UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

January 10, 1996
Holiday Inn Crowne Plaza
4255 South Paradise Road
Las Vegas, Nevada

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Garry D. Brewer, Session Chair, Afternoon Session
Dr. Edward J. Cording, Session Chair, Morning Session
Dr. Clarence R. Allen
Dr. Donald Langmuir
Dr. John J. McKetta
Dr. Jared L. Cohon
Dr. John W. Arendt
Dr. Jeffrey J. Wong

CONSULTANTS

Dr. Patrick A. Domenico
Dr. Ellis D. Verink
Richard Parizek

SPECIAL GUESTS

Dr. Ju Wang, Vice Director, Beijing Research Institute
of Geology, Chinese National Nuclear Corporation
Michael Folger, Managing Director,
UK NIREX, Ltd., Great Britain
Sir Richard Morris, Chairman
UK NIREX, Ltd., Great Britain
Mark Hammond, British Embassy, Washington, DC

SENIOR PROFESSIONAL STAFF

Dr. Leon Reiter
Dr. Daniel Fehringer
Dr. Victor Palciauskas
Dr. Sherwood Chu
Dr. Daniel Metlay

NWTRB STAFF

Dr. William D. Barnard, Executive Director, NWTRB
Paula Alford, Director, External Affairs
Helen Einersen, Executive Assistant
Linda Hiatt, Management Assistant

I N D E X

P R O C E E D I N G S

DR. JOHN CANTLON: My name is John Cantlon. I'm chairman of the Nuclear Waste Technical Review Board. It's my pleasure to welcome you here to our winter meeting in Las Vegas.

The blizzard of '96, together with the federal budget fiasco have imposed certain constraints on our agenda today, as well as on our support service. Nevertheless, it's particularly nice to escape from snowy and cold East Coast. I'm pleased to welcome many of you here to join us. Perhaps this is an endorsement of why so many people are choosing to move to Southern Nevada.

We have an interesting two days ahead of us. As most of you know, the Nuclear Waste Technical Review Board was created by Congress in the 1987 Amendments to the Nuclear Waste Policy Act, and the Board is charged to assess the technical and scientific validity of DOE's efforts in designing and developing the nation's spent fuel and high level radioactive waste management system, including the site characterization at Yucca Mountain.

In addition to chairing the Board, my field of expertise is environmental biology. I'm a former vice-president of Research and Graduate Studies and Dean of the

Graduate School at Michigan State.

Let me now introduce my colleagues on the board; Clarence Allen. If you'll hold your hand up there? Professor Emeritus of Geology and Geophysics at Cal. Tech. John Arendt, a specialist on nuclear fuels and transport of nuclear materials, former Oak Ridge individual, now a private consultant. Garry Brewer, Professor of Resource Policy and Management at the University of Michigan. Jerry Cohon, Dean of the School of Forestry and Environmental Studies at Yale University.

Ed Cording, Professor of Civil Engineering, University of Illinois. Don Langmuir, Professor Emeritus, Geochemistry, Colorado School of Mines. John McKetta, Joe C. Walter, Professor Emeritus, Chemical Engineering, University of Texas. Jeffrey Wong, a toxicologist and science advisor to the Director of the Department of Toxic Substances Control, California Environmental Protection Agency.

Past Board members who are now serving as consultants pending their re-appointment or replacement are Ellis Verink, Distinguished Service Professor Emeritus of Metallurgy at the University of Florida, and Pat Domenico, David B. Harris, Professor of Geology at Texas A&M. Pat is a geohydrologist.

In addition, I'd like to introduce those of our staff that were able to get out of Washington. Dan Fehringer on the side over here, Leon Reiter, and Vic Palciauskas. They are the three survivors of our crew.

We meet here during a time of significant change for the U. S. Civilian Waste Management Program. Congress not only declined to provide DOE with the funding that OCRWM requested to push its full program ahead; it gave the program a 40 per cent budget cut from its 1995 level. In both the House and the Senate, several significant legislative initiatives to restructure the Civilian Radioactive Waste Management Program are pending.

Ironically, these changes would add additional storage responsibilities and are taking place just as OCRWM's technical investigations at Yucca Mountain are really beginning to gather some momentum. During the next two days, we will be considering the implications of these and other changes.

Today's session will begin with a presentation from Dr. Dreyfus, Director of the DOE OCRWM program, but he was unable to get out of Washington, and Wes Barnes, the Director of the Yucca Mountain project will briefly outline what Dan might have said, and we can discuss some of these.

This will be followed by Wes Barnes telling us about the activities at the Yucca Mountain Site Characterization Project.

Rounding out the morning session, several members of the DOE's project team will discuss the results of key technical analyses and studies dealing with the waste isolation strategy.

The Board was briefed at its October meeting on efforts that the DOE's managing and operating contractor had undergone to develop a waste isolation strategy, something to which the Board attaches a great deal of importance. Therefore, we look forward to hearing how these efforts have progressed since our last report.

The DOE will also update the Board on its plans and progress for carrying out the underground thermal test in Yucca Mountain and on recent analyses dealing with the geohydrologic network in the Yucca Mountain. Both of these questions have been of considerable interest to the Board in the past.

The afternoon session, we had planned to have Margaret Federline from the Nuclear Regulatory Commission and Ray Clark from the EPA tell us about how those agencies are responding to the National Academy's recommended change in the standards for a repository. Both of these individuals have been caught up in the blizzard or in the budget fiasco and we will not be able to hear this. So we'll put this on to a future program.

Instead, we will begin this afternoon's session with a presentation from two distinguished foreign visitors about their national high level radioactive waste management programs; Michael Folger from the United Kingdom, and Ju Wang from the People's Republic of China. Ju Wang also had to cope with the unsettled budget situation, since the U. S. Counsellor section in Beijing was shut down and he had to do some end runs to get his visa prepared.

Hopefully, tomorrow will be better and we can devote the entire morning session to the question of how expert judgment will be used in the licensing process. This is another long-standing area of interest to the Board. In fact, in response to a 1991 Board recommendation, the DOE convened a workshop on expert judgment. Based on that workshop, the DOE has put together some general guidance for the use of expert judgment in characterizing the Yucca Mountain site. We will hear from them on that matter.

In addition, Michael Lee from the NRC and Steve Frishman from the Nevada Nuclear Waste Projects Office will present their views on this important subject.

Participants in three efforts where expert judgment has been used will then make presentations. The DOE sponsored a formal elicitation on volcanic hazards. Kevin Coppersmith, who led that work, will discuss what was done, and describe the preliminary results that were obtained.

We are also especially pleased that Alex McBirney, who served as an expert on that panel, will be able to join us tomorrow and share his experience.

The NRC, through their Center for Nuclear Waste Regulatory Analysis, also sponsored a formal elicitation on climate change. Aaron DeWispelare from the Center will tell us about the results of that effort.

Finally, the DOE will use expert judgment informally in preparing its recent total system performance assessment, and Robert Andrews of the M&O will describe that work.

Tomorrow afternoon, we shall hear from Ernest Smerdon, who chaired the National Academy of Sciences Peer Review of the DOE technical base report on surface processes. Although the DOE has apparently decided not to continue to issue technical basis reports as part of the formal site suitability process, the Board feels that the experience gained from this limited experiment may be valuable to the DOE in the future. For that reason, we have also asked Carl Johnson, representing the State of Nevada, to give the Board the State's view on the technical analyses contained in that report and on the processes used to prepare it.

Finally, several individuals from DOE will discuss plans for managing defense waste and surplus fissile materials. Although it has not been finally determined that defense materials of this type will be disposed of in the first repository, the Board believes that it is critically important that various parts of the DOE with responsibility in this area maintain close contact and communication with each other and with the public.

One point of procedure that we've asked each speaker to leave adequate time for questions. After each talk, whoever happens to be chairing at the time, will ask for questions and comments first from the Board members, then from our staff, those that have survived the blizzard, and if time permits, we'll ask questions from the floor.

I do want to point out, however, that as is true with all of our meetings, we have set aside on the agenda at the end of each session a public questions and comments period. So if you have a question and our schedule is moving along so rapidly you can't get it in, please write it down and bring it up at this end session that we will have.

To encourage as many people as possible to participate, we need to limit the time allowed to each individual making public comments. So please try to keep any remarks that you have to a five minute max, and if we have a large number of people that have signed up to talk, we may even have to reduce that a little bit.

When you come to make your comment, please go to one of the microphones in the aisles, identify yourself and your affiliation. And those wishing to make comments are urged to sign up in the public comments register in the back of the room at the sign-up table.

Now let me turn our first session over to Ed Cording, who will chair. Ed?

DR. CORDING: Thank you, John Cantlon.

I'm going to be chairing the morning session, as Dr. Cantlon mentioned, and are pleased to have with us people from the Yucca Mountain project office, who will be making presentations this morning.

Our first speaker will be Mr. Wes Barnes, who is project manager of the Yucca Mountain Site Characterization Office. Wes Barnes has brought to the program really a very extensive background in managing energy projects, both in government and in private practice, and most recently in private practice.

Mr. Barnes is going to be making two presentations this morning. Perhaps we may have the opportunity after the first presentation for some questions, and then proceed with the second. But Mr. Barnes has been in this position now I believe it's a little over a year, and this is a year in which we've seen major changes in the program and I believe some very major improvements in the management and the progress of characterizing the Yucca Mountain project. Wes?

MR. BARNES: Snow in Washington; fog in Nevada. There really is snow in Washington. There's fog here because I'm standing here instead of Dan Dreyfus. I'm too tall, I've got too much hair, and I'm not bright enough to take his place. So I'm going to do a summary of what he has to say. I have his speech in front of me.

I encourage everyone, especially the Board, to get a copy of Dan's speech and take a moment to read it. He agonized over this, and he chose some of the words very carefully, and I share that with you. Those are his comments to me as of last night.

The highlights are a where are we, and we're at a $400 million budget in 1996 with $85 million sequestered. So he's operating at $315 million, which obviously is a far, far cry from the program plan that we all admired last year. What's happening is $250 million of the 315 is coming to the mountain. That hampers my program a great deal because as you can imagine, I expected to go up $100 million, and I went down $100 million. That changes a lot of goals. In fact, it changes all the goals. The goal now is to reach a viability assessment by 1998, something we have defined.

Very basically, a viability assessment is that we are going to design the repository that would fit into Yucca Mountain. We are going to be able to tell the Congress of the United States how much it will cost to build that repository, how much it would cost to get to a license, how long it would take you to get there, and how long it would take you to get an EIS. That's what we expect to accomplish by 1998 with the limited funding available.

Dan goes on to talk about what it would take to reach a license. I really encourage you to read that portion yourselves. It's difficult, because when you're looking at spending $3.2 billion and you're cut back to possibly a billion between now and 1998, that leaves very little room. And that's basically Page 6.

He makes comment at the end, that we can't tell what the future is. You know how many bills are in front of Congress right now. You basically know what the President's position is. It's going to be fought out in the next year, 1996. I would be foolish to start guessing what that outcome would be.

I feel like he's watching me. Be careful, Wesley. With that, I'll stop and ask are there any questions?

DR. LANGMUIR: Langmuir; Board.

The Board's been wrestling with the definitions of some very important words, one of them being viability, trying to decide what you mean when you say you're going to be viable, or you used to be suitable. We tried the word acceptable the other day in one of our reports. These words need definitions if we're going to get some sense of what the real goals are. And when you suggested, Wes, that viability meant design repository accomplished, cost and time to repository and the EIS, that was a much less complete definition in that word than I think we envisioned.

We were looking to that word to mean something about having to do with scientific, defensible program which was an investment decision, as you said, that there was a probability that you could get licensed. That was part of the definition we were looking at.

Could you elaborate on what viability means?

MR. BARNES: Who is that guy? A rose by any other name. You can see that Dan tries to say something in his presentation about that. It's impossible to tell you now, until I design something, when I can strike out for that license. Believe me, we agonized over those two words also. It's really a two dimensional question you're asking me; one is what do the words mean.

Dr. Brocoum is here in the audience and I'm sure he will tell all of his troops that are here today, that we must have changed that name ten times, and finally we settled on VA. So it's a rose by any other name.

How do you get to licensing? We finally determined that with the limited money, what could we do, and we said what we can do for sure is design that repository. We know that. So we have a goal to do that, and the country will know where we stand, where the project stands in 1998 under that particular scenario.

Leaping ahead into my presentation, we have taken it upon ourselves here in Nevada to do some contingency plans, and it's work in progress, we're doing that right now. Is there some way we can get to a licensing date? I can't answer you today. But we here are doing that, and not alone. Dreyfus is aware of it. He's assigned some folks in Washington to work with us. We're not operating as bandits out here, but it is a work in progress.

DR. CORDING: Jared Cohon?

DR. COHON: Jared Cohon; Board.

Just to continue this line, I, as all my colleagues on the Board have been trying to do, is read Dan's statement while you've been talking, so the question may not be very well formed or well informed. But in reading on Page 4, what Dan Dreyfus says are the component--or I'm sorry, the specific work products of viability assessment, one thing I don't see--well, let me put one other point out.

In addition to those four products at the top of Page 5, he says, "The components of the assessment will make important contributions toward the development of a Secretarial recommendation, but they will not be sufficient for that formal action."

One of the things I would have expected as a specific work product would be a clear definition of what else needs to be known in order to make that specific or that formal action, the Secretary recommending the site. And maybe it's contained in this language and I don't see it. What's your understanding of that?

MR. BARNES: He really is watching me. When you constrict me, not you, but when the world constricts me the way they have, I don't have the resources--I'm laying off hundreds of people--to continue going down a path that we all understood, that we thought was the way to go to come up with all the answers. So what path can I walk then? We chose this one, the VA path.

On the other hand, we're not stupid and I wouldn't run--I personally as the project manager would not run the TBM without the science program behind it. I just won't do that. Now, they can remove me, I suspect, and put somebody in that will do that, just build a hole while you're collecting the data. That's what Dan's referring to. You're going to have more data towards that possible announcement by the Secretary, but it's not in the plan. It's being collected incidentally. We're not abandoning the science program at all.

DR. CORDING: Looking at the progress of the underground construction and access to the underground, it's obvious that you're really there, and in a very few months you'll be almost completely out of there if you continue on with the south portal. So you're now at the position of the Ghost Dance Fault, the first Ghost Dance Fault location where you would access the Ghost Dance Fault, not at the Ghost Dance itself, but at the drift location. And the thermal test area is being started.

There's a lot of things that the program has gotten to the point of getting to the place where a lot of that science that you've been looking for can be obtained for the program, and in my view, the sorts of things that can be done underground are really a very important part of some sort of assessment, whether it's viability or suitability.

Are you going to be able to accomplish those things and is that critical to this assessment and are you going to be able to accomplish them with, for example, this declining $250 million budget, which declines to zero I think it is in three years, or something?

MR. BARNES: Ed came out last month. Most of you didn't get a chance to sit down and talk, so he's closer--moving again into my next presentation--we are looking at things like that, because it's obvious to us and to our scientists that we've been here for more than ten years, we've collected a lot of data. I've managed to find funds in this year's budget to do things like create a Tiger Team to go look at trying to pull that data together, because the one thing we don't have, if there was some black eye for the project, there's no library, there's no table of contents, there's no way to look at all of it and present it. There is all those documents some place. I've never challenged the scientists in this program that he or she could not answer me and then go document that answer. So the data is here; it's pulling it together.

Is the work in progress? I'll tell you in months. I haven't made that final presentation to Dreyfus. He hasn't turned us off. On the other hand, I want to tell you very clearly he hasn't bought in. I do not have approval to do something else, so the viability assessment is still where we're going. Those are our marching orders today, and that's what we're doing. Everything else is extra to that, incidental to that.

DR. CORDING: So much of the things that you'd love to get to, and perhaps you can with efficient management, they aren't at this point essential to the viability, as it's defined at this point; is that correct?

MR. BARNES: Exactly.

DR. CORDING: Thank you. Yes, Donald Langmuir?

DR. LANGMUIR: Wes, you've been intentionally vague on what the plans are for the next year or two in terms of specific research. But, clearly, the program has made some decisions already, even though you're not talking about it here. When you cut 875 contractors and you cut dozens and dozens of scientists and cut back specific programs within the overall scheme of things, you're making decisions then, presumably, as to what the core science will be. And I could infer, but I'd love to have you tell me what you've decided the core is, because presumably it's been decided to make those personnel decisions.

Your topic says that one of the three subjects you'll discuss, or that Dan would have discussed, was research priorities, and that clearly is tied right into the personnel decisions.

MR. BARNES; Basically, for the viability assessment, we cut the surface program completely. We kept the TBM operating to some point. Now, as I lower costs, I'm going to run the TBM further. But that also means that I've got to do the science program associated with that operation. So the science in the tunnel is continuing. Basically, the surface is not. There are some things going on on the surface which you will hear later on, but for generalities, there is no surface program left at all.

Everything on the EIS has stopped. We have not formally gone out to the public and said we've stopped the EIS, but we are not currently spending any money on environmental impact statement for Yucca Mountain. In a nutshell, in those three arenas is what we're doing. I'm sure my friends in licensing, everything in licensing has stopped also. All those activities have ground to an absolute halt.

DR. LANGMUIR: Further question. To what extent, and I guess we'll hear about this later today, were these decisions as to what you'd cut and what you wouldn't cut based upon total system performance analysis in the program? Presumably, that's to be a guiding approach we're using here. Maybe I should wait on that question.

MR. BARNES: Well, I think for a very technical answer, the answer is yes, you should wait. But for the political answer, that's part of the viability assessment. What we will tell the Congress in 1998 is the performance assessment of the repository, how will that repository operate. So there's a lot of other things in performance assessment that are not going forward, but not that portion. That portion is going forward; how will the repository operate within the mountain.

Where is my chief scientist? Am I okay? Thank you.

DR. CORDING: Thank you, Wes, for this initial discussion. And perhaps we could at this point go on with your other presentation. I think you were leading into it with that question, so perhaps it would be best to do that.

MR. BARNES: You've got most of it already. A quick overview of where the project stands today is first of, personnel. We've laid off, to date, roughly 300 people. By the end of the fiscal year, I will lay off another 400. The federal staff stays intact only because I froze that last year and we haven't hired anybody and we don't plan on hiring anybody. So we'll be down to roughly 1200 people. Is that right? 1200 people in the M&O by the end of the year, and roughly 100 feds operating out here.

I suspect my budget next year will go down again to roughly 200 million from the 250 it is today. Now, that's what's planned. I can foresee other things happening, but that's the plan.

The tunnel boring machine, if you look at the report, which I'm sure you have, this particular board, of the board you asked me to create, the tunnel boring machine board of advisors, their first report gives me an A plus. So we're doing very well in the tunnel. We got through our initial year. We made mistakes, but right now, we're operating very well for two reasons; the training period is over, number one, and number two, we're in very solid ground. So we're making great progress.

There are times, by the way, when that's a pain because it's eating up more money than I want to eat up at the moment, but it's running very well.

The science program is folding itself down, but operating behind that machine very well. There's no glitches at all. We're collecting the right data. We are at the heater test alcove, we're past it, as a matter of fact, and we're going to start excavating this month into that alcove, which is Alcove 5. I won't get into Rick's presentation. I'm sure he'll do a good job with that.

The project report; I don't have any problems in Washington. I don't have any problems locally. I can see my friends from the state and the county are all here shaking their heads back there. How are you, Judy? Nice to see you. I don't think I've got any problems with any of them. If I do, they'd tell me on a very regular basis.

Questions?

DR. CORDING: Yes, Don Langmuir?

DR. LANGMUIR: We read that Senator Domenici has decided he's going to propose less funding or no further funding in a year or two, unless he sees "progress". Obviously, the progress you can show easily, most easily, is the TBM machine roaring through the mountain with a big hole. But those of us interested in characterization are more concerned about what you learn from that tunnel.

You're clearly going to try and get him in the tunnel and show him the tunnel, I gather, this year. I've heard that the invitations are out, if you can get them away from the budget problems to come look.

MR. BARNES: Yes, sir.

DR. LANGMUIR: But what else will you tell Congress? What can you tell them that you think will get their ear and keep support coming to this program, other than the visible hole in the ground, in the way of science and engineering that would support a license?

MR. BARNES: That's a tough question because you're not talking about stupidity. The Congress of the United States is an educated body. But, Lord, they're ignorant right now. Talking to this body is a joy, but talking to that body is a very, very difficult proposition. I enjoy Dan Dreyfus's confidence, and I say that because when Senator Murkowski came out here, probably the most important guy for us in 1996, Dan didn't bother to join us. He let me handle that particular day, and I walked around with him all day long.

To tell somebody with that level of education that we're proving what we already knew, that everything that these scientists knew years ago, they collected from the bore holes and knew about that mountain and knew from their education, as you do, we're now proving, it went almost nowhere. What he did understand was going down into that tunnel and seeing that it was dry. That was progress. He could see it, he could measure it, it could touch it, he could feel it. He understood that. I understand very clearly, sir, that tunnel boring machine is not the project, but it is the symbol of this project, and that's why I'm keeping it running.

DR. LANGMUIR: What happens when you stop drilling, when you're through with the hole?

MR. BARNES: I don't know.

DR. LANGMUIR: What does progress mean then?

MR. BARNES: I don't know. I honestly don't know. I know what it means to you and I know what it would mean to these scientists, but I don't know what it's going to mean to the rest of the world. I haven't got a clue. I'm going to do the job I was given to do as long as I can do it, or until the world makes it so hard I have to quit.

DR. CORDING: Other questions from the Board, or Staff? Leon Reiter?

DR. REITER: Would it be all right to go back and ask a question on the first topic?

DR. CORDING: Certainly.

DR. REITER: I just want to explore a little bit the viability. I know a lot of people ask questions about it, but really is it a change of term we're trying to figure out what this all means. We used to talk about something called early site suitability, and at that time, DOE told us early site suitability and we say we haven't found anything to stop us, let's continue working.

Then we talked about something called technical site suitability, which was formerly tied, as early site suitability was, to reaching agreements or reaching guidelines that were laid out in 960.

I guess the first question I want to ask is a two part question. Does 960 figure anywhere in the future plans of DOE? Are you going to tie viability or anything else associated with suitability to 960, or are you planning on giving it up?

And the other question is I wonder if, you say the site's going to be viable, do you have or your people who work with you have in mind some sort of a statement that if Congress asked you what do you mean is it viable, what's the likelihood that we could build a safe repository and have it licensed, do you have in your mind some sort of idea what's the highly probably likelihood, it's a toss-up? I wonder if you could give us any sort of insight on that, because we're all struggling with what that word means.

MR. BARNES: Remember the first question you asked was can I go back to number one, and the only person who answered you was the chairman.

Question number one, 960, 60, all the existing regulations, under the current situation, when the Congress made the move that they made, they took us off those tracks. 960, for example, talks about comparing to other sites. Am I doing that? Not at all. I'm not doing what those regulations want us to do. The program has no definition by the new Congressional terms. So do they apply? As much as I can follow them, I follow them. But I'm lost. I think we need change. I suspect that Congress will recommend change, if they decide to go forward with this program, to those very regulations.

DR. REITER: Excuse me, Wes. At one time in the past, DOE internally decided that even though it was originally for comparison of sites, it was also a good way to judge the suitability of a site, but that was a DOE decision. So I gather you're saying DOE now no longer believes that?

MR. BARNES: I'm going to take you to the next question. You can't tie them together; keep them separate, because they are separate. Look at Page 4. Page 4 clearly defines what the viability assessment is. And forget the word viability. If you look it up in Webster's, maybe it doesn't apply. As I said before, we agonized over what choice of words. Call it a rose, but that's what we're going to accomplish between now and '98.

DR. REITER: I guess I'm getting at you've laid it out, but that collective activities, do you think that, use the word viable, that that would give you some sort of high likelihood that the site can be licensed or built safely and licensed? Or is it sort of a toss-up still?

MR. BARNES: No. Don't twist that word viable to mean anything but these words on Page 4. Now, are we as smart as you are? I'm not sure. But you know that we've got the same itch that you do. And as I said earlier, we're trying to look at that. But for me to do that alone without the team that Dreyfus has let me pull together would be impossible. I am looking at those things. I'm taking a second look at contingency planning; is there some other way to get to the goals that you're talking about, to get to a licensing date. Can we do that? Can we collect ourselves at this point?

How many years have we been here, Russ, Dr. Dyer? What did he say? Since '78, since 1978, collecting data since 1978. Can I use that somehow to come up with a new licensing date that has any realism to it? Work in progress. If that's what you're looking for. I can't give it to you. I haven't got it.

DR. CORDING: Don Langmuir?

DR. LANGMUIR: Let me pick up on what Leon has been posing to you, Wes.

Looking at the definition of viability on Page 4, I find it very disappointing, and I doubt very much if I was a Congressman that I would give you a cent to do any further work. If you promised to meet a viability decision by '98, all you're telling me, the key words seem to be design and cost. We'll design something, we'll give you a cost of it, and all you're promising me in terms of science and engineering is you'll have an estimate of the probable behavior of a repository.

We wrestled yesterday in our closed Board meeting with what we thought suitability ought to mean, and I may be misstating it. John Cantlon has written down our consensus definition of it, but at least qualitatively, it was that the site could be declared suitable if we could agree that there was a high probability that a repository at the site can isolate high level waste. And some of us said another way of putting that would be that it can be licensed; that we have high confidence that it can be licensed.

I think without a definition like that, that you can have confidence in yourselves by '98, you're not going to get any more money. I certainly wouldn't give you any if you promised a viability decision as you've defined it by '98. We'd like to support you, but unless you have more confidence than you seem to have, it's tough to defend the program.

MR. BARNES: That's a challenge. I like that. That's a challenge. If I remember from the introductions, from the chairman's introductions, you've got some business background, you personally.

DR. LANGMUIR: A small, one man corporation.

MR. BARNES: But, you see, you know that when you write the check, there had better be something behind it. Look at me as just the project manager for a minute, and imagine what I wrestled with in the last six months, as soon as we knew that this was the number. Because of all the laws and regulations, do you realize it cost me a great deal of money to lay somebody off? I mean, I've got some regulations facing me. The Department of Energy has made commitments at nuclear sites where I have to pay months and months and months of salaries under certain contracts that I never signed, but I'm part of.

I share that with you because those are all the problems that come to my plate to manage this program. Then add to that, so now I know that the money you're giving me, I can't spend it on science, I can't spend it on TBM, I have to spend it on all these other things. So you say well, Wes, you've got $250 million. You can almost knock 50 million off the front that goes to these other things that I've got to put up with in the down ramping situation.

And knowing that, Dreyfus, for example, he's got 315. Look how strong he's being in carving 250 out of that 315 and giving it to me. His folks in Washington are having a fit, but he's being loyal to this project. We've got the lion's share. I say it's 70 per cent, but I'll bet you there's people in this room that will say it's 80 per cent, every dollar he gets, I get 80 cents of it. So those are all the things that I'm faced with.

Now, what can I do with that money? What can I guarantee you that I'll do with that money in 36 months or less? I can do that. I know I can do that. Can I do other things? Yes. We're taking a look at is there possibly other things I can do. In the interim, Congress, this is what I can do. I can guarantee you that. It makes sense.

DR. CORDING: John Cantlon?

DR. CANTLON: Let me pursue that just a little bit further, Wes. You probably will accompany Dan in Congressional testimony.

MR. BARNES: I hope not.

DR. CANTLON: If he's smart, he'll have you there.

And having sat in those chairs myself, I know the kind of questions that you get, and the statement that Don Langmuir just made is very likely to be the background in the mind of the question. And Don or you are going to have to answer the question what is the probability that that site is going to be licensable if we fund you in 1997 at "X" level, "Y" level, whatever it is, and you're going to have to give them some kind of an answer.

I don't think the answer you just gave, that you've got management problems, is going to carry much weight, having been a manager myself for 25 years. As bad as those are, I've been to state legislatures and you can't get away with that answer.

