to resolve the issue. Clearly, the issue is not resolved, so we're planning to write the report, and in that report, we will lay out further experiments. We're considering also bringing in some independent folks to look at our report and our path forward to see if we're thinking about all the stuff that we should be, some experts in this field.

But I think the bottom line here is we continue to work the issue, but my last bullet kind of gets the bottom line. DOE at this point does not intend to change the conceptual model for UZ flow and transport based on the fact that we have a discrepancy. The current conceptual model respects the presence of bomb pulse, or so-called fast paths in the UZ. And that conceptual model will be held as we go forward. But DOE does feel that it's still important to resolve these discrepancies from a perspective, I would call it in my words, of scientific credibility.

Moving into the cross drift, I don't have time to belabor this, but I'm going to go through results first of some of the hydrology testing, or at least some status of the hydrology testing that's going on in the cross drift, in the Board's drift, as was mentioned earlier.

We have learned a lot in the cross drift. I would absolutely agree with the former Chairman. And I'm going to talk today about Alcove 8 experiment where we're doing a drift to drift experiment, and also about the results of the Lawrence Berkeley work looking at seepage in the lower lithophysal unit of the Topopah Spring. That's taking place in this area of the tunnel. And also a brief overview of what we're seeing in the bulkhead experiment. Remember that we've got a series of four bulkheads. This one is current open. But when this experiment is full up and running, we have this entire back half of the cross drift isolated from ventilation. So, we're looking for natural rewetting, evidence of seepage, and as the Board has heard in the past, we're seeing some evidence of moisture build-up in there, and I'll talk a little bit more about that.

What's also shown on this diagram is the geologic contacts with the different sub-units of the Topopah Spring, for those geology purists in the audience who like to know where the different pieces of the Topopah are exposed in the tunnel.

For those on the tour yesterday, Steve Deats of the USPR talked at length about the results of the work that they're doing looking at lithophysae distribution or the cavities in the Topopah Spring and how that relates to stratigraphic--put in the context of variation within the lower lithophysal in particular, and how that relates to fracture distribution.

So, they've done a series of traverses using different geologic methods, and they're looking at a lot of different features, the abundance of cavities, the size of the cavities, rims and spots, meaning if you've been down there and looked at this rock, some of the cavities are actually open holes, others you can see almost white spots. They look like filled cavities. They're accounting for that. Those actually tend to be higher porosity areas. And then also evidence of lithic clasts. Those are clasts that were ripped up from the country rock during eruption, or within a volcano during eruption. And, finally, the matrix or groundmass.

This work is very important, and it's well integrated with the work that's going on in mechanical and thermal properties. The lithophysal porosity is a very important parameter in terms of understanding the mechanical and thermal response of the rock. So, we're doing this work co-located in a lot of cases with the actual thermal and mechanical tests, and just in general trying to get a broad understanding of the variation of lithophysal porosity along the length of the lower lithophysal.

I won't dwell on this. I mentioned that there were different ways, different methods being used to get at some of these features, and then there's also lithostratigraphic features, and then they've also then turned that into a calculated porosity for the lower lithophysal as a function of stratigraphic depth.

A couple observations. There's quite a bit in your backup. There's some detailed maps and a little bit more detail, along with a plot that shows the variation of lithophysal porosity along the cross drift that might be more meaningful. But, in general, and you saw this yesterday if you were on the tour, the greatest amount of the large lithophysae occur towards the upper part of the lower lithophysal itself. And, again, they're looking at vertical variations within the cross drift and comparing that with observations from boreholes, as well as outcrops to see how that may vary laterally, let's say, north and south along the block.

Moving into hydrology, Alcove 8/Niche 3, again that is where the cross drift crossovers over top of the exploratory studies facility. So, we're using the geometry there to do a drift to drift test. The scale there, it's about 20 meters apart. It's important for looking at flow and seepage processes in the welded tuff.

This is a diagram that schematically shows the experiment. Alcove 8, again, the distance here from Alcove 8 down to ESF Niche 3 is on the order of 20 meters, or less than 20 meters. We were doing a test, an infiltration test, along a fault that's exposed at the back part of Alcove 8, and it's also exposed in the front part of Niche 3. That experiment has been completed. That's what I'm going to focus on with a short set of bullets today.

We started just recently within the last couple of weeks a larger experiment, and that's a large plot experiment where it's not associated with a fault. We've got a 12 by 12 meter plot, and we're doing large scale infiltration, again ponding water and looking for flow processes through the welded tuff, and also seepage at a relatively large scale into the ESF Niche 3 below.

Dave Hudson, U.S. Geological Survey, is one of the PIs for this work, along with Berkeley scientists, and he just informed me this morning, actually, yesterday we hadn't seen anything, they found wet areas in Niche 3 this morning. So, there's actually dripping starting and it's been, what, about two and a half or three weeks since we started infiltration. And it's occurring, if you were out there with us yesterday, it's occurring in the plots closest to the entrance, the same place where we were using water during construction. There seems to be some kind of pathway there towards the front of Alcove 8. That's hot off the press.

I'm going to focus today on the fault test observations. Again, there's a small fault exposed at the back of Alcove 8. These are some of the basic observations that we made for that test. Remember, again we're looking at a steady head experiment, so we put a head on, a constant head there, and then let the feature take as much water as it can. Over time, we saw a steady decrease in infiltration rates. This tells you a little bit of the statistics about how fast the wetting front travelled, a little over a half a meter per day.

Of all the water that we put along the fault, nearly 10 per cent of the water was actually captured in

Niche 3 below. That 10 per cent number is also the number that we saw in Alcove 1. Remember the Alcove 1 experiment. We did a similar experiment from the surface. But that was in the Tiva Canyon, a different welded tuff, but similar properties.

We're seeing spatial variations in where we're seeing seepage in Niche 3. I've shown some of those pictures in past meetings. There's also fluctuations with time. We've introduced tracers for benzo gases, lithium and bromide, and looking at the break-through of those tracers in Niche 3 has allowed us to show evidence of matrix diffusion along the flow path and helped us a lot with building confidence in our models for the matrix diffusion in the unsaturated zone.

Seepage. Remember, we've got Niches 1, 2, 3 and 4 in the ESF that we're studying seepage processes I would say more at the meter scale, the drift scale. So, we're at a smaller scale than we were at in Alcove 8. Those ESF niches were in the middle non-lithophysal unit, which makes up the upper 10 per cent, or so, of the current layout. Niche 5 is about halfway down the cross drift. It's in the lower lithophysal unit, which makes up 70, 75 per cent of the current layout.

The tour then stopped here yesterday. We were standing right here looking down the tunnel. This again is an experiment conducted by Lawrence Berkeley Laboratory where they're looking at releasing liquid into boreholes and looking at seepage processes, and also the threshold for at what flux you would expect seepage to occur in the lower lithophysal.

There was some initial testing done last calendar year, and we saw no seepage in the roof. There was about 300 liters released and we, again, saw no wetting. We have since gone back in and there's always been a question about the mass balance in these experiments. You infiltrate water in a borehole above. You see a certain percentage seep. Where does the rest of the water go? So, what we have done is we have gone in and excavated a slow along the left-hand side with the hope being that we would then have a better mass balance on seeing where a lot of the water is going as it's diverting around the opening.

We have now done two tests here recently. I say we, Rob Trauts (phonetic) is the principal investigator for this work at Berkeley, and he's observed the wetting front and seen seepage. So we're seeing seepage in the lower lith now. We've also got better mass balance because we've got the slot there to take advantage of. So, that information is now being used to calibrate the seepage model as they prepare the models for the license application.

Also, Lawrence Berkeley is also conducting a set of experiments along boreholes in the roof of the cross drift to look at seepage as a function of, I would say along the distance of the lower lith as exposed in the cross drift, that's to get at heterogeneity, how fracture characteristics, permeability and seepage might vary. And, again, they're doing that in long boreholes, 20 meter long boreholes. They're drilled every 30 meters.

They've done three sets of these experiments, and they're intending here pretty soon, probably this week or next, to go in and set up for the fourth set. And they do a series of air permeability tests, and then go in and do liquid release tests as a function of distance in the borehole. They have a set of packers that isolate different elements.

In terms of understanding gained from this testing to date, again, they've completed tests in three areas. They're about to go in and start the fourth set of tests. The small fractures, a lot of which you saw yesterday, in the lower lithophysal, that's the character of the fractures in the lower lithophysal, are well connected. They're getting air permeability values on the order of above a darcy, on the order of 10 darcy.

The data do indicate that the lithophysal cavities appear to be sealed at the bottom to prevent water outflow, and there is evidence from the experiments that the lithophysae are actually not communicating. The water is not flowing through them. It's flowing around them, probably is the clearest way of putting it.

But, the bottom line with these experiments to date is they've struggled, or they've had to really work hard to account for evaporation, because we're in a ventilated tunnel, whereas the niche experiments have been behind a bulkhead with no ventilation. Here, they have to worry a lot about evaporation. They have accounted for that, and they've still established the presence of seepage threshold in the lower lithophysal. So, this data is also very important for calibration and validation of the seepage model.

The bulkhead investigations, remember a series of four bulkheads that block off the back half of the cross drift. This is where we've seen evidence of moisture build-up in the tunnel. We went in recently between the first and second bulkhead and we still see evidence of moisture, although it's less than we saw in the last entry, which was back in earlier 2002.

Remember the tunnel boring machine is powered off. We were having a lot of problems with heat sources in the tunnel. As we turned off that big heat source, we're starting to isolate smaller heat sources. So, there's still some small temperature gradients in there that are driving redistribution of the moisture. There is analysis and modelling ongoing to get the seepage aspects as well as the in-drift process to better understand the results. So that experiment continues.

Busted Butte. I won't belabor this. This is a test that you did not see yesterday on the tour. This was an experiment looking at the Calico Hills, similar to what's exposed beneath the repository. I'm going to talk briefly about some results from Canadian experiments. They took two blocks, two 2 cubic meter blocks, from the back half there at Busted Butte.

They were doing two experiments. Again, they've got two blocks. They're doing unsaturated flow and transport experiment, and the saturated, flow and transport experiment using real radionuclides. These are some of the observations that they've made in terms of Technetium and Neptunium. These observations, in terms of what's going on in an unsaturated oxidizing environment versus saturated and reducing environment in the block, are consistent with expected behavior. So, they're adding confidence to our results from Busted Butted, adding confidence to our knowledge.

Work that we're doing in cooperation with Nye County, again, we continue to collect information in support of our SZ models, our saturated zone models, in working with Nye County. This map shows an update of where Nye County is at in terms of their first three phases of drilling down gradient of Yucca Mountain.

Two things I'm going to focus on today, work that Rick Spangler at the U.S. Geological Survey has been doing using results from the Nye County program. He's been constructing cross sections, and you've heard about this before, to help revise the hydrogeologic framework model in support of the SZ model, the site scale SZ model. And he's taken the new information from the Phase 3 drilling and is in the process of revising cross sections, and those will all be incorporated into the basis for the license submittal in 2004.

The alluvian testing complex. This just shows the location of the alluvian testing complex down gradient of Yucca Mountain along one of the potential flow paths coming out of Yucca Mountain. This is a potential flow path as shown by the SZ model.

Because of the water permit issues at the site, the multi-well flow and transport experiment has been delayed. We had been doing some pumping of the primary well, 19D, prior to stopping work out there, and we're doing some observations from one of the observation wells, and we've been able to make some very interesting observations about the transmissivity. When you go to the multi-well observations as opposed to the single well observation, you get higher transmissivities. Also, M.G. Marry (phonetic) and others at the U.S. Geological Survey are also calculating effective porosity, and those again indicate higher values. Those higher values lead to slower velocities, slower travel times. So, there's a scale effect, but this is all being incorporated into the basis for the SZ model.

Moving on to thermal properties investigations, the engineered barrier clearly impacts coupled process and engineered barrier. We've got a program in both the thermal and mechanical area that's looking at both laboratory experiments, as well as field scale experiments, and that's integrated with the geologic observations and incorporated into the models. So, this is an overview.

There's some more information in your backup about what's going on in the laboratory program for thermal properties, thermal conductivity, and other parameters, lab tests at three different performing organizations using different techniques.

And, finally, we've got a field test program for thermal conductivity. We've got one test completed, two in progress, and two more planned for next year.

And just to remind you, the field thermal conductivity experiments are concentrated right now in this area of the cross drift. There's more slides in your backup of layouts of the tests, et cetera, but this is the results of the three field tests, and also the results of some model calculations that we've done. These field tests are being used to validate the model analyses.

The 6-hole test results were not available at the time that this report was starting to be prepared, and you can see that Tests 1 and 3 do do a very good job in terms of validating the results from the model report.

We have relatively high thermal conductivities from the 6-hole test, and we're still working out why that is.

NELSON: Which unit--

PETERS: Those are all lower lithophysal.

Lab properties investigations, you heard a fairly detailed presentation from Mark Bordenier (phonetic) on this program. Again, we're providing data in support of ground support design, rockfall models, and the thermal models. We're doing large diameter coring for laboratory testing, in situ field tests, flat jack or plate loading tests. We'll get rock strength and stiffness at the field scale, and also again doing the lab measurements.

Just a photograph of some of the lab experiments that are going on at Sandia National Laboratory. In large cresses, they're doing uniaxial compression tests with some of these large cores that we're sampling.

We're also doing, again, field tests, slot tests, where we're cutting slots in the rock, pressurizing a sliver of rock, I'll call it, and then in this case, actually taking the rock to failure to get ultimate strength. We currently have three of these either in place or planned. The first test here in the lower lithophysal is exposed here at the south ramp. An upper lithophysal test here. This Number 2 is at the upper lith. This is lower lith. And we're also about to start construction of a third test in the lower lith in the cross drift. Again, a similar configuration, adding heat to Slot Test 2 and 3.

These are preliminary results from the first slot test. Again, this is in the lower lithophysal as exposed in the south ramp ESF. No surprise, the stiffness or Young's modulus of the lower lithophysal samples is much less than what we get in the middle non-lithophysal. That's an impact of the lithophysal porosity. So, these are important observations. They're not unexpected, but they're still an important basis for our models, for the mechanical response of the rock, both thermally and ambient.

Moving to the Atlas facility, now moving out of the ESF and the cross drift, we're doing a set of mock-up experiments at the Atlas facility. As you've heard in the past, we've done ventilation tests. We had done mock-ups of drip shield tests and other types of tests in the past. This natural convection test was just recently completed. We ran that test at two different scales. We had a series of heated mock waste packages inside the drift and we were looking for natural convection within the drift itself, both with and without drip shields.

This is just a cartoon type figure that shows the kind of processes that we were looking at within that test, looking at, again, the effects of natural convection, both along and between hot and cool waste packages.

A very busy slide that tries to point out the results of some of the experiments. This is actual measurements here from the 25 per cent scale test, one of the simulations. This is a drift wall, the other drift wall, the waste package is here in the center. This shows the velocity distribution for that particular test. This particular simulation shows half of the drift and how well we've matched the velocity distribution using the FLUENT code.

So, the promising results are that the peak velocities that we get above the waste package are matched pretty well between the simulations and the actual observations. But these velocity distributions are telling us what sort of convection times are being set up within the drift.

Another set of experiments that were recently completed at the Atlas facility were put in place to look at what would happen if you did get a drip on a drip shield, how would that interact with potential breaches of the drip shield. So, they set up a set of experiments where they had two chambers within controlled environments. They controlled the temperature and relative humidity as best they could, and they did a series of tests where they dripped water, including splashes, rivulet flow, a very detailed test matrix, and looked at two different kinds of surfaces, a rough drip shield surface and a smooth drip shield surface, and did a series of experiments to build confidence in our model for how seepage flux interacts with the drip shield in a drip.

There's a detailed table in the backup that shows the results of one of those experiments. But we're in the very early stages of analyzing these results. I just wanted to make sure you're all aware that that test is out there, and we can talk more about it probably in future meetings.

The last two slides. I'll move to waste form investigations. These two slides are focused on work that's going on at Argonne. The other part of our program is going on at PNNL is Washington State. These are two results from long-term tests. One of the points I want to point out, these are very long-term tests. We're looking at eight and a half years long tests here. These are drip tests on spent fuel and we're looking at the cumulative release rate of a series of radionuclides for two different specimens.

The take-home point here is long-term tests, and they continue to go on, and we're seeing that the releases are converging toward two rates. One rate is what Jim Canaine (phonetic) refers to as the sparingly soluble elements, the actinides, and the highly solubles are converging at a different rate.

Also, continued work in looking at solubility and solubility controls, in particular Neptunium. This is a plot again from work going on at Argonne, looking at solubility as a function of pH of Neptunium, with model calculations for what the solubility curve should look like for two different kinds of solubility controlling phases, one Neptunium oxide, the other a Neptunium oxyhydroxide.

And the take-home point is here that it looks like Neptunium oxide is the conservative bounding controlling phase, but it appears that in reality, it's actually a schoepite type phase that may likely be controlling solubility of Neptunium in spent fuel. So, that's ongoing work as well.

Briefly about the June earthquake. We did have a magnitude 4.4 earthquake. It occurred within the aftershock zone of the June 1992 Little Skull Mountain quake. It was typical of earthquakes that we expect in the vicinity.

A little bit about some of the observations that were made by UNR from the strong-motion network. The accelerations were less than a tenth of a G at 3 kilometers, and much smaller, of course, as you moved further from the epicenter. We do have underground instrumentation in Alcove 5, and actually UNR is in the process of installing some additional instrumentation, but the reduction in peak ground acceleration was about a factor of 2 to 3 for the earthquake from what we saw at the surface network. But, again, this is typical of earthquakes, and it occurred within the aftershock zone of the Little Skull Mountain quake.

So, to wrap up, a very quick meander through the testing program that's going on in support of the basis for the license application submittal, both in the underground as well as in the laboratories, and we feel that it continues to address the uncertainties and provide additional confidence in our models and designs for the license.

CRAIG: Mark, thank you very much. That's not only a lot of material, but you finished on time, to within seconds. Questions from the Board, please? Richard and Michael.

PARIZEK: Parizek, Board. Mark, thank you again for the field trip yesterday, and also for the briefing.

What a breakthrough that you reported that occurred within two and a half weeks or three weeks. That's brand new. We didn't hear about that yesterday in the field. Is that better predicted by modelling? I know the experimentalist said he thought it would be about 45 days before a breakthrough would occur. So, I don't know if he's here in the audience, maybe he's wants--see whether he's happy or I'm happy. But how does the model forecast compare?

PETERS: I might ask Bo to give us an idea of what the model might predict because I'm not sure what that was. But let me say if you remember, yesterday there was discussion of the fact that we were mining, and I said this during my talk, but I'll say it again, as we were mining, we were using water for dust control and we saw some wetting in Niche 3 as we were mining, and my understanding from talking to Dave is it occurred at about the same place where we saw the wetting during the mining.

Bo, do you have any idea what the model would have predicted in terms of breakthrough?

BODVARSSON: The models for Alcove 8, Niche 3 predict breakthroughs on the order of weeks, and the reason that we cannot accurately tell you what the predictions are is the following. The tests, we just predict what the flow rates are going to be in the tests, and of course the permeabilities that we use in the models are not that accurate to tell you exactly what the flow rates are going to be. So, just like in Alcove 1 where you had the first breakthroughs on the order of tens of days, the same predictions were made for Alcove 8, Niche 3. And that seems to be coming true.

PARIZEK: Another question about earthquake effects.

Was there any ground low level responses of any consequence?

PETERS: No, there weren't any observations of anything. And as an aside, we also lock the tunnel down, and Sandia goes in and does convergence measurements to look for any, and we saw no evidence of any.

PARIZEK: Another question about the saturated block experiment for the Calico Hills, that showed reducing conditions, versus the unsaturated was oxidizing. Why the difference? There's report saying whether in situ values for oxidation and reduction also occur in the Calico Hills.

PETERS: Agreed. The reason for why it's happening in the block I think is still a point of discussion, whether it's microbial activity within the block itself, or they're even entertaining the probability--they were injecting fluorescein dye into the block as well, and whether that could be causing some problems with the redox in the block itself. But, as you pointed out, it's only in the saturated block that we're seeing those reducing conditions.

If we could demonstrate that that kind of thing translates to our saturated zone, that would be wonderful. Right now, we do not have the basis to take credit for reducing environment in RSZ.

PARIZEK: But that's something that's being watched for?

PETERS: Right.

PARIZEK: There were observations made in the cross drift of degradation along the margins of the tunnel, and those degradations were in part maybe the tunnel boring machine marks, but it seemed like there may have been some propagation of fractures into the wall beyond that. And if, in fact, that's true, does that affect the water flow or seepage around the emplacement drift, or might it if you start opening up, say, fracture permeability to the right or left of an emplacement drift? I don't know if that's in Bo's model or anybody's model.

PETERS: There would be near field effects. Let me maybe take it in an indirect way first. When Berkeley does the niche testing, they do air permeability measurements before and after. Remember, they drill the holes prior to excavating the niche.

PARIZEK: Usually up in the ceiling?

PETERS: Right. And they also do them to the side and right where they're going to be mined out. They do before and after air permeability measurements, and they see evidence of enhanced permeability in the crown, and little change along the wings, at least based on their measurements, typically an order of magnitude increase in the crown.

PARIZEK: Do we have those same sort of observations in the cross drift where these marginal side fractures seem to concentrate?

PETERS: Yes, and I've actually, since yesterday, I talked some to the folks who do some of the seepage modelling, because this came up in the cross drift yesterday, and they're looking at those kind of local scale effects when they calibrate the model.

PARIZEK: I have one more question. It's a maddening thing in the laboratory when the power goes off and the experiments are compromised, and so on. It would also be maddening if the water supply runs out in testing at the Yucca Mountain site. Do you have some sort of order of priority of the kind of experiments you would carry on and those you might have to scrap if your million gallon tank goes dry?

PETERS: I'm probably not going to touch that question. This is a point of litigation. This is in the courts right now, the issue of water.

PARIZEK: Well, it's just a question of priorities of which tests you might have to walk away from.

PETERS: Right now, we have no intent to do that. You drove by the storage tank that we have out there. That's, as of right now, we feel we can maintain the current program, at least in the short-term, with that water. So, we've not gotten down to any kind of prioritized list of what we would shut down first, say. But there is testing that is impacted right now by not having the permit, as I pointed out.

CRAIG: Okay. We have to move on. Michael, and then I have on deck Priscilla, Ron and Bullen, Board.

CORRADINI: Pass. Dick already got it.

CRAIG: Okay. Then Priscilla?

NELSON: Nelson, Board.

I feel it's very important that I say at this point that I was very impressed by the kinds of fundamental work that are going on right now out on the project to understand the behavior of the lower lith in particular. And it's striking, the differences in the lower lith response and behavior from the upper lith and the middle non.

This approach that Mark Borg (phonetic) and others are taking really has the prospects of a fundamental understanding of why things are the way they are, and how they're going to change, both with respect to stress, water and thermal pulse. And, so, I'm very happy that the project has done this, and I encourage it to continue.

The tie-in from the analytical, which is the approach so many analytical experiments being carried on, needs the physical experiments to verify, and that's incredibly important and it should be a high priority. So, there's my two cents.

My question specifically right here, given the difference in behavior between the upper lith, middle non, and lower lith, the decision about where to locate the crossover alcove, such that the length of rock involved in the experiment, including both the upper lith and the middle non, means that you're getting both effects, whatever they are, from both units, and with really very little hope of being able to separate what might be two different behaviors, one with very strong small liths, and one without.

Is there any prospect of being able to separate the two behaviors? Is the project planning on doing that in any way?

PETERS: That's a tough one to answer. You understand the history of why the ESF was primarily in the middle non, as you know, because at the time, we were looking at more the repository in the middle non lith. The reason that we're in the upper lith and the middle non in this particular experiment, we planned to cross over the ESF where we did with this experiment in mind. That was one of the drivers.

Would it be better if we had a test in one unit or the other? Yes, probably. Can we distinguish the differences, or the pagers? I think we can get a handle on it because we've got, if you go back to the picture, go back to probably 8, 9, 10, in there somewhere, there's up-looking and down-looking boreholes drilled up from Niche 3 and drilled down from Alcove 8. Those are there for active logging, neutron logging. And if I remember correctly, the contact, the actual upper lith and middle non lith contact, I think is about 20 meters difference. I can't remember whether it's two-thirds down or a third down. But it's within about roughly halfway down, you cross the contact. So, those boreholes down-looking might give us some ideas about how the two different units are behaving.

Now, one thing I would say is the water is getting through, so it's not getting diverted at the contact, despite the difference in fracturing and lithophysal environments. The water is clearly getting through the fracture network and across that contact. That's difficult. I'm not answering it totally because that's going to be a difficult thing to still back out cleanly.

NELSON: It is, and when you're offering this test as being one of the ways to calibrate models, and that part of it's murky, I mean, I'm almost wondering about excavation from the ESF level up and changing that. It seems important enough that I wondered about the drive to try to separate. There are a couple ways of doing it, and I encourage you to do it.

PETERS: That's fair. The one other thing I would say is remember Alcove 1 we did as well. Now, that's not the repository horizon, but it's still welded tuff, and that was all in a similar unit. And, actually, the observations in terms of breakthrough and some of the other observations were pretty consistent.

LATANISION: Latanision. I'm interested in the moisture in the bulkhead regions, and particularly the characterization of the moisture, or if you're characterizing the moisture that is being accumulated or collected. Is there any effort to do that?

PETERS: Yeah, we've got collection systems. Let me back up. When we first conceived the experiment, we didn't expect this amount of moisture to be involved in there, so early on, we didn't have all the collection systems in place. We put collection systems in place. It's difficult to collect a clean sample back there. What we think are clean samples look a lot like condensate. They're condensate, which has led us down the path, one of the lines of evidence that led us down the path that we think this is condensation from moisture within the drift.

LATANISION: Actually, what I was thinking about is whether or not you might distinguish by monitoring the chemistry of the water between condensate and water which has been transported and perhaps collected by leaching species as it's been transported.

PETERS: You said it clearly. That's our goal. We haven't gotten those two samples yet, but we have systems in place to attempt to capture that so that we can distinguish seepage versus condensation, is the way I'll put it.

LATANISION: Thank you.

CRAIG: Dan Bullen?

BULLEN: I actually wanted to pass.

CRAIG: Pass. Okay, Thure?

CERLING: Yeah, I was just wondering about the redistribution of water during the heating experiments, and wondering if that overcame the seepage threshold and how your mass balance was on the redistribution of water during the heating experiments.

PETERS: You mean the drift scale test?

CERLING: Yes.

PETERS: Let's take the first piece. We're not seeing any water dripping back into the heated drift during the cooling. We just don't see any evidence of that. The way I know that is you can look in the window, but also we have a camera system that we can take in that we think would be stains on the cans, or whatever. So, we see no evidence of that.

The second question had to do with mass balance?

CERLING: Just what the mass balance of water is in the redistribution of water as the system dries out around the heating area, water must be being driven out, and I was wondering if it was actually accumulating in the lithophysal cavities, if you pass the saturation threshold.

PETERS: It's hard to resolve. Our techniques are low volume average techniques, and so it's hard to resolve that. But let me talk maybe a little more gross scale, and then maybe we can get a more detailed question. It does dry out, no surprise, around the heat source. We don't pick up perched or high saturation areas above. It tends to drain. So, the wetting is to the sides and below. So, we don't see any evidence of saturated areas building up. We see evidence of water redistribution. It's rewetting, on the rewetting into the matrix, as you probably well know, will take years.

So, I think if I'm answering your question indirectly, we don't see areas of saturation build-up where you'd see cavities full of water at a gross scale.

CRAIG: Don Runnells?

RUNNELLS: Runnells, ex-Board member.

Mark, you mentioned the potential payoff in terms of credit for the reduced solubility of Technetium and Neptunium and perhaps other things if you could demonstrate that the saturated zone is in a reducing condition. The potential credit is enormous, particularly with respect to Neptunium, my favorite element. What plans, what short-term plans, what long-term plans do you have to test that redox environment in the saturated zone? And then I have a short one about the UZ.

PETERS: Okay. Briefly, the history, we've done work on looking at Eh-pH conditions in the upper part of the saturated zone in the past. We also did work in association with the Nye County boreholes. But we have yet to be able to provide a sound technical basis to allow us to take credit for more reducing conditions. So, there are measurements. Are we looking at a program to try to still go after that? Yes is the answer. There's in the plan, and I need to caveat that heavily, it's in the plan and it's pending all the budget and funding that you heard discussed earlier, there would be some additional work, do some additional work-overs of existing wells to try to still continue to chase that. Because we certainly agree that it would be an important thing if we can establish that we've got a strong basis for it.

RUNNELLS: The redox measurements that you have now from the Nye County wells, as I recall, are sort of in the gray zone.

PETERS: That's right.

RUNNELLS: They may or may not be reducing.

PETERS: And that's a clear way of saying that we're still not confident that we can take real credit, really bring in a strong basis for the more reducing character.

RUNNELLS: But you do have some plans to even try work-overs of those measurements?

PETERS: Correct. And also other wells that we can go in and clean out and do additional measurements, because we haven't done all of our WT wells, our water table wells, that were drilled early in the project, some of those we might go back in to do additional--we have plans to go back in and do additional measurements.

RUNNELLS: Thank you. A very quick question on the UZ. Another process, potential process that has not been investigated in terms of the unsaturated zone is the precipitation of secondary minerals. I mean, I know it's been touched on and talked about, but is there any ongoing work in terms of secondary minerals being precipitated along fractures that would further retard or precipitate sorb of some of the radionuclides?

PETERS: Primarily during movement of material during the heat phase, redistribution during the heat phase?

RUNNELLS: I'm thinking long-term. Once a waste canister is breached, the material starts to move down through the underlying UZ, the potential is there for precipitation of those materials in secondary minerals. Is there any plan to investigate that, either short-term or long-term?

PETERS: Well, there may be some people in the audience better to address this, but I'll take a cut at it, and I'm sure somebody will correct me. Under an ambient setting, I wouldn't expect the secondary mineralization to be a whole lot different than what we're seeing now, which is primarily calcite and opal. That would be my take on it.

RUNNELLS: You're going to have a whole bunch of new materials introduced from the canisters and from the iron supports, and so on and so forth.

PETERS: But in terms of taking credit for that in performance base, I don't believe we're taking any kind of credit for that in the performance base. I'm not aware of any work to really try to bring that into the basis. That's a very good point, but I'm not aware of the work that we would need to bring that into the basis.

RUNNELLS: Thank you.

CRAIG: Mark, and then Dave.

ABKOWITZ: Abkowitz with the Board.

Mark, this is going to be more of a 30,000 foot question in terms of the entire experimental program. Obviously, the experiments have been going on for quite a period of time, and during that time, the repository design has been changing, and the impact of those design decisions will result, as I understand it, in the construction of the repository across more layers and a greater proportion of the repository will be sitting in layers other than what was maybe initially anticipated.

Given Margaret Chu's comment earlier about the importance of trying to increase confidence and reduce uncertainty as we move forward, what plans do you have to take the experiments that were conducted, or are being conducted in their current locations, and be able to transfer them with confidence to the repository design as it changes?

PETERS: So, maybe to rephrase it, how representative is the program that we put in place at this point to the rest of the block?

