NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON THE ENVIRONMENT AND PUBLIC HEALTH

A REVIEW OF THE YUCCA MOUNTAIN ENVIRONMENTAL PROGRAM

Holiday Inn Crown Plaza Hotel
4255 South Paradise Road
Las Vegas, Nevada 89109
March 22, 1994

BOARD MEMBERS PRESENT

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Dr. Garry Brewer, E&PH Panel Chair
Dr. John McKetta, NWTRB
Dr. D. Warner North, NTWRB
Dr. Dennis Price, NWTRB

STAFF MEMBERS PRESENT

Dr. Daniel Fehringer, Senior Professional Staff
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Ms. Linda Hiatt, Management Assistant
Ms. Kathleen Downs, Staff Assistant

CONSULTANTS

Dr. Michael Bowers
Dr. Jim Ehleringer
Dr. John Koranda

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PROCEEDINGS

DR. GARRY BREWER: If everyone would please find a seat, we can get the proceedings going.

Good morning everyone, and welcome to the meeting of the Nuclear Waste Technical Review Board. This is the Panel on the Environment and Public Health.

I'm Garry Brewer, and I'm Chairman of the panel. I'm also the dean of the School of Natural Resources and Environment at the University of Michigan, where I'm also a professor of resource policy and management, and business administration.

Let me take a moment to introduce to the audience other members of the panel. This is the panel. John Cantlon is the Chairman of the Nuclear Waste Technical Review Board, a member of the panel. He is the former Vice President for Research in the graduate school at Michigan State University. We do talk to each other, we're friends, in spite of the Michigan State problem. He's also a professor emeritus of botany, and his field is environmental biology. So he is a natural to be on my panel. I'm glad he's here.

John McKetta is the Joe C. Walter professor of chemical engineering, emeritus at the University of Texas. John McKetta is one of the most experienced chemical engineers in the world with over 55 years of experience.

Warner North, in between the two, is a consulting professor in engineering, and economic systems at Stanford, and a principal in his own firm, Decision Focus, a firm involved in risk, risk management and assessment.

Assisting us today are three consultants whom we've invited particularly to help us understand desert ecosystems. Starting at the far end of the table is Michael Bowers of the University of Virginia. His specialty is in desert animals, especially the structure of desert-rodent communities.

Jim Ehleringer is next from the University of Utah, a broad experience in ecology and the physiology of plants, with special emphasis on desert environments.

John Koranda, third consultant at the table, is retired from Lawrence Livermore. He's an environmental scientist with years of experience on the Nevada test site.

We also have in attendance today, and I'd like to single out for special notice, Dan Fehringer of the TRB staff, who is primarily responsible for organizing all the details, and they are numerous, and has done a splendid job in putting this meeting together, for which I'd like to acknowledge and thank Dan.

Also new to the TRB staff, he's been here six days, is Dan Metlay. Dan, would you raise your hand? Our newest staff member in the back of the room. We were very fortunate to attract him from the Secretary of Energy's advisory board where he was primarily responsible for the recently published report on public trust and confidence.

Also, from the Board's staff, and again, because they do all the work, she's doing it right now, I'd like to acknowledge Linda Hiatt and also, Kathleen Downs. Kathleen is at the back of the room writing your names on tags.

One of the real pleasures--who have I missed? Oh, and Dennis Price. Pardon me, Dennis. Excuse me, Dennis Price of VPI. You were behind Wendy, and I didn't even see you. Dennis Price is also a full member of our Board from VPI. His specialty is in risk, transport, safety systems and the like.

The Nuclear Waste Technical Review Board was created by Congress in 1987 as part of the amendments to the Nuclear Waste Policy Act. Our job is to provide oversight, an unbiased source of expert assessment on technical and scientific matters related to the Yucca Mountain site and other issues related to high-level nuclear waste and waste management.

For those of you in the audience who don't know, we are mandated to produce two reports per year, and we report to the Secretary of Energy and to the U.S. Congress.

The Board itself is broken down or decomposed into various panels. This is the panel on environment and public health, and our job is to review environmental activities, the whole spectrum of environmental activities associated with the Yucca Mountain project.

The panel had been relatively inactive until last November when we as a Board decided that it was time, given the four, five years of data and activity on site, for us to begin to do a basic assessment; how are things going, what is happening, and how are things going at Yucca Mountain related to environment particularly, and to a lesser extent, public health.

To get us focused and to get on with the task of establishing what's happening, we invited our three consultants, and we invited numerous people in the Department of Energy related to the environmental program, which is headed up by Wendy Dixon, who will be our first speaker this morning, to come and make presentations to begin to get the Board up to speed.

At that time we said to everyone that we would like to come back for a second meeting and to take a tour of the Yucca Mountain site, and in the interim to have some more focused questions that would help us get a better understanding of not only the breadth and scope of environmental activity, but also to begin to raise questions about the relationship of this program, the environmental program, to other aspects, often much larger in nature and quite different in character of the whole of the site characterization project and prospect at Yucca Mountain.

Among other things that we learned and items that will be highlighted in the presentations today, the need to think seriously about connecting below-ground activity to above-ground activity, the need to--and we have with us, as you'll see as we get on with the day, individuals who are interested in making that connection.

We're also very much interested, and it was again born out in the informal discussions that we had in the field of Yucca Mountain yesterday, a very fine site visit from our point of view, with the integration of the environmental program with other things that are ongoing above ground even, some comments about the connections between the USGS, the environmental program, some amazing sort of serendipitous things being discovered as we wandered around and looked at things and asked questions.

As I have assured everyone that I can who's associated with the environmental studies program at Yucca Mountain, our job is to be thorough, to provide unbiased scientific and technical oversight and to essentially ask questions that will help the project do its job as well as possible.

Yesterday's field trip was very much in the spirit. I'd like at this point to acknowledge and thank Wendy Dixon and her group. These things are hard to produce. I mean, it's a three-ring circus on a good day, and often many more I suspect. It was a fine session. We learned a lot. It was extremely well done, and thank you very much on behalf of the panel and the Board. It was a good job.

The other thing about this Board is that we are a public body. We do our business in public, and accordingly, yesterday we had members of the public who had signed up taking the trip. The discussion was free and open. And very much in the spirit of that, you'll see at the end of today's proceedings, there is an opportunity for the public or anyone else to ask questions of members of our panel, of the Board, of our consultants, of those who are making presentations.

We have a round table prepared at the end of the day. If anyone wishes to make a statement at that time or to ask questions, please let our staff know, and that's primarily Dan Fehringer--Dan, raise your hand, here in the corner--that you have something you want to say in the afternoon. You're perfectly welcome to do so, and, in fact, consider this a sincere invitation.

All right. Let's talk about today's agenda. We open with Wendy Dixon, who will give us, again, a focused overview of the entire program for which she is responsible. I would hope that there would be some commentary on the evolving sense of what the Board is doing with and for the environmental studies program and questions that may come up.

This is a panel. We're here to learn things from one another.

The morning session is primarily related to technical and scientific studies, as you can tell by just a quick perusal of the titles and the people making the presentations.

The afternoon session is much more related to the question of, well, what if the site is selected to be characterized? The "what if" in this case is related to the whole range of activities that would then be triggered related to Environmental Impact Assessments and Statements.

Let me emphasize, because it is absolutely essential that everyone be, as a former president of ours said, perfectly clear about this. We are not in the business of practicing, recommending or doing anything that is legal. That's beyond the purview of the Board. We are a science and technical body.

Nonetheless, there are scientific and technical matters that are often triggered by, constrained by, driven by matters such as the Environmental Impact Statement, and we thought it prudent at this time, and a responsible act on our part as a Board, to begin thinking about and making everyone else aware of the science that would be necessary if you were to presume that the site were selected five years, six years, or whatever the time. It doesn't matter. What has to be done now to have adequate, appropriate science underway in place to inform the EIS process?

Let me again emphasize, we are not asking for a specific legal analysis of Yucca Mountain. What we are trying to do is to be prudent with respect to the technical and scientific issues that necessarily would be generated for the site to be selected. That's a terribly important thing to underline, and I'm doing it three times just to be sure that everyone here is perfectly clear what we're doing this afternoon. It's important.

Let me get on with it by introducing Wendy Dixon, who is the director of the Project and Control Division of the Yucca Mountain Project Office. She's going to lead the presentation today with a general scope overview of her program.

Wendy, welcome, and have at it.

MS. DIXON: Thank you very much.

Good morning. It's a pleasure to be here today to speak before the Board and the rest of the public that is present.

We have a very brief update in our environmental program, principally focused towards terrestrial ecosystems, which was the topic of conversation for the last Board meeting. Quite frankly, after the last Board meeting, we all left and spent some time discussing a number of the issues or some of the issues that had come up with respect to questions from the Board; principally their interest in long-term repository effects. There were a couple of questions that came out with respect to site suitability and how the environmental program played into site suitability.

And we felt that as an introduction to today's session, it might be worthwhile going back and discussing the framework from which we built our environmental program. And like I said, the focus right now is ecosystem; if not for the Board members specifically, most certainly for their consultants, who are perhaps less familiar with the Nuclear Waste Policy Act and its implementing regulations. And I think this might provide a little bit more in the line of insight.

Basically our program most certainly was developed with the recognition of the Nuclear Waste Policy Act and its implementing regulations, which basically gave us a number of guidelines on how to develop our environmental program.

The Nuclear Waste Policy Act streamlined the NEPA process for the Department of Energy. It required environmental review throughout the repository phases that we're heading into, and while NEPA most certainly is a great tool and a valuable tool and a necessary tool for public involvement and for the provision of comments and concerns and so forth, the Act also set up a number of other forums for public involvement, for public review, which includes the forum that we're in today for the Nuclear Technical Review Board.

And if we go back to the statute, it required a statutory environmental assessment for site characterization, and as the Board knows, our focus has been on the site characterization phase of this program.

The Nuclear Waste Policy Act said that site characterization is not to be considered a major Federal action. The Nuclear Waste Policy Act said that an Environmental Impact Statement was not required for site characterization, but it did require monitoring and mitigation of significant adverse effects during the site characterization time frame. And a lot of our program that you see was established from that statement in the Act, as well as obviously to maintain compliance with existing laws and regulations.

The Yucca Mountain EA was developed and that EA stated that there was no significant adverse effects that should be expected from Yucca Mountain site characterization activities. But to make sure that our assessment was correct, our monitoring program looks at potential site characterization effects and monitors for them, and is integrated and involved with all the site characterization activities that you see out there.

Mitigation activities and involvement includes our discussions on pre-activity surveys, our input into design as design takes place, our stipulations that we place on our engineers and construction managers to deal with things such as protection of endangered species, topsoil stockpiling, erosion control. We require reclamation in all of our activities.

So we're involved and integrated in all the activities that go on at that site from start to completion.

If you look at 960, which is one of the implementing guidelines procedures for the Nuclear Waste Policy Act, which is important to us, and this one ties to the suitability of the site, there is a statement up front about basically waste pre-closure and post-closure guidelines, putting the most weight on post-closure guidelines. And then you turn to where environment shows up, and there's several articles that deal with pre-closure guidelines for environmental quality.

In summary, these all basically say the quality of the environment will be adequately protected and significant adverse environmental impacts mitigated to the extent practicable during all stages of the program. Again, recognizing that there will be various stages that will take place.

If you take a look at that same regulation and look at what it says for disqualifying conditions, again these are summarized, but in essence it says that environmental impacts cannot be mitigated to an acceptable degree taking into account the programmatic, the technical, the social, the economic and environmental factors.

And then there's references to irreconcilable conflicts with previously designated resource preservation use.

If you look to post-closure guidelines and disqualifiers in that implementing regulation, you will find none. The 960 Regs dealing with site suitability, if you say how will the environmental side of the house deal with site suitability issues, the answer to that is through our Environmental Impact Statement. That is our document for site suitability on the environmental side of the house.

Again, if you go back to the Nuclear Waste Policy Act and look for what it says with respect to Environmental Impact Statements, it provides us a road map that says up front that our Environmental Impact Statement, as it relates to license application for construction, the first for suitability of the site, it says it does not need to determine or deal with the need for the repository itself, alternatives to geologic disposal, alternative sites to Yucca Mountain.

And as those of you involved with NEPA all know, the heart of an EIS is basically alternative section, and a lot of this most certainly has been streamlined by Congress when they developed that Act.

That Act is really also clear with respect, as I said up front, to requiring an environmental review throughout the repository time frame.

Most certainly, we've already talked about the site characterization phase, which we're implementing right now. There's the license application that we're moving towards in the near term that ties to the application to construct the repository. The Act spells out the need for an EIS at this particular point in time, but it also recognizes that between this particular application and the modification requesting authorization to receive and possess radioactive waste, there will be a lot of additional data that will be generated both from the site side of the house, as well as the environmental side of the house.

So the Act specifies that at this particular point in time, should we get to that decision, there will be a supplement to the Environmental Impact Statement adding the additional information that will be derived during that time frame.

And then it goes on to say that after the decades of monitoring a fully-loaded repository, we also recognize that additional information will be available, that that additional information will need to be factored in, and that with this application for closure and decommissioning and this decision to be made, there will be a further supplement to the Environmental Impact Statement utilizing that newer and better data that will be available at that particular time frame.

So in summary, the Nuclear Waste Policy Act and its implementing regulations ensure that the NEPA review applies to each of these stages, and by doing so, Congress enabled the Department of Energy through this continued environmental review process, through the obtaining of better data through time, to better gauge the needs of public health and public safety.

Congress further provided that any final decision on the repository soundness would rest on a period of study and observation of the fully-loaded repository, and that the integrity of this observation period be preserved by requiring that the waste that would be emplaced would be fully retrievable.

So the final decision is, obviously, some years away, and there's a series of decisions that need to be made before we get to that final point.

So then the question is where are we right now? Again, as I said, our focus has been very heavily on site characterization, and that phase will continue through the site characterization phase of the program.

But we're also at this particular point in time looking at and getting ready to prepare the Environmental Impact Statement that will be tied to the site recommendation, and depending upon budgets and so forth, there is a potential that we might start the scoping process on this particular EIS as early as sometime next spring.

We also recognize that the site suitability vehicle will be on the environmental side of the house, the Environmental Impact Statement. We're currently analyzing availability of site characterization data for the Environmental Impact Statement because information ties together, as you all have pointed out, and this is an important factor for us.

We've been analyzing the precepts of NEPA, including the judicial device called the Rule of Reason, and we've been analyzing what data will be available at later project stages that will allow NRC or DOE to modify any decision that is made.

So with that as a groundwork statement, I'd like to have Ron Green come up, and Ron will talk a little bit about some proposed modifications to our site characterization effects program that we're planning on implementing next year, and also, a little bit about our review of a study design as it relates to thermal loading impacts, that study design review to include input and comments that we hear from the Board and its consultants, review of what's going on overall, and other types of efforts and programs. And also, one of the purposes is to make a determination as to whether or not there actually is or is not a need for field experiments or field investigations.

So it's an open-minded review, and we need to keep that in mind, too.

On that, I'd like to introduce Ron, unless someone has any other questions before I turn the podium over.

DR. BREWER: Warner North?

DR. NORTH: I'd like to clarify one point on your slide No. 5, where you talk about what the EIS need not consider. Is my understanding correct that what is appropriate for the EIS to consider is alternative repository designs?

MS. DIXON: This does not preclude us looking at alternative repository designs.

DR. NORTH: Right. So alternatives to geologic disposal need not be considered, but alternatives for Yucca Mountain in terms of different repository designs, however that might be construed, definitely does stay in bounds the way the legislation and the regulations currently stand; is that understanding correct?

MS. DIXON: Correct. It doesn't say that no alternatives will be discussed, but it says that these key alternatives will not be included in the EIS.

So what alternatives will actually be in that EIS, I can't project at this particular point in time. We'll come up with some suggestions certainly during the scoping period. We'll be obtaining information and input from the general public.

DR. NORTH: But the point I wanted to emphasize is that given Yucca Mountain is found suitable, a very broad spectrum of alternatives for repository at Yucca Mountain might be considered, and the EIS may need to address that broad scope of alternatives for a repository at Yucca Mountain?

MS. DIXON: Reasonable alternatives, and we always need to keep the term reasonable in there. What are the differences between them? Is there a difference as it relates to environmental impact? Which types of alternatives might be tied to mitigation types of issues? But design issues most certainly would be included, yes.

DR. BREWER: Other questions?

DR. BOWERS: Yes.

DR. BREWER: Mike, please identify yourself.

DR. BOWERS: Bowers, consultant.

Wendy, site characterization, I don't have a clear definition of what that is. Does that provide a baseline for an EIS?

MS. DIXON: Oh, I'm sorry, no. The site

characterization process spelled out in the Act, in the implement regulations, and I'm sure Russ Dyer could talk to it in more detail, but that is the mandate that we characterize the site, understanding that with respect to a number of factors, environment being a very, very small piece of it, you're talking the geology, the hydrology, the tectonics, the volcanism, in order to make a determination as to its suitability for use as a repository. So it's a full-fledged investigation program.

DR. BOWERS: Predisturbance?

MS. DIXON: It's not predisturbance. If you look back to the Act itself and the legislative history that defines the Act and the time frames that Congress set up for the Act, they're basically within the same time frames, had ongoing at the same time, the site characterization activities, which are all disturbance activities--you know, that's the drilling program--the ESF activities that you see at the site, any trenching you see at that site. Everything that the geologists, hydrologists and so forth are doing out there is tied to site characterization. That's all going on at the same time frame as our monitoring activities are going on and as our preparation for the EIS is going on.

DR. BREWER: Other questions?

(No response.)

DR. BREWER: Thank you, Wendy.

MS. DIXON: Thank you.

DR. BREWER: Ron?

MR. GREEN: I had the opportunity to address the Board in the last Board meeting last November. I concluded that presentation with a brief discussion of several proposed changes that we were considering in the Site Characterization Effects Monitoring Program. And what I'd like to do today is to pick up where I left off and kind of talk about the Site Characterization Effects Monitoring Program with the proposed changes to kind of give you an idea of where we're going with this program.

Some of the reasons for the proposed program changes are listed here, specifically the location of the exploratory studies facilities have changed. Locations of specific activities are now better known. Activities are concentrated in primarily one vegetation association, and that's in the region from the north portal down to the south portal, and we discussed some of this yesterday on the field trip. And we found very little evidence of additional indirect effects on a lot of our monitoring plots out there from the five years of monitoring data that we've collected to this point.

There's essentially going to be four components, if you will, to the proposed program. The first two monitoring direct impacts and mapping of vegetation have always been considered part of our program. We'll continue working on those.

The third component, the monitoring biotic community is looking at indirect impacts, added indirect effects in addition to the direct effects, is the program that I presented last November in a little more detail. And I'll spend a little time this morning explaining the changes there.

And then the last component, there are long-term repository effects, is really a new component, and I'll make a few comments regarding that of what the status is and where we're at in regards to that.

DOE will continue to monitor direct impacts. Those are the disturbances that are occurring out there during the construction phase during the site characterization phase. The plan there is to--we do this through our monitoring mitigation program, which is essentially the pre-activity survey process and the post-activity survey process where we actually go in and document all the disturbances that are out there, map them.

The plans are to enter all that data into the Yucca Mountain project geographic information system so we can track those through time. And then once we get the vegetation mapping phase of it done, then we can track those disturbances by vegetation association.

The vegetation mapping hopefully will be completed in the next year and a half. Vegetation associations were identified at the site in 1982, and we identified four associations there in 1988 when we started our current phase of monitoring studies. We will continue to refine those using the existing data from '82 through '84 and the data that we've collected since 1988. We'll be using aerial photographs in conjunction with that data to map those associations and then enter that--get it digitized and enter it into the geographic information system.

I'd like to spend most of my time this morning talking about the monitoring biotic community component of our program. This is the program, like I said, I presented last November in a little more detail.

Again, the Larrea-Lycium-Grayia association, which is that area surrounding--that's in Midway Valley, and it curves in the lower areas, elevations, in the flat areas just below Yucca Mountain, is the area where most of the disturbances are going to occur. So our monitoring efforts are going to be focused in a one vegetation association; that is, where the most impacts are going to occur.

We will not be continuing the monitoring in the other three vegetation associations because based on current design, there's not going to be any, or very few impacts in those areas.

We're going to define, if you will, three sampling areas. The exploratory studies facility is really going to be considered our treatment area, or our impact area, and that's generally the region extending from the north portal south to the south portal, and it includes the muck storage area, which the precise location I don't think has been determined yet. The other area that we're going to consider is also the borrow pit area, which is not adjacent to this, but is a large disturbance over by Forty Mile Wash.

We will also identify one additional control area. We have existing control plots out there. We will continue to monitor those plots, but we will also locate additional control area distance from--more distance from Yucca Mountain.

And so essentially we'll have two control areas and one treatment area, and each of these areas will locate five or six plots. Initially we decided on five. We may add a sixth plot in our sample adequacy criteria.

We have existing plots in two of these areas. We're going to be locating additional plots this year. The locations of those haven't been determined yet. We talked about them briefly yesterday in the field and pointed out some areas that we're considering.

On each of these plots we're going to be measuring vegetation cover, production of annual plants, small mammals, and monitoring lizard populations, particularly the side-blotched lizard as the indicator species and recording our measurements of abiotic parameters; namely, air temperature, soil moisture, soil temperature, as we have been in the existing monitoring program.

This design, or these proposed changes, are essentially going to be phased in over time. This year we're going to continue monitoring based on our existing design using our 48 ESPs, and we'll be recording vegetation cover, small mammal populations, side-blotched lizard populations and the abiotic measurements on those plots. We will be locating these new plots this year, and if we have time, we'll probably initiate some measurements on those plots.

The primary reason for doing that is the majority of the site characterization effect activities really started in the winter of 1992 and 1993. So we have about anywhere from two to four years of predisturbance data before that on the existing study plots, but we only have one year of post-disturbance data. And so we want to get at least one more full year of data on the study plots under the existing design, and that's why we're phasing it in.

So the new monitoring plots will be used starting in 1995. So that's the current plan.

Okay. Let's move on to the last item, and this is the long-term repository effects. This is sort of a new component of the program, and there's really two phases here. One is the identification of issues and objectives, and we have not designed any studies. We haven't made any commitments to field studies at this time. We're really going through a process of identifying issues and possibly some objectives, and certainly this Board meeting today and the field trip yesterday and last November is considered part of that process. I think some of the issues for the round table that the Board has put together for this afternoon will be useful in helping us deal with the issues and objectives. We possibly will consult with outside consultants regarding issues and objectives.

So this is sort of an evolving thing. There's been no commitment one way or the other on specific studies, but we feel that we need to spend a lot of time in this phase here really identifying what the issues are and what the objectives should be.

DOE will certainly have input into this, and as we get into the EIS scoping process, issues and concerns will probably arise that will feed into this.

And from there, if we do decide that we need study designs or studies, then we'll develop study designs.

What is the status right now? We are continuing to review literature on thermal loading issues. We've made some preliminary contacts with ecosystem ecologists and modelers to possibly help us with identifying what some of the issues are. And we started the process of identifying information needs that could be provided by other project participants, say related to hydrology, geology, those types of information needs.

Some of the presentations this morning I think will be useful in helping bring out some ideas and issues, information that will be useful.

So in summary, the major change has really been a refocus of efforts and allocation of resources. We'll increase some emphasis on monitoring direct impacts, tracking the disturbances that are occurring out there. We'll continue our efforts to get the vegetation associations mapped out there. We'll be reducing our efforts on monitoring the site characterization activities, the construction activities out there, probably by about two-thirds roughly. And then last, we'll be initiating efforts to identify some of the issues related to long-term repository effects and things possibly needed for the EIS.

So with that, I'll open it up for questions.

DR. BREWER: Thank you, Ron. Questions?

DR. CANTLON: Yeah, John Cantlon, Board.

Ron, as you know, the Board has been interested in seeing an increase in the relationship between the various dimensions of the study going on out there, and you're talking about now concentrating on mapping the vegetation types.

MR. GREEN: Right.

DR. CANTLON: It's obvious that the hydrologists, their unit of work are watersheds, and I would ask the question, to what extent will your mapping activity now be done in the context of an overlay for the hydrology studies so that the working unit, the functional behavior of that site in terms of long-term repository performance, will have real working interplay between the surface ecology in the functional hydrology units?

MR. GREEN: Right.

DR. CANTLON: Is there some thought being given to that?

MR. GREEN: Right. We have just initiated the--well, in terms of scoping of how we're going to do the mapping work. And so that can be included in that process. We haven't come up with a final approach to mapping vegetation. I mean, we have some existing information. Well, the associations were mapped already, but we haven't come up with detailed procedures of how we're going to go through this process of mapping vegetation, and that certainly can be included.

We heard yesterday that USGS was producing a soils map. That will be useful. Maybe some discussions with USGS regarding watersheds can be incorporated into that process, and we're early enough in that task that we can make those considerations.

DR. CANTLON: And a follow-up question. Cantlon again. If you think about it in terms of the areal display of data and integrating them, then you have a second component of that, which is the functional interplay. The hydrologists in a sense estimate evapotranspiration almost as a function of difference. If you can get at some point in your study some real measurements of the evapotranspiration as real numbers that relate in some way to different vegetation types, now you've linked your data sets in functional ways and in the areal overlay.

MR. GREEN: So what you're suggesting, rather than just using species composition--

DR. CANTLON: Right, exactly.

MR. GREEN: --which we typically--you know, plant ecologists typically do, is to look at some of the functional processes and define associations based on some functional relationships.

DR. CANTLON: Right, exactly.

DR. BREWER: Other questions? Yes, Jim?

DR. EHLERINGER: Jim Ehleringer, consultant. Two quick questions, or one is actually a point, and that is in your monitoring program, my impression is that you're monitoring by per cent cover. And I'm particular interested in making sure that we can have a linkage ultimately made with the geological hydrological infiltration components, and there we recognize that per cent cover may not be adequate. We need to break things into functional units. And so I would encourage you in your data collection to make sure that you could break your cover into functional units.

MR. GREEN: Okay. Functional by species?

DR. EHLERINGER: Species, life history, a variety of characteristics that might have a functional--

MR. GREEN: When we record cover data, we record it by species.

DR. EHLERINGER: Okay.

MR. GREEN: So we have per cent by species. So we could collect, say if you wanted C-3s versus C-4s, or annuals versus perennials.

DR. EHLERINGER: Well, I think--

MR. GREEN: Or are you suggesting that we have another measure besides cover?

DR. EHLERINGER: No, what I'm suggesting is the possibility that long-lived perennials might have a different life history, a different water extraction zone and so forth in short-lived perennials.

MR. GREEN: Okay. And that we should collect our data such that we can separate those various components out?

DR. EHLERINGER: Correct. The other thing, and this in part reflects my background, is that monitoring per cent cover is a very static measure, and very little in what you're collecting relates to process. And the analogy is to look at the number of people in this room here, and then later on look at the number of people in this room, and we might see that the number has changed. We don't know anything about the health of the individuals in this room by monitoring only presence or absence, and the same could be true for the system, the ecosystem.

So I don't detect that there's any measure of water stress, of CO_2 exchange, respiration and so forth. And I'm not asking you to collect those measurements, just to be aware that you could learn a lot about what's going to happen to the system by looking at its metabolism.

MR. GREEN: So possibly, maybe continuing on, some of the shrub density where we're actually mapping individuals and have histories on individuals could possibly contribute to that; is that--

DR. EHLERINGER: Possibly.

MR. GREEN: Yeah, okay.

DR. BREWER: Any other comments or questions?

(No response.)

DR. BREWER: Ron, thank you very much.

One of the main focal points for the panel's consideration this morning and in general is the relationship between thermal loading strategies below ground and what happens above ground. And we have invited Dr. John Harte to present some of his work on soil warming in the system, not Yucca Mountain, but in a montane system. What are the effects of actually warming the soil over periods of time?

John is a professor of soil science at the University of California at Berkeley with appointment also in the energy and resources group. John's presentation is studies of thermal effects in a montane system. John?

DR. HARTE: Thank you very much. It's a real pleasure to be here and have a chance to address this panel and the public.