MR. BARNES: I suspect you're right. If you ask me, I have an answer. But I can't pull the whole thing together and satisfy all the things that I have to satisfy with the answer I give you. So Congress comes and says, and Murkowski did the same thing, the lovely senator did the same thing, he nailed Craun and I almost to the wall in that tunnel. Tell me, who's going to tell me this is the place? He said it over and over and over again, and we tried to explain to him what you already know, all of you already know. Can I satisfy all my audiences? That's what we're wrestling with right now, the team we've put together. How do we step out and do that? We may, come up with, we don't know, but we're trying.

DR. CANTLON: Let me interrupt you with just this further thing.

We pressed Jean to the wall a couple of years ago and asked her to give us some kind of probability statement when TSPA 93 was just beginning to see the light of day, and she hemmed and hawed a little while, but said probably 80 per cent.

You know, that's a fairly comforting sort of response and, you know, you may have to sit there and say we don't have really solid scientific data to make such a statement, but in my confidence working with the people who are looking at the numbers, I'd give you; you're going to have to same something like that.

MR. BARNES: Remember last year when Dreyfus, how clever, he always uses the term "jiu jitsu" and I see enough gray hair out here, you're going to remember what that term means, using somebody else's power and speed, so he's a jiu jitsu expert. So he goes out last year and says the odds of licensing Yucca Mountain are fifty-fifty, and the chairman of the NRC came unglued. Remember? He was so upset, he said that's not true; it's about 80 per cent. He must have gotten that from Younker. Dr. Younker is one of my favorite people. She's very, very intelligent. My only concern about her is she probably has her resume on the street.

DR. LANGMUIR: Langmuir; Board. Can I pursue this just a little bit further here?

I'm distressed because my perception now is that the management of the program has less confidence than the scientists and engineers that we speak to about whether the site is suitable. I think if you talk to the TSPA people, you talk to the scientists who have been looking at the site, I'm sure they have more confidence that you're proposing the program should have. Why can't we trust them to tell us that there's a probability that exceeds 50 per cent by quite a bit probably?

I mean, I'm concerned, and I will be for--I won't be around to see it--but clearly the long-term thermal tests and the corrosion tests that might have to take place in the repository that was being filled aren't going to be done. Lacking those, I might not make 80 per cent; I might make 70 per cent. That's still a pretty good number, and I think you can almost say that now, given what you already know about the site.

That's the kind of thing I would be telling Congress; not that I'm going to propose a viability decision based upon your definition here in three years, which is going to be the end of everything financially I'm sure.

Why can't you have the confidence that I think your team has got?

MR. BARNES: You guys better be laughing out there. You'd better be laughing, every one of you.

I'm not going to answer you, but I'm going to tell you a story. When I got here, I was sworn in one year ago this month, within 90 days, I started telling the team that there were too many people involved in the project, and not enough people involved in the answer. Susan Jones, who was in charge of scientific programs, was the only one who ever got up in front of me and wrote on the chalk board before she started talking, "I'm here to end site suitability."

In that year, a number of things have happened that I think are positive. Number one, a project manager showed up. It didn't have to be me; just a project manager. Number two, we got our budget zeroed out. I think that was great. It hurt like hell. It hurt a lot of people personally, and I'm sorry for them. But it woke up a lot of folks. And you're right; that team is together. They're standing up and they're part of the solution; not just the project. I'm proud of them. I'll leave it there.

DR. CORDING: Jared Cohon?

DR. COHON: Just to pursue this further, because I think it's so terribly important. I'm encouraged by what you just said, and it's consistent with everything I've read about you and heard about you. If you didn't think you could do this job still, you wouldn't be here.

And also continuing this theme of trying to put words in your mouth, let me try out something to try to understand just where we are at the moment.

Would this be a fair characterization of how things have shifted? That your focus has shifted from site suitability to a specific design, a design that's still in development, but that from that point on, the design will really define whatever additional work is done to establish whether that particular design is sufficiently safe to warrant moving forward.

And if that's a fair characterization, I would suggest that what that would do is probably make the approach, the current approach of further exploring and studying the mountain less robust in the sense that if in the future the design should have to change in order to become licensed, or because of some issue that comes up, we may find ourselves in the situation not knowing what we'd like to know about the site because we become so design specific.

So a two part question. Was the characterization a fair one? And second of all, do you see the same kinds of risks that I see associated with taking that approach?

MR. BARNES: Yes and yes. In fact, I think your description of viability assessment is excellent. It's taped, but we probably should write that down.

What does it mean? Naturally, there's risk involved. But for the first time, and this is Dreyfus's addition to our thinking, I'm going to walk out and say to you that is the design. See it? There it is right there. Then you can compare that against all the data that I have that I know about this mountain. And if this group, along with the other scientific bodies, say it's okay, it's okay. For the first time, you've got something to work with concrete. That's what we're going to put in the mountain.

Does it fit this mountain? Does it make sense? Am I going to poison people in Nevada? All of those answers will now come into reality. And in addition to that, how much does it cost? Pretty good goal. Pretty good goal based on what we've got to work with.

DR. CORDING: One comment that's been made, and the statement that's made regarding the viability decision on Page 5 of Dr. Dreyfus's paper is that the viability assessment is intended to clarify the most uncertain aspects of geologic disposal. So that's a term in there. It's not defined as to what that is, but there's that part of it which I think it goes beyond the bullets on the previous page in saying what you're doing.

Do you have any comment on what the perspective is on that, the uncertain aspects of geologic disposal?

MR. BARNES: Mr. Chairman, I think that was summed up in our last dialogue. You just can't tell till you get there. And does some aspect of this design not fit with the mountain? Would it fit better someplace else? I doubt it, because we're designing for here. But it's those kind of things, they'll come up along the way and we'll have to go get those answers, hopefully.

DR. CORDING: One other thing that I've heard some in various meetings and conversations is that this idea about you're coming to a point of design and a case that you could put forward, at least, and continuing an investigation, total system performance evaluation, the strategy development, which is in an environment in which the regulations are in flux, as well as the moisture flow in the mountain. But in regard to, for example, release based things, dose based things, and you're dealing with a viability decision, despite the fact that you don't know whether it's 10,000 years or what sort of dates are going to come out of some of these investigations by the EPA and NRC, what's the perspective of a program on that? I think that would be interesting to have you comment on that, being able to make your decisions in the light of that uncertainty.

MR. BARNES: I think as your day goes by, and you're going to see that these PhDs will answer the technical end of it, the political end of it is, just like a few minutes ago, I wasn't complaining about managing the project, I want to do that, saying that laying people off and spending money, something changes and you just have to deal with it. Because the only thing that's constant in my life has changed. Lord knows, I don't I've bit off more than I can chew, but I sure as hell know that I'm eating the elephant in this project.

You get more changes walking in the front door; I've never seen anything try to get constructed before that had this many changes. If you were paying for it and this was your house, you start out with $100,000 house and pay a billion dollars for it. Talk about change orders. So if it changes, if EPA gets that particular regulation written and we have to deal with that, we'll deal with it or shut down.

DR. CORDING: Isn't one of the factors that you're considering here that you're really trying to come up with what DOE feels is reasonable? And regardless of what the regulations may be or how they appear to be moving, that there's a basic conclusion by DOE and its scientific groups that you have a reasonable program that's reasonable even though you don't know exactly what the criteria will be, so you're looking at what is satisfying or satisfactory to you.

MR. BARNES: Thank you. Yes, absolutely.

DR. CORDING: Thank you. We have perhaps a moment for any comments or questions. I think it should be limited certainly to questions from the audience. But is there anyone from the audience or anyone else on the Board and Staff that wanted to make further comment?

(No response.)

DR. CORDING: Okay. Well, thank you very much.

MR. BARNES: You're welcome, sir.

DR. CORDING: We appreciate your participation with us.

Should we take a break now? Why don't we go ahead and take a break now, take a 20 minute break, and come back and then we'll start with the presentation by Rick Craun. Thank you.

(Whereupon, a recess was taken.)

DR. CORDING: We're ready to start the presentation. If you'd all please take your seats, we'll be starting.

We'll be hearing from Rick Craun, who is the Assistant Manager of Engineering and Field Operations for the Yucca Mountain Project Office. He made our first presentation to us as a Board a year ago at our winter meeting in Beattie, Nevada and he'd been on the project a few months at that time. And he presented at that time, as I recall, his construction goals for 1995, which involved completion of the north ramp in March of 1996, reaching the first drift to the Ghost Dance Fault in July, 1996. They're about a week away from that at present.

So as we hear this presentation, I have my own factor that I'm going to use to adjust the schedule and dates that Rick will be presenting to us. But the management of the program and Rick's part in that has been a major contributor, along with the work of the constructor to become efficient and the rock to perform as perhaps as people had hoped, if not expected. So we're looking forward to your presentation to us today, Rick, with that caveat about your schedules. Thanks.

MR. CRAUN: Thank you very much, Ed.

With that, I'll go ahead and get started. Today, I'm going to cover just a couple of topics; TBM progress, give you a quick summary of '95, a discussion of our plans for '96, and then some of the options that we're considering now for '96.

As of January 2nd, we were at 35+53, and as of this morning, we were at 37+61. The second note up there indicates that we may have set some world records. The TBM manufacturers love to keep records on how fast their machines go, et cetera. For a 7 to 9 meter machine, we may have set some records here. That's being confirmed, but just to share with you, the machine is running well. We are doing well in the ground, and the design is matched up well. So the machine is doing excellent.

Since '96, we have progressed about 1500 or 1500 meters. We have completed the excavation of Alcove 4, and that was in FY96, and we did reach the repository horizon on November 9th.

We've also just completed our 1000 hour maintenance exam. There, we did a 500 hour exam, and for those that may remember that, we had to do quite a bit of work on the head. We had just completed the excavation of some fairly blocky ground, so there was a lot of interaction between the rock and the head, so we had a lot of unusual--not unusual--a lot of wear on the head, so we had to reinforce that quite a bit. The 1000 hour exam went much better, much better for the machine. It's running fairly well.

Along with this 1000 hour exam, we had the second of our Board of Consultants meetings, and we've been doing some calculations on the percentage of load, how hard are we using the machine. And right now, the calculations are indicating we're really running at about 60 to 70 per cent of our available thrust on the machine. So we're really not over exercising the machine at all. That's really good for main bearing wear and just overall wear of the machine. So it looks like the machine is suited. We're making good progress, and it should have a good long life.

Well, I normally put up a curve, which I've got over there at my chair, but it has little charts going up, so I'd switch to a different format this time. The original program plan has this over here. As Ed indicated on the start, we are ahead of schedule. We are right now approaching Alcove 6, the first Ghost Dance Fault.

This Saturday, we'll be taking an Alpine miner down to actually the heater test alcove to actually try that. So this Saturday--the baseline is based on drill and blast--if we are successful with this test Saturday and the Alpine miner is able to excavate in Tsw2, we could be experiencing some more schedule improvements. The estimates we got from both Kiewit and the CMO and from TRW is that we might experience a 40 to 50 per cent improvement in our excavation rates. So we're hoping that that is successful. It will allow us to complete the heater alcove much sooner than planned. It will allow us to start some of the most important tests.

As you were talking to Wes about earlier, there are several key pieces of data that we're very interested in getting, and one is the heater test. So the sooner we can get that going, the more that will support science, the data acquisition for science. Later on in my presentation, I'll let you also see some of the ways in which engineering is going to use the data from the heater test alcoves. To us, it's one of the most important things we want to get going.

So it's nice that the TBM is running well. It's even nicer that it's getting us to those areas where we can get the scientific information that we need in order to confirm some of the hypotheses and the performance assessments and that sort of issue.

In the middle there, and you've got it on your handout, it has our best day, our best week and our best month and where the original schedule was, et cetera.

I just wanted to go through our FY96 baseline. It's what we started the year with. And then the next slide will give you a little bit of an indication of what we're looking at for how we're going to change it, because in about four days at the current excavation rates, we will be complete with our FY96 goals for the operation of the machine. So we are in the process of revamping those and changing those.

But our 96 baseline, we started out about--we were estimating ahead and trying to project where we were going to be. By the time we finished the year, we were 700 meters ahead of that point. So we started out 722 meters ahead of what we were trying to estimate where we might be at the beginning of FY96.

Our objective was to continue to maximize the tunnel advance and to minimize cost. We were going to excavate to Station 39+40, do Alcove 4, and excavate the first phase of the heater test alcove.

As I was indicating, if we are successful with the Alpine miner, a lot of this excavation, and I'll get to more detail in just a minute, may be accomplished in '96. and also the excavation of the first Ghost Dance Fault.

Our original plans for '96 were to complete all the surface facilities, the change house, water system, sewer system, et cetera. That was the baseline for FY96.

We just approved this week I believe, this week, the authorization to go beyond 39+40. So we are heading beyond 39+40. How far are we going? What we're looking at is we're trying to balance the cost that the TBM will incur. The scientific programs, for example, we are ahead of schedule and we will probably complete the thermal test alcove ahead of schedule. We've got to make sure that the science is right there behind us and ready to start.

So Susan Jones and I are interacting quite a bit to make sure that she's ready, as I'm accelerating it, she's ready. So it's a balancing act, as can she get ready to run the heater test, or do I need to think about shifting some money from TBM operation to science. And those discussions are taking place.

It's a very complicated set of discussions because almost every one of our work breakdown structure components is involved in this discussion. TBM operations affects--well, you won't know what the numbers are--it will affect systems engineering, it will affect the licensing people, it will affect the engineering people, everybody in the organization is affected.

So as we decide to go on with the TBM operation, a lot of interaction assessment needs to be performed in order to make sure that we give an integrated response that says yes, we can operate beyond 39+40.

Again, we did authorize to go beyond 39+40. There's a lot of discussions taking place. I know there's all day meetings today, not here, to discuss some of the details.

We're also wanting to--our current baseline is the 250 declining. Well, 250 declining, we put minimal construction work in the '97 timeline. And what we're looking at now is if our funding profile changes, what could we do, what should we do, what can we be in the position to do, what is needed to support science. Is there a construction piece that will give us another piece of critical data to help the scientists come up and Jean Younker's group come up with a TSPA that gives a higher degree of confidence that a repository at Yucca Mountain would function properly and safely? So that's the balancing act that's going on right now as we talk. It's supposed to be done this week.

Well, this doesn't fit. Yours in your handout does fit, so I'll cut this stuff off on the right here. This is the heater test alcove. And, again, we're going to be starting excavation this Saturday, so we should start the machine up this Saturday.

The original dates, and all these dates that you see on this are drill and blast dates. We've looked at what would be the effect, again, of doing an Alpine miner type application. Now, you'll see an ESD, the decoder ring is right over here, estimated excavation start date or estimated test start date. So you'll see starting in January of '96, we should be starting into the little stage or shake-down test area in March, starting back here in April.

In fact what we've actually done on both drill and blast and on the Alpine miner, we're trying to look at where we want to put the Alpine miner, so we're really trying to tie the construction sequencing together not only on the thermal test alcove, but on the first Ghost Dance Fault, because in this case, it's important for us to get the shake-down area constructed. It's important for us to get up to the Ghost Dance Fault, not penetrate it, but up to the Ghost Dance Fault so that it can then be penetrated by the scientists.

The actual construction penetration of the Ghost Dance Fault will be later on in the year. So we're balancing some sequences of events as to whether or not we go ahead--and we will--start with the Alpine miner here. Then as it's available during this shall we say a potential hiatus as a result of improved construction, we might actually move the machine over to the first Ghost Dance Fault access and start that up to the Ghost Dance Fault so that the scientists can start those tests, and then actually bring the machine back. So we're looking at the sequence of events as to what's the best way to operate or actually to construct the facility.

DR. ALLEN: What's the scale?

MR. CRAUN: Oh, I'm not sure, but this is, what, about 130, 60 meters. 130 meters.

DR. ALLEN: From the base tunnel?

MR. CRAUN: From here.

DR. ALLEN: From there, yeah.

MR. CRAUN: I believe those are typically measured centerline of the tunnel, the main drift. It's not intended to be a scale model.

And then just to give you a few more dates as to when the tests are going to be starting, et cetera, and Bill will get into this also, you'll see that a couple of our dates, we've asked, because of the original, or we're there sooner, we may be able to build it a little bit faster than we estimated, we're really looking at ways in which we can try to accelerate and bring back in time some of the other activities.

So these dates, in my mind, have the potential of changing in order to get them integrated a little bit better. So these dates are not finalized in my mind. But it gives you a good indication as to, you know, these top items are complete. We've got the design. We issued that ahead of schedule. The M&O did a great job of getting that out over the Christmas holiday period.

Here's where we're starting the main drift, the penetration from the main drift, and the shake-down test area where we're wanting to get those done. But that gives you an indication of how we're looking at building it.

Now, some of the data, what I wanted to do is back up and basically kind of just touch briefly, this really wasn't the purpose of this presentation, but I want to just touch on one of the questions I think the Board was talking to Wes about.

It's imperative that we not only build these facilities as efficiently as possible, but we need to always recognize and remember that we're building them for a purpose, to gather data for the scientists and for the engineers. Some of the information that will be used for the repository design, I got ahold of Kal and said, hey, give me two or three items that would help us understand, or that I could communicate and say, all right, in this shake-down test area, this is the type of data we expect to get and this is how we're going to use it.

So, again, I always want to bring you back to the fact that we're building these facilities not just to build them, even though I do enjoy building them, we are building them so that we can get access and gather data for the science and for the engineering community. In this case, it will be supporting numerical analysis for temperature distribution and the drift stress for emplacement drifts, et cetera.

And then, again, it also supports some of the data that we need for the engineered barrier designs, the waste package design. It will give some corrosion information, water chemistry that will be used in some of our corrosion models. So, again, it's not just to build the ESF, but it's to build it and to gather the data that we can use in both the design and, as Wes was talking to you about this morning, it's not the design of the repository. The way I would say it is a design that is integrated with a TSPA that indicates that it will work. You have a total system performance that says it will work with this design.

So it's not the final design, because in my mind, not all of the design will be done. There's no need for us to do some of the things that are very standard. The surface designs are very readily available. There's no point in us taking limited funds and putting it into those features. It's very important for us to make sure that we understand how will we accomplish the retrieval. It's a very key piece of the design of the repository.

There are other subsurface design features that are, in my mind, key. Now, those will be prioritized and those will be funded and those will be accomplished. Those items that also support Steve and Jean's area where the TSPA, where we have a critical performance parameter, whether it be the waste package or the engineered barrier system, backfill, which has been a very active topic of discussion. I think you're going to even hear more about it today or tomorrow. Those features that are critical to the TSPA need to be pursued to the point where we understand sufficient data so that we know that we can either build them or make them work, that we understand them well enough to predict their capability.

Now, with that, I'll turn over to some photographs. Photographs are always fun. You get to see them. I added one. I cheated. So I don't know if you have these in there. This one will not be in your packet. I went to a briefing yesterday and it was a good photograph, so I said thank you, and I borrowed it. I will return it. I will return it.

This is where the entrance to the heater test alcove will be. You'll see that as shown on the earlier chart, and you may not have really noticed it, the heater test alcove is as you're going in, it's on the left-hand side. That's where the conveyor systems are. That's where all the utility systems are. Normally, all the other alcoves have been on the right.

Well, what we've done is we've elevated the conveyor system, we're reworking all the utility systems to allow us to mine our way through that. We'll actually excavate through, and then we'll come back and we'll create a little niche and we'll actually excavate back. So we'll be actually excavating from both sides. But I just wanted to show that to you. It gives you a good perspective.

They do have lanyards on. I checked to make sure that everybody had lanyards on before I showed you the photograph. They all do have lanyards, so that's all taken care of. But that gives you a little bit of a perspective of how we're going to accomplish that excavation to the left.

And I'll go through these fairly quickly. It's more fun to see it in person, but I know some of you don't get an opportunity to see it very often. This I think is a good classic shot of the corner or the turn on the north ramp where we came down the ramp and were turning into the repository. It's just a fun picture.

In reality, when you walk down, or when I walk down this, what I see is the geometry and the symmetry and the smoothness and the ability for that machine. It's a well designed, well built, well operated machine. It builds good tunnels.

This is Alcove 4. This was done with an Alpine miner. I think we did all this work in I think five days, five or seven days. We broke all of our records, and it looks good when you're all said and done with it. Hopefully, that's what the heater test alcove looks like.

Did you have something, Ed?

DR. CORDING: Well, I just received a comment from one of my colleagues that said you rock guys are all the same, whispering to me, so I think you can take that as a compliment.

MR. CRAUN: Good.

DR. CORDING: Knowing your background has been in rock for the last year.

MR. CRAUN: I've learned a lot about rocks in the last year.

This is again another look at Alcove 4. It is one of our refuse chambers. You see some of the bulk heading put on there, in fact.

Just a shot of us starting to do some horizontal drilling for one of the tests.

And as Wes pointed out, we have had our Board of Consultants. They've proven to be better than my expectations. Both Wes and I were very much in support of forming the group, and it gives us a second opinion, a very good opinion. I mean, these guys, their backgrounds are very strong. And their first report was very positive. They did start asking a lot of tough questions. Their second visit, I will be honest with you, they asked a lot more interesting questions and there's a lot more opportunity for us to be successful.

During the exit of that second visit, they wanted to and we wanted them to focus more on cost effectiveness, efficiency. We feel that the machine is running fairly well. Larry Snyder, one of the Board members, is going to see if he can help us predict some bearing life issues and that sort of stuff. But we're wanting them to help us focus on looking elsewhere, and so we've given them all the data to help them assess how we're doing, and provide us some good critical feedback. And with that, we maybe have some opportunities to make some more changes and to improve our performance a little bit more.

And then, again, the next meeting is the 14th of February. With that, does anybody have any questions?

DR. CORDING: Questions from the Board? Don Langmuir?

DR. LANGMUIR: Rich, I think we've all been impressed with how efficiently this is happening and how quickly you're going. We were appraised the other day about a safety inspection which cost the program apparently, because you had to shut things down because of the way it was done, I gather 30 or 40,000 bucks was wasted, we thought.

MR. CRAUN: Well, I don't know that I--I was involved with that, I worked with Wendy's people on that and, you know, I've been on other projects where somebody got hurt, and then the recovery costs are incredible, what you have to do.

I think it's very important for us to take and demonstrate from a management perspective the right safety culture and attitude. That might have been the best $30,000 I ever spent in my life on this project.

DR. LANGMUIR: I think Ed Cording, this is Ed Cording's kind of question, I think he might argue that in commercial use, there's no need to shut a machine down. But, Ed, that's your question.

MR. CRAUN: Well, again, I may have been able to coordinate that better. I may have been able to coordinate that better, but we want to make sure that we don't--we're not so worried about production and rate of production and rate of tunnelling that we forget about safety. So if it costs us a half a shift or a shift, it may have been the best investment we made, again. And I think it's important that we, management, support safety, and I do, very much I do.

DR. CORDING: Your Board that you have now, your Board of Consultants is very aware of those issues as well, and I think they've been encouraging you and supporting you in the effect of safety operations.

MR. CRAUN: I have not talked to Jack about that issue too much.

DR. LANGMUIR: Did the Board suggest that you shut the machine down for the safety inspection?

MR. CRAUN: No, not at all. Not at all. I think the Board was very pleased in what they saw. The words that I got back, and the words that are captured in the report also are saying that it's an efficient operation. They talked a lot about how well integrated we are, being able to do alcove construction concurrent with tunnelling.

We have had a few people hurt, minor injuries, in the TBM. Some of them, in my mind, were careless. Some of them might have been able to be avoided. Again, I just think I will be tenacious in my position and say it's imperative that we not hurt anybody significantly on this project. It will shut us down just as quickly as money can shut us down.

DR. CORDING: I was interested in your schedule on the ESF--excuse me--on the thermal facility. You're indicating that you feel you can make more, or may be able to make more progress with the road header or the Alpine miner type equipment.

MR. CRAUN: Yes.

DR. CORDING: Which is a mechanical miner. In looking at it, I recognize as you go in and do the alcove, the first alcove to the right, there are construction niches--well, no, the shake-down and stage alcove off to the right, and then as you go in and get set up for the sequential drift mining, there may be some time that's required to devote to that as opposed to being able to continue to advance around and get to the thermal test drift itself. And I think of course the focus is on obtaining the information you need out of this drift, and I think, to me, one of the real priorities is that heated drift. There's some of the other types of test are things that can be done almost in parallel or target of opportunity sorts of things, and I'm wondering if there's some opportunity to get to that heated drift setup sooner than what you show here.

MR. CRAUN: Well, if we are successful, and I brought another slide thinking you might ask that question--I'm trying to do my homework--if we are successful with the Alpine miner, we might be able to make the first 150 meters on the heater test alcove by more like Aprilish, and these are ifish dates. Don't hold them to me, but if we're successful, if it's working as well as we anticipate, we might then be able to have the first 125 meters of the Ghost

Dance Fault by June, and then December on the last 45, and then come back on the 95 meters of the heater test alcove.

It's also a function, going back to the previous slide, is a function of getting really the testing in the shake-down test area, so that the scientists can go ahead and finish engineering or designing, specifying the equipment needed in the main drift and the drift test.

DR. CORDING: Can we get a copy of that other overhead?

MR. CRAUN: For you, Ed, yes.

DR. CORDING: Recognizing that it's a possibility rather than a commitment.

MR. CRAUN: Okay. But those are estimates. I mean, if you give me a week, we'll be able to, this Saturday, we'll have the mechanical mining equipment underground. We had a Low-Boy failure. I really wanted to have it down there last Saturday so I could give you another piece of data. But anyhow, our Low-Boy failed. So we'll have it down this Saturday, so within one day, we should have at least a first cut impression of how it's going to function.

DR. CORDING: Well, the rock, you're working in rock that's relatively hard with respect to its mineability, if you will, mineability with that type of equipment. But apparently you've gotten about the heaviest piece of equipment you can find, which is key to being able to handle that type of rock.

MR. CRAUN: I was also told that it's fairly fractured, so that will help it make its way through.

DR. CORDING: I think your plans are certainly in flux or progressing as you think about how you're going to take advantage of the progress you've made. As you've indicated, you're still working with that. I think we'll be interested in hearing about how far do you think you're going to go with the TBM. Are there other options on excavation, to cross the block, for example? Can you get to the thermal test to Ghost Dance sooner? Those sorts of things. As I understand, you're really trying to work through that at this point.

MR. CRAUN: If we were to continue on three shifts a day on the TBM operation, daylighting the machine at the south ramp, the south portal, but that may not be the right combination. Maybe the correct combination would be maybe two shift of operation of the TBM; take the money from that third shift and put into working with Susan on accelerating some of the test alcove experiments, some of those things. So that's the balancing act. If I'm just there to build a tunnel, then we could get it done a little bit sooner, but we may actually slow it down a little bit.

DR. CORDING: And I think the key thing that we've been looking at as a Board or thinking about is the sorts of tests particularly related to water flow, moisture conditions, age dating, those sorts of things, really, I think the reason you're down there, and I think that's what I think we're certainly interested in hearing.

Now that you're getting there, are you being able to take advantage of it? It's obviously a difficult task with the budget situation you're involved with.

Any other comments from Board? Yes, John Arendt.

DR. ARENDT: Since safety has been mentioned, I'd like to compliment you on the fact that production is not being compromised at all and that you are assuring that safe operations are being performed.

Now, in that regard, what is your accident experience, personal injuries, lost time accidents? Are there records being kept by the contractor? And what kind of experience are you having?

MR. CRAUN: Yes, sir, records kept, and I don't have those off the top of my head so I'm going to answer you as best I can. I'm more than willing to get you that data. We do have a sign posted as you come into the pad area as to the number of days since our last lost time accident, et cetera.

The last one I was involved with was a hammer fell down and broke or crushed a finger. So those are the types; they've been more minor in nature. We had a skill saw accident. So they've been fairly minor in nature, although not to the people that got hurt, obviously, but those did both result in, as I recall, in lost time accidents. But if you're interested in the lost time accident data and the accident data, we keep track of that and we do monitor that.

DR. CORDING: Yes, Don Langmuir?

DR. LANGMUIR: Ed Cording asked some questions about how you would shift the money around, so I think that question got answered for me, that you might take a third shift to support some science and get it started earlier.

MR. CRAUN: That's right.