ABKOWITZ: Yes, and what do you have in mind to try to address those issues that are most uncertain as you look at the question of transferability.

PETERS: I'll give a cut at a 30,000 foot answer, and then we can go from there. We've got the ESF in place that does a pretty good job of running more south in terms of variability in the middle non-lithophysal. We've got a cross drift that goes across a reasonable section of the entire Topopah. We've got boreholes and outcrops that give us some confidence, I'd say relatively high confidence, that we understand the lateral variability.

When we excavate the repository, there will be mapping, observations made and compared back to the existing dataset. Then that will likely be part of, and I'll call that performance confirmation, PC, commitment in the license. I'm predicting an outcome, because we're in the process of developing what will actually be in the PC program as a commitment in the license. But, in all likelihood, mapping will be there. So, that's one way of getting at it.

There will be other pieces of the performance confirmation program. Some of this stuff may actually get mapped into that. Then there's also a long-term program outside of PC that will be in place. Some of these will continue there. We may actually envision doing additional seepage measurements in other pieces of the repository as we excavate. But I'm, and now this is me talking, this is still work in progress to define that program. But I think we've got a strategy that would allow us to look at those kind of issues.

DUQUETTE: Duquette, Board.

Yesterday during the tour, I think we were told that anecdotally some fungus and other things were seen inside the bulkhead test. That suggests the presence of nutrients for those species, and I wonder what's being done about bacterial growth in that area, or at least analysis of what bacteria are present.

PETERS: We took samples when we first observed it early on in the experiment, we immediately took samples, and those were analyzed, and those have been evaluated relative to the overall microbial programs. Joann Horn of Livermore was involved when we first saw it in helping identify it, and how that might tie into MIC. The ongoing sampling is more focused on safety, but in general, it's the same sorts of species. It's penicillin is what we're seeing. But those observations have been folded into the program. What are they feeding off of? They tend to grow on things that were left behind as opposed to things that you would expect in an ambient drift. So, it speaks to good housekeeping when we start putting waste packages in a drift.

CRAIG: Mark, thank you very, very much. You've covered, as you always do, a huge amount of materials, and we're precisely on schedule.

Okay, we'll next move on to Steve Brocoum. Steve, where are you? There he is. Steve Brocoum is going to talk to us on the proposed Science and Technology Program. Steve is an earth science geologist from Columbia University. He's been here many times. Welcome back. And you've got 15 minutes. I'll warn you at ten.

BROCOUM: Okay. Today, we're going to talk about the objectives, which Margaret did cover to some degree, what we've been doing to date, potential science and program activities for '03, and a summary.

These objectives, you know, Margaret, obviously her's are the correct objectives, but basically we want to increase confidence in the long-term. This is science technology is kind of off-line, it's not the core science program which you heard Mark talk mostly about today. This is talking for years or decades of ongoing science to increase confidence in the program. This will not feed a license application. It may feed, you know, depending on the results of particular studies, the license review process beyond the license application. The license application, in current terms, is just around the corner, December '04 is roughly around the corner when you're trying to put a license application together.

Similarly, this program is going to go on for at least 50 years, 100 years, 300 years. Technology moves on. There will be new materials. There will be new ways of monitoring. You know, computers will be better. We want to take a long view on improving technology.

Finally, we want to achieve efficiencies in the whole waste management system. We want to look at the whole system and see where we can achieve efficiencies, if we can spend some money in our program here and save EM, put money elsewhere, that's something we'll be looking at.

Finally, we want to continue to promote technical excellence and maintain leadership in the program.

These are some of the things we've done. There's a lot of infrastructure that has to be put in place to set up a program, and we've been working on that. We've prepared a management plan. We have missions and objectives, and so on, and we've prepared draft functional responsibilities.

Since we were formalized, or as a task force this year, we're not in the '03 budget. So, for '03, as Margaret said, the budget is unknown. For '04, we are a line item in the budget, but what that amount will be is yet to be determined by the Department.

We've developed management process for soliciting and tracking and evaluating proposals. We will probably formalize the process for asking for proposals, and when that happens, we feel we will be overwhelmed with proposals. And we want to make sure we're fair to everybody and we can evaluate them and give justification as to why we make calls on certain things.

And, finally, we screened a large number of things for possible initiation of '03. The formalization of proposals wouldn't happen until Fiscal Year 04 time frame. Here, we're saying what we do for '03, and we kind of did a lot of brainstorming on that.

One of the visions that we have is making sure over the long run, we get the proper balance, or the, you know, balancing the natural and the engineered system. The program has been under criticism for too much emphasis on the engineered barriers. Some of the suits that are pending in the courts address that issue. We want to take a long view on this and make sure we can define the performance we'll be getting of the natural system.

We want to, of course, look at technologies that may reduce costs, and we also want to develop multiple lines of evidence. That's from a comment from the Board. That even came up this morning I think that someone was asked that question. So, we want to focus also on multiple lines of evidence.

We want to encourage participation by external groups. You know, sometimes people accuse us of being insular. In other words, I've been on the program for many, many years and I'm certainly insular. So, we want to make sure we get some new blood, new universities and other research institutions in addition to the ones we have on the program already. And many problems are common around the world, like the saturated zone, and we have lots of opportunities for increasing our international collaboration, and Margaret has expressed a great interest in doing that.

So, in terms of our management process, we want to encourage new ideas. We visited, for example, all the--well, not all, we visited Lawrence Livermore, Berkeley, Sandia and Los Alamos, we also visited the USGS, and we talked to these people. We got a lot of good suggestions on how we ought to set up a program like this. We intend to visit additional institutions, for example, PNL, Argonne, INL, also have discussions with those people.

April Gill has been at the center of trying to come up with a process, you know, for prioritizing proposals as they come in, assessing them, and even having a process on line so people that submit proposals can see what their status is. Again, this would be for '04 and later.

Also, since, you know, Mark described the core science program. As things in this science and technology program mature and they may become something that could transition over to the mainline program, we have to have a process for doing that also. So, we're thinking of what our interface would be between the science and technology program and the, say, the licensing program. And we set up a science and technology advisory group that consists of the managers of all key areas of the program to meet periodically to discuss these kinds of issues. The acronym for that is the STAG. So, we will probably formalize the call for proposals probably for the '04 time period.

We started basically this year with activities that were proposed generally internally, and did not make the cut for a proposed FY 03 funding. Earlier in the year, you heard about Peter Swift's prioritization effort, you know, some of the things made it and some of the things didn't. We now go down to about 27 activities that if we got a reasonable budget for science and technology, we might have some hope of funding for '03, that met the objectives that I showed and Margaret showed earlier.

As the budget realities kind of--as we face the budget realities, we further cull the list down to about eight, and management decided to focus on five priority activities, and these are all still under consideration and still being discussed internally.

The first is the shadow zone. We've heard about the shadow zone for years. I don't believe we've taken credit for it in the licensing case now. This is an area that we would like to look at to see if the shadow zone really exists and to see if that would add to the performance of the natural system. So, that was the first activity.

Depending on the amount of funding, we may also be able to do other studies. When we visited Lawrence Berkeley, Joe Waring (phonetic) made a suggestion that one of the things we could do in addition to this is to look at tunnels that were here before the atomic age, and then he would drill up in the ceiling of the tunnel and drill down in the floor of the tunnel and compare the bomb pulse above and below. That might be something that we can do, and it's relatively low cost. It would be something we can do if we don't have much funding to help plan this test, for example, when we do get more funding. So, it's a possibility.

The second is flow and transport studies at Pena Blanca in addition to what the program is doing. This is, again, trying to look for natural analogues. The NRC has looked at this for many years. We have been trying to get a larger program going. Abe Van Luik has been giving us a lot of good advice on that.

The saturated zone. That's very important. There was some discussion a little while ago. That is an area that we feel that beyond the mainline program, we may do some studies there. I think Mark was talking about the oxidation reduction state of the saturated zone. So that's another area we may want to look at again. It all depends on funding.

Alternative engineered materials. We had a meeting with DARPA, it must have been two or three months ago, and they showed us some new materials. These are non-crystalline metals, amorphous metals that have some very interesting properties. Those may have some application to our program, so we are discussing with DARPA how we can work cooperatively to maybe look at this again for the long-term. Now, these may not pan out. But I think it may be worth looking. It's new material.

Finally, coupled processes, looking at the thermal, hydrological or geochemical or the mechanical interactions near the waste packages and around. That's been an area of interest for a long time. That gets into the high temperature versus low temperature operating mode. This whole area is an area I think that we could look at in the long-term to help the program make the best decisions that it can.

Now, before we decide what we're going to do, we would like to bring external parties in to help us evaluate more specific proposals. Margaret has expressed great interest in making sure we get new ideas, new people looking at this, and one of the people that she's talking about bringing on board is Bob Budnitz, who is sitting back here in the audience. He will be joining the program as soon as all the paperwork is put together as a senior advisor on science and technology to Margaret to help her and help us evaluate these kinds of proposals.

So, basically, in summary, we are working on establishing the program to address those key objectives. It addresses issues and alternatives beyond the licensing basis. That's really important. It's beyond the licensing basis. The mainline program is working towards the license application. They have their core science. They have their design. That will feed right into the license application. These kinds of studies are for beyond that. They may impact the program after the license is submitted, perhaps during the review process, and even much later.

We are probably going to solicit proposals and we will have a formal process for evaluating them. We will try to do it as open and in as fair a way as possible. We can already tell from the visits we've made that we will be getting lots of proposals. The word overwhelm isn't too strong a word, in my view. And we're looking for suggestions from the Board and from other groups as to directions we are to go look at and how to better implement such a program.

That's my comments today. Thank you.

CRAIG: Thank you, Steve. Questions from the Board, please? Don Runnells?

RUNNELLS: Runnells, former Board.

Steve, could you just summarize for us the current status of the studies at pena Blanca?

BROCOUM: I'd like to call Abe. Can you do that for us, Abe? The last I heard, you thought you had gotten the drill rigs across.

VAN LUIK: Abe Van Luik, DOE. The drill rig never made it across the border. We have cancelled that contract and we are now actively searching for a qualified drilling contractor within Mexico to do the drilling for us, and we hope to move forward in a matter of months in doing that.

RUNNELLS: Thank you.

KNOPMAN: Knopman, Board.

Steve, I'm just trying to understand a little bit better what the scope of your program is. You were really clear that you're looking beyond licensing. Is that correct?

BROCOUM: That's correct, yes.

KNOPMAN: In Margaret's presentation on Page 10, she has a bullet on the relationship of the science and technology program to licensing, and there are three bullets here. The first is may enhance confidence during LA review. Two, relevant information will be integrated into licensing related activities as appropriate. Three, some activities may be implemented through the performance confirmation program.

Can you take that and then help me cross-walk to what you said?

BROCOUM: Okay. What I said is the work we're doing, since we haven't even started yet, will not feed the license application. The license application is due in December of '04. So, to make an application, the work that's going to feed it has to be more or less defined in '03. So, I don't think there's any work in the science and technology program, even if we started work today, that could feed the license application.

18 months after that, submission of the license application, begin the hearings, the Atomic Safety and License Board hearings. At that point, we may have some information that could shed some light on issues that come up at those hearings and beyond. So, I said during the licensing review process, but not during the initial submission of the license application. Because the mainline program is trying to get a license application written for December '04. They have their technical basis to do so. So, we will provide information as we develop it for the mainline program to put in their licensing basis beyond submission of

the license application. So, in my mind, the demarcation is it can't be done for the license application.

CRAIG: Dan Bullen?

BULLEN: Bullen, Board. Actually, I want to compliment you on the five topics that you selected in your sort of winnowing down of ideas that may be applicable for the science and technology program. And I want to maybe come back to something that I mentioned when I asked Margaret questions this morning, in that if you identify a couple of these programs, and I'll just take, for example, the drift shadow effect.

A lot of the work that would need to be done would take more than a year or two to do, and so I look at stability of funding and types of issues that you run into, and as you run into the post '04 license application and you get into maybe construction authorization, and you look at the long-term, it's the concern that the Board has always expressed that you get something started that's a great idea, and then all of a sudden, the fundings gets real lean in some years, and I'm looking at how you'd want to preserve these kinds of issues so that the long-term goal isn't lost in the shuffle of we don't have enough money this year. And, you know, turning things on and turning things off you usually lose data, you lose people, you lose essentially momentum.

And I know Margaret is trying to institutionalize it, but maybe you could comment a little bit more on sort of the stability of the program, stability of funding, and how the long-term ideas are going to be fully incorporated.

BROCOUM: Well, let me start. I think Undersecretary Card is committed to this kind of a program and would like to institutionalize it. Margaret Chu has made very clear that she would like to institutionalize it, and she said she will make it happen. There's been talk of a percentage of the funding. There's been talks of various ways to maybe do this. I don't know how we'll do it, and so I can't give you an answer on that. But I think the term is long-term, like for example the drift shadow zone is a three or four year experiment, and many of the other ideas will be multi-year ideas also. So, it really won't work unless it can be instituted and continued for a long period of time. Exactly how that's going to happen, I don't know, because there's a lot of issues, as you know, with funding the program that Margaret went over this morning.

BULLEN: Thank you very much.

PARIZEK: Parizek, Board.

Steve, I thought that there was some credit taken for dry-out under the emplacement drifts, and this argues that you don't have invection in fractures and it really is a diffusion dominated process. And, if so, then the drift shadow enters into the performance assessment, and if so, that's a place you could drill and perhaps get data that the moisture content is lower or not in a year. Kind of the sites around, and this has been talked about off and on, but isn't that one of those things you could do, or in the act of drilling, would it take a while for the--but am I correct that you do take credit for this diffusion dominated flow?

BROCOUM: I thought for the LA. Are we taking credit? We're not taking credit for the LA, but I'll let the experts here--

BODVARSSON: Bo Bodvarsson, Lawrence Berkeley Lab.

There is in TSPA, and some of the people can correct me if I'm wrong, some credit taken for actually putting some of the waste diffusively from the drift into the matrix. If you do not have seepage into the drifts, then there is no water really to dissolve the waste and, therefore, the thinking is that diffusion is the only mechanism that can get you from the drift into the matrix.

However, there is no credit taken for that there is drier area of the drift because of the shadow, because actually the fracture will not flow because there is no flow there in all likelihood. So, that is not taken into account, but the diffusive releases are taken into account.

Did you understand that? Does that make sense to you?

PARIZEK: Yes. Do you have some candidate sites where a drift shadow could be investigated if someone asked you for one?

BODVARSSON: Yes, we have been looking at a few sites that are possible areas for a drift shadow being looked at. In actuality, you know, there are many, many sites you can look at. You can even look at an old building that was built many years ago, and you can look at that building and of course you will see a shadow. In my mind, the concept can be done in many, many places, so we are looking at different tunnels close to NTS, close to the Hoover Dam, for example, and also some areas in New Mexico for the concept, especially for drilling like you propose.

PARIZEK: As you visualize that experiment, that's not too complicated a time frame, or is it a year or two?

BODVARSSON: No, I think you're absolutely correct. That is not too complicated, because basically what we want to do is simply compare the chemistry of the waters beside and above the drift to that below the drift, with the understanding that the water flows around the drifts from seepage areas, that the chemistry underneath the drift is going to show longer ages and different chemical signatures to those around and above the tunnels.

DUQUETTE: Duquette, Board.

I'd like to address this alternative engineered materials issue. My understanding was that the science and technology advisory team that you're proposing would be post-licensing. Do you envision a change to a new material for the canister or the containment modules after emplacement starts? Because it seems to me this is going to have all kinds of other effects, including a change in the design.

BROCOUM: There has been debate on the program on this, and I don't think we want to exclude the potential of, say, repackaging later if you even have a hundred or a 300 year time frame, and materials will advance over those years. So, I think we're going into the license application with our current design. If we decide to change it, of course, we have to submit a license amendment to the NRC. But there's been quite a few discussions in this even at the Undersecretary Card level.

CRAIG: The corrosion of C-22 is not on the list. In fact metals is not on your list, and yet there have been many reviews, including one by the Board, that identified a number of possible mechanisms by which that might occur, and there still seems to be no agreement on how to extrapolate corrosion.

BROCOUM: I believe that's in the mainline program. If it's in the baseline program this year or a future year, it's not an S&T. So, that's how we've defined and separated the work. So, work is for the baseline, even in the out years, doesn't go to S&T. So, if it's in, and Mark Peters is the one that's mostly been involved in helping us separate what's in the baseline program from what should go in the S&T, and that's not always an absolute line. Many times there's some judgment there. But, in general, that's what we're trying to do.

CRAIG: Thanks, Steve. Any other questions?

(No response.)

CRAIG: In that case, we'll move on. Thank you very much. And we now turn to my part of the world, California, across the border to Inyo County. Andrew Remus of Inyo County and Michael King of the Hydrodynamics Group. Andrew's biography says he lives in Independence, population is 400. I haven't seen anything like that. I have to add that that is one of the finest jumping off places in the whole world if you're going to great country. Thank you, and welcome to the Board.

REMUS: Thank you, and good morning. I'm Andrew Remus.

CRAIG: I'll warn you when you've got five minutes to go.

REMUS: Okay, thank you.

I'm Andrew Remus. I'm the Project Coordinator for the Inyo County, California Yucca Mountain Nuclear Waste Repository Assessment Office. I thank you for the opportunity to present our Yucca Mountain Regional Ground Water Monitoring Program today.

I'm joined today by Mike King of Hydrodynamics Group. He's our primary contractor for hydrologic studies. And we have John Drewvers, he's our national affairs and policy consultant, with us today. And also Courtney Smith, he's my assistant in the Yucca Mountain program.

Inyo County has a great appreciation for the role that this Board has played in providing for the past eight years thorough and balanced oversight of the Yucca Mountain program, and we welcome the opportunity to speak to you.

Inyo County has been an effective unit of local government since 1991 under the 1987 Nuclear Waste Policy Act. The focus of our oversight program has been of both transportation of nuclear waste and radioactive nuclide transport from Yucca Mountain into Inyo County via the Regional Ground Water System.

We began utilizing affected unit funding in 1996 to investigate the possibility of a hydrologic connection between the Yucca Mountain site and California's half of the Amargosa Valley. These investigations led us to believe that there may indeed be the potential for radioactive materials to escape the repository block and be transported into Inyo County.

More specifically, we are concerned about the impacts on the major spring waters in Death Valley National Park. These spring waters serve protected habitat and domestic, commercial and tribal uses, and are the only source of usable water in the park.

As an adjunct to the 1998 Nye County, Nevada Early Warning Drilling Program, Inyo County conducted a three year investigation into water behavior in and around Death Valley National Park, including investigations in the Funeral Mountain Range. That program ended this last winter, and this spring, DOE awarded the county a $4.9 million research grant designed to explore by means of a series of deep monitoring wells, the possibility of a hydrologic connection between water resources in California and the Yucca Mountain project.

Mr. King of Hydrodynamics Group will present the County's current technical program, and our project team is available to answer any questions.

KING: Thank you very much. I'm going to focus on the technical issues on the program we've been developing since 1996. Let's state the problem, and then we'll go through our program.

We know for a fact that there's a lower carbonate aquifer system about 6,000 feet underneath Yucca Mountain. Modelling from the one well that has penetrated this indicated that there is a significant upward gradient in this lower carbonate system.

One thing we also know is that the relationship between this lower carbonate and the carbonate springs in the Furnace Creek area of Death Valley is not well known. And this is where, thus, our studies have been focusing.

To continue to state the problem, the hydrologic data we do have strongly suggests that the Death Valley springs are supported by inter-basin ground water flow through this lower carbonate aquifer system.

We think that some portion of that flow through the lower carbonate system may be from Yucca Mountain. We don't know, but we're certainly going to investigate that.

Let's get a picture of the problem we're trying to state here. So, here's Yucca Mountain. 40 miles to the south is the Funeral Mountain Range where we enter Inyo County, and then here are the spring flow systems. A number of the research models show various flow paths for ground water. Obviously, Inyo County is down gradient of Yucca Mountain, and possibly within one of these flow paths.

So, what evidence do we have for this hydraulic connection? A lot of the geochemistry of the springs in Death Valley are incredibly similar to the lower carbonate samples that we have over in the Amargosa and at Yucca Mountain.

When you map the structural geology in the southern Funerals and up through Yucca Mountain, we see that most of the models show the presence of the lower carbonate as this interconnecting bed of permeable materials.

Both the U.S. Geological Survey's regional model and the Nevada Test Site model all show flow paths going from the lower carbonate aquifer supporting this spring flow into Yucca Mountain. So, they have strong confidence that they think there's this connection.

Based on that, what's Inyo County's concerns? Obviously, we're worried about radioactive nuclide transport through the lower carbonate into the Death Valley springs.

On a side light, they're also worried that this lower carbonate aquifer with this upper gradient actually is a barrier to ground water contaminants from radioactive nuclides. This is a good thing. Now, through all the course, whether it's planned development or whatever, if there's any degradation of that upper gradient, it could certain impact the flows in the springs, as well as provide radioactive transport into the lower carbonate system. We think if it gets into that system, it's going to go south.

Let's talk about a grant program now, the program itself, or the problem. The program is to have the U.S. Geological Survey map the geology of the Southern Funeral Mountain Range. That program is pretty well done. It has one more year to go. Based on that geological framework model and current geophysics which we just finished, we would then put together a hydrologic framework model. We'll present a conceptual model today.

The next plan is to drill four deep wells into the lower carbonate aquifer system. Three of these will be on the east side of the Funeral Mountain Range. One would be in Death Valley National Park along the Furnace Creek Fault. We're also planning one shallow well, a thousand footer, at the Travatine Spring in Death Valley National Park. So, one of our key roles is we do want to penetrate the lower carbonate aquifer system.

Once we have the wells, we're going to pump test them, get some hydraulic properties, get some water chemistries, and then feed that information back into our hydraulic models to try and characterize ground water movement through the system.

This is Chris Fridrich's conceptual geological framework model. You have the lower carbonate system in the Southern Funeral Mountain Range. You have some corsite materials to the north. To give you a reference here, these are the major springs we're talking about, Navas, Texas and Travatine Springs at the Furnace Creek Ranch area, which I think if you're going on your field trip, you'll stop by.

On the east side of the Funerals, we have one outcrop of the paleocarbonates. At this point, we'll have some discussion about that point here. So, the question is how is ground water moving through this system?

The next slide will show you a detail of the east side of the Funerals. We just completed our geophysics program which involved time domain magnetics and gravity surveys. Seismic reflection was not going to work because of the steep nature of the faulting. So, we have a number of lines, one of them running right through the outcrop sections in this direction, as well as through here. Remember, this whole area is wilderness, so we had to leave no imprint on the land when we did our studies.

There is one private sector of land within the park, which is one of our target locations, and between this point and this point, a well location is non-wilderness. This is park wilderness. This is BLM wilderness. So, we're kind of in a box.

Here's some of the results of one of the lines, just to illustrate the kind of work that we're coming out with. We actually started measurements at the outcrops in the mountain range there. We went out to where the outcrop, the blue being the lower carbonate signature. As we step off of that, we come over to this private property section, and we find that there is a portion of the lower carbonate sticking up at a depth of maybe 200 or 300 meters. So, we're looking at some deep wells to penetrate into that system. So, this is just one of our target zones.

We've done this geophysics through the whole program, and now we're feeding that back into both the geological and hydrologic models to help improve our understanding.

This is all a conceptual hydrologic framework model through the southern Funerals. This framework was put together first by Chris Fridrich, who did the geological modelling. So, I had him project down to the structurals, on the surface and the subsurface. We then added some of our geophysics out in this area.

What we find is we have a low permeability, low being very low, almost a barrier to ground water flow through the central mountain. But what we do find is that there are some spillways or flow paths through the mountain range. These are some suggested pathways on how water would pass through the system into the springs we just talked about, Travatine, Texas and Navas. There's also the Naval Spring, so there may be a potential pathway for flow through a lower portion of the mountain range.

So, John Bredehoeft, my partner, decided to try and model the system to try and characterize can water flow through this mountain, and then how does it help us in defining where we drill our wells. He took Chris's geological model which you just saw. We take the total flow from the Death Valley spring system, which is measured over a 20 year period, so we have a good record on that total flow.

And then based on that flow, John back calculated what the transmissivities would have to be, both through that spillway to the north, and then what it would have to be going down into the spring system. So, you see some pretty low permeabilities.

So, based on that, John goes in and he does his model, and what we see is you have a hydraulic head here of about 2,000 feet in the areas we're going to be drilling. The springs are at sea level. And based on that head and this flow path, you can actually--water will pass through this system, and it looks like we do have the connection we were talking about.

Now, to illustrate it a little more, here's the Furnace Creek Fault range, here's the area of essentially no permeability, and this is how we have this incredibly high head pushing water through this lower permeability material, which is reflected in the Death Valley springs.

To recap a couple of points here, we know the lower carbonate exists. We have a well that's penetrated it at Yucca Mountain. We know there's an upward gradient based on our thermal modelling of that gradient. Death Valley springs are most likely supported by inter-basin flow through the lower carbonates. The chemistry of these springs are carbonate springs. We know that.

Lower carbonate flow path most likely exists through the southern Funerals. This is based on our surface geology, mapping, that it exists, and that our hydraulic modelling shows that there is this scenario that would allow it to flow through the mountain range.

Another thing that we want the Board to consider is the maintenance of this upward gradient in this lower carbonate aquifer. If you keep the integrity of that upward gradient at Yucca Mountain, you certainly have something that adds to the natural barrier to transport into the lower system.

We also worry that if that water table drops in the lower carbonate, it will certainly impact, based on our modelling, the discharge in Death Valley National Park. And that is their only water supply for a million visitors a year. Thank you.

CRAIG: Thank you very much. Questions from the Board?

PARIZEK: Parizek, Board.

Have you ruled out the possibility of a flow around the southern end of the Funerals, and then up the fault, Furnace Creek Fault, and just not had much seepage through the mountain at all? You have these spillways that are postulated, and you're drilling, they prove that out, if you drill and don't find much water going through the spillway areas, then you're still looking for some other pathways.

KING: Well, that's been suggested that there is a pathway down by Death Valley junction, up through that area. We did some geophysics over there in support of the National Park Service drilling program, part of the tribal drilling program, their monitoring. We found the lower carbonate there, but the Furnace Creek Fault is an incredible barrier, we think, to ground water flow in that area. So, we think that the heads are very high to the north, and that the gradient may be to the north.

CRAIG: Dan Bullen?

BULLEN: Bullen, Board. Actually, could you show Slide 12, get back to that real quick? The one before that. I'm sorry. This is actually a question that Mrs. Devlin whispered in my ear as the slide was going by, and so I'll take some partial credit there, but it's sort of valid.

I know your understanding is to try and take a look at the deep carbonate aquifer, but the question that arises here is you're showing Amargosa Farms area as--you know, where is the Amargosa River on this and what kind of impact would it have on the type of flow field that you're looking at?

KING: The Amargosa River is--well, this is a fault running through here, and the Amargosa River does pass through in very close proximity to one of our sites. There's a number of shallow wells in there, and part of our program is to rehab. those wells and try and characterize what that chemistry is in that upper zone. So, we do want to pay some attention to it. Maybe in the future, and if money allows, we'll put in an intermediate well in here in the tertiaries.

BULLEN: Thank you.

CRAIG: Other questions? Don Runnells?

RUNNELLS: Runnells. You mentioned that you're going to take a look at the chemistry of the water. You heard the discussion earlier today with Mark Peters about the importance of the redox parameter of ground water. If that ground water in the deep carbonate aquifer is reducing, the solubility of things like Technetium and Neptunium is diminished enormously. So, my question is are you going to look at oxidation reduction potential in those deep ground waters?

KING: The program we've looked at for geochemistry is to try and characterize the source of the water, where it's coming from. So, the program so far has dealt with major anti-cations, and then we've had some support from the USGS on doing oxygen, strontium and uranium, again, which go to source. We've done some Carbon 14. So, the chemistry beyond that has not been done, other than we did calculate pH and conductivity and other parameters.

RUNNELLS: If I were working for DOE, I would come to you and say may we please measure the redox potential in your deep ground waters.

KING: We are offering these wells, we're going to put it out on the internet, and they're open for any and all scientists to take samples. So, that is part of our program, to allow UNLV and others who are more capable than we are to do that.

CRAIG: Debra, and Richard.

KNOPMAN: Knopman, Board. Could we just see the last slide? I just want to pick up the last bullet of the last slide in the summary of your key points.

You express concern about the maintenance of the upward gradient in the carbonate aquifer. Is there any way through historical records that you can trace changes in the gradient just by virtue of increased pumping in the Amargosa Valley, for example? Or is there simply not enough penetration--

KING: We only have three wells. One of them at Yucca Mountain, Bredehoeft Well, which was an exploratory well that's plugged, and then Nye County just completed a well. One thing that we do see in all of the Nye County wells is we're seeing this carbonate signature in a lot of their samples, which never penetrated the carbonate. So, we do think there's this upward gradient. It's an interesting question. It's a little bit beyond what we have data to evaluate.

KNOPMAN: Well, I'm just wondering what you think they could be doing that would substantially change the--

KING: Well, we do know there a, when you look at the record at Devil's Hole, there seems to be a correlation between that and pumping in the Amargosa, and that is a source of carbonate water. So, there seems to be some relationship to that development and what flows we see in the lower carbonate or what supports you see from that system.

PARIZEK: Parizek, Board. As a follow-up question related to that same topic of maintaining an upward gradient in order to protect the Death Valley springs, and really that's more a withdrawal problem, it seems to me, how you keep people from tapping into that aquifer and maybe pulling down the head somewhere in the Amargosa Valley area. Amargosa Farms wells are all to be shallow, as I understand it, and they may or may not affect the upward gradient as a result of increased withdrawals in the future. But deep wells in the carbonate aquifer would be an issue that would be your concern, and is that a Board issue for us to discuss?

KING: Well, it could be a policy issue, what ground water use you allow down gradient, or in this system. With the increased growth, there's been some suggestion, and I'm not the one to say who or what, that they might go in and actually start mining water out of the lower carbonate to support growth in this area. Whether that's going to happen or not, it is certainly a policy issue that should be considered, that maintaining what barriers you do have may be a Board issue, whether it's policy or how you do that with the State Engineer's Office, I don't know.

CRAIG: Jeff?

WONG: Since I'm a consultant, I'm going to ask a question.

I'm looking at again the diagram that Dr. Bullen had you put up, the flow paths. This is not a scientific question. This is just sort of a policy question, if you'd like to talk about policy. But if in fact according to the DOE's projections at Amargosa or their point of compliance, their estimated dose at 10,000 years complies with the regulations, why would you care what the dose is, or why would you care about doses that are farther down gradient in Death Valley?

KING: Because of the large permeability of the lower carbonate aquifer system, those flow paths may actually be fast paths. You know, some of the earlier modelling we did, and it hasn't been verified, we're seeing between a recharge bed in the Amargosa and discharge in the springs, 25 years. So, that's short. So, again, more work needs to be done, because we're the first to start looking at the regional issues in this detail, so we're suggesting more work.

WONG: So, I mean, what you're saying is that you could have higher doses down gradient from the compliance point, or equivalent doses?

KING: Well, Ii have no way of saying that yes, there is or no, there isn't. Is this a potential fast path? Yeah, I think it might be based on what we've seen in the discussions, and obviously some of the modelling will help to see if it's even an issue.