Six years ago I became interested in trying to understand the possible effects of global warming on ecosystems. Now, global warming is a top down sort of heating process. The atmosphere is the source of the radiant heat from the carbon dioxide that will warm soils, plants, and to simulate global warming. We set up an experiment in a subalpine meadow in the Colorado Rockies in which electric heaters were placed above a meadow ecosystem, and over several years now, we've been monitoring the effects of this heat source on the ecosystem.

What I'd like to do is describe how we went about this study, some of the results that we've obtained, and then at the end of my comments, I want to suggest some ways in which the experimental strategy we developed could be applicable to the Yucca Mountain site, but with one major modification, which I will talk about, which takes into account the fact that nuclear waste heat source is a bottom up heating of the ecosystem, not a top down heating.

This just shows the cast of characters, my co-PIs from UC Riverside, post-doc, a number of doctoral students and other student assistants. The funding for this project comes primarily from the National Science Foundation, and the annual cost of this project has run over the last three years at about $150,000 a year, most of that from the NSF.

The major questions that we've set out to answer in this project are to characterize the feedback mechanisms that act between soil, microclimate and vegetation, and how those feedback mechanisms will influence ecosystem response to global warming.

The other issue here is to what extent global warming might itself be influenced on a large scale by how ecosystems respond to climate change; that is, for example, if global warming causes a net increase in decomposition processes in soil, that could lead to large additional fluxes of carbon dioxide to the atmosphere, which would augment the carbon dioxide perturbation from the burning of coal, oil and gas.

So ecosystems have the potential to either amplify or mitigate global warming, and we wanted to understand to what extent that might actually occur.

Now, to really understand that, you have to do experiments on a much larger scale, and, of course, we're doing that as a society over the next 100 years. In the course of warming the planet, we will be learning about these feedback mechanisms. But what we're trying to do here is on a much smaller scale of a subalpine meadow to see what kinds of effects occur, and then through various techniques of scaling and extrapolation, to try to understand what those results on the small scale might portend on a global scale.

The site characteristics are shown here, the location in Gunnison County, Colorado. We're working up at an elevation of about 9,600 feet. Unlike Yucca Mountain, we have a lot more precipitation than four inches a year. We have about 28 inches of rain and snow per year. Most of it is snow. The snow-free season is typically June 1st through November 1st, and the only other thing here I want to mention is that we have 10 experimental plots, and they are each 3 meters by 10 meters. That's about 10 feet by 30 feet in size.

We have a very ecologically diverse site. There are approximately 80 species or angiosperms of forbs, graminoids and shrubs, with almost all of those plants long-live perennials.

We're measuring a number of characteristics of the site, soil temperature and moisture at three depths and 90 horizontal locations every two hours, carbon dioxide fluxes, methane fluxes, carbon stocks, plant productivity, recruitment, distribution, the phenology of vegetation. That means the timing in the annual cycle of the vegetation, when do they set seed earlier than that? When do they flower and bud? And how is that influenced by the warming?

Nitrogen pool sizes and turnover rates. Nitrogen is a limiting nutrient in this ecosystem, and so we're interested in whether climate change could alter the availability of this critical nutrient.

We're looking at the soil mesofaunal, the little bugs in the soil that are very important in turning over the litter and recycling nutrients.

We're measuring the xylem pressure potential of shrubs. It's a physiological measure of water stress in plants. Leaf temperatures, piezometer readings, and actually a number of other things, particularly a number of meteorological variables that aren't listed here.

Here's a picture of the site. You can see the overhead heaters. They're about eight feet above the ground, and they supply a uniform flux of approximately 18 watts per square meter over the ecosystem.

And one of the things that I want you to notice on this slide is the fact that we're dealing with a transition zone of vegetation. We have Artemisius sagebrush up here in the tops of each of the plots, and as you go down the slope, you get into a wetter area with a very distinct transition in the characteristic vegetation.

The plots are remarkably, or they were I should say, before we turned on the heaters in terms of distribution of vegetation, soil characteristics, microclimatic properties and so forth.

Here's a schematic of the experimental design; ten plots. There's about a 10-foot gap between each plot, which effectively prevents meteorological influences of heated plots upon control plots. The heaters shown here, the locations of the moisture and temperature probes and just a simple schematic of the vegetation that illustrates an increasing density of vegetation as you go down the hill. This plot, by the way, greatly exaggerates the slope. I mean, the picture exaggerates the slope.

One of the things that we've learned from this study, by the way, is that the vegetation play an enormous role in modulating the response of the soils to the heating. You see here one of the two Campbell scientific data loggers, which automatically record 180 soil moisture and temperature values every two hours. I'm in the process of downloading onto a lap-top the data. We have to do this every several weeks, and pull off about one-and-a-half megabytes of data every two weeks, which then can go into a big central database where we can analyze it.

One of the results that we've seen, which is now part of a publication which is in press in Ecological Applications, is a remarkable and totally unexpected strong diurnal cycle in the temperature difference between the heated and the control plots. What you see up here is totally ordinary and expected; namely, in the control plots, there's a diurnal cycle of soil temperature. Nobody would be surprised by that.

What was surprising was to see that the daily data, the two-hourly data over--this is one typical week in the summer of 1991. The two-hourly data exhibit a sharp increase in the temperature difference between heated plots and control plots. Having seen it, we quickly figured out the explanation. For the experts in the audience, it's a Bowen ratio phenomena. The heaters dry the soils of the heated plots, dry soils when they're subjected to sunlight, raise their temperature more than wet soils because the sunlight hitting wet soils, a lot of that solar energy goes into evaporation rather than raising temperature.

And we now have a fairly simple soil microclimate model, which quite nicely simulates this sort of behavior. But it was something that took us by surprise, and, in fact, when I talked to the people doing the general circulation models of global climate warming, this effect wasn't in their models, and they now realize it should be, and it wasn't because they didn't have a realistic enough model of soil hydrology in the interplay of energy and water balance in soils. But it's totally understandable in retrospect.

Another result that I want to show you is a rather dramatic effect on soil--I'm sorry, on vegetation. What you see here are three classes of vegetation. Shrubs are dotted lines. Dash lines are the grasses and sedges, and the solid line are forbs. And the control plot forbs did much better than the heated plot forbs. This is Julian date, so here's July 1st roughly. This is the summer of 1993.

The difference between the control values, which all emanate from this date, and this one for the heated values, that two-week difference is because snow melted two weeks earlier in the heated plots. So these start out earlier. Despite their earlier start, the forbs don't do as well. The forbs are the leafy plants without woody stems, and they don't do as well in the heated plots as in the control plots.

The woody shrubs, however, in this case Artemisia, the sagebrush, does considerably better in the heated plots than in the control plots. And the grasses do about equally well in both. We have a lot of supporting data from various locations within these plots to back up that result.

This just shows two of my students with a CO_2 chamber. This chamber is placed down on a plot, and held there for about a minute, during which the carbon dioxide level in that chamber either increases or decreases, or maybe stays the same. If it increases, that means the ecosystem over that square meter was pumping CO_2 into the atmosphere from soil decomposition and plant respiration. If the CO_2 in the chamber decreases, it means that photosynthesis was taking up carbon--was removing carbon dioxide from the air and incorporating it into the vegetation.

What we've done now over the course of about four or five months is to measure on a diurnal basis, around the clock every ten days, the CO_2 fluxes from all of our plots. And what we find is a very interesting story, and this is just one particular day in August in which what you see up here, I believe that's the wetter zone of the plots, and I believe this is the drier zone. Let's look at this one for a minute, the lower one, please.

What you see here, if you could just raise that up a little bit, carbon flux time of day, midnight, 4:00 a.m., 8:00 a.m., 12:00 noon, 4:00 p.m. Yeah, 4:00 p.m. and 8:00 p.m. And what you see is that in the heated plots, carbon was not stored in the ecosystem as effectively as in the control plots. The control plots showed net carbon gain over the course of that day. The heated plots showed net carbon loss. This is just one day of many in which we did the measurements. On numerous days, especially in July, in the heated plots, by noon there was net loss of carbon. In other words, the plants in the heated plots were respiring, not photosynthesizing on that over that hottest time of day.

If you add up over the whole summer the change in carbon storage in our heated plots compared to our control plots, there was a loss, a relative loss of 100 grams of carbon per year per square meter in the heated plots compared to the control plots. That's a lot of carbon. And if you multiply by the area of the earth with this kind of habitat, it adds up to something on the order of a billion tons of carbon per year, and that's interesting because the total accumulated increase in the atmosphere from fossil fuel burning is only about two-and-a-half or three billion tons of carbon per year.

So what we're seeing is the potential from global warming for a sizable positive feedback to the carbon cycle effect on climate change with warming causing ecosystem responses which enhance warming.   And we've now carried out these studies through the winter as well. We even see this effect to a much more subtle extent over the snowpack in fall and spring.

Another one of my students with one of our methane chambers. We're looking at the extent to which warming affects the consumption of atmospheric methane. Methane is a very important greenhouse gas, the second most important greenhouse gas after carbon dioxide. And one of the characteristics of our site is that the soils are consuming methane. All throughout the grasslands soils of the world, you find methanotrophic bacteria which consume atmospheric methane, and it's an important process because the total rate of consumption of methane by this natural process is about equal to the current rate of increase of methane in the atmosphere. In other words, if something happened to this methane consumption process, it could double the rate of increase of methane in the atmosphere.

And what we've found in the course of our work is that warming does indeed have a strong influence on methane consumption in our soils.

Here's some of the major conclusions of the whole experimental setup so far. We've seen a significant midday peak in incremental temperature in the heated plots. We've seen snow melt advancing by 10 to 15 days per year because of heating. Early snow melt has an enormous effect on the summer moisture regime in the soil because a good deal of the whole annual input of moisture to this ecosystem comes with snow melt. If it comes two weeks earlier, that means by mid-summer, it's been drier for two weeks longer, and that can have a major effect on soil moisture, and, therefore, on plants.

We've seen shrub production enhanced, forb--it shouldn't say "and grass." It should just say "forb production depressed." Vegetation phenology significantly affected by the heating, but in a way that's highly species dependent. And we've also noted that although the flower budding, flowering and seed set are advanced by heating, the duration of the reproductive cycle is not speeded up. In other words, the time between flowering and seed set, for example, is unchanged. So the internal clock of the plant stays the same, but it's responsive. Everything happens sooner under heating.

Soil mesofaunal diversity and abundance were enhanced by heating in 1992, which was a very wet year, and during 1993, it was depressed.

We found net carbon loss from our heated plots compared to our controls, and we found that methane consumption rates in the soils are maximum at intermediate soil moisture levels and that heating can dry soil moisture to levels that are below the maximum methane consumption rate. So as heating progresses and soils keep drying, the methane consumption rate was observed to decrease.

I think that's the end of my slides. And I just want to conclude with a comment. I want to conclude with just one comment about the applicability of all of this to the topic in hand.

One of the things that we have found to be absolutely critical in what we're doing is measurement of physiological processes, and in particular, such measurements, which I haven't talked about here because I don't have time, but the xylem pressure potential data that measures plant moisture stress, the characterization of vegetation into groups, understanding better how different groups of vegetation take advantage of either more drying or less drying, more water. The processes are very different for these different classes of vegetation, and even within the forbs, there are a number of very interesting phenological differences.

So I want to stress, as was also stressed by Jim Ehleringer in his comments to the previous speaker, the importance of doing--and by John Cantlon--the importance of doing functional and process-type measurements, not just vegetation cover measurements.

The second thing I want to say is that the top down heating that we're doing, while it's totally appropriate for a global warming experiment, it's probably not the most appropriate way to go about studying effects of heating from an underground nuclear waste.

And what I would propose, I won't be here this afternoon to talk about this in more detail, so I wanted to get it out now, is I don't think it would be terribly difficult, and I think it would be very exciting to think about the idea of tunneling under, maybe only at a depth of 10 or 15 feet underneath an area of Yucca Mountain, the Yucca Mountain ecosystem, and placing in that tunnel an electric heating source, a large electric coil, and letting the heat dissipate up to the ground. You don't want to go as deep as this test tunnel because it's too deep and it will take too long to see the effects at the surface. But you don't need to if you're only trying to characterize the ecosystem response as opposed to the geologic response.

So to characterize the ecosystem response to heating, what I would recommend is tunneling in under maybe 10 feet below the surface, 15 feet. Such tunnels I believe can be done safely and fairly inexpensively and quickly, getting a big powerful heat source in there, and then characterizing the very same kinds of things that we're looking at here at the surface. And I think within a year or two, you'd start to see some very exciting effects, and I think they'd provide the very best clues that you could possibly get to have the Yucca Mountain ecosystem as going to respond to buried waste.

Of course, you won't learn what happens over hundreds of years in a five-year experiment, but as I hoped this has indicated to you, you can start to see some very dramatic effects in even just the first or second year of such a study.

Thanks.

DR. BREWER: Thank you, John, for an interesting presentation. Are there questions? Yes, Jim?

DR. EHLERINGER: Can I ask a broader question? This is in part, just for the record. Could you tell us about other groups that are doing similar ecological types of experiments?

DR. HARTE: Yeah, that's a good point. To my knowledge, we're the only group doing top down heating of an ecosystem with the kind of suspended heaters that you've seen here. There are several groups that either have been or are now doing a different kind of ecosystem warming experiment, using buried electric resistance wires. There is a group that's doing such a study in the Harvard forest in Massachusetts; Fahkri Bezzaz, a professor at Harvard, Jerry Mellilo and others who are involved in that. And it's done with a forest where it would be very difficult to suspend heaters above the canopy, or if you do it underneath the canopy, then you're sort of doing something a little peculiar also.

And so they chose to forget about the vegetation; we won't look at direct effects of heating on leaf temperature, transpiration and so forth. Instead we'll heat the soil and focus attention on nutrient dynamics.

So that's been the main focus of their study. That started about the same time ours did.

And there's another study that Van Cleve had done some years back in the Arctic, and actually, I think it was in an arboreal forest with underground heating wires.

And there's a third underground heating wire experiment in the country of northern Sweden.

I don't think underground heating wires are the right way to go in the Yucca Mountain site. I think that you can't get the heat fluxes you want with that kind of thing, and you also get very irregular patterns of heating because you get zones of heating right near those wires that are quite high, and I think you'd be better off with something more along the lines of what I described. But that should be discussed.

DR. EHLERINGER: One last question, and that is are there natural or anthropogenic analogs, such as coal fires?

DR. HARTE: Yeah, there are all kinds of ways to look at natural climate gradients and ask how do the vegetation or the nutrients, or whatever, vary along those gradients.

One of the things we found in our experimental site is we have three other gradients we can look at besides the difference between heated and control plots. First of all, there are hot years and cool years, dry years and wet years. So we can contrast vegetation growth across years.

There's also a zonal difference. When you go from the tops of our plots to the bottoms, there's a sharp temperature and moisture gradient.

And finally, even within just the tops of the plots and within just the controls, and within a given year, there's natural spatial heterogeneity. And what we have found is that those natural gradients are a very poor indicator of how the ecosystem processes respond to the heating. In other words, looking across natural gradients can be very misleading. I can talk to you more about that in detail later, but I'm very cautious of it.

One of my students did a study of white fir in the Sierra Nevada using common garden experiments where trees from eight different source sites are all transplanted to common gardens--they're not really gardens, but they call them that--and then their growth in a climate regime that's different from the one they started out in and studied. And what she found is that genetic differences among the different source sites has a huge effect on the response to the shift in climate through transplantation.

And that's one of the problems with looking at natural gradients, is there are also gradients in genetic adaptation across natural gradients, and it's very difficult to unscramble that from the effect of the perturbation.

As far as more kinds of catastrophic events, or sharp discontinuities due to hot springs and so on, either natural or man-made, there are a number of opportunities. We were talking yesterday briefly about the Alaskan Pipeline as a heat source because you heat the oil through the pipeline. The trouble is that the area around that has been so disturbed by people for right-of-way, maintenance and so on, that it would be a very poor place to try to learn something about effects of climate on ecosystems.

There are undoubtedly other ways to go about doing this, and I'm sure a little bit of cleverness will uncover some interesting prospects. But you do have to be very careful when you deal with long-term--with gradients that have been there for a long time because of genetic adaptation.

DR. BREWER: John, thank you very much for a very interesting presentation.

We're going to break now and reconvene at 10 o'clock promptly.

(Whereupon, a break was taken.)

DR. BREWER: Our next presenter is Tom Buscheck of the Lawrence Livermore Labs. He's been modeling the transfer of heat away from the repository to the surrounding geologic strata.

We started from the top down literally with John Harte, and now we're going to take it from the bottom up.

Tom Buscheck of Lawrence Livermore.

DR. BUSCHECK: I have to say this is the first time I've given this talk, and I haven't been able to work on the compact version of it yet. I'm also very happy to see that there's a growing list of heater test advocates out there.

I'm not going to talk about this slide as I have at about 10 other meetings. I'm just going to point out the major units from it.

We basically have welded units, which all end in the letter W, which are presumably a lot more fractured than the units that end in N, the non-welded units. And the non-welded vitric units are these two green units, and especially the PTn is considered to be quite significant. It's been discussed a lot in previous meetings. It is presumably acting like a large sponge, which prevents liquid flow in fractures from getting down deep in this fractured system at Yucca Mountain. But as we'll see later in this talk, this unit also could be a dramatically important vapor flow barrier at Yucca Mountain as well.

So when I talk about the PTn, think about this unit that's about 30 or 40 meters below the ground surface.

And I'll just throw on this nice view graph while I talk about this slide.

Basically, they're the major heat flow mechanisms in the unsaturated zone at Yucca Mountain. The primary mode of repository-driven heat flow is heat conduction. Even if convection eventually plays an important role, convection could only play an important role of conduction, first perturb the hydrologic system to allow that to occur.

We find that temperature rise at the ground surface will be no less than that predicted by "heat conduction only" models. That's basically the bottom disturbance that one would expect.

In addition to heat conduction, heat flow to the ground surface may also be enhanced by two-phase convective effects, possibly arising from one, two or all of the following effects: the two-phase heat pipe effect, mountain-scale, buoyant, gas-phase convection, and also, these are mechanisms, and then the features of the mountain, the heterogeneity and whether the heterogeneity may give rise to vertically contiguous fractures that are more permeable than the surrounding rock, may give rise to focused vapor flow, which could have an important impact on the ground surface temperature rise.

The third and first effects generally require the presence of boiling conditions. And as I stated, convection enhanced heat flow to the ground surface requires that high permeability fracture pathways are well connected over large distances, at least several hundred meters in extent, in order for the effects of that vapor flow to be significant.

And what I've shown over here on this other machine basically is what I'm about to talk about, mountain-scale convection, focused vapor flow. We also have buoyant convection occurring in the repository itself, but that doesn't have any impact on heat flow at the ground surface per se.

I just want to briefly talk about the models we've used to do this analysis. We're using, as we have over the last eight years, the V-TOUGH code, which is Lawrence Livermore's version of Karsten Pruess' TOUGH code that was developed at Lawrence Berkeley Laboratory.

We're also assuming an equivalent continuum model, which cannot explicitly represent non-included fracture flow. However, for this analysis, that is not a limitation. I think this very, very adequately represents the gas flow and the fact that liquid flow may occur and non-included fracture will not have much of a bearing on how that gas flow emanates.

Our models, because of--well, our models basically assume that the hydro-stratigraphic units are horizontal and constant thickness. That's a simplification. However, for this type of sensitivity analysis, I think that's not a critical limitation.

For this study, we've used three types of models. There's a repository-scale model, which basically models the entire mountain out 15 kilometers from the center of the repository. We call it an R-Z model because it takes advantage of axis symmetry about the center of the system. And so by using this axis symmetry, we can effectively model three-dimensional effects with a two-dimensional model, and that's why we cannot have dipping beds. But, however, I think we very accurately represent the heat and mass balance, that we do not take into account topographic effects of the fact of dipping beds.

We also have a repository scale, a vertical cross-sectional model, which can take into account dipping beds and topography, but it's less accurate with respect to long-term heat balance in the system.

And we also have a drift-scale model, which is also a vertical cross-section, which looks at the detail of heating from individual waste packages. The initial vertical temperature saturation and pressure profiles correspond to the geothermal and the pneumatistatic pressure gradients. And we've assumed a variety of recharge fluxes. The thermal loading history generally occurs with instantaneous heat starting at time equals zero.

I'll start with the basic mode of heat flow; that is, heat-conduction-only. What I'm plotting here is--and I was--this came about through various conversations I had. I heard there was a lot of interest of looking down at the base of the root zone, and I was told that was approximately three meters in depth. So most of what I'll show today is looking at the temperature disturbance three meters below the ground surface.

What I'm plotting here is a maximum temperature rise three meters below the ground surface as a function of areal mass loading. And for point of reference, the SCP thermal load is right around here at 50 MTU per acre.

This is what people have called the 114 kilowatt per acre case. It's about 155 MTU per acre. What we find is over a range of thermal loads, this linear increase, from about, in this case, .3° up to about 1.2° rise, a linear increase in that temperature rise. But as we go to higher areal mass loadings, because we have a fixed number of waste packages, the repository becomes smaller and smaller. So eventually, in this range the repository effectively acts as thought it's infinite in areal extent, so the maximum temperature effect does not feel the edge cooling effects.

But as we go to higher areal mass loading, the edge cooling effects do impact the center, and we no longer have a linear rise with respect to areal mass loading. That's why you see tapering off of it up here.

What I'm plotting here is for three rather extreme cases--not extreme cases, but a range of cases. The temperature rise, three meters again, three meters below the ground surface as a function of radial distance from the center of the repository. And I realize it's hard to translate radial distance to area, so at the last minute I also included the area enclosed by that location so you get a feeling for how much of the ground surface would be disturbed.

And you can see that we got a more compact, more pronounced disturbance naturally for higher mass loading, and for the lowest areal mass loading we've considered, it's a much lower disturbance, but it's spread over a considerably greater area.

Incidentally, these are the ranges of cases that are currently being considered in the thermal loading systems studies. So a lot more of your results you're going to see have these types of MTU per acre. We've been focusing on a more, kind of a unified approach with the designers in terms of what options we're modeling.

To start again with the heat-conduction-only case, what I'm plotting here at 200 years after emplacement is a temperature disturbance for the heat-conduction-only case here on the right side. What we find is no two-phase effects, that we have a very uniform temperature gradient away from the heat source. What I'm plotting here is a temperature rise above ambient. Because we have a geothermal gradient, we plot temperature differences, not absolute differences.

And you can see at 200 years of thermal disturbance effectively has not reached the ground surface yet. I'm also plotting the gas phase field which is imposed by the system. So even though it's "heat-conduction-only," there's actually two-phase effects going on. But because of the low permeability, we do not dry out very much rock. And this shows the dimensional saturation profile for that case of 200 years. Even though there are no fractures, we did dry out a small amount of rock and develop a condensate zone below that dry-out.

Now, the next effect in terms of enhancing heat flow is what we call the two-phase, heat-pipe effect, and others have looked at this as well, Karsten Pruess and Yvonne Tsang and Lawrence Berkeley Laboratory and others, and what we find in this case, we're in an intermediate range of permeability where the permeability is not high enough for mountain-scale, buoyant convection to dominate the hydrothermal behavior. However, it is large enough to allow heat pipes to develop.

And so we can see a region here with a very flat temperature profile. This is where heat flow is substantially enhanced by virtue of the counter current flow of water vapor away from the heat source, condensate draining back to the heat source.

And we even see that below because there is some capillary flow coming in back, and the small aperture fractures back to the heat source.

You can see that in that case at 200 years, we've already dried out, oh, about 120, 130 meters of rock, and there's a saturation buildup above and below. But because mountain-scale convection is not important at this range of permeability, we see that the vapor flow is symmetrical about the heated horizon, and we get a very uniform distribution of temperature and saturation changes.

We can look in more detail at the temperature profile as we go from a conduction-only profile to one in which the heat-pipe effect enhances that heat flow.

Here is the conduction-only profile. You see a very uniform temperature gradient. This is now at 400 years. We find that the ground surface generally starts to respond at about 300 years to heating effects for the heat-conduction-only case. With the heat-pipe effect, because of the very high effect of heat transfer coefficient in this region, it's as though, as far as heat flow is concerned, that we virtually remove this amount of rock in terms of the rock being an insulator between the repository and the ground surface. And so we get an enhancement, and so we find that instead of the maximum temperature rise requiring 1,500 to 2,000 years to reach the ground surface, it requires about

800 years when it's enhanced by the heat-pipe effect. The duration of time is roughly cut in half by virtue of that effect.

What I'm going to have to--I'm going to have to jump ahead in one of my slides because I forgot the superimposed, the heat-conduction-only calculation. And so ignore all these other curves. They come later in the talk.

The purple curve is the heat conduction only curve. As I said, it peaks at about 1,600 years at about 1.3 or 1.4° above ambient at three meters depth. And out here at 5,000 years, it's about .8° above ambient.

Now, these two curves are for the heat-pipe enhanced case. The red curve is when we use a relatively low gas phase diffusion coefficient. Basically, that's probably as low as we could consider. Others at LBL, have--and we have also looked at the soil literature and found that there may be a substantially enhanced gas diffusion coefficient applicable to Yucca Mountain, perhaps as high as two, but there's been very little--there's no experimental evidence to nail that down at the present.

So what we did is we looked at a wide range of this parameter and found that this gas-phase diffusion effect, enhanced gas-phase diffusion, can somewhat enhance the ground surface temperature effect. The enhancement, I think, is relatively small compared to the fact that we're probably not going to measure thermal conductivity within this type of variability. But nonetheless, you get somewhat of an enhancement.

But what's of greater significance, I believe, is the fact that the liquid saturation rise three meters below the ground surface is enhanced by nearly a factor of two. What I'm plotting here is a liquid saturation, and in the model we assume initially there's approximately 70 per cent. That's in gravity capillary equilibrium with a water table.

And you can see that in around 5 or 600 years, the flow of water vapor in the mountain is now reaching the ground surface, and we're starting to get an elevation by virtue of this heating effect.

This is the same curve plotted out to 50,000 years. And so while we get something on the order of about 5 per cent increase normally at this low diffusion coefficient, going to the higher diffusion coefficient, that increase is somewhat greater than 10 per cent.

And you can see out in the long time frame that the disturbance, the temperature disturbance, decays relatively rapidly--not rapidly, but it decays in this fashion. However, the enhancement and liquid saturation of the ground surface persists much longer we find in our models than the overall disturbance in temperature.

And so we feel that not only are there thermal effects of the ground surface that need to be considered, but the fact is, is that the repository is capable of conducting and diffusing a lot of water vapor to the ground surface as well.

So I would like to then move on to the next topic of convection enhancement effects, and that's mountain-scale, buoyant, gas-phase convection. And in your packet, if I go over this too fast, I think it's all very adequately explained with this numbering system I have here. But basically what happens is that as the repository heats this area here, this gas column that sits within the footprint of the repository becomes warmer and less dense than the ambient gas sitting outside it.

So this cooler, denser gas then starts to displace this. This actually works as a chimney effect, and this sort of works as a draft. And this cooler, denser gas moves in, and as it's heated up, its relative humidity is substantially lower, and, therefore, it's able to take water out of the water matrix. It becomes then more heavily saturated in water vapor, and as it moves up through the heated horizon, it then moves down temperature gradient, and then condenses, leaves its condensate along fracture walls, and that condensate, then, may either imbibe in the matrix or condense and flow back through the repository system.

For this particular example, this was from a calculation done for the SCP 49.2 MTU per acre thermal load, and this is a plot I believe at 1,000 years. And at this point, there are no longer any boiling temperatures present. So this process does not require boiling conditions to drive it.

And so we do not know with the cooling continuum model to what extent this condensate drains down through the system. However, that, I believe, would not have a large effect on the major concern we have here and how does that enhanced gas or water vapor flow affect temperatures up at the ground surface.

Another effect that we have found in our analysis is that as this rock dries out, it dries below the gravity capillary equilibrium point, and, therefore, water that is imbibed up from the water table--and if this effect is very pronounced, you could actually add water to the system. But what it will also show is that there's a component of water vapor leaving the system, and so the extent to which these balance is yet to be determined.