DR. LANGMUIR: Just a minor point question; what materials are going into the tunnel now that are going to stay there if it becomes a repository? In particular, I was looking at the floor, and I presume that that's simply cuttings from the tunnel boring machine that are used to floor the tunnel. What is down there? What's going in there?

MR. CRAUN: That's an invert. It's an invert.

DR. LANGMUIR: Right.

MR. CRAUN: We manufacture that invert. The current design has the invert as a temporary device. There is a design which will allow us to, especially on the rib area, on the non-rib area, it's easier and one can imagine how one takes an invert out and actually removes it from the ESF. In the ribbed areas, there is actually a design which will allow us to span across above the invert and hold the ribs in place, and then go ahead and remove the invert.

The ribs, the rock bolts, the wire mesh, the channel, the lagging is all classified as permanent. So it came with the QA program. It is intended to be a part of the repository. Now, that may not be the final design. For example, we may line it. You know, one might say that if you're expected a 130-some year operations period from the time you start loading fuel to the time you actually close, one, if it's a viable repository, that a rock bolted system and a rib system may not be the best system to have. The current design--I'll just go back to my statement--the current design does have them as a permanent part of the repository, and that is why they are Q.

Now, we've put a great deal of effort on the ribs themselves. We've gone from a W8, we've also put together a design for a W6. I believe when we first started, and I'm going back a little bit in memory so I may get some of the numbers a little softer than I'd like to, but I believe we had about a 300 per cent improvement, or reduction in the cost per rib. When we first started manufacturing, I believe we were doing mag particle on all of the Phillips welds on all of the W8s, and we've been able to refine that down to more critical welds.

And so the program, the M&O, TRW and the team mates have really rallied together to help us pull those requirements down. In fact, we are in the process of again, I think Alden Segrest, I don't know that I see too many of the design folks here--good, they're off designing--they've just now reissued all of our tunnel support specifications yet again, and the purpose of that was to really help us as we gain experience to focus in on those critical performance parameters that needed to be validated in the QA program, and focus just on those. So we've been able to get our construction efforts, the record efforts on what kind of records we keep on the installation of these devices, we're continuing to work that to what we consider to be a more optimum set of data. And that's really saved us time and money.

DR. CORDING: I know you had looked at a shaft drift option for potential Calico Hills exploration. What is the status of that at the present?

MR. CRAUN: Well, right now, there's not a lot of activity going on, Ed. I think we're looking at several different options. I mean, unfortunately, our focus is a little more--I don't want to say short-term--'97 is really what I'm really trying to focus on.

As you were mentioning earlier, there's some discussion of, you know, how far up the south ramp do we go. Do we try to build a north ramp extension? How would we do that? Would we do that with our 25 foot TBM? Would we do that with an alternate machine? Would that be a fixed price type activity? A lot of those discussions are taking place.

Calico Hills is still in the discussion. It's not off the plate yet, but there's been no formal--we have not started the design for that shaft, nor for the cross-drift.

DR. CORDING: Thank you. Other questions from the Board or Staff?

(No response.)

DR. CORDING: I want to thank you very much, Rick, for your presentation.

MR. CRAUN: Thank you.

DR. CORDING: At this time now, we'll continue and we're getting a status update from Jean Younker on the waste containment and isolation strategy. Jean is the manager of Suitability and Licensing Operations for the M&O, and she'll give us an update on progress being made with the editing of the waste containment and isolation strategy, which has been developed over this past year, and even past months, and perhaps some reflections on the modifications that have resulted from comments received as a result of reviews of the developing strategy, the internal reviews.

So, Jean, as usual, we look forward to your presentation.

DR. YOUNKER: Thank you. I appreciate that introduction.

You've probably said some of this. I was busy trying to get hooked up here to the microphone. This strategy has been under development, as you know, for a year. We talked about it in kind of a very early form in the Beattie meeting a year ago, and then we previewed the actual text version as it was evolving in the October meeting. That was before it had been, or I think it was about the same time it was transmitted to DOE formally to start the DOE review. And that review was conducted in October and November of '95.

We've had a lot of just excellent interaction, in my view, during informal comment resolution meetings, talking with the reviewers and trying to understand their concerns, trying to understand the technical information that they thought we had missed as we put the document together, and in turn, trying to help them understand what we thought we had put together for them.

So it was really, I think to me, this was one of the real advantages of finally getting the text written, getting something down that people could comment on, and then we expect to get the final revised text coming out of that review and the comment responses to DOE reviewers, and this I could have updated late last night to tell you, I don't think we're going to make it this week, but we're certainly within a week of getting it to DOE. So we're into the final revisions. As we go through discussions with the commenters, I think there will be a few additional changes to the text, and my guess is probably next week, I'll be able to transmit the final revised text.

I already commented on this a little bit, but the benefits of putting the strategy down on paper in more detail than what we've had it in the briefing packages that you have heard us present here, was that it really forced us once again another level, I think, of integration of performance assessment with the site and design activities. And that of course is something that you all have been telling us we needed to figure out ways to make that happen over the years that we've been talking with you about the project activities.

I think it also, and this is probably to me the most important thing on this slide, and that is that it really got us into a position of looking at what we've learned to date and really trying to figure out what it means in terms of safety of the site, both with regard to the kind of engineered system that you're going to put at the site, as well as the way the natural barriers will perform.

So I think it forced a lot of go back and look at what do we really know, and then, likewise, what don't we know, where are our key questions remaining. And that leads you then to what still needs to be done to really sharpen up some of your--the validity of some of the pieces of the arguments in the hypotheses that kind of portray the strategy.

And then also help us focus on getting the needed information efficiently. I think you'll see as I run through the elements of the strategy this time that I think we have a bit more detail into what are the key pieces of the information or the hypotheses as they break out for each of the elements in the strategy. That's been sharpened through the review process, as you would expect. So I think we have a little bit better definition of the remaining work or the tests that need to be done.

Going back and reviewing very briefly, the objective of the strategy was to provide projections of the containment and isolation capability that's adequate to support a near-term decision whether to proceed with repository development.

We, of course, are operating within this framework that you heard Mr. Barnes talking about with DOE needing to get some kind of an investment decision, viability assessment. You've heard the various names that we've been using over the past year as we've wrestled with what we were going to do with this last set of adjustments the program has gone through.

And so one of the key objectives was to get the information focused in a way that you could really do a safety assessment of the site in a defensible manner on the basis of the information that you were able to pull together at whatever point in time you need to do that.

For the purpose of this strategy, we define waste containment and isolation in a fairly broad sense, meaning the containment part is to contain the waste for thousands of years during the high-inventory and high-temperature period of repository performance. And the second part, the isolation part, to limit the dose to any member of the general public at any time. So we have the two parts or two goals of the strategy.

And although I think I've said this before to you, it's not based on a set of standards per se, we are consistent with the recommendations that the NAS panel gave about how the standards should be written, in that we've looked at the longer time frame. We have moved away from just looking at the 10,000 year cumulative release type of standard, to looking at the dose rates.

And the approach that's been taken hasn't changed much since the last time I briefed this, other than I think it's getting crisper each time that we go through another review and discussion of it.

The performance assessment calculations that we're basing our understanding on lead us to believe that there's a set of mechanisms operating that could result in very long containment at this site. And of course you know the strongest driver on that is how dry will it be for how long, how dry will the packages be. And that we will get, if you look at the nominal release that is predicted or the doses that are predicted from TSP in '95, you see that you're in the hundred millirem per year in the background level of doses for the nominal case.

And so the approach was then to use that to look at the subset of factors that you could validate through near term testing and analysis, and to carve that out then in hypotheses related to those original five attributes of the system that you've heard me talk about before, which to run through them quickly, little seepage into the emplacement drifts gives you that dry environment for the engineered barrier performance to occur within. That leads you to then the potential for containment for thousands of years.

Given that containment for thousands of years, at the time that you do have any breached packages, the basic environment, together with the low flux, gives you low mobilization rates from those breached packages, therefore giving you limited release from engineered barriers, both because the waste packages are still there for a long time, at least parts of them are still there, together with any kind of other engineered barriers you may add, like, as Rick Craun suggested, looking at the potential for using backfill or some other type of engineered material in the drift around the waste packages that will add to help limit the release, and finally, the dilution that you get during transport through the natural barriers.

I have one slide for each of those system attributes that then states the hypotheses, and this is where I think there's been a little bit more detail evolved in terms of what specific hypotheses can you test in the fairly near term to get a better handle on the validity of this overall approach that's being laid out here.

The hypotheses for seepage, stated simply, the percolation flux at the repository horizon is low; much less than the bounding value that we've been using of .2 millimeters per year, as supported by current information. The second piece of that then, the seepage rate into the drifts will be a small fraction of this amount, whatever that amount ends up being, because of the effects of capillary forces causing the water to not want to move into that large opening. And, you know, we've known this effect existed for a long time. What we're now doing is focusing in on what kind of reduction would you get from whatever the percolation flux is, due to the presence of the underground openings.

For the containment hypotheses, it's broken out now into four kind of succinct elements; the amount of liquid water actively contacting the waste containers will be small, as a result really flowing from the previous hypotheses. The humidity in the vicinity of the waste package will be low for thousands of years. This is one that gets into the whole area of what kinds of response will you see in the host rock due to the heat from the repository, and the various predictions for how long the relative humidity will stay low are very key.

Corrosion rates for all mechanisms will be negligible under low humidity. And then, finally, the outer barrier of the waste package will provide cathodic protection for the inner barrier for thousands of years. You'll recall from the presentation that Bob Andrews gave at the last meeting, that this whole question of what kind of prediction you get by using that two layer waste package system is one that became really important in waste package performance as a part of TSP in '95. And this is one that it looks as if it can really buy you a lot of performance for your engineered system.

The hypotheses related to mobilization; flow rate of the water that can contact the waste in the breached waste packages will be low. The solubility of neptunium is orders of magnitude below the current bounding value. You know that the radionuclide that gives us the largest, the highest doses in the long term calculations of course is neptunium, and that's because of the values that we're using for the solubility of that species.

There are some reasons to believe that those values may be high for our specific environmental conditions. And so if it turns out that we move in that direction toward lower solubilities, then clearly the radioisotope that plays the largest part in our peak doses in the long time frame could become less of a problem for the site.

Current values for spent-fuel dissolution rates provide reasonable bounds. That's something that I think we have fairly decent confidence in right now. And colloids formed during dissolution of spent fuel do not remain stable under repository conditions. This is one that in my last presentation, I think Dr. Langmuir helped me with to remind me that this was probably one, in his view, that we should not be spending a lot more time and be real concerned with. I think that was a fair representation of what you told me last time.

For transport through the engineered barriers, the hypotheses are seepage into the emplacement drifts will be insufficient to saturate the engineered barriers, so you'll end up with thin films. You'll end up with discontinuous water films. Diffusion coefficients for transport within that waste package will be very small, and that the backfill materials have very small diffusion coefficients for transport on surface films. And this is, of course, assuming that you are going to look at the value of adding backfill, not that we've made the decision that backfill will be used. And that contaminants will precipitate in the pores. If you do get some dissolution in the materials moving with the water, there's the potential for evaporation or for chemical reactions to occur and actually precipitate those contaminants out in the pores.

For the last piece of the puzzle, for dilution, the flow rates of water contacting the waste will be low. We've carried that all the way through, as you know. Flow in the saturated zone is much greater volumetrically than the flow contacting the waste, and that you will get strong mixing when any of the water moving through the unsaturated zone reaches the saturated zone. You'll get mixing below the water table. So that puts together the pieces for dilution.

In terms of what the recent observations in the ESF mean to us, I think that emplacement-drift seepage is recognized by all of you and all of us to be so important to containment, mobilization, EBS transport, dilution and, therefore, it's just crucial to the strategy.

I think the strategy is motivated by the ESF observations that support the very low flux rates in the Topopah Spring. We had already presupposed, if you look back at the environmental assessment for Yucca Mountain and at the site characterization plan, there are some assumptions about, based on current information at that time, what kind of flux rates we would see in the Topopah Spring, and it sure looks like as if the general conceptual model we have for the site is coming through the set of observations quite well.

It looks like one new piece of information that you're aware that we're getting is the age of the fracture coatings that we're finding in the ESF as well as the pore water dates. And the oldness, the age of those dates being 100,000 and older is certainly something that we're going to have to put together as a part of our current understanding, but it does fit quite well with that conceptual model that we've been carrying forward.

Lack of dripping fractures is an important observation that helps to support the strategy, I think, and then the new indications, recent indications that flux may be diverted by the Paintbrush non-welded unit. How extensive that diversion is, over what time frame it's operated and over what time frame it would continue to operate, given climatic changes, obviously is a key piece of information.

There are the cross-cutting issues that we haven't embedded in each element of the strategy, but we obviously have to look at, such as the thermal effects. And in terms of moving forward in the near term, I think we've become convinced that you have to rely on the short-term thermal testing and then on the waste packages providing adequate containment during that thermal period. And information I think is leading us to be fairly confident that we can get that kind of performance in this environment.

For the climate change question, we clearly will have to develop bounds on pluvial infiltration rates, look at sensitivity of our hydrologic models to higher infiltration rates, and then get at what is the signal, what's the message we should be getting from the ages of the fracture coatings and the pore water that are apparently so old.

For tectonics, seismicity and volcanism, our potentially disruptive scenarios, we're at the point now for most of these to just get at risks by taking our information and looking at the occurrence probabilities to predict consequences, and get at the consequences and the potential impact that has on the releases.

I think we've pretty much, except for moving into the longer time frame, we pretty much have the information and the view of the authors of the strategy, at least to move forward with those consequence predictions.

For human interference, demonstrate that the site is not a likely target for future resource exploration or development. And that's been a position I think that we've taken from the very early days of the environmental assessment.

The strategy builds on previous work and is supported we think by the ESF observations so far that are suggesting the very low flux at repository horizon levels. I think the strategy tries to get at the key issues and points to what can be done to address them; provides a tool for integrating the pieces of the puzzle, the performance assessment, the site characterization testing and the design activities.

And we think that by focusing on what is important and testable, the strategy gives you a way to evaluate performance to support these near-term decisions that DOE will be facing about viability and the potential for continuing Yucca Mountain as a repository.

Thank you.

DR. CORDING: Thank you, Jean. Questions? Okay, yes, Don Langmuir.

DR. LANGMUIR: Langmuir; Board. I enjoyed it, Jean. I think it's really moving along. It was moving so fast in fact that I have to ask questions of clarification.

DR. YOUNKER: Sorry.

DR. LANGMUIR: But on your seventh overhead, and I'm going to just go through by numbers here, one of the Board's concerns, and I thought the program's concerns, has been the need for long-term testing of corrosion at high relative humidities. And I wonder if you could just comment on this. My sense was that you had one of the labs, this was Livermore, doing or setting up to do a bunch of long-term corrosion tests. Is that, do you think, necessary or not? My sense was there was large uncertainty related to the rates of corrosion at high humidities, particularly at temperatures above, say, 70, 70 to 100 degree celsius.

DR. YOUNGER: You're talking about at high humidity?

DR. LANGMUIR: Yes, at high humidity, and my guess is with all the moisture in the system, you're going to be at 100 per cent relative humidities, even if the temperatures are high, essentially all the time, given the water present in the system. Could you comment on that, and comment on your first two bullets in that connection--your second and third bullets, rather?

DR. YOUNKER: I'm certainly not enough of an expert on that topic to comment very much, except to say that I think the calculations that we did both as a part of TSP in '95 and some that Tom Buscheck has done suggest that you will see relatively low, I mean in the 50 to 60, maybe 60 per cent humidities, and that at those humidities, the corrosion rates go through that inflection point.

You know, the kinds of materials we're looking at, my understanding is that you will see a greatly decreased corrosion rate when you get below about 60 per cent humidity. But I do have people in the audience who could answer your question and I'd be very pleased to call on one of them to give you more detail if you'd like me to do that.

DR. LANGMUIR: Sure. Let me just make an argument first, and then maybe you could ask someone in the audience.

My sense would be if you're going to close off the repository at some point, which you will, and stop ventilating it, the sufficient moisture in the system, you're going to come to relative humidities of 100 per cent with the water that stands in the pore spaces in the material once you close it off, at whatever temperature you come to.

DR. YOUNKER: Once the temperatures have gone through the--

DR. LANGMUIR: Yeah, once you get below 100 degrees, then you're in that zone of high corrosion.

DR. YOUNKER: There's people who are very familiar with the modelling that we have to support that that are in the audience, probably Dr. Clark.

DR. CORDING: Bill Clark?

DR. CLARK: Bill Clark, Livermore.

Don, I guess maybe the latest work that we've been doing in terms of modelling the repository and a layout itself is localized disturbance thing that Tom Buscheck has been working on. There's an indication that in fact we can get that relative humidity down extremely low, down into the 10, 20 per cent range. Backfill may be something that will help. Backfill may not be necessary. This is something that the modelling work is ongoing now. This is the purpose of the underground heater test.

Jean was very accurate. For most materials, corrosion allowance materials, alloy steel as an example, or basic steel, there is a very, very sharp transition at 60 per cent relative humidity, below which if you get there, you can essentially cut most of the corrosion off, as we know it now. We have a very, very comprehensive extensive test facility being put in. We are looking at immersion. We're looking at high humidity above the immersion bath on a whole host of different kinds of configurations and materials that are in the program now that are all candidates. We are also looking at extremely low relative humidity, high relative humidity, et cetera.

We think right now, based again on models, yet to be proven, yet to be tested, that we can in fact keep those drifts dry, dry being well below 60 per cent relative humidity, for tens of thousands of years. Once we can get underground, once we can start some of those tests, once we can do some of the backfill studies that the systems people are now setting up and which we will do some of that work also, then we can either say yea or nay.

In addition to that, there is some activity been restarted again, and it's looking very good in terms of doing things like ceramic coatings on the outside of these things, very thick coatings. If we can move forward with that and that turns out to look like something that is feasible, we wouldn't worry so much about those instances where we would get some high relative humidity in some of the drifts.

So right now, all I can tell you is the models are indicating that we have a very good opportunity for design to keep low relative humidity. We have materials testing that's going to show that if that in fact happens, that this material will last for a very, very long time, we have to prove the models when we go underground. And that why, and I know you mentioned it earlier, it's so imperative and critical that we get these large scale tests going.

DR. LANGMUIR: Do you really need large scale tests, Bill, to answer the question that I posed, which maybe you're not concerned about, that you are liable to reach 100 per cent humidities relative to any water there, which will be there, once you put your waste packages in? I can see if you've got backfill and you're really cooking the backfill up with a canister, you may be below 100 per cent because there's no liquid water anywhere near you, and you're in a thermal gradient, less likely so if you have an air space around the canister and no backfill. Do you agree with that?

DR. CLARK: Yeah, but we're going to look at this as we start the large scale underground heater test. You know, the modelling indicates we're going to drive that water away and it's not coming back. You know, whether we can really do that or not, whether that really happens in real rock is yet to be seen. But everything indicates that if we lay the repository design out in such a way that the water in fact has to be driven away at the high temperatures and has a way to get out of there, a way to exit by shedding, by evaporation out the top or whatever, we don't think we're going to have water on those waste packages for a very, very long time.

When it comes back, of course, that's now getting back into what Jean was talking about with cathodic protection. Remember the packages are cooler and how corrosion kinetics are down quite low, all different types of thing happen. But, again, if the modelling is correct and the testing we've done so far is accurate, it would indicate that that's going to be a very, very long time, well beyond the regulatory period.

DR. LANGMUIR: Are you talking about 10,000 years, 100,000 years, do you think?

DR. CLARK: Well beyond 10,000.

DR. LANGMUIR: Are you getting up towards the most recent dates we're concerned about, millions of years?

DR. CLARK: You mean million?

DR. LANGMUIR: I had a few more if I might. These are shorter.

Jean, on overhead Number 8, one of our biggest concerns obviously for the long-term repository is what happens to neptunium. And I guess it went by so fast that I didn't understand what your bullet meant. Could you explain what Bullet Number 2 is there on neptunium?

DR. YOUNKER: It's just being presented as a piece of the hypotheses that says mobilization rates will be low. And we think, as you pointed out to us I think, that there is some evidence now that the solubility that we've been using is too high for this particular environment. And so that would be one of the key things that we would try to drive out of the strategy to do whatever you can do to establish whether or not the solubility value is too high.

DR. LANGMUIR: Something else that was brought to the Board's attention was the possibility of looking at neptunium in terms of the total inventory in the fuel, and then considering how much you'd get in groundwater in you release it as a percentage or a fraction of total inventory as a function of time. And that might bring you back up again to some higher values. That's something that you might want to think about looking at.

Finally, one of the very important observations for Congressmen going down in the tunnel and for us on Friday is going to be that it looks dry. But one of our concerns is that the ventilation is doing that, and that without the ventilation, maybe it isn't so dry. Now, you've got age dates, which I'm very encouraged about. I'd like you to tell us something more about those age dates, suggesting that fracture coatings perhaps are 100,000 years old or older. I presume that's dead C-14; is that how that's been observed?

DR. YOUNKER: Your next talk--

DR. LANGMUIR: Dennis is going to talk about that?

DR. YOUNKER: Dennis is going to talk about it.

DR. LANGMUIR: Okay, that's extremely important I think to us, is whether in fact it will be dry when you turn off the ventilation system, and if you miss some things that you would observe in terms of seepages if you had that ventilation turned off.

DR. YOUNKER: The question of what the ventilation is doing and whether or not you could, you know, it could cause you to not see seepages that are there is one that I think we from the strategy would drive as very important, and I know that in some of the prioritization that's going on right now as to what additional scientific program testing work could be done as we shift dollars around, that's one that I think is going to get high priority.

DR. CORDING: Leon Reiter; Board Staff.

DR. REITER: Jean, Leon Reiter, Staff. I have a couple questions.

The first one really has to do with sort of independence or interdependence of the hypotheses fatal flaws, and that is, let's put it this way, if you look at the hypotheses, it seemed that in every one, a critical thing is little amount of water getting through. And I think we're all encouraged by the fact that the water you find and the dates, the age of the water you find now seems to be on the order of 100,000 years. I guess this is sort of a what if question. What if that's not true or what if it doesn't happen, to what extent would this be a really fatal flaw of the repository, planned repository, given the fact that it's mentioned in all the hypotheses, if the infiltration was a lot higher, particularly in climate change?

Now, I know that some of your own climate people have told you you can expect up to four times as much precipitation as we have now. I guess associated with that, and maybe Abe can tell us that, at what point would the infiltration rate become a serious problem?

And the second question has to do with the last hypotheses, dilution. I'm not a hydrologist, but I've heard it said enough times that it's going to be very difficult for DOE to establish really what the dilution in the saturated zone is. If this is true, what does it place upon you conceptually and numerically?

DR. YOUNKER: Okay, let's start with the first part of it. Refresh my mind.

DR. REITER: It has to do with the importance of the fact that there's always going to be a low infiltration rate. What significance would be a higher, and how much higher would you start getting into trouble?

DR. YOUNKER: Let me answer part of it, and then I'll see if Abe wants to say something about the TSPA and what it tells us.

This is a strategy for a dry site. I mean, it's a strategy for Yucca Mountain, taking everything we know about Yucca Mountain and trying to put together the best kind of case you can make for the way we think the system will function. And I think in terms of how much flux would be too much, you know, what would cause the system to fail, you know, I always go back to the question of how much is it worth to you. I mean, there are probably all kinds of compensating things you could do and trade off if you end up with evidence that suggests you do have a lot higher flux.

Everything we have found to date, and all of our modelling that we started out with in the late Seventies and early Eighties suggests that's probably not going to happen. But, you know, we obviously have to consider it, and the way you do it is to do sensitivities on that issue and performance assessment.

Abe, do you want to mention what we've looked at in terms of the impacts of climate change?

MR. VAN LUIK: This is Abe Van Luik, DOE.

Yes, I would like to refresh your memory. If you'll look at the charts in the TSPA-95, for example, that go for very long times, you see some undulations which are from periodic climate changes where we do double and quadruple the amount of water coming into the system. So basically, the things that we have analyzed so far do not address the scenario, except for the most optimistic cases on the left-hand side of the chart, you know, of the CCDFs that we have calculated.

The left-hand side would correspond to what this new data is suggesting may be the case. The right-hand side where the consequences become a little higher, but still not extremely high, from our perspective, already indicates the types of fluxes that we would see if this new data is not true and if we are pushing water through at the rate which is physically possible in the mountain.

So I think in TSPA-95, we have actually already bounded both what the new data suggests and what you're suggesting, that it may not be true. That's my reading.

DR. REITER: I thought somebody said that if you achieve certain infiltration rates, there would be problems, and you were sort of limiting those rates based on your information. I'm trying to find out where would you really begin to have problems? What kind of infiltrate rate is it? Is it 1 millimeter, is it 5 millimeters?

MR. VAN LUIK: Well, I think we can go back as far as the NRC's own calculations back in 1991 suggested that anything beyond 2 millimeters per year at the repository level was a problem. I think we generally, we went up to that same level in TSPA-95, and that's where we begin to see that we have to take some extra measures in the engineered side of the system to counteract the advective flow that would happen at those flow rates.

So basically, my gut feeling is about 2 millimeters a year, and we have to start looking seriously at a backfill or at a ceramic coating to give us very long-term performance. But even that, you know, is just an opinion based on what we have done so far. We're not done yet.

DR. LANGMUIR: Let me put it this was, finding a higher infiltration rate, at this point you don't think is necessarily a fatal flaw?

MR. VAN LUIK: I don't think it's a fatal flaw. For one thing, I don't think you will find a uniformly high infiltration rate or flux rate at the repository level. I think you might find zones where it is higher than other zones, and if we can identify those zones and figure out physically what's causing it, we can bypass those in the emplacement process. So I don't think that in and of itself is the fatal flaw for the system.

DR. CORDING: Pat Domenico.

DR. DOMENICO: Again with that flux, a couple of things. First it's a hypothesis; I presume that means it's something to be measured, which I gather is not an easy task to measure a flux. But that being said, the past iterations you did demonstrating what the change in the flux will do, I do believe were based on a 10,000 year period, and now you may be faced with 100,000. How does the flux enter that if you have to consider this longer horizon? Was what Abe just told us, does that still hold, or is it independent of the time period? In other words, during the pluvial periods, would it be independent of the larger flux if you had to go to a longer time period according to standards?

DR. YOUNKER: Well, TSPA-95 did include calculations that went out to the peak doses, and so it went out into I guess to a million years, they actually ran the calculations. And I think what Abe said does apply.

I was going to say that I think the other piece of this is the one that we suggested in terms of what's the role of the Paintbrush non-welded in diversion. You know, even if you get those higher infiltrations at the surface, the question is what passes through is percolation flux and gets to the repository level, and I think if the signal we're setting from those fracture coatings is telling us that the last time we had a lot of, or at least a lot of flow that could precipitate that kind of material passing through was

100,000 years plus, then that's probably something we really need to look at in terms of our conceptual model, the hydrology and the past hydrology.

DR. DOMENICO: Even the diversion above the Paintbrush as a hypothesis is very difficult if not impossible to actually get some handle on.

DR. YOUNKER: The key observation would seem to be, though, how much water are you seeing when you look both through the places where there are no fractures in the ESF, and when we encounter fracture zones like the Ghost Dance that Rick was talking about earlier. I think looking at some of those things that could be conduits if there is water in transit right now, will also start to be another important piece of the puzzle.

DR. DOMENICO: When we had our last discussion, informal discussions, I seem to recall the flux was the key to everything. As the flux gets lower, the site gets better in terms of the doses, and I believe that still goes. But now I'm hearing that as the flux gets higher, you're looking at design features to compensate, because you can't change the dilution and you can't change a lot of other things that are natural.

DR. YOUNKER: Well, I think any time you look at total system performance, the way you think about it clearly is to look at what you expect from the various components. And if there are easy ways or acceptable ways of gaining more performance from one of the engineered barriers, like a backfill or like the waste package, by using some kind of coating, you know, you would naturally look at that and see whether that makes sense I think within the context of the total system. At least that would be my way of moving.

DR. DOMENICO: Thank you.

DR. YOUNKER: I did miss the last half of Leon's question, which was the question of dilution, and Pat kind of brought it up. And I guess you were asking about how important it is and how difficult it is.