WONG: If you have higher flow rates, would you expect greater dilution?

KING: I don't know. I'm not a geochemist.

WONG: You know, I'm asking the questions and I'm not a hydrologist. So, thanks.

KING: Sorry.

CRAIG: Okay, I think this means we're at the end of this session. Thank you very much. I understand from our chairman that there are some public comments, so our procedure is that the chairman handles the public comments, and you can then tell us when to come back from lunch.

CORRADINI: Right. Thank you, Paul.

I've been informed that there are five individuals that cannot stay for the public comment period this afternoon, and wanted to speak now. Given the number and the fact we want to give everybody about an hour for lunch to get back to the afternoon program, I would request that we keep the public comments to less than five minutes. I'll just go down the list and call up the individuals. If they could just come up, identify themselves. If they're representing an organization, tell us that. Otherwise, if they're representing themselves, and come to either one of these microphones.

The first one is Judy Treichel. I hope I pronounced that correctly.

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force. Just very quickly, two points on the things that were said this morning. In Don Horton's presentation, it showed the ever elusive design, and that worries the public that there is not a firm design, and the DOE is not going in to NRC and saying here's what we want to build, this is it. And then if they need to change it later, or if they assume it will change, it's too early to put in a license application. If it turns out they need to change it, they can amend the license or make arrangements later.

I was concerned also when Margaret mentioned early and often NRC interactions, with NRC/DOE interactions. Those should not be done without public notice and public accessibility to those. Likewise, if DOE wants to meet with the Board, we believe that that should be a public meeting.

What I wanted to say, and I am staying and I'd like to say something later on as well, but I didn't want any of the Board members who are leaving to leave before I had a chance to say that I want to give a very, very heartfelt thank you from myself, from the Nuclear Waste Task Force, and from the public that we advocate for for the wonderful public participation opportunities, unique and wonderful public participation opportunities that you have given to us.

You have listened to DOE. You've had the discussions, just as you have today, but you have been the only group that we've ever been around, or that exists, that have allowed us to come into that, and when there were important things, you actually allowed for interactions between us and the Department of Energy. We could ask our questions. They were either answered or not. But it was on the record. And that's unique and I would certainly ask that it continue.

People have come to expect the TRB to ask questions during technical presentations, as happened this morning, that reflect the public concerns, and a good example of that was some of the go-arounds that have gone on about whether or not doses that are being stated have been probability weighted to make them look less than they would actually be. And we have really appreciated that, and it's really led to some great understanding.

There are terms that are used that even seem very simple, like risk benefit, safety, and those terms have to be identified because they mean very different things to different people, and I would also urge the new members of the Board to continue doing that.

And just, finally, once again I really want to thank all of you who are leaving. We in Nevada will miss you very much, and I implore you, please, keep in touch. You know how to get ahold of us and we would love to be able to do that.

Thanks so much.

CORRADINI: Thank you. Our second commenter is Mr. Joe Carson.

CARSON: Good morning. I'm from New York, so I can speak quickly. But I'd like to cover my information. I think it's quite relevant to the purpose of this Board.

My name is Joe Carson. How many people recognize my name? I'm the first and only eight time prevailing and still aggrieved DOE Nuclear Safety Engineer, still employed by DOE, still in the Q class, and DOE is still ignoring my safety issues, which is why I'm now an eight time prevailing whistle blower. DOE has paid over $400,000 in my legal bills to date.

I have to disagree with Ms. Chu and Mr. Horton about safety conscious work environment. I must give DOE credit. Ten years ago, DOE's nuclear safety rules were explained to me very clearly. Three rules. One, in DOE, if it's legal, it's ethical. Two, if DOE gets away with it, it's legal. Three, the only right a concerned professional has in DOE is the right to seek employment elsewhere. So, those are DOE's safety rules. I think any objective review of my case would show they've been applied again and again and again. Still as today, will not even address the safety issues.

I did not become a whistle blower to have the experience of prevailing. I became a whistle blower because of safety issues. DOE's response is we weren't ordered to address your safety issues. We were ordered to pay your legal bills. We will not address your safety issues. The same DOE representing here safety at Yucca Mountain.

I'm a licensed P.E. I have to critique the presentations here and also the Board's website. Being a P.E. is a legal status. I have a legal obligation to blow the whistle when necessary, similar to if I'm a law enforcement officer, I see a crime, I just can't look the other way and be a law enforcement officer. A member of the public does not have to get involved. As a P.E., I see something wrong, DOE suppresses it and it's safety, I have to blow the whistle or I have to resign. That's the rules of professional conduct of the State boards across the Nation.

Because being a P.E. is a legal status, I have to at least comment. The Board's website does not make clear its members who are P.E.s, because being a P.E., again, it's a legal status. Legal encumbrances follow from that status.

My thought for Yucca Mountain and for DOE is that it can't be adequately safe unless the safety professionals for DOE, the contractors, the work environments--well, number one, the safety professionals are trustworthy. By trustworthy, I would mean ethical, competent and accountable, and they're working in a safety and security conscious work environment. I don't think that either case is true in DOE across the board.

I know I'm running out of my time. I just want to hit the high points. Some recommendations on the last page of my memo that I would ask the Board to consider.

I was involved in an accident investigation about eight years ago at a DOE nuclear reactor. There had been a fire, and the DOE represented that the facility that burned was not a nuclear facility, even though it produces high-level nuclear waste destined for Yucca Mountain, because before the uranium in the facility was exposed to neutrons, it wasn't that hazardous. So, my question is why do we need Yucca Mountain? We can store new nuclear fuel in a garage, and DOE's point is it's not that hazardous when it's new.

I mean, with respect to an accident that contaminated a number of workers, contaminated a nuclear reactor, and caused a measurable release to the environment, DOE's response was that facility didn't merit a safety analysis because when the uranium was new, it wasn't that hazardous.

So, I'd request the Board have DOE do a different professional opinion on the point. DOE has now made this representation to the President. I took it as far as I could go within the system, and DOE is now on record in a report to the President saying a facility that produces high-level nuclear waste is not a nuclear facility.

Now, I do mostly worker safety inspections. Two years ago, Congress passed a law, and the President apologized to DOE workers for being put in harm's way by DOE without their knowledge and without having protection. DOE treated these workers as expendable, is my point of view. I think it's a workplace self and safety disaster of national scale that hasn't been owned up to yet by the safety professionals in America, including the National Academy of Engineering, that I would allege played a role in this disaster similar to that of Arthur Andersen at Enron. NEA took many, many millions of DOE's dollars to do reviews related to workplace and public health and safety in DOE, and never once mentioned the existence or relevance of P.E. licensure or the code of ethics for engineers to the matters being observed.

When you're an engineer, you're always an engineer. You have to adhere to the code of ethics. Part of the code of ethics is you see that other engineers do, too. It's implemented on a strict honor code. That's part of the law.

I'd make the same request to you with the NWTRB. You have some P.E.s on your Board. How is the NWTRB recognizing and accepting your legal responsibilities? I have not seen anything. I've stamped my piece of paper. I didn't see anything stamped by any of the engineers presenting anything today. If they are P.E.s, I think they have an obligation to stamp their reports. That's what the licensure rules require.

If anyone thinks I am out of bounds, please--I really want to get this issue identified and resolved, what a P.E. owes as his professional duty to the public and workplace self and safety, and what the profession owes to a P.E. who risks his life to do his duty.

I've approached Senator Harry Reid (phonetic). I've asked him to put a hold on Carmen Slateral (phonetic), the DOE Deputy Secretary, he's a nominee to be DOE Deputy Secretary, put a hold on his nomination, which means his nomination will not come up for a full vote until I've requested Mr. Reid have either DOE go forward with the settlements or agree to have a third party look at my case and make recommendations to resolve it, including the safety issues.

My point is trying to settle with DOE is like trying to settle with a kidnapper or a terrorist. They're trying to ruin my life, and if they say oh, have we--are you ready to quit now, Joe, it's a very cruel game. I think everyone in this audience and the public in Las Vegas are partially held hostage by DOE's cruel game.

Thank you.

CORRADINI: Thank you. Our third public comment is by Ms. Peggy Mays Johnson.

JOHNSON: It's hard to follow somebody seven feet tall.

My name is Peggy Mays Johnson, and I'm the relatively new Executive Director of Citizen Alert in the State of Nevada. So, this is my maiden voyage before you all.

I usually start my presentations with a few disclaimers. One, I am not a scientist and, two, I am not a transportation expert. However, I do bring 30 years of political experience, which I believe qualifies me as an expert as far as where the Yucca Mountain project is at this point. The decisions made thus far have been solely political, and now we, the people, look to you to make somewhat belatedly, albeit the scientific determination. You see, this is not just a Nevada problem, as Dr. Chu rightly pointed out when she discussed the transportation challenges.

In 1987, Citizen Alert established a national nuclear waste transportation task force, because we realized that there were those transportation challenges that finally the Department of Energy is recognizing. We believe that you, the Nuclear Waste Technical Review Board, must, and I emphasize must, spend an appropriate amount of time on those transportation challenges.

We, Citizen Alert, is challenging the Department of Energy and the Nuclear Regulatory Commission to determine the transportation routes as soon as possible, and to hold hearings along those routes, in order to develop an environmental impact statement to support those routes and to please tell the truth.

I have been interviewed by many members of the international press since I took over in April, and I must tell you the world is watching us, and the world will be watching you.

Thank you so much for your determinations.

CORRADINI: Thank you. Our fourth comment is from Ms. Abby Johnson.

ABBY JOHNSON: My name is Abby Johnson, no relation. I'm the nuclear waste advisor for Eureka County, Nevada. Eureka County is one of the ten affected units of local government, for those of you new to the Board, that have been talked about on and off today. Inyo County is one of our AULG colleagues.

The reason why Eureka County is primarily concerned about the Yucca Mountain project is because of the possibility of a rail spur being built through Eureka County that would transport nuclear waste through our county and would be operated for the life of the project, or even longer.

Three things. I want to thank the outgoing Board members, and I know people have already said this, but it bears repeating. Thank you to the outgoing Board members for your willingness to ask the tough questions, and to pursue the answers, and especially Dr. Cohon, your patience in allowing public sessions and listening to the public and making sure that that happens at every meeting. I really think that's a great thing that you've done, and I just want to say that.

The forum that this Board has provided has been and continues to be most needed. I believe that transcripts from your meetings will serve future generations as a historical record of how Yucca decisions were made, and how the key questions were answered. I think that's when people say how did they do this, and then you go back to the TRB meetings and you see how that dialogue unfolded and how those questions were answered.

Now that the transportation is finally on DOE's agenda, I would encourage the Board to engage in the transportation issues, which is part of its charge from Congress, and to apply the same level of concern to transportation that you've applied to the repository issues. I think now is the time to start doing that, and as a transportation county, we would certainly appreciate your full attention to that matter.

Thank you.

CORRADINI: Thank you. And I was told we have two individuals, Ms. Sally Devlin, a member of the public, and I can't read the second name. I apologize.

DEVLIN: Thank you, Mr. Chairman, members of the Board, members of the Staff, and many, many dear friends, and we welcome you to Nevada. And we thank you not only for coming time and time again, but we're going to miss many of you really, especially Alberto and Jared. So, I can only say we appreciate the time and effort that you've made to come here and disrupt your life, but you're welcome into our lives.

And I will say I am Sally Devlin from Pahrump, Nye County, Nevada, and I life in the shadow of the test site and Yucca Mountain, and I'm here ten years, going on ten years, and at my very first NWTRB meeting when John Cantlon was the chairman, it was in the basement under Cashman Field, and there were 400 or 500 people there, and it was a brainstorming session. And I sat in the room with 40, 50 people. On my right, was Russ Dyer, on my left, was Bob Loux. To the left of us were seven Indian tribes, and they said don't worry about Yucca Mountain, it's never going to happen, we're going to cure all you white guys. And that was my introduction to Yucca Mountain.

I am so delighted, and I really do mean delighted, having read all those millions of pages from NRC on the licensing, that you have to talk to the Indians. That's on the next to the last page. It's in my report.

The other thing is I am so delighted that you finally, after all these years, recognize that Yucca Mountain is on the test site. So, I would like a request, and I see that our Director Chu is there. One of the things the public doesn't know is how to communicate with anybody. I have never seen a flow chart. I did get one from EPA. I've never seen one for NRC, and I've certainly never once seen one for DOE.

And the reason I'm standing here is it is so disturbing to us, the public, who are going to be totally affected by this whole process, which I think is already three years behind, according to the GAO report, but I really feel that you have not communicated with us fairly. And by that, I mean you are DOE. Now, you're not talking to DOD who's going to put 7,000 metric tons of classified waste--and I will tell you flatly, as I have at every meeting, you can't put classified waste in my mountain. Because if you try to, how are you going to prevent my bugs from eating the canisters? You didn't mention anything hardly about my bugs.

If you've got fungus, you've got my bugs.

And I would like 9 million, you know how I asked Ken Hess for 100 million for a hospital, we may get one, and I hope Bechtel will work with them, but you don't mention a word about the canisterization. You don't mention a word about the bugs. And as far as I am concerned, unless you test the bugs on the canisters in situ with microbiologists and engineers, or rather geologists who have never worked together in their lives, you don't have a valid claim to the proper testing.

The word I hear, and I love it, from our friend Don Horton, and he used my favorite word a dozen times, model. To me a model is the front page of Vogue. I have heard it for ten years, and it seems all that you do do, you don't do in situ. Where do these reports go? I've told Dan about the Carl Stetsenbach's (phonetic) report of the chemistry at Stovepipe Wells. He never found out how old the water was of the flow. Where did the report go that cost $3.2 million? I don't know. I hope others will look into it, because it does have the chemistry.

But this is my point, and that is I want a flow chart. To me, you should be working with DOT. That's how I got into this. Because at the time, the only route you had to put the railroad through was through Pahrump, and I said over my dead body are you going to do that, and that's when I went back to school. And, so, we had a lot of fun.

The only other thing besides the flow chart that I think you could do is communicate with one another. And before I leave the podium, I do want to say something. I want to thank the Board a million times. I've been with three different Boards, and I shall miss those leaving, and I shall welcome the new ones. But the most I miss is if you remember last year on September 11th, we were all at the Crowne Plaza on the day of the tragedy, and that was a very sad day. Many people couldn't get out of town to go home, what have you. And I think it affected us all, and I do want to express a moment of sadness, of fear, because our biggest fear to me is terrorism, especially on transportation and canisterization, the whole thing. But I really feel we owe you a debt of thanks for coming, and that we owe those who perished, and what might come from this, a debt of thanks.

CORRADINI: Thank you.

HUDLOW: I'm Grant Hudlow. I'm a chemical engineer and CEO of Allied Science, Incorporated. It's an environmental company. And I also belong to ENRAN over at UNLV. I have extensive work here already just from this morning, and I'm going to write that to you. I do want to highlight just the, because I think it's important that you consider, there's some things missing from this conversation.

The first thing that's missing is that it's illegal to put more pollution in a polluted area. That's EPA laws. The second thing I want to mention is that the transmutation option is not being considered. The NRC has said that if somebody will do that, they will consider getting rid of the waste through transmutation. Transmutation has been clearly demonstrated since the Sixties can turn the waste, the 70,000 tons of waste, into a trillion dollars of electricity. It seems like a no brainer, rather than throw this stuff in the water, especially near my house, that a trillion dollars would be more beneficial.

The other thing is that there's a French study of a French satellite by the USGS, and it shows a compression fault that goes from Pahrump through the Amargosa Valley, and unfortunately, they stopped up at Lathop Wells, and the upwelling of the carbonate that you're talking about comes up there and provides half of the water for the Amargosa Valley according to Clauses (phonetic) studies anyway from UNLV.

There's also another carbonate spring that goes up in the middle of the test site through a volcanic area. So, the upwelling that you dismissed so handily earlier is still there, and it's just a few miles away from Yucca Mountain that it's broken through. That carbonate spring interestingly enough is 2,200 meters deep, which because we're at basin range, the basin is sinking and is very deep, but there's still a connection between the carbonate that's over beneath Yucca Mountain.

The main thing I wanted to mention that's missing from this is also a violation of Congressional law, results management is missing. And for technical people, we tend to ignore that. I can say from personal experience, it's very painful to get into that. I can say also from personal experience that it expanded my technical abilities by an order of magnitude, and I don't really need to pay too much attention to agonizing over technical details now. They just come to me, and it's because the people skills increase the use of the technology.

Unfortunately, Congress didn't put an enforcement mechanism into results management, and so we're dependent on the GAO, which are also bureaucrats, to write about something that they know nothing about. The place to see results management, there's a few consultants who are pretty good at it. The industrial turnaround experts are the ones that are brilliant at it. Lee Iococa is an example. I don't see that kind of an approach anyplace in the Yucca Mountain project and DOE. Al Alms (phonetic) was another expert. When I asked him why he wasn't spreading that to the other DOE people, he said, Grant, I don't have time. I have to get money from Congress. Well, that's a terrible shame to have a resource like that that's missing and lost.

Thank you.

CORRADINI: Thank you. I wanted to remind others that may have public comment that we'll have a period at the end of the day. Please sign up in the back with Linda Hyatt or Linda Coultry.

It's approximately 12:15. We'll reconvene at 1:15. See you then.

(Whereupon, the lunch recess was taken.)

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

CRAIG: Okay, could you take your seats, please. We're about to start the afternoon session. Board members, please, especially. And the rest of you--would you folks in the back of the room please sit down? Chris Whipple. Chris Whipple is observed to be interfering with a board member. Okay, good.

The afternoon session now begins and this first session is what I particularly enjoy being Chairman of because I get to introduce Bob Budnitz. I'm not a nuclear type of person and so I haven't known nuclear people for all that long, but Budnitz didn't start out that way either. He started out as a physicist. A Harvard physicist at that, which is a very good thing and along about 25 years--or maybe it's 30, I don't know--we got to know each other and we keep running into each other periodically ever since. Most interestingly, in the men's room at the Munich Airport. It was one of those random things that happens. More frequently at the Berkeley Rep

Theater. Anyway, be that as it may.

And Frank Spera from Santa Barbara. So, yeah, it's time for you to go now. And you've got 15 minutes. You get to divide it as you wish and I'll warn you when you've got about five minutes left, Bob.

SPEAKER: Oh, we're in transcript mode here so that people who matter will hear.

SPEAKER: It's not on yet, so--

CRAIG: Now. Is it on yet?

BUDNITZ: Yeah. Okay, I said it's going to be two and 13.

This panel was impaneled by DOE, actually by, I think it was Bechtel SAIC, in May. Six panelists. The other five are earth scientists and volcanologists. I'm not. I was chosen to chair this because I knew something about the Yucca Mountain project, which the others didn't know much about. And this is the cover page of our interim report which was published last week and anybody that wants it can get it by asking, I suppose ask Gene Runkle or ask the project or ask me.

Six members, Frank Spera, who is here, is going to give the talk, from Santa Barbara. But Emmanuel Detournay, from the University of Minnesota, Larry Mastin from the U. S. Geological Survey, F. E. Pearson from Britian, and Allen Rubin from Princeton are the others.

And it's their report, they are the volcanologists. And all the technical content in that report, which is an interim report, is theirs.

I'm going to drop off of being the chair of this after this presentation. And the reason is because, as you heard earlier, I'm about to take a position with Lawrence Livermore with an assignment to go to Washington for two years to support the Science and Technology Program, and I can't do this at the same time.

I'm just going to do one more slide and then turn it over to Frank.

The charter. To review the technical basis used in the analysis of the consequences of these events, and to recommend any additional tests that would significantly strengthen the program. And what strengthen the program means, in plain English, is that it would significantly enhance our--that is everybody's--understanding of these events, meaning igneous events that could affect the repository.

So with that I'm going to turn it over to Frank Spera. He's from Santa Barbara. He's an earth scientist and he is going to talk about the principal lessons and insights from the interim report which was published last week.

Oh, one last administrative thing. You can say it.

SPERA: What should I say?

BUDNITZ: Sorry. The interim report was published last week. The panel has a final report due at the end of the calendar year. If it isn't the end of December it will be in January some time, in which all this is going to be wrapped up with this book, A Work in Progress.

SPERA: Thank you, Bob. Yes, actually, I believe the final report is Feb 1.

BUDNITZ: Okay.

SPERA: '03, and a public meeting second or third week in February.

Okay, what I'm going to do is go through a little bit of the technical detail, and then try and summarize it. This is an interim report. As Bob indicated we had a kick-off meeting in May so we've been working on this for the last four months or so. And I'm going to try and summarize the salient aspects of our work to date. And it is--and I want to emphasize, it is interim report. We are still working on this.

The first point is the--we can actually use the volcanological history of the crater flats volcanic zone, the volcanic region proximate to Yucca Mountain Repository, and in particular we can spacially look at the crater flat zone and in terms of time period, we could look at the last five million years of geologic history and essentially use the past as the key to the future to try and predict, at least bound and estimate. Where do we want to put bounds on? Well, we'd like to put bounds on a number of things. Number one, the style of the potential volcanism, what we call the eruptive chronology. Volcanoes, each volcano is unique, specific, has a specific eruption history, characterized by variable products and variable intensity. All right. And we can use, we can look at previous volcanism and crater flats to put some bounds on the expected modes of behavior.

We also need to estimate volumes, characteristic volumes of eruption because, clearly, our mandate here is to study the dike, drift interaction. And so we need to know what the volume of the possible eruptive volumes are. And we could put some limits on this based on geologic history and a number--a reasonable number, something like a tenth of a cubic kilometer.

As far as eruption durations for this style of magmatism, the style of magmatism, we anticipate that potentially could happen. The rates, the magma flow rates divided by those volumes gives a characteristic period, right? And the periods of these kinds of eruptions are measured in days or weeks or months. That's the scale. That's the time scale.

The other point is that by studying the volcanology and igneous petrology of the crater flat salts we can specify what we expect volt composition, devoid of the volatiles--volatiles are water and CO-2, which are very important--but we can specify the volt composition of the magma. This is important because if we're trying to understand the fluid dynamics of the magma drift interaction, we need to know the thermodynamic and transport properties of the magma. And unlike redwood trees, if you've seen one magma, you have not seen them all. All right. They are very specific. And we are very confident of this composition, that the potential disruptive Yucca Mountain magma would be alkaloid salt.

And finally we can also use the theologic history to put some bounds on the water and CO-2 content of the magma. This turns out to be the critical variable because magma scan a wide range of properties depending on the dissolved water and dissolved carbon dioxide content, so we need to know this number. And the status of this is that we basically understand this pretty well. The previous DOE work is fairly robust in this area. Some refinements are possible, in particular trying to narrow the range of the water CO-2 ratio of volatiles dissolved in the melt. That's a refinement. I think we have a pretty good handle on this.

The second point of the interim report is that we can use available thermodynamic and transport property models to determine the state and specifically the properties of this potentially disruptive magma. All right, so in other words, once we fix the composition and the volumes, we can now use some thermodynamics and try and understand the state of the material. And, this is really critical because the--basically in intuitive terms, pipe pressure or even a few kilometers depth the magma is, most of the volatiles are dissolved in the magma. So if you're looking at this material it's essentially a one-phase quasi-incompressible fluid discus. But as the magma decompresses, the dissolved volatiles come out of the solution, they go into the vapor phase. This vapor phase is quite compressible. All right. And so the state of the magma changes in proportion to the volume fraction of vapor in the mixture.

So, in other words, to summarize, a magma is a mixture of melt, which is essentially a liquid, plus vapor. And the fraction, the volume fraction of vapor to melt varies rather strongly in the last few hundred meters of ascent. And that determines the state of this material and so it's very important to know what the volatile content is. And this table here just shows different total dissolved volatiles, the total weight percent of H20 plus CO-2 in the system and it shows the pressure in black and in red, or whatever color that is, the depth, approximate depth, at which a critical value of about .7 is attained in terms of the volume fraction of the gas. So this will be called the fragmentation pressure.

Okay. The status of this work is, this is something the Panel has more or less completed. We have completed most of this work and it's all in the report. We considered, we looked at the previous DOE work and essentially redid it using other models, maybe more recent models, more or less confirming that work. We have done some elaboration on that. We've included some parameters and some estimates that weren't explicitly considered in the past. We feel strongly that any analysis of dike, drift interaction, the fluid mechanics thereof, should use these properties that we've calculated because they are more internally consistent and more complete.

Okay. The third point is that we're trying to look at now, step back and look at the sequence of eruptive products from analog volcanoes. In other words, it's not inconceivable, I mean in fact most eruptions begin with, characterized by certain kind of material that's erupting, certain properties. And the qualities of this material change during the eruption. Sometimes sequentially, sometimes episodically, sometimes even periodically. And the implication of this, which I'm not going to read through all this, is just that, again, it's an intuitive thing. The initial state of this magma at the time the drift system or a drift is breached, the state of the magma at that moment is really critical.

And, an easy way to think about this is just to consider three extreme scenarios. Imagine in one case a melt dominated the system which is a discus liquid, not a lot of dissolved water and CO-2. So this would be an example of an analog on the surface of, would be for instance a lava flow. Compare that to a homogeneous bubbly flow where perhaps the volume fraction of vapor is 30 or 40 or 50 percent. You know, somewhat compressible, higher velocity flows, a lot more turbulent, and then going all the way over to an extremely blow low rich two-phase mixture in which perhaps the flow is separated in the sense that there is a vapor phase that's moving at a velocity different than the blobs of liquid.

So--I guess my time is up here?

CRAIG: Nope. That's five.

SPEAKER: Five minutes.

SPERA: Okay. So the essential point here is that the initial state of magma at the moment of dike drift interaction is something we can put some bounds on using the geologic history, using thermodynamic and transport models. All right.

And so the next step is to actually do the dynamics. All right. And this is actually the essence of the problem. And it's a hard problem. But, we are developing--the panel is working on this--a comprehensive picture that links the fluid mechanics to the fracture mechanics to understand how the dike will actually propagate, right, using the appropriate properties. I'll come back to this in a second.

This is like a very, very simple model of what one of these things actually looks like. This is the--a magma-filled dike and so there's some magma in this region. There's bubbles in here. And an important point is that these cavities--is that these dikes have what we call tip cavities. So these are regions at the very top of the propagating dike that are filled with gas. And the pressure of this gas is a function of the rate of supply of volatiles through the magma front here to this gas cavity. And the rate at which this gas can leak out into the country rock. And preliminary calculations that we've been doing seem to suggest that the pressure in this cavity tip is rather low; lower than some previous analysis has indicated. And that's partly because the top through which these cracks propagate are quite permeable. All right. And this is important because here we show a picture of a propagating dike and you see that the tip cavity here, and a couple of examples here, an important point--and we haven't solved this yet--is the sizes, characteristic sizes of these cavities. They can be between a meter and 100 meters. It depends on the properties of the rock, the elastic properties, and the normal stresses that are occurring across the crack.

The point here is that if these tip cavities are very long, on the order of 10s to 100s of meters, then the pressure gradients in this region can be fairly small. All right, some of the earlier works, looking at shock wave development starts with a pressured material at 10 megapascal into one atmosphere. On the other hand, in more realistic models with these tip cavities the pressure gradients are much smaller in that region.

So, the main point here, not to get bogged down in details, but just to point out that to really understand this problem, we need to couple the fracture mechanics to the fluid mechanics using the volcanological information. As far as this problem in particular, we expect to at least pose this problem in technical detail and provide a road map for its solution. We want to consider explicitly the effects of magma and tip cavity gas pressure. That's very important. What's the length of the tip cavity as well as the gas pressure in the cavity, and we have great hopes that we'll actually do some numerical simulation on this problem in time for the final report in February. It's a difficult problem, a tough problem, but we feel that it's a solvable problem.

And so, I'll finish up here in the last 30 seconds.

The principal points in the interim report, we can constrain the physical properties of the stuff, understanding the phenomena of dike propagation is the essence of this problem and it's a solvable problem, but it does require understanding the relationship between the fracture mechanics and the fluid mechanics using the volcanological history and thermodynamic properties of the magma. Is that my last one?

And I guess the final point here is that, just reiterates the idea that it is a coupled problem; that we do need to solve this thermomechanical issue and couple that to the fluid mechanics.

CRAIG: Thank you, Frank. A couple of observations. Michael, you're first on my list.

There has been extensive discussion in the Board about probabilistic weighting of doses, and these events that you're talking about have loomed very large in there. I see now we're starting to really get some grasp that actually are showing the probabilistic weighting. This doesn't have to do with your report, and that's good. That's good. It's now becoming increasingly clear from the DOE documentation of it that these are in fact from a point of view of a probabilistic dose over the first 10,000 years. He uses the most important class of events. So it's really good that this report is being done and it's really important because it is the big issue from a numerical point of view.

Secondly, the interim report I found absolutely fascinating. It's the best example I've ever seen of a place--of an interim report that really spelled out that it was an interim report and the things that you should look at and not worry about, and things that were going to be dealt with with last year. It was not an easy report to read, especially for somebody like me who doesn't understand this material. But it was a very interesting report. You really did a nice job on it. So, thank you.

CRAIG: Michael. And who else is next?

CORRADINI: I guess I had a number of little questions. Let me start with maybe the most--I see that you've bounded everything relative to how you can get the flow out of the region. How are you going to decide which of these many bounding models is the one to use, or are you going to look at all of them and see which one creates the biggest effect and take that? What's your strategy for getting some clarity as to what is the one you're going to count on?

SPERA: I think the approach would be to try and put limits on the critical variables. In this case, for instance, the gas contents of magmas, to look at the elastic properties of the country rock, range of permeabilities, and initially, you know, set up a general model that would be parametric in the sense of being able to account for a wide variety of these things, and actually run some models looking at extreme end member behaviors. And trying to get a handle on the dynamics from that point of view.

The problem is that, you know, it's not simply just a review of available literature but it does--this problem hasn't actually been solved in quite detail. Volcanologists coming from one school sort of consider it a pipe in the ground and magma in the pipe. People who do fracture mechanics worry very much about the actual propagation of the fracture. Coupling these two is more or less a state of the art problem. But I think in general in these kinds of studies the way they do it is to parameterize the models and look at a wide variety and take it from there.

CORRADINI: But then just to follow up and then I'll wait a bit. So that let's say that you develop a parametric model and there are some key parameters which you'll estimate, and then you'll do the calculation and get some results. Is it the pure--is it the panel's approach to stop there or is it to also suggest a range of parameters you see most defendable and then carry out the calculation of that regime and look at those results relative to impact? Where is--I'm trying to decide, is it stopping the parametric model, is it essentially giving your best estimate as to your judgement as to where the key parameters are? Where does it stop?

SPERA: It certainly includes the former. And we're hoping to actually go through and complete the analysis. That's a little bit dependant on the time frame and what we can do. But your point is well taken.

CRAIG: Dan Bullen.

BULLEN: Bullen, Board. First, let me compliment you on the great amount of work that you've done, in addition to being a peer review panel. I mean this is a tremendous amount of new information that you're trying to coalesce between volcanologists and the rock mechanics guys.

SPERA: Right.