Now, you're going to see a very wide range of bulk permeability calculations, and this was done in advance of a lot of information that we're yet to obtain in site characterization. And the primary motivation in this, or one of the primary motivations was the hypothesis by Ben Ross that he thought that we might have a high enough bulk permeability at Yucca Mountain whereby buoyant gas-phase convection would prevent the repository from getting above the boiling point. He put that out there, and so I decided that I would very aggressively look at this problem and see under what conditions that hypothesis might hold true.

And this is not directly related to the ground surface, but I just want to show that, in fact, there is a great deal of dependence of repository performance to bulk permeability. And basically bulk permeability as it increases, this buoyant convective system becomes stronger and stronger. And as you can see for the SCP thermal load, when we get up to about 80 darcy, that we no longer have any boiling. We, in fact, do cool the repository so there are no longer boiling temperatures on average.

Now, that's with a smeared heat source model. When you consider large packages, there will be individual packages above boiling. I can almost guarantee it. But this is just kind of a global sensitivity study for average heating conditions.

For the high thermal load, when we get up to the same point, about 84 darcy, we reduce the duration of boiling by about 40 per cent. So we have not eliminated the duration of the boiling period. And you can see there's a wide, wide range where the duration of boiling is virtually insensitive to the permeability.

Now, I'd like to show always in our studies, and also in this talk, I start with the most extreme just to show the processes, and then as I impose hopefully more knowledge about what we know about the system, we can see the possibilities are going to be constrained.

On the left side we have a situation, and it's where we have 84 darcy throughout the whole system. It's all uniform. It's totally unrealistic in that regards. But just look at the process. We can see that water vapor--gas is coming from the atmosphere. It's sweeping up through the repository and exiting the mountain above the footprint of the repository.

And we see that this has a dramatic effect on the temperature profile relative to what we predicted for the conduction only case. You can see for conduction only--these are both at 200 years. With conduction only, the disturbance hasn't even reached the ground surface. With the heat-pipe enhancement effect, it still hasn't reached the ground surface, but where we have an open convective system, we have boiling temperatures within 50 or 60 meters of the ground surface.

And what I'll also show for this example, the fact that this system, the buoyant flow is so strong that 100 per cent of the water vapor causes 100 per cent of the condensation to occur above the repository. None of the condensation occurs below. And we see an elevation of the condensate all the way to the ground surface.

Now, I'm plotting the dimensionless change in saturation. Because the PTn has a huge initially dry capacity, large porosity, relatively presumably in this model, relatively low initial saturation, we won't see a dark blue area. But, in fact, there's a lot of increase in saturation of the PTn that's not indicated by the dimensionless change. But we have a large change of saturation all the way to the ground surface.

Now, the next thing I did was considered what if the permeability and the non-welded units was substantially less? And in those units, I'm looking at the Calico Hills, all these units, and the PTn. And in this case, these units --this has a 320 millidarcy permeability, and from all my discussions with people in the site characterization program, I think it's fairly reasonable.

And in the Calico Hills, I'm assuming 280 millidarcy, and I think the mode for the data that so far Gary Lecaine's gotten, and there's a study down on the Calico Hills down at Los Alamos, the mode is around .2 darcy.

So the data we have currently, these would be the types of properties that we would expect with the current knowledge we have.

But what we can see is that this acts as a vapor cap. It's like a brick wall. This vapor is not flowing through at this point now. There is a finite amount of vapor flow. However, because of the contrast in permeability, whereas these vectors are going right out the stack to the ground surface, you can see that the convection system is circulating in this fashion.

Now what we find is that water vapor more gradually reaches the ground surface primarily by gas-phase diffusion. So it's a more gradual process, and there's a lot less delivery of heat to the ground surface as a result of diffusion.

I just want to show the temperature profiles for these set of somewhat extreme--not somewhat, probably fairly extreme cases. Again, the focus on the mechanisms of enhancement.

And going back to the previous curve, you can see these again are at 200 years. Two hundred years, this is conduction only. This is enhanced, the heat-pipe effect, but if 100 per cent of the water vapor is flowing upwards, we get no two-phase, heat-pipe zone below, only above.

And, in fact, for this 84 darcy case, this blue case, where we have no cap in the PTn, this gas flow is so strong that we do not even get this heat-pipe zone up to the boiling point. In fact, we have a sub-boiling, heat-pipe zone, which I don't believe--I'm not sure if anyone has encountered this before, but we have an isothermal region of almost 200 meters in extent of about 80°C. What happens is, is that vapor flow is moving so quickly away from the boiling front, the boiling front can't keep up with it. And we're able to establish this refluxing system without the benefit of boiling conditions.

Now, when we put a--we put in this red curve, we've actually reduced the permeability in the Calico Hills, but not in the PTn. We find in that case we actually enhance heat flow to the ground surface. It really surprised us, and I'll have time to explain why I think this is happening. But then the other two cases we've considered, we've also reduced the permeability in the PTn, and in those cases, we have a shallower temperature profile or gradient because of the fact the vapor flow is, you know, by convection is being stopped at this point.

If I'm going too fast, somebody scream.

I just, again, want to show, talk a little bit about repository performance, and this, again, is for a range of bulk permeability from about a microdarcy all the way up to more than 100 darcy. And what we're plotting here is the maximum column of liquid water removed from the dry-out zone as a function of permeability.

The reason I'm doing this is in order for this convective effect to reach the ground surface, we have to be generating water vapor. And so this sort of gives you a measure of how strong that effect might be.

And if you wanted to think about this in terms of rock dry-out, you would multiply these numbers by about 15 because we start initially with about 7 per cent water by volume in the system.

What we find is, is that for this range, with a three or four magnitude range of permeability, we get a relatively flat plateau, and this is where the heat-pipe zone is dominating, but buoyant convection is not. When we get above 1-10 darcy, the effect of mountain-scale buoyant convection starts to enhance the amount of vapor flow that's being generated.

In this case, I've gone all the way down to the SCP thermal load. The SCP thermal load under boiling conditions generates a very, very small amount of water vapor. However, if we go in excess of a darcy, we find there could be a dramatic increase in the amount of water vapor generated by repository heat.

In this situation, and also in this situation, all this dry-out occurs under sub-boiling conditions.

Just to show you another way of looking at the sensitivity of the hydrological impact of heat, I'm almost plotting over the same range of permeability the net amount of moisture added above the repository horizon as a function of permeability.

And what we can see for a high thermal load, the green curve, is that it's pretty much dominated by the boiling conditions. There isn't a sharp break where mountain-scale boiling convection causes a dramatic change in the regime. But at the lower thermal loads, at around a darcy, 1-10 darcy, we see a dramatic change in the behavior. And even for this 27 MTU per acre, which is 20 kilowatts per acre, we find that even though the peak temperature was only 60°C, that if the system is permeable enough, we could generate a huge column of water buildup above the repository. Now, I'm not saying this is likely, but I think it's really worthwhile to look at the systematic sensitivity of the system over a range of conditions just to understand the mechanisms.

Now, I'll keep this curve up, and now I want to get to the curve that's of more interest to this particular meeting.

This is the maximum temperature rise, 3°--three meters below the ground surface as a function of bulk permeability. And we see the same type of sensitivity around the same types of break points, about a darcy or so.

And I want to emphasize that in this type of situation, we need to have a connected bulk permeability that's ubiquitously large. If you have a heterogeneous permeability, what you'll find in this type of convective system is that the low permeability values will dominate whether or not this process is going to occur or not. In other words--I wish I had a larger table. I'll use this smaller figure.

We have a system that's very, very large scale, and if we have any permeability layering or just very heterogeneous permeability, we'll find that what it's effectively allowing this flow will be the harmonic mean of the permeability, not even the mean or the mode arithmetic average, but the harmonic mean.

So this is probably very unlikely, but I think it's worthwhile to show that type of behavior nonetheless, so that when we go underground, we have other reasons to try to characterize the three-dimensional nature of the fracture permeability in the mountain.

I also think that we are going to be able to diagnose whether this type of behavior is relevant or not with the use of underground heater tests, and our analysis shows that we'll be able to determine whether this is likely within two years into a heater test.

But, again, what we find here is that all cases start to take off at around a darcy. Actually, the lower the AML, the less sensitive it is to this effect because we're generating less water vapor on average.

Now, so I'm starting with the most extreme range of possibilities, and then we'll be working our way downwards as we try to impose more reality to the problem.

This is, again, that 84 darcy case, and I'm plotting it at the 5 and 50,000 years. And this is just, again, to look at the mechanisms that are plausible, but perhaps not relevant, you know, given once we learn more about the permeability distribution.

This is the case with 84 darcy throughout the system. What we find is that rather than taking 1,500 years to build up to its peak, it literally increases at around the 200-year time frame, virtually instantaneous--not instantaneously, but over about a 50-year period of time, from zero up to 12°C.

But relative to an underground heater test that we would do, this still may be a rather gradual increase in temperature. You know, 50 years ago, 12°C is still going to be less shocking than the situation of doing it in a year.

It was quite surprising to me, again, that if we didn't--if we reduced the permeability of the Calico Hills, we would cause this type of a spike. But if you look at the temperature changes throughout the system, this caused less of a perturbation than this, if you integrate the temperature delta-ts above ambient over the whole area.

But these convective heat flow processes can grab you by surprise at times, and I think it's been very helpful to look at a very wide range of conditions. So, hopefully, you know, we're not encountering conditions that do catch us by surprise.

What we notice is that this large increase in temperature is also accompanied by almost an instantaneous increase, or over a 50-year period in time, increase in liquid saturation, and we're reaching 98.8 per cent saturation, which is the critical liquid saturation for fracture-dominated flow in the equivalent continuum model.

So at this point, we're generating enough vapor flow just below the ground surface where we're getting a mobile liquid phase in the fractures draining that flow back downward.

When we reduce the permeability in the Paintbrush, we drastically reduce the range of possible outcomes. The purple curve, I've reduced it just in the PTn alone. The green curve, I've reduced it in the PTn and Calico Hills, and in the gold curve, I've reduced it throughout the entire mountain, down to about 280 millidarcy.

And what's interesting is that at a given point in time, it didn't matter whether 5 per cent of the permeability was reduced or 100 per cent.

We also find that when we put this cap in this PTn, that delays somewhat the increase in saturation, but it doesn't prevent it from happening. It gradually occurs. And the reason this is delaying is this delay due to the fact that binary diffusion takes longer than the convection of water vapor up to the ground surface. And even where the permeability is substantially less, we still see a rise in the liquid saturation at that point.

And this is just the look at how persistent these changes are, and in general, you can see that the temperatures are more spiky, but the change in saturation is much more persistent.

Now, I'm going to try to focus in a little bit more in hopefully more realistic scenarios, and these are the cases--all the cases where the PTn had a permeability of 320 millidarcy. And this is the case of heat-conduction-only. What we find is that within 1,500 years or so, all the cases here are within a relatively narrow range of impact. And so even though we've only reduced the permeability in this case, only 5 per cent of the mountain has a reduced permeability versus being reduced everywhere, we get very similar temperature behavior. And what this tells us is that the PTn is an extremely important unit to characterize with respect to this permeability and how the buoyant convective impacts at the ground surface might be felt.

If we can reduce the uncertainty about the permeability in the PTn and show that it's less than about a darcy, we can greatly constrain the range of possible outcomes that we would have to include in doing our analysis, say for an EIS.

And you can also see that for a 50,000-year time frame, there's also a relatively narrow range between the conduction only, the purple curve, and these other cases, where most of the mountain was quite a bit more permeable. And we do see persistent saturation changes in the cases where a lot of the mountain did have a large permeability, but for the case of the smaller permeability, we see lower perturbation in the saturation.

And the heat conduction only case actually does perturb the saturation because that's only a loosely-applied term. We actually do have vapor flow in that model. So there's a finite amount of enhancement of liquid saturation.

I'm just going to pound home at this point with one more example about the importance of understanding the permeability in the PTn. In these cases, I'm going all the way up to 168 darcy.

This is the temperature rise at the ground surface, and what we find is literally a four or five degree range in possible outcomes. And I forgot to plot the 168 degree case down here, but it plots right in this family of curves. We find that there's like a quarter of a degree variability.

And so, again, information--about 5 per cent of the system takes the range of possible outcomes and reduces it by a factor of more than 10.

And also out here, you can see the same effect out at the long time frame. Understanding that the PTn may be a vapor barrier is extremely important in constraining what may happen at the ground surface.

Now I want to move on to the topic of heterogeneity. This particular slide was--I was planning to do this work, but it was partly motivated by the last review Board meeting on thermal loading done last August when Don Langmuir said he really finds all these calculations with average heating and average condensate return flow as interesting, but, you know, we know that it's heterogeneous, and what about that born-loser waste package that unfortunately gets all the condensate?

And so we've worked our level best at focusing as much condensate in very small reaches of the repository as possible because we want to test various hypotheses, both high and low thermal loading, but this was more motivated initially by high thermal loading. You know, if you have this average above boiling system, can you say that above boiling system is dry? Under what conditions would it not be dry?

And so what we did, we used both drift scale and a larger repository scale model to focus condensate flow by virtue of focusing vapor flow, and the way we do this is we have a high--this should be k_b zone. Everything's fractured in this model. And we have a high k_b zone that's intersecting that poor waste package, and here's a neighboring package that was just more fortunate than it. And this permeability out here has been substantially reduced.

So as we heat the system, the gas pressures in this part of the system become much higher than the gas pressure in this chimney. And so we get preferential vapor flow--almost all the vapor flow seeks that pressure sink. And also, the drift itself acts as a manifold, which enhances the ability of that gas in the less-permeable rock to get up the chimney. So we can be also focusing flow by virtue of the effect.

And so when we model this thing in three dimensions eventually, we need to be looking at the third dimension of focusing as well. In this case, the model is two-dimensional, so it's infinite in this plane.

So we have all this enhanced vapor flow reaching the zone, flowing up, condensing and draining downward, and we have some enhanced condensate drainage downwards as well.

In general, we find for focus--well, we're talking about focusing due to vapor flow. We could also talk about focusing due to condensate drainage, but for this particular problem, it would not impact ground surface temperatures. We're more worried about what happens to this water vapor that's coming up the stack.

In general, there's several factors that influence the degree of vapor flow and condensate flow focusing, and the possible persistence of two-phase refluxing conditions in the vicinity of waste package. First, focusing requires that there be a minimal bulk permeability in this nominally fractured rock. Otherwise, it wouldn't generate a significant amount of water vapor.

Focusing also requires a substantial contrast in bulk permeability between the high k_b zone, bulk permeability zone, and the low k_b zone. And then focusing also increases with spacing between these high k_b zones.

We started with looking at the high thermal load and found that for some period of time in this example that the refluxing zone did break through the what otherwise would have been a dry zone without the benefit of focusing. But in time we found that other parts of the system that weren't seeing this focusing were redirecting their heat flow into that area. What happens is that this zone works like a cooling fin, a preferential zone of heat flow. And so other regions of the system where the heat flow is heat conduction dominated and not getting all that additional condensate flow, in fact they're getting less condensate flow because of condensates flowing to this zone, this area, are able to adjust this cooling fin. And so the heat flux vectors are now coming into this zone, and eventually if there's enough heat, you can begin to overwhelm this focusing effect.

And that we find, like within nine years, the water is no longer potentially dripping on the waste package, and in time, this redirected heat flow is overwhelming this tendency for focusing. But while the heat flow is adjusting itself, it's continually going into the zone. Vapor flow is continually going into the zone. Rather than impacting the waste package, it's going up the stack, and it's greatly accelerating its progress in reaching the ground surface.

Now, I just want to show that we've done this analysis across the range of thermal loading conditions because there have been some time quotes attributed to other talks. For instance, the heat-pipe effect was attributed as being the fatal flaw of the hot repository. So I want to see if it's only the hot repository where it may possibly be the fatal flaw.

And I looked at the SCP thermal load, 49 MTU per acre, and for the same exact conditions, except we're using a lower heat output locally, we found that dripping at the top of the drift could persist for about 65 years. It takes longer because there's less heat coming out of the neighboring packages. The mean boiling front is moving very,

very much more slowly so the trajectory of heat flow is less favorable for bringing heat into this born-loser package. Whereas here, the mean boiling front is moving way out; there's more of sort of a catchment area that heat flow can be directed into this area, and so we see a longer persistence of dripping. But eventually in this example, there is enough heat flow to prevent that refluxing from coming all the way back down on top of the packages.

Now, I made one subtle change. I thought, well, maybe they can't get 10-year old fuel in the way they assumed in the SCP. What if that fuel is 20 years old? And so we changed the heating characteristics by aging the fuel.

In that case, the fuel, instead of boiling for 660 years, the region was only above boiling for about 159 years by that very, very subtle change in heat output. The other thing the subtle change in heat output did was it did not allow enough heat flow to come in here, and so this heat-pipe refluxing zone persisted, and it was never eliminated by virtue of focusing heat into this region. In fact, we found a mobile liquid phase in the fractures for at least 1,575 years in the system well after it stopped boiling.

So the effective heterogeneity and the effective flow-focusing and heat-pipe effect are important whenever you put heat-producing waste in the mountain.

And just to show--well, I'll dispense with that.

Now, going back to my philosophy, looking at extreme cases first and then moving on inward, I'm looking at an example, not with a drift scale model, but with this cross-section or repository scale model, and in this case we can model heterogeneity that occurs at a scale which is much larger than a drift spacing.

And in this case, we're assuming that these fracture zones or high k_b zones are a thousand meters apart. In fact, given the repository dimensions, it's almost impossible that you could have these large zones be much further apart than a thousand meters because you'd be off the side of the repository somewhere.

So what we have is we're following the temperature profile down the middle of this zone versus 500 meters away in the nominally-fractured rock. What we find is at 50 years, we have this very large heat-pipe zone, and, you know, potentially what conditions near the waste packages in that zone get out there 500 meters away, everything as though it's conduction dominated. There's no influence of this heat-pipe zone.

Within about 100 years, there's enough focusing of flow into the zone, heat-flow now, that we start to overwhelm the refluxing effect, and we start developing a dry-out, seen, as this split in the temperature curve. At 200 years, the curve in the heat-pipe zone and the nominal curve are approaching each other, and we can see at 1,000, 2,000 and 10,000 years, you get to a point where the temperature profile pretty much doesn't care whether you're in that zone or you're 500 meters away.

So at this point, the repository performance itself is virtually invariant with respect to proximity to the zone.

Now, what I want to show is the fact that at the ground surface, the performance is not invariant with proximity for some period of time. And this is, again, probably a very extreme example where the high k_b zone has a permeability that's 8,400 times greater than the 1,000 meters adjacent to it. But, again, it's to look at just the process and to see how long that process may be prevalent.

In the middle of that zone, at the ground surface, the temperature rise was almost 25°C, and as we move out, I think what's significant is that--not significant, but we've got a significant perturbation in temperature, perhaps out 50 meters on either side of this high k_b zone. If we move out further than that, or actually at 50 meters, it's a rather small perturbation relative to the nominal curve.

Now, if the contrast in permeability is only 84, which, in fact, may be also quite large, the focusing effect reduces the temperature rise. The temperature rise has been reduced by a factor of three. So you can see the sensitivity in the contrast in bulk permeability.

We can also look at for the same examples how long a liquid saturation three meters below the ground surface is perturbed, and what we find is that this perturbation due to focusing lasts for approximately 2,000 years. However, the perturbation of the saturation field lasts substantially longer. As we observe from mountain-scale convection, the same generally applies for focusing, that we continue to focus vapor flow into this region, but at later time, it doesn't carry a significant amount of heat relative to what conduction is doing in the total system. But it still can be introducing a significant amount of water vapor and condensation up in that zone.

If we reduce the permeability contrast to 84, we find that the extent of that perturbation, about the nominal perturbation, lasts for about as long as the perturbation on the temperature.

So what this is saying is that if we don't have a really large contrast, that both the temperature perturbation and the saturation perturbation may be constrained in time.

Again, this is a very extreme example, and I just wanted to show it in terms of looking at processes. If we get down in the contrast in the range of 10, we don't see any focusing at all.

Now, what do we do to try to diagnose the potential of these effects, and I think there are two primary things;

one that I won't talk about because it's not my area. But I think the pneumatic permeability study being led by Gary Lecaine of the USGS is extremely important, and the other part of the picture that's very important I think is the use of heater tests and diagnosing the potential of these heat-flow enhancement mechanisms.

And what I'm showing here is a test that we've designed a couple years ago, which could be applied at the main test level. It could be applied at Busted Butte. It could be maybe even applied at Rainier Mesa, and basically the calculations would, to a certain extent, apply to all three systems, especially Rainier Mesa--especially Busted Butte and Yucca Mountain because the properties are so similar there.

But what we find is that given spacing that's probably the minimal spacing we could be heating with, that it takes roughly two years to coalesce the regions of dry-out between these heated zones.

So for that reason, we think we probably need on the order of two years before we get kind of a homogenized or coalesce disturbance.

What I'm showing over here on the left side is a simplification of this discreet heat source model. This model is a cross-sectional model, so it doesn't truly represent things in three dimensions.

So we do the same thing we do with the repository. We take the heat source and smear it over an equivalent disk size to do a lot of our sensitivity analyses.

And in this example, we're plotting the saturation perturbation, dimensional saturation perturbation, and saturation, you know, decrease and increase, relative to the center of the heater test, which is here. So this axis symmetry to be rotated about this situation.

If we have less than about a darcy of permeability, we find that the vapor flow in a heater test like the repository system is vertically symmetrical about the heated horizon. So we get this type of uniform dry-out in a relatively uniform condensation.

If we go to permeabilities that are, say much greater than say 10, we get the situation here on the left where 100 per cent of the steam flow is going upward, 100 per cent of the condensate is occurring above the heated horizon, as I showed in the earlier repository examples.

There are various means of diagnosing whether this is happening, and I think temperatures are the best way of diagnosing. And also, as we'll be doing at the large-block test at Fran Ridge, we're improving our ability to measure saturation changes, but in addition to that, the temperature perturbations give us a very good signature as to what's happening to the direction of vapor flow.

I'm plotting here, the blue curve, the nominal--not the nominal case, but the relatively low permeability case, but probably maybe relevant to Yucca Mountain. Right now, 280 millidarcy, based on what I've seen in initial pneumatic pressure measurements, it's in that range with a welded tuff. What we get there is a symmetrical distribution of temperatures because the vapor flow is symmetrical. And as we go out in time, that temperature distribution is quite symmetrical.

However, if buoyant convection is going to be dominant, we see a very pronounced asymmetry appearing even at one year. Within two years, we're getting a large difference in the extent of the boiling front between the case of symmetrical vapor flow versus vapor flow dominated by buoyant convection.

And I believe that within two years, a test that heats on the order of a third of an acre, or perhaps up to an acre, but a third would be minimum, we think, required, we would be able to see how important this effect of buoyant flow may be.

I think it's important to do this type of thing because right now, it's difficult to say with certainty that the permeability measurements being made in situ are directly the same property that we're using in the model itself. We have to determine the relevancy of those permeability measurements, the heat-driven buoyant flow, by doing those measurements very rigorously in an area and then heating it and determining whether, in fact, that permeability is relevant to this buoyant convective process.

I'll just move on to my conclusions. The primary mode of repository-driven heat flow is heat conduction.

Temperature rise at the ground surface will be no less than that predicted by heat-conduction-only models, and that we've predicted or calculated that the conduction only temperature rise, three meters below the ground surface, will range from .3° for the low thermal loading case we're considering in the thermal loading system studies, up to about 1.4° for the 155 MTU per acre case, which actually is a higher AML than we're currently considering in our range of thermal options.

The 110 MTU per acre case gets up to about 1.2° above ambient.

We find that heat flow to the ground surface may be enhanced by two-phase convective heat flow, possibly arising from a combination of the following effects: Two-phase refluxing, called the heat-pipe effect; mountain-scale, buoyant, gas-phase convection; focused vapor flow due to heterogeneity and a particular heterogeneity oriented vertically. These convective effects, particularly the first and third, do require boiling conditions to be significant at the ground surface. They do not require boiling conditions to be significant at the waste package environment, however. So there's a distinction there.

Heat flow to the ground surface can be significantly enhanced if the bulk permeability distribution facilitates substantial mountain-scale, buoyant, gas-phase convection.

If the permeability in the non-welded Paintbrush is large enough to allow this buoyant convective system to communicate freely with the atmosphere, convection may enhance heat flow to the ground surface for tens of thousands of years. However, a reduced permeability in the PTn greatly throttles most of this enhancement effect that may arise due to a large k_b being in the welded Topopah Springs units.

A reduced k_b in the non-welded Paintbrush would greatly restrict the extent of this enhancement and would largely limit it to about a 1,500 year time frame.

So rather than significantly perturbing the ground surface or enhancing the heat flow to the ground surface for 50,000 years or 100,000 years, if we find that the PTn is substantially less than a darcy, or not even substantially, a darcy or less, this perturbation, or this enhancement, will be restricted to about 1,500 years.

Focused heat flow in widely-spaced, and I emphasize widely, vertically-contiguous high k_b zones can enhance ground surface temperature rise for on the order of 2,000 years. This focused heat flow requires a fracture connectivity from at least the repository horizon all the way to the ground surface.

If you have a fracture system that does reach the ground surface, we'll have the same situation as we had with the Paintbrush being restricted. You'll have enhancement up to a point, and then that vapor flow will be limited by diffusion to get to the ground surface.

And it also requires a large contrast in bulk permeability between that zone and the neighboring rock. It requires remoteness from other vertically contiguous high k_b zones. Otherwise, if you have a lot of high k_b zones vertically contiguous, they'll all be competing for a limited amount of water vapor, and you will not see a whole lot of focusing occurring.

It also, for ground surface effects, it requires substantial boiling conditions. In fact, when I reduced the AML from 155 to 77 MTU per acre, it dramatically dropped the effect at the ground surface.

We also find that gas-phase diffusion, and in particular what's been called enhanced gas-phase diffusion, causes a modest increase in ground surface temperature rise, but it can cause a significant increase in the liquid saturation buildup at the ground surface.

We also find the convection-enhanced ground surface temperature rise is also accompanied by a substantial increase in liquid saturation. Convection-enhanced liquid saturation rise will persist much longer than convection-enhanced temperature rise.

And lastly, the combination of pneumatic bulk permeability measurements made through the repository block and large-scale in situ heater tests are critically important to diagnosing the potential significance of convection-enhanced heat flow to the ground surface. The most valuable pneumatic bulk permeability measurements will be those which determine the effectiveness of the PTn unit as a vapor flow barrier.

I'll be happy to answer any questions.

DR. BREWER: Thank you very much. Are there questions? John Cantlon, Board?

DR. CANTLON: Could you go over again for me, Tom, it's been suggested in some of our discussions of this possibility of high rises in the heat-pipe area, that there should be some kind of a thermal block where you have high heat that would prevent water from moving down. Some of your models, however, though, show water moving down against what looks like a vapor pressure gradient. How does that--

DR. BUSCHECK: How does that work?

DR. CANTLON: Yeah.

DR. BUSCHECK: Well, how that works is that the necessary permeability to give rise at that chimney effect is so large that the aperture of those fractures are incredibly large relative to the amount of liquid condensate that's generated.

Basically, in order to generate that effect, you have to get all this vapor flow flowing up. Once it condenses, it occupies with 1/400 of volume, or whatever that the vapor flow does.

So it's impossible to get that vapor flow from preventing the liquid flow from draining down through it unless there is a substantial enough boiling region to cause it from draining down through it.

So that's the primary reason why the vapor pressure gradient has no impact on the ability of that liquid condensate to return.

I'd like to say that if we could model the--and we will in the future--but if we could model the non-equilibrium drainage of that condensate flow, that would not change the fundamental nature of that vapor flow focusing.

DR. CANTLON: But the heat distribution would be such that it would be unlikely that it would reach the package; is that correct?