DR. REITER: Yes, some people I hear saying it's really going to be very difficult to establish what that is, and I wanted to know how important that was, both quantitatively and conceptually.

DR. YOUNKER: Well, I think it's very important because just the volumetric, you know, taking the small amount of flex that we estimate will pass through the repository and putting it into the larger volume that you get below the water table, gives you a very important factor, you know, order of magnitude, several orders of magnitude reduction, and TSPA-95 shows that, and just simple calculations will show you that. So I think it's very important.

The question of how you're going to be able to get at that, there is some planned field testing that will help us I think, and that I think will get high priority if we're able to move the program along.

Our concern, like yours, is that there is certainly evidence from the whole vast literature out there that looks at contaminant flow and mixing and potential for how, you know, some plumes just plain don't mix, and there's some good reasons for that. Some of those don't seem like they would apply at Yucca Mountain. So we're going to have to look at other sites, look at the kinds of chemistries that you observe in contaminant plumes that do not mix, and compare that to what we have at Yucca Mountain. I think some of that work is just going to be essential to help us build confidence as to what kind of concentration reduction we will get through dilution.

DR. REITER: So if you have a serious problem in establishing that dilution potential, is that a serious problem for the site?

DR. YOUNKER: I think that the ability to rely on some reduction and concentration through dilution is very important. I don't know, you know, once again I would go back to the balancing question of what other system component can you bring in if it looks like as if you're going to have trouble showing how much dilution you can count on. You know, I think some will happen. I mean, it happens; we know it happens. We measure it and we see it in nature, but the question for this site, how to substantiate how much we can, maybe that will be an uncertainty that you will have to compensate a little bit, go a little bit further in your engineered barrier system design than you would if you were able to pin it down better.

DR. CORDING: Looking at these various portions of the hypothesis that reduce the dose ultimately, really, the unsaturated zone retardation, anything in the unsaturated zone is really absent below the--you really haven't included that at all. Is there, particularly when one looks at an uncertainty with respect to humidity calculations and the fact that some of these thermal tests won't be available for several years, are there things that the unsaturated zone can do for you that will help compensate for that, for example, waste packages going sooner than expected?

And, of course, there's other things in terms of delay, retardation, that it can do, whether it affects peak dose or not. I was just wondering what your thought is on that, and I understood there's been some comment on that issue with respect to the development of this within the DOE.

DR. YOUNKER: Right, that's very true. And what you described is exactly what the current text says, as we're responding to comments and as we've gone through and wrestled with this. But as you correctly point out, the unsaturated zone retardation doesn't buy you much in terms of bringing down peak doses for the troublesome species like neptunium.

But from the standpoint of potential for early failures, if something does go wrong, if there is a localized area where you could get some water transport earlier because it's colder or because it's a fracture zone we didn't spot as a potential fast flow path, then if you did have early failures, I think a conservatism in the strategy or a good kind of sense that we are on the conservative side is that you certainly would get some retardation of some of the species. If you hold some of the short lived species for as long as it looks like you would, for some of the ones at least, you could really improve performance a lot, and it's a good backup for those potential early failures.

DR. CORDING: Do you think that maybe then become part of the strategy?

DR. YOUNKER: It is written into the current text.

DR. CORDING: The other item is you state in your first view graphs that the intent of this strategy is to aid in supporting a near-term decision, and you're focusing on that obviously. What thoughts do you have or plans are starting at this point to think about how you take this strategy and make it a strategy that leads you to licensing? I mean, are there other portions of the system that you study further? This is one strategy that gets you through, you know, you get from where the water is coming in, the flow comes in to where it gets out in the accessible environment, and you have kind of a linear approach to that. Are there other things that you would add to this? Or what do you see now that you would do if you were looking at the further study post this initial decision point?

DR. YOUNKER: Well, I think the same thing that Mr. Barnes talked about, you know, we have a fair number of people working on the contingency planning that would help support. You know, if you use something like this to make your case, the part of your safety case for this design that we will put forward, then what more will you do in order to either recommend the site if we were still operating under the current regulations, or to take the site forward to licensing, and I can't comment explicitly on, you know, how much more it will take. But I think that most of us working on it feel like that this is a big piece of the case you will have to make.

There's certainly some other information. If you look at Part 60 right now, you write a license application, it will have to have a comprehensive presentation of our understanding of the site. Some of that specifically feeds to the hypotheses that we're trying to test here, but certainly the whole preclosure performance spectrum has to be supported, you know, from preclosure radiological safety compliance, for example. So there's a lot more to add, much of it I think already available.

DR. CORDING: And perhaps that would be described, though, in terms of a strategy that would be perhaps expanded from what--you describe it as a strategy expanded from what you're describing for the first decision. Is that what you might do?

DR. YOUNKER: Well, I think what you're asking me to do is kind of look ahead and say what more would I have to do in order to say put a license application in front of the NRC that they might docket. And those kinds of questions are exactly the questions that we're wrestling with. You know, if you get to this viability assessment with a good strong case and a design of the level of maturity that we're going to produce, then what else will you have to do.

DR. CORDING: I think it's partly, looking at that now is I think relevant in that it helps you define what you're really accomplishing with the viability decision and what more you really are going to be doing. It's not that everything ends at the viability decision. You're going to be doing more. How do you see that fitting in? And perhaps you can't describe it in detail at this point. Your focus has to be on first things first here, but I think that that seems to be part of providing a perspective that allows people to buy into various parts of the strategy.

DR. YOUNKER: I couldn't agree more.

DR. CORDING: Thank you. Don Langmuir? One or two more questions.

DR. LANGMUIR: Looking at your overhead Number 10, which is the dilution one, I gather, remembering this now that we didn't have dilution in the saturated zone as a factor in TSPA-95, it wasn't brought in yet and it's now being brought in, as we all think it should be, as a consideration in future TSPAs?

DR. YOUNKER: It is there in TSPA-95.

DR. LANGMUIR: It was in 95 also?

DR. YOUNKER: Yes.

DR. LANGMUIR: Excuse me. Okay, the back of the envelope, which I'm not a hydrologist, so Pat could do but I can't do, but I'd like to have a feel for it from you, an obvious first factor in any concerns about getting to the accessible environment with radionuclides is your assumed dilution factor. When you come from the unsat zone down and you hit the saturated zone, you get a saturated thickness, do you come up with figures like 1 to 1000, 1 to 10,000 is the probable dilution you'd expect under the repository, assuming total mixing, or if you assume you're going a long way ultimately mixing in the whole sat zone?

DR. YOUNKER: I think TSPA-95, and Abe, you may have to correct me on this, but I think you get something like two or three orders of magnitude for dilution in the saturated zone.

I think it was two orders of magnitude. And I think in the strategy, the very primitive calculations that we've done trying to kind of get orders of magnitude probably takes it a little bit higher than that, thinking that that is a fairly conservative number, but open to all the questions that Leon Reiter brought up.

DR. LANGMUIR: This obviously ties directly into Leon's infiltration rate range of option.

DR. YOUNKER: That's exactly right.

DR. LANGMUIR: Is 1 to 100, is that the least dilution for the max infiltration rates?

I guess I would be interested in what the least would be and what your best assumed reasonable value would be, too.

MR. VAN LUIK: I would love for someone from one of the disciplines to answer this one. This is Abe Van Luik, DOE, again.

It's my opinion that the upper bound--no, the lower bound on dilution came out about two orders of magnitude, and then it went according to--you know, we did this probabilistically and put in ranges of data, and I think it went as far as maybe four orders of magnitude. But I would love for someone here that was involved in it--

DR. YOUNKER: Dave Sevugian is back there.

MR. VAN LUIK: Oh, Dave Sevugian maybe can answer this better than I can.

MR. SEVUGIAN: Dave Sevugian, Performance Assessment. You're testing my memory here, but I think we had a question when we reviewed the document from Sandia, for the absolute worst dilution, if you assume glacial conditions, 10 millimeters a year in the unsaturated zone and a very low flux in the saturated zone, was a factor of 10. The expected value was in the range of, depending on the unsaturated zone flux, it was somewhere between 40 to 800, and that was at the 5 kilometer boundary. That's the best I can remember the number right now.

DR. CORDING: Okay, thank you. One last question from John Cantlon.

DR. CANTLON: Jean, obviously as you've moved ahead, the role of the engineered barrier system has gained in prominence, and if we listened to Wes Barnes' presentation this morning, it now essentially is the point of departure in his relationship with Congress.

That also is going to raise a problem in the EIS determination where alternative technologies are really going to be pressed on you. And I guess it surprises me that the possible role of fillers in retardation in the mobilization question, you have no data on and don't even seem to be thinking about it. Could you expand why that is such a gap in your thinking?

DR. YOUNKER: Abe, do you want to comment on that? You looked like you wanted to say something.

MR. VAN LUIK: I was hoping that someone was here from Systems Engineering, because we just sat through a presentation on their study, and they are, in fact, considering fillers as one option for meeting some of the performance goals for the waste package. Beyond that, I don't know very much. But if there's anyone here from that study, it would probably be helpful to stand up and say something.

DR. YOUNKER: It looks like they're working on the study.

MR. VAN LUIK: Well, that's exactly what they're doing today. The presentation we got was that they were looking at a couple of alternate filler materials. But, you know, this is all contingency systems engineering type work.

DR. CORDING: All right, thank you very much, and thank you, Jean. We've moved back on schedule here. And we have as our next presentation, an update on site investigations. We're interested in hearing about things that are being learned, and some of which are of course in real time as progress continues with the ESF work. So Dennis Williams will make this presentation. He's Deputy Assistant Manager, Scientific Program. Dennis?

MR. WILLIAMS: Good morning. One of the things I'll do in this presentation on the update of site investigations, I'll really slide over into Bill Boyle's agenda items that are on your official agenda in the, let's see what was it, in the area of what we've learned recently in the testing and into that thing that's identified as a "plumbing system."

With regard to site investigations update, I have a little note on here on surface-based, I put a couple items in there parenthetically. After listening to what our project manager had to say today, I might be falling into a little bit of the same trap that he is, thinking because our surface-based program isn't as extensive right now as it has been in the past, that really nothing is going on out there. Well, that's again a bit of a misconception.

We do have the C-hole complex that's actually pumping water out of the saturated zone, looking at things like the dilution, and we're also getting ready to do some pump tests, single hole pump tests in Wt-10 way down on the south end of the site, and up at G-2 up in the large hydrolic gradient area. In addition to that, we've got a lot of surface mapping going on. So we've got quite a few things that are going on in the surface-based program. Again, not as much as we would have liked, not as much as we had last year, not to the satisfaction of a lot of the staff at DOE or contractors, but it is there.

I'll make a couple comments with regard to the relationship of the waste isolation strategy. This is something that myself and Susan and Jean Younker have been working on. Last year about this time at Beattie, we talked about the waste isolation strategy. We had dozens and dozens of overheads that we worried throughout there, showing diagrams of tests. And I don't want to really get into a lot of the testing today. This presentation is oriented more towards the outcomes, the results.

The first part of it I'd like to talk about is a little bit of the geology, because we've got some surface-based predictions that we've made and we're comparing those to the underground observations, a little bit of a follow-on to what I talked about in July at Salt Lake City at the Board meeting, just hit the Drill Hole Wash Fault, the repository horizon, a couple items on rock quality.

Then we'll get into the hydrology part, which gets into some water age dates. I know there's a lot of interest in that. Fracture-fill age dates, a little bit on the pneumatic instrumentation and what we're seeing out of that, and then some hydrologic observations in the ESF.

This next visual, we pulled a few things out of Jean's presentation with regard to the hypotheses on waste isolation. Again, those that are really relevant to the scientific programs are the low seepage, low mobilization rates of radionuclides, and the dilution.

One of the things that came up this morning was a matter of management confidence versus staff confidence. Well, I'm a manager in DOE, but I probably have more of the staff perspective from a confidence standpoint, and I'd like to share with you a little bit why I do have some of that confidence.

When I see some things like this as far as hypotheses for waste isolation, I kind of build me a little idealized setting of what would be the idealized picture of an area of rock, dirt, whatever, to give you confidence that you had something that would work.

In this case, I looked back at some of the things I've dealt with as far as hazardous waste and sanitary landfills, and basically you're trying to get water out of it from evapotranspiration. You're trying to get water to flow off of a surface. You're trying to develop some barriers in here to downward flow, and ultimately, you're down at a water table where you have some dilution.

On the dilution, maybe I wandered behind Roy Williams at the University of Idaho in the early Seventies too much chanting, dilution is the solution to pollution, but maybe that's where a little bit of my bias comes from.

Anyway, in this type of a scenario, nothing much is going on in here. That's where you would engineer your facility or take advantage of your facility.

The high level relationship between waste isolation strategy and hydrology and geology, obviously the hydrologic processes are key to waste isolation in our natural barrier. These processes of course are in large part dependent on the stratigraphy and the structure of the geology. The geology provides our framework and of course our site investigations, data and analyses tell us about this framework and give us that confidence that we're talking about.

Predictability; I had larger diagrams like this in the July presentation up in Salt Lake City, but we make predictions on the stratigraphy, on the structure, what we're going to get in the ESF. These are just a couple of them that I pulled out. Basically, the Bow Ridge Fault, we predicted at 1+69--or, I'm sorry, I'm going to get trapped into my stationing problem that I had trouble with before--169 meters. And where did we observe it? 199 meters. Where did we hit the Pre-Pah Canyon? That's basically at the top of the Topopah Springs. 1028 meters was the predicted, and we hit it at 1020 meters. So we have that kind of a system going with regard to prediction, and of course that gives us confidence.

Drill Hole Wash, what did we predict? Drill Hole Wash Fault, we predicted it at 2100 meters. What have we observed? A couple of faults down there, much smaller than we had anticipated as far as width, but basically running in the ESF from 1900 to 1940 meters. This isn't the width of the zone. The zone is very thin, but it's coming out of the left wall at 1900 meters and going into the right wall at 1940 meters. We'll be able to see that underground Friday whenever we go on that tour. Vertical offset on that of about 4 to 6 meters. It's got dominant strike-slip movement on it.

The other fault possibly associated with that zone at 2265 meters, it's a north trending fault, about two meters of offset. I do believe in your package, it says 22 meters. That should be 2.2 meters. We did have a couple of typos in that package.

This shows you a little bit of what we had in hand when we made the prediction as far as the explorations out there. This was the surface trace of the fault. As you can see, most of the area is covered with alluvium. We were coming off of some of the features up here that were in the bedrock, and a few shallow drill holes that were in the vicinity.

You do have an as-built section more or less just for your records to show where we are hitting these things as we move along with the excavation of the tunnel. This particular section runs from 1400 meters over to 2800 meters. These are the locations of the faults as we hit them. You do have a decoder ring in there as far as what these different symbols mean as far as the lithology, but basically we're talking about the Tiva Canyon up here, the bedded tufts, we get into the Topopah, we have the upper non-lithophysal, then we go into the upper lithophysal unit which we will see a great deal of when we go out there Friday, because for about a kilometer here, you say in that unit. It all looks the same. It's interesting to geologists, but not very many other people.

We will get down at the middle non-lithophysal. It was predicted out here at about 2700 meters. This is the as-built showing where it was hit, but we do have in your package, I don't have an overhead of it, but we do have--oh, I do, here it is--preconstruction section. We predicted it out here at 2700 meters on that particular contact. This is how we depicted the Drill Hole Wash Fault going into the excavation phase. Of course you saw how it turned out.

On penetrating the repository horizon, we have a plan view along the alignment of the ramp, moving down here, again, predicted at 2700 meters. We hit it at 2720. TBM is at 3674 right now, and of course moving south.

You do have a little cartoon in your package that shows some of the distinguishing lithologic features of this particular contact that we derived from boreholes and from an exposure at Fran Ridge that allowed us to make the prediction on where this particular contact would be located. This is a cartoon of what the wall looks like at that location near 2720 meters. We will see that. It's still well exposed in the tunnel whenever we go in there on Friday.

Some of the key things of course are lithophysae content reduces; that's one of the reasons why the repository horizon was picked, was because of the low content lithophysae. And one of the things that's quite noticeable is we increase larger high angle fractures in that repository horizon rock mass.

I couldn't make a presentation on the geology of a tunnel without talking about rock mass quality rating systems. Rick always loves me when I do this. This is all the data points that we've gathered since the beginning of the tunnel right here at 3600 meters. Basically, that Q system setting over here, it's a Norwegian Geotechnical Institute system of factors of RQD and joint percentages based on empirical data from a variety of tunnels that have been excavated around the world. We keep track of that on five meter intervals.

We have our ground class ratings over here. Again, as I mentioned in the Salt Lake presentation in July, we had basically predicted largely in the fair and poor category on most of the rock conditions in the ESF. We can see the way the numbers are coming out, that we're probably in the good to fair. We're probably better on that than we've predicted.

If you're going to miss a prediction somewhere, it's probably better to go this way than the other way. So, again, building confidence, we can build the tunnel out there.

The hydrology, we talk about the water age dates, fracture fill dates, gas phase, the pneumatics and the hydrologic observations in the ESF.

The diagram that we tend to go back to is based on Montizar and Wilson, 1984, I believe it was in the SCP. It's the conceptual model, cartoon, whatever you want to call it, but it basically shows a west-east cross-section of the mountain. It shows the major geologic units, here depicted as thermal mechanical units, the Tiva, the Paintbrush, the Topopah, Calico Hills, Bullfrog, et cetera, getting down, and then the water table basically broken by the major faults as we know them in the area, this depicting the Ghost Dance and the Bow Ridge.

What are we getting for dates scattered throughout here? The key is over here on the side, back to the A, B, C, Ds, et cetera. Basically, up at the Tiva, moderate water. When we move down into the Topopah Springs, we've got dates of the 200,000 year range for unsaturated matrix. Down here at the perched water, and the perched water sets in here not really at a definite stratigraphic horizon, but very near the lower part of the Topopah and the upper part of the Calico

Hills. So varying from north to south, you actually move across the stratigraphy a little bit.

We don't know for sure what's causing that, but hopefully the data that's coming out of pneumatic, the age dating, everything starting to synthesize together will give us a better understanding of that.

Down here at D when we're lower in the Calico Hills, we have again 200,00 year old water; down in the saturated zone, 15,000 year old water by one measurement, and less than 50,000 by the Chlorine 36. There's been a bit of an anomaly on modern water down in the Calico. Of course, some of you recall that's way over on this side. That was in UZ-16. Tritium, I don't think we still know for sure exactly what's going on with that particular situation.

Maybe a few thoughts on the "plumbing system." I think the upper part of this is getting well defined. I mean, we've got 91 neutron holes out there that go down into the near surface for infiltration. We know a lot about evapotranspiration. We know a lot about run-off. We know those things.

Between the intercepts that we've had in the PTn from drilling, and we've also got Alcove 3 at the top contact, Alcove 4 at the bottom contact in the tunnel, we're having a great--we've got a lot more understanding of what's going on with regard to that.

We also have those pneumatics, the pneumatic instrumentation packages that go across this boundary. We measure the pneumatic response to barometric pressure changes above, internal and below. We'll see a bit of that data a little bit later on. But we're starting to understand a lot more about this part of the system, geohydrologic system or "plumbing system," whichever you prefer.

When there was a mention made of humidity, one of the things that we'll see when we go out there on Friday and go into Alcove 3 with the ventilation shut off, it's a nice humid spot. So it's very interesting. And we ventilated it for a while, and then we saw what we were doing as far as drying out, put a bulk head on it, shut off the ventilation, and the water comes back.

A bit of a heretic with regard to Rick, but it would be interesting if we shut down the TBM for a period of time, week, two weeks, three weeks, shut off the ventilation, what happens in the ESF, what does it do. I think that could be a test, a good test. Anyway, I'll get away from that before I get deep.

Fracture-filling materials. We've got a typo here. We couldn't figure out whether we had 50 or 80 analyses, but it was 50 samples and 80 analyses of U-series. So that's what my climate folks tell us. We were scrambling around yesterday trying to figure that one out, and all of our samplers were in the tunnel. In fact, you never saw such a cast of scientists in all your life as we had in the tunnel yesterday capturing fracture-filling samples.

The whole package, apparent ages 100 to a million years, or 100,000 to a million years. We have a couple of real nice ESF clusters; these which were collected up in the summer of '95, and there's a lot of data points in here, there's ten samples that were in this vicinity. This is after you get below the PTn in the tunnel, so it's the actual tunnel data. And then we have another set of samples, 25 samples, I believe, in this area, a cluster that they took in--let's see, that's further down the tunnel--late last calendar year, and the clustering of course are in the dates over here, 240 to 310,000 years largely in this area.

Repository level; they were collecting samples yesterday. These samples here are from core samples going through the repository horizon, quite a scatter as far as scatter of locations from north to south, but we'll be having some real concentrated sampling in the ESF as we move south.

Pneumatics; currently seven bore holes. We've added SD-12, I think, since the last time we visited with you. Again, for review, we've got pneumatic instrumentation in UZ-4, UZ-5, NRG-7a, NRG-6. We have temporary instrumentation that we put in and out of NRG-5, permanent instruments in 12, and UZ-7a, which is right on the Ghost

Dance Fault, we have a permanent instrumentation package in that now. Likewise, Nye County has instrumentation packages in ONC-1 and--no, their package is in NRG-4.

Part of the pneumatics, part of the confidence on that is to be able to predict a response, predict what it's going to look like before the TBM goes by. You've got a couple of simulations here, one I'll show as a viewgraph. I think you've got an extra one in your package. Basically, we have what the barometric pressure shows from atmospheric. We have a simulated that comes out of Lawrence Berkeley Laboratories, UZ modelling, and then we have the measured response to that barometric pressure, and I think you can see that the simulated and the measured is comparing quite nicely.

We can do more and more of these things in a variety of areas, not only with pneumatics, but predicting flux, if we ever get to that point, predicting stratigraphy, predicting structure, predicting rock quality. These types of things give us confidence on whether or not this thing will work as a site.

This one is hot off the press. It's basically a tracing of a dataset that came out of SD-12, the instrumentation package that was very recently put in here, and that's why it's going to be a little difficult for an understanding. It's the time period of November 27th to

December 4th of this last calendar year.

The capping up here shows you where we actually had the grout set up around the instruments lower down in the bore hole. Basically an open bore hole setting here putting instruments in, putting the grout in to isolate those instruments, and then actually seeing when the grout is setting up, such that it's sealing different isolated areas of the bore hole off.

What are we looking at? Basically, it's an upside down as far as the stratigraphy goes--I'll just turn it over for a minute. Tiva, PTn, the vitric at the bottom of the PTn which fits into the thermal mechanical classification system as PTn, however, lithostratigraphically, it's the top of the Topopah, and then these particular traces further down in the Topopah. You have the depths on the chart there at the bottom.

We see a response here that's very similar to the other responses that we see below the PTn, the fact that the responses, the barometric response is subdued and delayed. You see the delay setting here between peaking--or the valley is probably the best here, the Tiva Columnar really representing the barometric, the atmospheric barometric pressure.

You see the same thing in the PTn, and then the vitric cap rock, but then when you get lower in the Topopah, you see the shift, very similar to the response that we see in the other pneumatics, with the exception of UZ-7a, which is in the Ghost Dance Fault, and it looks like an open hole all the way to the bottom.

Hydrologic observations in the ESF; we had some predicted things with regard to saturations. This is probably pretty easy to make a prediction off of these because we did have core out of the area, so you could be pretty comfortable. But the rock down around the ESF, around 90 per cent saturation. Above the Topopah Springs welded, when you get up in the Tiva, 60 per cent saturations. Of course, when you get in the PTn, the bedded units, you've got high saturations and low saturations, and we'll see some of that in the tunnel when we go out there on Friday.

No perched water in two miles, 3700 meters, 3.7 kilometers, no perched water. We didn't predict any. You've probably been told numerous times that perched water was a contingency test if we ran into it, but nobody felt that we would hit perched water in the ESF, and no dripping fractures.

What are some very preliminary conclusions with regard to the water-age dating? Paintbrush probably isn't precluding the downward flow. Pneumatics have shown the same thing. Water flow in the rock matrix is very slow, 0.1 to .1 millimeter per year calculated. That sets lower than that bounding value that Jean uses.

Faults and fractures may act as zones that allow water to flow to lower portions within the geologic section. This is an interesting one because we'll go out there and we'll stop at a fault when we're out there in the ESF, the one at 2265 meters. We'll look at that fault zone, it's had a little bit of tension on it so you have a block that looks like it's rotated a little bit. So it's something that you would say, hey, this should be open. There should be some fracture fillings in there that would indicate water moving down that thing. It doesn't have any. So what's going on in this particular structure that appears to be open at the ESF level?

Okay, you go on down on the Ghost Dance Fault, of course we've got pneumatic instrumentation, ten packages of pneumatic instrumentation in that particular bore hole going down below the repository horizon and it appears to be open from top to bottom. So we need to sort that out to understand what's happening with regard to faults and fractures.

Perched water; the perched water at that Topopah Springs/Calico Hills contact basically from west to east, what does it represent? Maybe it represents lateral flow coming in from the Solitario Canyon side. It's a hypothesis that we have to test.

And, of course, looking at the saturated zone, where is the water coming from, how is it getting there, what are the aquifer characteristics and how does that relate then to the dilution that we're considering. And things like we're doing right now, pumping on the C-wells, some of our single hole pump tests and work up at G-2 may provide the answer to that question.

I put these two together, a little bit for your benefit as kind of a summation, but with the age dates of the water at the repository horizon, 200,000 years, fractures over here in the 200 to 400,000 year range. That part of it may be starting to fit together.

And what have we learned? The ability to predict from surface-based tests, probably getting pretty good on the stratigraphy, geologic contacts, rock quality, pneumatic response, those items.

What have we found? The surface-based tests can't do it alone. We've had a lot of back and forth about that over the five years that I've been here. Many of us have felt, and I think this is confirming that we need to go underground to verify those conditions to give us that good lateral look at the repository horizon. But both those surface and subsurface geologic and hydrologic studies enhance our understanding of the site.

What's the last thing? The confidence. Great news; I feel that we've got great news here because, number one, we can build a hole out there and it will stay open. I had my doubts when we started, but that's what we've got now, and in my mind, it looks real good. Available space. You remember some of the early studies about the geometry of the pork chop, it basically cut off on the north end because of the Drill Hole Wash Fault. The Drill Hole Wash Fault may be pretty close to not being significant.

Can we move further north? I think that that's a real possibility. And I put it as the beginnings of an understanding of how the hydrology works in the unsaturated zone at the repository horizon.

I say the beginnings of an understanding because we're starting to get some data. We're starting to get quite a bit of data that looks good. But if you go to the back of the book and get the answer, the answer may be that it looks pretty good, but what are the processes that allow you to draw that conclusion?

One of the things that we hammer on a little bit in our discussions around the AMSP staff is, hey, this is probably a pretty good site. We all feel it's a pretty good site. But can we defend it in the regulatory arena? And most of the time it comes down to what we need is the parts for the defense.

We feel comfortable, we feel confident that this thing can work based on our experience with other things, hazardous waste sites, dams, civil projects, those kinds of features. Will it work? Yeah. Can we defend it? We probably can eventually. We probably can't mount a regulatory defense today based on the information we've got.

Thank you.

DR. CORDING: Thank you, Dennis.

One of the observations that I was able to make just walking through the tunnel, at the point where you'd already gotten down into the repository level and turned the corner, but it was interesting to see the really high quality in the lower lithophysal zone. There's some very large lithophysal vugs or voids there that are in the order of size of baseballs to basketballs. And that's the sort of thing that can break up a core, but it's not going to do much to a tunnel.

MR. WILLIAMS: Right.

DR. CORDING: And there's actually less fractures in some of those sections; they're a little bit softer materials and less able to propagate fractures, natural fractures.

So I was wondering if there's any thought there about look at that as that zone itself has any potential for consideration as part of a repository, or do you really feel that we should be down in the proposed lower zones below that, the Topopah Spring?

MR. WILLIAMS: That's really not my bailiwick. But I guess what I would encourage the repository designers to do is go back and look at their original criteria for making a pick on a horizon, and maybe look at what they're getting now from direct observation of the lower upper lithophysal unit, and of course the upper middle--or the middle non-lithophysal unit.