BULLEN: But my questions harkens back to the fact we went to the mountain yesterday and we saw lots of fractures and we know the mountain breathes and there's a lot of gas movement and water movement within the mountain. And, I look at your bleeding of that gas pressure in the crack tip to the rock that's around there. How do you incorporate the heterogeneity as it gets to the top 300 meters or so? You know, the different layers become fractured and it's not a easy continuous homogeneous material, and so it would seem to me that that's kind of a different animal to get your arms around, and so I was just wondering how your model deals with the heterogeneities in the mountain and the fractures, and the loss of pressure in that crack tip as it comes up?

SPERA: All right, well, we haven't dealt with it to date. Yeah, I mean, clearly, there's a fracture permeability and I think one thing is perhaps trying to get, you know, put some bounds on it. What the preliminary calculation suggests that just for the ball park figures, even dropping them in order of magnitude or so that--and what's a surprise is that the pressure in the cavity is quite low. It's actually to the forward pressure at that depth. So we may be in a regime where it's leaky enough to allow this gas, even, you know, giving an order of magnitude the other way. But yeah, that's clearly a very important point, and you know, hopefully we can incorporate some of the measurements of the anisotropy permeability that are available.

BULLEN: Thank you.

CRAIG: Thure.

CERLING: Yeah, I guess mine was just a related question just in how are you going to deal with the problem of this, basically we've got a three-legged volume, 30 percent by volume, of floor space, but it's in two phases, in sort of a 10 percent matrix and 20 percent large voids that mostly don't fill with water. Just wondering how far you're going to take this continuum from fully saturated to--

SPERA: As far as the permeability structure and how that would--yeah, that's a good question. I would have to consult with Detournay and Rubin on that point. We, you know, we--yeah, I mean that would be very important to be able to have a good handle on the bounds of the structure of the permeability and porosity, so I can't really answer that directly, but that's certainly something we have to look at. And we may need to bring in some additional expertise in terms of reservoir property values from DOE.

CORRADINI: Can you--Corradini. Can you help me with--just tutor me a bit here. So if I have picture one on the left where I have a long link scale and I have a low pressure, what's driving the dike up? I thought it was the gas pressure fracturing at the fracture point that was opening the crack. Am I missing the physics there? What's driving that up?

SPERA: Yeah, what's driving it in this case to the right is--well, this should be like that--is the excess magma pressure.

CORRADINI: Which is not the gas--

SPERA: Which it needs to exceed the dike normal pentile stress.

CORRADINI: Okay, right.

SPERA: It needs to exceed that by some increment.

SPEAKER: For the thing to propagate upwards.

CORRADINI: So that can be exceeded by a few bars? Because one of your conclusions, I was reading through it, said that if the link scale here is 100 meters, I could see a pressure in that gas void region there of maybe a few bars rather than 100 bars. So I only need that to essentially continually push the tip up?

SPERA: Yeah, the excess pressure.

CORRADINI: Okay.

SPERA: Right. Right.

CRAIG: Other questions? Staff? Oh, Richard.

PARIZEK: Parizek, Board. The missing part of the analysis then would be in the way in which this energy transfers into disrupting canisters or waste packages, right? You mean we'll set some environmental conditions within the repository, but in fact how an individual waste canister is going to be behave is not part of your group's analysis?

SPERA: Yes, we would be--once we have the dike propagation problem down, we will be looking at the conditions aflow down the drifts and looking at the, in some sense, easier, more standard problem. So we do hope to do that. I guess I didn't really represent that very well here, but that is something we want to consider. It's actually in one of the chapters of the interim report. I think Chapter 6 kind of deals with that, some of those issues. But we do plan to look at the flow into the drifts and the effects on the waste packages, the dynamics of that. But thank you for pointing that out because I kind of focused here more on the root problem.

CRAIG: Still another one.

CORRADINI: Yeah.

SPERA: Yes.

CORRADINI: I'm sorry. Corradini. So since, I guess I interpreted what Dick was saying also that you were dealing with what you call the root problem, but I guess I forgot in Chapter 6, so one of your conclusions in 4 was that the, and in 6 you repeated it, that the pressures down the drift compared to a previous analysis, whose names I forget at the moment, will not be of such a large pressure. So how are you intending to model, because again I'm guessing here--I'm guessing the phenomena is such that it would be a high void fraction phenomena, not a low void fraction phenomena.

SPERA: You mean into the drifts?

CORRADINI: Into the drifts.

SPERA: Of flow into the drifts.

CORRADINI: Yeah. How would you intend to model that, because all that I'm aware of in past modeling of this is essentially flow of magma, or whatever you want to call it, down there carrying away objects. But here, I could have a very high void fraction regime which is essentially droplet flow. And what's really cruising down those drifts is just gas.

SPERA: Yeah, in the case of the larger tip cavities, that would be the first material to see the drifts, and it may even be a gas at a few bars. In the Woods, et al study, I think maybe that's what you're referring to, they took the very idealized situation of a magma at 10 megapascals seeing atmosphere pressure in the drifts. And of course, that will lead to shock waves, and they mainly focused on the shock wave hitting the end of the drift and bouncing back and the carnage that would occur. So, yeah, but the point is that, indeed, the volume fraction of gas in the magma, below the magma front, is spacially variable as well as the gas in the tip. And the state of this material is changing very dynamically. And I mean that's one of the reasons why we study the eruption phases of earlier eruptions because the nature of this material can change during an eruption from a very gas-dominated to a very incombustible melt. And we want to be able to cover the spectrum of those types of behavior.

CRAIG: Okay, thank you very, very much. Nice report.

We turn now to the update on corrosion--sorry, correction. We turn now to Bill Boyle, Postclosure Thermal Conditions, How Hot Should it Get. Bill Boyle is Senior Advisor for Regulatory Policy, and he is a geologist from UC Berkeley. Civil engineering I guess is where you got your Ph.D, not geology, it says here.

BOYLE: Both.

CRAIG: Both.

BOYLE: So depending upon which audience I'm talking to, one will either claim me or deny me.

Okay. I think people can hear me.

CRAIG: Warning means five minutes

BOYLE: Okay. Good afternoon, and thanks for the opportunity to make this presentation. And I'd like to acknowledge Martha Pendleton, who had a significant role in helping put this talk together, and also John Scott and Lisa Feedar, if she's still here, because they take care of the graphics in the presentation.

So why is this presentation taking place? It was that Dr. Chu (phonetic) suggested it because this topic has been around for a while, an item of correspondence between the Board and the DOE. And there's the five new board members, and Dr. Chu thought it might be useful to have a talk on what it is we've done on this topic and where it is we're going.

Now, you can see the title. It was, Claudia Newberry and I were discussing this when the talk came up, well, what do we call it? And we didn't want to call it hot versus cold. That sounds so antagonistic. You know, it's a term that people use, you know, on the project, and that sort of thing. And, plus the cold really isn't cold. It's perhaps less hot, but it's not really cold. And as we were discussing the title in Claudia's office, she happened upon a book on her book shelf. It was the 1992 National Academy of Sciences Report, Groundwater at Yucca Mountain, How High Can it Rise. And it was that title that, with modifications, led to this title. The Postclosure Thermal Conditions at Yucca Mountain, How Hot Should it Get. Next slide.

The topics I'll briefly address today are, what were the fundamental technical issues, work and analyses done to look at the postclosure thermal conditions, and work to be done in the future to address the decision on hot or less hot, if you will. Next slide.

Okay, this is DOE's understanding of some of the, the major technical concerns the Board has raised through the years on this topic. One has to do with corrosion, that at the higher temperatures, there's perhaps increased possibility of greater corrosion. And the second has to do with coupled processes, if you will. What's going to happen to the rock and water in the presence of heat, the water all by itself, thermo-hydrologic, or the rock water and heat together, what changes might take place. And in particular, the hotter it gets, just the sheer volume affected by the heat becomes larger. And, if we're uncertain about that, what effects might that have on performance? Next slide.

All right. As I mentioned to some on the field trip yesterday, for all the countries that are considering repositories for heat-producing waste, understanding the effects of the heat is quite fundamental, so even before the Board existed, you know, the Department knew it had to study the effects of heat, but I think this slide reflects in part some of the specific contributions that Dr. Cohon mentioned this morning, this topic of, you know, a hotter design or one less hot was one of the specific areas of contribution that he mentioned. And these are some of the things that the Department did, at least in part, specifically to address some of the concerns raised by the Board.

The first is we did this, it's called the LADS study. Many people are familiar with it. The License Application Design Selection Report, where a cold design was looked at as well as various hotter designs.

We evolved the Site Recommendation Design from a earlier design, the Viability Assessment Design, that greatly reduced the impacts of higher temperatures. I'll show another slide in a bit on that.

Just last year in response, in part, to some concerns raised by the Board we did the Supplemental Science and Performance Analyses Report that looked at a lot of effects of temperature, specifically with respect to corrosion and coupled processes. We enhanced our experimental program, and this is ongoing, and we instituted the Waste Package Materials Performance Peer Review. Next slide.

Now, this is just a simple graphical representation of one of the changes we made through the years. I don't want people to read anything into any of the details. Tom Buschek of Lawrence Livermore National Lab created this for me to get across one of the changes we made. The VA stands for viability assessment design, and that was back in the 1998 time frame. And for those of you that were on the field trip yesterday, it was that design that would have had 500,000,000 cubic meters above boiling, and that would have been the rock in between the two red lines. It's a, you know, rectangle, if you will, extended in the third direction. That's what it was for the viability assessment because the spacing of the drifts was on the order of 20 meters. But because of concerns raised about, well, what would all this water that might pond up above, what might it do to the rock in terms of ceiling fractures or causing dissolution. There was some concern about that. We switched to the site recommendation design which greatly increased the spacing between the drifts, between 80 and 90 meters, and so what these red lines show for the viability assessment design, it's the boiling isotherm, if you will. Everything between the two lines is boiling. Whereas, for the site recommendation design, it's only the material with inside the elipse. So we greatly reduce the volume of rock that was going to be subjected to boiling. So at least from the rock point of view, we made a great stride in switching from a design with a boiling isotherm there and there, to boiling isotherms around the drift. Next slide.

Okay, so that was the change for viability assessment to site recommendation. And as I mentioned for the Supplemental Science or Performance Analyses, we did even more changes. We looked at even lower postclosure thermal conditions, which, if you think back to the prior slide, the rev ellipses would go away. There would be no boiling isotherm for the lower postclosure thermal conditions. And in the SSPA, we had three main goals. One was to introduce new data. Another was to better address uncertainties, and the third was specifically to add in temperature functions if we could, most specifically for corrosion and coupled processes, because there's many things in the TSPA that are actually temperature insensitive, like the climate and regional seismicity. So the areas where we have the temperature dependencies work for the corrosion and the coupled processes. Next slide?

And what we found is is at the subsystem level, for example, like CO-2 presence in the repository, it's concentration of CO-2 gas over the life of a potential repository, varied between a hot repository, red, and a less hot one, blue. Similarly, PH of the water that might be present around the repository varied between a hot or red, and a less hot, blue. And these aren't that different, but if you notice, the red goes away because the water is all gone in the hotter repository. We couldn't--they couldn't even calculate a PH. So the point is, is we have many, many instances of this. And we presented them about a year ago at a panel meeting with the Board on the various changes that we had made in the SSPA. Next slide.

We take those results and we roll them up into the TSPA and what happens is that the system level, the results for the higher temperature operating mode and the lower temperature operating mode essentially look the same.

Now, this brings up the question, is that real or is that some limitation of our models? Now, we tend to believe that it's more likely that it's real, in part, for any number of reasons. One is our scientists have been working on it for a while, and they are, you know, highly qualified and their work is reviewed internally. But also our work is reviewed by groups like yourselves, Nuclear Regulatory Commission, independent reviewers that we bring in, like the Nuclear Energy Agency and the International Atomic Energy Agency. We've had our TSPAs reviewed by Bob Budnitz and Chris Whipple, and others in years past. Nye County reviews our work, Clark County, State of Nevada. So we're reasonably confident, you know, based--if we have fundamental errors or even discrepancies, they will be brought to our attention and we'll have to deal with them. Next slide.

We produced a report before the decision by the

Secretary and President report we did. We promised it to the Board and we finished it in February of this year. It compared hotter and less hot, and this gets back to a question Dan Bullen raised this morning about whether our choice of hot was premised solely upon TSPA, and the answer is no. You can see preclosure these calculations are independent of TSPA done by a different group of people, different managers. And for hot and less hot, both passed the regulatory limits. But for preclosure, the colder repository because it's just bigger and open longer, tends to--tends to be perhaps more expensive, ot also perhaps have greater safety hazards, simply related to increased excavation or longer time frames. Postclosure, even though there is more uncertainty related to the hotter condition at the system level, they are the same, as I just showed on the previous slides. And the results of this study indicate we don't need to make a decision now and we'll keep getting data to make a decision when we have to. Next slide.

Plans to address the postclosure thermal conditions, this relates to questions asked of Don Gordon earlier this morning. The people in TSPA will analyze a design that leads to postclosure thermal conditions similar to the SR design, similar to the one that produced the red ellipses on that figure a few slides ago. But even doing so, this does not preclude closing in a cooler mode. You know, we can get it cooler any number of ways--adjusting the amount of aged fuel, aging duration, ventilation, derating waste packages. And subsequent decisions will be informed by the results of ongoing tests, analyses and modeling. Next slide.

These are some of the tests that will be used. The drift scale test people saw yesterday. The cross-drift test is yet to be. Some of these other tests, the geotechnical test, Mark Peters talked about. Some are ongoing and others are planned.

This next slide is just a variation on the one I just showed you. There's a time line. The SSPA was last year. The SR, site recommendation, was earlier this year. License application would be December, 2004. If all goes well, 2007, and you can see the listing of the tests over here. The phenomenon they would address described by these acronyms over here--thermohydrologic, thermo-mechanical, and how they are spaced out, bearing in mind that, you know, the ultimate decisions are out in this time frame, so we still do have time to gather information and make a decision later for the final decision on hotter or less hot. Next slide.

And recent test results, I'll jump to the bottom line first. Mark Peters talked about some of them already, and Gerry Gordon will talk about some more, indicate that large portions of the repository have relatively benign in-drift environments for corrosion for extended periods of time, and portions of the repository will pass through aggressive in-drift environments for a shorter period of time than previously thought. And we're still continuing to do work to get a better definition of what those conditions are.

And I think that's my last slide.

CRAIG: Okay, thank you. Thank you very much, Bill. One of your arguments for the high and low are certainly interesting and it was good to see how the repository design has changed over time. It certainly is better than when it had an enormous amount of water sitting above it in an unstable posture. A number of the Board's arguments on lower temperature design would not be expected to show up in the TSPA type analysis because they relate to surprises and unexpected failure modes, which just aren't built into TSPA. If your experts aren't right about what the error bars are for certain processes, then you can be in trouble. And, one of the Board's concerns, or connected with that, very difficult to quantity, but nevertheless, potentially important. So I would suggest that in a comparison that that point is one that needs to be borne in mind, even though the numerical results don't demonstrate it.

BOYLE: Right. Right. And I get your point very well. To the extent that the unforeseen surprise is deleterious, and to the extent it relates to the temperature, then there's, you know, the hotter temperature perhaps, you know, seemingly that that's a negative towards it, but, you know, our people are aware that we--particularly during the Supplemental Science and Performance Analyses, we kept after people to try and broaden their thinking in terms of, well, what could go wrong. Push the distributions out, you know, to encompass, you know, all possible, even unknown bad events.

CRAIG: Alberto Sagüés. Who else? Dan Bollen.

SAGÜËS: Yeah, Alberto Sagüés, Board Consultant. My first observation was going to be what Paul just mentioned, but I wanted to indicate it's not just uncertainty, it's lack of data which also plays an important role in that, right? Like, while we look at corrosion in the, say your 150-degree regime, only today where the Board is going to be presented some of the initial information in that regime to any significant extent. So the HTOM projections over there were based on extrapolation from information on lower temperatures. So it was not just that there was an unknowns and knowns, there was just simply no data. Is that correct? Is that a correct way of saying that?

BOYLE: Well, I don't--with respect to it's the word "no", no data, that, you know, without the particulars I won't address, but I'll address the more general issue that, you know, by their nature repositories do involve a lot of extrapolation, and that gets back to my comment. We've had very competent people working on it for years and years, and then competent groups like yourselves review us in the NRC. And so, there are checks and balances on some of those extrapolations. And data always helps, and so, you know--

SAGÜËS: Just one follow up. I would say in the case of the Nuclear Waste Technical Review Board, yes, the Board as evaluated the results, but that doesn't mean that the Board agreed with results implied in those notes over there, right?

BOYLE: Right.

SAGÜËS: So that they--

BOYLE: No, I understand. I don't want to get into--

SAGÜËS: --omissions did not update--

BOYLE: You know, the board didn't always--isn't necessarily always in agreement with some of those results, and that's why I, in the description of the system level results I raised the issue, is it real or is it simply, you know, the results of the limitations of our models, which is a question. You know, I don't know that the Board ever framed it that way, but I think that's the gist of the question. But we benefit from questions like that. You know, when the letters come in we just don't throw them away. We look at them and we question, all right, what's the basis for the concern, and you know, what should we do about it.

CRAIG: Dan.

BULLEN: Bullen, Board. Can we see Slide No. 12?

This is a great time line because it tells us, you know, relative to license application what's coming up in about two years. You know, you've got a couple of decisions where you're saying your costs and analysis and tests and the license application is going to go in. But if the license application goes in and, as I understand it now, the peak waste package thermal loading is up to somewhere around 17 kilowatts. Is that, I mean ball park, or--

BOYLE: I've heard that in discussion. I don't know what--

BULLEN: Let's say it goes up to 17. Let's just say it's a hypothetical here. Not that it is now. And if you go waste package facings of two meters you might be able to keep it cool, but if the waste package facing goes 10 centimeters, which is four inches apart, I guess the question I have is when do you do something that's irreversible to the mountain? I know the construction is the same for LTOM and HTOM in that blue region.

BOYLE: Right.

BULLEN: You've got the decision for postclosure simulation in there, and maybe you pick a panel or a drift or two and you say, I'm putting them 10 centimeters apart and if it looks like it's deleterious, the old rock mechanics problems or water movement or something, I can take them out and abandon those drifts or refill them, or whatever. But when do you make an irreversible decision that may impact the mountain? And I guess that harkens back to I've got to have a license application. If I only have a license application to go hot and then I all of a sudden see these kinds of things in my confirmatory testing, it's different. It's a difficult task, I guess, to back off from hot. Whereas, I look at just the logical sense, if I decided to go cold with two meter waste space facing and all of a sudden had the data from the drift that says I could go hot, I can slide them together, pack more waste in there and address the issue of going cold to hot. I guess hot to cold and cold to hot is just another mind set as you get here--

BOYLE: Right.

BULLEN: --and I just wondered the rationale behind it maybe.

BOYLE: Well, that issue has been brought up through the years by the Board, as a matter of fact. Why not characterize it as starting cold and leading to hot rather than we'll start hot and if we have to go cold, we will. And I guess it is that we feel reasonably, you know, bearing in mind that both paths and there is some cost difference--so that's why we're at least in part staying with hot.

But back to your question on when is something irreversible done. And for me, that gets out to when we close it up, for the most part. But any time during that operational period if we were to determine, let's say, that 10 centimeters was too little, well, you could take some of the waste packages out of any drift and space them further apart. You could ventilate longer. You could refrigerate. It's simply during--before you close it, it's simply a question of money. You know, and I don't know that there's anything really irreversible, other than--even, we start hot and we drive away more water to start with. Well, given enough time, the water is going to come back.

CRAIG: Debra?

KNOPMAN: Knopman, Board. If we could look at Slide 9. They've kind of posed a trade-off here, or not maybe you didn't pose it, but you--one presents itself. Under preclosure, the second bullet, you say, well, preclosure safety hazards and costs may be higher for a lower thermal condition. And then under postclosure you say, well, but you have less confidence with the higher thermal condition. And, the program's, the judgement that the cost issue, at least for now, outweighs the loss of confidence--or maybe you wouldn't put it that way--the reduction in confidence or less confidence, that you're willing to live with in a postclosure system. That, to me, implies that you have some implicit sense of what level of confidence you're comfortable with, and that you're above that threshold now. Can you express that in quantitative terms?

BOYLE: Probably not, other than I could--I think you used a good adjective. It's implicit. I mean it has to be. And, I wasn't involved in, you know, necessarily the meetings or decisions, you know, at the highest level. I can guarantee you I wasn't. But for the site recommendation--but I will offer that this confidence, you know, it's the confidence and subsystem effects, which, based upon our best ability now, when we role them up into this system level, there isn't any change in confidence.

So the trade-off is between, you know, this lack of confidence in the subsystem results which Professor Sagüés might or Professor Craig might argue, might actually, in the future, translate into something. But to the best of our ability now, it really doesn't translate into a substantive difference in the measure that's--one of the measures that's of prime importance to us and that's the postclosure dose. So then it's the cost, and I guess I wasn't in the room when these sort of discussions were held, but I think most people, when they would look at two things that seemingly were the same, but one was more expensive. Many people will tend to lean towards the cheaper.

KNOPMAN: Well, I'm trying to get a sense of the program sensitivity to confidence because that has a lot to do with the ongoing research program and the science and technology program.

BOYLE: Right.

KNOPMAN: And if we're already at a point where the program has sufficient confidence in the science it has, then I think, you know, lots of people could raise questions about why are you then still doing all these experiments? How much more confidence are you trying to get?

BOYLE: Right. And I think people tried to get it across in different ways this morning. It's although we're doing some of this, you know, other work that Dr. Brocoum described, the science and technology, as of this moment we're not doing it for licensing. We feel we have enough confidence to enter in through the licensing phase when we file our license application in December, so some of the work is being done for other reasons, like decreased costs for example. Now, some of those, like take for example the drift shadow. Perhaps as that work goes ahead it will demonstrate that, well, we don't need a drip shield, so some of the work, it really isn't related to our confidence in licensing. We believe we have enough to go ahead with that. Some of that work is for other reasons. To help define a better system, if you will.

CORRADINI: Can we go back one slide? Corradini.

Okay, if you said it, maybe I missed it. What are the key parameters that differentiate HTOM and LTOM? I dragged out the transmittal of the white paper which you referred to, which is the same plots.

BOYLE: Right.

CORRADINI: Can you remind me, remind us, what are the key parameters that made these two different?

BOYLE: Well, it's the temperature. It's--

CORRADINI: What did you do to the repository design? That I'm clear. I'm with you there. But what did you do to the repository design. The spacing, the ventilation, what were the key things that made one operating mode termed high temperature and one operating mode termed low temperature?

BOYLE: Well, it was done in the model itself. They used pretty--my recollection is they used the same TSPA model layout and arrived at the temperature differences by turning a knob on the model, if you will, of ventilation. You know, how much heat could be removed via ventilation. Just to get insights into the analytical results. There really wasn't a comparison of two completely separate designs, one hot and one cold.

CORRADINI: Right.

BOYLE: It was through a temperature knob. And that temperature knob affects the rate of corrosion and also those thermally-coupled processes in the rock. And then those results get translated into these dose calculations out at 18 to 20 kilometers, thousands of years in the future.

CORRADINI: Right.

CRAIG: My recollection from--and the others can confirm or contradict me, was that the primary--from their presentations is the primary variable had to do with the corrosion functions for the C-22. That was a primary temperature dependent, and it was a moderately strong temperature dependence for that, yeah.

BOYLE: We did other--

CRAIG: Is that correct?

BOYLE: Yeah.

CRAIG: Is that your recollection, Dan?

BULLEN: Yes.

CORRADINI: So let's pursue that. So let's say it's a--what I heard you just say was it's something, you're not sure, it probably had something to do with ventilation in the model. Okay. Let's take that as a given. Now I go along and I say all right--and let's go down the path. Debra actually brought it up. I think it's an excellent way of thinking about it. How much uncertainty would you have to have in the corrosion temperature effect to cause you to be a concern based on those curbs? That's what I'm--I think she said to you and I thought you were going to go back to the curb, so since you didn't go back to there let's take you back there and ask you that question.

BOYLE: All right. I mean, now people can--and I think different decision makers would get at it different ways. Now bear in mind these are for the nominal case results. We're not taking into account the volcanism that Professor Spera talked about. I mean the standard, this is up around here. 15 narratives. There's 10. So remember the standard is up here, so we're orders of magnitude less. We can go to the metallurgist, the corrosion people, and say, okay, give me unrealistic models. You know, take the tail of the distribution out-- we can do it artificially, we can do it in a rational way, and we could start twisting that corrosion knob. Because the results are very, are more sensitive to that corrosion knob probably than any other knob. That's what Professor Craig was getting at. Dr. Bullen agreed. That we could twist that knob and see how we move either the mean, the red curb or any of these gray individual results and people could see, you know, at what point would they start becoming less confident or more nervous, if you will. That's an approach to get at the problem, and that's what we tried to do in these results. We strongly encouraged all the scientists we dealt with, you know, to take the blinders off, forget about the quality of the data, quality in a QA sense. Any knowledge you have on the subject, give us your range, and try not to be constrained. You know, and that's what we got. We're still orders of magnitude below the limit.

CRAIG: All right, Dave. Last one.

DUQUETTE: Duquette, Board. Have you tried to do it the other way where you actually set the oars on the line at the maximum dose and took a look at what would have to happen to get you there?

BOYLE: You know, I think people have. Undersecretary Card has asked that question in various ways. You know, it's like, okay, what if? What if somebody forgets to put the waste packages in, and various things like that. And I think Peter Swift is one of the talks after myself and he'll show some one-on analyses where barriers were added in one at a time that gives some insights into, you know, the effect of the system. We've done similar calculations in the past, including in the supplemental science of performance analyses and then the SR, TSPA where we remove entire barriers to see what is the result. And so you can get insights into which things are more important. And if something really goes south, you know, tremendously badly, you know, where you might end up. You know, how much closer to the standard you'll be. So we do those types of analyses. And, you know, the waste package is important. There's no doubt about it.

CRAIG: Okay, thank you very much, Bill.

We now turn to corrosion. Gerry Gordon, who is a metallurgist. Metallurgical Engineer from Ohio State University. And, Gerry, I'll give you a five-minute warning.

GORDON: I'll try from this side.

Good afternoon. Can I have the first slide.

For the next 15 to 20 minutes I would like to update you in two principle areas. One, the expected aqueous environment that might exist on the surface of the drip shield or the waste package, and in somewhat more detail the initial results that have been obtained to evaluate localized corrosion at higher temperatures. Previously we were limited on the project to about 120 centigrade. We've since gone up to 150 centigrade, and there are two general types of environments, both brine solutions, and probably the more relevant environment, thin aqueous deliquescent brine films. Next slide.

This is a schematic of the evolution, if you will, of the aqueous environments that there are two sources of water that can contact the drip shield and the waste package. One is the seepage waters which are, as long as the drip shield is intact, are diverted around the waste package. And the other are waters that are contained in various types of dusts and deposits that could settle on the drip shield and the waste package surfaces and could react with the relative humidity to deliquesce and form very thin aqueous brine films. That's the expected environment on the waste package surface at least until the drip shield fails. Next slide?

This is an example of a, maybe one of the limiting type of environments. It's a high calcium starting water, although it's in small type here. It picks the water from the THC model at a particular time in the repository time temperature history. In this case, this is for a high temperature operating mode at 1600 years, and the temperature is down to about 92 centigrade. And under these conditions taking this high calcium starting water, which is one of the potential pour waters, and evaporating it using EQ36 model removing water so the activity of water is going down with time, and the concentration of the species is going up. And what we see at very, very high concentrations for this particular starting water and the ordinate on the right is in roughly parts per million, it's in milligrams per liter, is that both the calcium and the chloride are over 100,000 ppm, which is maybe--there may be on the order of 15 percent each. The nitrate is also high, although lower than the calcium and chloride. Surprisingly, at least for me, there also is a fairly high concentration of bromide iron, which is there in this pour water at roughly one part per million starting composition. It's an order of magnitude lower than the chloride, but it potentially is an actor in terms of corrosion.

Also importantly, the fluoride content, because of the high calcium in the water, precipitates out and drops to very low levels, and the PH of this water, which starts out at about 8.3, drops at these very high concentrations to about 5. Next slide.

The Board has seen this chart probably several times in the past. I put it up only to show the three types of corrosion mechanisms that potentially can lead to a breach in the alloy 22 outer barrier of the waste package. The dominate mode is general corrosion. That's the mode that is the predominate one in the TSPA because, based on a range of measurements, the corrosion potential over the full range of temperatures and environments that have been looked at are less than the critical potential for breakdown of the passive film, or repassivation. The rates in general corrosion are on the order of .01 microns per year mean, which is about 100 angstroms per year. There is a potential for localized corrosion if the corrosion potential exceeds a critical potential, and under those conditions the corrosion rate can go up by orders of magnitude. And in parallel there is a potential for stress corrosion cracking. Both alloy 22 and titanium grade 7 are extremely resistant to stress corrosion, but they are not immune in some of the relevant environments. And consequently, the project has chose, rather than to deal directly with stress corrosion to mitigate against it by mitigating the well residual stresses, which are the principle driver for stress corrosion on the waste package. Next slide.

Although alloy 22 is very resistant to localized corrosion, it can suffer that type of attack under very oxidizing conditions, and under conditions where the ratio of chloride to oxyanions, in particular nitrate, is high. Under those conditions the nitrate tends to act as an inhibitor. If it's low or absent and we have oxidizing conditions, we can break down the passive film and get localized corrosion. We'll see some examples of that as I go through the presentation.

There are data in the literature generated by the project that indicate that at molar ratios of chloride to nitrate greater than about five to one, there is the potential for localized corrosion. And when we look at the range of molar ratios for relevant environments, they tend to be less than three to one and often closer to one to one. Next slide. Next slide, please.

What I'd like to review are some of the initial higher temperature localized corrosion tests both in bulk environments and some limited testing that has gotten underway on thin deliquescent aqueous surface brine films. These are tests that are being done in calcium chloride in mixtures of chloride and nitrate up to 150-C. Next slide.

I won't dwell on this. It's there for your information, but in the bulk environments these are the two types of crevice specimens that we're using to evaluate the material's response. The multiple crevice assembly and prism samples, both of which have a teflon washer that's pressed tightly against the polished metal surface to form a tight crevice.

These are some data in phimolar calcium chloride at 90 degrees centigrade that illustrate the benefit of nitrate. If you look at this prism sample after it has been dissembled, you see the outline of the crevice and in the case of chloride plus nitrate at a ten to one molar ratio, after this cyclic polarization curves that I'll describe, there's no evidence of localized attack. To the contrary, in purified molar calcium chloride, one does get initiation of localized attack at the edge of the creviced area, and it tends to spread outward and we think that's because very concentrated hydrochloric acid is formed as a result of hydrolysis reactions in the crevice chemistry and that spreads outward. And in fact you can see it in terms of gravity as it runs around the sample.

These are two cyclic polarization curves for these two specimens. This upper curve, this test, one scans the potential of the specimen in the solution at temperature in the positive or oxidizing direction and measures the current density which can be related to corrosion. And if we look at the calcium chloride plus nitrate, we get to a very high potential in current density and then reverse the scan. And, we've picked as a critical potential, and this is somewhat arbitrary, the potential at which the corrosion current reaches 20 microamps per square centimeter on the forward scan. And if you look at that critical potential for the nitrate containing solution compared to the no nitrate solution, you can see it's significantly higher. It's a critical potential, but with the nitrate, in reality, it's a potential at which we have transpassive dissolution and perhaps oxygen evolution. It's, the actual critical potential to break down the film is still higher. Without the nitrate the film does break down. You can see that here. And so the critical potential drops from 800 millivolts or so down to a couple hundred millivolts. Next slide.