DR. BUSCHECK: It depends on how much condensate we focus relative to how much heat flow by conduction can be focused into that region. Everything we've analyzed, we've looked at probably 100 cases to date, shows the probability of reaching the waste package decreases with increased thermal load.

DR. CANTLON: Second, follow-up question. You've seen the amount of calcium carbonate in the fracture systems, particularly near the surface. Do any of your models assume any kind of filling of--

DR. BUSCHECK: What you say is exactly what we saw at Fran Ridge, like one or two order of magnitude reduction in the bulk permeability as we got up to the ground surface. Right now they're not included. We will look at that, and there are other effects that we aren't including.

There are details of the very shallow ground surface that we can, and hopefully will look at, to get a better determination of those effects, but I think that will --even if the PTn weren't a cap, or, in fact, say if you had this chimney effect through the PTn, that there will be a cap sitting right there at the ground surface, and that subtle buildup in gas pressure, which could dramatically affect how well that unit works as a chimney. In fact, I think it will have a much stronger effect than the PTn has because the PTn is causing the convective system from being an open system to being closed. But the closed system still has a way of flowing outward. If you have a chimney that's capped, there's no place for that to go, and you could greatly disable that chimney effect by virtue of that restricted permeability at the top.

DR. CANTLON: You indicate that the thermal effects are going to be relatively short-lived in the order of 1,500 years?

DR. BUSCHECK: You know, as we constrain what's more likely, I think--and what I didn't mention here is that if this buoyant convective effect is important, it's not going to be important everywhere. The worst will be that we may have convection perhaps within some limited zones, and so you won't see that uniform rise throughout. I think that would be highly unlikely.

DR. CANTLON: On the other hand, you show that the moisture effect has a very long-lasting, thousands of years. And since moisture is the limiting ecological varier, or ecological variable in deserts, that could, in fact, enhance the vegetation growth in a fracture-rooted system.

DR. BUSCHECK: I think there's a definite potential for that.

DR. BREWER: Other questions? Dennis Price?

DR. PRICE: Does your time zero start at closure, so it's a closed--

DR. BUSCHECK: Oh, no, we have an effective--well, before we did the system study at some arbitrary time. It doesn't really matter whether it be--if at 30 years of emplacement, whether it be at the 15th year or whatever. It's on effective time we're turning on the heat source.

But for the system study, for the whole repository, we've ramped it up to account for the sequential emplacement of waste. For the drift scale model we, of course, turn it on instantaneously because the package is either on or off.

DR. PRICE: What effect, if you maintained retrievability--some of your effects that you show are relatively early, 100 years and so forth, the first 100 years. What effect if you maintain openness for retrievability for a long period of time do you anticipate would have on some of these results?

DR. BUSCHECK: Well, I doubt that it would have a large effect unless we went to heroic efforts to ventilate the system, and it has yet to be determined what fraction of the heat we could remove.

I think a very interesting calculation, which I plan to do, would be to allow--do some of the worst case cases I analyzed, and then turn the system off at 50, 100, 150 years. Say we have some extended period of retrievability and discover that things were the worst case, how soon would that pulse be dampened at the ground surface? At what point would there be possible damage that you wouldn't be able to preclude by virtue of getting the waste out. I think that would be a worthwhile exercise to do.

But in terms of retrievable period, unless things are different than I've been told, I don't think we're going to remove a substantial amount of the heat and/or the vapor, unless things are different than I've heard, to change that perturbation.

DR. BREWER: John Harte?

DR. HARTE: How difficult would it be to run your models not just up to three meters below the surface--

DR. BUSCHECK: It runs right through the ground surface. That was just arbitrary data that I just applied just because in talking with like Charlie Malone, I thought their interest was approximately at that depth. Pretty much you can just--the model has some limitations, and I should have maybe underlined them more. One of them is we don't consider a finite heat transfer coefficient at the ground surface. And in my looking at heat transfer coefficients that are relevant, there would be a very small possible delta-t right at the top of the system, and that's not being included. So in other words, the ground surface temperature is pegged, and so you can just basically take that temperature rise and interpolate or extrapolate to whatever depth you want, probably in the first 30 meters, because after the first 30 meters, the thermal conductivity of the underlying unit, the Paintbrush is substantially different.

DR. HARTE: Do I understand you to mean that you have a boundary condition at the surface--

DR. BUSCHECK: Yes.

DR. HARTE: --which is no change?

DR. BUSCHECK: Right now, it's no change. It assumes a high heat transfer coefficient at the boundary layer itself, and it also does not include any ET enhancement, the way the ET may enhance by latent heat transport up the plants itself. And John Nitao, my coworker, has written the chapters of Jacob Bear's upcoming book, looking at these types of effects. And he's been doing this in conjunction with remediation work at various DOE sites.

DR. HARTE: Because depending on whether you dry or moisten the soil at the surface, your boundary condition at the surface could look very different because--

DR. BUSCHECK: Yeah, all these calculations were done with the repository perspective in mind. And we've done our best trying to understand into the sensitivity, but certainly there are details and possible improvements that need to be put into subsequent analyses. One could, if there's a finite heat transfer coefficient at the ground surface, one could impose a delta-t at that point sort of arbitrarily, and just displace everything accordingly.

DR. HARTE: Yeah, that could also affect your responses at greater depth?

DR. BUSCHECK: No, I think it would have a negligible response at greater depth, very, very much so. You're going to get a steady state heat flow through this boundary layer, and once that happens, you're not going to be--it's not going to be coupling back into the system.

DR. BREWER: Mike Bowers?

DR. BOWERS: Yes, some of the discussions yesterday revolved around concerns about how increased infiltration would affect the repository. Your model assumes a constant water content for the model?

DR. BUSCHECK: Right now it assumes gravity capillary equilibrium, and we've looked at other recharge assumptions, but when we did that--well, for this heat flow at the ground surface, that will, I think, have a very small effect, because you could dramatically increase the saturation to account for pluvial scenarios. But the fractures are still going to be largely gas-filled, and if they're not largely gas-filled, we have a much smaller bulk permeability than what would be necessary to cause the gas-phase--you know, heat enhancement effect.

So I think with regards to looking at the ground surface, the pluvial scenarios would not affect the heat flow to the ground surface in a substantial way.

DR. BREWER: Other questions from the Board? Tom, thank you very much.

Our next presenter is Joe Hevesi. Joe is with the U.S. Geological Survey and is conducting studies of evapotranspiration, and also infiltration at and near Yucca Mountain. So we're getting closer to the surface, Joe.

I should note to the colleagues on the Board that Joe is a late entrant, and will produce a paper for us after the meeting.

MR. HEVESI: Dim the lights a little bit, please.

DR. BREWER: You can also get rid of--

MR. HEVESI: Oh, yeah, one of these is going to have to go. I will need one at one point. If I can just--yeah, I'll raise it up when I have to use it.

Tom, can I borrow your pointer? Thanks a lot.

I better not, I'll just use Tom's. I might go crazy with it.

This is a package study here that I'm involved with. I'm working under the study plan for the characterization of unsaturated zone infiltration, and this package has been directed largely by Alan Flint, who's in Paris right now on vacation, and I'm trying to fill his shoes. I'm sure he'd be happy to be here. He's really the expert on the ET measurements, and I've been doing some of the ET modeling.

My work has been concentrated in this activity, natural infiltration, and also in characterization of meteorology, and this activity here is--is that too loud? The characterization of meteorology is concentrating on precipitation as input to the hydrologic cycle, and the ET studies are occurring in this activity here. But this whole package is integrated.

And we're looking at this really in terms of providing that upper boundary to the model, such as Tom was presenting, because this is what they really need. And so we're trying to interact with the modelers in terms of providing an upper boundary condition, although we're focusing mostly on water flow, not so much on the gas-flow effects, and also the thermal effects we haven't been dealing with too much.

But this is a schematic of what that upper boundary might be like, but it's a scale question because soil physicists often think in terms of centimeters here in the depth scale, but this might actually represent the whole unsaturated zone in the repository, which is 1,500 feet.

And the idea of this slide here is that when somebody is interested in net infiltration, it may depend at what depth you're at, and this is constantly changing. It may be sending pulses through the system, and you may have to go down to some depth before you reach a pseudo, and the big question becomes at what depth does this start happening, whether this is positive or negative, and what the surface boundary, our surface boundary condition, which then becomes the surface boundary condition of Tom's model, what this condition looks like. And here we see evapotranspiration occurring at the surface.

And then we have to carry that model across a fairly large two-dimensional scale, and so it's really a three-dimensional problem, and well this, of course, is the potential repository boundary. I was told not to use vertical slides, or actually I'm running into trouble with vertical slides, and so it's titled, and north is to the left.

But this boundary here is the LBL model boundary that we've been working closely with in terms of providing upper boundary condition, and that boundary is reflecting some of the major fault boundaries, at least here. Here, Solitario Canyon and the southern boundary is a little bit more arbitrary. But that's their model, and we're working on that scale.

And in terms of our studies, we're conducting studies in terms of water balance, in terms of forming our conceptual models and numerical models. So we're interested in net infiltration, but we have to keep track of the whole system.

And, well, this looks simple enough, but, well, it's not that simple because the problem with arid environments is that the errors in measuring these terms will completely override the I value. So we have a problem there. But we don't throw the water balance out because it's a framework for which we develop our studies and our numerical models. And today I'll eventually get to evapotranspiration and how we're modeling this.

So these are the things we're considering in terms of affecting that water balance on the two-dimensional site scale. And I should mention here that this is not a complete list. Vegetation should be included here, and it's not just timing of precipitation, but intensity, duration. It does snow at Yucca Mountain, so we're also interested in type, and we're starting to see evidence of a snow cover when it lasts maybe a week, two weeks, having an influence on the system.

Now, this is an example of a watershed type of a model, and again, this is terms of a water balance, but this shows the scale of what we want to model the water balance on, and it's a sub-drainage of larger watersheds. We need many of these to cover the area of interest, but it's a scale that we're interested in working on. The boundaries here are nice because they're natural boundaries forming a closed system, the natural divides of the drainages.

And if Dave Beck and his group with the surface hydrology program put a gauge or a flume at the bottom there, then we have a nice closed system because we're not going down very deep. So we're not worried about the lateral flow effects across these boundaries.

And what this shows are some of the boreholes we have in this particular drainage. This is WT2 Wash. The photo is outdated. There's now a big drill pad here. I believe this is the trace of the Ghost Dance fault. There's a large artificial out-crop here where they had the surface exposure, and the roads have been widened to some extent, too.

Now, this is an example of what we're after. This is in Pagany Wash. The mouth of the drainage here is down here, and this is north. It's a fault-controlled drainage north of the potential repository.

And the idea here is to set up a grid, and this is a regular grid here where the dimensions are 250 feet, and the examples on the bottom here are Richard's equations type, one-dimensional flow models for each cell going down, and we see little pulses here representing environmental effects, precipitation and evapotranspiration.

Now, this model is incomplete because we haven't connected it yet with the surface flow component, and we're looking at the kinematic wave theory. We don't think that's going to be too difficult because the surface flow component is largely topographically controlled, and then we need a resistance term like the manning's end coefficient, or something, which we are attempting to measure.

And then precipitation distribution, I'm not going to get into that too much today, but we're mapping precipitation using geostatistics, and we're developing a stochastic model for that. I'm going to concentrate on evapotranspiration for an area like this and how we might begin modeling that.

This shows the network of neutron access boreholes as part of the natural infiltration study, and this is not updated. It's an EG & G map. There are some additional boreholes along the crest. We placed one here right on the Ghost Dance fault to look at some of the fracture flow problems. We have a total of 97 boreholes that we're logging now on a monthly basis, and the depths of these boreholes are averaging about 20 meters. Some are shallower. Some go down to 50 meters.

And the idea here is to look closely at the water balance at each of these sites. We're measuring the water content change. We have material properties because of continuous coring, so we can make a stab at net infiltration if we go down deep enough in the profile. And then we're measuring precipitation at all these sites, too.

The weak point in a way is the evapotranspiration because we're not measuring that at all these sites. We have a network of five full Penman weather stations where we're estimating potentially evapotranspiration, and we have a couple of Bowen ratio stations set up.

So I'm going to get into how we're going to model evapotranspiration at those sites.

And now in terms of the water balance, I'd like to show the components delta-s, which was a change in storage between the ground surface and some depth, and then I'm going into estimate of the net infiltration term hypothetically somewhere down there.

This is for two boreholes in Pagany Wash. It's an alluvium. We haven't reached the alluvium bedrock contact yet, but the boreholes do extend down. So we can watch the alluvium bedrock contact.

And this is very typical of alluvium. This is 1993. We see five months and a nice--to this, this is nice --downward movement of a wetting front in both cases. We have a little bit of a complication here, but there's boulders in the alluvium. Sometimes we detect wash-out zones behind the casing, which we have a method of dealing with now, so this is okay. We can model this type of a change in water content. It's not too complicated, and it's fairly consistent to the alluvium.

Now, when we get into bedrock situations, this is up on the side slope now. This is WT2 Wash and 53 and 55. The alluvium cover is pretty shallow here, and we start getting strange changes in that delta-s term, which aren't quite as straightforward.

This change here is interesting. It's not a big volume of water, but we have a hard time getting these pulses down here to 10 meters using matrix flow properties. So we're considering this is possible of fracture flow. And we were standing close to this site yesterday when we were at the Ghost Dance fault study site. The fractures are filled, but it doesn't mean that at this site they're not filled.

And we also have to look at the--we haven't really looked at the material properties of the filling materials. So we're looking at that now, too. We're not sure what the permeabilities are of that material.

And this might also be related to evapotranspiration. We have a south-facing slope and a north-facing slope, but we can't separate the two at this point.

And it gets real interesting when we go to the ridge tops. I should have mentioned when I started showing these slides, 1993 was an anomalously wet year in the record, which we've been collecting since 1984. '83 and '84 were wet years, and then '92 and '93 were also wet years in the record.

And for these particular boreholes, these are the--well, N-71 was installed in '84. This is the largest change we've seen. N-15 was not around until about a year and a half ago, and so we can't really relate that to a historical record yet.

But this is a very significant change. When we do the mass balance here, we get a greater increase than what we measured in precipitation. So we're assuming we don't have to assume surface flow there. We saw surface flow at the site, and so that's a surface flow effect. Even though it's a ridge top, it's situated in a gully. It's in a headwater drainage area of Pagany Wash, and it doesn't take much of an area to begin concentrating precip to get this type of effect in terms of concentrating water. We don't have to have a flash flood effect.

Our hypothesis right now is these sort of things are related to fracture flow at the surface and not so much matrix flow. But I'm not going to spend much time on this phenomenon today because I'm going to go back to the alluvium. We're having better luck in understanding what's going on at the present.

This is indicating that one of the things we look at is the variability of the system in terms of the geology. That was one of the elements on that initial list. And if we go below the depths of these changes, we can hypothesize that maybe we're a pseudo equilibrium down here and make a stab at net infiltration.

And this slide also shows that fractures may be important, but it's not straightforward because, again, it depends on whether those fractures are filled or not because this straightforward relationship between fracture, density and the depth of penetration of those profiles doesn't come out.

And this is just a picture of--I would have liked to have the Ghost Dance fault pavement study site here. I believe this is one of the NRG sites showing some fractures with filling.

And so we can't ignore this. We're seeing evidence of this in the profiles in the near surface, and our program now in matrix properties and surficial materials is going to concentrate on measuring and modeling this type of a situation.

I think Allen will be presenting some preliminary modeling results at the high level waste meetings coming up.

And what I'd like to do now, I think, yeah, I'm going to have some overheads.

This will relate to an estimate of current net infiltration rates. This is coming from the matrix properties program with the exception --well, the matrix properties program is collaborating on this, too, the exception of this relative situation. What we're looking at is what are the water contents that we're seeing at the near surface, and then the rest are the material properties measured on the core samples.

So we're going to assume that equilibrium condition, and this is the PTn that Tom was talking to, where we're getting the--I'm talking about--where we're getting the high permeabilities.

So the potential infiltration rates here, especially when we go to the PTn, are extremely high.

Yes, I should emphasize that--I showed the slide on fractures to emphasize that we're not ignoring fractures, but now I'm ignoring fractures in these overheads.

And this shows that if we strip away the alluvium, this is the surface hydrology. But in the next view graph, the caprock will be stripped away because we're going down a little bit lower to a depth where we think the equilibrium is being established, and we saw a lot of changes occurring in the caprock.

And if we take the matrix properties, then, and the assumptions that we've made in matrix flow, this becomes interesting because for the most part, we see pretty low rates in terms of net infiltration over the potential repository site. But then we get into some high rates of 13.4 millimeters per year, which to us, 13.4 millimeters a year is a fairly high net infiltration rate from what we're seeing in terms of what makes it through the alluvium.

What's interesting about this also is that these units are lining up in many cases with the channels of the washes. So the outcrops are occurring in the channel area. So we may not only have the higher permeability, but a concentration of flow in some cases.

And, well, I can't get into this too much today due to lack of time, but this was integrated with the LBL study already in terms of their model, and I think Bo was showing some preliminary results where he's predicting some of the high water contents they observed, and that's because of Drill Hole Wash. Again, we're having the high permeability unit appear here.

So if we integrate with the LBL model, I'm showing the grid now, we just overlay it. And, well, you can see the idea there. That's the grid density, and this is really what the surface boundary condition for the model then looked like. And this is what Bo Bodvarsson and his group were then using.

And the point here really is that this is a snapshot in time maybe. It may not be right at all because it's only matrix properties, but we're having a high spacial variability, and that's not even looking at the temporal variability. This is just spacial variability. And we're also ignoring runoff events in the washes here.

So if we get back to faults, we'll have to start looking at potential zones that are related to the fault traces, and that's one of the reasons we placed one of our latest boreholes on the Ghost Dance fault. We'll be looking at that very closely.

In fact, I did look at the profiles for that borehole, and there's about two to three meters of alluvium cover there. It is in the channel, and the alluvium absorbed the impact of the 1993 wet year. So if we have a thick enough alluvium cover, it doesn't seem to make it into the fractures.

And this gets back to this map here. This is what we originally started out with in terms of providing LBL with an upper boundary condition, and now we're working backwards, because in this situation, we pretty much can predict what the response in the alluvium will be now, if it gets to be a depth of two meters. And I think actually this depth here is going down a little bit shallower to one meter. And the ridge tops and the side slopes are something we have to start modeling now in terms of understanding that.

That's it for that.

This is a view. We're pulling away from that infiltration now and looking at the other components of the water balance. Now we're back to the other components of the water balance, and this shows the full Penman weather station and the heated tipping bucket rain guage on the surface, and, of course, evidence of precip, and I'm going to ignore the effects of snow for right now. But that's something that we're going to have to deal with in terms of, say a storage term in the water balance.

And I won't show the temporal record yet. So we are looking at temporal variability, but we also have to look at spacial variability. We can create these maps because we are measuring precip at each of the neutron boreholes, and we're using geostatistical models here.

Now, this is a little bit outdated. These storms are 1990 storms. We've looked at approximately 50 storms now that we've mapped geostatistically, and we're developing a stochastic model of using geostatistics.

And the important thing here is there is a difference between winter season, summer season, rainfall. This is a minimal winter season storm. We get a more regional effect. In the summer season, we have localized conductive storm cells, and we may have a local--this is about a one-inch storm, which often does produce a runoff event in the washes. But we might be having runoff here, and nothing here, and that might be the only runoff event for that year for the mountain.

So we have to look at spacial variability in terms of precipitation, but I won't get into that today.

And then the other component of the water balance, this is one of Dave Beck's group's gauging stations right here, and we're working closely with the surface hydrology group in terms of setting up the gauging stations and the flume sites relative to watershed boundaries.

Now, there was a runoff event in '93 that was measured at that site, but it may have been road related. So there's a little bit of a problem there in the way the hydrology and the roads are currently interacting.

In 1984 there was a significant runoff event. This is Pagany Wash. This is upstream from the gauging station, which is down towards the right and down about 200 yards upstream from the gauging station. And we did have a runoff event in this wash in 1984, and we picked it up in the array of boreholes. They were installed about a month previous to the runoff event.

And this is in terms of saturation and the redistribution of what was measured due to that runoff event, and the red here is the decrease, the blue is the increase. So we have a redistribution moving downward as we would expect. I'm sorry it's in terms of saturation because you can't look at this slide and really do the mass balance, but I can tell you that most of this loss is due to evapotranspiration. It's not due to the increase we see here. And then we're also picking up evidence of lateral flow in five.

So we wanted to model this in terms of understanding what the heterogeneity of the system might be doing here. We're just inserting layers. We're using the TOUGH model here, but we're setting it up in terms of isothermal conditions. We're not looking at vapor flow here, and it's not really a good model. But we're trying to simulate the lateral movement here, but what became interesting to us was that we realized that we can't just stick a constant potential upper boundary there to start simulating ET. We had to get a handle on that, and this shows that if we stick a constant head ET boundary up here, we have a hard time--we just set up a steep gradient, and we have a hard time pulling the water out of that zone.

So we started thinking in terms of root zones, and we like it when we hear things like a two-to-three meter root zone is not unreasonable.

And now I'm going to get into evapotranspiration, and the upper limit estimate, of course, for potential evapotranspiration is a Class A pan. This has been set up in Jackass Flats since 1990, and this is not updated either, but we see generally the same thing every year in terms of the maximum average daily values in the summertime being about 10 millimeters a day, and this is after the pan coefficient is applied. And then, of course, in the wintertime we go down to less than two millimeters to one millimeter a day.

The differences we see here, this is a regional network, Death Valley, Boulder City, and we see some differences we think is an oasis effect due to increased humidity from Lake Meade and irrigation activity. And so that's showing up on this.

But anyway, we can use this as an upper limit to our models, and then we can make an attempt at estimating actual ET. This is shown at the Bowen ratio station, and we do have one in Pagany Wash now, and one is being installed in Split Wash.

And I'm not going to present results from this station today because I'm going to move on to modeling. And the Bowen ratio stations are agreeing with the models to some degree at this point.

We want to model this in terms of an energy balance, and this is the modified Priestley-Taylor equation, which is a modified version of the Penman-Montieth equation, and the alpha coefficient here is representing missing invection term. And usually what is done, is this is set to 1.26, where the 26 stands for 26 per cent of the energies from invection.

But when we get into arid environments, what we do instead is we set up the alpha coefficient as a function because we're not just energy limiting. We're limited by the available moisture, and this becomes a function of a saturation.

And the rest of these parameters can be measured, or they can be modeled based on solar radiation, which itself can be modeled. And air temperature, there's some long-term air temperatures out there at some of these stations, such as Desert Rock Airport in Las Vegas.

And this is just an example of radiation measurement. Net radiation is being measured here, and the model I'll show is in terms of incoming solar radiation, and one of the things about solar radiation that's important is the topography. But the topography is known, so that's easy to incorporate, and we can incorporate the effects of blocking ridges and then the seasonal effect, the sun angle and position in the sky. That can all be modeled if you know your latitude and your elevation and make an estimate of air turbidity.

Well, I failed to mention that the effect there, the north-facing slope was showing less solar radiation than the south-facing slope due to the blocking effect, and that was WT2 wash.

So we're going to go back to Pagany Wash. This is that transect. The runoff event was affecting these boreholes here. This is a deep borehole that we're not monitoring on a monthly basis, but the boreholes painted in white are being monitored on a monthly basis.

We're going to develop the model here, and then we're going to see how it verifies at this borehole up here. And the thing to pay attention to here is the vegetation cover and the heterogeneity of the alluvium.

And this is just a simple Richards Equation type of model that we're using. We're modeling the upper boundary not as a specified head, but as a specified flux term. So we're forcing precip into the system, but it is limited by the available storage capacity. And this is a two-hour specified flux term that we're using. So we have information down to 15 minutes actually in terms of solar radiation. And we're also assuming a root zone here so the specified flux terms are included in the model for all elements within the root zone.

And this shows the record that we're applying. This is the precipitation measured at the site in terms of millimeters a day. The dry year, we're having a drought condition here. '89 was also a drought condition.

So we're starting out with a dry profile, and then we get into '92 and '93, we have the anomalously wet winter seasons, and we did not get a large summer thunderstorm in that area for this record, so that that would be missing from this record, and we're waiting for it.

And this is in terms of 1.2 alpha coefficient in the Priestley-Taylor energy balance.

Now, the interesting thing here is now we're only at five millimeters a day in terms of maximum potential evapotranspiration. So we're only half of the evaporation pan already, and we think that there's a fairly large invection term out there. So this a conservative estimate of potential evapotranspiration.

And then when we compare the monthly values, this is in millimeters per month. The red is the precipitation, and the sine wave is the potential evapotranspiration with the measured changes in the profile at four depth intervals. This is the shallow zone, .3 to 1 meters and 1 to 2 meters. We see a good correlation with wintertime precipitation because it's greater than the potential evapotranspiration.

Then when we get down to the lower part of the profile, this surprised us at first because we're starting in 1990. We thought we were beyond the effects of the infiltration event, but we're still getting a drying of the lower portion of the profile, and we also get the wetting front moving down to this zone here.

And this is what we're actually trying to model, but what we're doing is we're predicting these changes here, and these are the monthly logs in the shallow portion of the borehole, by adjusting the parameters defining the evapotranspiration function, the alpha prime function. And when we get a fit, we assume that we've developed some type of a calibrated ET model. And this change here is the drying-out effect in the lower portion of the profile.

Now, this shows a wetting front moving down in both cases, and then a drying of the profile. So we're assuming that matrix flow here, but we're in alluvium, I don't think this is a bad assumption, and these profiles are nice in terms of modeling.

And these are just initial test cases. Again, the change in water content. This is for the upper .3 to 2 meters, the lower part of the profile. No ET, we just flood the profile. Here's the precipitation events. Surface potential evapotranspiration. We're still slowly flooding the profile because we can't pull the water up using Richards Equation only.

And if we have a root zone, we dry the profile out instantly because the total potential evapotranspiration is approximately six times the average annual precipitation at the site. So the energy is there to suck the system dry. We have to use a residual line metric water content, and then we can start matching the profile. But we're still drawing out too fast, so this shows that we have to develop a function in terms of volumetric water content.

And this will just show that the model--we had a deep root zone because we're failing in terms of--it's a two-dimensional problem down below, and we're counting for vapor flow. The actual root zone rest means only two to three meters. And we're also including a seasonal effect. This is somewhat simulating a growing season, but this is empirical. We did not talk to the biologists, so this is what made the model work, what fit the profiles. And this just shows the fitting of the--the red is the seasonal effect, and the blue is without that seasonal effect. And that's just fitting of the profiles. The black is the model with the seasonal effect.

And I was going to show a view graph of the actual rates. I'll just talk about it briefly.

The maximum rates, actually there were three millimeters a day in terms of evapotranspiration, and in general, they were never above two millimeters a day, and they decrease rapidly. If anybody's interested, I can show you those graphs later.

And when we go to a terraced location, we see that we're off now because this borehole is not affected by the runoff event. But the upper zone in terms of the ET and the root zone, we're still able to match somewhat. And if we go downstream, this is vegetation now. We're on the terraced side. There's a Bowen ratio station. We're still getting a good match. So we almost expected to see a big difference there because of the vegetation, but we're still matching. The problem here is that we haven't really separated vapor flow and actual transpiration. It's all lumped together in this empirical model, so we have to work on that now.

And these are just slides I just wanted to show to wrap up. This problem of dying vegetation was actually--or heat stress in terms of reducing vegetation was actually--until last week, but we have been considering changes in the surface environment because we have to--that's the only reasons we're setting up watershed models because we have to account for this. In many cases, we don't think that we're looking at the natural system, the ambient system anymore anyway.

This is an analog of a worst-case scenario, I guess. This is 1992. We have stripped vegetation. Of course, we also have compacted material, and we have an artificial drainage. But if we follow this down, we have a runoff event, and this is occurring north of Pagany Wash. I would imagine we've increased net infiltration in that zone. It's a limited area, but this is the PTn unit. I don't know if it's--it might be up too high, but this continued down. And if the alluvium isn't absorbing all that, it could be heading towards--I'm done, sorry.

DR. BREWER: No, that's fine.