But when you look at that rock out there, it's real interesting because when you start looking at it in detail, and I'm referring to the lower portion of that rock, the lower portion of the upper lithophysal, it's almost like there's a lot of little fractures or incipient fractures in that rock mass. And we're going back and look at our RQD measurements, I mean, when we drilled the holes. What were we looking at that gave us those lower values that ended up with lower Q values.

And I think you can understand that when you're drilling that hole, that four inch diameter hole, especially air drilling conditions, you're going to rattle that core around a lot, and if there's a potential for it to come apart, it will, and that's reflected in the RQD.

DR. CORDING: We have a very short period of time, but I certainly want questions from the Board. Clarence?

DR. ALLEN: Just a couple of questions. You mentioned the fracture fillings in the repository horizon. To what degree do we see literalization of the fault zones as distinct from the fractures, and have any of those been dated, and to what degree is that a ubiquitous feature in the fault? And, secondly, you mentioned what is possibly the Drill Hole Wash Fault. You had evidence of strikes of displacement. I assume that has to be from slick and slides I guess, and if so, to what degree are these ubiquitous among the various faults you've seen?

MR. WILLIAMS: I could probably give you some anecdotal remarks on what I've just observed going in and out of the tunnel, but I'll leave the details up to some of the folks that we've got out in the audience that have studied it a little bit more.

But with regard to the first one, do you see any fracture fillings on the faults, I can't recall--let's see, we looked at Drill Hole Wash yesterday. I don't think I remember seeing a lot of fracture fillings there. And, likewise, on the Bow Ridge, when we went through that--you know what the Bow Ridge looks like up at Trench 14. It doesn't look anything like that at the bottom. As far as actual fracture fillings coming out of the fault down there, I'd have to defer to our folks that are doing the direct sampling of it.

On the faults, the movements, you can see slick and slides on some of the surfaces. Maybe Tim could give us--he spent some time with the geologist on those features yesterday, if he'd like to--Tim Sullivan, DOE team leader for the geology team.

MR. SULLIVAN: Tim Sullivan. Good morning, Clarence.

First off, slick and slides are not ubiquitous in the ESF. In fact, they're very uncommon. The mappers showed us yesterday at least two locations where near horizontal slick and slides were preserved. One was on the Drill Hole Wash Fault, what we're calling the Drill Hole Wash Fault at about Station 1940 that Dennis pointed out earlier. And again on a fault further along in the tunnel right near the mapping entry that you'll probably have an opportunity to see tomorrow.

The normal faults which predominate in the tunnel, to my knowledge do not exhibit slick and slides. We wouldn't expect them to be preserved in the lithophysal units I don't think anyway.

If there's anyone else that would like to comment further on that, they're welcome. But, again, slick and slides are pretty unusual in the tunnel.

The Bow Ridge Fault zone is not--you know, there are fracture fillings in the tunnel with thicknesses of carbonate that range from a few millimeters to as much as a quarter of an inch or more. That is not typical of the Bow Ridge Fault, at least the exposure that remains, although it has been heavily sampled, and maybe John would like to comment on that briefly, John Stuckless.

MR. STUCKLESS: John Stuckless, USGS.

The Bow Ridge Fault has a small amount of calcite on it. It is only on the footwall, which is an interesting observation, if you wanted to make that a saturated feature, it would have to be on both the footwall and hanging wall. But there isn't very much there.

The thing we're looking for most, Clarence, is when we get over to something like the Ghost Dance Fault, which is a large fault zone and very well brecciated, to see if we get a difference in dates of the material there.

DR. CORDING: Thank you. Brief comment from Don.

DR. LANGMUIR: Yes, not a comment, but a question, hopefully brief.

One of the big controversies that was raised by the State of Nevada, and a concern which I think the NRC picked up on last year or year before, was the issue of pneumatic effects in the tunnel, and the possibility of radionuclide releases which would be compromised, the studies would be compromised by the ESF.

This is the first time I've seen testing, pneumatic testing results in your presentation, Dennis, and I was very intrigued to see that the modelling of the pneumatic testing measurements were excellent by and large. You could predict the pneumatic effects very well.

I guess I'd be curious what that's telling us, what the relevance of that is to potential releases of radionuclides. We've measured pneumatic effects, we can predict them. How does that tie into concern, if we still have them, about potential releases of radionuclides pneumatically?

MR. WILLIAMS: I'll go to Jean and her crew on that one.

MR. VAN LUIK: Abe Van Luik, DOE.

Under the what we expect to be the new regulatory scheme, we're not looking, we're not concerned with Carbon 14 because it will not be a dose contributor. We've done calculations very pessimistically and shown that doses like .12 millirem per year can be expected for a poor individual living on top of Yucca Mountain from Carbon 14. We always felt that was a non-issue, so if the new regulations are dose based, that goes away as an issue and the pneumatic pathway becomes an issue of very little importance, except that it does give us insight into the connectivity of the different units in the mountain. And I think from a geological/hydrological perspective, this is very important information now. From a release dose perspective, it becomes a moot point.

DR. LANGMUIR: Okay. And none of the other gaseous radionuclides are an issue, I take it. That's the inference.

The iodine, for example, is not an issue.

MR. VAN LUIK: We expect that some of the iodine may be released as a gas from the waste form itself, and then will be transported once it hits the host rock in the aqueous phase. That is our expectation and our conceptualization of that particular mechanism.

DR. CORDING: We need to move on. One more question from Jared Cohon.

DR. COHON: Your Overhead 21, when you report your conclusions from water-age dating, the last one says saturated zone water originates primarily from the north. It says saturated zone water originates primarily from the north; that's one of your conclusions.

MR. WILLIAMS: Okay.

DR. COHON: Did you show us data that substantiates that, or is that other data?

MR. WILLIAMS: No.

DR. COHON: Okay. Are you likely to learn, or are we likely to learn any more from the ESF about the saturated zone than this kind of thing?

MR. WILLIAMS: From the ESF on the saturated zone? I don't think so. We're a long ways above the saturated zone.

DR. COHON: I know that. Are there other tests planned on the saturated zone?

MR. WILLIAMS: We've got the C-well complex that's anticipated to be quite a long-term pumping and tracer testing complex, and then we have a variety, or quite a large number of older holes around the mountain area, what we call the WT holes, the water table holes, that were drilled back in the Eighties that we're going back in and cleaning those holes out and doing single hole pump tests in them to try to get some aquifer characteristics.

Single hole pump tests of course are a pain. Whether or not they give you real good data or not, we've had a lot of discussion about that. But those particular holes are available and we're giving it a shot.

Likewise, up on the north end, G-2 goes down through what we thought to be the large hydrolic gradient, and we're evaluating that a little bit more. If there is a large hydrolic gradient, that is saturated zone up there at the north end, and we would understand more about that. But right now, that's about what's on the books.

DR. CORDING: Okay, thank you very much.

DR. DOMENICO: Ed, just one quick one.

DR. CORDING: Okay, Pat.

DR. DOMENICO: There's a statement Topopah Spring water flow in the rock matrix is very slow, .01 to .1 millimeters per year calculated. How was that calculated?

MR. WILLIAMS: One of the hydrology guys on that. Russ Patterson, would you care to--

MR. PATTERSON: Actually, I'm going to defer that to Bo.

DR. CORDING: Bo Bodvarsson.

MR. BODVARSSON: Bo Bodvarsson, Lawrence Berkeley Lab.

The way we have done that is we have moisture tension and saturation data from about 15 wells all throughout the mountain. And the saturation shows below full saturations in the Topopah Springs and in the repository region. We then used our three dimensional EOC model and we match all of these wells simultaneously, and what that gives us is an estimate of our flux through the matrix that is required to give us these saturations, given the rock properties that we measure from cores.

And the actual indication based on these data are that the flux through the repository horizon is less than .1 millimeter per year. That does not preclude us, you know, some fast flow-through faults or major fractures.

DR. CORDING: Thank you very much. We're cutting into the time of Bill Boyle here, and we know that what we're hearing are some very interesting information, but we'll continue now with Bill Boyle's presentation. And his is an update on site in situ thermal tests.

Bill is the geoengineering team leader on the project, and will be discussing some very important aspects I think of how soon they're getting started on thermal testing and what they're going to be able to do, this being one of the major issues with respect to waste isolation.

I think we're quite interested in this presentation. Thank you.

MR. BOYLE: Thank you for all still being here. I know that we're close to lunch. I'll try and go quickly.

One thing I wanted to do was weave a thread through the talk about the tests are important. Ed just said it. But I think a conversation that happened earlier today as a result of questions for Jean's talk, the conversation between Dr. Langmuir and Dr. Clark; one eminent scientist believes that the relative humidity is going to be close to 100 per cent, another says no, it will be somewhat less.

We can provide some answers about that. I'm not saying it gets back to Rick's a design versus the design. These tests will help us provide answers as to what is the true state of affairs.

This is my interpretation of how the thermal testing might relate to the waste isolation strategy. These words are mine. I'm told that some people like to contrast between flow and flux. I didn't go to that detail. And people might disagree whether the thermal testing results relate to Hypothesis 1 or not. But whether it relates to three of them, four of them, I think it's quite clear that thermal testing is an important issue.

The one hypothesis that's not on there is

Hypothesis 5, which is dilution. And, myself, I don't see how the thermal testing will provide us much information on that.

Now, what I wanted to do here was give an idea of where are we at in terms of thermal test data. Dr. Cording had mentioned earlier today we're right where we want to be. The machine is by the thermal test alcove, we're just getting ready to go ahead and get a lot of data.

On the other hand, we're in the position of not being where we want to be, and I'll contrast our position to that of dams around the world. I read recently in Civil Engineering Magazine I think there's over 20,000 dams around the world, and when some organization goes to design and construct a dam now, they can rely upon all that large data base of information as to what works, what doesn't work, why does it work, how long does it work. We, on the other hand, don't have 20,000 repositories to refer to. We have to provide our data base on our own for the most part.

So in the one sense, we're right where we want to be. If we continue the course, we'll generate answers, not necessarily the answers, but we're in a bit of a difficult position compared to other large and potentially lethal projects like dams, in that we don't have a great data base of empirical evidence. And that's largely because rock masses generally are not heated.

There have been some experiments in Southern Nevada. I think many people in the room are aware of them, but there may be some of the newer Board members who are not. In the early Eighties, there was a large scale in situ test at the Nevada Test Site in rock, different from the rocks we have. And later in the Eighties in what's called G-tunnel, there was a single element heater test by Livermore and also another one by Sandia in rocks that are more similar to what we have, but not the same rocks.

And what was interesting out of those experiments as far as I'm concerned, and I talked about it with Dale Wilder who was involved with both sets of experiments, is that the water didn't necessarily behave in those experiments like people would have guessed going into the experiment. What it turns out to be is people learn things as a result of the experiments.

Now, again, to contrast that experience with dams around the world, with 20,000 of them around the world, for the most part in the site characterization and design and construction of a dam, they don't discover new processes that are going on specifically for that dam. Most of the phenomena you will see have been experienced elsewhere.

We also have a fair number of laboratory tests, and this is where you would do a very similar thing for a dam project; you would go out and get core samples and tests and get material properties, and we do have a fair amount of data in that respect.

Now, the history of dams is through the course of time, they've gone from simple to more complex. They've gone from small to large, and they've gone from hardly being around at all to having large lifetimes. Well, again, we don't have that luxury with repositories.

We had a strategy in a document for thermal testing published last summer that essentially said that was our strategy for our thermal testing. We were going to start small, simple and short and proceed through a whole series of tests to longer, larger and more complex. And I'd like to think that we still can follow that strategy to the extent that we can, but there are always pressures of time and money.

Now, what information is to be provided by these ESF thermal tests? Rick had already brought up this subject this morning, and I'll go through it again briefly.

One thing is just shakedown, that is, does the equipment work underground. Do the organizations that are working down there, do they interact correctly. Are the systems in place to handle the paperwork, data acquisition and all that? So we will have a shakedown phase that will help us with that.

Another thing to get out of the tests are the processes and the parameters, and referring back to the G-tunnel and the climax tests where in those tests, it became apparent that perhaps there were other processes acting that people hadn't originally thought about. And so it's--I'm not saying it's likely or probable, but there is a chance that in the course of the in situ tests in the ESF, we may discover that processes are acting that people hadn't really considered in the modelling or in their design.

We're also in there to measure parameters on a sufficiently large scale, things like thermal conductivity, the strength of the rock, the deformation of the rock, modulus. And in that sense, we can again compare it to dams.

As I mentioned, I think in the site investigation for a dam, they're not really looking at new processes that nobody else has ever encountered in their experience with the existing 20,000 dams. But they do do large scale in situ site specific tests to measure parameters, such that even though they know all the processes, they still made these parameters of a sufficiently large scale that it's useful for their design.

Now, some of the information will be used in the preclosure, and Rick talked about that this morning. But it's also very applicable to the postclosure, and in my personal opinion, I think based on the discussion between Drs. Langmuir and Clark, that the tests in the long run will shed even more light on the postclosure behavior.

Now I'll start to address the tests themselves. This is a different view of something that Rick Craun showed you earlier today. This is a plan view. We're looking vertically down on the ground surface. The north portal is out here for the Board, and the Board members who are going to go in, you're going to come in this way. You're going to come around.

And back to something Rick said, you know, in a sense, this is in some ways a cartoon, or just a schematic. I doubt that these lengths are to scale or anything like that. It's just to give you an idea of where things are with respect to each other. It's right as you come out of the curve on the inside portion of the curve, that's the location of the thermal test alcove on the repository side of the Ghost Dance Fault.

Now, this is a cross-section of that thermal test area. This is a vertical slice showing us how in a schematic sense, how we're to get down to the test area. Rick had showed this earlier. It looks like a J-hook. It was shown on the diagram I just showed you. But here's the main drift, and the drift will have to decline, and then it turns around and comes back, and then goes horizontal.

This dark line here is the contact between the upper lithophysal zone and the middle non-lithophysal zone.

We want to be approximately 10 meters below that contact when we make the turn to stay away from the large baseball, basketball size holes in the upper lithophysal zone to make the test more understandable for the time being. You know, a large scale test is difficult to interpret anyway, and to introduce the confusion of the lithophysae on how they would affect fluid flow just makes the test difficult at this time.

Now, these are nominal numbers. The geology may not cooperate. We may find that the dip of the beds is deeper, but this design will work for now, and as the excavation progresses, core holes will be drilled up to find out where this contact is to make sure that we stay a proper distance below that contact.

On some of these diagrams, you'll see reference to Phase 1 and Phase 2. This is Phase 1 of the test, and I have no diagrams to show you about Phase 2. Phase 2 would be a large scale long duration test that more closely approximates the conditions you would have in the repository. This test, which the colors didn't turn out very well, these short lines here which are red in the original, these are wing heaters. These lines are instrumentation holes to be drilled from the thermal test drift itself. These green lines are instrumentation holes to be drilled from the access observation drift over there. There would also be floor heaters in place on the floor of the drift.

This test, in order to get an answer in a reasonable amount of time, the wing heaters accelerate the heating of the rock. The way in which heat is put in the rock will be greater in this test than in the repository, and we may get processes to occur that won't occur in the repository with the lower heating rate, which is one of the reasons for having a larger scale longer duration test.

This test is good and it does provide us answers sooner for some issues, but it doesn't answer all questions.

Here is the shakedown phase of the test, and I'll talk a bit more about it in a minute. This is a cross-section. Again, I doubt that the colors turned out so well. But we have a letter code for them. Here's another plan view of that J-hook, and this diagram is a cross-section, a vertical slice through the earth looking at the access observation drift. It's not horizontal; it's declining. And the heated drift itself with the MPC sized in-drift heater.

All these various holes represent the instrumentation holes that will be placed around the drift. As of now, typically three of these cross-sections of instrumentation would be emplaced for the heated test drift. The temperature holes, neutron holes, which are there to measure water content, chemistry holes, again, hydrology holes for where is the water. Mechanical holes refers to how is the rock deforming. The ERT; electrical resistivity tomography is, again, a technique for looking for where is the water.

This test would have a heating cycle for 18 to 24 months and a cooling cycle for 18 to 24 months. The in-floor heaters are roughly 800 watts per meter along the length of the drift, six or eight of them. The wing heaters, which are right here, I think are up to 15 meters long with a variable heat output along the length of the heater, somewhere in the neighborhood of 100 or more watts per meter squared.

Again, I'll point out, somebody asked earlier what are the scales. It is a metric scale, but again, these are generally we're not going out for a firm fixed price contract on these drawings. At this point, they're there to help us.

For this same test, we may have as many as five other cross-sections of instrumentation mainly to be used to measure the deformation of the rock mass, but also because we need corrections, thermal corrections, on the bore hole extensometers. There will be temperature measurements made at least along these instrumentation holes, and also perhaps in the inclinometer bore holes.

Some of the inclinometer bore holes, as you can see, will be slanted. And if there's enough water set in motion to enter that bore hole, there's a chance that with the decline, we could get the water at the end.

That was the heated drift phase of Phase 1, or the heated drift portion of Phase 1. Again, the colors didn't come out too well. This is the shakedown phase, which would be started earlier. Again, this is a cross-section through the shakedown phase. It's a single heater, approximately 5 meters long, I think again 800 watts per meter of length.

All the other holes either going into the section or in the section are various instrumentation holes. We don't have them all, we don't have the little box with the code, but on my color originals, you can tell the difference in the colors. But the neutron holes are for water, ERT where is the water, thermal is, you know, essentially thermometers, convergence pins are to measure how much the drifts deform.

This test will have a heating cycle of nine to twelve months and a cool-down cycle of nine to twelve months. It will have about 1,000 feet of instrumentation holes drilled, whereas the drift scale test will have about an order in magnitude more, 10,000 feet of instrumentation.

I wasn't able to show you all the instrumentations for the shakedown phase in the other cross-section view. This is not a cross-section, but a plan view. There will be a hole for a Goodman Jack, which is a device for measuring the deformation of rock. There will be chemistry holes, hydrology holes. MPBX is, again, a device for measuring how much will the rock deform when the heat is added. And rock bolt load cells will be used to measure the loads in the rock bolts.

Now, another thing, like I said, we're not going out for a firm fixed price bid on these diagrams. We may have fewer rock bolt load cells. When we see you in April and July and October, things may look slightly different.

Now, Rick had much nicer pictures of this than I have, but essentially, it's a snapshot. That's where you're going to be on Friday. That's the thermal test alcove location. I'll skip the other one. You'll get to see it yourself in person.

Now, I have three schedules here. And, again, the first one is our baseline. That's when we did our FY96 planning, this is what's there, and it relates to what Rick had. If you remember the schedule he had, he had an asterisk by the 12/96 start-up of the shakedown. Plans are well underway to drag that forward.

That start, what I call Scenario 2, that really doesn't require that much more spending on the science side of the house, and I can't really say on--I have two Scenario 1's. But there's Scenario 2. It moves up to August of '96. And I think Rick mentioned there are ongoing conversations between the engineering, design and construction side of the house and the science side of the house of changing these schedules.

Rick had mentioned one thing of speeding up the tests is to use the Alpine miner, while I'm convinced that even if the Alpine miner doesn't work, his organization has shown that they are going faster than people used to think they could go. And I think whether they have to do a drill and blast or Alpine miner, science may be in a position of chasing after the constructor, which is fine. That's a position I'd rather be in than waiting.

This third scenario actually does require a lot of money, I mean millions, both on the science side and maybe a million or so on the design and construction side of the house. But this is not new money in the sense, this is money that would have probably been spent in FY97 anyway. But if we could get savings elsewhere in the project and spend the money in FY96 now, it doesn't significantly bring up the start of the shakedown test, but if you look through all these diagrams, it does significantly bring up the start of the larger drift scale test, which really is the more important test as far as I'm concerned, the bigger and larger and longer test is generally better.

For those of you interested in dollars, the baseline funding on the science side for FY96 for these in situ tests is roughly $2.4 million. I think the design and construction side may have had up to a million dollars or so, plus or minus, for the construction of the thermal test alcove. And then there's various other costs spread throughout the other WBS elements, safety and QA and things like that that I'm not aware of what those costs are.

But just to give you, and these are very rough estimates, some number pulled out of thin air, that over the course of the next two years for the shakedown phase, design, construct, buy the equipment, all the implementation, might run as much as a million and a half dollars, or as little, depending on how you look at these things.

The drift scale phase, which is a longer test, that might run as much as 9 or $10 million, and the large scale long duration test, which I did not show you, but would involve multiple drifts, might run as much as $20 million spread over a number of years.

Now my summary. To me, geologic disposal is based on using equipment largely supplied by nature. We get what we get, and there's not much we can do about it. We can't take the mountain apart and put it back in place.

However, how we handle the heat is one critical variable that is largely in the project's control, and the sooner we have an idea of what we want to do with that heat, the better off we are. Averaged over the past few years, we spend on the project out here maybe a million dollars a day. And even though these tests will be so long they can't provide information for the ACD, the advanced conceptual design, due in March, they may not provide all the answers for 1998 Viability Assessment.

The sooner we get them done, the better off we are in determining whether we're correctly spending our what's now and over the past few years been a million dollars a day. And that's that.

DR. CORDING: Thank you very much, Bill, for an interesting presentation and a timely completion.

I would think it would be interesting to see how you can, with the budget situation and all, and kind of coordinating construction and excavation and science are drilling holes and installing instruments, how you can optimize that. And I agree with your point that the most important thing is to get to that drift scale experiment. Now, if you have to do some other things ahead of that, yes, you do them.

But it seems to me that whatever you can do to get to that drift scale experiment, perhaps doing, you know, continuing to run that machine in there to excavate, and currently with some other things you're doing, if you have the ability to do that with the funding and those sorts of things, that would seem to me to be desirable to be able to move that start-up date of the big test as much as you can.

Are there any other comments from the Board? Yes, Don Langmuir?

DR. LANGMUIR: I'm encouraged, too, that things are moving so expeditiously along.

I have a concern and always have that it will be extremely difficult to evaluate mountain scale effects. And I'm thinking here particularly of the coupled process effects, and in this connection, I'd like your thoughts on the design itself of some of the instrumentation monitoring. You've got drill holes in which you're going to place devices somehow to sample for chemistry and hydrology, and I wonder if they aren't going to become themselves the principal route of movement of fluids in the thermal gradient. That's just one concern I've got.

MR. BOYLE: Right.

DR. LANGMUIR: In which case, you're not really measuring the mountain at all; you're measuring the effects of your engineered system.

MR. BOYLE: Right.

DR. LANGMUIR: How do you get around that one?

MR. BOYLE: Well, the test designers are aware of that. And to use for an example the extensometer holes, the MPBX holes, they're going to be grouted so that we're not going to see water in those. They will not be conduits.

DR. LANGMUIR: Okay. But how then are you going to and when can you expect to see the effects of coupled processes? I mean, the big unknown is always going to be this mysterious reflection process and potential precipitation and dissolution at some distance which influences thermal effects and the isolation of the waste.

MR. BOYLE: This Phase 1, the heated drift scale test, will provide some answers. That's set up to perhaps get water flowing off the sides, but not necessarily in between multiple drifts, which is what we might be able to see in the large scale long duration test, that isn't that well defined yet.

But the answer, if you want the answer, let's come back 10,000 years from now or whatever. To me, that's the only answer, and again I contrast our situation with dams around the world, that even with all the experience, if we were to go down to Hoover Dam, they've monitored it every day since its construction. Even with as much knowledge as they have, it's just prudent to keep watching after things.

So I view our thermal testing won't provide definitive answers in our lifetimes or for many generations, but it's the start of a performance confirmation, if you will, that through time will provide as definitive an answer as anybody will ever get.

DR. LANGMUIR: This isn't going to satisfy Congress in three years or five years. I'm wondering if you're still looking at geologic analogues as a way to help Congress through this? In other words, you've got at Yucca Mountain some intrusive effects, from which you've got measurements of fluids that you can infer from the mineralogy that's changed historically next to those intrusions, you've got other kinds of contact metamorphic phenomenon around the world with secondary effects that you can identify through thin sections of geologic evaluation.

Is there enough of that information available to you, either at Yucca Mountain from historic records like the Schon Levy work, or from other places, to help you through the arguments on what the significance of these effects might be to performance at Yucca Mountain?

MR. BOYLE: I don't know. I'm not entirely confident that there is enough information.

DR. LANGMUIR: Is anybody looking at that?

MR. BOYLE: I can't answer that.

But back to an issue that Dennis brought up. You mentioned that maybe Congress won't buy off on this. I can only speak for myself personally, that you might have been able to tell one of the threads through the talk is I'm an empiricist, using dams as an example. We have no empirical evidence for repositories.

I would go to Congress today and say I have enough faith in Benton and Dave Stahl that they can design waste packages to last hundreds if not thousands of years. We'll give you an answer on the repository somewhere out in the future, but we have every confidence that it's going to work today. And then I would monitor the heck out of it. And I think in a sense, that's what people with dams do. They don't know all the answers up front either, otherwise there wouldn't be any instrumentation at Hoover Dam, but there is.

DR. CORDING: Thank you very much, Bill. And we'll look forward to hearing how you exceed all these schedules as you perceive. Thank you.

We're finishing our session this morning. And our schedule will be such this afternoon that we'll have time for an hour and a half lunch. So we will be meeting again at 2 o'clock, but please be on time. At that point, Wes Barnes will be adding some comments I think that we'll find of interest, and then we'll proceed with the session by Garry Brewer. 2 o'clock. Thank you.

(Whereupon, at 12:30 p.m., a luncheon recess was taken.)

AFTERNOON SESSION

DR. BREWER: Good afternoon. I'm Garry Brewer of the Board, and I will be chairing the meeting this afternoon.

The topic on everyone's lips, of course, is transportation, and our agenda is somewhat modified from the one that you see on the formal presentation. Let me give you the plan for the afternoon.

The first presentation, or the first presenter will be Wes Barnes, who would like to spend a few moments amplifying on some of the comments from this morning, and, also, answering whatever the questions the Board or others in the audience might have for him. He'll be speaking for ten to fifteen minutes, or however long, as long as the questions come.

After that, we will hear updates on the programs from the United Kingdom and the People's Republic of China. It has been a matter of importance to the Board, and I think a utility to the American program, that we have been in relatively constant contact with other nations who are in the process of trying to figure out and solve the disposal of high level nuclear waste, and the two programs that will be summarized this afternoon, you see two very different places in terms of the amount that they have been involved, and the approaches that are being taken.

At the end of the comments from our international colleagues from the UK and the People's Republic, we'll take a short break, and then return to have not a round table--and this is not anywhere on the agenda, but is a consequence of many questions and loose ends that I think came up in the morning meeting. We'll have an opportunity after the break for questions, commentary directed to any of the presenters from this morning. I will do my best to kind of serve as the ringmaster for this, and to direct traffic.

At the conclusion of this period of Q and A, Q and A of loose ends--we'll just call it that for the purposes of identifying it--we will then go to the last item regularly scheduled on the program, which is public comment, something that the Board has always done, and I think, with good effect.

What we didn't do, because we had very few staff in place this morning at the beginning of our session, was to ask members of the public who wished to comment to please sign up with Linda Hiatt. She's easily identified, because she's in red, and right there. Raise your hand, Linda. And this is so that we have some sense of who you are and, on the sign-up, what you represent; yourself or whatever the organization. The sign-up sheet's in the back, and that will conclude the day.

We have, from what looked like it might be a one-hour session, I think we have taken full advantage of the opportunities created by the storms in the east, and we're going to have a good session this afternoon.

At this time, I would like to turn the podium back over to Wes Barnes, the Director of the project here in Las Vegas.

Wes?

MR. BARNES: Thank you.

I asked to be wired for sound so I could move and shoot at the same time. I think they taught that at the Academy.

This morning, a couple of you said--the Board members, I'm talking about--that what I said could be taken to be management's more negative than the scientists are about the project. I want to start by telling you two emotional stories.