These are two cyclic polarization curves for the--one for the prism and one for the multiple crevice assembly specimens. And 150 degrees centigrade, nine molar calcium chloride or 18 molar chloride. And the margin against localized corrosion is the difference between the corrosion potential and the critical potential. And for both of these types of crevice samples that's on the order of 200 millivolts for these conditions.

Also important is to establish that the corrosion potential is stable with time and doesn't slowly increase and ultimately reach the critical potential. And, you can see that in this case the corrosion potentials do plateau out.

This is a summary of a lot of cyclic polarization data. The curves on the left are for calcium chloride with ten to one chloride to nitrate. The curves on the right for pure calcium chloride. The upper curve is the critical potential, the lower curve the corrosion potentials. And you can see there's very significant margin until you get to the very highest temperatures. You can also see that at the higher temperatures, if you compare this curve with this one, that the nitrate is apparently undergoing a redox reaction on the metal surface that is increasing the potential of the sample. So it's decreasing the margin relative to pure calcium chloride whereas at lower temperatures, because of its inhibiting effect, it provides added margin.

Also, I put on on this chart the boiling points of these different solutions. The nine molar calcium chloride boils at 154 centigrade. Very high boiling point. And that's about 50 percent calcium chloride, so it's an extremely concentrated solution. Next slide.

Although we were looking at bulk solutions previously, the more relevant case is where we have a thin aqueous deliquescent film on the metal surface. In that case the mass of water is very limited and it's possible to get separation of the local cathodes and annodes to a much greater extent than in a bulk stirred solution. And that can lead to changes in the PH. And we can see that as we go through the next couple of slides. Next slide.

What I'd like to describe are some tests that have been done at Lawrence Livermore using a thermo-gravimetric balance system. The salt is deposited in a polished metal surface using an aerosolization technique. And then it's very rapidly transferred to a controlled humidity, controlled temperature chamber on the thermal balance, and the weight change is monitored as a function of time as the sample starts to deliquesce. A number of conditions have been looked at. I'll only describe this first 150-C, 22-1/2 and a half percent relative humidity case. You can see that on the next slide.

Both alloy 22 and two types of control samples, a glass slide and a platinum specimen were run through this experiment. You can see the very rapid initial weight gain as the calcium chloride picks up the water through deliquescence. And then there's a weight loss in all of these samples, and after about 25 hours or so you reach a stable weight. The scatter is within the sensitivity of the thickness of the deposit and the sensitivity of the balance. We're dealing with fractions of a milligram.

But, what apparently happens, and it's being confirmed through analysis, is that initially you pick up the water, the calcium chloride starts to react with water forming calcium hydroxide and hydrochloric acid. Hydrochloric acid is volatile at these high temperatures, and so you start to precipitate out calcium precipitates and then the excess water evaporates.

Thank you. Go to the summary slide.

Just then to briefly summarize, the waste package surface environment is strongly influenced by the presence of the drip shield in that it diverts seepage waters around the waste package. And, consequently, brines on the waste package will exist as thin deliquescent films. The calculated lifetime in TSPA of the waste package is equivalent to the regulatory period and beyond, as you've seen in past meetings. Even if the drip shield were to fail, the recent results indicate that the margin against localized corrosion does exist, although at temperatures approaching 150 C it's very small. Remember, these are initial results. There's a lot more testing under way and it's planned to go on to LA and beyond. And the main focus of that is to reduce the remaining uncertainties in this long-term degradation behavior.

Thank you.

CRAIG: Thank you, Gerry. Questions? Dan Bullen?

BULLEN: Bullen, Board. Could we go to Slide, oh, let's start with 15 I guess.

I'm very interested in the tests so far, the tests that you have under way. And I guess I'm going to harken back to a consistent question that I've asked all along. And that deals with the effect of radiation. And in previous presentations to the Board, radiation, or radiolysis wasn't a problem because we didn't have a thin film that was at temperatures greater than the boiling point and now we do. And so if we have a radiolysis effect that is compounded by the high temperature deliquescence effect, have you got plans to do radiolysis experiments in association with these set of four, or more experiments?

GORDON: We don't currently have those plans. If you remember, we did try to simulate radiolysis by adding hydrogen peroxide to the environment, and it did increase the potential of very high hydrogen peroxide levels by maybe 200 millivolts. The dose on the waste package when it goes into the mountain is about 1000 R per hour. After 100 years it's down by about an order of magnitude. And by the time you get to these kinds of conditions, it's pretty low.

BULLEN: Bullen, Board. But the problem that you run into now is that if you're in the conditions that are like this, you're at the peak radiation time. Is that not correct? I mean I'm going to see 150 degrees C at a time that's going to be within the 100 or so years after emplacement. And so the peak radiation time is going to exactly mirror the peak thermal pulse, and so I guess the question that comes to mind is, the recent data that came out of the German program for another alloy C-4, which is similar to C-22, where they saw an effect at about 100 R per hour, as opposed to 1000. And so I guess I would strongly urge that you again address the radiolysis issue and take a look at this because the last think you need is for a waste package emplacement to occur and you have, while this is under control with respect to corrosion or localized corrosion, and then, "Oh, shoot, we forgot about that radiation again", and then somebody is going to look back in the transcripts and say, you know, about 20 years ago Bullen said you had to do that. So, I'm just really concerned that even though you've done the simulations, the real institute test, put some radiation in there, get a cobalt 60 source and do it in a hot cell. I know it's expensive. Argon will do it, but whatever. Those are key issues.

One last question, with your perseverance, if you go to the back-up slide #2. This is the problem with doing back-up the slides because, you know, we kind of look at them here. Let's see if I can find the right one. Back-up slide #2, if you will. Next one. That one. I'm always perplexed by this slide. I'll just keep asking the same question over and over again. But if I look at the bottom left, and I've got the emplacement period here, and I've got this relative humidity that keeps going up, if I'm ventilating during the emplacement period, how come I'm not taking out all that water? Because we were in the mountain yesterday. It was about 20 percent relative humidity in there. It was really dry. I just don't see it getting to 75 percent, so why does it do that?

GORDON: I'm not the right one to answer that.

BULLEN: I know, and it's just one of those questions that--

CRAIG: Okay, okay.

BULLEN: Thank you.

CRAIG: I have four people on deck and we're tight on time.

BULLEN: Thank you.

CRAIG: Ron Latanision, followed by Alberto Sagüés, Don Runnels and Richard Perizak, if we have time, Richard.

LATANISION: Latanision, Board. Let's go to #11.

Jerry, am I correct in the perception that there are still no observations of localized corrosion on smooth surface without crevices.

GORDON: That's true, in--

LATANISION: That's correct?

GORDON: --the range of relevant environments have been tested, yeah.

LATANISION: Right. That's good. I'm very pleased with this electrochemistry, with one exception, and that is the question of why it is useful to arbitrarily choose to identify the, I think you said 20 milliamps--or microamps, why arbitrarily do that?

GORDON: These are initial sets of tests. We do plan to do the repassivation potential measurements using the Tsujikawa technique where you break down the film and then you ramp down to where the current no longer increases at a hold period. Also, we have in our plans, and they are getting under way, a whole range of potentiostatic tests at a range of potentials for these different environments to measure the repassivation potential.

LATANISION: But I mean if I look at those data objectively, I would say that, you know, you've got very convincing evidence to show that you've got localized corrosion in the case where you do have a crevice geometry. And I would say, conversely, that in the case where there is no crevice, it looks absolutely stable, and so choosing an arbitrary--as a definition, an arbitrary data point to suggest that that is a critical potential, I just don't think it is consistent with the sciences.

GORDON: I think I pointed out that in reality it isn't, but it's a, by definition, a critical potential, but it's really in a transpassive range.

LATANISION: Right. But that's the point. I mean it isn't--

GORDON: Right.

LATANISION: It just seems to me that setting up a potential element of confusion perhaps later on when it isn't necessary. I think this--is great as it is.

GORDON: All right, these are the very initial data at high temperatures. We do plan to do the more definitive measurements of critical potential.

LATANISION: Right. And you are looking at temperature sensitivity in these experiments as well?

GORDON: Right, the full range. We planned to go up as high as 180.

LATANISION; Good. Thank you.

SAGÜËS: Yes. Let's see #13, please. The trend on the open circuit potential on the lift is intriguing. That was--was that a surprise that it would start--

GORDON: It was a surprise. We knew there are nitrate redox reactions that can occur, but to date we really hadn't seen them affecting the corrosion potential. But the kinetics apparently are such that it may be above 130, 140 C, there is an effect.

SAGÜËS: Yeah. That's almost like a 400 millivolt climb over the 90 degrees. That's a major deviation in the excursion of the open circuit potential. Assuming, of course, we don't even know yet what would happen over long periods of time, right? There could be perhaps another one on top of that?

GORDON: Well, we have tested it in these environments. I think I showed some of the results, and it does plateau out.

SAGÜËS: It looks like 6000 second curves, you know, like a couple of hours.

GORDON: Well, right.

SAGÜËS: As you showed in previous meetings, the open circuit potential experienced dramatic excursions upwards upwards in the long-term test facility. I'm very curious to see what would be the terminal values of that under realistic conditions.

GORDON: There are long-term tests under way at Livermore so we'll have those data available.

SAGÜËS: Because right now you're getting virtually

no gap there. A 50 millivolt gap or a 30 millivolt gap, from an engineering standpoint, I don't think that--
One last question on this: We were at the site yesterday and the issue of microorganism growth and more growth and the like in the east-west drift kept on being mentioned. Apparently there was a dramatic amount of mold in the drift. What is the MIC, the microbiologically induced corrosion program? How is that coming along? Are there any tests being done on MIC?
GORDON; There is a pretty significant effort at Lawrence Livermore. And Joann Horn periodically reports on that. I think at the last corrosion meeting she gave a paper. So it is being looked at and they are trying to define the more aggressive environments that the MIC can lead to, and then test in those kind of bulk environments. She is doing a series of biotic and abiotic tests.
SAGÜËS: Okay. Any potential synergisms with this that could make the crossover even more?
GORDON: Well, bacteria at 150 C, you know, it's a pretty high temperature. She has been testing up to maybe 90 C.
CRAIG: All right, Don Runnells.
RUNNELLS: Runnells. Gerry, one question. It's just organizational. It isn't clear to me how this fits into an organizational plan. I mean are you in charge of the

design, the choice of metallurgical testing? I mean I--

GORDON: No.

RUNNELLS: There's nothing between the lines in that question. I just don't understand the organization. How do you decide which result--which experiment should be done? Is there someone who is saying, "Hey, these are the particular waters we should be concerned about"? How does that work?

GORDON: Well, it's under the Engineered Systems Project. And that includes the effort at Lawrence Livermore National Laboratory, and the THC model calculations and so forth. The EQ36 calculations. So the calculations and the experiments are planned out. You know, there's an annual planning consistent with the budget to lay out the programs. They get reviewed and we end up with a test plan and a model. At the same time a modeling effort is going on in parallel.

RUNNELLS: Let me see if I understand. Do you sit down in a meeting with the modelers and the modelers are giving you the results of which are the most sensitive variables, such as the corrosion rate and there's somebody else there saying these are the waters that we're concerned about and--

GORDON: There's very close interaction with the modelers and the experimentalists.

RUNNELLS: And then your group, you specifically, design the metallurgical--

GORDON: I don't, no. I'm just part of the Engineered Systems Project.

RUNNELLS: Well, who does that? Who says these are the experiments that we should be doing now?

GORDON: Well, the experimental program from the modeling perspective is under Dr. Tammy Summers at Lawrence Livermore. And currently in the testing area at Livermore it's Dr. Greg Godowsky.

RUNNELLS: Okay, so they are the ones who will pull together the modeling, the sensitivity tests and so on, and then say to the metallurgical testing team, these are the things that we particularly need? Is that the way it works?

GORDON: It's a team effort. No one in isolation does it. We interact and all the modelers, the experimentalists, materials engineers, all interact and we come up with what we think is the program that will give us--reduce our uncertainties.

DOERING: If I may help. Tom Doering, BSC. I'm the manager responsible for the Engineered Systems Project and underneath the performance assessments. What we have here is we have the environment, we set the environment, we look at the EPS environment, we work with the natural systems to understand the natural systems, the environment--and we work with the, inside the drift environment. That's where we talk about the waste package material behavior, modeling, testing programs. So we have all those things that have pulled together in one location so we have a lot of synergisms going on in that sense. Gerry is one of our senior staff in metallurgical areas that we all get together and do this. So nobody does this in isolation. It's actually a team effort in doing this. In fact we've got it so, we set up a lot of teams in the last year or so to get us further synergism also, so we pull the right resources as we need them. So as we have the teams together they resolve the issues and then they move forward on those.

CRAIG: We're going to have to cut this off. You look so unhappy. So, one last sentence.

RUNNELLS: One last quickie then. I think that was 90 percent of the answer to this quickie.

There's almost an infinite range of waters that you could be dealing with and the folks in the State of Nevada, the scientists who sponsor us in Nevada, have a different set of waters than you're dealing with. Apparently this team effort you just described is the focus of which are the most important waters and which are the ones we don't worry about; is that correct?

DOERING: Again, what we're dealing with, the infiltration waters while we deal with the natural systems and we take a look at what's the probablistic distributions of the waters coming in, and then we take a look at how it is behaving inside the drift when it comes in contact inside the drift and outside the drift. And then we do the probability distribution of which waters do we do on the waters, on the packages themselves, or the drip shield themselves.

GORDON: We are addressing the waters of the State of Nevada has raised and some of the impurities like lead, arsenic. I didn't have time to go--there's a very large test matrix.

CRAIG: Okay, we are going to have to move on. There were several more board members who wanted to ask questions, and please do those during the break because we're--

GORDON: Thank you.

CRAIG: You can do it to me, Richard. When you're chairman in the future meeting you can do the same thing to me.

PARIZEK: Oh. --may go away by then.

CRAIG: All right, we now move to Update on Repository Design. Gordon Pederson. And I happened to observe--Joe Carson in one of the citizen's comments, observed the importance, before lunch, observed the importance of the PE designation. That is a, being a professional engineer is hard to come by, hard work, and it means a lot, and Gordon Pederson's resume says he is one of those. Congratulations.

PEDERSEN: Thank you. Appreciate the opportunity to address the Board. I have a few people from Repository Design, Christine and Allen Linden in the back, that participated in some of the studies I described. And Larry Troutner is hiding back there somewhere. He's my boss in repository design and I owe him the privilege of being here today.

I would like to just provide a brief update of where we've gone with repository design over about the last six months since SR submittal and talk a little bit about the design evolution process.

The next slide shows some background information on why we did this. The waste package design is subsurface design. We're fairly well evolved for SR with the surface design, which everybody felt was pretty straightforward. Using common technology was less important to site recommendation, and there are some areas there that we wanted to look at. We also wanted to look at the construction sequencing and costs of the repository, given the likely funding constraints that these programs experienced to insure that we could meet DOE's goal of getting the repository built in 2010 with LA submittal in December of 2004.

Next slide was discussed this morning so I'm not going to re-review the same material again and I'll just skip that one. Go to this slide that looks at the process.

We have a series of six studies that we performed. Underground layout, some look at early performance confirmation testing, ground support and invert design, how to emplace waste packages and drip shields. Primary focus on surface facilities, and then looking at any requirements there may be for potential aging. We had a couple of support studies to make sure that we weren't getting into trouble with our science assumptions. Came up with some recommendations for further review in March, and have proceeded to this point in time. We're in the process of reviewing and approving the changes. I would have to characterize the changes at this point as being fairly minor. Basically no changes to invert, or very minor changes suggested in ground support and fairly minor constructability changes in subsurface, but some more significant changes in the surface facilities.

If we go to the next slide, you can see in the surface facilities, we've moved well away from a very large wet storage facility that would have been very difficult to build in the time period permitted to smaller phased facilities, one wet and two partial emplacement dry facilities. It does provide a phased facility, but it is a full capability facility up to 3,000 tons per year. Subsurface, looked at one panel to five smaller panels that I'll show in the next few slides, and looking at a-- considering a wheel transporter instead of rail gives a lot more flexibility in both building design and movement of waste packages around the site, particularly when we're into multiple buildings.

The next slide shows changes that we're considering at this point in time were not part of the original studies on waste package looking at potentially simplifying closure and potential improvements. The waste package through-put and the surface facility design is the bottleneck, as you would expect, with the complex closure wells.

We're also considering an off-site training facility. This would be a facility where we would mock-up the technologies for welding. Post-weld heat treatment, could conduct operator training, have in operation several years before site operation for operator training and also to mock up our original equipment so when we installed it in the repository, it has already been tested.

The next three slides I'm going to go through fairly quickly. The first one shows the SR base line subsurface layout. The next slide will show the revised layout, and the third slide shows superinfusion of the new over the old.

This is the current design that's being considered. It has five face panels. The first panel is small, eight drifts, hold about 4,000 tons. The first four panels are enough to hold 70,000 metric tons at a two-meter spacing. That was just our assumed spacing to conduct the layout and see how much real estate we required. And as we identified, we will start some performance confirmation testing initially in Panel 1.

The next slide shows the new layout in cross-hatch red versus the SR base line and you can see that it uses essentially the same real estate. We've come just a little bit farther to the east. We've cut off a portion to the north and to the south where we have potential rock fracture issues and uncertainties in water table.

There's approximately nine kilometers less total excavation for more usable drift lanes with this current design. So for both the surface facilities and the and subsurface as a result of the studies, the construction cost is reduced.

The next slide shows the potential PC test facility. The commitment here is to start performance confirmation testing early with initial emplacement. This shows one concept that was looked at in the studies, which is a parallel drift with some observation alcoves off to the side. The timing for installation of that would be concurrent with the construction of Panel 1. And Panel 1 is achievable to start operations in 2010 with concurrent construction started on Panel 2.

The next three slides show the current waste handling facility design and two or--and then the dry facility with a couple of options. As you can see, and I think the Board probably remembers, the SR design used very large wet storage pools, a very, very large building. Very difficult to construct, particularly in a dry site, and I had some--really locked this in to manufacturing and other technologies very early in the designing construction process, so it didn't offer much flexibility in our design.

Where we're considering going is three-phase process facilities with initial dry transfer facility that has some processing capability. A wet remediation facility and then a second full capacity dry transfer facility. I believe it gives us the opportunity to learn from the first facility on line and be able to enhance the design and equipment that we procure for the later facilities. Say, on this, we also show one of the surface storage areas. We now think for staging input to the repository we need, at most, 300 or 400 tons of storage, perhaps even less than that, and it would be a very small pad at this point in time.

The next slide shows the concept that we studied for the initial dry facility, and then the full capacity dry facility. These are currently being worked in the design process to come up with more discreet sizing and processing capabilities.

The last slide I have looks at potential aging capability up to 40,000 metric tons. We don't believe we need aging. We don't believe it really does much for the design at this point, so we don't need the pads, but we did want to look to see that we did have space for potential pads. There is a requirement for retrieval and we wanted to make sure that we had space for future retrieval if it's required.

That concludes my presentation. Be happy to answer any questions.

CRAIG: Thank you. That was early. Well done.

NELSON: I've got three questions that maybe could be answered all together. You talk about wheeled vehicles versus trains, and I'm wondering whether that means underground transfer or actually using wheeled vehicles for placement. And if you're thinking about that, does that free up some degrees of freedom in slope and orientation of the drift? And how did you establish the orientation of the drifts?

PEDERSEN: Okay, well, I think for the drift orientation, I'm going to ask Al Linden to address the question. He has probably participated in the study and is probably better qualified than I am to answer it. The answers to your questions are yes, we are considering them for movement of the waste package down to the drift, but not actual placement. We still have a gantry because we have a drip shield.

NELSON: So you're still constrained to general rail slopes?

PEDERSEN: No.

NELSON: No? Okay.

LINDEN: On the matter of the drift orientation, there was a analysis done approximately two years ago to determine the orientation and it was established that, I believe it was 30 degrees off the maximum fracture orientation to reduce the sizes of the rocks.

NELSON: So this is the key block study--

LINDEN: Yes.

NELSON: --that was done, not really considering so much the observed properties that we see now to the lower left.

LINDEN: Yeah, it was the--the study was done, I believe it was most of that information was--

NELSON: Yeah, I think so.

MEMBER: And, Allen, can you answer the slope question, and turn radius question?

LINDEN: Yeah, the wheeled transport that they are looking at is capable of handling tighter radius turns and higher gradients. The plan layout that we have right now still, we have not modified it. It still maintains the flexibility to go with either the wheeled or the railed.

NELSON: And just one final question. If I look at the layout it appears to me that Panel #1 is mostly in the upper lift, maybe submiddle MON. Was there thought given towards extending an early panel to understand the behavior of the lower lift in which most of the repository lies?

LINDEN: Panel 1 is broke down into 60 percent, as is actually in the lower lift, and 40 percent is in the middle MON.

NELSON: That's not very apparent from the layout, so if there is additional information on that, that would be good. Do you have a slide?

LINDEN: I actually have a slide here for--

(Pause.)

NELSON: This is almost as good as the Homeland Security color coding.

LINDEN: It actually shows up better on my computer. The green is--it's very hard to see, actually, I'm sorry about the colors. This is Panel #1. And if you see the green, that is the middle MON lift. And you see the band that runs through the there. On the outside of Panel 2, here is the blue, which is the upper lift. We have some upper lift in Panel 4. This yellow band is the lower lift and then we have a little bit, you can see the purple. And on here, and there's a little bit there, that is your lower MON.

NELSON: Okay, and just finally, how much below the level of the CRB is the Panel 1 placement level?

LINDEN: Top of my head, I believe it's about 20 meters.

NELSON: Thank you.

CRAIG: Dan Bullen. Probably has a question about blending.

BULLEN: Bullen, Board. About what?

CRAIG: Blending.

BULLEN: Well, not quite, but Bullen, Board. Could you go to 12, please. My question this morning was what's the minimum requirement to get a facility up and operational. And I'm assuming that that's everything that's included with the 1 and the circle around it, Phase 1?

PEDERSEN: Yes.

BULLEN: So you have to have that. So you don't need a wet storage pool to get up and going?

PEDERSEN: That's correct.

BULLEN: Okay. So that's going to save you some time in construction in not making a big building. But I still get a little perplexed by your nomenclature here, and if you would have just said a couple of half capacity buildings I would have divided it by two and said they each have a capacity that are equal to each other. So when you give me half a capacity building that has 500 to 1000 metric tons and then the other half happens to be 2000 to 3000 metric tons, it looks more like a third, two-thirds or a quarter, three-quarters. So either take those numbers off or don't call it a half capacity building, whichever the case may be. And so is it not a half capacity building with 1000 metric tons, or is it a half capacity building and they each have--

PEDERSEN: Well, the intent when we did the study is that they would be half capacity building. And we would have a total processing capability of up to 3,000 metric tons a year.

BULLEN: Okay, so the bottom is the total, the 2,000 to 3,000 is the total of the two for Phase three? When you say dry two has 2,000 to 3,000 capacity. You're just confusing me with nomenclature here.

PEDERSEN: Yeah, we'll fix that.

BULLEN: Okay, and then I--you just raised one more issue with your last slide, which is the storage pads. One more. 14. And you mentioned that, you know, retrieval is one of those issues that you have to be concerned about, so on these pads will there be shielding devices so that you can bring 1,000 R per hour waste package out, or where are you going to put it?

PEDERSON: Well, clearly, there would have to be shielding if you brought a package out, and how that would occur would be designed.

BULLEN: It just won't happen very fast is what you're saying?

PEDERSEN: Exactly. We just wanted to--the purpose of this study was to demonstrate we had capability.

BULLEN: Right. Thank you.

CRAIG: Richard.

PARIZEK: Parizek, Board. On Slide 6, you talk about replacing large full penetration welds. Is this also for the C-22 or just for the stainless steel, sort of a modification--

PEDERSEN: Just for the stainless.

PARIZEK: The question really relates to the previous speaker. If you take 10,000 packages, take the diameter, take the number of welds you've got 1,000--I don't know what you have, 1,000 kilometers of weld in C-22. And the question is how is that stuff performed? And we didn't see any corrosion information presented on the welds for C-22. Now, that's the previous speaker and that was the question I was going to try to get him to answer before.

PEDERSEN: Welds. They are included in the program along with the base model.

PARIZEK: Are there data, though, already in the welds?

PEDERSEN: Oh, there are data, yes. The welds and the long-term corrosion test facility, and more recently there are thick section welds that mock up the welding process on the closure weld. And there are in test electro-chemically and the whole range of tests.

PARIZEK; Thank you. Thank you both.

CRAIG: Carl Di Bella, do you have a question?

DI BELLA: Thanks. Carl DiBella, Board Staff. I didn't think you saw me, Paul. I've got a question about the repository layout. Could you put Slide 9 up, please?

Okay, here you're comparing the SR layout and the proposed layout. And the SR layout is these blocks outlined in blue with the so-called primary block being the upper two-thirds of that left block. And your new layout, the one I understand is being used throughout the project, but hasn't yet become the formal base line, basically uses that primary block except for the top part. It's the red cross-hatch part, and then goes over and, at least for the hot design, uses a little bit of the left of that lower block, I think it was called before, although it's not necessarily lower. Now, my question is this: In its sufficiency statement that NRC sent to you last November, they said that--that sent to DOE last November--they said that if DOE were to use a design other than the SR design, meaning like a low temperature design, or they also said or one of the designs shown in the SSPA, that additional information would be required. So my question is, for the new layout you're using now, the red cross-hatched area, have you gone back to NRC and asked them what additional information is required? And if so, what is that additional information?

PEDERSON: Yeah, to my knowledge, the presentation that you've seen has been given to the NRC in this forum, but we have not had a detailed discussion with them to see what additional questions that they would have on a design for surface or subsurface for both pre or postclosure safety. We do have a number of meetings planned with the NRC, but I can't, off the top of my head, tell you what they are.

CRAIG: Last question is Dave Diodato.

DIODATO: Diodato, Staff. I wanted to talk about your potential test facility on Slide 10, briefly.

What--you mentioned performance confirmation activities that could be performed there, so what I'm wondering is if there are any specific performance confirmation activities you had in mind that could help you to evaluate the question of whether a hot above-boiler repository or below-boiler repository helped you to make that decision in terms of specific criteria, if you can envision at this time that would be conducted, tests there?

PEDERSEN: I couldn't address that. I don't know, Mark, if you can address it, or--oh, Jim can.

BLANK: Jim Blank, Lawrence Livermore National Lab.

The emplacement drift #3 on that slide is an accelerated, thermally accelerated drift, that would have ventilation that would least on the order of five to 10 years so that by the time the 50 to 60 year test is over, we will have seen the thermal pulse for a 10,000-year repository situation. That is it'll have a pre-ventilation and peak followed by some cooling. And then a second peak that's the post ventilation peak, all occurring with in the 60-year time frame, followed by cooling. We can set the entry side of that drift to have LTOM temperature peaks in the post ventilation, and the exit half to have HTOM, or alternatively, we could choose to use two different drifts and have one of them have the LTOM temperature history and the other one have the HTOM temperature history. So yes, we are thinking about that. We're thinking about ways we can make that happen using aged fuel or derated waste packages. But to get a in-drift temperature and humidity history, that's an accelerated version of the 10,000 year history.

DIODATO: Well, that's interesting. That gets about half the question. The half that's hanging out there still is what would you look for that would help you to make your--evaluate your decision about whether to go above boiling or below boiling?

BLANK: One of the key things I would look for is, in the presence of drip shields, I would look for condensation driven loops that would provide a steady source of invective water to the waste packages in the two different options. That would be one thing I would look at. Another thing that you could look at would be changes in the near field, geochemical or geomechanical changes.

DIODATO: Thank you.

CRAIG: Okay. Thank you very much, Gordon. I think we're now at the end of this session and it's time for a

15-minute break.

(Whereupon, a recess was taken.)

CHRISTENSEN: If my colleagues on the Board, most especially, could take their seats I'd appreciate it.

While you're doing that, I would like to note, a number of individuals have commented on the need for the Nuclear Waste Board to give some very serious consideration to transportation issues, perhaps more broadly, issues dealing with waste management. So I would like to announce that in fact this has been a matter of some consideration of the Board and that we will be having a panel meeting on the 10th and 11th of December devoted exclusively to waste management issues. This will be the waste management panel. And, most particularly, focusing on transportation issues. That will be--that meeting will be here in Las Vegas. The details of that meeting will be posted on our website relatively soon. And I encourage as many of you as can and are interested to join us in that meeting.

I'm going to declare that we have a quorum and begin. My name is Norm Christensen. I'm a member of the Nuclear Waste Board, and I'll be chairing this last session of this board meeting. This particular session is to be devoted to the Total System Performance Assessment, otherwise known as TSPA, and related barrier analysis.

According to regulations, TSPA is the primary means by which the acceptability of the repository will be judged. It is a very complex numerical model, or perhaps better, a group of numerical models, and has been the subject of much Board discussion and many Board comments. Most recently, in our January 24, 2002 letter, we stated that, because of gaps in data and basic understanding, we have limited confidence in the current TSPA estimates.

Today we'd like to continue this discussion and concentrate on two topics related to TSPA. The first is what appear to be rapidly-changing estimates of repository performance and the differences in these estimates produced by different organizations. I'd like to show you two slides that illustrate some of these changes.

The first slide shows three estimates of the mean dose rate for the nominal scenario produced by the Department of Energy in the year leading up to site recommendation. The first line on this slide marked 12-00 is from the TSPA Site Recommendation, or TSPA SR. The second marked 6-01 is from the SSPA, the Supplemental Science and Performance Analysis. And the third curb marked 11-01 is essentially the official performance estimate used in the site recommendation and final environmental impact statement.

The second slide shows differences in mean performance estimates or the igneous scenario. That is igneous intrusion. The black line is from 12-00 TSPA SR, and the blue estimate is from 6-01, the SSPA. The point of showing these slides is just simply over roughly a 12-month period, rather significant changes occur in the kinds of outputs that TSPA is giving. The changes in the mean estimates over the course of one year shown in both slides were due to changes in assumptions, models, approaches and data. Some of them affect the 10,000-year regulatory period while others seem to effect the post 10,000-year performance.

If we looked at other organizations, such as the Electric Power Research Institute, EPRI, we would also find changes in mean performance estimates, some that are outside the bounds of the three DOE estimates I've just shown.

Now, the Board is interested in learning about the sources of these differences and their significance and what they can tell us about our understanding of the repository system and the ability to estimate performance.

The second topic we'll deal with will be devoted to barrier analysis. That is using performance assessment to estimate the role of different barriers, natural and engineered, played in achieving waste isolation. We'll hear two presentations on these topics. The first from

John Kessler, with the Electric Power Research Institute. John is the Manager for Spent Fuel Storage, Transportation and Disposal, at EPRI. He will be followed by Peter Swift, from the Yucca Mountain Project. Peter is the BSC Subproject Manager for Performance Assessment, Scope and Strategy, and Manager of the Total System Performance Assessment at Sandia National Laboratories.