MR. HEVESI: I didn't have a chance to rehearse this, so--

DR. BREWER: No, no, that's fine. That's fine. We just wanted to be sure if there were questions that we would have time.

Are there questions from the panel? John?

DR. CANTLON: You mentioned in a couple of comments there of the differences in vegetation as being one of the variables. Do you visualize getting together with the people doing the vegetation mapping and trying to do an integrated session?

MR. HEVESI: We had a field trip yesterday, and it was very exciting to me because we started talking about exactly that--well, yourself was there. And that is very exciting to us because that's exactly what we need to do. If we see a relationship, that would be a tremendous help to us because the vegetation can be mapped.

DR. CANTLON: Exactly.

MR. HEVESI: It's like the topography with the radiation load. These are exactly the sort of things we're looking for.

DR. BREWER: Other questions? John Koranda?

DR. KORANDA: John Koranda. Joe, you're essentially following seasonal pulse of water, vadose water actually, in a discreet watershed, aren't you?

MR. HEVESI: I show the discreet watersheds in terms of that's what we want to spread out to, but before we get to that point, we have to understand what's happening at the borehole sites. And in essence, those are calibration sites for the watershed model.

If we get a runoff event in a watershed that we've set up for a model, the main calibration there will be one of the surface flow gauging stations in terms of the hydrograph. But then we can also calibrate it in terms of the measured changes in delta-s in the boreholes, which will be in the watershed.

DR. KORANDA: Well, I was just wondering if you had considered putting a pail full of 99 per cent deuterium water at the upper end of it. A pail is one of the more precise ecological measurements. USGS has done this many places. Deuterium is easy to detect.

MR. HEVESI: Well, we do have an artificial infiltration program, but that scale is beyond the scope that we're considering. We're considering small plots and rainfall simulators and flooding some of the borehole sites.

That's a very interesting point, and, no, I haven't considered that.

DR. KORANDA: Well, you could detect it even in the plant transpiration. I have a method with a plastic bag and a hypodermic syringe that even a geologist could use, and collect a sample of plant transpiration to be analyzed. And so I think it would be a very powerful tool.

MR. HEVESI: I agree. This thing of plant transpiration, again it was lumped in the model. We've really made no attempt to separate it out, and I had a hard time imagining even a two-to-three meter deep root zone for the plants I was showing in the model calibration site. The creosote bushes are there, and they might go down deeper.

DR. KORANDA: Right.

MR. HEVESI: But they were spread out, and we can look at some of the rooting depths in the pits, but in terms of--this is why I became interested--in terms of John's working in xylem resistances and potentials. That's exactly what we need.

DR. KORANDA: Now, the open pan transpiration only approximates potential of ET when related to continuous agriculture vegetation?

MR. HEVESI: Yes. Yeah, the problem there is with the fetch.

DR. KORANDA: Yeah, right.

MR. HEVESI: And that's why we don't look at it too closely. We're just glad that the Priestly-Taylor isn't greater than the--

DR. KORANDA: That's right. Yours is always going to be less than that.

MR. HEVESI: That's right. And that's really just an extreme upper boundary, where we know we can't go above that.

DR. KORANDA: Right. Thank you very much, Joe.

DR. BREWER: The whole issue of the integration of the various parts of the study at Yucca Mountain has been a concern from the very beginning to the Board, and I think we begin to see some of the reasons for that in the presentation and the questions that were just raised, the relationship of the geology to what's going on above ground, the deeper geology in the sense of Tom Buscheck's presentation.

We are pleased to have Russ Dyer standing in for Bob Nelson today to talk about the integration of the environmental program with the larger program at Yucca Mountain. Russ, welcome.

MR. DYER: That you, Garry.

I would like to step way back here where we're talking about integration, and I'm not going to talk very much about technical integration and technical details for the various programs, but I want to step back and look at programmatic issues.

I've talked to the Board numerous times over the years in a variety of roles. This is my latest role as the deputy project manager. Bob sends his regrets. He's tied up with the OMB today.

I have, let's see, a couple of announcements I'd like to make first. As you're aware, we recently reorganized the Yucca Mountain Site Characterization Office, as it's called now. Wendy has a new title, the Assistant Manager for

Environment Safety and Health. And the Scientific Programs, Susan Jones, who was my deputy as RSAD has recently been selected as the Assistant Manager for Scientific Programs. So I pass that on for your information.

If we look at the environmental programs, really we're looking at two components, and I can draw a line about here, and the things we're probably most comfortable in talking about are these things below the line. Those are things that are going on now. Those are things that are associated with monitoring and mitigation of ongoing site characterization efforts. The stipulations that the environmental program puts on our various PIs before they can go into the field or on to construction activities before they're allowed to proceed.

There is another component of the environmental program, which probably has the major interface to the top level program strategy, and I'm going to talk about that a little bit, actually quite a bit a little bit later. This has to do with the content and schedule for the Environmental Impact Statement. As Wendy said earlier, the Environmental Impact Statement is that part of the environmental considerations which would consider environmental impacts due to repository construction and operations.

And then this other component here is the consideration of what information that is generated by testing design and/or performance assessment should and could feed into the Environmental Impact Statement.

Wendy and I were joshing a little bit, a little bit earlier. I've used a diagram similar to this for the Board with a Copernican view of the world here, except I've always had performance assessment in the middle before, but we now have environmental programs as the center here.

But, again, we're talking about essentially an ongoing program right now that is the controls specified in the monitoring for field construction in the site testing programs. And what I would like to talk about is the new program strategy, which you probably have heard Dr. Dreyfus allude to in some of his testimony. I know some of you have been before congressional panels with Dr. Dreyfus, what it means as far as a little better detail into the EIS schedule for our program and how it interfaces with testing design and performance assessment activities on the broad scale.

And to set the stage here, just a little overview of this new program concept, which is driven by a recognition of reality; namely, that the program that we thought had been accepted and approved has--excuse me, let me rewire myself here. Can you still hear me in the back?

Okay. This is the program that was approved in 1991 known as the Mission 2001 Program, which had a scope schedule and budget associated with it. This is just the funding profile associated with that. This is the actual funding level that we received through this year, and, obviously, there's a large shortfall of work that we thought needed to be done that we have not been able to accomplish.

The recognition is that there is an enormous battle wave going out here and that the likelihood of getting this level of funding in the near term, or even in the history of the project, is not realistic.

So we were directed to go back and look at alternative programs and try to construct a proposal for a program which has a high probability of what we think is a higher probability of being acceptable and achievable.

We, being a team of senior managers within the civilian radioactive waste management program from Dr. Dreyfus down, were given the charter essentially to look at three scenarios and given some assumptions and constraints for each scenario. The one that has become what's called the administration proposal is also known as Scenario A. It's a restructuring of the program to respond to the existing situation, building in management efficiency wherever possible, and we put a great deal of emphasis on prioritization of program elements.

And certainly one of the critical underlying assumptions here is that there is availability of some enhanced funding mechanism.

Another very critical compliment of this is that this particular program takes place within the existing legislative and regulatory framework. There are no changes to any legislation or regulations envisioned with this particular scenario.

Scenario B, which we looked at a little bit, was to restructure the program, again looking at an enhanced funding scenario, but considering changes to the legislative/ regulatory framework. This was such an open-ended possibility that we didn't pursue this any further. Most of the things that we came up with as potential things that should be pursued don't require a change in the framework, but rather a dialogue between ourselves and the regulator.

And the third scenario is what's called a resource constrained future. It assumes flat funding at essentially the same level that goes to the whole program as of this year, 1994 dollars, and again, its constraints are within an existing legislative/regulatory framework.

I'll talk a little bit about what the impacts are of these two scenarios, A and C. As you're well aware, we're currently in the budget negotiation and testimony phase with Congress. Candidly, this is not DOE's decision to make. We've put two proposals on the table. Congress, by the appropriations action, will essentially choose one of these scenarios for us.

Our preference, as I said, is the first one, the administration proposal, and our strategic goals that fall out of that are, number one, to determine if Yucca Mountain site is suitable. Wendy talked a little bit earlier about what some of the suitability standards are associated with the environmental program. In the preclosure guidelines, there are, I believe, one suitability criteria, one disqualifier, one qualifying condition, and three disqualifying conditions in the preclosure guidelines.

We would use a phased approach, or a step-wise approach to pursue licensing with the NRC. I'll talk about that a little bit later. There's initial application, NRC, that's assuming that we go down a hierarchy here. If the site is suitable, then you pursue licensing. The first action is a license application for construction. After the construction authorization, there is a license application or amendment to receive and possess waste. The NRC would come back with and grant this license to receive and possess waste. After that, after some period of time, there would be a license amendment action on our part for closure.

And what we have tried to build in here, as I'll show you in a little bit, is a large degree of the National Academy of Sciences rethinking high-level waste philosophy, where one doesn't try to make a definitive case at a premature point in time.

Another key part of our program concept is stakeholder/technical community and public acceptance, and, of course, reducing cost, improving efficiency and optimizing the schedules.

This is a schematic, and I'll take a minute or two to walk you through this. In the philosophy that we laid out in the Site Characterization Plan of 1988, there was some level--what we have plotted here is time versus confidence. In the Site Characterization Plan, we essentially had an everything-at-the-beginning philosophy. The implication you get when you read the SCP is that we will have a well-developed body of knowledge and a high level of confidence at the time that we go in for the initial license application, and we showed that on here, that level of confidence, whatever it may be, as being this level.

Whenever we look back through 10 CFR 60 and through the National Academy of Sciences report, we realize that there is really an opportunity and more or less a mandate for taking advantage of the increase in knowledge and certainty that should accrue with time. And what we've done is broken the program out into a series of phased or step-wise actions that take advantage of knowledge that accrues over time.

This point here, technical site suitability, is really mismarked. It doesn't set up here, but really sets down here on this curve. I'm not going to pretend that confidence or knowledge is necessarily a monotonically increasing function of time. There's going to be some jags in here, but we believe that as time goes on, as more tests are conducted, as information accrues, that we should get better confidence as to what the performance of the system and the system components are going to be.

The technical site suitability decision that we've shown right here is not the same as the site recommendation report that must go to the President, but really is a DOE decision on investment. At this point in time, we think, if we have not found anything that would suggest the site is unsuitable, we think that at this point in time we should have enough information to make essentially a judgment that this is worth investing money in, essentially a national decision.

Under our scenario, some testing is going to be deferred from this early phase into this period of time between up to around 2007, 2008--well, roughly in here, where we need to demonstrate that we have sufficient confidence in the ability of the site to isolate waste, that one can file a meaningful license application to possess and receive with the NRC with a high confidence that it would be acted upon favorably by the NRC.

So the key dates that we have in here are still license application for construction authorization in 2001.

We think we can still meet an operational date of 2010 for a repository system.

There are, obviously, some assumptions and constraints associated with this. This is, undoubtedly, a higher risk approach to the license application, the initial license application and that that was laid out in the SCP. There are no changes to the statutes or the regulations. However, in our planning, we have taken every advantage we can of the flexibility that reside within those regulations and statutes.

The funding, of course, is critical, as I'll show you on the last slide in a minute. Without the enhanced funding, not only in the fiscal year '95, but in the following years, this is not an achievable program.

Competing with the resources to fund the Site Characterization Program at Yucca Mountain is the reality that the multi-purpose canisters and addressing the waste acceptance by '98 issue that the Secretary also faces, this is going to be competition for resources. And I'll show you what the implications for that are on the next slide.

As I said, license application in 2001, providing that Yucca Mountain--that we do have a positive finding suitability. It's possible to initiate waste receipt, we think, in 2010.

Another key part of the concept is that we have an enhanced expanded period of retrievability, performance confirmation, up to perhaps 100 years, going well beyond the initial 50 year from initial emplacement period that was part of our previous strategy.

To go to my last slide here, and what we've laid out here is three time lines, and I'll talk generally about the time lines, and then I would highlight those portions that are specific to the environmental part of the program. And by the environmental part of the program, I mean that dealing with EIS.

The top time line is that that was laid out in our Mission 2001. This is the program that we've been unable to really accomplish because of resource constraints. And if you see this, originally we thought we could support a license application in 2001. That has now pushed out to around 2005, 2006, with a construction authorization in 2009, and an operational system at about 2014, 2015.

Under the administration funding proposal, these lines that you see here just tie back major actions onto the second time line. This is Scenario A, or the administration funding proposal, and the key components here are license application in 2001, construction authorization in this time frame, and then a license to possess and receive and essentially an operating repository system in 2010.

Now, associated with that is the site

recommendation report at this time frame and the TSS, or the Technical Site Suitability determination, in about 1998.

Things that are associated with the environmental part of the program would be the notice of intent essentially to initiate scoping for the EIS in probably the spring of '95, about a year from now, followed up by a draft Environmental Impact Statement in the '98 time frame, and then with a final Environmental Impact Statement that is very close in time to the site recommendation report and the final EIS.

I should note whenever I talked about the phasing of applications and amendments to applications, that as you go through that with the license application, the amendment to receive and possess, or receive and store, and finally the amendment for closure, each of those is also accompanied by a supplement to the EIS.

Now, the third one, the third line on the chart that I'd like to look at is what's called the level funding outlook. This is the one where the program as a whole gets level funding essentially equivalent to fiscal year '94 funding. We have competing resources for this funding with the MPC program, and what we find is that we can't--if you try to find license application, site recommendation report on this line, they're not there.

As of about 2003, 2004, assuming that we put all of our emphasis only on the determination of site suitability, the best we can do is a technical statement of site suitability; that is, this investment decision, this investment recommendation. We do not acquire enough information, have high enough confidence, that we think we can make a site recommendation report or a license application. There is no licensing activity that goes on in this time frame. There is no EIS activity that goes on in this time frame because as we said, the EIS is tied to a repository situation. We would be doing essentially no repository design in here, so we would not have the information to support the repository implications.

Okay. With that, I'd like to hold myself open to questions here. The Board?

DR. BREWER: Let's see, Dennis Price?

DR. PRICE: Yes, just a quick question. Does the term special account mean the same as off-budget, revolving fund?

MR. DYER: Off-budget, revolving fund. I think there's a new politically correct name for it that is--I think it is special account. I think that's the politically correct name.

DR. BREWER: John Cantlon?

DR. CANTLON: Yeah. In the level funding outlook, you get roughly a five-year displacement forward in time for your TSS.

MR. DYER: That's correct.

DR. CANTLON: And in a sense, what you're looking at there is an operative budget of approximately 300 million a year for five years, which is an added cost to the program.

MR. DYER: Yes, yes.

DR. CANTLON: And I presume that's hopefully in the congressional thinking as they look at the options?

MR. DYER: I didn't bring the full stack of slides, but the third scenario, if you look at the total system cost, life-cycle cost, that's about a $14 million program because it stretches out for so long.

DR. CANTLON: Right, right. So it goes from six to fourteen--

MR. DYER: Right.

DR. CANTLON: Six plus the fourteen.

DR. BREWER: Any other questions from the Board?

Thank you very much, Russ.

We are running about 10 minutes late. If everyone would try their best to get back at 1 o'clock, we can be back on schedule.

(Whereupon, a luncheon recess was taken.)

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

DR. BREWER: If everyone would please take their seats, we'll get going here on the afternoon session. We're just a little bit late, but made up some time. If everyone would join us here?

Okay. The first speaker of the afternoon session is Dr. Charles Malone from the State of Nevada. He has been for a long time the environmental scientist working for the state with respect to the Yucca Mountain Project. Charlie was with us yesterday for the field trip.

In the interest of time, let me turn it over to Charles Malone.

DR. MALONE: Thank you. It's a pleasure to be here today. Let's turn the first slide on, please. It shows the cover sheet.

Those of you here today who also attended the November 22nd meeting of this group have heard the state's views about DOE's current ecosystem program at Yucca Mountain. And, today's presentation will pick up from there and lay out a conceptual approach for addressing the principal challenges faced by the ecological program. Before moving into that, I want to review some aspects of what was said at the November meeting.

Our principal conclusion at that time was that if DOE's current approach at Yucca Mountain prevails, the ecosystem processes related to thermal impacts will not be adequately addressed or understood for the purpose of assessing potential impacts from a hot repository.

Next, we posed a question of what should DOE be doing now regarding repository environmental impact assessment. And, answers to that question, as shown here. FIrst, we recommended that DOE should adopt and pursue the concepts of processed-based ecosystem ecology. Second, we recommended that an effort should be made to identify what the critical functional processes of the present ecosystem are at Yucca Mountain. And, third, we concluded that an adequate study and impact assessment of the ecosystem at Yucca Mountain will necessitate the use of process-based simulation modeling to predict long-term conditions, at least as far into the future as 10,000 years.

Now, continuing from there, what we'll present today is a strategy for undertaking what the state believes should be accomplished for the repository Environmental Impact Statement. Following the November meeting, we've focused our activities, our environmental activities, on the long-term ecological potential of the Yucca Mountain site.

By ecological potential, we mean the capability of an area to support a functional ecosystem regardless of the species involved. Ecologists can't predict what species will occupy an area very far into the future, but they can gain insight into what long-term environmental conditions might be like and where their ecosystem processes can function under those conditions. This then is the concept that we're pursuing as a basis for reviewing the issue of future ecosystem responses that might result from a hot repository and, in turn, their potential consequences on repository performance.

With regard to the issue of repository performance, we've framed the central question to be addressed in the context of environmental performance assessment. Thus, the question is can thermally-induced impacts from a hot repository alter the surface environment to the extent that repository performance might be affected? This correctly implies that it isn't the issue of protecting the ecosystem at Yucca Mountain that is of primary importance, but rather the potential for adverse interactions between an alternate environment and the ability of a repository to perform over the long-term as required. The site characterization plan that DOE is pursuing now to determine the suitability of Yucca Mountain for a repository makes no allowances for a connection between the biotic environment and the repository.

In an effort to shed light on the importance of the Yucca Mountain ecosystem to repository, we've followed the unified ecosystem approach and integrated into that is the theory of ecosystems as interactive networks. We have also chosen to adopt the process-functional approach to studying ecosystems as opposed to the population-community approach currently being pursued by DOE. In attempting to conceptualize the process-functional approach, one is faced with the vast complexity of ecosystem processes and the question of the critical ones that have to be considered in order to understand the ecosystem.

To cope with that type of complexity, the most successful studies use an expert advisory committee to help guide their assessments and the use of such a panel, we feel, is essential here to successfully completing an effort like that discussed today. Researchers also have the concept or also use the concept of anthropogenic environmental-forcing factors to gain insight into the effects of environmental changes. And, although John Harte didn't use those words this morning, that's what he was talking about in his work.

As shown in this figure, environmental-forcing factors are external variables that drive the internal variables of an ecosystem and, in so doing, influence the energy, budget, and material cycles within the ecosystem. Increases in atmospheric CO_2 and temperature, for example, are the principal anthropogenic environmental forcing factors involved in global warming, an issue that must be addressed for the Yucca Mountain site along with subsurface heat from the hot repository.

The distinction between environmental-forcing factors and resources is clouded by the fact that some forcing factors that drive ecosystems also are resources. Carbon dioxide, for example, is a naturally occurring resource and it becomes an environmental-forcing factor when natural levels are altered by humans, as in the case leading to global warning. Heat, on the other hand, is a resource modulator that also becomes an environmental-forcing factor when it is added to an ecosystem, as in the cases of global warming and a hot repository.

Environmental-forcing factors can be assessed in terms of their effects on the resources needed by an ecosystem to sustain functional processes and maintain ecological integrity. Thus, we adopted a resource-based approach for addressing ecological potential at Yucca Mountain and response to both global warming and a hot repository.

Now, in the case of Yucca Mountain, the environmental-forcing factors will be atmospheric CO_2, temperature, and moisture. What information is needed for the research-based concept? With respect to a hot repository, the first thing is to understand the characteristics of the environmental-forcing factors involved.

We got some insight into some of this this morning with Tom Buscheck's work. So, we have questions about when will a temperature increase reach the root zone, when will it peak, and how long will it last, how large an area will be affected, and will the local climate be altered by the thermal effects of a repository? In addressing these kinds of issues, we've used information available in the literature like as shown in the next figure.

Some of this doesn't correspond with some of the best cases that were put forth by Dr. Buscheck this morning. Most of that information that he presented on the top 3 meters of the soil surface was new to me and I'm working with information that was available in his earlier publications that dealt with largely the depth from the top of the surface down to about 7 meters. In that work, the affected area has been assumed to be circular and covering about $7km^2$ or 3 square miles or approximately 2,000 acres. After 200 or 300 years, perhaps as long as 500, a depth to 7 meters would begin experiencing increased temperature. Peak temperature would be reached after 600, 800, or 1000 years, along those lines, and last for a few thousand years. As for the temperature profile to a depth from the surface to 7 meters, we've assumed a range from 2 degrees to 13 degrees. There will be a mosaic of temperatures across the surface, as well as at various depths due to the eco-topographic diversity at Yucca Mountain and the variable nature of the bedrock.

Next come questions about the nature of the ecosystem at Yucca Mountain when the subsurface temperature begins to increase from a hot repository in a few hundred years. Now, this would probably occur, we're assuming, somewhere between 300 and 500 years, and because of the anticipated effects of global warming, it can't be assumed that the ecosystem around the year 2500 will resemble its present conditions.

This figure reads from bottom to top and everything pertaining to global warming or below the dotted line pertains to global warming. Climatologists believe that a doubling of pre-industrial CO_2 levels in the atmosphere ultimately will lead to an increase in global mean temperature by about 3 degrees Centigrade from what the current level is today. The present mean temperature already is about 1 degree above pre-industrial levels and there's anticipated increase of another degree within the next 50 or 60 years and it's also anticipated that we'll start seeing substantial stress to ecosystems in about that time frame.

There's uncertainty, of course, about the rate of global warming, but we're assuming that the peak increase of another 3 degrees from today's present temperature now will occur in about 100 to 200 years. By the time thermal impacts from the repository reach the root zone at Yucca Mountain, say in 300 to 500 years or so after the repository is loaded, the vegetation in the ecosystem will have changed from what they are now. The question then becomes how to determine the potential of the ecosystem after another 300 to 500 years and this is where the concept of environmental-forcing factors and their effects on ecosystem resources help define potential environmental conditions and indicate whether the future conditions would, in general, be ecologically favorable or adverse. We believe that this is the best that one can hope to accomplish in looking into the future.

Once the environmental-forcing factors have been characterized and we have a ways to go on that yet, the next challenge is to identify the minimum number of components and connections within the ecosystem network that are needed to understand how an ecosystem functions and to predict how it will respond to anticipated environmental changes.

Some of the functional processes and issues important to ecosystem potential are shown in the next figure. And, that primary production is, of course, a principal factor regarding ecosystem responses to environmental-forcing factors. For example, if an environmental-forcing factor alters the eco-physiology of vegetation at Yucca Mountain, changes in many other aspects of the ecosystem also would be expected to occur; for example, among consumers and decomposers, as well as the ecosystem processes that they're involved in.

Water and nitrogen have significant effects on plant growth in deserts. If global warming were to increase drought conditions, reduced availability of nitrogen also would occur. A feedback effect would result because low nitrogen availability, in turn, reduces water use efficiency. Further complications of that sort would result from the opposing effects of concurrent increases in atmospheric CO_2 and in temperature because increased CO_2 stimulates plant growth, and in deserts, higher temperature typically reduces growth.

This figure summarizes some of our preliminary conclusions of our work to date. We believe that the resource-based approach to understanding ecosystem processes provides a framework for using predictive models to address long-term ecological potential. A variety of alternative species may be available that can fit the altered environmental conditions of a site and, if not, the site will remain dysfunctional. It will lose its integrity and remain in that state until a match between compatible environmental conditions and biotic components is achieved.

Soil-water-plant-atmosphere interactions and feedbacks are of primary importance in determining future ecosystem potential and understanding these relationships is fundamental to predicting ecological potential and the future conditions at Yucca Mountain. We feel studies of that nature should begin soon. Our preliminary analysis of the long-term ecological potential at Yucca Mountain is leading us to believe that until empirical information to the contrary becomes available, the worst-case scenario should be pursued for the site.

This figure presents such a scenario. Here, extreme environmental conditions as reflected by the first four items in the figure ultimately lead to the elimination of vegetative cover at the site. And, finally, increased wind or precipitation contributes to accelerated erosion and infiltration. So that, finally, the question becomes would the performance of a repository be influenced?

And, our recommendations to DOE in regards to the question posed by the worst-case scenario is that by integrating environmental impact assessment with performance assessment, insight into the issue of long-term performance of a repository can be provided. The use of processed-based ecosystem science that includes a resource-based approach and quantitative modeling to understanding environmental changes will help accomplish this.

The state also encourages DOE to convene an expert advisory group to guide the study of the long-term potentials of the ecosystem at Yucca Mountain and its consequences to repository performance. The approach that I've discussed today and that's recommended by the state requires specialized expertise and, by using an advisory panel, the DOE also would gain better understanding of the uncertainties involved in predicting long-term ecological potential. Lastly, we hope that the standard adopted for studying the ecosystem at Yucca Mountain becomes one based on the best that ecosystem science can offer and that the concept of legal sufficiency is made compatible with good science.

Thank you.

DR. BREWER: Thank you, Charlie.

Are there questions from the Board?

DR. BOWERS: Charlie, if you could separate the effects of the biotic component of the ecosystem to the performance of the repository, is there anything about Yucca Mountain ecosystem that makes it special from other ecosystems in the northern Mojave Desert?

DR. MALONE: No, I don't think so. The ecosystem that's there is--the only thing is that it's in the ecotone between the Great Basin Desert and the northern Mojave. And, from the literature I've read, ecotones are considered to be the most susceptible to things like global change or any stress because the plants are already at their limits. The vegetation and other species are coping with the--or their physiological adaptations to that environment are tenuous.

So that stresses would have more effect within the ecotone than they do otherwise. That's the only thing I can think of.

DR. BOWERS: But, we're talking about ten square kilometers compared to thousands in that ecotone?

DR. MALONE: Yes. And, no, I don't see anything much different within that if that's what you're talking about.

DR. EHLERINGER: The last time we were here in November we heard a lot of data on tortoise studies that DOE was conducting and on re-vegetation studies. What you're proposing here is a refocusing of their efforts towards an ecosystem concept instead of the community approach that they have. Are you suggesting that they do not need to continue the studies on tortoises and re-vegetation?

DR. MALONE: No. I think certainly the work that they're doing now will be useful in determining long-term future ecological conditions. And, what is needed is a melding of the community-based approach, population and community consistent approach, with the process-functional approach. And, to go with just one or the other won't do. You need really both and then they need to be integrated. This is where, I think, it's essential to get a panel of experts together to identify what the critical functions are, what the critical compartments and components of the ecosystem are, and narrow down as finely as possible what's considered to be the essential characteristics of the ecosystem and the biota that have to be characterized.

DR. BREWER: Other questions or comments?

(No audible response.)

DR. BREWER: Thank you very much.

DR. MALONE: Thank you.

DR. BREWER: This morning in my opening remarks that kicked off the panel's activities, I made much of the fact that we are not doing law; we're not trying to practice law. That the presentations of this afternoon--and, this is by way of reminder--are really intended to put the legal context and framework, in terms of constraints and timing, into better view for us to think about--all of us together to think about the scientific and technical implications. In inviting our next two speakers, have not entertained or begun a legal analysis of Yucca Mountain in terms of the Environmental Impact Statement or anything else. I'm making much of this because the Board's primary, if not sole reason, for being is to examine science and technical details related to site characterization and the eventual disposal of high-level waste.

What we are seeking in the next presentation and the one that follows are general matters of guidance that can be factored into everyone's thinking, the Yucca Mountain Project, DOE, and the thinking of the Board, indeed, as to what are the things that we should be working toward and working back to the present in terms of the science that must be in place. I think in Russ Dyer's presentation, the very final presentation before lunch today, one had some sense that perhaps the draft EIS and EIS activities are not--from my point of view, not well-integrated as yet in terms of the full implications from a scientific point of view of what's involved.

So, let me repeat to be really boring about it for about the ninth time, we are not doing legal analysis. What we're trying to do is to see what the implications are of the legal framework in which all of this activity is being undertaken; implications for science and technology which is our business on this Board.