One is Daniel Dreyfus. Dr. Dreyfus was Scoop Jackson's Staff Director, so the committee that Murkowski today chairs, Dan was their Staff Director. Dan's a longtime member of the Democratic Party, very well wired in Washington, very well respected. He waited a long time for a Democratic President, and he probably could have had a lot of jobs. He chose this one to make his contribution.

Wesley Barnes owned a consulting company in

Washington, D.C. in 1995, '94, and I was making more money than I'm making today, with less restrictions, and less people telling me what to do, and, honest to God, I didn't pay anybody to kick me in the shins.

When Dan called me and said he had this job open, I took a lot of walks with my wife, talking about did we want to come to Las Vegas. It's still stunning to me to come out of a meeting dressed like this, and open a door and see it's Vegas. You should come here on vacation, not to work or something, so there were other places for us to go. We are not negative about the project, not at all.

I'm very proud of my scientists. You heard, in the next three presentations, the data they're collecting is adding to their belief in the project. You heard how comfortable they are with what they're doing. I know what they're doing.

And there's one other remarks that I made. Chief scientist, I used that phrase, chief scientist. I'm in a hiring freeze. I can't hire anybody, and you, and other people that recommended that I hire a chief scientist, I have a deputy, a Ph.D. geologist, Russell Dyer, James Russell Dyer, and I think of James Russell Dyer as my chief scientist. He's a Ph.D. He's been in the program for a number of years. I think he knows what he's doing, and we've been blessed to form a working partnership in my first year here, so I hope that clears that up about the use of the chief scientist.

Am I negative, more negative than the scientists? Probably, probably. What I'm trying to do is shield them from the outside world and let them do their job. I think that's what the Project Manager should do, amongst other things, so I have to worry about that. They're upset enough when I tell them their budgets are going down constantly, but I want them to achieve the same objectives.

Guys, is there something else I'm supposed to say while I'm up here, all my coaches? Is that it?

(No response.)

MR. BARNES: I'm surprised Russ didn't pull out a piece of paper and say, "Well, I've got these notes."

(Inaudible comment.)

MR. BARNES: Well, before I ask you to make a little ten-minute presentation, which I'm going to do, what else should I say?

DR. DYER: Russ Dyer, Department of Energy.

Wes, let me ask a question on the part of everybody. Where do things stand in Congress now? Things have been back and forth. It looked like there was going to be action, there wasn't going to be action, so what's Dan's current view, what's the conventional wisdom of what's going on and what might happen to this program and this project?

MR. BARNES: Mr. Chairman, do you want me to answer that?

DR. BREWER: Yes, by all means.

MR. BARNES: The Congress comes in for two years, as you all know, so the 104th Congress is going into their second year, so when they adjourned for the Christmas holiday, they did not go sine die. The Congress didn't die. What does that mean?

Any bill that was introduced last year is still alive this year, so there are probably a half a dozen meaningful bills in Congress that would change this program. Some of them declare that we, the government, will lease Area 25 to a private consortium. Some tell us that we're going to build interim storage in Area 25 right in front of the mountain.

The industry told Dreyfus and Barnes that one of those bills, the Upton Bill, would pass before Christmas. It did not. It could not get to the floor, and the reason it couldn't get to the floor--everybody wanted it, they were pushing the Speaker of the House of Representatives to bring that bill to the floor--was it was a budget buster, so they needed a rule from the Rules Committee that would give them relief to bring a bill to the floor that would bust the budget.

When I first heard that the Speaker even put it on the calendar, I thought, "He's either got the rule in his pocket, or he's going to postpone it for two weeks, and they're going to go into recess." He postponed it for two weeks and they went into recess.

That's a long answer to I don't know.

DR. LANGMUIR: Wes, could I ask a related question?

MR. BARNES: What Dr. Dreyfus is getting ready to do is get ready to go in front of Congress in March and tell them how much money he wants, and what he wants it for. To the best of my knowledge, the President of the United States would like to see us finish site characterization of Yucca Mountain before interim storage is ever built, or a site is named. That's my understanding.

DR. LANGMUIR: Wes, have you any feel for whether any of the legislation that's been proposed could get enough support to pass over a Presidential veto, which is likely? There's a whole lot of legislation out there, but as long as there's a veto...

MR. BARNES: That's a very good question. I don't know what that answer is. On the House side, I believe there were over 200 cosponsors to the Upton Bill. The companion bill on the Senate side was introduced with ten, and I think now it's got close to twenty. I don't think that makes it. What is it, two-thirds for a veto? So 200 is not enough--435, that's--that won't get the job done. It depends on what it passes with.

It seems to me that the industry is putting all their money on the Upton legislation. They're pushing every state, the 35 states, to push that particular piece of legislation, so my guess is they're not going to come off that position right now. You know as well as I do that any organization in Washington, including the government, their first goal is self-preservation.

DR. BREWER: Okay. Are there other questions from the Board for Wes Barnes?

(No audible response.)

DR. BREWER: Wes, thank you very much.

MR. BARNES: You're welcome, sir.

DR. BREWER: For the reprieves.

Now I'd like to focus the meeting on developments in the repository programs of two other countries, England and the People's Republic, as I mentioned before.

In England, the Nuclear Waste Disposal Program is in the process of being reexamined. United Kingdom NIREX Ltd., the company responsible for the siting, characterizing, and building of a repository for the disposal of intermediate level waste, is in the midst of a public inquiry brought on by its proposal to build a rock characterization lab at a site in Cumbria County.

The Board believes there may be some parallels between the issues being raised in England and the U.S., as those involve a look at the current priorities of their nuclear waste disposal programs, and the scientific and technical bases needed to support these priorities.

In contrast, the People's Republic of China is just beginning the process of developing a work program, characterize its site for the disposal of high level waste. From our vantage point, much progress has been made in a relatively short time frame in the People's Republic. The program is now focused on a specific area of the Gobi Desert in northwest China, a desert area that shares some similarities to our own Yucca Mountain here in the United States.

Let me now move on to introduce one of our guests, before introducing the speaker from United Kingdom NIREX. We're pleased, really honored to have Sir Richard Morris and Mr. Michael Folger with us today. Sir Richard has served as Chairman of UK NIREX Ltd. since 1989, following his career in private industry. Most recently, from 1980 through 1990, he served as Chief Executive, then Chairman of Brown & Root Ltd. Sir Richard is also currently Chairman of the Advisory Boards of Kellogg Oil & Gas Services, Ltd., and M.W. Kellogg, Ltd. He was knighted by Her Majesty, the Queen, in June of 1992 for his services to science and industry.

A heartfelt welcome to you, Sir Richard, and many thanks for the courtesies shown to us in the past by UK NIREX and others of our colleagues and friends in the UK.

Now, Mr. Michael Folger, our speaker, served as the Managing Director of NIREX Ltd. since 1991. Following education at Cambridge University, Mr. Folger undertook increasingly responsible positions in and out of government, which culminated in his service as Senior Vice President in the London office of Dean Witter Reynolds.

From there, at Sir Richard's urging, from what I understand, Mr. Folger departed to serve as Managing Director of UK NIREX. He twisted your arm, as we say in the American vernacular.

Mr. Folger will provide us with a brief update on overall developments in the UK's waste disposal program. Then he'll mention a few of the issues surrounding the use of expert judgment, and probabilistic risk assessment.

Welcome, Michael Folger.

MR. FOLGER: Thank you, Chairman.

Ladies and gentlemen of the Board, Sir Richard and I do very much welcome the opportunity to meet you and give you an update on progress in the UK. I think it was June, '94 that the Board--and I think it would include still some of you here today--visited our site at Sellafield, which is on the coast of northwest England, and had some discussions with some of my colleagues at that time.

Make the magic lantern work. We're getting there, we're getting there. That's it.

DR. BREWER: No. At this point, Michael, you say, "Are there any questions?"

(Laughter.)

MR. FOLGER: There we are, a fine shot of NIREX, and there is our site, our prime object of interest, and I think it's interesting to look at its location, because it's both geologically and historically interesting. This is the reprocessing plant of British Nuclear Fuels at Sellafield. There is a Magnox power station, and this, of course, was the British Hanford. That's it, so, in there, these were the early piles for the production of military plutonium now being decommissioned.

Our focus of activity is a mile and a half, three kilometers inland from there. This shows some of our drilling rigs. You can see the rising ground behind the facility. You can see the sea, the Irish Sea. So, that was a scene in assessing which some of you may recall from '94, but, since then, as reflected in the background materials which I think I sent on ahead of time to members of the Board, we've seen quite a lot of progress.

I'll keep my remarks brief, because I think in the questioning, some of the differences, but some of the similarities with the U.S. program will come out. I will stick with a high level overview. As you've heard from the introduction, I am not a scientist. I did run some numbers at MIT, but that was at the Business School, not in the Geology Department. My scientists are today, in Day 51 of a public inquiry where Greenpeace and Friends of the Earth are producing some of their spook science, so I'm afraid it's me that you have, rather than my Director for Science.

The national policy on radwaste disposal in the UK was reaffirmed by the British government in a "White Paper," curiously enough, last Fourth of July.

The financial and philosophical arguments about the timing of disposal were reviewed in detail. The British government set its whole review process in the context of the Rio declaration and sustainable development, and it confirmed deep disposal of radioactive waste as an important part of sustainable development. In the UK, those international organizations are seen as very important, and are an important part of the debate.

The clear conclusion was that construction of a deep repository should proceed as soon as reasonably practicable, once a suitable site has been found, and my company's program to identify such a site was given full backing. The precise timetable for availability of the repository was explicitly recognized as depending on the scientific requirements for establishing a sound safety case.

There was a recognition, too, of the time needed to secure planning permission, or what, in U.S. Parliament parlance would be called zoning approval for each phase of our program, and, also, for the regulatory approvals needed along the way.

I should stress that there is no special legislation governing our program. As we proceed to make these investigations, and, in due course, we proceed to develop a repository, we are treated in just the same way as someone seeking to deliver a shopping mall, as it were. There is no special legislation. NIREX is a company with slightly special constitution, but it has no special powers. It must proceed each step of the way as if it were a private developer, though, of course, with a very full and proper framework of nuclear safety regulation.

The priority for deep disposal in the UK, and the focus of NIREX's current responsibilities is intermediate level wastes which arise from reprocessing of fuel. We do not, at this time, have utilities which have taken a firm decision to propose direct disposal of fuel. Historically, the fuel has been reprocessed through that plant that you see there. There may, within time, be utilities proposing direct disposal of spent fuel, but at the present time, the focus of national policy is on disposing of the intermediate level wastes from reprocessing, which can be thought of as roughly equivalent to the TRU wastes, as you call them in the States.

And there is a rather difficult to fathom photograph of a 500-meter drum--it stands about five feet high--filled with swarf stripped, being the fuel cans stripped from a metallic uranium fuel, which was what was used in our Magnox reactors, and it is that material which is the main driver for the disposal in the UK. It's a magnesium alloy material. It's of a mixed character, relatively bulky, and less stable, chemically, than vitrified high level waste or oxide fuel.

And the continuing strand in government policy has been that because of its chemical form, its relative bulk, we're looking at disposing between 200,00 and 275,000 cubic meters of that material. We should be moving ahead to solve that problem first, with spent fuel and high level waste, which pose, in our view, a somewhat lesser technical problem --I know that may not be the view of all our U.S. colleagues, but, because of its chemical form, this material is the one to get ahead with in the view of the UK government.

Because it contains magnesium, and because we are not blessed with a Gobi Desert or a Nevada desert, and we have a wet geology in the UK, water plus magnesium will cause hydrogen and gassing off, so all these containers are vented, so there is no hermetic seal. There's no possibility of cladding the material with copper, which is what the Scandinavians are looking at, and I was interested to hear the idea this morning of ceramic coating of the fuel rods in the U.S. That's not open to us. If you attempted to clad that or hermetically seal it, it would split open because of the generation of gasses.

High level waste from reprocessing in the UK is currently being converted into solid form, after which most of it will be held by British Nuclear Fuels at the site that we saw earlier, in a passively safe surface store, to cool down for at least 50 years.

Last year's White Paper announced that the government would be initiating research work to define a specific long-term strategy for that waste, which would also cover any spent fuel which the utilities do offer for direct disposal. That strategy is envisaged as leading to disposal in an underground repository separate from the NIREX repository, separate, probably, in geography, and certainly separate in time, because it will be 2075, 2080 as the indicated date, the second half of next century, before anyone proceeded with a repository for that material because our philosophy is to let the high level waste cool before disposing of it.

So, against the helpful policy background of the government reaffirming deep disposal, we have made some excellent progress over the last 18 months with our site investigations. In terms of specific expenditure at Sellafield, we have committed something like $300 million up to the spring of '95 through a program comprising about 20 deep boreholes, some running to a depth of more than two kilometers, and other studies, including seismics, electromagnetic studies, and the full panoply of geophysical studies.

In the light of that, we've been able to prepare a first-cut, risk-based safety assessment for the crucial groundwater pathway, and all this is in relation to a conceptual repository. I very much found myself sympathizing with some of the remarks this morning, about the importance of understanding the site in relation to a conceptual design rather than burning up too many taxpayer dollars up front in a detailed design for the final repository.

But, the probabilistic modeling that we've done of the performance of a conceptual repository shows good performance for a range of future climate states for such a repository at Sellafield, and the numerical yardstick that we have is a ceiling on the annual risk to a representative member of the critical group at any time in future, and that has been set at one in a million per annum, or 10^{-6} target ceiling, and, of course, depending on what the ICRP recommendations are at any time, you can translate that risk ceiling into a dose target.

And the result we have is robust and not just to the setting of different climatic assumptions, but also to alternative treatments of the performance of key features of the Sellafield site, and I should, perhaps, at this point set out that site.

I showed you previously the sea. Here is the Sellafield reprocessing plant. Here, inland, is our focus of interest. This is the nominal repository zone, high land behind this, rising to 3,000 feet, which happens to be the highest land in England, if not in Scotland. A volcanic rock, a tuffaceous rock--a very different kind of tuff from that which you have at Yucca Mountain, much harder--it lies in the area of interest some three or four hundred meters below the surface, and there is an overlying sequence of sandstones, and a breccia, impermeable layer, somewhat broken up, but, nevertheless, present to the western part of the site.

So, that's the setting that we have, and the evidence is that there is a very sluggish flow of saline water, not a brine, but something perhaps 50 per cent or 100 per cent more salty than sea water in this underlying rock. There may be something of a U-tube effect driven by the exposure of these rocks inland, in the high mountains, so a small upward driving force and some component of upward movement through here, but, in the overlying sandstone layer, essentially, what, as a layman, I perceive to be a flushing action, heavy annual rainfall, 60 or 70 inches running off through the sandstone, and, therefore, carrying any material that is taken up by flows through the repository zone, a mixing action, and carrying it offshore.

For disposal of the reprocessing waste--and I showed you a classic form of it a few minutes ago--our concept provides for packaging in stainless steel drums which are then set within a cementitious backfill, which is expected to hold the pH above 10.5 for around a million years.

So, the concept that we have is that within the stainless steel drum, what I showed you, although we are not making verified claims for its performance, we believe that, in due course, we shall be able to set down a life of perhaps 1,000 years for that stainless steel drum. That will take care of the fission products, and the cementitious backfill present in hundreds of thousands of tons of quantity will suppress the solubility of the actinides.

In this setting, we have no alternative but to assume full resaturation by the groundwater within a short space of time, tens or hundreds of years once the repository is closed, so we are planning for total resaturation, but, of course, at that depth, there will be little oxygen present.

There will be a reducing chemical environment, because within the waste itself, there are hundreds of thousands of tons of ferrous materials, so, yes, we're well aware that those--even stainless steel, in certain conditions, can corrode, but there, in the absence of oxygen and a reducing environment, we think there's a long life for the containers.

So, with the containers taking care of the fission products, the cementitious backfill taking care of plutonium and the other actinides, the radionuclides which define safety performance seem to us to be the mobile and long-lived species, like Chlorine-36 and Iodine-129, and, over the very long term, the Uranium-238 daughter, Radium-226.

For such a system nestled down here in the volcanic rock, the key hydrogeological parameters at Sellafield have become pretty clear. They are the annual flow through the repository nearfield, the flow through this volume here, and the volume will be the high hundreds of thousands, perhaps a million cubic meters of excavated space, which will be backfilled.

Our current calculations suggest that through that volume, there will be an annual flux of about 100 cubic meters, so that's a very slow changeover rate, a very slow flushing action.

The second part of the equation that appears to be crucial is the dilution of that flow when it encounters the overlying aquifer. I had some difficulty understanding the drawings this morning until I mentally turned them upside down. Our aquifer is on top, rather than underneath, but the dilution that we believe is there from our preliminary calculations is a factor of about 1,000, which, by coincidence, seems to be DOE's current view at Yucca Mountain.

The volume of flows through the repository is very important in our case because it needs to be sufficiently low to ensure that the chemical conditioning by the backfill, the suppression of the solubility of the actinides, in particular, is not prematurely exhausted.

Flow through the repository also determines the spreading time of the source term in our modeling, which is the time taken for release of radionuclides in the water from the nearfield into the geosphere.

Together with spreading in time during transit through the geosphere, what happens to it once it has got into solution, we have determined the effective dilution of residual radionuclides which are released from the repository, and, hence, the associated radiological risk.

For a naturally evolving repository at Sellafield, leaving aside the idea of human intrusion or extraction of water from wells in the sandstone, our base case modeling, taking account of uncertainties, gives realizations generally the right side of the 10_{-6} contour in terms of individual risk.

I apologize to people at the back, because you can't see this bee swarm in here terribly well. That is some 500 probabilistic realizations, Monte Carlo simulations of different views of the site, with different views about parameter uncertainty in key respects, and for each of those cases, one can define and calculate a source term spreading time, and geosphere spreading time, and plot the outcomes for each pair of those key parameters against the safety contours, and the further out you are, the lower the risk, in general terms.

So, this is the, in red here, is the regulatory target. I would stress that that is a target. Our regulators have set out some really quite helpful guidance, stressing that you can't determine a safety case by a single number, but it's reassuring that, although that isn't a written in blood limit, the great majority of this bee swarm of outcomes lies healthily beyond 10^{-6}, round at about the 10^{-7} level, and that picture, which is for the peak risk at any time, is, of course, consistent with--oh, I seem to be going in circles here; sorry. I'm trying to go back to the slide that I jumped over.

This is a deterministic calculation rather than a probabilistic one, but it's the simplest one to understand to make our point, that it's actually Chlorine-36 which is actually hidden under the red here, which is the driving, defining nuclide which defines the safety performance through the first 10,000 years, and you're into 100,000 years before anything else makes much of a contribution. So, that's just an illustration of the fact that we are not planning for total hermetic containment. We are seeing some risk from Chlorine-36 after just a thousand years, but, obviously, at a very small level. The annual risk at that point would only be 1 in 10^{-12}.

In the longer term, it's the Radium-226 which picks up and defines the long-term safety case, so it's really chlorine here, and then, very soon, Radium-226, and there is a perspective that the chlorine is so mobile there's nothing much you can do about it except dilute it, and the Radium-226 is going to come, and over time scales of 10^6 or 10^7 years, ten million years, that's with us on the planet, and there's not a lot we can do about that, either.

An important focus of our ongoing work is obviously to tighten up our estimates of volume flow and of dilution, and validation of our models to increase our confidence in the natural discharge projections that we have; separately, the impact of intrusion by wells into the sandstone, which you will recall is being addressed. Encouragingly, even the current deterministic modeling of that case, with conservative assumptions about the nature of the wells and associated population patterns and lifestyles, gives a risk outcome well within 10^{-5}, which is within striking distance of 10^{-6}.

So, our next step, to shed light both on the conditions in the sandstone layers, and, more generally, in the deep rock to look at the evolution of a naturally evolving repository, is to build that extra confidence in our observations.

Selecting Sellafield as the repository site--which we've not yet done--that needs to be based on confidence, sufficient confidence to submit a planning application for repository development, and, also, to make applications with the UK's Environment Agency, and our Health and Safety Executive. The Environment Agency roughly parallels your EPA, and our Health and Safety Executive roughly parallels your NRC.

And, to build our confidence in the models, and our view of the site to that point needs, in our view, to be driven by access to information from an underground experimental facility, which we call the Rock Characterisation Facility. This is a site-specific underground rock lab to be developed in three phases over ten years, at a planned depth of 650 to 900 meters below sea level.

In broad concept, it's very similar to the ESF, and I found myself sympathizing with remarks this morning about the fact that one can have a good grip on a site, but it's essential to get below ground to build the safety case to the point where you can take it to the regulators. But, in our case, our focus is on testing the characteristics of the rock and of the hydrogeology in a saturated rock environment, rather than a geological setting lying above the water table.

The first phase of the RCF is the sinking of two shafts, each five meters in diameter, some 700 meters deep, and that will be done through a very closely-instrumented array of boreholes, seven or eight boreholes within a few tens of meters of each other, because that will be a very important drawdown experiment, enabling us to see how the water actually flows initially through the sandstone layers, and then, once we get into it, how the water actually flows in response to this pressure differential when we're in the volcanic rock.

There will then be two further phases. Phase 2, in the red color here, is driving roadways, fairly small, by drill and blast, rather than a tunnel-boring machine, and then the third stage will take that out further so that we have a kilometer-long total array of galleries for access by our scientists, and, down there, we will be doing many of the things that we heard about from the Experimental Studies

Facility.

We have, in that Phase 1, a drawdown experiment which we can follow. In the later phases, there will be lateral drilling, pressure testing between boreholes drilled from the facility, tracer tests, and so on. There will be a step change in our confidence levels from that first phase, which will run for about four years, from the point where we can start doing it.

On that basis, and assuming we get a conclusion on our current planning application in the course of next year, we could have a repository in operation by 2012, but we may need to take longer.

To our regret, in December of '94, our request for planning permission for the RCF as an exploratory and research facility was refused by the local planning authority, the Cumbria County Council mentioned in the introduction. This refusal was despite significant local support for the RCF as a research facility from the general public. We are now, as I said, into Day 50 of hearings, which should go on for another month or so. They are conducted by a government inspector under our standard zoning laws, our Town and Country Planning Act.

That statute does, however, have considerable flexibility, and the Secretary of State for the Environment has published advice to the inspector which has allowed a thorough debate of relevant issues, including the emerging safety performance, the emerging safety assessment for a repository at the site.

We have been able to set out that emerging safety assessment, taking account of the encouraging scientific results which I have explained in very summary terms. We've also been able to report to that planning inquiry the strong support for the RCF approach which we've had from our Royal Society and from our Radioactive Waste Management Advisory Committee, the RWMAC Committee, which is roughly analogous to the Board itself.

Witnesses appearing for the objecting parties, including Environment Resources Management, a U.S.-based firm, which has supplied witnesses for the County Council, and various academics for Greenpeace and Friends of the Earth, have, through the inquiry process, been able to set out their counter views about the promise of the site. In many cases, these have not been set in a coherent, probabilistic safety assessment framework. We've had lots of taxonomic discussion of the geology, emphasis on its complexity, without anyone--or with few of those witnesses being ready to come down, or reach out from that specialism to debate what that may mean or what it may not mean in terms of bottom line safety performance.

We judge that the public perception of our science case is emerging strengthened from the inquiry process. We didn't seek the inquiry. Its cost, taking account of our interest charges this year, will be close to $80 million, but it is proving to be a good opportunity to expose some of the poor science that's been ranged against us, and to raise public awareness of the high quality of our own work.

By and large, sensationalism has been avoided through the inquiry process. The disciplines of having to submit evidence in advance, in writing, has enabled some of the wilder claims about earthquake risk, and so on, to be subject to searching cross-examination, and to rebuttal evidence.

Aside from the supposed unsuitability of the Sellafield site, a primary focus of some objectors, particularly the County Council, has been the basis of my company's historical decisions to investigate from amongst a list of 12 sites evolved in the late eighties, to investigate first two sites, Dounreay in Scotland, and Sellafield, from the short list of 12, and to have chosen those because there was a degree of support for the nuclear industry in those localities.

In evidence, the company has been quite open about the basis for its decisions, but as all the 12 sites were assessed to have the ability to meet the tight 10^{-6} target, it was legitimate and reasonable to take account of local understanding and support.

We've also reaffirmed the importance and relevance of cost considerations as a matter to be given due weight in site choice, providing the safety requirements can be met.

There has, I know, historically, in the U.S., been a debate about the approach to site selection, and, indeed, much of our work in the 1980s followed U.S. examples in using multi-attribute decision analysis to rank siting opportunities, but, at the end of the day, what we have done, we believe--and this has gone unchallenged in the inquiry--fully meets the International Atomic Energy Agency guidelines, and I think that issue is now being seen in a much more mature context than it was by some commentators before the inquiry opened.

More generally, we have revealed summary information about all the 12 sites across 30 different attributes, including our specific desk-based analysis of their safety performance, and I think that's demonstrated our commitment to openness.

I note that tomorrow there is to be some discussion of expert judgment and its place in probabilistic safety analysis. That issue has come up in our inquiry in the UK. Objectors have naturally sought to emphasize the fuzziness of some of the judgments used in setting up probability distribution functions for various site parameters, and also, aside from the data uncertainty, the model uncertainty involved in drawing up a model for behavior through hundreds of thousands of years.

But, my impression is that this issue has not really taken off as a big deal, colloquially, in England. Most of those who follow our affairs, and many of the witnesses at the inquiry are geologists who come from a discipline which, by definition, almost, has to accept the necessity and the unavoidability of expert judgment, but we've been able to set out clearly how we go about moderating and organizing the process of expert judgment elicitation.

I've circulated materials in advance which some of you, I'm sure, will have had an opportunity to look at, so I don't think that is currently, in the UK, a major issue. There is a, I think, a share perception, certainly, between ourselves and the regulators reflected in the report by Professor Watson of Cambridge University, prepared back in '92, which set in place the Sandia Labs approach to the same issue.

Before finishing, and leaving that subject, I should just mention that we've had an interesting debate in the UK through 1995, not just on the general policy, not just on whether we should be allowed to proceed with this experimental facility, but with two other areas, which I could cover in questions.

Firstly, the appropriate regulatory guidance about what an acceptable safety case for deep disposal should encompass. I've mentioned the 10^{-6} risk target, and that assessment of performance against it is recognized as very important. It's been confirmed recently that that should be done in terms of expected values of outcomes, and I think that the National Academy of Sciences report on that matter, which is something we've closely followed in the UK, has been quite influential.

In addition to the 10^{-6} target being explained a little more clearly, the regulators have said a great deal, in generally sensible terms, as we would perceive it, about the impossibility and the danger of being drawn into a debate simply about numbers. One's got to have a multidimensional safety case, which, certainly, through the longer time periods, looks at other comparatives, natural radiation, and so on, as well as performance in relation to a risk target.

Generally, we detect some convergence between UK thinking and the National Academy of Sciences approach. We'll be interested to see how the U.S. regulators pick up that report.

The second issue which has come up, which I mention because I think it may be of some interest in the U.S. context, is whether a more prescriptive approach should be taken in future in UK practice on site selection.

I mentioned that NIREX is a private company, broadly can take its own decisions, which have to be rational and sensible, and so on, but we do not have a statutorily-driven process for how we go about site selection. There was a government-appointed study group report in early '95, which recommended a somewhat different approach. In particular, it recommended consideration of quantitative hydrogeological indices to rank sites on a desk-based basis, to give safety even above and beyond the 10^{-6} level, to give that a greater weight, with cost and other socioeconomic factors not taken into account until a later stage.

The idea was, also, that final site selection should be done by government, rather than the repository developer, and that there should be a multiplicity of possible sites announced, and extensive public consultation in each area. All that would have been overseen by a new "Commission" to see this process carried through.

In the White Paper, perhaps not fancying the idea of making itself responsible for nominating the site, the government did not retrofit any such approach to the NIREX program, but it did indicate that aspects of the study group's thinking should be borne in mind in future in selecting a site for high level waste disposal. So, that's another issue which we could cover in questions, if it's of interest.

So, that, Mr. Chairman, is a somewhat breathless account of where we've got to in the UK over the last 18 months. Obviously, there's a lot of science underlying those summary curves that I've shown you. Thanks for your attention, and I'd be very happy to take questions.