Following these two presentations we will assemble a round table to discuss some of the issues raised, and I'll give you more on that later.

And I'm pleased to invite John to come forward for his presentation. I'll follow the same drill that my colleague, Dr. Craig did, and we'll have the dinger go off five minutes before the end of your talk.

KESSLER: I'll do my best to dance around the table here. What I'll talk to you about today is pretty much what Norm just talked about. I'll go through quickly, very quickly, in the 20 minutes I have, our Total System Performance Assessment code, I'll quickly mention the scenario that we included and excluded in the model. You're going to see the results from some of those recent results, then the barrier analysis, and then I'm going to finish up by talking about the role for TSPA in demonstrating safety, since there's this clear interest in understanding how we can rely on it when they change so much.

As opposed to the DOE model which uses a Monte Carlo approach to doing probabilistic assessments, we use a Logic Tree approach. We use, in this case, a limited number of branches instead of realizations with discreet probability and parameter value for each branch. In our particular case we have a very limited set of branches that subsume or abstract a lot of processes. We have a branch for the infiltration rates, which is really that flow near the surface that's going to go deep into the mountain. And we have a focus flow factor that takes that general infiltration rate and focuses it into some areas and obviously defocuses it at other areas by the time you get down to the repository level. We have a seepage fraction set of branches that basically says the number of containers with invective release where you actually have active dripping past so many of the containers. We have combined branch of the solubility for radionuclides and alteration time based on the assumption that the chemistry that affects one in one direction will affect the other in that same direction. And then the last branch that we have is retardation in the unsaturated zone and the saturated zone. So the implications of the simplified approach and the Logic Tree that we're using is that the approach provides an estimate of the mean or the probability weighted mean, but that we're not going to capture the full uncertainty range since we've averaged in in each one of these limited number of branches, a lot of the detailed realizations that you see with the horse tail diagrams you're getting from the DOE results. So that also means that we have a strong need for sensitivity studies to make sure that we've got the right branches when we proceed there.

So, given the limited amount of time, I'm going to launch pretty much right into the model and the results.

What you'll see considers really only the normal release mode, at least at present. We're in the process of adding some things that are more than just the normal release mode. So really, that normal release mode is the container and cladding has got to fail for the diffusive release to begin, and the drip shield failure allows invective release where, that is, local flow is high enough were you actually would have dripping on containers.

And then we have a combination of an invection and diffusion through the unsaturated zone and saturated zone to 18 kilometer fence posts. The standard normal release mode. What's not in the EPRI model, we don't have igneous activity, human intrusion or colloid aided transport, yet.

This result is also another one of those hot-off-the-press, we are in the middle of incorporating new information, fixing things that we found needed fixing from last time, and--whereas aiding diffusion to our results. And this did change our results quite a bit, at least at certain times. So this is our new total dose, again--oops, I think I cut it off the end here. Oh, I've lost the labeling on my axis. --is the probability weighted mean does in millirum per year for the, what we call the big eight or 10 radionuclides that contribute the most to dose. And what we have here is an initial rise due to our assumption of one waste package that failed in emplacement. And this release at these early times is dominated by technicium and iodine and it is mostly diffusion related that's now causing that--I'll show you in a minute. You'll see how adding diffusion in the one early failure changes our early release assumptions. And then at later times we have release dominated by the actonides, primarily neptunium, and basically the slope of this, the graded, which increases is pretty much in lock step with our container failure model, with some offsets for the delay of time in the UZ and the SC.

Since Norm and Leon were both interested in how has our model changed, I thought I would sort of go through that. The last report we put out was our February, 2002, Phase 6 report, and I'm going to call that the old results, versus the ones I just showed you. And what have we changed since then? Well, we fixed one error related to tracking release from the zone with this flow focusing that increased things by a factor of 5-1/2. We incorporated an extra branch for a low probability of considering a much higher seepage fraction. That is a much higher fraction of containers that would actually get dripped on. We did that. That increased doses by about a factor of 4. When we add a diffusion release from all containers, which we only had from some before, it's a mixed effect depending on the time that we're looking, as I pointed out in their previous curve. So the revised probability weighted mean annual doses are higher now for the 10,000-year does. Our old value was basically zero. Now that we've added diffusive release, our new value is up at about 10 to the minus 3 millirum per year. The peak dose, the old one, is about 10 to the minus 1, and the new is right up about three millirum per year.

So seeing the same curve, and not having the graphic expertise of John to deal with, I used the old sharpie method of adding our results on top of the DOE results to show where we were, where we lie, basically. So that the old results are--where we were in February of this year is here. With the addition of diffusion and the corrections and adding additional branches, we now sit up in this range. And, again, I'm speculating. Maybe this will come out in the discussion afterward. We suspect some of our differences are, is that we have a delay here. I believe that these both assume some early failure release. Our delay from these early rises is due to our improved, basically, we take more credit for the saturated zone delays in transport than I believe the project does. And the fact that we're failing more than--more containers basically earlier than the project does I think means that we're higher there, but again, we haven't quite discussed that all the way through with the project. And we slope up slower, is due to a multitude of reasons, one of which certainly is our container failure distribution compared to the projects.

I've probably talked my way through half of these.

Yes. We assume better saturated zone performance meaning we've got slower travel times. I'd say both of us use diffusion models that are probably conservative in the sense that we assume a very good diffusive half-way connection between the container and the surrounding rock. And now we both need to work on that, Abe.

EPRI assumes one early container failure. I believe the DOE plot shown assumes less than one container failures. That's right, Pedersen? Like at about quarter or so on those?

PEDERSEN: For those plots that were shown.

KESSLER: Right, for the plots that were shown, it was about a quarter of a container failed early, probabilistically, at least back then. And I think we assume better cladding and container performance in terms of that slope-up at later times.

Other things that we do that are different, our time steps are much courser. That is we're taking thousand-year and 10,000-year time steps versus the much shorter time step the project sees. And, as our curves tend to be a bit smoother, we're also going to lose fidelity, so to speak, in terms of seeing the effects of any processes or events that occur over very short time periods because of that.

We also fix our long-term climb to full glacial maximum. We don't allow variations. That's another thing that may change our results a bit.

Getting down into an example of a few details here, our biosphere dose conversion factors are somewhat higher. I think we're probably making fairly conservative assumptions about the importance of non-drinking water pathways. Dust inhalation is one that jumps out at us, is perhaps a little bit different about the assumptions that we're making.

We've also based doses on a plume size with a groundwater flux of 750-acre feet a year for the total groundwater for the primary dose releases. And it's probably an underestimate again. I think that's more our modeling artifact than real in the sense that we have a very limited vertical extent of our saturated zone model that's confining that plume size.

Okay, on to barrier importance analysis. Our purpose was to assign some sort of general value to the various components of the Yucca Mountain system. Our motivation is sort of a defense in depth. We're asking are all the eggs in one or two baskets. A lot of concern about the waste packages doing everything. The natural system is doing nothing. We wanted to be able to evaluate that. So we wanted to provide insight on important features, events and processes or FEPs. What we do is some barrier neutralization, which are more the one-off or more than one-off analyses. And I'll give you one or two examples of those.

And then spent some more time talking about our hazard index approach, which is one-on and then another on and then another on that some of you have seen--I revised that--which is sort of a variance of the full neutralization approach.

Okay, an example or two of our analyses for the one-offs. This maximum likelihood wet branch is we did some analyses basically on one realization that represents, or at least in a branch that represents a fairly high probability, but nevertheless, was one that probably would tend to maximize dose and maximize effects here. So let's assume that our base case is this solid line, and if we take out just the drip shield, we see in our analyses that the drip shield--removing the drip shield by itself doesn't seem to change much. But when you take out both the drip shield and the container we get somewhat of a significant increase in the doses, such that our 10,000-year dose is up here around 10 or 20. The peak may be at around 200 or 300.

Still pretty reasonable doses considering that we are assuming, I would call it the Sagüés disaster scenario, where all of the containers and all of the drip shields fail early, and we certainly went what if that, into the end there where we assume they absolutely all fail. And, I would argue that having doses that peak in the 10s to low 100s millirum is--gives me confidence actually. Assume, making that kind of a severe assumption.

Another example I threw up here is let's just take cladding out as the only one barrier that we take out. Again, you can see that the doses would go up a little bit. What I wanted to show was that there's cladding, has potentially a bigger benefits if now you take out the drip shield and the container and the cladding. You could also look at this as saying we're removing the major EBCF, the major engineered barriers here. And again, with those gone, we're still up here at around 100 millirum per year with the peak when we have, say that the actonite is starting to come out in the 100s of millirum per year for this one scenario, which again, somewhat overestimates total dose. But again it gives you an idea of clad--effects of removing all three barriers.

Where we remove all the barriers is what we call our hazard index approach. Really, I eliminate all the barriers, so this is very much a theoretical exercise. And then we're going to add the potential barriers one by one, and the amount that's hazard index, or we do it in terms of dose, is reduced to indicate some sort of potential barrier importance for each barrier that we're adding.

So begin by suspending your disbelief because the starting point is the highest imaginable dose. Take all 70,000 metric tons of high level waste, dissolve it into 6/10th of a cubic meter of water, which is roughly how much a person drinks in a year, and one individual would get a dose of something like 10 to the 17th millirum per year. Purely theoretical. Obviously. But why are we doing this? We want to add all the FEPs and get some sort of, you know, semi-quantitative estimate or understanding of the importance of each one of these barriers, is one way of going at it. We can also include things like basic engineering decisions, the repository layout, how much you spread the waste out should affect your dose and we wanted to capture that.

So we looked at these barriers of groups of FEPs, however you want to look at it, sort of in this order. Think of this as starting from the inside of the package and working your way out the entire system. So the first one here, we have it labeled 3000-year alteration time. The waste, instead of dissolving instantly, dissolves over 3,000 years. The next one is moderate solubility adding in solubility limits. Then we let the cladding fail over time, then we let the containers fail over time. Here is adding now as part of it with invection and part of it with diffusion, so we've got sort of a limitation on the number of containers to get wet, and then a diffusive barrier, and then we disburse the packages throughout the repository instead of concentrating them all in one place, adding sorption in the EBS. Then we start adding in the first five kilometers in the natural system. Then we add basically sorption to that natural system, then we add some more of the natural system, then we add out to 18 kilometers and then we throw in the BDCFs for all the other pathways.

Okay, so let's start at the top. Hazard index. There's my 10 to the 17th. Really, only the peak of this matters. Throw in 3,000-year alteration time, basically adding in the barrier of the waste form dissolving over time and bring that hazard index down by three orders of magnitude because it takes 3,000 times longer to dissolve things. Now we add in moderate solubility and solubility limitations, mostly from the actonides, bring that theoretical dose down further. With the cladding failure over time it helps at the beginning, but eventually all the cladding fails so you're back to where you were. Now we add in containers failing over time and indeed that reduces things by several orders of magnitude, again, if all of the containers started failing over time, you get back to where you were. Drip shields failing over time doesn't really help much more when we add it that way. Reducing the number of containers that can actually get dripped on and allowing only diffusive release from the rest bring it down some more. Spreading out those waste packages over the repository rather than in one place, again, gives us dilution and that brings the dose down a lot. If we add in some sorption in the EBS it brings it down and you see, and so on. We can actually show how this theoretical dose decreases with each barrier that you add. Okay, well, it decreases this amount for each barrier because we added them in that order. Order matters in how you add these barriers. And we can tell different stories depending on which way we add them. So here's one where I'm basically lumping all, what are mostly engineered barriers. Let's throw all those barriers in it first. So we start here and come down to here when we add in all the engineered barriers. That's something like nine-ish orders of magnitude. And then we add in all the natural barriers and we're down however many more orders of magnitude that is.

Now, what if we switched it? What if we put in the natural barriers first? You can see the relative contributions of those barrier's changes, but there are still big contributions from all of those barriers. Actually, when we add in the natural barriers first you can see that our peak here is right about at the dose limit. So what we're arguing is that, even with just the natural barriers, we're pretty much able to do the bulk of the job or at least the Yucca Mountain natural system is able to get us most of the way down to the regulatory dose limit. And then if you add the engineered barriers on after that, it only reduces the dose so much more.

So the conclusions are that many barriers can contribute substantially to performance. Not all the eggs are in one basket. The amount of performance does depend on what other barriers are assumed to be there. The example is the engineered barriers. If you add them first, you get nine orders of magnitude reduction, but if you add them after the natural at zero to five orders of magnitude reduction, depending on what time you're looking at. And the natural barriers alone reduce the dose to roughly on the order of natural background levels.

So what's the role for all these TSPAs? As Norm just pointed out in his comments, the concern is expressed over the instability of TSPA results. They keep changing on us with time so how can we trust them? I would argue that that's to be expected as we learn more in potentially the design changes. We've certainly seen these kinds of features contribute to changes in the performance assessment. I guess I'd say that I would trust a performance assessment that changes with, as we learn more, than one that was pretty stable as we learn more.

As we look at these TSPAs we have to keep them in context to what we're really after, which is all the other aspects of insuring safety. For example, we have regulations that are pretty protective. We've got a dose limit that's less than 10 percent of natural background. In addition, that dose limit is to some reasonably maximally exposed individual, not just an average person, but an RMEI. Multiple barriers are required. So what if we're wrong about a few aspects of our TSPA models? The NRC multiple barrier requirement off-sets the risk of being wrong on one of those barriers. We have some margin. We know how we can deal with things. Many of the other aspects of our current performance assessment models are conservative. That gives us some additional confidence. And this use of margin also provides some insurance, meaning we're not right at the limit, but we're still below it. So we can be wrong and still feel like the majority of the barriers are likely to work and we will be okay.

So what are some alternative then to TSPA? Some would argue, well, maybe we better go back to the 10 CFR 60/960 subsystem performance criteria. I would argue that no, we shouldn't do that. There will be things like the groundwater travel time, release rate limits, higher level findings on all kinds of subsystem performance. I think that's a pretty specious argument. We're still going to require performance assessments if we went back there.

Thousand-year groundwater travel time requirement is going to mean we'll have to generate performance assessments for that kind of thing.

Furthermore, I think after all the work we've all done, our understanding is solid on the idea that some system components matter more than others. And that should be part of the regulation and I argue that the current Part 63 has that in it.

Should we use multiple lines of evidence? Yes. Many TSPA models already have multiple lines of evidence built into them. There can be independent ones as well. Those multiple lines of evidence are best used with TSPA. They can help you understand your performance assessment and natural and other analogs, if imperfect, still provide good insights, so they should be done.

Performance confirmation activities. Are they going to be helpful? Absolutely. Long-term testing to challenge some of those TSPA models will certainly be an important component to having confidence that whatever regulatory decision is made is the right one.

CHRISTENSEN: John, you're about done.

KESSLER: I've got two more to go. Sorry, Norm.

So we've got two major roles: The quantitative regulatory compliance and we want to tell many stories about Yucca Mountain's safeties and risks by using alternate models, barrier analysis and the what-ifs that I showed you.

Well, certainly, both the compliance assessments and the stories are model dependent. The stories change as the models change, and our confidence in the regulatory and social decision will likely come from a combination of those compliance assessments and the stories we tell with them.

Now I'm done. Sorry I'm so long.

CHRISTENSEN: Okay, thanks. Questions? Dr. Runnells.

RUNNELLS: Runnells. John, the example you gave of the importance of the sequence of adding the barriers was one that I can't put my head around in terms of reality. You have the natural barriers dropping that thing by some huge number, nine or more orders of magnitude. Then you add the engineered barriers. How is that physically meaningful? I mean I can see it mathematically, but I can't imagine how you could actually do it because by the time you talk about adding engineered barriers, the material has already moved out of its source location. So just explain that to me, please.

KESSLER: We start with some source term, okay? The engineered barriers basically would, if we add the source term on, we basically have an instant release if we have no engineered barriers. Right? And so then it would--the natural barriers then govern how that release comes out through the system with time. Now, if we add the engineered barriers, basically what we're doing is, we're saying it's not an instant release anymore, but it's a release rate that's governed by the way the engineered barriers perform. So that's how it works functionally in the model.

RUNNELLS: Does that have any physical reality? The other way around I can see it in terms of physical reality. But the way you just described it, how would you physically, actually, possibly do it? You couldn't, could you? It's just mathematical manipulation.

KESSLER: It's more than mathematical--you're half right. I agree with you that eliminating all the engineered barriers has, in its strict sense, no physical meaning. Nevertheless, what we were after was to try to get some understanding of what happens if the engineered barriers are pretty much degraded and we're only relying on the natural system, or vice versa. So there is some physical meaning, I would argue, it's the idea of removing the functionality of these engineered barriers. Don't think of this in terms of we're actually taking, physically, the barriers out. So we have all the same geometry we had before. We're just taking out the functionality of those barriers.

RUNNELLS: I'll catch you later because what you're just describing is not what I think I asked. I'll catch you later.

KESSLER: Okay. Sorry.

CHRISTENSEN: Mark?

ABKOWITZ: Abkowitz, Board. John, I was curious if you could give me a sense of the--from a quantifiable standpoint, how many inputs are required for your model as opposed to the number of inputs that are required for the program's model?

KESSLER: I think the program's model has something like 3,000-ish parameters, about 1,000 of which are adjustable, something like that. You need to ask Peter when he gets up here, but it's something like that. I haven't counted up the number of parameters that we have, but I would argue it's more like 100 or 200. And only a handful of which actually get varied in a probabilistic standpoint. And most of those are subsumed in the definition of each one of those processes that are in those branches.

ABKOWITZ: So which one do you feel is more accurate?

KESSLER: I think that they are both modeling the same process. I think that, while--you know, when I showed our overlay on theirs, we're capturing the same behavior of the system, maybe from a slightly different approach. Well, definitely from some different approaches, but the general behavior of the system and what are the important processes and parameters in those. I think more or less we're on the same page on those things. Okay?

ABKOWITZ: That was somewhat of a loaded question so I could offer a comment.

KESSLER: It sure was. And the point is is that I can't tell you which one is more accurate. Ask me 10,000 years from now and I'll tell you.

ABKOWITZ: Well, I want to offer a comment. You don't need to defend yourself any more at the moment.

I get concerned about how well we can microscopically analyze complex systems. And so I think we get ourselves into a situation, a delicate balancing act in terms of trying to provide as much explanatory detail as I can, but it puts us in a situation where we have so many inputs that we know so very little about in terms of empirical data or other forms of data that we have confidence in.

So one of the things that I'd be interested in exploring as the TSPA process goes down the road, not just from your efforts, but from those of the program, is the extent to which we gain or lose sensitivity as we draw back from the very microscopic way of looking at this to a, maybe a more, what I'd call screening level. But it could be that we have more confidence ultimately in the screening results, even though we've given up perhaps some of the theoretical precision that could have been gained otherwise. So that's something I'm personally very interested in watching as we move forward.

KESSLER: One of the things we all need to be doing are sanity checks. The back of the envelopes with some simple calculations. I know the project has done it. I know we do it. What I show you, of course, is the bigger picture, but all those are happening behind what I've showed you here.

CHRISTENSEN: Debra Knopman?

KNOPMAN: Knopman, Board. John, let me just make sure I understand why you're--why EPRI is doing this barrier analysis. There are multiple reasons why one would undertake this. One is to try to demonstrate the robustness of the barriers, which appears to be what your primary motivation is. Another is to use it for insight, less for the results, per se, of certainly the cumulative results--just picking up on Mark's point--more for what it tells you about what you get when with various processes. And, what the consequences of uncertainty are in those for any particular process at certain points in time. And that in the consequent guidance you might get for a research program or performance confirmation or whatever. What's EPRI's motivation here for doing this analysis, and what have you actually--what have you gotten out of it besides being able to say natural barriers seem to do pretty well without anything else?

KESSLER: We were after a couple things. We wanted to--our main concern, what started us on this is are all the eggs in one basket. Is really the waste package doing everything or are there other parts of the system that, just because of the way we're looking at it, we're not seeing it. And we wanted to look at this, let's take all the barriers out, let's add them in in different ways to understand how much a barrier might contribute if other ones, due to our uncertainty or some bad news from performance confirmation program or something, causes us to rethink or potentially, in the extreme, eliminate the functionality of some barrier. So we were after that. So we wanted to do something just simple in the sense of let's take a barrier out, let's take them all out. Let's add them to see what's there. Are there barriers that are hidden behind others, in a sense that we were looking at cladding or drip shields or something like that, or absorption in the natural system such that, well, it doesn't look all that important and so maybe for research priorities, it doesn't look that important. But when could it become important? When would it really make a difference? It might make a difference if there are other barriers whose functionality maybe we're depending on, but maybe they are not there due to new information. So all those things we wanted to get some sort of handle on with these analyses. The primary one was are all the eggs in one basket. That was certainly what--

KNOPMAN: There's no uncertainty analysis embedded in your model?

KESSLER: In this particular case there's some uncertainty analysis. A lot of this is for a single branch or a single realization. So these are more sensitivities rather than uncertainties. The uncertainty part only comes in in the question of just asking what if that barrier isn't there. The uncertainty of being confident that we have or don't have a barrier's functionality available.

KNOPMAN: But no uncertainty in the modeling of these processes that produced these results.

KESSLER: In the hazard index, no, not in our model. In the one-offs, yes. We still put the rest of the--all the branches are in there in terms of when we do the one-offs. That's still there.

CHRISTENSEN: Other questions? John, thank you.

I'd like to invite Peter Swift. Peter Swift, BSC and Sandia National Laboratories and he'll be talking about the DOE Performance Assessment and Barrier Analysis.

[1]SWIFT: Well, I'm Peter Swift. I'm going to try and do as well as John did at getting back and forth through this slot.

I want to start off here by acknowledging the others other than me whose work this represents. And I won't say them all, it's dozens and dozens of people, and of course a Total System Performance Assessment is built on the work of hundreds of people. But the people here to be acknowledged are: Rob Howard, who is the deputy manager of this department--thank you, Rob--and the TSPA team, lead for many years by Jerry McNeish, who is the department manager of that group, and towards the back, Dave Sevougian, Don Kalinich, Pat Lee, Sunil Mehta, and I apologize if I missed anyone there. Those are the guys who ran these analyses and to whom the credit should go. George Saulnier is not here.

[1] **"WE HAVE BEEN INFORMED BY THE DEPARTMENT OF ENERGY THAT THEY HAVE MADE CHANGES IN SOME OF THE ASSUMPTIONS USED IN THEIR ANALYSES. AS A RESULT, SEVERAL OF THE SLIDES USED BY PETER SWIFT IN THIS PRESENTATION HAVE CHANGED. ATTACHED TO THIS TRANSCRIPT IS A REVISED SET OF SLIDES AND AN EXPLANATION OF WHAT THE CHANGES WERE AND WHY THEY WERE MADE."**

He was one of the designers of this analysis.

Next slide, please.

Leon had asked me to start off with an overview of what he then covered very well in the one slide that Norm put up, which was a brief review of the overall results of recent DOE TSPA's. So I will walk through that and provide a little more information on them and provide a basis for questioning, and then I will go to new results that this group has not seen yet of an evaluation of what we call sequential addition ("one-on") analyses, similar to some of what John just showed.

Next slide, please.

The most recent work that the Board saw was a presentation by Jerry McNeish in January. Those were the results of analyses done to support the Final Environmental Impact Statement to support the site recommendation. And the model that the TSPA Department developed for those analyses in shorthand we call the FEIS model. It was similar to the model used for what we call the SSPA, the supplementary analyses done in the spring of 2001.

The next full update of the TSPA that this group or any other will see will be the one that supports License Application in 2004. We do not have a full TSPA schedule between now and then. But the current work here, it's interim work, it uses some minor updates to the FEIS model.

An important caveat here: this is not, in an NRC sense, fully qualified work. Some of the models and inputs are not validated. The calculations are controlled, they're reproducible, they're archived, we think they're good analyses, but work needs to be done on the validation of the inputs.

Next slide, please.

All right, this is a figure from--it's actually a figure that I showed to the Board in June or July of 2001 showing the comparison of the results from the TSPA-SR, the Site Recommendation TSPA, done in the fall of 2000, with the new analyses that Bill Boyle talked about, which took into account additional uncertainty and inputs, new scientific data available as of the spring of 2001, and which lifted alternative thermal operating modes.

All three of these are mean annual curves. And Bill showed them as a horsetail a minute ago. I'll show that in a minute. But these are the means, and most of what I'll show today are means.

First-order results that we saw there, what happened when we revised the model? Well, we included some early waste package failures that produced this portion of the curve down here (indicating). Slower waste package corrosion delayed the main rise in dose from there over to there (indicating). Lower solubilities, particularly of neptunium, also plutonium, lowered the peak out there (indicating). We had an enhanced long-term climate model that put in full glacial climates. They produced wetter conditions, and that produced the spikiness in the curve you see out there. Each one of those is a glaciation of North America.

And a very important point, thermal effects are small at the system level. They're not small at the subsystem level. The model that produced these curves had plenty of things that were sensitive to temperature--relative humidity in the drift, corrosion rates, pH--however, those things--the period when temperatures were strikingly different is a period when the packages aren't leaking, except for those that went through early failure. So it's not a surprise to us that we didn't see much of a difference in results. The main action occurs later in time when the temperatures are quite similar.

Next slide, please.

These are the horsetails, the results of 300 realizations shown on each. That's what you get when you do a Monte Carlo analysis, you sample from uncertain inputs, produce multiple realizations. The spread in the results is a measure of the uncertainty in our model output.

The summary measures put on it, the 95th percentile, the mean in red, the median in pale blue, the 5th percentile is off-scale here. The regulatory performance measure of interest here is the mean. That's what the NRC is regulating on. And these are not actually regulatory plots, because the NRC calls for 10,000 years only, which--somebody help me out here--1,000 to 10,000 in just the first line there. And they call for a probability weighted mean annual dose considers all scenarios. This is just the nominal scenario. So that leads us to look at the igneous scenario, which turns out in the first 10,000 years to be well above the phenomenal as probability weighted.

Have the next slide, please.

Where that takes me, then--actually, sorry, I should go to the FEIS update, the most recent results the Board saw. This is the third slide that was on the introductory viewgraph that Norm showed. And here we have the red curve is a high-temperature operating mode mean from the spring of 2001, the black is the fall of 2000, and the two green and blue ones here are a revised model where we took out temperature dependence in the corrosion model. This was due to, basically, advice from our Waste Package Corrosion Team that they were not confident we're going to be able to support that, let's see the results without it, and those are the results we went forward with in the final Environmental Impact Statement, the blue curve. With taking out temperature dependent corrosion, we are really pretty insensitive to differences in the thermal operating mode at the system level.

Next slide, please.

Now, this is the igneous activity that we came to here. And what I've chosen to do here to make sure that we understand what we've done, these are not probability weighted, these are what we call the conditional dose. This will be the igneous dose that people would get if they were there for it. And here is--I guess I didn't say up there how many that is--I think that might be 5,000 realizations pasted in there, but only showing every tenth, 2,500 curves for an event happening at Year 100, and then the dose a person would get living 18 kilometers from the site, or in this case 20 kilometers from the site, in each year following. So we're on the order of I believe the mean up there is around 13 rem in the first year that a person who was there would get. This was the model we used in the fall of 2000, the TSPA-SR model.

That dose would drop off in later years. By Year 2000, the person beneath the site is getting a relatively much smaller dose, about 1 millirem, it looks like. And the dose they're getting is from living on top of contaminated ash from farming and breathing, eating, doing what people do.

The set of curves down here are means from distributions like those above for events happening at different times in the future. So if a volcano happened at the Year 5000, it would look like that pale green curve. My apologies to the people looking in black and white. And the dropoff in the first year peak from these conditional events should be, and is, we've confirmed it, that is the radioactive decay curve. That's the change in repository inventory due to radioactive decay. Most of the rest of the dropoff on those curves is due to the soil removal model that we used in SR. Basically it allows for loss of agricultural soil due to primarily wind erosion.

Now, that's the conditional dose, that's not what NRC is regulating on, they do want a probability-weighted dose, so I'll show you how we get to it from there.

Can I have the next slide, please?

And I know it's in the handouts, this summation sign comes out as an E. You might want to correct that, it is not an E, it should be a summation. What I've shown here in this sketch are examples of hypothetical dose curves, like the ones in the previous slide, just means only, let's say, for events that might happen. Volcano One might happen at Time One, Volcano Six at Time Six, and so on.

Let's assume for a second that the volcano could happen in any year in the future. Our analysis doesn't know what year it could happen in. But a person living at any year in the future could therefore be getting a dose from a volcano that happened far before their time or shortly before or right in the year they're alive. That person's probability-weighted dose is the sum of the probability of all the events through time times the dose associated with each of those events, including the events that might happen after their lifetimes. Those might have a zero dose, but there's still some probability that a volcano didn't happen before they lived, it might happen after they lived.

If you go through this summation through time, the initial times, the first year dose, the conditional dose, is high, but the probability is fairly large that the event hasn't happened yet. It may happen later. At later times, as we go out through time, the probability the volcano has happened before the time that you lived, that probability has gone up. And after 10,000 years, the annual probability is 10^4 times higher than it was in the first year that you're living on top of contaminated ash.

So what happens is that this summation through time starts off relatively low, climbs, and then starts to drop off again as radioactive decay reduces the inventory. This is what we thought the formula would give us.

Next slide, please.

And actually does. The first time we ran it we got this black curve down here (indicating). That black curve there is the mean generated from the conditional doses shown two pages back. The right-hand part of the curve out here (indicating), those are groundwater doses associated with volcanic event. It damages packages within the drifts, takes a longer period of time for those doses to reach the 20-kilometer boundary. Eventually, however, those become doses become significant. In an SR model, by 10,000 years they've exceeded the ashfall dose.

All right, the blue curve here (indicating) and the red one are the SSPA doses. We changed them and we changed the model some, so the eruptive portion of the dose--that's the blue curve--went up and the groundwater portion went down. And we know why they went up, basically reasons are given here. We had some changes in our event probability recommendations from our volcanologists, changes in our biosphere dose conversion factors from our biosphere team--that has to do with the inhalation dose from ash--changes in the wind speed data set we used, and a change in the estimate of number of packages that were damaged in an eruption. And the groundwater dose went down primarily due to changes in the solubility models.

All right, I'm going to--can I have the next slide here? Oh, that's right, I just had to show you that we have the full uncertainty distribution associated with those curves.

Next slide, please.

All right, here--I hope I have enough time to cover this--switching here to the current work. This is work just essentially in progress, it was just finished a few weeks ago, we're still evaluating it, are sequential addition barrier analyses, analogous to the EPRI "one-on" analyses. Sequence is important. John said it and I'll say it. The most obvious example is that if we put the waste package in first, yes, it does mask the other barriers. It doesn't mean they aren't working, it just means that we can't see it if we put that in first in an analysis.

The sequence I'm going to show you here focuses on the natural system. We put that in first deliberately so you can see its effect. And we left out the--not left out--we put diffusion in at the end of the sequence primarily to facilitate comparison with EPRI's analyses published in February. At that time they did not have diffusion in their model.

Caveats: the work here is for insight only. First of all, it's not NRC qualified, or not qualified for NRC use. Second, the NRC does require multiple barrier analyses under 63 Part 115. We're not sure this is it, so I don't want people thinking it is. It might be, but we don't--this is not being presented as part of our License Application. And this is only nominal performance, just remember.