Now, having said all of that, it's my pleasure to introduce Ms. Elisabeth Blaug who is an attorney with the President's Council on Environmental Quality. She's going to give us a tutorial. I hope that's what you're doing, Elisabeth.

MS. BLAUG: Uh-huh.

DR. BREWER: Thank you. On the requirements of the National Environmental Policy Act with particular emphasis on areas that are likely or could be of concern when preparing the EIS for Yucca Mountain, knowing full well that Yucca Mountain has got its own special characteristics.

So, Elisabeth, if you would?

MS. BLAUG: Being the good lawyer that I am, I don't have any slides or--I'm just going to talk from sort of the top of my head, but not really.

The requirement to prepare an environmental impact statement for Yucca Mountain comes in the NWPA and it notes that when the Secretary makes a recommendation to the President to approve the site recommendation, she must also at the same time submit a final Environmental Impact Statement. So, we need to sort of work back from there to determine what the proper time is to begin the EIS process and I'll get to that in a minute.

But, first, I want to explain what the Council on Environmental Quality is and a little bit more about NEPA. NEPA was passed in 1969 and it was the first broad environmental statute in, possibly, the world. It was the first piece of legislation that was passed by President Nixon in the decade of the '70s. And, the purpose of NEPA is to encourage the productive and enjoyable harmony between man and his environment. Title 2 of NEPA created the Council on Environmental Quality which serves two primary functions and one is to advise the President on national and international policy implications--environmental policy implications and perhaps, more important, to oversee Federal agency implementation of NEPA. Pursuant to that, we promulgated binding regulations in 1978 which I have some copies of. Title 1 of NEPA or, more particularly, 102(2)(c), provides the means by which agencies consider and integrate environmental values into the decision making process which really is what NEPA is all about. And, it's done through what's called a detailed statement in NEPA which we all know as the Environmental Impact Statement.

NEPA, unlike Endangered Species Act, for example, or any other media-specific law that we're all familiar with --CERCLA, RCRA--is not a substantive law. It's procedural in that it does not stop an agency from taking an action. It was not responsible for the snail darter potentially stopping the TVA Dam back in the '70s. What it does do is requires agencies to integrate environmental values into the decision making process and there are two key components that allow them to do that. The first is through public involvement. Through getting the public involved early-on and throughout the process, it really does promote better decision making. And, number two, through the alternatives analysis requirement including mitigation measures or measures to mitigate the adverse impacts of any action that an agency takes. Those are the two key components.

And, as I'll explain throughout my discussion, NWPA has very--as Wendy said, streamlines the NEPA process. So, it has some provisions in the Act that are a little different than what NEPA normally requires; one of those being alternatives analysis. As Wendy also mentioned, the Act is very specific in what alternatives need not be considered; that including sites other than Yucca Mountain. I do want to make clear and reiterate that that does not foreclose the options such as alternatives in project design or methodologies. Those are still alternatives that must be considered. And, also what must be considered is the no-action alternative. What are the impacts of doing nothing? in other words, of not building a repository on Yucca Mountain? So, those are requirements under NEPA and the CEQ regulations.

The process for NEPA normally would begin when an agency formulates a policy or a plan or a regulation; when it first formulates, when an idea gels to the point where enough information can be presented to the public to begin the NEPA process, to decide what are the important factors that we must consider or how can we make this decision better? The time is not once the decision is made because then the NEPA process is kind of futile.

But when, as here, a decision is made--and I think that we can probably all concede that the time at which the EIS process would begin would be once the site characterization studies are completed or at the time DOE has a pretty good sense that the characterization studies, if they do indeed determine that Yucca Mountain site is feasible, that would be the time that DOE would want to start the formal scoping process.

The first thing that DOE is going to do when this time comes is to publish a notice of intent in the Federal Register which will briefly describe what the proposed action is and what the possible alternatives to that proposed action might be. And, it will also describe what the scoping process is going to be, if it has a sense of how many meetings it might hold, if indeed there's going to be public meetings. There's no set way to hold scoping sessions, but public meetings are generally the most popular way to get the public together. It will describe where the public meetings will be held, how they'll be held, why they'll be held.

If you look at the 40 most asked questions about the CEQ regulations which are back there on the table, it emphasizes that the scoping process--that is getting the public together--can begin before the notice of intent appears in the Federal Register. In fact, the scoping process can and, especially in this case, should begin before the decision to even prepare an EIS is made. So, in other words, I think it's a very, very good idea that the scoping sessions are going to start, as I think I heard, early next spring '95. The reasons why I highly encourage this is, by doing this, you achieve two very important goals. And, one is the earlier scoping sessions truly do lead to better informed decision making.

When you get the public involved, you will find out things--you will develop information that you never knew was out there. The public always adds to the decision making process. Number two, it assures the public if you start the public scoping next year, it assures the public that you have not made decisions that limit your options once the EIS process actually starts. In other words, the public really does have a say in what the EIS is going to look like. The public really does have a say in what the decision is going to be. Angela is going to talk after me and she's going to talk a little bit more about the scoping process which includes through the public involvement, other agencies, Indian tribes, whoever is interested, what is going to go into the EIS, what the EIS is going to look like.

When is scoping over and when can the agency move on and say, okay, it's time to start writing or drafting the Environmental Impact Statement? This project, needless to say, is probably unprecedented. It's a big project. It's probably controversial. It's going to affect not only the local people, it's going to have effects on a statewide level and, surely, on a nationwide level. And, for that reason, DOE would want to set their scoping session accordingly. That is I would strongly encourage holding local meetings, statewide meetings, and national meetings.

I know that, just by way of an example, NRC is in the process of they completed what they called a generic Environmental Impact Statement for the relicensing of power plants. And, after the draft EIS was issued, they received a lot of comments from states and public on what they felt were some deficiencies and some concerns that they had. So, NRC initiated a series of additional scoping meetings based on a regional basis and, so far, they've proven to be pretty successful. So, there's no set way of deciding when, how, where, why, how many, but I think DOE needs to think very strongly about having them on a local, state, and a national level and I think that's where they're headed.

It's up to DOE to determine when it feels that it's covered the gamut, when it's talking to enough people, and when it feels comfortable, and that it's turned over every stone that it needs to turn over in order to get the most information available. It's at that time that it starts to draft the draft EIS.

What's that draft EIS going to look like, what is it going to address? Again, Angela is going to talk more about what goes into the EIS. In terms of what it's going to look like or what sort of form it's going to take, because this is such a unique project and it's not something that stops at a definitive time line--we're talking about hundreds and thousands and tens of thousands of years into the future --CEQ encourages the use of a creative drafting and that there's various options of how you want to prepare the EIS, what form you want it to take. For example, in our regs at 40 CFR 1502.4, there's various ways to evaluate raw proposals, such as this. One is by using perhaps phasing the project through stages of technological development or, as I think was mentioned earlier, by supplementing the document. Maybe, there will be some provision in the EIS and the record of decision, which I'll get into in a moment, of some timetable of how often the document is going to be supplemented, in addition to when new information is received. There is myriads of ways to create the document. Again, we encourage creative thinking and it's something that CEQ will be happy to work with DOE on. It's something that I'm sure the public will be more than happy to have their input into.

So, DOE gets these hundreds and thousands of comments and recommendations from the scoping process. It has to put it into a document. Sometimes agencies, before they issue the draft document to the public, like to prepare what's called a preliminary draft EIS and that would be circulated to the relevant agencies that have either acted as cooperating agencies or they submit it to experts or just entities that they think might want to get a first crack at it before it's released to the public. Again, that's called a preliminary draft EIS. You can call it whatever you want, but it's circulated before it is issued to the public.

After the draft EIS is ready to go, then DOE would publish a notice of availability in the Federal Register which makes the document available to all interested parties. In the DEIS, DOE may, if it has one, select and identify a preferred alternative. That is the alternative that it thinks would most likely be its mission and that would be including considerations of not only the environment, but the economic concerns and other concerns.

It's also at this time that the Environmental Protection Agency, pursuant to not only Section 309 of the Clean Air Act, but also pursuant to a memorandum of agreement between CEQ and EPA, rates--it evaluates and reviews and rates the draft EIS for two things. It rates it for the environmental acceptability of the proposal and also for the adequacy of the information contained in the document. So, in other words, the document may have the best scientific studies, the best data available, it has everything in there that has to be in there, but it's still environmentally unacceptable. That can happen. It doesn't happen very often, but it is possible. So, it will give it a rating of, say, EO-1, EO-2, EO-3; EO being environmentally objectionable, 1 being perfecting fine, 3 being not so fine.

If the document should get, through the eyes of the EPA, the worst rating, then it means that it could be referred to CEQ unless DOE can resolve the problems that EPA has--with EPA and any other agency that's involved in it. If resolution cannot be reached and DOE stands by its document, then EPA likely would officially refer the matter over to CEQ. Although we've had numerous, numerous requests for our involvement in an official referral, we've only accepted approximately 25 over the last early 25 years. We only accept referrals that are of national significance. Although I don't think it would happen in this case, this probably would be if it were referred a question of national significance, and we would try and work with DOE and any other interested agency in trying to work out a resolution and we would ultimately issue an opinion on what we think the resolution should be. The referral process does provide that ultimately the President can make the final decision. That's never happened.

Assuming that all goes well and EPA is very happy with the document which we all know is going to happen, then a comment period of at least 45 days must be provided for. That is the public gets a chance to submit comments to DOE for 45 days. That doesn't mean that DOE has to hold additional public hearings. It means that DOE has to accept comments from the public for 45 days. Now, I know that judging from the time line that I saw before lunch, it looks like DOE is anticipating at least a couple years to consider and incorporate all the comments that it feels it might receive which is probably--they're probably going to take very bit of that time. DOE must respond to all comments it receives. It doesn't necessarily have to respond to comments like "I don't like this project, I hate this project, I don't want this project in my back yard". Those are not the kind of comments that DOE has to respond to. It has to respond to any and all substantive comments.

Now, I'm sure there's some anticipation that there's going to be a lot of comments received and how on earth are you going to respond to all those comments? There aren't enough trees to create all that paper that's going to be needed. By way of another example, an interagency Environmental Impact Statement team that prepared the Spotted Owl EIS which was just recently issued received 110,000 comment letters; not comments, comment letters. And, what they did is they used representative comments. Obviously, with 110,000 comment letters, you're going to start seeing the same comments over and over and over again. You can clump those comments together into one category and respond to that category of comments. So, it's really not as daunting a task as you might think.

Those comments must be incorporated into the Final

Environmental Impact Statement. DOE must explain why it rejected certain comments and, for those comments that it does not reject, it must incorporate them into the FEIS. Once the FEIS is completed and ready to go, again a notice of availability must be placed in the Federal Register and there must be a minimum 30 days, what we call, cooling-off period before DOE would ultimately issue its decision. That 30 days is the time for the public to get one last look at the document and, while it's not really what we would call a final comment period, it's a chance for folks to pull down and figure out if there's any outstanding issues that need to be resolved.

Let me just re-emphasize the Environmental Impact Statement is not a decision document. It's a document that evaluates the environmental impacts of a proposed action. The decision document is called the Record of Decision or the ROD and that is also a public document that is issued no less than 30 days after the Final Environmental Impact Statement notice of availability has been published.

The Record of Decision is also--it doesn't have to be published in the Federal Register. It does have to be made available to the public. I think with an action like this, it would probably be in DOE's best interest to publish it in the Federal Register because of the great interest. The Record of Decision is a concise document and it tells the public essentially why it came to the decision it did, why it chose the alternative it did. The Record of Decision is going to briefly state which alternatives were considered, briefly which were rejected, if any. It's going to cite, identify an environmentally-preferable alternative if that alternative was different from the alternative that was selected.

Again, a point I feel like I should make is NEPA is not a substantive statute. It does not require DOE or any other agency to choose the most environmentally beneficial alternative. Again, the purpose of NEPA is to insure that DOE and any other agency integrates environmental values when it makes its decisions. The goal is to mitigate as many adverse environmental impacts as you can, but it is not to necessarily come up with the most environmentally beneficial alternative. I just want to make that clear.

The Record of Decision will also articulate that all practical means to avoid or minimize environmental harm have been adopted. In other words, any mitigation measures that were practicable have been adopted. And then, any other mitigation measures or monitoring and enforcement measures that might be taken by DOE which I think in this case it would probably be a very good idea. Any commitment that is made in a Record of Decision is a binding commitment. Any mitigation measures, any monitoring measures, any measures that are taken that have led to this decision are binding. That Record of Decision is a binding document.

I should note that for some odd reason nobody has ever filed a lawsuit in which a plaintiff has argued that a Federal agency has failed to implement mitigation measures as outlined in its decision. About 15 years ago maybe, a lawsuit did start to go through the Courts, but a settlement was reached between the agency and the plaintiffs. So, surprisingly, I can't cite any case law to you that confirms what I've just told you, but believe me, it is true.

Okay. Just a few additional points I want to make about what goes into an EIS before I take questions and Angela continues. CEQ regs require agencies to consider the cumulative effects of its proposed action; that is the effects of past, present, and reasonably foreseeable actions, regardless of who, what, whether there's a Federal agency or a non-Federal agency or a private entity takes those actions. So, what is reasonably foreseeable, 10,000 years into the future? We know that there's probably going to be impacts 10,000 years into the future. They're reasonably foreseeable, but how on earth do you assess those impacts?

CEQ regs at 1502.22 discuss what happens if you need information that's either unavailable or incomplete in order to make a decision. If there's incomplete or unavailable information that's essential to a reasonably informed decision in selecting a preferred alternative, if the cost is not exorbitant in finding that incomplete or unavailable information, if it's not exorbitant, then that information has to go into the EIS. Now, what's exorbitant? I can't answer that. That's something that every agency has to decide for itself. If the cost is exorbitant or if the information is simply unavailable, there is no credible scientific evidence or study or methodology that's going to allow you to determine certain information 10,000 years into the future.

And, there's a few things that have to be discussed in the EIS. That is, first of all, the agency must explain that there is incomplete or unavailable information. Agency has to draft a statement of the relevance of that incomplete or unavailable information. Why is that information relevant to informed decision making in this particular action? The agency would then have to summarize the existing credible scientific evidence which is relevant to evaluating the impacts; that is what's the most, what's the best, what's the most feasible, available, credible, scientific evidence that DOE, for example, can rely on? How far can it go before it becomes just simply not feasible. And then, finally, the agency's evaluation of such impacts based on theoretical approaches or research methods that are generally accepted by the scientific community.

I should also note that I've heard several times the phrase "worst-case scenario" and the CEQ regs at one point noted that a reasonably foreseeable impact was the worst-case scenario. It's reasonable to foresee the worst possible situation could occur. After our determination of worst-case scenario, we just came to the conclusion at one point that that really wasn't a feasible way to evaluate information. So, what our regs now address is low-probability catastrophic impact if there is credible scientific evidence to support such an occurrence taking place. So, in other words, if you have evidence that suggests that slight possibility though it might be, this is the worst thing that could happen, it must be addressed in the EIS.

I think I'm going to stop there and go ahead and take questions. I've tried to leave a little bit of time.

DR. BREWER: Fine, Elisabeth, thank you very much.

Are there questions from members of the Board?

(No audible response.)

DR. BREWER: There will be, as I should remind everyone in the audience, we will have a moment after the break for public questions or presentations. And, after the break, we go to a round table where all the presenters will be around the table. We'll have a discussion among ourselves, at which point the public can also ask questions.

So, right for the moment, are there questions from Board members or our consultants?

(No audible response.)

DR. BREWER: Fine. Thank you very much. Stick around. I think there will be conversation later.

MS. BLAUG: Thank you.

DR. BREWER: The last speaker on the formal agenda for the day is Ms. Angela Foster who is an attorney in the Department of Energy's Office of the General Counsel. Angela will review for us some of the case law that has developed as lawsuits have challenged various aspects of NEPA and as Court decisions have interpreted the laws' requirements. I hope that she will also be able to share with us some of the hard-won lessons learned in the experiences that the Department of Energy has had in preparing other EISs in the past.

Angela, please?

MS. FOSTER: Well, it's good to be here and I hope everyone is doing okay. I'm going to try and stick to a text here because I don't want to overlap what Elisabeth has so well stated.

As you are already aware, the EIS has not been done regarding site characterization only because there is explicit language in the Nuclear Waste Policy Act that so excludes it. This is considered a preliminary decision making activity and, thus, the EIS was not required at that stage. However, NWPA does indicate that once the Secretary decides to recommend to the President that decision itself is a major Federal action. That will start the major Federal action and then that is when your EIS needs to be submitted to the public, as well as to the President.

There are many provisions of NEPA, but one of the key provisions and one of the ones that causes a lot of litigation has to do with Section 102(2)(c). This is the provision requiring preparation of an EIS for major Federal actions significantly affecting the quality of the human environment. Courts have stated that Section 101 states that the agency must consider the environment, but nevertheless, the decision falls within the agency's discretion to examine and choose between and among competing public interests.

As Elisabeth has stated, again the agency can set and accept an alternative that is environmentally acceptable, even though it is not the environmentally-preferable one. So, there is a great deal of discretion given to the agency in making its decision. The Courts tend to evaluate the procedure whether or not all of the Is have been dotted and the Ts have been crossed. The decision itself is left up to the agency. This is considered the Courts trying to avoid the administrative law aspect. So, that's why they just stick to the points that are clearly--where the criteria has already been set aside by CEQ guidelines.

So, the Court evaluates whether or not a hard look has been made of the alternatives and whether or not a rule of reason has been applied in looking at the alternatives and whether or not alternatives have been dismissed arbitrarily and capriciously or whether or not they've been fully evaluated. Courts also frown on conclusions being made at the outset, but yet the process is just followed as a matter of just jumping through the hoops to give the perception that this is something that is justified, the processes for information to be obtained by the Department of Energy in this instance and also to inform the public. And so, it's a situation where we're trying to get as much information as we can to make the best informed decision that we can. It's not a matter of just an obstacle that so many people view it as.

The alternative analysis that is required in a NEPA document and EIS document, Courts consider whether the proposal that is made by the agency forecloses the opportunity to consider alternatives. There have been unsuccessful attempts to present alternatives that clearly would not be feasible, but yet they're just presented as a matter of jumping through the hoops to give the appearance of giving a hard look, but the Courts look at this very harshly and will dismiss the document altogether. Not that that's a lesson learned by the Department of Energy firsthand, but that is something that we've witnessed by reading the cases.

Agencies shall not commit resources prejudicing the selection of alternatives before making a final decision. So, the whole decision making process is so that you can enhance your decision making in the long run.

The range of alternatives available. You're not to overlook reasonable technology and transportation. Note that infeasible alternatives are certainly unreasonable, but feasible alternatives may not be unreasonable. I have a quote here from a Vermont Yankee case that has set a precedent that "the detailed statement of alternatives cannot be found wanting simply because the agency failed to include every alternative device and thought conceivable by the mind of man. Time and resources are simply too limited to hold that impact statement fails because the agency failed to ferret out every possible alternative."

So, there is a rule of reason there and the only problem with that is people may view the reasonable analysis as being so vague. But, if you assume that you're a reasonable person, then most likely you're looking at reasonable alternatives. And, rule of reason governs both the alternatives that the agency discusses, as well as the extent to which the agency is to discuss them. So, you may go into a great deal of length regarding some alternatives and not others depending on the situation at hand. If it's something that's extremely controversial, as the situation you have here, then clearly you need to analyze with a great deal of depth.

Detailed discussion is not required of alternatives that are deemed remote and speculative. Agencies do not have to look at a crystal ball to figure out what their alternatives will be and in assessing the alternatives. Just take a very hard look at that. Therefore, I would say that since the Nuclear Waste Policy Act is very explicit about the alternatives that you do not need to discuss, such as the alternative sites, you do not need to go into any discussion of that or whether or not there are alternatives to geological disposal. But, that does not foreclose the obligation that the Department has to actually discuss the methodologies or technologies available. So, you can go into a discussion of waste packaging, for example, or thermal loading design or whatever else you may see as being something where you can learn from the public, as well as the public can learn from you.

Nothing is set in stone from the very beginning. There is opportunity throughout the process to supplement the document, the EIS document. So, where this process may go on for a very long time, we may have a situation where a lot of the information and the data collected at the early stage may be stale by the time the document is actually written. So, a concern--well, one way to address that is to allow for the supplement of the documentation. So, if insufficient information is available at the outset, there is opportunity to supplement that document later on in the process.

One caution there, there is no requirement that the supplemental EIS go through the scoping process. So, you may--the Courts may frown on you saving the very best for the last, meaning the supplement, and denying the opportunity for the public to fully evaluate what you're presenting. So, I would strongly suggest thinking the process out in the very beginning so that you do not have to second-guess yourself later on in the process.

As Elisabeth mentioned, you need to discuss the no-action alternative, as well, and that means what if you did not do the action, at all, and what would be the impact of that? And, clearly, the obvious would be that you'd be breaking the law since the statute clearly states that you need to take this action. That can be addressed, as well as assessing the impacts of if you decided to do just that, the environmental impacts. But, do not avoid the obvious simply because you assume that anybody that knows anything about this project would know that you're required by statute to go this route.

Since the action itself is the recommendation by the Secretary to the President to develop this repository only after site characterization is at its finish, is completed, then I would say the baseline starts at that particular point. You take the site as it is at that point. Even though you have undergone site characterization, you do not revisit what the impacts were of the site characterization phase in itself, but you take the site as it is which would mean after site characterization has already taken place.

Another very important issue that I see here since this may have broad implications would be the transportation impacts. And, I would say that since you do not know the details of where you're going to be transporting this from--even though you only know where it's going to end up, you do not know what routes. The best you can do at any given time is all that is asked of you by way of NEPA. So, if you do not have sufficient information, if you do not know from whence all of the materials will be coming, then it's best to just assess as you best know how, as fully as you can, and then, supplement after that.

Now, the scoping process is a process that needs to start at a very early stage so that you can determine the scope of the issues that need to be addressed in your document and how these significant issues relate to your proposed action. The intent of the process is to solicit public input to the Environmental Impact Statement and then, after you go through the process, sometimes this is a matter of comity. You're allowed to disburse the document, your responses to the individual comments, or groupings of the comments, as Elisabeth mentioned earlier. So that you can also inform the public in a way that they may want to help you out further along in the process without feeling that you're recalcitrant in any way.

Again, I cannot overstate the point that the purpose of the whole process is to allow everyone the opportunity to make such an intelligent contribution to such a project that is going to have such wide and broad-sweeping implications for years to come even after we're all gone. And, not only that, the participants get a chance to meet one another, hear each other's concerns, things that they may not have even thought of themselves, and this only encourages further debate and discussion.

Again, I would not want you to be afraid of the process simply because you think, well, it can be very antagonistic at times. But in fact, as long as you realize that the agency does have the discretionary power to choose its own action and since you have received explicit direction by way of Congress in this regard, this is something where you just try to make it as comfortable a ride as possible.

And, that's it. I'd entertain questions if you have them.

DR. BREWER: Thank you very much.

Are there questions from members of the Board?

DR. NORTH: I wonder if you could give us an example to illustrate the time requirements and what the process entails in preparing the kind of EIS that might be required for Yucca Mountain given we go ahead? I'm not sure what a good analogy might be. The first one that occurs to me is a project that's now dead, but was approved not too long ago and that's the Superconducting Supercollider. Was there an EIS prepared on that?

MS. FOSTER: Yes, that's true. I must say this is a very unique situation, but I can tell you this. No EIS document has gone through the process in less than a year. In fact, closer to two years is the typical process time that's allowed. We try, at least of late, to expedite the process as much as possible. But, I would say since this is such a controversial area, the importance would not be so much on trying to get things rolling as quickly as possible, but to be as thorough as we can from the outset. So, it's hard to put an actual time frame on it. I cannot say that because then I would be precluding the opportunity for maybe some very valuable discussion that may be prompted later on in the process.

DR. NORTH: Let me ask a follow up again. Has there been such discussion to your knowledge within the Department of Energy with respect to how long it might take to prepare the EIS and what level of resources might be required for the effort to put the document together?

MS. FOSTER: No.

MS. DIXON: Can I help you, Angela?

MS. FOSTER: Yes.

MS. DIXON: The answer is yes, there has been. This is something that we--

MS. FOSTER: Oh, there has--yeah. In your office, there has been, but certainly not at headquarters. We would not even begin to try to speculate on how long it would take.

DR. BREWER: Other questions from the Board?

(No audible response.)

DR. BREWER: Angela, thank you. And, please, stay with us for the round table in a moment.

There is one additional comment that I would like to read into the record. We also invited the lawyers from the U.S. Environmental Protection Agency in the Region IX Office. Region IX because of its location will be the lead agency, although not--the legal counsel in terms of all the environmental impact activity. And, the lawyer in charge is Jeanne Dunn Geselbracht. She's the Environmental Review Section, Office of Federal Activities. I'd like just briefly to read what are the summary points of this letter into the record and then it will be available, the whole thing, if anyone wants to see it when we finally put our notes together.

"I'm sorry that EPA will not be attending the March 21 and 22 field trip and meeting regarding the Yucca Mountain Project. In response to your questions regarding EPA's role in reviewing environmental impact statements, I've enclosed a summary of our anticipated involvement." And, it covers much of the territory that you mentioned, essentially all of it.

"We would also like to take this opportunity to stress a few additional points to keep in mind as you work with the Department of Energy on this project." Number one, "The EIS must rigorously explore and objectively evaluate all reasonable alternatives." A comment that both of you made in your presentations.

Number two, "The EIS is a public disclosure document. It is the public's opportunity to review a proposed project. Information should not be withheld from the EIS on the basis that it will be included in the license application. The licensing process is not as accessible to the public and agencies become vulnerable to legal proceedings when their NEPA documents lack important information. Agencies do not gain time by withholding information in the EIS; in fact, they often lose time by getting tied up in litigation."

Point three, "Also, please note that health and safety effects should be discussed in the EIS. Environmental 'effects' as defined in 40 CFR 1508.8 include ecological, (such as the effects on natural resources and on the components, structures, and functioning of affected ecosystems), aesthetic, historic, cultural, economic, social, or health effects, whether direct, indirect, or cumulative."

And, the final point that she makes, "We recommend early scoping for the Yucca Mountain Project in light of its national and regional significance, public controversy, and the potential need for long-term studies to address public concerns."

I will, as I said, make this available. It will be public, obvious to everyone involved.

MS. BLAUG: Could I just make a point?

DR. BREWER: Please do.

MS. BLAUG: I just want to point out that EPA's role in the NEPA process is to evaluate and rate environmental impact statements. If you have legal questions, implementation questions, NEPA and the CEQ regs, you should talk to DOE lawyers or the CEQ lawyers and EPA would not act as legal counsel in that aspect. They are simply reviewers and raters of the document.

DR. BREWER: She makes that very clear and also it breaks down the various categories of evaluation according to environmental impact and then adequacy of the statement itself. It's all part of her letter to us.

MS. BLAUG: Great.

DR. BREWER: We are a bit ahead of schedule which is quite wonderful. So, I'm deemed as a chairman, I guess, or boss. What I would like to propose is a 15 minute break with a reconvening at 15 minutes until 3:00 o'clock at which point we will make available time for any public comment before we get into the round table itself. We have one person who has volunteered. If there's anyone else, please come now and let us know that you would like to spend some time talking with us.

Other than that, thank you very much to the presenters this morning and this afternoon. All the presenters at the round table. All the presenters are invited to join us at the round table with our consultants. Board members will kind of fade from view and then we'll have time for open Q and A.

Thank you all very much.

(Whereupon, a brief recess was taken.)

DR. BREWER: Because we've picked up some time, what we are going to do is, we have one request from the public to present--and I'll get to the introductions in a moment--and then, as soon as the round table is over, members of the Board and staff and consultants will stay here, and what we will do is have our own closed executive session, and we will politely invite the public out at that point, we'll stay in this room. And then there will be a break, and we're having dinner together at seven, as a Board, just to let everybody know this change in plan.