DR. BREWER: Thank you, Mr. Folger. Thank you very much.

Are there questions from colleagues on the Board? Don Langmuir?

DR. LANGMUIR: Michael, thank you for the opportunity to hear this and be updated. I was over there last spring and gave a talk for the Board at a meeting in which NIREX described their program, and I see some changes and some developments since that time. I'd be curious to have your thoughts on them.

Can you find the first slide, which was the Sellafield--it may not be the first slide, actually; the one that showed the Sellafield cross-section, with the proposed repository shown on it.

MR. FOLGER: Sure.

DR. LANGMUIR: I was amused by your observation that this was an upside down Yucca Mountain, perhaps, going from unsaturated to saturated.

At the time I was there listening to discussions of the site, there was concern that the fracture zone shown on the illustration coming up through the tuff might potentially conduct flow upward from below, and in the eyes of the objectors, make the site unsuitable, and you pointed out that that's been acknowledged as going on in this case, and that there's dilution with flows moving towards the down dip, towards the ocean, with dilutions of perhaps one to a thousand, this sort of thing.

Another objection at that time, among those from the environmental groups, was that perhaps the concentrations radionuclides, even if they were being diluted, might be a problem in the shallower horizons, with the uprising of potential fluids from a repository.

I just wonder where you've all come with regard to that concern at this time.

MR. FOLGER: Well, to answer that in two parts, yes, some of the faults don't come into the sandstone, some do. In general, across the site, when we put our boreholes down, we find that the flowing features are not the fault zones. The fault zones are well-mineralized because the faults haven't moved for many tens of millions of years. But, to have any kind of intrusion which has a differential conductivity compared with the adjacent material can give you, as it were, a kind of ruling effect, so that flows contract up it.

The kind of path lines that we generate show--and, of course, these are for tiny flows, you understand. I mentioned perhaps 100 cubic meters through hundreds of thousands per annum--that they will tend to move up and follow some of these dislocating features.

When they get into the sandstone, there's a kind of refraction effect that I'm sure you're well aware of. It's a little like the sine ratio equation for refraction of light, that when a flow moves from a denser medium into a less dense medium, it's refracted, so we get flow paths, for some cases, which do come up, and then run out this way, relatively close to the surface, but perhaps 200 meters, which is a lot of material.

And, our rule of thumb, historically, has been that we want to be a minimum of 200 meters below the surface. Here, because the sandstone isn't terribly suitable as a repository medium, we're 650-700 meters deep, but I don't think we want to be too exorcised by the fact that some of the output, through long historical time, could come within 200 meters. That's still a long distance, and, as I mentioned, our deterministic evaluation of agricultural wells, which might run, perhaps, 50 meters deep, show that even with very conservative assumptions, we do not have a significant issue there. There are conservatisms there which we believe we can relax through time.

DR. BREWER: Okay. Are there other questions from the

Board, colleagues? John Cantlon?

DR. CANTLON: Yes. You were commenting about the resistance to the siting of your Rock Lab, and, as I recall from our visit, there was some kind of an agreement that that site itself where the Rock Lab was could not be a repository site? Am I recalling correctly, or is there no such understanding?

MR. FOLGER: No. The terms of the planning permission, the zoning approval that we get for it will allow it to be used only for research and experimental purposes. There is a whole separate procedure, which, perhaps, I should have mentioned, which is that once we get the Rock Lab built, and once, as we expect, but we can't be sure, once it begins to validate what we've found from our surface investigations, and we've, therefore, got a strong enough safety assessment to take to the regulators, at that stage, there will then be, as Sadam Hussein would say, the mother of all inquiries, which will look at the zoning issues and the safety issues, all in one giant procedure, which will go on for a total period of three years before we get an answer.

So, no, we are not saying that this Rock Lab site is excluded from consideration. There are countries that have had rock labs on that basis, and Canada is one, Switzerland another, Sweden, sort of, but if you press the Swedes, it is entirely possible that they may propose a repository within a few kilometers of their lab.

DR. CORDING: Ed Cording.

I was interested in what you see--and you indicated the three years time following the exploratory site to make some decisions about making it a repository, that three-year period.

What do you see in terms of a time for going through a process of, say, a licensing, to, say, approval to build a repository? Do you have a time? Is that being kept open and flexible?

MR. FOLGER: Yes. Basically, we are, you know, we are financing this thing by prepayments and loans from our principal users, so time is money for us, and there's actually an interest charge on our income statement every year, so we have a real discipline not to take more time than we need to.

But, in broad terms, starting from today, if we get approval for the Rock Laboratory by mid-'97, that's the middle of next year, then within about five years, we would have actually dug those shafts and completed Phase 1 of our investigations.

Our assumption, backed by a lot of outside advice, not only to us, but to the regulators, is that Phase 1 will give us enough confirmatory data to make an application for development approval under the zoning laws, and an initial application for licensing from the safety authorities.

There will then be a three-year period for this great public inquiry, and then there will be seven years of construction, so we're talking about, five plus three plus seven is fifteen, so 1997, plus 15, gives us 2012 as the implied date for going into business. Five years for Phase 1, three years, then, for the inquiry, seven years to build the facility.

I might say that there will, of course, be continuing activity in Phases 2 and 3, which we need to do anyway, to build our final safety case, to get our actual license loosened for all applicable conditions in 2010, 2011.

DR. BREWER: Leon Reiter of Staff.

DR. REITER: It's unfortunate that we don't have the other discussion today, which had to do with the National Academy report and proposed standard.

One of the big issues of contention among some people is the time period that's being proposed. As you're well aware, we've been looking at 10,000 years, and, indeed, there are bills in Congress which would also stipulate that as the period, but the National Academy said, "Let's look out to periods up to a million years, when peak dose occurs," and I notice that you show plots out to 10 million years, at least 10 million years.

Could you give us your perspective on, at least in your thinking, with respect to the UK, about how long should this period of concern be? Should we give equal emphasis to different time periods, and the difficulty of computing these kinds of things, the long time frames.

MR. FOLGER: Well, I think the first thing to say is that at the political level, the politicians have made a sort of moral judgment that future generations are just as important as current generations, and that's all to do with this sustainable development principle, which is really coming to bear in the UK. So, in principle, we are looking to protect future generations to the same standards as today's generation, and I think that's why the politicians are saying it's an open-ended commitment.

And when you say to them, "Well, you know, how many super novas have we got to model?", and all of this, their eyes glaze over, so the answer to it is a fairly pragmatic one, perhaps, which is that the 10^{-6} target is an aspiration that you have to show you're working for, for all time, and, certainly, there will be, you know, a big upset if everything ran along fine and dandy for 100,000 years, or a million years, and then suddenly shot off to give a very bad outcome.

But the regulators, I think, have done quite a good job in their latest consultative document, which I circulated, in recognizing that the kind of proof you can offer, the kind of evidence you can adduce in these different time periods is very different, so we don't think that we would be relying on probabilistic safety assessment numbers in the same way after 100,000 years as we would after the 10,000 years, and that beyond a million years, as I think I mentioned, it's recognized that one can start to appeal to natural levels of radioactivity, and how far you're adding to those.

So, that's a kind of a fudgy answer, but I think our regulators and politicians have determined to establish the principle that the future matters just as much as today, but recognizing the practical limitations on that.

DR. BREWER: Mr. Folger, thank you very, very much. We now are going to move on to our next speaker in the international segment of the program.

Our next speaker is Dr. Ju Wang from the People's Republic of China. The Chinese Nuclear Power Program is relatively new and still small, but expanding rapidly. At the Beijing Research Institute of Geology, where Ju Wang is Vice Director, those involved have recently begun a search for a site for a repository to support the Chinese Nuclear Power Program.

We understand that the Chinese are considering a potential site in the Gobi Desert that has many similarities to the Yucca Mountain site, including an arid climate, a potentially very deep water table, and seismic activity.

Ju Wang is here today to provide us with an update on developments in the Chinese Repository Development Program. With a staff of 19, he's directing the site characterization program. This follows on his work as a member of the coordinating expert group for the deep geologic disposal of high level radioactive waste in China. His background is in geosciences at the undergraduate level, with a Master of Science and a Ph.D. in geochemistry.

I want both to welcome Dr. Ju Wang, and also to thank him very much for the courtesies extended to Don Langmuir on a recent trip to China.

DR. JU WANG: Thank you, Mr. Chairman.

It's my good pleasure to have the opportunity to be here to report the latest progress for China's geological disposal of high level radioactive waste, and I should take this opportunity to express my sincere thanks to the Board members, and also to staff members of the Board, and also special thanks should be given to Ms. Paula Alford for all of her effort for the issue of my visa during the shutdown of the U.S. Embassy in Beijing. Now, back to my topic.

I'm the speaker on behalf of my colleagues working for the China National Nuclear Corporation, so we'll talk about deep geological disposal of high level radioactive waste in China, and my talk will be in these parts:

At first, the introduction, and also, the organizational structure, and the third will be the DGD Program, means the Deep Geological Disposal Program. The fourth will be the progress in site selection, and the fifth will be the special area for the preselected area that is Beishan Area, Gansu Province, Northwest China, and, also, I will mention some other studies which have been conducted, and the last two are the summary.

Now, first, I will have a very brief introduction to my talk. China, as other countries, is also facing the problem of how to safely dispose of the nuclear waste. China's nuclear industry was firstly established in 1955, and, since then, a lot of liquid high level nuclear waste have been stored, and all of them have been stored in the stainless canisters, and they are waiting for vitrification.

During the recent years, China has developed the nuclear power plant, and now, on the Chinese mainland, we have two nuclear power plants. The first is in the Guangdong province, southern China. It's called the Daya Bay nuclear power plant, and the second is in the Qinshan nuclear power plant near Shanghai. It's the number one industry city of China.

During the next five years, totally, eight reactors will be built, and this first slide shows the location of the nuclear power plant here. This is the Liaoning nuclear power plant, and this will use the reactor from Russia, and, also, this is at Qinshan, and, also, all of the reactor will be built by the Chinese, Chinese engineers. And, also, there is another one from Lin'ao, very close to the Daya Bay nuclear power plant, and also, this maybe we'll build with a corporation with the French scientists.

And, as this transparency show you, the total capacity, and, also, the nuclear power plant on the Chinese mainland, and this Qinshan nuclear power plant is still in operation. It works very well, and, also, this is in operation. During the next five-year plan, totally, four new nuclear power plant will be built, and this is the capacity, so a total capacity will reach about 20,000 MW by the year of 2010. So much for the introduction.

Now, I will talk about the organization, organizational structure for China's Nuclear Waste Program. Now, this table, I'm sure, is the organizational structure. Now, all of the activities related to nuclear industry is managed by the China National Nuclear Corporation. The abbreviation is CNNC, and this is a big corporation. It has about, staff members, over 267 staff members, and all of the nuclear fuel cycle, all of the activity is related--is responsible by this company and for the nuclear waste disposal.

Totally, there are four bureaus involved in; that is, the Bureau of Planning, Bureau of Nuclear Fuels, Bureau of Science and Technology, and Bureau of Safety, Protection and Health, and this corporation is supervised by China Environmental Protection Bureau and the China National Nuclear Safety Bureau.

Last year, a corporation was built called Everclean Environmental Engineering Corporation. This is a special corporation, is very similar to SKB in Sweden, which is in charge of the site selection, site characterization, repository operation, design, closure and monitoring, but, at present, most of the effort involved are in the low and the intermediate level waste.

And then under these four bureaus and the corporation, we have a Coordination Expert Group for the geological disposal of high level waste. At present, this group is in charge of research and development program, siting and the site characterization, repository design, construction, and environmental assessment and some related fields.

For the Coordination Expert Group, as we know, the nuclear waste disposal is related to all kinds of scientific things, and nobody can solve this problem by themselves within his research field, so this Coordination Expert Group, the experts come from different institute. The first is the Beijing Research Institute of Geology, which I work for, and the second is the Beijing Institute of Nuclear Engineering.

The third is China Institute of Atomic Energy, and the fourth is the China Institute for Radiation Protection. They have different responsibilities.

From my institute, we are responsible for site selection, site characterization, for geology, geochemistry, and the nuclide migration. For the Beijing Institute of Nuclear Engineering, they are responsible for the repository design, repository construction, performance assessment. For the China Institute of Atomic Energy, they are responsible for nuclide migration and environmental assessment, and the China Institute for Radiation Protection is responsible for the safety analysis.

Now I will talk about the DGD program. In 1985, the China National Nuclear Corporation worked out a program called Deep Geological Disposal Program, and this program is divided into four phases. During the Phase 1, the site selection and site characterization will be done, and during the Phase 2, about 30 years later, it will be then repository design, and Phase 3 will be the repository construction. Phase 4 is the repository operation.

And now we are in the first one, and between 1986, we did nationwide screening for the site selection, and during this stage, we have selected five areas for the high level waste repository. Between 1986 and 1988, we selected some district within the five regions we had selected for further studies.

Since 1989, most of our efforts have been concentrated on the Northwest China. That means the fifth district we are doing work on.

I'll show the site which has been selected. This mark shows the preselected area during the first--between 1986 and 1988. This is Southwest China. The second area is Southern China. The fourth is Eastern China, and the third is Inner Mongolia. The fifth is the Northwest Gansu province of China.

Considering the rapid economic development along the coast areas, maybe number two and number four will not become selected, because there are a lot of population and a lot of industry, and a very good economical potential, and all efforts have been concentrated in this area.

Before I talk about the preselected region, I should talk about the siting criteria for high level waste. That included two factors, socioeconomical factors and the natural factors, and we have considered that the distribution of nuclear industry in China, and the animal, the plant resources, and the potential mineral resources, and, also, the attitude of the public and of the local government, the requirement of national environmental protection laws, and also the feasibility for construction and the operation of the repository.

There are a lot of factors, but we have considered that the most important is the distribution of the nuclear industry in China, and also, the economic potential in this country, and, also, the potential mineral and the animal and the plant resources. These are the most important social factors. And then there are natural factors, also; natural geography, including topography, climate, hydrology, and, also, the geology, including crustal stability, earthquakes, active faults, and others, and crustal stress, crustal thermal flow, host rock type, hydrogeology, and engineering geology, and we are at the beginning stage of the site characterization and site selection, so we have considered that crustal stability is a very important factor for the selection. If an area is not stable, of course, we will not consider it.

Let me talk about the progress in site selection, which I think I have mentioned about that. Now, during the regional screening, these five regions have been selected, and now we are in the period of district screening. Since 1989, all effort has been concentrated in this area.

This is a geological map showing the Beishan area, Gansu province. This area has been selected as a potential area, and let me show, this railway connected Zhejiang province, and to the central China, and also to the coastal area of Xingjiang Port. This railway will go to Amsterdam of

Holland. This is called the Mainland Breach.

Now, this is a corridor area, which is--and north of there, there are some oil in this area here out of this map, and you can see we have selected the granite as a potential host rock for our repository, and this is a geological, this is--and over here there are some granite distributed.

In this area, we have selected six districts, one in this granite, called the Yumenzhen area, and, also, this is the Changchum district, and also here, Qianhongquan and Jiujin, and also here. This is another area.

The work we have done is to the crustal stability for this area, and, also, we have done something about activity, activation of this big fault, and, fortunately, all of the data is sheer zones, and later, there are some fragile movement, but it seems the quaternary, these faults are lucky enough, they have not fissures to showing the movement of these faults.

This map shows the Moho, Monrovich discontinuity iso-depth contour map of the Northwest Gansu Province, and this is the location of our preselected area. This is near Qilian Mountain, which is a very active area, with a lot of earthquakes, and, also, that area is a still uplifting area.

Also, there's a regional magnetic anomaly map of Northwest Gansu Province, China, and this is the location of our preselected area.

This map shows the distribution of seismic center in Northwest Gansu Province. Now, China has a history of over 2,000 years, you can find some earthquake recording in the historical files, so we have some good historical recording there, recent earthquakes. These are earthquakes larger than seven, but in the selected area, there never have been no earthquake larger than three have happened in that area.

This is the seismic zoning of the Northwest Gansu Province, and this is the location of the preselected area, and then this map shows the big earthquakes which have happened during the past 1,000 years.

This map shows the regions of neotectonics, and totally, in that area, it can be divided into three parts, and the first part, this part is Qilian Mountain, intensely uplifting region. Now, this area is still a uplifting area, and a lot of active faults are distributed in this area.

This is a corridor depression region. This is just a depression area, and, see, this makes transportation, I mean, some railways and the highways go through along this area. Without this depression area, there's no access to the Xingjiang provinces, and this shows the Beishan slightly uplifting region. The uplift rate is about .6 to .8 mm/yr.

This map shows the classification of crustal stability of that area, and we are considering that this area is very stable, and this is a sub-stable region, and this is a unstable region, down to the Qilian Mountain areas. And within this stable area, also, according to the distribution of fault, we have classified. This area is divided into several sub-areas for further work.

This map shows one of the granite which has been--which some work have been done for this granite. One of the work is the activity of this fault, and also, the integrity of the rocks of this granite, and also, the fissure distribution, and also, the structures in that area.

Along this fault, we have done some geophysical investigation, and this is a cross section of delta T by high resolution magnetic survey. This figure shows that existence of the fault, and also, this is a, from this to the right is the distribution of granite, and from this to the left is Pre-Cambrian metamorphic rocks. And also, this map by EMAP survey, shows the existence of the fault here and here. From this over, the area is granite.

Now, let me show some slide of this area. Don't you think it's Yucca Mountain? The main shape is very similar to that, and there are no habitations there, and the precipitation is about 6 mm/yr, and, also, the evaporation reaches 3,000 mm/yr, and this shows a fault which has been showed in this way, the fault. And, also, these are metamorphic rocks, and this is the granite.

This is the main shape of the granite, good expression of the rocks, and also is, I think is a paradise for the geologists, not for the citizens. And, also, this shows some of the rocks, metamorphic rocks, metamorphic rocks south to the area.

This shows a small fault, a small northeast fault within the granite.

These are some fissures of the metamorphic rocks along the sheer zone. It's the east/west tracking sheer zone.

Fortunately, we have found something funny in the desert, in the Gobi Desert. Sometimes, we can find some flowers there, but these can be eaten, is good for your eating during lunch in the field.

Do you want to endure more?

(Laughter.)

DR. JU WANG: Other studies has been conducted. Except for the site selection and the characterization, we have done some others, also at the very preliminary stage. They are site selection for underground research laboratory. Because of the lack of money, the construction for underground research laboratory has been postponed.

Also, some experiment on radionuclide migration, insitu, and also in laboratories; and a study on natural analogies; study on buffer and the backfill materials and their geotechnics; study on the speciation of transuranic elements in solutions; study on heater test; and also, a study on models for safety and environmental assessment.

Well, I will come to the last part of my remarks. The safe disposal of high level waste is a worldwide challenging task. Although China has made much progress in this field, still, I think there is a long way to go. For example, a policy act related to nuclear waste disposal should be established. Up to now, we don't have any Nuclear Waste Act established, and, also, a more effective organization should be formed to promote the related work, although we have a expert group, but this group don't have much administration power or responsibility for the--don't have much power to control the money, and the money also changes.

And, also, we want to find a way to raise enough money for the safe disposal of nuclear waste. You know, in the cycle of nuclear fuel, nuclear waste disposal is the end of the circle, and, also, because China is developing very, very quickly, all effort has been concentrated on the head of the circle, nuclear power plant, and, also, some of the operators of the nuclear power plant don't want to pay money for the waste.

And, also, we have a shortage of world-trained scientists, and, also, we are seeking international cooperation, international effort to help us to train our scientists, to send them abroad and to learn some experience from other countries, and I think the information exchange is very important for the safe disposal of radwaste.

China is willing to learn the successful experiences from other countries; for example, United States, United Kingdom, also Sweden. Also, we are willing to strengthen international cooperation, and China is also willing to share its own experiences and achievements with other countries, only for the purpose of protecting the living environment of the human beings, and also protecting the earth, and, also, we are at the beginning stage of high level waste disposal, and, also, I'm sure there are a lot of commercial opportunities, so thank you for your attention, and also, I will be very glad if you have any comment on our program, and any suggestions for our program.

Thank you.

DR. BREWER: Thank you, Dr. Ju Wang.

Are there questions from the Board colleagues? Don Langmuir?

DR. LANGMUIR: Ju Wang, when I was over there, we talked about the repository horizons you were considering in this proposed area, and although I think you were introduced as--the statement was made that this is much like Yucca Mountain, my recollection is that we were looking at groundwater analyses from the repository horizon; in other words, that this was, at least at the moment, you were thinking about a saturated zone system in the granites, for a variety of reasons.

Could you support me on that, or disagree with me, or explain what your thinking is right now in terms of where the repository might be?

DR. JU WANG: I think this repository will sure be in the saturated zone, and, also, I haven't caught your meaning of everything. I haven't caught your meaning of other things.

DR. LANGMUIR: Well, I can recall the analyses of waters that I was shown that were from the potential repository horizon were saline, and I asked the question, why? You know, we would have all thought--we said, "This is wonderful. You've got a big desert out there. Why don't you stick it in the Gobi Desert?" And I think you gave some reasons why that wasn't appropriate; that it was not easy to get there, the transportation, the ability to maintain and access a repository in the Gobi Desert was not a good option.

DR. JU WANG: For transportation, yeah.

DR. LANGMUIR: This is what I can recall being told.

I didn't quite catch the precipitation amounts that you suggested you had. Was it 60 mm/yr or 6 mm/yr?

DR. JU WANG: Oh, let me recall; about 60 mm/yr.

DR. LANGMUIR: So, climatically, it's not different, not too different, even drier than Yucca Mountain.

DR. JU WANG: Very dry, but we have found some surface water there.

DR. LANGMUIR: How deep is the potential repository horizon that you're thinking of?

DR. JU WANG: It will be about 500 meters down to the, yeah, the depths will be, and also, the conceptual design will be the shaft tunnel model, and also, the waste will be the vitrified waste after reprocessing.

DR. BREWER: Okay. Clarence Allen?

DR. ALLEN: Clarence Allen, Board.

You said that one of your two most important criteria for locating the site was its relationship to places where nuclear waste was being produced, and, yet, now you have picked as your principal site one that is thousands of miles away, and I was wondering why, although I realize the same question could be asked in this country, with maybe the same answers.

DR. JU WANG: In the United States, you have all of your nuclear power plant in the east, and, also, you put your repository in the west, but in China, we have some nuclear facility in the west.

DR. LANGMUIR: Isn't the answer related to the fact that you have nuclear test work that was done in China, the nuclear bombs and experimental work with contamination resulting from it is very close to the site you're proposing?

DR. JU WANG: That's correct. You mean the nuclear test site is close to this area?

DR. LANGMUIR: Yes. Isn't that close to this area?

DR. JU WANG: No, no. About--let me calculate, about 1,000 or several hundred meters, kilometers from this site.

DR. BREWER: Okay. John Cantlon, did you have a question?

DR. CANTLON: Yes. Do you have anything equivalent to England's Greenpeace opposition party as your antinuclear community?

DR. JU WANG: No. Until now, I haven't heard about that, but some local people, yes. No, the public knows very few about that, but, of course, as the construction goes down, we will have to publish it to let the public know that, but during the People's Congress, you can hear some objection to it, as in for the construction of this repository, of course, we can hear some party who oppose this.

DR. BREWER: Don Langmuir's got another question.

DR. LANGMUIR: You said something in passing that I thought was intriguing, that you knew for certain, was my implication, that there had been no earthquakes for a thousand years out there. This tells me there's a record somehow, somewhere that's been kept that's at least that old in writing in China. Does this apply to earthquakes and volcanos and all the other potential--Clarence Allen is nodding his head that he knows.

DR. JU WANG: And, you know, is there any other geological hazard in that area, you mean recent volcanism, is that your meaning?

DR. LANGMUIR: Well, no. I guess the point was that there is evidently a record in China, written record that's fairly exact of past events of this kind.

DR. JU WANG: Yes. May Dr. Allen knows that. We have a huge book for the recordings of the total earthquakes.

DR. ALLEN: This is Clarence Allen.

We know more about some Chinese earthquakes that occurred 2,000 years ago than we know about earthquakes in California in the 1920, 1950 range. The record is truly elite, and this particular area has long been the corridor for this.

DR. BREWER: Okay. Other questions from Board or staff?

(No audible response.)

DR. BREWER: Dr. Ju Wang, thank you very much for a most informative presentation. We appreciate it very much.

DR. JU WANG: Thank you very much.

DR. BREWER: What I'd like to propose is a very quick break. Everyone get a cup of coffee or do whatever else they have to do. We'll start at four o'clock, promptly.

(Whereupon, a brief recess was taken.)

DR. BREWER: What I am proposing to do here is to reopen the conversation that was going reasonably well just before lunch--Russ, please join us at the front table--by way of leaving open any kinds of questions from Board colleagues or staff directed to any of the people making presentations, or their surrogates, so, I think I'd like to start, basically, with Russ Dyer, who indicated he wanted to spend a couple of minutes just making general comments, and then we'll take it from there.

I'm going to stay here for the purposes of kind of directing traffic. We will do this for about 20 minutes. We have two members of the public who have indicated that they wanted to make statements or ask questions. We'll try to limit that to about five minutes apiece, and, with good planning and a strong hand, we should be finished around 4:30, 4:35. That's the plan.

Okay, Russ.

DR. DYER: Thank you, Dr. Brewer.

What I'd like to do--and it's with a certain amount of trepidation here. I may be pouring gasoline on dying embers, but I'd like to address some of Dr. Langmuir's earlier questions and comments.

Namely, we were talking about the viability assessment, and there seemed to be--maybe there's a tactical error here by not having the word "science" explicitly listed in the list of things that need to be accomplished for what our definition of the viability assessment is, but implicit in there, under the Total System Performance Assessment, is that there is a credible technical basis that is developed, and, of course, that's where the science program contributes that technical basis.

Now, the challenge, of course, is taking the information from the science program, abstracting the relevant and appropriate information out of there to create the models for the performance assessment, and that's going to be a real challenge that we have in front of us.

You also asked a question about what the relationship between the TSPA-95 and prioritization in the program is, what we're doing in the way of science. There is a core science program, even under the diminished program that we are currently embarked on. There is a core science program that we are committed to pursue, and it is driven, in large part, by a evolutionary understanding of what's important about the characteristics and processes out at Yucca Mountain.

Many of the things, as Dennis said earlier, many of our early ideas and understandings seem to be confirmed. There are some other ideas, some other areas for which we still have uncertainties. Now, are those uncertainties important uncertainties? What are the important uncertainties that we really need to address, that we really need to put our resources on?

And, we have used, I think, performance assessment to help us with that. We've been using the waste isolation strategy, at least in its early formative stages, to help us try to understand what is the short list of assumptions and hypotheses that really need to be tested to understand whether or not this is a viable system, and there is absolutely a component of science that must stand behind this thing called the viability assessment.

I didn't want to leave you with the, perhaps the impression that there was not any component of science within this thing.

DR. BREWER: Okay, Russ, thank you.

Start with Ed Cording.

DR. CORDING: Yes. Russ, in some discussions, we've heard that it's basically a matter of summarizing what information you have. A lot of information's been collected on the science on the exploration, but I have a feeling that, you know, a strong feeling that there's much more that's being obtained and that's achievable than perhaps you're willing to commit to at the present time, saying that, you know, we will have this other information, but with the sort of progress you're making at this point, and the efficiencies you're achieving, and with some reasonable level of funding that's even a reduced level, but something that, at least, is continuing, it would seem to me that you're in a situation where you're going to obtain a lot more--you could obtain a lot more and there'll be a lot that could support, that is more than, at present, management is willing to commit as part of this viability decision.

That's my impression from what I'm seeing. You're almost breaking through to a large amount of information, and no new data.

DR. DYER: Yes. Certainly, the rate at which we have been gathering information, say, for the past, oh, six to twelve months has been really impressive. I hope we can keep that up. That may be serendipitous, but one of the critical things that we have to do, of course, is convert all the data into knowledge, and that takes time.

We can either spend a lot of resources in acquiring new data, or we can take a very careful look at all the data that we have, use it to refine our state of understanding, and to steer the subsequent program.