Next slide, please.

I'll start off with this is the bottom line. This is what the system looks like now with all the barriers in, it's the full model as modified very slightly from where we were last fall and winter. The modifications: first of all, we added Sr-90, Cs-137 for transport. We don't need them for what we think of as a real system model, but we do for these barrier-removed models because you get some very fast releases. In the full system, except for the volcanic scenario, they're not there by the time we start getting doses. We once again updated our long-term climate states. The effect is very minor from that. We now are using a regulatory specification of 3,000 acre-feet of water per year. And one other thing not listed up there, these results now do include one waste package early failure per realization. We did that to get a better coverage of the early failures.

So we can come back to that slide later if we need to, but next slide, please. In the interest of time I'm going to move very quickly here. Here are the cases we've got, and you'll see these curves in a minute:

Case 1: No barriers. This is the hypothetical, all waste dissolved directly in 3,000 acre-feet of water per year. A little different from what EPRI has done, but that's 3,000 acre-feet is what the regulation specifies, the NRC and EPA regulations. It's not, by the way, all the radionuclides in the waste, it's just the ones that we're tracking. We don't have dose conversion factors for the very short-lived isotopes in our models, so we aren't counting them here.

Case 2: Added waste-form degradation limits.

Case 3: Added solubility limits and colloidal stability.

Case 4: Added cladding.

Our concept here--and can I have the next slide?--okay, our concept here--this is just a picture of what the mountain looks like--was that somehow we'd manage to build a repository where we left the waste on the surface, bare fuel rods sitting on the land surface. The only barriers functioning here are those associated with the waste form itself. You asked about realism here. There's no realism in this. I'm very confident this will not be a design, but there it is. So this gets us through our waste form barriers.

Next slide, please.

Now we're going to add the Natural System in. Add surficial soils and topography. Basically this converts rainfall precipitation to infiltration. Now we've got a water term, infiltration flux.

Case 6: Add the UZ flow and transport below the repository. Now we've got transport processes, retardation and so on, working in UZ flow and transport.

Case 7: Add the Saturated Zone.

Next slide, please. And I won't stay on this one, this just shows the sam information.

Next slide, please.

Now we're going to put in Engineered System components in these analyses. And here's a question as to why we called the UZ above the repository part of the Engineered System and not part of the Natural System. We could have called it either way. But the main effect here, what we're doing at this step is we're changing the infiltration flux to the seepage flux. Most of that change is due to the drift effects: capillary effects, thermal effects. So our idea when we called this an engineered barrier here is that at this point we're putting the waste in an engineered facility, we're putting it in a tunnel underground.

Case 9: Add the invert. We get sorption here on the crushed rock invert.

Case 10: Add the drip shield.

Case 11: Add the waste package. In both cases we're adding the degradation models with it.

Case 12: At this point we add diffusion in. We'd left it out until now because we were trying to get something comparable with the EPRI results.

Next slide, please. Next slide, please. That just shows the same thing.

The results. Probably many of you have already been looking at it for a while. That is equivalent to the hazard index that John showed. Much of the difference there is due to the different dilution factors, amount of water we used. The curves here (indicating), there's no surprise here. They're listed here in the same order I just listed them through. So you see adding waste form degradation, adding solubility, adding cladding, adding the surficial effects to go from precipitation to infiltration.

What you see on this sequence here--I have a summary over here on the right--in this sequence the UZ below the repository provides the biggest incremental impact on both the time of the dose and the magnitude of the peak. The time of the peak shifts from somewhere before 100 years for all those to somewhere out here, what, 40,000 years I think. That's the first full glacial. And the magnitude shifts of the peak shifts from somewhere in there to somewhere down there (indicating), about four orders of magnitude. The UZ is doing a lot here.

Okay, next slide, please.

This one didn't work very well in the handouts, I apologize. The point of this is just to show a simpler version with some of the sort of noise taken off, so in the components that show the biggest effects. In the interest of time, I think I'm going to slide right over that one.

Next slide, please.

We did one more case to look at--what we didn't have the luxury of doing was the number of cases that John did, where he switched the order, put engineered first, natural first. We have one such case here, our Case 13, where we put the saturated zone in before we put the unsaturated zone transport, just to see--in the previous slides the saturated zone wasn't doing very much, it was effectively masked by the unsaturated zone. And not surprisingly, the same things cause delay or reduction of dose in both. So we put the saturated zone in first, and so you can see a jump there from infiltration to saturated zone transport and see what the--essentially what benefit, in this sequence, you get from the saturated zone itself.

Next slide. Conclusions here. The first part of these conclusions have to do with the first part of the talk.

The changes in TSPA results from the site recommendation to the present: They're consistent with the purposes of the analyses. We had an increasing emphasis on realistic treatment of uncertainty, less reliance on bounding assumptions, which is where we were in the fall of 2000. It's consistent with incorporating new information as it becomes available. For example, the igneous disruption scenario, doses there went up because we had new information about BDCF's, wind speed, and so on.

This point I think is fairly important, uncertainty in relatively few processes drives changes in the results. Yes, we have many, many hundreds of uncertain parameters and we're looking at ways to perhaps make better use of that number, but not all of them have big effects on the uncertainty in the outcome. Waste package performance is important: early failures, general corrosion rate. Solubility limits are important, and obviously things that have to do with igneous activity are important for the total dose.

And the "one-on" barrier component analyses provide good insight into nominal performance. Keep in mind, though, order does matter. If you're looking for them to represent a physical reality, they aren't going to. They're deliberately non-realistic analyses, very much like the neutralization studies in that case where you imagine we neutralize, say, the unsaturated zone above the repository. This is physically impossible, we're not going to remove the top half of the mountain, but we can model it. Same thing happens here where we choose, for example, to model UZ transport before we even put a drift in. That case, by the way, could be conceptualized as thinking of the waste form as if it were an ore body, take the fuel rods and somehow embed them in rock. But this is not physically possible, it's just a way of looking at the system.

And I'm going to stop there.

DR. ABKOWITZ: Abkowitz with the Board. I wanted to make sure I understood the one-on concept. I understand that the order matters, but is it fair to assume that you would expect to see either none or some improvement with the addition of each barrier sequentially?

MR. SWIFT: Yes. If the process is actually a barrier. Diffusion is not, that's why Case 12 where we added diffusion to make a point of saying that's not a barrier because it allows releases from the non-seeping environments under drip shields. That's the only exception to that. Otherwise, yes, you should see either no effect or reduction.

DR. ABKOWITZ: Okay, if we could turn to Slide No. 19. I was curious, you've just explained why the diffusion curve shows a higher risk when added to all the processes that have been added prior, but how do you explain on the upper part of the curve where you have the--for example, the "add waste-form degradation curve"--

MR. SWIFT: Do you want a pointer?

DR. ABKOWITZ: Yes, please, thank you. Just tell me how to use it.

MR. SWIFT: There's a button.

DR. ABKOWITZ: How do you explain--if you believe that assumption, then you would like to believe that each of these things is going to be lower than the previous one or along the same line, or perhaps slightly different because of the uncertainties, but you've got some inflection points here that I'd like to understand why they happen.

MR. SWIFT: It's because of the question of the release rate. The first curve, the black one that drops off steeply, obviously everything is higher than that later in time because you're inventory limited, you've taken the entire inventory and gotten it out already. So things that push the peak out in time will show an increase at later times just because you've still got an inventory left in there to be releasing dose. In many of these, except the first one, we've actually gotten all the inventory out, and they're still falling off out there at later times.

DR. ABKOWITZ: Okay, so this is--if you were to carry this out over billions of years, you'd eventually have all of the waste out in every scenario.

MR. SWIFT: Yes, every one of these. I'm not sure what the geologic meaning of that would be, but--

DR. ABKOWITZ: Okay. Thank you.

DR. CHRISTENSEN: Dan Bullen.

DR. BULLEN: Bullen, Board. Could we go to Figure 4, please? I want to understand this a little bit here. The short time period, from 1,000 to 10,000 years, is essentially the results of a quarter of the waste package failure, on average, probabilistically weighted, or however you do that?

MR. SWIFT: No. No. One package failed.

DR. BULLEN: So you failed one package?

MR. SWIFT: Out of 11,000.

DR. BULLEN: Out of 11,000, and that's the difference between LTOM and HTOM because of that. And I guess the concern that I have is--and I know you haven't incorporated this into the model for waste package degradation--but I harken back to what Gerry Gordon showed us a little bit earlier about Alloy-22 corrosion in critical potentials as a function of temperature, and Alberto Sagüés brought up the point that, you know, at 135 to 150, the difference between the corrosion potential and the critical potential is not statistically significant. I mean they look like they're the same. And so I think about these things sitting here being close to each and, you know, statistically varying, and then I think about it sitting there for, I don't know, 1,000 years at about this time. And I'm thinking, okay, 1,000 years at 135 degrees versus 1,000 years at 90 degrees C and I've got a difference in corrosion potential of 100 millivolts versus 1,000 millivolts. I just can't imagine that there wouldn't be some, in your model, difference in the temperature-dependent performance of those.

Now you don't have that in here now, but the way I look at that, it's just like I just can't see it only being an order of magnitude. Because if this is one waste package failure, I could probably scale that out of 11,000 and say, "Okay, what if I failed 100 waste packages?" It's going to scale up linearly, right?

MR. SWIFT: Yes.

DR. BULLEN: So it's going to go up 100 times, so I get a two-order magnitude difference just because--and I'm only talking--I'm talking a complete order of magnitude difference in the corrosion potential. So I still don't buy your last statement, thermal effects are small at the system level. I think thermal effects are massed.

MR. SWIFT: Okay. I'm not going to debate that last statement. But the difference--we do see an effect of the different corrosion rates at different temperatures. We see it out here, though, when we start to see general corrosion. The red curve is consistently to the left of the blue curve out here. The high temperature is consistently to the left of the blue curve. That is because in the model the packages that are hotter early basically got a head start on general corrosion. When a system cooled down--this model--they'd corrode at the same rate, but they had picked up a head start. They lost quite a lot of the wall thickness back in this time period here (indicating) when it was hot, but it hadn't gone all the way through until you got way out here.

DR. BULLEN: Bullen, Board. I completely agree--

MR. SWIFT: Okay.

DR. BULLEN: --and I think those are right. My concern is that this is localized corrosion, and if it happens early on, then the head start is much greater.

MR. SWIFT: Right.

DR. BULLEN: I mean you can be 90, 95 percent through a wall, and then if I'm doing bulk corrosion, whoops, I'm through a lot faster.

MR. SWIFT: Yes. Okay.

DR. BULLEN: So those are the issues.

MR. SWIFT: In these models, the conditions, chemical conditions to a localized corrosion did not occur. They could have occurred and did not occur in these models.

DR. BULLEN: Right. Thank you.

DR. CHRISTENSEN: Debra Knopman.

DR. DUQUETTE: Duquette, Board. I just want to emphasize that if you change the mechanism of corrosion, it will change your model altogether. And if you--God help us, if you lumped in stress corrosion cracking, for example, you'd have a completely different model, and I presume you haven't done that modeling yet for disastrous failure due to cracking or penetration due to localized corrosion.

MR. SWIFT: Yes, actually, the question is a fair one, and I'm not the person to answer it. That would go to our waste corrosion team if someone here wants to try and field it.

MR. GORDON: Yes, Gerry Gordon. We do model that stress corrosion cracking in the TSPA. Because the cracks are limited in length because of the distribution of the residual stress and they're fairly tight, the release through them is fairly low, but it is modeled. Also, the calcium chloride environment is very unlikely on the waste package as long as the drip shield is intact.

DR. DUQUETTE: If there's drip shield?

MR. GORDON: If there's a drip shield.

DR. CHRISTENSEN: Debra Knopman.

DR. KNOPMAN: Knopman, Board. Peter, in your presentation and in the write-up on this I get the impression that there's surprise that the largest reduction in the peak of the mean annual dose is coming from the unsaturated zone. Is this new? Is this a new insight?

MR. SWIFT: It may be. We haven't shown results in this way before. Am I surprised? No. I think the order of sequencing the matter is so much on analysis that you could structure analysis to show a large contribution from--I think if I put in the invert first with bare waste sitting on a bed of fresh tuff gravel on the land surface and took credit for diffusion and sorption processes and invert, I could show that to be an important barrier that way. But yes, the UZ looks good in this.

DR. KNOPMAN: Then just to be clear, this is the first time that the program has done a one-on analysis?

MR. SWIFT: Yes.

DR. KNOPMAN: Okay. All right, that's all.

DR. CHRISTENSEN: Paul.

DR. CRAIG: Paul Craig, Board. I'm happy when I hear some presentations that let me be really positive, and yours and John Kessler's fall into that category. For the first time today I have seen evidence that the mountain really does not only something but a lot. And the two complementary presentations really do a nice job of making that point. Now we have to look at it, the first time we've seen it, but nevertheless the initial message is a really clear one, and I would call it new.

The second thing which I find fascinating is on page 7. And Steve Frishman and Liz here, and Jerry Kohn, who I wish were here, should be very happy to see your conditional dose showing these very large doses when you don't do the weighting. As we've heard in many, many exchanges, this has been a source of enormous controversy, and it all has to do with presentation and communication, and you're now doing it right, so thank you. Steve is here, but I wish Jerry were here to hear it, too, because it's been a major point of his for a long time.

DR. CORRADINI: Corradini. Can we go back to--well, it doesn't matter the slide, let me just ask the question. There was a discussion before about the presence of the drip shield and then the presence of, or lack of presence of, the container. You did this in a certain fashion where you put on the--if I understood this correctly, you put it on the drip shield and you put on the waste package. Did you do the reverse?

MR. SWIFT: Not in this analysis, but we have run one-off analyses going the other way, where we've tried taking out just the waste package or taking out just the drip shield.

DR. CORRADINI: Which has the bigger effect?

MR. SWIFT: The waste package is a larger barrier.

DR. CORRADINI: Okay. By how much? Or the other thing that I guess I'm curious about is the way you show this is in terms of total dose. Still something is not satisfying to me the way this is being shown. Is there a better way? Have you thought about a way of doing this? Because showing dose, mean annual dose versus years, or however you do it, doesn't strike me as a manner. If percentage is an appropriate way, is it a factor based on the one-off or one-on? Do you know what I'm asking? On how you would show it. Because what the previous speaker, John Kessler, had shown relative to natural versus engineered I might have argued it a different way, but it also would bring up what Dr. Craig was saying, the natural barriers are on the order of the same effect as the engineered barriers.

MR. SWIFT: I'm not sure I have a good answer to that one. Yes, we have thought about many different ways to show this kind of information, and someone's always not fully satisfied. I think I would prefer, actually, to take the full system model, put everything in it, and then look at what the barrier contributions are for things they actually do really contribute to, like productions in water flux. So that I would like to compare the effectiveness of the, for example, sort of the infiltration model with the surficial soils. I think that would tell me the most if I saw that as a ratio of precipitation to, you know, flux into the model and the flux out of the model.

DR. CORRADINI: Then let me ask you a different way. If I had this and you did the full system model, and you have confidence in the TSPA, how much of a change in a barrier would you need to see before you'd actually want to keep it in versus taking it out? Let's use the drip shield as an example. If I selectfully took out the drip shield and I saw less than an order of magnitude change in the dose, what was its worth in terms of dose effect as a barrier? Have you asked the question that way?

MR. SWIFT: That would be a question for the DOE to address. Our function in performance assessment is to analyze the design as given with the drip shield, and we can show what importance the drip shield is, but I'm not going to say whether it should be there or not.

DR. CORRADINI: Okay, but I'm asking it--maybe I didn't phrase it exactly right, but let me try it again. I'm saying if the confidence--if the TSPA is being used for regulatory means and now you're using it as a way of looking at sensitivities, you can also use it as a way of performance of the design. So if you're going to do that and you have confidence in the procedure or the calculational method as a relative comparison--let's say I don't understand or I would not believe the absolute number, but I believe the relative number in a PRA sense--

MR. SWIFT: Sure.

DR. CORRADINI: --how much confidence would you have to have? How much change in an effect would you have to have before you actually would say, "I don't want to remove it," or "I really need it"? See my question?

MR. SWIFT: In fact we have used PA based analyses to support design going back as far as the LA Design Alternative Study back, what, just after the viability assessment. We have not tried to quantify the performance required from a barrier to be worth having. Some of them--the natural barriers are there, you know, they're there.

DR. CORRADINI: Yes, but I'm--

MR. SWIFT: Yes, you were wondering if we're getting out money's worth out of the drip shield, for example.

DR. CORRADINI: I wouldn't ask it exactly that way, but one might phrase it that way.

MR. SWIFT: It would be an okay question to ask.

Rob, are you going to offer to make a comment her?

MR. HOWARD: Rob Howard, DSC. We have done those analyses in the past, as Peter said, with the LA design selection. I would be very cautious to use a TSPA analysis with dose as a measure for looking at the importance of a barrier. The way the regulation defines barriers in terms of what a movement or a radionuclide movement, not in terms of dose or a barrier function, could be the one that water contacted the waste form, which the drip shield does quite well. But if you took it and looked at it in terms of dose, which is not necessarily a barrier function as defined by the regulation, you could lead yourself down a pathway that you might not want to. So the tool itself has to be used with extreme caution when you're talking about doing those kinds of value engineering studies.

DR. CHRISTENSEN: Alberto?

DR. SAGÜÉS: Okay, well, I was partly guilty back in January, I believe it was, when you made an initial demonstration of this type and I put myself in uncertainty school, and I must say that indeed you have prepared a wonderful lecture on this particular issue and the slide after this. So I really appreciate it. So the principal too, me out of uncertainty school. Not right now, but anyway--

I just wanted to make one more comment on this igneous--the eruptive event. That is that of course from a regulatory standpoint this is what is required, but I think that the thinking has been not just to be restricted just completely on regulatory issues, but I'm just wondering how the general public may be viewing a thing like this. And then now that you explained it so well, what is being done, then, it is taking the probabilistic effects of these events and then adding them up and then doing the proper operating. But I think that this is sort of akin like saying that you have a chance of getting a cut in your arm or your chest that's maybe 1/8 of an inch deep. You know, and you can have one of those every year and you will survive. But if you integrate over 50 years, that's a 6-inch cut, and that is lethal.

So this is indeed a different--a very different animal from a yearly dose in which you presumably in large part of it you get out of your system. This really is averaging things that may be asked to be averaged from the regulatory standpoint, but from the real world consequences standpoint it has a different meaning. Isn't it so?

MR. SWIFT: I think perhaps the whole group here in a few minutes could comment on that. But I would offer as a comment that, first, you're correct in saying it's a different animal than some sort of real dose a person might get. It is, it's a probability weighted dose, it's risk. But in fact I think people do make risk-based decisions at all levels. I think the rest of the group that's going to be here in a few minutes will comment on that. Airplane travel is a good one. The consequences of something going wrong there are not acceptable, and yet we do it.

DR. SAGÜÉS: Okay, thank you.

DR. CHRISTENSEN: Peter, thank you, and I think this is a good segue into the next segment.

I'm going to invite the various panelists to come forward as I sort of introduce the panel discussion. We've asked the Roundtable, a group of individuals representing various segments of the project and those outside the project, the following questions: What are the main causes for differences between TSPA results? How significant are these differences, and how do they affect confidence in TSPA results? Can any common conclusions be drawn from the different barrier analyses? And what do these analyses say about the relative significance of natural and engineered barriers at Yucca Mountain? And finally, can barrier analyses be improved? And if so, how?

Participating in the roundtable will be our two previous speakers, John Kessler and Peter Swift. They'll be joined by Abe Van Luik, Senior Policy Advisor for Science and Performance Assessment at the Yucca Mountain Site Characterization Office; Steve Frishman, Technical Policy Coordinator for the State of Nevada's Nuclear Waste Projects Office; Tim McCartin, Senior Advisor for Performance Assessment at the Nuclear Regulatory Commission; John Garrick from the NRC's Advisory Committee on Nuclear Waste; and Bob Budnitz from Future Resources. Bob, who appeared earlier, was also a member of the review panel commissioned by the DOE to review TSPA for the 1998 Viability Assessment.

We'll allow each of the roundtable participants who didn't make a presentation to provide opening remarks, limiting themselves, hopefully, to three minutes each. We'll then open up the discussion to the roundtable participants and Board members.

I'd like to remind you that at the end of the roundtable discussion Board Chairman Corradini will open the meeting for public comments.

And Abe, I invite you to start this off. And I'll try to kind of direct questions from my seat back at the desk.

MR. VAN LUIK: Okay. My name's Abe Van Luik, I work for the Department of Energy, I'm a senior policy advisor to the

Project Manager's Office. And I am very pleased with this particular session because I think we have shown some very recent work that basically takes us another step towards licensing, and hopefully also towards more public acceptance of the Yucca Mountain repository.

What we have shown in these analyses is, if you are dumb enough to put waste on top of Yucca Mountain, you get unacceptable results. If you're smart enough to put it in tunnels, even though you haven't put any metal barriers around it, except you have accepted the waste exactly how it comes, you've got a dose within the first 10,000 years of about 100 millirem, right around there. And 100 millirem, if you've read your 10 CFR 63 supporting materials, is what the EPA and the NRC say is an acceptable additional dose for the public. However, they recommend--and they did in their regulation--you need to partition a part of that 100 millirem to waste management because of a lot of other sources in the universe.

And so we're not dumb enough to put things in naked drifts, so we put a waste package around them, we put a drip shield around them, partially because of the added confidence that it gives us, even though it doesn't show that much in a dose curve. And lo and behold, what we come out with is almost no dose for 10,000 years and a peak dose that now, when we use the regulatory view of the biosphere, is below

100 millirem, right around a half a million years.

What this tells me is that basically if this repository functions, if we build it as we say we're going to build it, and if it functions as we see that it's going to function, it's basically safe forever. And that's a very good message from a DOE perspective to be able to take out to the public, to policy makers in this country. You know, it feels good, and I think every step we've taken since that December '00 SR calculation has either jumped that curve a little bit up or a little bit down.

That one in 6-01 we were all excited about because it really brought the dose curves down, but then our own people--and I have to give them credit for this--said, "This is well and good, but there's really not very good support for that temperature dependency in our corrosion model." And so to our credit, rather than being self-serving, we honored our own people and took it back up. And so to me the changes that we see in the modeling have been in response to the Board saying get more realistic and in response to our own people saying, "You may be more realistic, but there's not a good basis." We're a learning organization.

Thank you.

DR. CHRISTENSEN: Thank you. Having no other logical way to do this, Bob, I'll--or John, pardon me, I'll move it to you.

DR. GARRICK: I had some viewgraphs, but I'm not going to use them. I want to just talk to a few points. My name is John Garrick, I'm listed here as a member of the Advisory Committee on Nuclear Waste. I'm not speaking in behalf of the Advisory Committee on Nuclear Waste nor the Nuclear Regulatory Commission. My comments are Garrick comments.

My perspective and my remarks today is as an advisor to the chairman of the Nuclear Regulatory Commission on a number of topics, and the topics that have been most influential in farming some opinions about the topics of today is trying to assist the NRC in the transition from a prescriptive based approach to regulatory practice to a risk-and-farm approach to regulatory practice. And if you look at the letters and reports we've written to the chairman over the last five or six years, you'll find that there is a rich tradition of advice from the ACNW on that whole process and how it might be implemented.

The other thing that's influenced my position on some of these issues is my background and history with probabilistic risk assessment. I've been very active in that field as a methods developer, as a practitioner, and as a pusher of the risk sciences. And so when I look at the performance assessment, I look at it in the spirit of does it meet and match the tradition and the spirit of what contemporary probabilistic risk assessment has come to be.

And that happens to be one of the primary sources of my concerns with some of the earlier models of the performance assessment.

As far as problem areas are concerned, I've had problems in the past. I've been very encouraged with what we've heard today and what we've seen evolving since the TSPA-SR. The concerns I've had fall into two principle categories. One is adopting a risk perspective. I think the approach has not been a real risk approach, it's been a probabilistic analysis of compliance, probabilistic compliance analysis, not probabilistic risk assessment. The models haven't been realistic, the evidence has not been clearly linked to the quantification process of the models, and it's been difficult at times to understand the scenario of structuring that they have done and how they integrate across the various scenarios.

There's also a problem in another general category, the communication--a simplified physics-based model that is abstracted from the very complex TSPA, which is abstracted from hundreds of process models and analysis models. If that abstraction can be performed, it seems that it would be possible to abstract from the TSPA-SR and the Supplemental TSPA and the FIS-TSPA to a physics-based model that is much simpler than what we have to deal with in looking at the real model and that that would be very useful in communicating results to the public, and also in addressing this whole question of the barriers and how they add to each other.

Now, these will come up in the discussions, but I want to address them a little bit. With respect to the questions we've been asked, what are the main causes for the differences between the TSPA models, I think that we've received some excellent information today on what those are. I think one of the big mistakes that's made in presentation of results are to make presentations of point values on something that has at least two orders of magnitude of uncertainty between the 5th and 95th percentile on the performance measure, namely dose. If you show the curves as they should be, you would see that they're much closer together than they appear over what was shown to us today in the opening remarks.

The other causes for the differences in the various models is I think some of them are much more risk-oriented and some of them are much more compliance-oriented. And of course we have heard about the modeling assumptions and the great changes. Just between the TSPA-SR and the TSPA, the supplemental one, there were something like 16 additional uncertainty analyses performed. There was something like half a dozen or more fundamental basic pieces of new information, and it doesn't take much investigation of those various changes to justify the changes that were made.

As far as the significance of these differences, they might be considered significant in an absolute sense, but I don't see them as very significant in the context that we've seen them presented today. First off, the low dose values are very small numbers. And if they're varied by factors of 3 or 4 in a parameter for which we have an uncertainty of two orders of magnitude and between the 5th and 95th percentile, the curves that present it that way would look like they were essentially on top of each other. And furthermore, for long times, for times beyond 100,000 years, there tends to be convergence in the results, and of course this is primarily as a result of some improved assumptions on the solubility of the contributing radionuclides.

And as far as the barrier analysis is concerned, I think I'll wait on that until we get into the discussion. I will only say that I think one very important lesson we've learned from the whole Yucca Mountain exercise, it is much easier to design an engineered system to a quantitative specification than it is to characterize a mountain to the point where you've quantified its containment capability. And as an engineer, I wonder what I could do with $4.5 billion in terms of designing, just getting to the point of designing, a barrier that would last a million years. I think it could be done.

Also, I would like to say on this whole issue of barrier analysis that there seems to be a great deal of discussion and debate about the engineered systems versus the natural systems. To me that's not the right question that we should be asking. The question that we should be asking is, what is the safety to the public and the protection offered to the environment as a result of an integrated system? And I'm pleased to see on the basis of what we've heard today that we're beginning to pick up on that. And we certainly shouldn't be tying one hand behind our back if it turns out that it's much easier to achieve that fundamental question by putting more emphasis on one system than the other. We should not be arbitrary about saying that X percent of the safety has to come from geology and then X percent has to come from the engineered systems. To me that's a silly question.

So that's just a few opening remarks.

DR. CHRISTENSEN: Steve Frishman, State of Nevada.

MR. FRISHMAN: Well, I'm just starting on a slightly different subject. I was glad to see the portrayal of volcanic consequence. And I think maybe the presenters are getting it right in the sense of presenting in a way that actually provides an estimate of a real number as opposed to a number that is meaningless.

As you know, I've been interested in this question of contribution of barriers for quite a long time. You've heard me speak on it in different ways at different times. It's kind of remarkable what the two different methodologies that we saw, or different approaches, really have in common. And what they really have in common is, because of about two years ago a presentation of a bar graph by DOE that showed contribution of barriers, what these two approaches have in common is they've been trying to increase the perception of the natural barriers ever since. And so that's what they have in common.

I think that if you look at the analyses, you can see the way the performance assessments are being done right now. You can see that, first of all, the dependence is on the engineered barrier. And while that may mask some other things, you can take the mask off a little bit and see how things are working. If you fail one container, you can see that the natural system is essentially letting go, and it lets go early. So then it's a matter of the biggest uncertainty in the system being the reliability of the container. So you have a case where you're saying yes, we can meet the regulation, and we can meet the regulation in spite of what the natural system might be or contribute, but then you're masking what might happen if you are very wrong about the container. And we have reason to believe that there is at least some chance that the Department is very wrong about the container. So you're going to go into licensing saying that we're relying on this container and that we expect--or we want people to find that there is a reasonable expectation that for the first 10,000 years it will perform as we say, but at the same time we have this tremendous uncertainty that isn't going to be about the containers, it isn't going to be understood at the time that a decision is made by the Nuclear Regulatory Commission, and we also can look at the performance assessments and see what happens if you stretch out in time. If you take away the delay of the container, or caused by the container, you can see the natural system at work. The projected doses zoom almost immediately.

So the fact that the engineered barrier is being relied on for regulatory purposes, and then the fact that that reliance carries the biggest uncertainty in the performance assessment, and third, the fact that if the uncertainty--you know, if the worst case of the container comes true, then it's pretty clear from DOE's own work that the natural barriers have very little delay factor and have very little containment factor.

One thing you have to remember--and we keep seeing this comparison, you know, the 10^{13}, and it doesn't tell you anything about how good Yucca Mountain really might be. First of all, it's a number that, you know, has no real meaning in the system because the question that is being asked of Yucca Mountain is can it contain the waste, meaning what gets out, not what you put in. And in the case of the analyses that we're all looking at, everything that's soluble gets out, and the waste container is only delaying that. And it's I think even more important if you know that everything that is soluble gets out, we also know where it goes, and we know that it comes back to the surface in the accessible environment at Franklin Lake Playa. So we ought to be working with something where the natural system keeps it from being released back to the natural system in a way that is accessible.

So we'll be talking more, but I think you certainly get my point.

DR. CHRISTENSEN: Thank you. Tim McCartin with the NRC.

MR. MCCARTIN: Hello, I'm Tim McCartin with the NRC, I'm a senior advisor for performance assessment. Along with Dr. Garrick's caveat, I'd like to say I am speaking as Tim McCartin, I am not presenting an NRC staff position.

And I'd just like to address one of the questions raised to the roundtable, and that was can barrier analyses be improved, and if so, how? And I think they can be improved. I have problems with the one-off, neutralization, one-on analyses. They're interesting, but they aren't very informative, in my opinion, in that you see doses change, but in terms of getting an understanding of why, you have very little understanding. And I think the example of depending on what order you put the one-on/one-off analyses, etc., change the results. I'm very uncomfortable with analyses that depending on how you do them the numbers change or your perspective could change. You need to interpret the analyses correctly. I believe you need to bring to the table a lot of other information. And I would suggest that one source of information that one could do when they're doing barrier analyses is look at a delay time. And I think just about every aspect of the repository system can be somehow put into a delay time, be it the release rate, be it solubility limits and the amount of water flowing, be it matrix diffusion in the unsaturated zone, retardation in the saturated zone and the alluvium, etc.