We had one request from the public to make a presentation of about five minutes. The individual is Marty Rose of the Desert Research Institute. He has comments on paleoecology.

Marty, would you, please?

MR. ROSE: I'm going to use some overheads here, but what I would like to ask for and argue for is a consideration of the long-term paleoclimate record that we do have for this area, and the long-term paleoecological record for this region.

A lot of the discussion that I've heard about ecosystems or about vegetation seems to me to be taking a rather static view. What I would like to argue for is that a dynamic view of ecosystem processes be taken, not only over a few years, or tens of years, but thousands of years, given the very-detailed record that we have for this area.

I'd like to just go to a couple overheads. We have a very long record from this part of the country of paleoclimatic variability and paleoecological variability. This offers us a dynamic view of ecosystem processes, and it can also help us in parameterizing some of these models that have been discussed.

The records that we have, primarily that the Desert Research Institute has been looking at over the last few years are based on palynological records, or the analysis of pollen from lake cores, and the vegetation information that's present in pack rat middens. When we analyze these, we can get a long-term view of species change, change in different tacks over a fairly long time period.

My own background is in paleoclimatology and biostatistics, and I work with tree ring records, which, in the Great Basin, afford us a long-term view, thousands of years long, basically covering the whole Holocene, on a year-by-year record. This tree ring series, which have not really been considered to date in the climatic characterization portion of the plan, are unequivocally the most precise, accurately dated indicators of climate change during the Holocene. With these, we can resolve annual and seasonal changes and variability within the frequency domain looking at periods ranging from about two years in length to 1,000 to 2,000 years in length.

This record may, in fact, co-vary with other paleoenvironmental indicators, such as the pollen records and the pack rat midden records that, for example, my colleagues, Peter Wigand and Dave Rhode have worked on. They represent a different part of climate characterization, a different part of the frequency domain.

These millennia-long records can give us a handle on the duration and the frequency of occurrence of events, of climatic events of different magnitudes that you cannot get characterizing climate variability with instrumental and historic data. I'd like to offer just a few comments on that.

I'm really pleased with the instrumental record and instrumentation that's being done out on Yucca Mountain. I think it's going to afford us a good handle on spatial variability. I question, though, what it's going to tell us about temporal variability.

It's very easy to demonstrate that there's no way a short period of instrumental record adequately characterizes climatic variability and change. Even when we go to longer records, say, a hundred years long, we'll still see differences between those records and what we can see, for example, in the climatic record that covers several thousands of years--well, from the present throughout the Holocene.

It can also give us improved chronological control, the high-frequency chronological record. There are ways of using it to help in quantitative reconstructions when we make the jump from the Holocene to the Pleistocene. It can certainly help in offering an improved interpretation of climate, of the relationship between climate and vegetation dynamics; with further analysis, could give us an unparalleled isotopic record throughout the Pleistocene, primarily of carbon, oxygen, and hydrogen, and, finally, it can allow us a very detailed look at the geomorphic response to climate change that we can't get looking at only a record of a few years.

This portion of a tree ring record from eastern Nevada, for purposes of discussion--just to humor me--assume that this is a mean level of climate, and the lines go up above. They represent cooler and mesic conditions. Where they drop below the line, they represent hotter and drier conditions.

The period of instrumental record on the test site, or at Yucca Mountain, I mean, corresponds pretty much right out here to the very end at the tail of this curve. Even if we take the available instrumental records from nearby in the western United States, let's say that'll get us back a hundred years. That's that chunk of the curve.

I could line up ten of these screens around the room, just to show the paleoclimatic record available for this area, using tree ring series. You can see, just in the last thousand years, some of the changes that are present. If we further took the palynological record and the record of vegetation change and dynamics that's available, we could line up 30 of these screens around the room. What's amazing is some of the changes that take place, and the rapidity with which they take place.

Now, if we take this record--I just have one more--if we subtract this long--this is the long-term mean. Let's say we subtract it from each one of these values so that we have a series of positive and negative departures, then let's sum those, just like we would on a year-by-year basis. Let's sum them just like we would a checking account balance.

What you see when you do that gives you a record that's probably reflective of changes that occur, sort of at a landscape ecology level. Again, we're this blip out on the end of the curve, and just in the last thousands years, if we regard changes going up as changes to relatively more mesic and cooler conditions; changes going down as warmer and drier conditions, there have been some profound changes that have taken place just in the last thousand years, and we have to consider the last 10,000 years.

All I'm asking, in terms of the information that's available, is that we not solely let the historic instrumental record have primacy over the long-term and very-detailed paleoclimatic record that we have. This would be analogous to letting the tail wag the dog. We have the dog. It's in our own back yard. We can see what it looks like, and we can observe its behavior, so let's just take that into account.

I realize I've had to cover a lot just in a couple of minutes, but if anybody has any questions, I'll be happy to address them.

DR. BREWER: Thank you, Mr. Rose.

Are there questions from anyone around the table? Yes, John; John Koranda.

DR. KORANDA: The chronology doesn't get us back to the post-glacial maximum.

MR. ROSE: Right.

DR. KORANDA: But do the lake settlements and the palynology get us back that far?

MR. ROSE: Oh, no, this record gets us back over the--throughout the last 9,000 years. It won't get us into the Pleistocene, but there are ways of using--looking at the response of certain tree species and woody shrub species, that we can model the tree ring and the climate relationship. These are some of the same species that occur in the wood rat middens and in the palynological record that do make the jump from the Holocene into the Pleistocene, and, in fact, you know, go back 30,000 years or so.

DR. KORANDA: In that distinctly warmer period, do you see changes in the pollen flora, then, in the Great Basin?

MR. ROSE: Yes. Where the tree ring record that I have, that spans the Holocene, overlaps, let's say, the palynological record from lower Peranicate Lake. There's a record that goes back 4,000 years. You can see the same changes that I see. You see them at a much lower frequency, because you're dealing with a different record, and a record that's not dated, obviously, to a yearly basis, but you do see these changes, and one of the reasons why we would want to look at this record in detail, especially where it overlaps the palynological record, is that we can create quantitative reconstructions of precip or temp, certain aspects, or of drought, something like the Palmer drought severity index that goes back, let's say, 4,000 years.

We can then calibrate in a numerical fashion the palynological record with that, which we can then take that farther back in time in a quantitative standpoint, not a strictly sort of verbal argument about what we believe is happening.

DR. KORANDA: You always feel better when you can find another independent marker in your chronological material, and in lake sediments, they often use the deposition of cesium 137 from fallout, which, per 1964, is the maximum. Have you done anything like this?

MR. ROSE: Well, I mean, that brings up a very interesting point. When you look at the annual tree growth records, and we can look at, we have a network of chronologies from Mexico up into Canada, covering the whole western U.S. We have stuff in the eastern U.S., and virtually all around the world.

If you consider those annual growth layers as little time packages or time capsules, when we look at those isotopically--and cesium is a very good example--we can pick up--and if you did the analysis on a yearly record--you can pick up all those cesium spikes, the exact year in which they occurred.

If you did that on a yearly basis, you could pick--let's say in the fifties or sixties--you could pick that up, and if you looked at it over a very detailed spatial domain, you could probably even map out changes in concentrations.

MR. HEVESI: Joe Hevesi, USGS.

I'd like to comment. The difference between the historic record and some of the long-term records that we've been looking at is not so much in terms of the variability, sometimes, but in the parameter itself. The long-term records may give us yearly values for like average annual precip, but we really become interested in storms, specific storms, and we have to look at storm frequency, even out hourly rates of rainfall.

For example, our best estimate right now--I shouldn't say best, but a rough guess of a 100-year event is approximately a three-inch storm for a summertime thunderstorm, because we see this appearing in some of the longer term records, and we do need to relate that to things like the El Nino oscillation and the--well, we were looking at the 750,000-year record for a deep one-dimensional model to the saturated zone. So, we do need to integrate the two scales, but I just wanted to mention that average annual rainfall is not a parameter that we're real concerned with, although the model has used that to model infiltration.

When we took our best estimate of average annual rainfall at Yucca Mountain, the answer was zero, and two millimeters north of there, but we don't believe that model. If we double that, we get 20 millimeters at Yucca Mountain, so that's easy to do. We just need to know the amplitude of those graphs that you just presented.

MR. ROSE: Well, I mean, these were just the tree ring index series themselves. This is work that--actually, a lot of it was sponsored by the DOE during the eighties as part of their CO_2 program, and at the time, we had no idea that it would perhaps be relevant to something like this.

It was work I did over about a 15-year period with my colleague, Don Graybull, at the University of Arizona. In answer to your comment, I think there's probably a lot of work that could be done in relating what happens--and, again, we can look at some of the stuff on a seasonal basis--what happens in years when you have very high precip, let's say, in this area, okay? It's basically stuff that happens during the winter. Is there any sort of association if you were to categorize the frequency of storms, as you guys do. In real wet years, do you tend to get more of one kind, you know, than other?

But, this information is certainly important in terms of vegetation and what's happening with surficial processes, and it's a record that goes back a long ways in time.

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

John Cantlon had a comment, to get us into the round table at the same time.

DR. CANTLON: Yes. The tree rings, obviously, of necessity, have to be based on environments where trees grow, which doesn't include Yucca Mountain itself, and I guess one of the questions is, has anything been done to extend the tree ring analysis technique into the desert shrubs themselves?

There was some early work done on Big Sage up in the cold desert. Has anything been done down here with any--

MR. ROSE: Yes, there has been with certain shrubs, but I would take issue with one point.

We don't need to have trees directly on top of Yucca Mountain to make inferences about paleoclimatic variability at Yucca Mountain. We get trees that have a strong regional macroclimatic signal, I mean, we have Mount Charleston, the White Mountains, Telescope Peak, places like that, but, in answer to your question about shrubs, the work that was done on Big Sage Brush was sort of a big push in that direction and not that much has been done because there hasn't been that much interest until recently, and I think that some of the work going on at Yucca Mountain may be a prime example of where we would want to look at variability in woody shrubs and relate it to climate, and look at differences over the various topography.

DR. BREWER: Thank you very much.

One of the general purposes, as I stated at the beginning of the day, for having the round table, is to allow easy exchange, questions and answers based on what all of us have heard today, and one of the general points of having the Panel convened, why we're here, is to see if, out of the day's discussion and some thought and some focus, we might be able to offer constructive suggestions to the Yucca Mountain Project about their environmental studies array.

And, several of us convened, caucused at lunch, talked about what we had heard in the morning. There are some not planned, but at least there has been some thoughts given to at least several of the more salient issues.

John Cantlon, exercising his power as the chair of everything, would like to lead things off.

DR. CANTLON: Well, I think it would be useful if, in our discussions, we can look at the relationship of the below-ground hydrology that Tom Buscheck has been trying to model, to press that up to the point where we're able to make a solid coupling between his models for the upper surface, and the hydrology studies that are going on, and, explicitly, I think what we need to identify is what do each of you need in your studies, what do you need to get done in order to make those data sets talk to each other in an effective way that will be useful in trying to predict the future behavior of the repository?

And, then, pushing that forward to Wendy's planning requirements, she's got to begin thinking about how this environmental program, in its next and future cycles of funding, what those data sets need to be looking at. We're talking about modifying study plans that will be very different from the study plans that were in the base plan.

Likewise, we've got a set of environmental data, ecological data out there that are not yet very well articulated, and I guess I would say not articulated at all with surface hydrology, and we mentioned this morning some very, very simple things that can be incorporated, really, into next year's funding and maybe even this year's funding, and that is, looking at the mapping challenge, to try to make that as an overlay to the watershed units which are the hydrologists' functioning units.

Now, many of those watersheds cut across three vegetation types, from the upper portion of the slope, and cut across two or three of your hydrologic types that you have, the upper, very top, the upper slope, and then the lower alluvial areas that you were talking about this morning, Joe, and now, the question is, do those coincide with the vegetation boundaries, or are they different?

If they coincide with the vegetation boundaries, then you have an opportunity of getting an areal extent, which is now denied you because you've got drill holes, and if one can now begin to get some sort of quantitative estimate of the hydrologic behavior on a surface area basis, now we'd begin to get a handle on what potential inputs are into the repository, what the potential outputs are as we think of Tom's characterization of the heat pipes actually pumping water up into an ecosystem. We need to begin to think what would happen if you now began to drive water up into the bottom of some of the ecosystems.

That means that the vegetation mapping should begin to be looking at the fault lines that are out there, or the major fracture systems. What are those vegetations like? How might they change if you start feeding them water? Going to these paleoecological studies here, we may actually end up with some trees growing up on top of, you know, they were there, the midden. The midden data show that there were trees there at one time.

So, and then the "So what?" question that Tom

O'Farrell was talking about yesterday. What difference does it make? It may make none, but if you're going to be able to provide the kind of information in the EIS, you've got to be able to say, "We've got data that says that's not important. There'll be zero impact on the repository and, therefore, we don't have to worry about it." But, you better be ready, I think, to put some hard numbers behind that.

I think that's really the challenge and we're now talking about process and, obviously, we're dominating the thinking in terms of the hydrologic water relations, evapotranspiration component, because that's the guts of repository performance. If you've got the handle on those processes, then, with the minor exception of the evolution of $C^{14}O_2$, which you may also want to look at at some point, because you're going to get a pulse of that out at some point, it may not make any difference at all, but you better be ready to answer it.

DR. BREWER: I think maybe for the purposes of keeping this organized, we invite responses from the three of you, and then we could take it to our consultants if we wanted to add, and then maybe talk about the policy part of it with Wendy toward the end.

Please.

MR. GREEN: Ron Green, EG&G.

Okay, we start talking about mapping on a functional basis. Let me throw a question back out at the consultants and the Board.

We always think in terms of mapping based on taxonomy classification and life form and that type of thing. What specific type of functional measurements do you think we ought to be measuring to start mapping? I mean, we've got to define some functional process by which we make classifications.

DR. CANTLON: Well, I'll respond, and then I think we can go over to the consultants.

Clearly, what you're going to be mapping are the things that you have knowledge about now, which are your vegetation types, and the hydrologists have maps of their watershed, and then of their individual cells within it, and the location of their drill hole.

In order to make that coupling, you're going to need to understand some measured evapotranspiration. There are a number of techniques by which you can arrive at that, with varying degrees of probability, varying degrees of uncertainty.

Individual measurements, you heard some comment from consultants and--

MR. GREEN: So, basically, what you're saying is we need to classify a vegetation unit by, say, the amount of water in uptakes, the amount of water in transpires, those types of things?

DR. CANTLON: Well, the hydrologists are already calculating it by difference by studying the changes in the soil moisture storage. That's what their neutron data provide them, and their other techniques for measuring soil moisture.

Now, the question is, to what extent can you get vegetation data which now look at the transpiration component of that, and it is the variance in the vegetation coverage that will alter the transpiration component.

The transpiration component is the one that reaches down in the fracture system and extracts 10 to 30 feet, depending on the nature of the shrubs, which gives you a lot harder handle on what the environmental or the vegetation pattern on that slope does.

Again, coming back to Tom O'Farrell's comment, it may not make any difference in the final thing, but you've got to know.

DR. BREWER: Any other response over here? Joe?

MR. HEVESI: Yes, I'd like to respond.

I did not have time to talk about this, but it was on my list in the first slide, and it's probably a good thing I didn't talk about it, because then no one would have had time to eat lunch.

But, a part of our package is surficial materials, and the reason that is there is specifically with the watershed models in mind, and that is the main way we intend to extend some of the information we're getting at the boreholes across the site.

And, the way we do this, currently, Scott Lundstrom with the USGS has mapped out seven surficial units. This is in addition to the Scott & Bonk map that everyone's been using, and then what we're involved with now, we're working closely with Scott and we're going to test the hydrologic parameters of these materials now, and then, hopefully, if everything works out, all seven units will have their own set of hydrologic parameters, and they won't be crossing boundaries, and then we can immediately use that. That's part of both the artificial infiltration program and the surficial materials program.

DR. EHLERINGER: Let me give you an example of what I was trying to mention earlier today about functional units, not breaking it down by species, and so forth.

A question that I think might be of interest, or a data set that might be of interest to the geologist is between the depths of zero and one meter, what plant or plants can extract moisture? Between a depth of one and two meters, what plants can extract moisture? At a depth below three meters, what plants can extract moisture?

And the question is, can you provide quantitative information that tells them which plant or species are capable or not capable of extracting that moisture.

For the past decade, in biologies, date isotopes have been a very powerful approach for demonstrating quantitatively the water zones from which plans are extracting water. The USGS has already conducted studies at the Nevada test site showing that the range of isotopic compositions from winter, summer, spring in deep water is very different and very identifiable, so this might be one different way of assessing quantitatively the different kinds of forms that you have on the mountain, and one way that you could link between the biology and the geology.

DR. BREWER: Anyone else want to follow up on this general topic of the linking, or the potential linking of different disciplines here? John?

DR. CANTLON: Yes, let me fill in, too. Joe added the soils, which I had neglected, and, clearly, putting the soils in there as the big storage area is a critical one.

We also have a meteorological system out there which the hydrologists are already integrating, but the question is, have those been integrated as well with the ecological side as they could be. You've gone through two wet years, two dry years, now you're getting maybe a more normal year. You may get another normal year. It would be very useful to see to what extent the climatic data that you have really can be utilized as a way of understanding the future.

We spent some time yesterday out there looking at the cementation of the fracture system. Clearly, Tom mentioned this morning the role that that may play, and, clearly, that will play a role in the whole area of infiltration, but we could see from the rooting of the plants, that plants are actually rooted down in those filled fissures, and what we don't know is how extensive, how deep the roots are, so some work could be done there, but we do need linkage with the geochemistry, because if Tom's model turns out to be the operating system there where you're pumping what will be essentially distilled water up into that system, what happens to the fracture system?

Are you going to start leaching those fracture systems from the bottom, as it condenses and then runs down? It could well be you'll begin cleaning out those fracture systems, and we're now talking about time periods of several thousand years.

DR. BREWER: Tom Buscheck?

DR. BUSCHECK: I think that scenario is probably not that likely, because where the refluxing is probably going to have a greater potential of altering the fracture properties is where we're in the boiling condensation zone, and under no circumstances will that pertain to the shallow system, so I believe that the condensate generation will be--I have a hard time believing it's going to overwhelm the capacity of the matrix to imbibe, but I have a hard time believing we're going to see this very substantial non-equilibrium fracture flow. The rates are just too low, and I think we're going to be able to bound that.

So, in terms of changing fracture properties, I have a hard time believing that. However, you know, if you have different plant species that are rooting in fractures, breaking up the rock, eventually, could we be reducing, further reducing the permeability of the shallow fractures by beginning to form more soil material within them? I think that would be an interesting thing to pursue.

In terms of process, I think the one process that, from my perspective, has been least studied in this area--and, in fact, has virtually been unstudied--is gas-phase diffusion. It's been studied in soil literature, but in terms of fractured rock, there's virtually no measurements that I know of, and I think it can vary, possibly, over a range of ten or even twenty, and, as I was showing earlier today, if you get a delivery of a lot of additional water vapor and condensation without the benefit from any convection; in fact, it could occur with minimal fracturing, and so, I think that that may be one--we may be able to--if we could measure that parameter in a relevant way, we may be able to determine a process which is more ubiquitous, because it's not dependent on the variability of fracturing; but, on the other hand, perhaps the diffusion coefficient itself may be property dependent, or dependent on the fracturing or the matrix, but, in any event, it has not been really delved into, and I think it's a very important one, because, as I've showed you, it could have an impact in enhancing delivery of liquid to the system for 50 or 100,000 years.

DR. BOWERS: I'll just make a short comment, and that is, I guess, the operative word is that we think that more of an integrated study needs to be done that considers biological and geological, hydrological processes, and a number of us were quite surprised that, I guess, very little discussion had gone on amongst the different agencies doing these different parts of the Yucca Mountain Project, and we would like to encourage more collaboration.

DR. BREWER: Okay. Any further sort of discussion or Q&A on the topic of coordination and integration and putting the disciplines together? I think that was the general theme.

There was a second topic that came up in--

DR. CANTLON: Before we leave that--

DR. BREWER: Oh, from the public.

DR. CANTLON: Yeah. Well, no, we've got a couple of participants. I don't know why they're sitting back there.

They should have been up here at the discussion table. Tom and Kent, come on up. I've been taking your name, Tom, in vain here, so I think you ought to be given the mike to defend yourself if I've misquoted you.

DR. BREWER: Basically, a second topic that came up in the discussions this morning, and then, really, focused on by several members of the Panel at lunch, we were talking about it, is the whole question of the alternative design, which was mentioned in both of the presentations from the lawyers this afternoon, and, Warner, you, for one, have mentioned it. I wondered if you could frame it as a question to get us going?

DR. NORTH: Well, I think the challenge of NEPA is going to be, how do you respond with the significant information? I forget the wording in the law, and the two legal representatives can remind me.

The question I raised this morning had to do with, do you have to consider alternatives for repository design? I believe the answer is a clear, "Yes." One of the alternatives is no repository. I think I got a clear "yes" on that as well.

MS. DIXON: I guess I'd like to ask for clarification on that from the CEQ. The Nuclear Waste Policy Act basically says that the need for the repository shall not be an issue, and if you take the no action alternative, it seems to me that that's contrary to the Nuclear Waste Policy Act that says the need for a repository is not, you know, an alternative to be discussed. I'd like you to clarify that for us.

MS. BLAUG: The purpose for addressing the no action alternative is in any proposed action, you're always going to have a proposed action, and so, you could then say, well, why even address a no action alternative if you have a purpose and need for it?

The reason is to establish, for example, a baseline.

MS. DIXON: I realize that, but the reason why I was asking was that the Act, as a statute, can obviously override NEPA on certain issues, and I just wanted to get a feel for-- I realize what NEPA says with respect to the no action alternative, but it seems like the statement and the Act makes it difficult to say, let's presume there's no repositories and no--

MS. BLAUG: No. If Congress had intended for DOE not to address no action, then it would have, since it pretty much articulated those alternatives that it did not have to look at, so, because of that, the no action alternative has to be addressed.

And, I should add that there's lots of case law that notes that alternatives that are beyond an agency's jurisdiction, even if it's a Congressional mandate, has to be addressed.

DR. BREWER: Is there follow-up on the point, or, Warner, had you finished asking the question?

DR. NORTH: Yeah. I was going to ask Wendy if she had further follow-up on that point.

MS. DIXON: No, I don't. I mean, just like I said, my immediate reaction when we were talking about alternatives to geologic disposal was that saying that there was no alternative was contrary to, and maybe we're slicing hairs here, so...

DR. NORTH: Well, to respond to Garry Brewer's challenge to frame a question, the question that occurs to me is, what are the alternatives that we will want to consider when it comes time for the EIS process, and what does that suggest to us in terms of important questions for what information might we need to consider choices among those alternatives?

For example, to pick up on Tom Buscheck's, if we have fracture zones at the repository depth that might communicate with the surface, might we want to design repositories that either avoid those fracture zones, or possibly seek them, because they have beneficial properties in terms of the dissemination of the heat in a more benign fashion.

DR. BREWER: Tom Buscheck?

DR. BUSCHECK: First of all, we've looked at that, and we've found under the most extreme scenarios we could not substantially reduce the duration of the boiling period by virtue of these zones. It was relatively insensitive. In fact, even for the marginal boiling cases, I couldn't find a great deal of sensitivity, so that process, I think, we cannot take advantage of that process in terms of trying to dissipate the heat.

One parameter--and believe it or not, I didn't consider a parameter in the sensitivity studies, but it's an extremely important one, and that's the depth of burial. The depth of burial could have a profound effect on the ground surface effects, and as we go to a larger and larger repository footprint, are we going to be going to shallower and shallower depths by necessity in terms of, you know, other needs? Some surface needs may have to--I mean, mechanical conditions may not be conducive at a greater depth, and I think we need to take a good look at realistic topographies for various layouts, and it's possible that even though you have a lower thermal load, the fact that you may be 100 meters closer to the ground surface could, in fact, function in the same way as a 100 meter thick heat pipe zone in terms of bringing you effectively closer to the ground surface.

So, you know, there may be a lot of variability as we go to other options, where just looking at this one depth of burial would tend to mask, so I think there are other complex considerations that I want to be looking at, and so, in other words, it's not clear that going to a larger repository necessarily means that you have less substantial ground surface temperature effects if you start to come closer to the ground surface in some instances.

DR. BREWER: John Cantlon?

DR. CANTLON: Yeah, I want to follow up with Tom on that.

You were commenting this morning that as you went into the higher heat load repository, one of the sort of tradeoffs is that the area of impact gets smaller.

DR. BUSCHECK: Correct.

DR. CANTLON: It shrinks because you're pulling it in. This presupposes that you're limited to the 70,000 metric ton starting point of the base plan, I presume. There's a lot more waste out there, and there will be a lot more waste around, and while we are limited at the moment, obviously, by the legislation that we're operating under, Congress has been known to change its mind in this field, and one of the questions I guess I have is:

Has any models been run, assuming that more fuel would come in, even in a hot repository, so that the scale of it increases?

DR. BUSCHECK: I've considered up to 200,000 metric tons; not recently, because we're working more specifically with designers, but, before, when this was more of a free-lance effort, we looked at a lot larger repositories out of convenience because I didn't want to change my grid, for one thing, but that was very early on, but I do have some of that analysis, and the edge effects are much, you know, as I was showing you, doing the AML, the center of the repository at the ground surface sees edge cooling effects, so you don't get a monotonic increase and ground surface temperature rise with a conduction-only case, as a function of AML, but it starts to acid quite a bit.

MS. FOSTER: If I might, I'd like to address Wendy's concern having to do with the no action alternative.

I wanted to state that the no action alternative does need to be addressed, because there is no explicit language in NWPA that excludes such discussion, although you can take some comfort in knowing that the discussion can be very brief. If a major consequence of not doing the action is that you would be violating the law itself, that can be stated, but your discussion does not need to be very extensive regarding the no action alternative, as with the other reasonable alternatives.

DR. BREWER: Anyone care to pursue this?

DR. CANTLON: Maybe it would be useful to sharpen up what the individual groups need or think they need from each other, as guidance for Wendy in thinking ahead and planning her program. What are the areas that you would need in order to make the data sets articulate more effectively?

MR. HEVESI: Joe Hevesi, USGS.

One of the hypotheses that we started out with in terms of vegetation was that maybe we'd see a difference between north and south facing slopes in terms of radiation load. We're not seeing a real clear relationship come out yet, although yesterday I thought I saw some greener plants on the north slope in a couple locations, but yet, that needs to be quantified, and if that can be mapped and related to radiation load, that would be nice.

The main difference in terms of vegetation that we see now is in terms of the side slopes, ridges and washes, like the original model was set up, and most of the evapotranspiration is occurring in the washes, with the larger--I'll call them creosote plants. I forget the scientific name, and this is one area where we can get together, I guess, but the alluvium cover, higher storage capacity, easier routing abilities for the plants, bigger creosote bushes we get more about the transpiration, but we don't know that much yet about the roots and the fracture systems.

MS. DIXON: I guess I would like to second Dr. Cantlon's statement, and I think the point is not for USGS to make a determination as to the difference in vegetation in the north or south basin slopes, because we have that information. The point is that if there's information that you need to conduct your models and analyses, whatever that specific information need is needs to be provided to our office so that we can provide you all with the information if we have it, and if we don't, we can obtain it.

MR. HEVESI: Joe Hevesi. I totally agree, and I was enlightened to that yesterday when we were out on the mountain.

DR. BOWERS: I have a question.

Do the scientists on this project from EG&G and USGS, can they converse directly, or do they have to do it through your office?

MS. DIXON: They can converse directly. Sometimes there's a problem that exists that somebody has information. Most of the information on this program, all the information on this program is public, it's talked about in public forums, it's presented in technical program reviews. It goes into our technical data base, but irrespective of all of those points, there are times when certain scientists aren't aware of data that exists, and for those people who are collecting the data, it's hard for them to know that, you know, there is a data need somewhere. It's really up to the need party to make a statement or make a request or whatever the case might be.