DR. BREWER: Okay. Clarence Allen?

DR. ALLEN: Yeah, Clarence Allen.

Do I understand it correctly, then, Russ, that the viability assessment is very, very different from the determination of suitability, or the investment decision? In both of those cases, you made a decision. You made a decision that the site was suitable, or an investment decision.

In this case, you're simply setting out a series of milestones that reflect the present economic status of the program that you think will most logically carry it towards the final end here, but at the end of--it's not a decision. It's a series of milestones; am I right?

DR. DYER: I don't think it's a decision, unless it results in a negative decision. I mean, it is a series of things that you would do along the way to an ultimate license application. We have a lot of information that we have acquired in the past that has formed the basis for the program as we know it. Now, we laid out a series of things in the program plan. Now we're--the program plan has not been accepted by our sponsors, at least it hasn't been funded, so we put in place this interim program, and it is--it's not quite everything we wanted it to be, but it is certainly, we think, a positive step forward toward where we need to be eventually.

DR. BREWER: Any follow-up, Don Langmuir?

DR. LANGMUIR: I guess I can take blame for what's going on here a little bit, and I'd like to get back in it again.

My short thoughts on this start with, I think you're underselling your own program. That was the feeling I got from the speech this morning that Wes was reading, that Dan wrote, that I think we, as a group, and the Board feel you've done a terrific job getting where you are, especially in the last few years, and that real progress has been made in the program getting to a point where you could, with some confidence, argue that you have a site that, in all probability, could be licensed at some future date, and that you could have some confidence in making that statement.

That's what I expected to hear this morning as a kickoff to this session, and I think viability as an approach to '98 is a retrenchment. It's a shortchanging of the program. It's a backing off from where you've really gotten yourselves to, as evidenced by the speeches and the presentations that followed the introduction, and I, you know, I think it wasn't in the flavor of what we were--we enjoyed hearing, and I think learned further about the program today in terms of the various facets that were being accomplished, and the--Jean Younker's statements about the--I forget what you call it--the waste isolation strategy, the dating that's been going on, at depth, which I find is bringing to me some closure on, and some confidence in the site characterization program.

For example, a lot of this stuff is really terrific progress. I didn't get the sense that management agreed it was, and I think if you're going to have a program that continues beyond '96, you have to agree it is, and you've got to support your own program, and sell it well, and I didn't see that, and that's, I guess, where I came from this morning.

DR. DYER: Okay. Obviously, the technical progress that we've made, I think, has been really great, some of the things you've seen here. We've waited years for some of this information to come out, and now we're beginning to see it, and we haven't seen any big surprises.

Remember that when we structured this--and it was done in a real hurry, whenever Congress came back with some information for us--what we put in place was a program that was worth doing, would get us somewhere, and could serve, at that time, we were thinking, well, perhaps this program will not survive, in which case we need, essentially, to go through a--wrap up all the information into a body of knowledge that could be, perhaps, used by somebody else, or as a follow-on project, or it could be used, essentially, to demonstrate what we know and what we don't know, because I think, really, what we're--what we need to demonstrate, what we need to tell Congress is both sides of this, what we do know and what we don't know, and I think we know a lot more than we thought we knew, or were willing to perhaps admit we knew, because we were being very conservative.

DR. BREWER: Okay. In the interest of sharing the wealth here, does anyone on the Board have a question for anyone but Russ Dyer? I'm trying to take you off the hook if I can, and, if not, ask him another question.

John?

DR. CANTLON: Since he's the head honcho, I think he's the right guy to field them.

DR. BREWER: Russ, I was trying to be polite.

DR. DYER: Thank you, sir.

(Laughter.)

DR. CANTLON: Let me make an observation, in part, an interpretation of how, at least, I think of the rationale for what has been a kind of a disappointment in the way the Dreyfus and Barnes statements came on to this technically-focused Board.

One way to interpret that is that these two statements were essentially political, not technical. It really looked at the fact that Congress lost confidence in a funding pattern that was tied to a technical approach to site characterization and disposal development, and the reason we're disappointed, of course, is that we're not politicians, and we don't always understand the fact that people in charge have to essentially present a political face as well as looking backwards towards the technical and scientific underpinning for their work.

So, I could understand it if you didn't want to buy onto that analysis, but, clearly, Congress isn't interested in more science qua science. Congress wants a damned repository quicker and cheaper than the trajectory indicated it was going to be delivered, and the Dreyfus and Barnes statements are essentially an acknowledgment of that.

You can either acknowledge that, or we can go on to the next question.

DR. BREWER: Russ?

DR. DYER: If you read the exchange between Bennett Johnson and the Senator from New Mexico, there's little doubt that that's true, yes, sir.

DR. BREWER: Yes, Don Langmuir?

DR. LANGMUIR: Just a related comment, tied to John's, that the other thing that it's saying, it sounds like, to me, is that you've resigned yourselves to the Congress's proposal that there will be a, potentially a site for storage on the property at Yucca Mountain, and this may take priority over the repository.

I think when you back up, as you have, you're kind of accepting that approach from the Congress, without resisting it, and, also, you're accepting that you're going to have less funding indefinitely if you don't resist and explain why you should resist a site at Yucca Mountain for storage, which is going to take your funds away.

DR. DYER: Let me try this again. I guess one way you can structure a program is by figuring out what your budget is, and then figuring out how much you can do. Perhaps a better way would be to figure out what really needs to be done, then fight for those budget dollars, and I think we have gotten down to the point where we are really getting down to what we think are the critical things, and, so far, we still have a viable program. We can still do a lot of things, so, in a way, I'm somewhat encouraged.

This is a skinnier program, but it may be, in the end, it may be a better program.

DR. BREWER: Okay. Ed Cording, do you want to follow up on that?

DR. CORDING: Yes, I'll follow up on that, Russ.

You know, several years back, the program was always ramped up, or ramping up to a $700 million a year number or something like that, and so all the infrastructure and everything was there to support that. And, right now, what you're--what I'm hearing is that you're going to support what you--some say a pessimistic view of the budget, or what you've been told about a decreasing budget. You're going to try to have an infrastructure that can handle that, and you can still get work done, and that you're not going to be ramped up for some potential--you're not ramping up for some potential higher funding level without justification.

But my impression would be that you're going to be fighting for a level that allows you to continue that at, say, something like a, you know, some minimum level that you can continue in the future, and have a lean and mean infrastructure that allows you to take advantage of it, and to really obtain information and get results under that funding scenario.

Do you think that's what's going to happen?

DR. DYER: Sounds good to me. I don't see another workable option.

DR. CORDING: But when you look at this viability decision, are you prepared to come up with a viability decision under the reduced, the 250 and reducing funding, the zeroing out of the funding? Are you going to be able to obtain this viability decision with that type of funding, or do you really think you have to have a level-type funding, or if you have a level funding, could you be doing pretty much what you decided to do in the program plan?

DR. DYER: Well, we started out assuming a decreasing budget, what you might call a worst case, and we think we have a meaningful program that we could do under that circumstance. Now, it's not a zero risk program. It's not a program that resolves all questions, and it's not designed to be. There will still be some outstanding questions at the end of that program.

If more resources are available, perhaps the suite of unanswered questions at the end of it is somewhat to considerably smaller.

Wouldn't somebody else like to share some of the fun here?

DR. BREWER: Anyone else like to follow up with one or two more questions? Don?

DR. LANGMUIR: I guess I would plead for a redefinition of viability. You could make a viability decision almost now, and you could take what you've done on design, and you could take what you've done on cost estimates, and you could say, "Well, I know what I know about the science without pulling anymore together, and I can make a viable decision today." That would be a very cynical thing, I think, to do, given all the energy and effort that's gone into this program so far.

I'd like to see you put change, at least probable behavior repository in the verbiage there into a probable ability to isolate waste as a goal in '98. I mean, that would be, at least, a concrete product which would follow all that's been done before, and all the expenditure and energy that's been put into this program. This is a very pessimistic goal, in my view.

I hope you don't go to Congress and present this as the way the program is going to get in three years, because you won't get to '97, I don't think, if you do.

DR. BREWER: That's not exactly a question.

Anything else? Yes, Pat Domenico.

DR. DOMENICO: I have a technical question. It should be addressed to Dennis Williams, but I think he's probably gone, and maybe--

DR. BREWER: Russ is here.

DR. DOMENICO: I've been to WIPP, like almost everybody. I've seen the water on the walls, and I presume I would see more if the ventilation system failed there. Alcoves are not that difficult. Are you planning any place to develop an alcove, instrument it, isolate it from the ventilation system, and observe? Is that anywhere in anybody's plans?

DR. BREWER: This is the other Russ. This is Russ Patterson.

DR. DOMENICO: Patterson, yes.

MR. PATTERSON: Yes. Actually, we have some work going on in some of the alcoves this year. We've developed a study that we're calling ESF moisture, and we're doing some of the same sort of measurements that Nye County started doing as far as putting a temperature, moisture, humidity probe on the tunnel-boring machine. We're also doing that throughout the ESF.

We've also, as I think Dennis alluded to earlier this morning, we closed off one of the alcoves and saw, of course, that the humidity rose very rapidly, and we've had some discussions about trying to do a test where we would close off an alcove completely and take humidity measurements, and look at that.

We're also--Alan Flint has been taking some samples, some core samples in the site of the ESF to a distance and taking those samples to try and figure out what moisture is within the rock, and we're trying to figure out how much the ventilation is taking out of the ESF, and I think we have some numbers on that, and perhaps--I don't think there's anyone out there that can help me with that right now.

DR. LANGMUIR: I could help you with it.

MR. PATTERSON: Do you have--

DR. LANGMUIR: At least a rumor has it that from 5,000 to 10,000 gallons a day are leaving in the ventilation system.

MR. PATTERSON: That's what I was--yes, that's about what I heard, too, but I wanted to get a more exact number if we could, but that's about right, and so, I think we are looking at that, and that is something that we need to address and will be addressing.

DR. DOMENICO: Well, I don't know if you equate that to a flux or not, probably not, because you've got water in storage there, but then sometime in the future, we may get some idea just on how much water is entering that tunnel, and how it, more importantly, might be varying with time.

MR. PATTERSON: That's right.

DR. DOMENICO: Under really controlled testing, because it seems like, to me, now a very critical question that's--the answer to which is open to you now. I mean, you can go after it now.

MR. PATTERSON: Yes. I want to second a few things that Russ said that, as long as I've got the opportunity, that I think we've made great strides in the last year in getting to where we can start to answer some of the technical questions that we need to answer, and we are still making progress toward answering some of those questions, even under the reduced funding, and I don't think it's quite as bleak as possibly the picture was painted this morning, so I just wanted to add that.

And flux rates, I believe, is one of the areas that we're looking at pretty heavily, and it matches with our waste isolation strategy, so it's something that we need to be looking at and putting great effort into.

DR. BREWER: Jean Younker wants to follow up.

DR. YOUNKER: It's kind of a follow-up, but it's also getting back to Don Langmuir's comment.

One of the things I don't think we've been clear on today, although I think it kind of was between the lines, is that along with that referenced design that Dr. Dreyfus keeps repeating is one of the major elements of his underpinnings for his viability assessment, there is to be a performance assessment of that design that shows how well that design will perform, using the best available information for the site.

So, I would expect that--and I know our contingency planning certainly is aiming at, in the '97 time frame, doing an update to TSPA-95, at least elements of it that we can, and improving upon that, so I think there was never any intention, although maybe it sounded that way, to not have a strong performance assessment component to the viability assessment basis.

DR. LANGMUIR: I guess I'd like to see an expansion of that definition which includes all of these things that we expected to hear, and which were missing from the verbiage. I guess that's the problem.

DR. BREWER: Okay. Ed Cording.

DR. CORDING: I had one question on the waste isolation strategy in terms of you're in the process of completing that now, and what sort of schedule do you have on that in terms of it becoming a policy, or a guideline? Just what is the intent on that?

DR. BREWER: Jean?

DR. YOUNKER: I have an impression that this is probably one that Steve Brocoum should handle.

MR. BROCOUM: Steve Brocoum, DOE.

Our original intent was to issue the document after the DOE review was completed, which will be completed next week, but you heard Mr. Barnes this morning talk about the contingency planning, and the contingency planning team has requested that we delay issuing any policy documents until the contingency planning has had a chance to assess, for example, the waste isolation strategy, just to make sure that whatever they come up with is not in conflict, if you like, or if there are conflicts between what they're working on and the waste isolation strategy, they'd be resolved before the document was issued.

The DOE is not issuing a document next week, for example, and then a month or two later, say, "Oh, we have a new contingency plan that we're going to implement," and we'll be out of synch, so that document will be held up pending that review.

DR. CORDING: Steve, what type of group is evaluating the contingency plan? Is it regarding scientific testing programs?

MR. BROCOUM: The contingency planning is evaluating all the issues that have been swirling around this table for the last half hour. It is looking at what more can we do, can we do more than a viability assessment? What schedules can we do on that? But that effort has recently started, and is underway right now. We have set up a steering committee and a working group under Jane Summerson to do that work, and that work is being presented to Wes Barnes, who will then present it to Dan. Dan has allowed us to undertake contingency planning, but he has not allowed us to implement any of the ideas, if you like, or the issues that they've brought up.

DR. BREWER: Okay. Thanks, Steve.

Yes. Jerry Cohon of the Board.

DR. COHON: Virtually all of the talk about budget and viability and what the latter means has focused on Congress, and for good reason. However, I think that we may be entering a period here where DOE may face a real problem in terms of keeping confidence from the public, putting aside Congress.

You run the risk of putting yourself in a position where a member of the public, who's followed all this, would be tempted to conclude that a repository, the definition of a viable repository is, as suitable as it may be in 1998, given whatever money has been able to be devoted to the study between now and that year, I mean, there have been elements to this in what some of you have said that recognizes that risk; that is, you can't have something that's triggered entirely by the money that's available.

But, I think you really have to pay attention to how this is going to be communicated to people other than Congress, especially given the kind of funding history we've had up to now. How do you maintain credibility when you, presumably hypothetically, make an announcement two years from now, or the Secretary does, "This site is viable. We should go ahead." You have some explaining to do.

That's something--I don't know how you respond to that if you want to. I'm not asking you to, I'm just making a comment.

DR. BREWER: If anyone cares to respond, please do.

DR. DYER: I'll give it a try, because it's a very real topic that we've talked about. There is a, certainly, an issue of credibility here. There is an issue of confidence. How do you retain or build both of those? And I don't have a good answer for you, not at this time, but it's something that we're trying to negotiate our way through right now.

DR. COHON: Having made the point, let me offer my own advice. I happen to think that the progress you made with TSPA is, in and of itself, very promising, and it's also something that you could hang this case on; that is, two years from now, you might have something really very cogent and credible to say because of your ability to do total system performance assessment.

I think that's the key, but as much progress as you've made, there's also no question that you've got to make more for that to be the foundation.

DR. BREWER: Russ?

DR. DYER: You're absolutely right. This thing in '98 is centered around a total system performance assessment.

DR. BREWER: This is the other Russ, Russ Patterson.

MR. PATTERSON: I'll add one thing. I think one of the things that's kind of hidden in the definition of the viability assessment or decision, whatever you want to call it, is the TSPA part, and all the science that goes into that TSPA, because I believe the next TSPA, which I believe is called TSPA-97, which will actually be in '98, will look much different than the last TSPA, because--and my areas of hydrology and geochemistry will have much different flow models and transport models than what we had, and we're using them for that one.

So, I believe there's a lot more science that'll be going into those. We're synthesizing a lot of things now, and a lot more data that will go into the TSPA, so that TSPA actually should be a better product than the last one.

DR. BREWER: Jerry Cohon.

DR. COHON: Just to emphasize a point, and these models will be different because of the data you're getting now, because of the ESF, and, I mean, that's such an important point to make, and to emphasize that and clarify it so that members of the public and members of Congress can see how the pieces fit together. I think that is the most hopeful and powerful thing you have to say.

The reason you have a hope of pulling this off is because the pieces really are fitting together, whereas, before, they were all disparate, and not at all connected. But, now, with the progress in TSPA, because you've got the tunnel drilled, that you can see things and you're getting data, things really do gel, so it's a much more hopeful and positive message than the one we started with.

DR. BREWER: Okay. Anyone care to comment, respond? Additional questions from the Board?

(No audible response.)

DR. BREWER: If not, I will end this particular part of the question and answer session, and now turn it over to the two members of the public who have signed up. I would like to enforce a five-minute rule, and I think that's adequate time to say what's on your mind.

Our first member of the public is Tom McGowan, a regular who has spoken to the group on many occasions in the past. Mr. McGowan, if you would take one of the microphones here, and if there is an organization or institutional affiliation, please let us know. Five minutes, sir, if you would.

MR. McGOWAN: Thank you. It's been rumored that I'm affiliated with the American public. The headquarters are unknown.

DR. BREWER: That's not bad. That's a good start.

MR. McGOWAN: Thank you, sir, and how much time do I have left, incidentally?

DR. BREWER: Four minutes and 56 seconds.

MR. McGOWAN: Got that. I'll be succinct, whatever that is.

Honorable Mr. Chairman, esteemed members of the committee, foreign guests and meeting attendees, my name is Tom McGowan. I'm an individual member of the public, residing in Las Vegas, Nevada.

The TBYMS study committee's report on findings and recommendations raises more questions than it provides answers, and avoids exercise by the NAS of its Congressionally-mandated discretionary authority and responsibility over U.S. policy issues in the genuine best public interest, but, instead, relegates that authority and responsibility exclusively to the EPA and the U.S. NRC, and I believe the words on the public record at that point of transference were something to the effect of, "Hot potato in your lap."

At this juncture, the entire nuclear waste issue is complex as -- to context is the singular, fundamental, crux issue question of the prospect for the attainment of the degree of political and public acceptance requisite to surmount the barriers of unresolved uncertainties, complexities, and deficiencies which, combined, define the TBYMS study committee's report, as well as the subject topic of its study, the Yucca Mountain repository initiative and site characterization study requisite to the establishment of exposure risk standards, regulatory compliance standards, and therein, suitability licensing, as a time and budgetarily-constrained guesstimate of an approximation, set within a limited, finite, micro-increment of a vastly greater dimensional domain, naturally ordered as in a state of variable dynamic flux, and during all of the geologic time scale continuum.

Thereas, two diametrically counterpoised alternatives, scenarios, pertain and will persist pending definitive selection and conclusive address and resolution in a timely and assured, effective manner.

With regard to the first alternative scenario, as currently configured and constrained, the Yucca Mountain repository program--and, incidentally, I'll interject something. I wish to frankly and sincerely commend everybody concerned with that effort. I'm talking about DOE, Steve Brocoum, Wesley Barnes, everybody. These guys are good soldiers. I may not be right there rooting with you and, you know, helping you along. I don't think you need me as a crutch. You're quite articulate. You have done a hell of a job with virtually not enough to work with. That still don't make you right, but I've always believed in a level playing field.

Incidentally, Dr. Cantlon, nice to see you're back. Don't bother. Where was I?

Oh, as configured and constrained, the Yucca Mountain repository program will not achieve political and public acceptance, requisite with respect to operations, inclusive of construction, transport, emplacement, closure, and post-closure activities except and solely via the establishment of exposure risk standards by the EPA and of compliance regulations by the U.S. NRC under color of law, and virtually via government by fiat, which is a form of dictatorship, and, thereas, unacceptable in this particular nation, still.

Clearly, as duly noted by the NASTBYMS study committee, neither the EPA nor the U.S. NRC could conceivably hope to discharge their mandated responsibilities within the limited time allotted, and particularly not with any substantial assurance of a reasonably unconstrained open, public discussion process, as recommended by the NAS committee, and, particularly, also not if it snows, and for your assurance, I'm told that we also have a flake in the White House, why bother about the snow? Didn't mean that, sir.

As, clearly, the public tends to be more so reactive than proactive, and rather than being responsive to objectivity and logic, instead is more so subjective and emotional, hence, the public perception of risk is more closely understood and addressed as the perception of perception, rather than a risk or the perception of it.

The peril inherent in the current aggressive, but appreciably constrained paradigm resides in the potential instance that wherein political and public acceptance, ultimately, is unattainable, then that probably could cost the entire program to date, inclusive of time, budgetary and other resources, is also and perhaps more so unacceptable, since it is both tangible and unretrievable, rather than projected and avoidable.

In the second and preferred alternative--got to be a Shetland pony in here somewhere--enthusiastic political and public acceptance is ensured, readily attainable, but not via persistence in the current and projected paradigm.

Finally, and as an element of the preferred alternative, ensured effective obviation of human intrusion, in entirety and in perpetuity, is also readily attainable, but, again, not via the current and projected paradigm, but reliance upon any combination of natural and engineered barriers, and traditional post-closure monitoring activities.

Wonder what he's talking about, folks?

In postlude, it is reassuring to note that the Congress has directed the EPA and the U.S. NRC, et al., to assume that human civilization will continue to exist--that's page 143, I think--throughout the distant future, notwithstanding the absence of any reference whatsoever to the conceivability of a Supreme Being and Creator of the entire universe, everything in it, including the Congress of the United States, the NAS, and human civilization, or something in one sense or another similar to it.

You have my sincere sympathy, because I think you're at a stage now, we're at a very important juncture. We need to decide, in the words of the artist, Paul Gauguin, who are we? Why are we here? Where are we going? That's the question. It's not a nuclear issue at all. It's an issue related very intensely to our innate human nature.

DR. BREWER: Mr. McGowan, that's--

MR. McGOWAN: If you know something is right, you proceed with it. How much time?

DR. BREWER: You have none. This is five minutes.

MR. McGOWAN: I beg your pardon, sir. Thank you so much. What was your name again? Anyhow, we have your name.

DR. BREWER: Thank you very much, Mr. McGowan, and for your five minutes.

The second member of the public who has indicated they'd like to speak is Parvis Montazer, representing Nye County, and if there are other organizations you represent, sir, would you please say so?

MR. MONTAZER: I am Parvis Montazer, consultant to Nye County. I just have some technical comments and questions regarding this morning's presentation.

DR. BREWER: Thank you very much for having technical questions and comments, yes.

MR. MONTAZER: First, I'd like to compliment everyone for the presentation. That was an excellent presentation in a short time. There were a lot of information that I hadn't heard before that was enlightening.

The main question that I have is regarding the age dating on the fracture filling, and since this is the first time that I've seen this data, I'm not quite sure how these samples were collected, et cetera, so my comment may be a little bit ignorant, but the way I see these, or I've been taught these fracture fillings occur is with times over hundreds of thousands of years, or millions of years, layer-by-layer, millimeter-by-millimeter, basically grow, and when we take these samples, we're averaging a layer that may be--or sampling a layer that may be 100,000 years old or a layer that may be 5,000 years old.

We have gaseous-phased data which, at least it's our concept that it's somewhat in equilibrium with the fracture filling that show our Carbon-14 activities are much more recent than 100,000 to 200,000-year range that was presented this morning. This Carbon-14 activity mainly comes from what I know from UZ-1 and some of the other boreholes, a gas sampling that has been going on by U.S. Geological Survey for quite some time.

Therefore, I believe that the gaseous phase, at least in Topopah, may be more representative of the latest recharge, and the age of the--the recharge, rather than the whole fracture filling age.

The problem that we have is that in the Topopah Spring, because of the tunnel right now, the entire pneumatic conditions are disturbed from the data when we look at some of our most recent data collected on a pneumatic responsive zone of the boreholes. It's my initial perception that all of the boreholes seem to be responding, at least in the Topopah Spring, to the barometric fluctuation in the tunnel.

So, my concern is that have we really disturbed the Topopah Spring to the point that we cannot get gaseous chemistry anymore? Is C-14 and tritium dating basically out of the question in Topopah Spring? And, you know, if that's so, how are we going to really verify these models as far as the infiltration and percolation, et cetera, are concerned?

A quick comment on the humidity regarding the waste package is I believe--and I've made these comments before to the original author, Dr. Buscheck, of Lawrence Livermore--humidity is a misleading indicator in this condition in the repository sense, and I think the project should change this to another well, I should say, a comparable, comparative number, and, basically, humidity of 10 per cent at 100 degrees C can have five to ten times more moisture content than a 100 per cent humidity at 20 degrees C. Therefore, I can't see why we are concluding that the lower humidity, we have less corrosion. At what temperature; under what conditions?

And I think the project needs to look at the actual--all the other components that are involved in there, and I know that the scientists are looking at it, but the way you come across in these presentations, it doesn't--it sounds misleading.

DR. BREWER: You have about one minute, sir.

MR. MONTAZER: Okay. One quick thing is, is it my understanding that the thermal test room is going to be drill and blasted? I think I heard that. If that's so, why is it? Isn't that kind of contradict the fact that we're putting a tunnel, with a tunnel-boring machine, and we're going to be testing the--thermal testing in a...

DR. BREWER: Okay. If I was listening correctly, there was a statement with some general questions that may or may not be able to be answered. The final statement was a question, and I'd like to address anyone on DOE who can provide an answer, and this has to do with the thermal testing alcove.

MR. REPLOGLE: Jim Replogle. I'm standing in for Rick Craun.

We, this weekend, will be testing the road header to determine if that, in fact, can do the excavation in that area, and if you'll stay tuned in, I'll give you the answer Monday on how we're going to do that. We don't have an answer at this point. We hope to be able to do it with the road header that we're moving in this weekend for a test.

DR. BREWER: Don Langmuir?

DR. LANGMUIR: I'd like to seek an answer to Parvis's first question. I think there are people in the audience from the USGS who have either sampled the fracture fillings, or at least are aware of the age dating. I think John Stuckless back there has talked about some of this with us earlier in the day in his answers to questions.

Could someone address the uncertainties that are apparently inherent in analyzing fracture fillings as we've been looking at them so far in the ESF? Maybe John.

DR. BREWER: Would you please identify yourself, and the institution?

MR. STUCKLESS: John Stuckless, USGS.

Unlike the engineers, we are not going to have an answer by Saturday night. The dating that is going on is projected to have answers by the end of February, just as a start, so what you saw today is very preliminary information.

The second thing is that what they have attempted to do is to get the outermost layers, which probably still represent some sort of an averaging. Another problem we've run into is that the lithophysae, lithophysal cavities that have been sampled have multiple generations of calcite and opal in them. Some of it appears to be a paulopost-type deposition. Some of them appear to have a floor of the cavity only, with modern Pleistocene-type deposits on it.

We do know that we have multiple generations. We do know that some of our samples definitely represent averages of a few bands. The significant part of all of this, though, is the very that if they were very much modern material in there, we would see average ages that were much younger than the 90,000 years. The dominant age that's coming out of these things at the moment is around 250,000 years. If that's one end member for water moving through here, to be able to pull it down only to 90,000 years suggests that in the last recent past, polocene, very little water has gone through there.

Parvis also mentioned the problem, potential problem of a gas-phased transfer. This is certainly a very real thing, but it does not affect the uranium series dating.

Neither uranium nor thorium move in a gaseous phase at the type of temperatures that we're looking at in the potential repository horizon, so we do have Carbon-14 dates--and I think people are aware that we've published them--that have modern carbon in some of these fracture coatings. That's only from the drill cores.

It is not totally clear that some of that isn't possibly contamination from--it's also all G-1, which was drilled wet, and had an awful lot of organics dropped down it in order to try to keep circulation, so it's not clear that those ages are reliable. We are redoing that in the ESF, and this time we're going to make an effort to make sure we don't have any modern contamination of the samples.

Carbon-14 can, in fact, analyze only that very last layer that was deposited. By accelerator mass spec methods, we get by with very, very small amounts of material, but, anyway, all of this, hopefully, by the time this Board reconvenes, will be completed in the repository horizon in the ESF.

DR. BREWER: Yes. We look forward to hearing about it. Thank you very much, John.

I'm going to take the prerogative of the Chair, because we're now at five minutes to five, and call this meeting adjourned.

I want very much to thank everyone who participated on a very ad hoc basis because of the scheduling problems that we've had. I think we had a good and full and productive meeting. We're adjourned until eight-thirty tomorrow morning.

(Whereupon, at 4:55 p.m., the meeting was adjourned until 8:30 a.m. on January 11, 1996.)

1