And I think, however, there's one very important thing about those aspects: they are very radionuclide specific. And that's the part of the information that I find is very lacking when you just see a dose number. You don't know which nuclides are contributing, how much, etc. I think if you looked at possibly, I don't know, 15 to 20 main radionuclides and looked at what each barrier is doing in terms of delay time, certainly a release rate, Iodine 129, one of the soluble radionuclides, is a very small fraction of the inventory. A release rate has the potential to do a lot for a nuclide like Iodine. Neptunium, there's a lot of it. Well, release rate doesn't do as much in terms of preventing a health risk because there's a lot of the inventory. But there's a solubility limit with neptunium. Every radionuclide, the system treats it differently, and I think if you lay out--I would propose for a barrier analysis you could lay out by radionuclide what part of the system is doing what.

The other reason I like delay time, when I see dose numbers, it's very difficult for me to say whether I believe that dose number is correct. It's a dose number. A more fundamental value, like a delay time, I can look at possibly natural analogues, I can look at the experimental evidence, I can do more detailed process modeling, and get an insight, do I believe the delay time for neptunium or Americium-241, whatever it is, but you can relate that to specific information that is out there for Yucca Mountain. And that's what I think would be the benefit to multiple lines of evidence that the Board has supported for many years. Why should I believe, say, plutonium is absorbed? What kind of information do you have? I would say you have experimental information, you probably have analogue information, possibly, from other sites, and you have detailed process models. And it's a way to put the understanding into context, and I would say the barrier analyses, if it went to a delay time, a more fundamental process, it would be more useful.

DR. CHRISTENSEN: Thank you, Tim.

Bob Budnitz.

MR. BUDNITZ: I'm Bob Budnitz, for 21 years I've worked all by myself in Berkeley, but I'm just about to become a Livermore employee whose first assignment is going to be back at RW1 in the Forestall (phonetic) Building working with Margaret Chu on the Science Technology Program, just to tell you who I was and who I'm going to be. But as all my life, and I assume for the rest of my life, it's my views, nobody else's.

I'm really puzzled by something, and I'll just try to lay it right out. There's no such thing as any analysis that should ever be done unless the analyst has a clear objective in the analysis. If somebody asked me how many people are in this room, I'd say, "Why do you want to know? It's about 100, unless you need to know exactly." "How tall are the people in this room?" I don't have to know how tall they are to know that they can all fit through that door. It's by inspection. So depending on what question you ask, I mean I might really want to know, in which case we'd measure everybody and get a distribution. If you really, really want to know, you do it to the millimeter.

Analyses need objectives for them to be carried out by the analyst, for the analyst to decide what pieces of it to concentrate on and what pieces of it are less important. Because every analysis, except the simplest 2 plus 3 is 5, is an abstraction of some reality for the purposes of something. And it's the purposes of something that you need to concentrate on. So all of this stuff has to do with the objectives in the analysis.

And in that sense I understood, I kind of thought, what John Kessler's objective was, he said it. I saw the slides and I understood what Peter's objective was, and he said it. But I'm puzzled about what the objective is in some of these kind of oddball things. I understand why if you don't know the retardation potential between let's say 10 and 50 of some parameter, why you might want to do an analysis if it's 10 or if it's 50. But I don't understand why you would assume what the objective is of assuming that the saturated zone isn't there. You can assume something you don't know about it or something you're uncertain about, but to assume that it's not there, I don't understand that except in some sort of a cockeyed intellectual sort of making you feel good. Maybe there are some political objectives, but I don't understand what the technical objective is.

The technical objectives of these things ought to be to help us all understand the performance, to help the project design a repository that is as safe as it needs to be, but not foolishly, and to help the reviewers review it to assure themselves that that's so and to help the regulatory commission review it with their objective, which is to decide whether to give a license or not. So NRC has to do analysis with its objectives and the project has to do analysis with its objectives and the reviewers have to do review or analysis with their objectives, and they're all different. They're related, but they're all different.

And in that sense, while I understood some of this, some of it doesn't make sense to me. The ACNW and jointly with the ACRS about two or three years ago asking the question about what defense in depth meant for a repository. And I was privileged to be one of the experts asked to be there for a couple of days with Bob Bernero and Tom Burley, and we struggled with what defense in depth means for a repository, and that's a piece of what this is about, all this multiple barrier analyses about defense in depth, such as it is. I understand defense in depth for an engineered system like a reactor, I really do, but here the concepts are very confused. And I just want to emphasize that some of the questions you've asked belie understanding of some of those issues, some of these questions. Without accusing anybody of not really understanding, I assume everybody in the room understands what I'm saying, but if you ask these questions in the wrong way or they can be misunderstood, then you're really confusing rather than helping us all understand.

So with that background, let me just point something out. Certain bounding analyses are wonderful if your objective is to show something is bounded. As I said, it's easy for me to prove without measurements that everybody can fit through that door, it's a nine-foot door. And so I don't want to decry that at all. Some analyses require the full uncertainty treatment, others don't. Some analyses require realistic treatment, others don't. Some analyses are for the purposes of compliance and others are for the purposes of understanding, and they're related, but not the same.

So when I bring that perspective to these questions, well, the first question, what are the main causes of differences among the TSPA results? Well, see, they're in the assumptions or the data or the models. What else could it be? Unless somebody made an error, you know, or errors. How significant are the differences? Well, the answer to that is in the first question. I can't answer that except by saying it that way. And the other questions I don't know how to answer. Common conclusions about the barrier analyses? Depends on what your objective is. If the project has an objective of seeing do we need to spend $4.7 billion on the drip shield? That's an objective for which an analysis can be designed--several--which then cast light on that. Could we make it out of something else besides titanium? You can do an analysis about that as an objective.

But separate from that I don't understand this, and so I'll just leave you with that, with just one last thought. As important as these curves are, the mean values of all of these analyses are only a piece of the state of knowledge that emerges from that analysis. The full state of knowledge of the analyst is richer than that. And sometimes that richness is more important than the mean. Sometimes the objective is the mean, so that's okay, it's compliance or something. But if you don't notice that, you may find yourself fooling yourself because you ignored the principal, often the most important information in the analysis is not the mean but it's the state of knowledge of which the mean is only one piece of the distribution, which represents what the analyst thinks, is the result.

And with that I'll just pass it on.

DR. CHRISTENSEN: I think at this moment, as I would have suspected, we are going to be short on time for what I think could be a very long and rich discussion. And what I'd like to do at this moment is open maybe the floor up to members of the Board to ask questions of the panel, members of the panel to ask questions of one another. We have about ten minutes to do this. So Bullen and then Mike.

DR. CORRADINI: Can I ask a question? Are you giving up the floor?

DR. BULLEN: Sure.

DR. CORRADINI: Bless your heart. Okay, Corradini. I want to ask Dr. Garrick, can you go back to--you made a comment and I'm intrigued by it because you've thought about it a lot, that you probably could give me a little bit more time on it--tell me what you mean by a physically-based TSPA model as a higher level to what appears to be a--I don't want to say abstracted--but a set of models, and then could that be used for some of the things that Bob had suggested now in terms of objectives, where I have a very specific objective and I want to look at the presence of a drip shield, or the absence of it, or its benefit, or some other engineered barrier? You mentioned it briefly as something you thought was important to have.

DR. GARRICK: Yes. Well, look at the nominal case, for example, in the recent analysis that has been performed. It's pretty clear that as a result of doing uncertainty analysis there has popped up a contribution to the dose in the 10,000-year time frame, namely Carbon 14. The truth is, when you turn up the microscope on what they did on the Carbon 14 analysis, it's a very realistic analysis. It's not a risk analysis at all, it's a very simplified assumption that really obscures reality. And what I'm suggesting is, if you take the big model, say for the nominal case, where you now know that we've reduced the 300 nuclides and 50 actinides down to less than a handful, and now I've got an opportunity for a major reduction in scope of my analysis. I can go back and trace those radionuclides in a much simpler fashion, with not all the baggage and complication that comes from all the screening and the screening of features, events and processes that they went through to do the big model, and just focus on those three or four radionuclides in a fundamental coalesced physics-based model and answer some questions for people with respect to barrier analysis that are very straightforward.

I agree with the Board member as well as Bob Budnitz that a lot of the discussion we heard today was pure fantasy about the on/off business. That isn't what we want to hear. What we want to hear is, if you take the fuel elements into the tunnels in a paper bag and put them there, what kind of performance do you get out of the repository. Or, on the other extremes, if you just take the waste package and put it in the garage, what kind of performance do you get. Now that's very useful. And I think we do need to get away from these fantasy kinds of concepts and address the questions that are on people's minds.

But as far as the physics model is concerned, this is a common practice that's done in science, taking something very complex, reducing it to those things that are the dominant drivers of the results, and just focusing on those and then reducing the number of scenarios down to the few that dominate the results and build a model based on that. It's a very feasible thing, it's done all the time.

DR. CHRISTENSEN: Abe, do you want to respond to that?

MR. VAN LUIK: Yes, I would like to. Actually, our MTS contractor, because they have to review the work of the M&O, have built a simplified TSPA model, and we have gotten it simple enough to run on anyone's PC. I'm sure that all of you have a PC that's more than 400 megahertz. It will run on that in just a few minutes. The model actually--a version of it--was put into an interactive CD-ROM that I've taken to high schools and showed high school teachers and students how to use it. So as a communication tool, I agree, it's very useful, but we haven't cranked it up to the point yet where we can actually do the kind of sensitivities on it that you're talking about. But we listened to you several years ago, and to the Board, too, saying, "You need something simpler so people can actually intuitively learn what you're doing." We're working on it.

DR. CHRISTENSEN: Dan Bullen.

DR. BULLEN: Bullen, Board. A follow up on that. By the way, it was again developed by your MTS contractor, and I think my graduate student did that, so I've got to put a plug in for him here. But the other issue that I want to ask is that we've asked for that long ago and you--the fall-on question is, is that still based on TSPA-BA, and has it not been upgraded to TSPA-SR or TSPA--SSPA or whatever the follow-on is? What's the basis for that model, and is there a plan to upgrade it and actually use it to help us get a handle on the physics of what's going on?

MR. VAN LUIK: It is my purpose in life to make sure that that happens at some point. But you know, we have had some priority difficulties lately, supporting SR, etc. So we will get to that, yes.

DR. BULLEN: Thank you.

DR. CHRISTENSEN: Other questions? Dr. Parizek.

DR. PARIZEK: To Bob Budnitz. The nation would like to save maybe 4.5 billion on the drip shield. How would you go about doing a simplified direct analysis that addressed that simple question?

MR. BUDNITZ: Well, it's a simple question but there is not a straightforward answer to it.

DR. PARIZEK: I didn't think there would be, but you made it sound like there was.

MR. BUDNITZ: No, I don't think--no, no. I think I know how to structure a set of analyses with that objective, but I don't think it's simple. Okay, I mean the idea that it's simple isn't necessarily the case. Because in fact part of the decision--remember, the objective is to protect the public by having a lot of margin for those doses that have been in the rule making, the public rule making by the two agencies, okay? And you not only want to do that but I hope that there's lots of margin.

And then you have to ask the question about whether the margin is simply the number or whether there are certain other considerations. And you may decide that even though you could meet it without it that this additional defense--well, I'll call it defense in depth or additional barrier--is important because you're not as confident as somebody else is in certain assumptions. So that's why, although I know how to do the analysis, I'm not going to predict how you do the decision based on the analysis, okay? I think I know how to structure that analysis. Probably you do, too. I don't know how you'd structure the decision until after it was done.

I mean, you know, just to give one obvious metaphor, if I were blind, the first eye would be a big deal. The second eye isn't such a big deal compared to the first one, you know. If you want to know is it the left eye or the right eye I'd want, I don't know.

UNIDENTIFIED SPEAKER: Both.

MR. BUDNITZ: Oh, of course I want both. But when you make a decision, you have to decide going in and then you have to reevaluate as you're making a decision what your objective is. You have to decide what the analysis objective is and then decide what the decision's objective is, and sometimes that decision is not simply that bottomline number, although it's as important as it is. It has to do with confidence and so on. So I would argue I know how to structure that analysis. I sure as hell don't know how to structure the decision. That's a tough one. That's why we have decision-makers entrusted with that by the political system.

DR. CHRISTENSEN: Very short comment?

MR. VAN LUIK: A very short comment is I think this illustrates part of the answer that I would have given to the chairman's question about since this analysis is this way, why don't you eliminate that. I think Gerry Gordon gave a talk where he said that calcium chloride could be a problem. It probably isn't, but one way to make sure that it never is, is to have that drip shield. And so even though in our calculations it doesn't show up as being very important as a confidence enhancing feature, we have to weigh that in as another thing into our total equation on how to design this repository.

MR. BUDNITZ: I just want to throw out a comment. But assuming that the mountain is a reducing environment and doing that analysis is stupid. It's not, and it never will be. It's this mountain. So you have to stop somewhere.

DR. CHRISTENSEN: Mark, is it real quick?

DR. ABKOWITZ: Okay.

DR. CHRISTENSEN: We're over here, so--

DR. ABKOWITZ: I have to respond to Abe's question or comment. Abkowitz with the Board. It becomes an issue at some point because you don't have trillions and trillions of dollars to throw at solving these problems. So you get to a point where you have to ask the question, "What's the net return on investment for the next unit of safety that I might get?" So I don't think we can duck this question. At some point here we've got to ask the hard questions of, you know, where do we spend our precious scarce resources and when is enough enough. And I don't have the answer, but it better be part of the question or we're not going to get very far.

MR. VAN LUIK: I totally agree. I'm just saying that we have to have a comprehensive look at what this barrier is buying us rather than just a bottomline number on a model that doesn't--because it considers it not likely, doesn't even consider high concentrations of calcium chloride. I'm just saying there's more to the mix than just the answer or the model.

DR. CHRISTENSEN: Maybe that's the--it is the note on which we'll end the discussion. I'd like to thank the speakers for their presentations and the panel for the really stimulating discussion. Turn this meeting back over to Chairman Corradini.

DR. CORRADINI: Thank you. Thank you again, Panel.

While they're resituating themselves, let me make one announcement and then we'll have the public comment period. A couple of people have come up to the staff and asked for a clarification. The panel meeting on transportation and waste management system issues is scheduled to be held in Las Vegas on December 10th and 11th at the Crown Plaza in Las Vegas. I think that was the one piece of information that we didn't know earlier when Norm was mentioning or announcing this. So just once again, it's scheduled for December 10th, 11th at the Crown Plaza in Las Vegas.

Okay, according to the public comment register we have five individuals who have signed up for public comment. We have a little bit--we're a little bit behind, but I think I want to try to make sure we give them the same amount of time as we gave the individuals in the morning, so approximately five minutes. I'd like to call on Brian O'Connell, Mr. Brian O'Connell.

And again, if you could just state, if you're affiliated or representing an organization, who that is, or yourself.

MR. O'CONNELL: Right, thank you very much. I'm Brian O'Connell with the National Association of Regulatory Utility Commissioners, known as NARUC. I will be brief because I submitted my comments for the record, so I'd just like to summarize a few points.

There are many stakeholders in this project. This is mostly for the benefit of the new members. There are stakeholders beyond the State of Nevada. Forty-one states have contributed to the Nuclear Waste Fund, which is maintained by Congress, which might be part of the problem of why we're not getting much return on that investment. So far $17 billion have been collected from rate payers, actually from utilities, who pay the Treasury. But the utilities turn around and ask their rate payers to contribute, so it's the rate payers' money. And my organization, as the association of public utility commissions, is looking out for the interest of the rate payers. So I attend most of these meetings, try to comment on most of the documents that have come out on the program, but our members are frankly interested in the dollar investment in the program.

Margaret Chu was correct when she said there is a fiscal constraint on the project. But that is simply through the doings of Congress, and we're going to work very hard as an association, as a representative of stakeholders, to help her fully access the funds that come into the Treasury for their intended purposes.

The Nuclear Waste Fund was designed in 1982 and is well designed. One mil per kilowatt hour of nuclear power generated and sold goes into the fund, but so far, on an annual basis, about ten percent, ten cents on the dollar, that goes in is being appropriated. Somebody asked me, "Where is the rest going?" Deficit reduction and other worthy national purposes, but not for the program. So we're working very hard to help Congress now that they've gone through the hard part of deciding that they want a repository to now pay attention to the means of doing it.

Thank you very much.

DR. CORRADINI: Thank you. Our second individual for public comment is Mr. Herbert Inhaber.

MR. INHABER: I'm representing my company, Risk Concepts. The title of the talk, which will also be brief, is "What is the quality of Yucca Mountain science and engineering, and how can it be improved?"

Most of the people here are familiar with the Letter Report of January 24th in which the Board said, "The technical basis for the Department of Energy's repository performance estimate is weak to moderate at this time." The phrase "weak to moderate" has been printed in many places.

One of the most dramatic uses was by Senator Harry Reid of Nevada in a story in The Washington Post entitled "The Senator Explodes". As you know, Senator Reid is more than nearly 1 out of 100 senators when it comes to a proposed repository. That is, private groups have said if the science was as weak as the Board stated, the entire analytic structure for approval was equally weak.

The Board arrived at its conclusions by dividing their estimate of the science and engineering into three categories--weak, moderate and strong--and then they went into the strengths and weaknesses of various aspects of the program. But this is--how shall I put it?--a typical committee style analysis of research productivity and research--research productivity.

I'm not going to criticize the Board because I'm sure they did the best they could and they probably spent considerable time coming up with these numbers. But the adjectives that they used suffer from at least two difficulties: a) they are not quantitative; "weak," "moderate," and "strong" is not a quantitative approach; and secondly, it's not completely clear if the results are reproducible.

If another--now I understand that there are four new members--if those four new members replaced four previous members, it's entirely possible that the Board might come up with a different analysis today than they did last year. So the question then is, if I am making these criticisms--and I hope they are gentle criticisms--of the Board, is there another approach? And I believe there is. This is an approach that is used by a number of other government agencies--the National Institutes of Health, the National

Science Foundation, the Organization for Economic Cooperation and Development in Paris, and a host of other organizations--in which they try to measure the output of science as opposed to looking merely at the input. That is, the dollars that go into science.

For example, in the latest science indicators as issued by the National Science Foundation, they have approximately two dozen tables devoted to what I would call "scientometrics". It sounds like a real complicated word, but basically it just refers to the sociology of science, and this goes back decades. People have studied it on and off for decades because science is an enterprise unto itself, and sociologists and other scientists have tried to figure out how does science produce what it does, how can we say that one research program is better than another other than polling people in a committee and coming up with an answer.

So I am going to suggest that scientometrics--and I hate to use that word--be used to try to evaluate the science and engineering programs of Yucca Mountain in addition to what the Board has done. Now you might say the goal of Yucca Mountain science is to meet regulations not to produce Nobel Prize winners, which is true. On the other hand, NASA has a very goal-oriented program in terms of manned space flight, so in that sense they are very goal-oriented, but they are very proud of their science accomplishments. And if you look at their web site, you'll see endless discussion of all of the wonderful things they've done in science.

And as well, the Department of Energy has said because of the complications of trying to analyze Yucca Mountain, one of the most difficult scientific questions in recent years, DOE will develop innovative approaches to science and engineering. And in principle, those innovative approaches can be measured.

There are many techniques used in scientometrics. Obviously I'm not going to try to describe them. They use citation results, co-citations, and a host of other ways of estimating the effects of a scientific and/or engineering program.

In the little handout that I placed on the table, I quoted some results based on a very brief look at how Yucca Mountain has been treated. The most cited paper on Yucca Mountain in the past 20 years was written in the Journal of Science, and one of the authors was Kristen Shrader-Freshette. Some of you may have heard of her. She has been extremely vigorous in pointing out the many defects of Yucca Mountain and the entire approach. And the paper that was quoted, cited a total of 350 times, says numerical modeling of natural systems is "impossible". So you can see what approach that she and her colleague took in that paper. So this indicates that the paper that is most highly cited in terms of Yucca Mountain is diametrically opposed to the approach which was taken.

So to summarize, the Board raised legitimate concerns in its January 24th letter. The Yucca Mountain Science and Engineering Program is one of the largest single-purpose science programs in the history of the world. The interest of the rate payers--the previous speaker was talking about rate payers--is clearly not served if the science and engineering program is weak to moderate. And I believe that by using scientometric techniques we may be able to improve it substantially.

Thank you.

DR. CORRADINI: Thank you. Our third public comment is by Dr. Jacob Paz. I can't read the writing very well, so do I have that correctly, sir?

DR. PAZ: My name is Dr. Jacob Paz, I'm representing my company and myself.

First of all, I'd like to thank the Technical Review Board for reviewing my letter which has been sent and make a recommendation to Yucca Mountain project management on the issues of complex mixture of risk assessment and immigration competition for the nuclear heavy metals. I'd like to thank Abe Van Luik also, although we have different, sometimes, philosophy, and I can somewhat lean with Tom and Jerry.

My technical comments for today are the following:

Number one, on the corrosion, I think what's missing here and should be included in the testing is this issue of the sulfate which is present in the mountains. It can issue oxidation reductions and potentially corrosion.

Second, on bacteria. How many of you have seen the cone in the ocean? Bacteria can grow under various conditions, and it's quite possible--and I don't know--that in the future those bacteria can drive and maybe cause corrosion on the metals.

Third comment: I think, in my opinion, we have to look at the engineered barrier. More engineered barrier, like I take it there was a scenario, suppose there is complete failure and corrosion in the chemistry in the engineered barrier, it could increase the burden on the natural barrier, the zeolite, and subsequently increasing the migration of radionuclides. At this point of time we have various uncertainties, but who will compete with whom? The heavy metals or the radionuclides, which one will replace? And this is a recent proposal which I'm going to submit to YMP.

Second, I think that what is missing here--and I pay attention--that 75,000 years from now we're going to see an increasing amount of level of decay of long-term actinide.

And finally, on calling EPA, NRC, I made a proposal on complex mixtures. If they are open, I will provide everyone with all dissertations, all the information.

That's all. Thank you.

DR. CORRADINI: Thank you. Our next commenter is Ms. Judy Treichel.

MS. TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force. I want to speak primarily to the new members of the Board. It feels like the members that have been here for a while that we know each other very, very well. But coming in at this time is difficult, and especially when you have a board that sort of turns over and you have members coming and going. We in Nevada have been here for a very long time, and we were thinking the other day we've seen our seventh Secretary of Energy. So we have really seen this thing from beginning to end, and it's very difficult when people come in and have to start over, and it's something that's so terribly important to us.

One of the things that I mentioned earlier was the fact that people don't always understand words in the same way. And there were words that were thrown up here during technical exchanges and technical discussions that don't mean the same thing to the general public. And the general public here in Nevada are the ones who are going to have to live with the decisions that have been made, whether they're made here or in the courts or in the Congress.

But a good example is the word "risk", and I'm always a little hesitant to talk about risk when John Garrick is in the room, we've been known to have practically shooting wars. But, you know, risk can be a calculation, which is consequence times probability, but for the general public it's not multiplication, it's not the result of doing some arithmetic, risk is what comes from when you do something dangerous, and that's the way that they see this. And if the public were to do a risk benefit analysis of Yucca Mountain, they would come up with something that would say that there'd be millions of people on transport routes that would be at risk, and finally Nevadans would be at risk, but those who benefit are the ones who want to make more waste. And when the public does an analysis, they sort of thing--and it should be--that when you do a risk benefit analysis, it's one individual. I take a risk for a particular benefit, not I take a risk and somebody else benefits. So you need to be sensitive of those things when you're thinking about the jobs that you do.

There is also the idea of an acceptable risk or an acceptable dose. An acceptable risk is when I go to the doctor and I sign the permission slip and I get an X-ray. That same risk is not something that we in Nevada are being given informed consent about. So it's not acceptable for us to get a risk or a dose from a repository. We haven't signed up and we haven't signed on. And I'm going to start wearing a whistle around my neck, I think, that I will always blow when I hear somebody say that a dose is a fraction of background, or it's the same as a chest X-ray. Don't say it that way. It is in addition to. It's not the same as and it's not a fraction of, it is in addition to. We give our permission for the doses that we take. This is one in addition to those that we have signed up for or given our informed consent for.

For the new members coming in this is a very strange time. As you know, the site's been recommended by the Secretary, the recommendation has been approved by the President and gone on to the Congress, and it's been approved there. But the fortunate thing is, in this country we have a court system, so when terrible mistakes are made like that, we can go to the court system and perhaps get those things overturned. So I don't think you should assume that Yucca Mountain goes along just because it's gotten a Congressional Seal of Approval. It may very well be unrecommended and it may well be very different and not have the kind of progress that people are talking about making here. There's also a science and technology program that's just barely gearing up and getting going, and Dr. Chu talked about jump starting the transportation work.

Because DOE has been so flexible with this thing, I really don't think it matters to you. I think you're going to be doing about the same thing that you've been doing, because it's not very clear what actually was recommended. And I'm not particularly worried about the work that you do, I'm pretty familiar with that, and I feel like I know kind of what you're going to continue to do.

I'm very worried about who you do it for, and I worry that when I hear statements like when Bob Budnitz said, "We need to just get the best repository we can." That's not why you're here. Just because this thing was recommended, it's not a done deal, and your job is not to help them make just the best darned dump that there can be. Your job is to oversee a program, to be extremely critical with it, and I would worry a lot if any of you came in with a Nuclear Energy Institute mindset or feeling like the nuclear industry was important to be served. They've got a problem, yeah, they've got waste, they want to make more waste. That is not something that Nevada has signed on to help.

I also worry if in starting to look at transportation if there's a feeling amongst any members of the Board that transportation is doable and that we've done transportation before and so it can just go ahead and take place without serious problems. All of that transport that's gone on before is not like the Yucca Mountain transportation campaign. It's very, very different, just as this repository program is different from any other. And the repository program, and perhaps transportation--we haven't looked at transportation very much, but I know that the repository program is sort of prone to a situation where when one thing gets fixed something else or two other things break, and we see that continually.

And every time I come to a meeting--and I've been coming to them ever since my children were the age my grandchildren are now--and I've seen vast changes just today in repository design, and those are fixes which the next time you have a meeting you're going to see presentations and there's going to be a lot of very different things because something else got fixed because this fix broke something. So it's a very strange program.

And just to finish, I really expect you, and the public expects you, to be very, very skeptical, to be extremely harsh critics, and to be as open to the public's point of view, even though they use different and sort of simplistic wording, to be as open to their point of view as you are to the Department of Energy and as you are to the nuclear industry or to NARUC or any of the other organizations out there. It's the public that winds up taking this thing. And with the Board in the past we've had a really unique and unusual shared trust, and that just simply has to continue.

Thank you.

DR. CORRADINI: Thank you. Finally, public comment by Ms. Sally Devlin.

MS. DEVLIN: Again, Mr. Corradini, welcome, and welcome the Board, and again, last is always best, right, guys? And I really want to say again thank you so much for coming here, for coming to Nevada. I hope some day you'll come back to Pahrump, and I will not feed you cookies, I promise. And we love it when you come here, and we'll be looking for that transportation thing that got us into this mess.

I do want to say that I have been working for two years with the State of Nevada to try and get broadband here, and it's finally in the legislature. And my word to the legislature is xenophobic because Nevada does not go outside of Nevada, and they're very self-conscious about their gambling and prostitution and all kinds of good things. And Russ Dyer will get the new menu, so everybody call him, not me. But that's what goes on here in Nevada.

You'll understand that, Michael, when you get older.

But anyway, I used the word "xenophobic". The word I want to use for this toastmaster's meeting, ladies and gentlemen, is "continuity". And NRC opened this up because, you know, I read every page of that horrible report. And what I found in that report was they said they can do anything they want, and they're supposed to form boards and they're supposed to get people to do this, that, and the next thing. And I was talking to Gordon because he's going to be around another 60 years, I'm going to be sitting on top of Yucca Mountain playing gin rummy with Abe Van Luik for 300 years, because there's no money for stewardship, remember? And so we do have a bit of a problem. Who is going to take over this project? Students today want money, they don't want to learn, they don't want to innovate. They certainly don't want physics. There are 25 graduate students working at UNLV on transmutation. Not one is a physicist. A lot of mechanical engineers, a lot of civil engineers, but no physicists, and very few in metallurgy.

So anyway, we've got a problem with continuity. Where are you going to get the people--and this is not discussed--to do all this stuff for the next 60 years? You're already three years behind the times according to the GAO, so that brings the time table, if I have enough fingers, to 2005, then another three years and another three years, so you're already at 2010 if it goes through, then you've got to dig the hole, and so on. And of course I'm saying how can you dig a hole--and I read Nye County's report on the ventilation, and they didn't consult with--who is it?--the Interior Department. They didn't have a mining engineer on it. How can you do transportation without DOT? How could you do my canisters and my blood testing in situ without DOD and finding out what they're going to put in those canisters? How can you do drip shields without my bugs?

So I'm really concerned. Continuity is something I think the Board should seriously look into, because that is the future. And excuse the expression, but you know, the future, who knows what's going to be in politics, who knows what the terrorists are going to do, who knows what the politicians are going to do, who knows what Bush is going to do, he terrifies me. And he terrifies me because I don't think he'll consult with me, and I would be thoroughly insulted on that. You're supposed to, you're supposed to communicate.

And so my word "continuity" I hope goes back to Washington, and I hope you really do think about it, because it is a major problem. Who are the educated people? And I consider everyone in this room has an educated behind at least, right, to sit here all these hours, what else have you got? So I'm just saying we've had a lot of fun over the years, but it's true.

So may I suggest you look into this continuity of education and who's going to carry on, because it is not us, it's going to be the next generation and the next. And although Abe and I will be sitting for 300 years, and I hope that Madam Chu, our director, realizes there's absolutely no money for--what is it?--when you close the mine. And this is a mine. So let's get everybody involved and let's get everybody educated and let's get everybody interested, because to me it's been a fascinating subject, and I can't wait to see you all again at the next meeting.

Thank you. And I have a present for our new leader, and it's not a geriatric periodic table because nobody wanted it, it's the July National Geographic magazine, and it has--

DR. CORRADINI: Thank you, I have it at home.

MS. DEVLIN: Oh, does anybody want it then? Here I thought I was bringing you a present.

DR. CORRADINI: I'll take it if you don't want it, but I have one.

MS. DEVLIN: Well, I brought it to you because I thought it was a very nice article and well written.

DR. NELSON: I'd love it.

MS. DEVLIN: All right, Priscilla wants it, and you have it, so she gets it. So thank you again and--

DR. CORRADINI: Thank you.

MS. DEVLIN: --welcome to Nevada. Come again soon.

DR. CORRADINI: And bless you for calling me young. Thank you. The former chair had another story about Las Vegas, so bless you.

Okay, let me wrap up by thanking everybody today for our meeting. We I think ended up pretty much on time, and I want to thank all of those involved: Dr. Margaret Chu and the DOE headquarters staff as well as the Yucca Mountain Site Characterization Office staff, State of Nevada Nuclear Project's Office for their insights early in the day, the Yucca Mountain Project contractors, in particular Bechtel/SAIC Corporation, Inyo County, EPRI, and then finally, as we always say, but I don't know if we say it enough, thanks to the NWTRB staff. As usual, and this is only my second time, it ran flawlessly, and it was not due to me, it was due to the staff. Thank you, Bill, and thank you to all of the staff.

That's it. Also, I'd like to thank the people in public comment. I have to admit this is my first time at this, I am truly amazed, you have a range of comments. Hopefully we'll get a larger range of comments as the years go along. Thank you all.

(Whereupon, the meeting was adjourned.)