There most certainly is not any problem with participants talking. The only thing that cannot be done is USGS cannot assign EG&G work scope, because I control that. They most certainly can pick up the telephone and ask what the data availability issues are, and that data will and can be provided to the Survey or anyone else.

DR. BOWERS: What about real time-intensive, collaborative efforts? I mean, is that possible, or do these have to be informal?

MS. DIXON: Most certainly. No. If there is a study design; as an example, let's say that the Survey needs information that we do not currently have. The study design can be developed. There would be a request that would come basically from, in this particular case, Susan Jones to Wendy Dixon that would say, "We need the following efforts in our budget for 1995." We would identify what the needs are; I would on my part for EG&G, she would on her part for USGS. The study design would be a collaborative effort. The lead would be whoever needs the data, though. I mean, we would provide whatever they wanted if it was their study design, and vice versa if it was ours.

MR. HEVESI: I'd like to respond to that. A good example of that, we do have a study in plan in Split Wash where we will have a Bowen ratio set up. We're pulling the casing on two boreholes and we plan to instrument them in terms of getting water potential measurements, temperature measurements, and this will be coupled with a network of TDR probes, and also soil heat dissipation probes, heat flux plates, and also, a radiometer network.

And, I see a lot of room to get together on vegetation in that area. This will be taking place in Split Wash.

DR. BREWER: Okay. Thank you very much.

DR. KORANDA: I guess I had a question, or maybe it's just my usual blurting out, but yesterday seemed to be a revelation, mainly to me, but I think to people like Joe, because he began to see that a lot of other people were doing things that impinged on his area of work, and I think the business that we're talking about right now; for instance, instrumentation to determine the radiation loading on north/south slopes, transpiration levels, that sort of thing.

In the area of mapping, for Ron, I would do one thing. You're stuck with the plant association concept here, I think, at least for the present, and I would certainly look into multi-spectral satellite coverage, because you can do some fantastic things, and I think those plants, associations have different reflectance values. You can see the difference, and so you know they have different reflectance values.

With that data, you can produce a map that will show you only that association, or pretty close to it. Also, you can buy, on the floppy disk, USGS quadrangles in a TIFF file format on which you can map yourself crazy. So, these are the things you should be doing, because I've only seen one or two maps on the screen in the two meetings we've been to.

DR. BREWER: Ron Green, would you like to respond?

MR. GREEN: Ron Green, EG&G. I'd like to make two comments.

I don't think I want to give the Board the impression that, you know, this idea of talking to each other is a new revelation. I think, you know, we're changing directions and so we're addressing new issues, and so, the idea of talking to each other is very timely, and so, I think that's why, all of a sudden, you know, these issues have come up, and it's not because we haven't been willing to talk to each other, but the questions that are posed now require that, and I think we can go from here.

In response to John's comment about multi-spectral scanning, we certainly have that capability. I know it's been discussed to get an MSS overflight, and nothing's been said or nothing's been committed. EG&G has a multi-spectral scanner that they use. We can also purchase commercially available data if we want, so we have, you know, two possible sources. That's certainly a possibility.

DR. BREWER: While we're on the general subject, I heard--in your discussion this morning, it seemed as though the whole monitoring design effort had changed from what we heard in November, and I'm wondering if this is a piece of that change or there are some other things that we ought to be talking about around the table, or is this a non-issue?

First of all, is it true that the monitoring design approach has changed?

MS. DIXON: Not yet. It will. That's a plan for the future. We are not modifying until we're finished with this year's data.

MR. GREEN: Right. It's being phased in, and the plan right now is to have that new design in place in '95.

DR. BREWER: Well, this might be the occasion to be thinking about, you know, the general and specific things that we have been putting our finger on right now, I mean, in terms of cooperation of data design, monitoring, the whole range of things; right?

MR. GREEN: Oh, you bet. Yeah, this discussion is very timely.

DR. BREWER: Okay. Other general topics that we need to take up?

Tom O'Farrell, since we invited you to the table, would you tell us here what you told us on the bus yesterday going to Yucca Mountain about the worst case scenario, and who cares?

MR. O'FARRELL: Tom O'Farrell from EG&G; I guess, the resident provocateur.

Any time you have a multi-disciplinary program such as this, that also has superimposed on it science needs, but then, ultimately, you have compliance needs, there is a need, I think, frequently, to stand back and ask questions such as the one that I asked yesterday, which was, basically, if you do project yourself through a series of probable or possible events, and you do come up with a worst case scenario--and the one we were addressing yesterday was if, in fact, the placement of a repository would ultimately lead to the removal of all of the vegetation at the surface, my question was, "So what? How could that ultimately--if you projected that happening, how would that disqualify the site as a repository?"

It was meant to, I think, help stimulate discussion, because what we're going through right now--and Mike Bowers has pointed it out--is really not different from what we experience during the international biological program, when you get a group of people from different specialties working together on a very large project, and they don't always talk to each other.

Just to perhaps extend what John Cantlon was saying before about having our data sets talk to each other, I think the data sets will talk to each other when the people talk to each other.

Part of getting people to talk to each other is sometimes as simple as making them aware of what other people are doing. I think this is where this particular Board is serving an important function in getting people together, and why do I say that? You are probably, in my experience on this effort, the only group that sits down and listens to what everybody is doing.

When the geologists, when the hydrologists, when the thermal modelers meet, they meet separately. We don't meet together, so it's awfully difficult to find out that if Joe has a need in terms of vegetation information, I mean, we're over in the Valley Bank Building thinking thoughts about desert tortoises and other things. We don't hear about his needs, and vice versa, he doesn't hear about our needs necessarily if he isn't called in to be an expert before the Panel.

So, it serves a very helpful function, and I think that if there were and are more needs to get the scientists to work together, I will emphasize one other thing that we will have to get, and that is what you're focusing on right now, is the science can be wonderful, but unless it is directed, ultimately, to help make the decision as to whether it's suitable, number one; and, number two, it has to be suitable information to help with the NEPA compliance process. That means that Tom and Joe also have to know what's involved in the NEPA process, because, ultimately, Wendy is going to be the focus for all of this information and putting together this monumental document, and it's extremely important.

And I think that what Ron said just a little while ago, you have started to focus the efforts of people, not just scientifically, but also focus them on the next transition, which is into the EIS, so I think, in a way, you all have an extremely important function for us, because you have helped to, in essence, take the scales off of some of our eyes. I mean, we find out what other people are doing. It's because we were brought together, and perhaps that's one of the things that we could all use, would be more workshops together with people from different disciplines, to learn a little more, as you all have done in your Technical Review Board meetings.

DR. BREWER: Thank you, Tom.

Wendy, would you like to respond?

MS. DIXON: I don't disagree with anything that Tom just said, but I still think it's really important that we put a lot of this burden on those people that take the lead in study designs, and I say that's necessary, you know, to make a call on whether or not there's someone out there collecting that data, or the data is already available, because one of the things that we are always going to have as a problem is the time to do--how many workshops are a reasonable number of workshops, and we're already under a lot of pressure and a lot of heat for having too many already, and not getting real work done.

There's a way to take care of both, and that is, there is a reasonable level of these interfaces, but there's still a burden that needs to be placed on participants, all of our participants to make sure they check on the availability of data needs prior to initiating an investigation, and I think we need to keep that burden there, and the managers will help facilitate that interface.

DR. BREWER: That's good.

Warner, you look like you want to say something.

DR. NORTH: Warner North. Well, I'll throw this out for discussion, following your comment.

I think one of the problems that the Board has observed is that those study plans, once they get on the list, change very slowly. There are some things we would like to see in study plans that we've been waiting for for quite awhile, and some things that are in the study plans that are probably inappropriate. So, we're a bit concerned about the way that whole process gets managed.

Now, with respect to the EIS, I think we've heard the phrase, "scoping," and we're beginning to understand a little bit what that means in terms of process. There's also a content dimension, too.

One of the items that would clearly seem to need to be on the list is what happens to the vegetation at Yucca Mountain and, therefore, what happens to the ecological system as a result of the repository? On what level do you want to try to answer that question?

The proposal was made by Charles Malone, maybe we ought to look at a worst case of no vegetation at all, and we heard from Marty Rose, that talked about the paleoclimate issues, a subject that this Panel has heard a good deal about in some previous meetings. It would appear that one could look back into that record and ask, "What happened when it was hotter or drier, or when it was cooler or wetter?", and looking at the pack rat middens, the pollen information, the ostracods from the old lakes and the like, one can try to address that question.

But, to me, the problem is, who puts it all together, and who adapts the study plans to make sure that the information that might be needed five or ten or fifteen years from now, when the EIS process finally gets going, who's going to take responsibility to see that that task gets done? And, right now, I guess I'm not clear at this point if anybody's got the assignment of thinking through, what are we going to need for the EIS, and assuring that on one of the time frames, of which we heard three from Russ Dyer, this work is getting done, and getting done in a timely and suitably comprehensive fashion.

MS. DIXON: I guess I'd like to take a wave at that series of questions, and if I miss any one along the way, please so advise.

I guess Point No. 1, what you all, for the most part, have been involved with up to this point in time is primarily, with a few exceptions, if I might say so, the site characterization, geotechnical, geohydro-side of the house, and what they deal with is their study plans, as mentioned in the SCP, as coordinated with and concurred on by the NRC that define what their work scopes are.

When you talk about the EIS that we're moving into, we're no longer talking about their study plans for the NRC. Those are not our issues of concern, and like we expect them to let us know what input that they need to have from our side of the house to do their analyses, we're going to have some burden to provide them with a suite of information as to what we're going to need for the Environmental Impact Statement.

Some of that will be developed, obviously, by our own expertise and understanding. Some of it will come from issues that come out of the scoping process. We have not entered the EIS process yet, but we'll be sitting down with our cooperating agencies and doing some brainstorming and discussions, and so forth, on data needs and who's going to do what and how it's going to be put together. There'll be a team assigned. There will be a very formalized process that kicks this whole thing off.

But you have to understand that it will be separated from the study plan analyses that are being done for NRC license application-types of issues.

MS. FOSTER: And I might add that that is the reason why the environmental arm of the Office of General Counsel at DOE is not involved at this stage, because you have not actually started your NEPA process. Once you do get into the EIS, that's when our arm comes in to help, and I might say, that decision of the Secretary, he or she, will be whether or not to recommend to the President whether or not you will have this repository.

So, that is another reason why the no action alternative needs to be discussed, because it is not a given, and as long as we keep referring to this as a proposed repository, there is a good reason for that every time you say that. It's a proposed repository.

MS. BLAUG: Getting back to the--I've heard the "So what?" theory pop up time and again, and it's during the scoping process that, based on the studies that have been initiated so far, if you come to certain conclusions that some information is just--even if the worst scenario will result in really de minimis impact, or a "So what?" impact, bring it up in the scoping meetings and express your views, and if everybody else agrees that it's a "So what?" proposition, then it doesn't go into the EIS.

DR. BREWER: Thank you.

Dan Metlay, did you want to follow up on this?

DR. METLAY: Yeah. I just have a quick question for clarification.

Russ Dyer, in his presentation, suggested that the licensing strategy is--either has been changed, or is in the process of changing to the more phased licensing approach. With that, is there any change in the strategy that the Department's using for EIS preparations? Will there be sequences of EISs? Will there be some data that, under the old strategy, would have been included at an early stage, that you now intend to postpone to a later stage?

DR. BREWER: That's a good question. Who'd like to respond? Wendy?

MS. DIXON: I'd be glad to.

The environmental review aspects will go arm-in-arm with the overall programmatic strategy, but I guess I would like to clarify that, really, what Russ was presenting, when you go back to the Nuclear Waste Policy Act and its implementing provisions, it is not anything any different than what the regs themselves specify, because the regs do specify that there are a suite of decisions that need to be made, not just the one, and with each suite of decisions, there'll be more data, both technically for site characterization types of issues as well as for environment, and it does state up front that with each suite of decisions, there'll be basically a supplement to the original environmental impact statement, so you will have a number--several NEPA documents that will be created over a very long period of time, using the additional information that's generated.

Now, obviously, NEPA goes, to a large extent, hand-in-hand with the data that's being gathered, because if the data doesn't exist and it's not to be collected for 50 years from now, it obviously will not go into this EIS, and it's not dealing with the decision at hand, so that the NEPA documents will be tied to the decisions at hand, and the availability of data will be tied to that as well for both the programs.

DR. METLAY: Could I just follow up? Maybe I can rephrase the question.

Will there be information which, under last year's approach to pursuing a license application, would have been provided in the initial EIS, but which now will be provided in supplemental EISs?

MS. DIXON: I guess, to some extent, I don't feel real comfortable with that question because the full data set of what would go in either that EIS or this EIS most certainly has not been developed, because we are not there yet, so, you know, in concept, perhaps my gut reaction would be yes, but with respect to something defined and definitized, I don't have an answer.

DR. BREWER: Okay. Thank you very much.

Warner, and then Joe.

DR. NORTH: Warner North.

Following up again on this, I'm trying to understand a little better the time lines involved, and the difference in the proposals that Russ Dyer described to us in terms of the comparative schedules.

The administration funding proposal, which I gather is the plan that program management would like to implement, assuming they get Congress to pick up the tab, talks about a notice of intent in mid-'95--that's about a year from now--to be followed by a draft Environmental Impact Statement in mid-'98, which is four years from now.

Given the way the process works, the discussion with the Office of General Counsel, the need for extensive public meetings which may be at a local, a state, and a national level, and the extensive tie-ins with other work in the program, particularly the findings of site suitability and the license application, and given the need for somebody to carry out the public meeting process and the writing of documents, which is quite time-consuming and, therefore, expensive, now, is there a plan in place for how this is going to get implemented on time, on budget, or are we going to find, "Oh, surprise, surprise, we've got to do another EIS, and this means another X million bucks and an increment of time that we hadn't counted on."?

MS. DIXON: To the extent that we have our plans together at this point in time, we've started to roll in the budget demands of the NEPA process. Now, most certainly, I will not second-guess that our proposed figures are 100 per cent accurate, because until you go through scoping, you don't know how many comments you're going to get from the general public, what those comments are going to be, how long it's going to take to address them, and so forth.

The reason why there's a long schedule in there for this particular EIS--and if you looked at what DOE's trying to do with turnarounds for EISs, and that's two years--our schedule is more than twice that. The reason for that extra-long duration is because we realize that scoping--we're going to give more time to scoping so that we can make sure we get the public's input on this EIS than what's normally allowed, and because of the complexity of this EIS, we're providing considerable more time for comment on the document than what is normally provided, because we realize there'll be a lot of interest.

We've gone through this before with EA and the SCP. We know what kind of magnitude of comments we get, and we feel that we would be foolish to try to plug ourselves into a real optimistic, you know, that the average time is, or what DOE would like to see is, because we don't think it would be successful, and we'd be criticized for being unrealistic, so what we've tried to do with the time frame that we've provided is to be realistic.

Now, again, we haven't gone through scoping, and we might have to adjust it, but I think we have enough conservatism in there right now to satisfy, you know, the answer to your question.

MS. BLAUG: Could I add one thing?

This particular action is rather unique in that-- and I'm not commenting on the time line at all, but just to make the point that it's very unusual to have an action, to propose an action that truly is going to probably interest every person in this country. Every person in this country is a potentially interested person in this action, because not only the actual construction of the repository, but of the transport from the various states to the repository, and, because of that, I would think that DOE would want to be extra, extra careful in ensuring that everybody who has some interest in this and wants to provide some input is heard.

MS. FOSTER: I would state that not only is that DOE's intention, but that is what DOE will accomplish. We intend to make sure that everyone has the opportunity to be heard.

DR. BREWER: Thank you very much.

We have a bit more time, and there are a couple of scientific issues that came up in the presentations that we should probably return to and end the discussion with.

One of those had to do with John Harte's presentation on whether or not there's some thermal or heating studies that could be done, and I turn to Dan Fehringer, who reminded me of the fact that we had this presentation this morning.

Have you got additional things to say about it by way of framing a question or kicking off the discussions?

DR. FEHRINGER: My comments are very simple. I just wanted to solicit the views of others on his suggestion. He's suggested studies very similar to the ones he's carrying out, but conducted possibly from a tunnel underground, and do people think that would be feasible? Would it be useful, and would it be worth the cost that it would take to carry it out?

DR. BREWER: We'll probably start with Tom, since you're the man with heat underground.

DR. BUSCHECK: I had different feelings this morning. Initially, I liked the idea, and then in discussing with people, I thought, we're going to be shocking the system. It could be taking a thousand or even two thousand years to get to the state that we would try to get the system to do in perhaps one year's time, and, therefore, ecologically, you might be inducing things that are totally irrelevant.

However, in thinking about it, I was thinking about my models and how the coupling between the shallow system and the atmosphere is something that I've sort of put on the back burner, and, right now, I'm building up a lot of saturation in the upper part of Yucca Mountain, and I think I'm potentially grossly under-representing that mass transfer to the atmosphere, and so I think a shallow heater test would be very useful for establishing that mass transfer process.

And so, I think there'd be, independent of the ecological effects, I think that there would be a lot more validity in the types of modeling that I'm doing, and, at the same time, I think that one should view this for other disciplines as a process-oriented, rather than an ideal analog. It probably would be a very non-ideal analog, because we're going to increase the temperature in a step-wise fashion relative to the actual system as it works out.

DR. BREWER: Any follow up? Jim?

DR. EHLERINGER: Could I just start off with an overall comment?

And that is, that based on the comments and presentations we got last time, the kinds of field work we saw underway yesterday, and some of the discussions we had this morning.

My impression is that a good share of the data which is being collected by the terrestrial ecology group is a static, non-process-oriented data set that I believe has very limited application for the EIS, and, in particular, things that are process-oriented, such as the thermal studies, may have more direct bearing than monitoring where a desert tortoise is going in a non-critical habitat, than the ability of an area to be re-vegetated, or the ability to germinate seeds.

I think that whether we adopt or suggest the adoption of thermal studies, things that are more process-oriented, which are going to link to the hydrological and geological, are going to be critical if this is going to be a supportable EIS.

DR. BREWER: Anyone care to respond to that? Ron Green, do you have an interest?

MR. GREEN: Ron Green, EG&G.

Well, you have to go back and look at the purpose of why we designed the studies the way we designed, and that was for the particular purpose of looking at, basically, construction activities. That data set gives us a good description of the Yucca Mountain ecosystem.

The last component of the program that I put on the board this morning was to look at issues and objectives for the long-term studies, and, really, that's where we're at, and that's why we're entertaining these questions, is that these are new issues and concerns that have come up, and your comment is duly noted.

DR. BREWER: Okay, very good.

John Cantlon, do you want to follow up?

DR. CANTLON: Yeah, I would just comment that the take-home value of the descriptive studies is going to be in the utility of mapping in detail processes of the hydrologic area, so I don't think they're of minimal value at all. I think you probably reached a point where the value of continuing them forever is probably questionable, but bringing it out as a good data set for mapping is an excellent idea.

I'd really like to pose another question to Tom, and we haven't--I alluded to it a minute ago, but are you comfortable with the level of interaction between the geochemistry and your hydrology, particularly now as you've looked at the sort of vapor transfer system in more detail?

Are you getting the kind of interplay with the geochemistry that is going to be needed to characterize that system?

DR. BUSCHECK: I think for the types of things we're talking about right now, I think there's more variability, just with regards to the system as it exists, than the system as it may be perturbed by virtue of geochemical interactions, and Bill Glassley would shoot me if he were here, but that's my first remark.

But, I think that--I guess the geochemical effects, I think, are more important in that boiling condensation zone. That's just my point of view as far as altering the intrinsic properties of the system. I haven't thought a lot about this shallow surface effect, and I may have a different answer in a couple weeks, and I think that there is a potential for more interaction with geochemistry in the shallow subsurface, so I don't want to step on their toes.

So, what I said is just sort of a gut reaction, initially, that, again, I don't think that when we're below the boiling point we're going to be moving nearly as much fluid around and be causing geochemical changes, but something could crop up in the next couple weeks that may jog me, to cause me to think that there is a need for a lot more interaction, so I guess I'd leave it at that.

DR. CANTLON: Just--and this is really less related to this Panel's operation than some of our other panels--there's been a very important question on $C^{14}O_2$ loss from the site, and the whole business of whether, by solubilizing some of that material, you can get a great deal more transfer and uptake of the $C^{14}O_2$ in the system and, therefore, you degrade the risk element of $C^{14}O_2$ release, and it would seem to me this is a good geochemistry kind of--

DR. BUSCHECK: It didn't take two weeks, actually. If we have all this vapor flow coming up to the ground surface, we could be inducing a caliche formation that would be far greater than anything you would normally anticipate, and so, yes, that could have a very important effect on--

DR. CANTLON: I just think there's an element there we need to pursue, and it's a little different from this, but it does have a biological ramification.

DR. BUSCHECK: I think it points out--I think that we could end up, if we don't field shallow heater tests, we'll spend more money debating what may have happened, and that's one of the problems I think we have on these thermal issues.

There's so much debate going on that's unconstrained by physical measurements, that I find it really frustrating, and I think we'll end up creating as much thermal energy just arguing about it.

DR. BREWER: Thank you.

Joe?

MR. HEVESI: I'd like to respond to the general issue of integration amongst the various groups, and I think from many of the researchers' point of view, the one plan of doing this has always been in terms of letting the site scale modelers, such as Tom and the LBL group--I'd like to cite Bo's model again, and I know there is some more thermal modeling going on. They tell us what to do, because we're establishing their surface boundary condition, and we do have meetings every six months. There's an unsaturated zone modeling meeting, and various groups are pulling together on this, and Rick Spengler, and I see Gene Martin in the audience. We all get together and try to do the integration, and that's one of the reasons you saw some of the interesting mesh geometries. That's done specifically in terms of where boreholes are located, what Rick Spengler and his group are telling the site scale modelers, and then there's geostatistical modeling involved, also.

And, what I'm seeing now is a little bit of a full circle in terms of we're providing them with the data, and, now, how do the models affect what's going on on the surface, and that is a little bit new, in some respects, but we have been doing some integration work.

DR. BREWER: Tom Buscheck, do you want to follow up?

DR. BUSCHECK: Well, I'm not actually following up. I just want to clarify something again. What I said about the geochemistry, I don't see a lot of potential for dissolution, because I don't think the flow rates of the condensate are going to be that great, but I want to emphasize, I think that the precipitation due to evaporation of calcite or whatever is really an important point, so I want to back off the statements I said earlier about the geochemistry is virtually a strike zone, but...

DR. BREWER: Anyone else want to pursue this general topic? Yes, Charlie Malone.

DR. MALONE: I'm Charlie Malone with the State of Nevada.

I'd like to ask Tom if he's given any thought yet to what the movement of water and the vapor phase and things of that nature from a hot repository might have on the local climate?

DR. BUSCHECK: How small is a microclimate? I mean, I don't think--I have a hard time believing that it would affect the climate that much, unless--well, I mean, to what extent, going to having a stand of trees there over a square mile or so, does that change the climate?

DR. CANTLON: It's like one reactor.

DR. BUSCHECK: I don't think it would change the climate enough where it would critically couple into what we assume for the--

DR. MALONE: Well, I was thinking about fog in the wintertime, and more precipitation from that fog, and then much drier in the summer with the added heat from the top down, in addition to the bottom up.

DR. BUSCHECK: I think it's debatable whether it's going to be a lot drier. Right now, I think it might tend to be wetter.

DR. MALONE: From climatic change, or the repository?

DR. BUSCHECK: No, due to the repository itself. I think, perhaps, the worst scenario would be if we had minimal gas-phased diffusion, minimal convection of water vapor to the ground surface, and just conduction. If we only heat the surface by conduction, and no additional water vapor, that's potentially a worst condition, but, from what I've seen--and I actually also think that it's possible that you may have worse conditions at an intermediate thermal load, whereby you will not drive water vapor to the ground surface, but only get a rise in temperature.

There's a potential there, and I think we want to keep an open mind about that, because I think that, actually, there's a huge difference in how much water vapor we could potentially drive up to the ground surface, and it almost goes as the areal mass loading squared, or perhaps even stronger than that, so if the temperature rise is more than offset by the increase in liquid brought up by the repository, the impacts may be more benign, or not benign, but less deleterious for a higher thermal load than some intermediate thermal load.

DR. MALONE: Thank you.

DR. BREWER: Thank you.

Unless there is some burning issue--oh, a burning issue. Ron Green.

MR. GREEN: I'd like to address a question to Jim Ehleringer, just to get a comment or a feel from you.

What do you think the utility value, or even the limitations of doing a heating study would be?

DR. EHLERINGER: Of doing what?

MR. GREEN: Doing a heater study, looking at--

DR. EHLERINGER: I guess I would toss it back and ask how could you avoid not doing a thermal heating study? I would imagine by the time that you get public input, if you don't have data to suggest what might be happening to the surface, you're going to find yourself in an indefensible position. That's a personal view.

DR. BREWER: Okay. Warner?

DR. NORTH: I had another issue I thought I would throw out for further discussion, and that is, are we spending too much money on the desert tortoise at this time? It strikes me that the information that would be most valuable to have is whether we're having any adverse effect on the population, and yet, from what I gather, it's extremely difficult to get the input on a long-lived specie that migrates a lot so that you would be able to get a very good handle on that question, especially looking at the Yucca Mountain area, as opposed to looking at this much larger area of Nevada where these animals live.

I would wonder, could we get something that would be acceptable for compliance with the various provisions of the law, and what would be needed for the EIS process by having radio transmitters on a much smaller number of tortoises, and relying on the fence, the 35-mile-an-hour speed limit and other prudent measures to avoid unnecessary kills of tortoises.

DR. BREWER: Would Tom or Kent like to respond to that?

MR. O'FARRELL: No, we didn't. The studies of the desert tortoise are designed to obtain the information in a short-term basis, and, to me, short term is five years or less to gather information that will be useful in the compliance process for the Endangered Species Act.

We have absolutely no goal of studying the desert tortoise for 65 or 100 years to gather information to fit into, perhaps, a life table or something like that. We are, obviously, interested, and feel that it's one of the most prudent courses of action to gather information such as, what are the sources and rates of mortality for the desert tortoise? That's not going to take a great deal of time, and it applies directly to the compliance, because one of the most significant questions and the things that people are most concerned about with the tortoise is, are you going to be killing tortoises?

I guess you'd have to have a large number of tortoises telemetered, certainly large compared with other studies. You are able to answer that question in a shorter period of time. I would suggest that if we had five or ten tortoises telemetered, with the amount of time that it would take us to obtain information and sources of rates of mortality, it would take us a longer period of time.

The studies on reproduction, which are also part of a population model, will also be gathered in a relatively short period of time, given that we continue to have and experience years of different precipitation; i.e., different food supplies. That information, again, will be obtained in a relatively short period of time; the food habit studies, which come up periodically as well. Food habit studies, I would suspect, will take no longer to finish up than the studies on the reproduction and mortality.

You have to finally decide what you consider to be most prudent, and let me emphasize something. The studies of reproduction, mortality, food, are not required in the biological opinion. The tasks of obtaining the information on desert tortoises were proposed to the DOE as prudent measures so that you could, in fact, when you came into the

EIS process, have the best available information to make some decision as to how your activities--particularly, transport of materials along the roads--how that would influence the desert tortoise.

DR. EHLERINGER: A quick question. Does the February ruling about critical habitat have any bearing on your future efforts in this area?

MR. O'FARRELL: The designation of critical habitat doesn't have any bearing on what the future of our studies have. You, I think, have a different idea--and maybe it would be helpful--of what critical habitat means. That doesn't mean that the habitat on the test site is less valuable than what was designated as critical habitat.

DR. BREWER: John Cantlon had a question about food supply

DR. CANTLON: What raised the question, taking your worst case scenario, and denude the whole mountain, what impact is that going to have of any real significance to the turtle population? Not much, probably; is that right?

MR. O'FARRELL: Well, I guess it would depend if you were the tortoise that was...

(Laughter.)

MR. O'FARRELL: And I say that because that's one of the things that people tend to forget. The Endangered Species Act is directed towards the conservation of the species, not of the individuals.

DR. CANTLON: But if we were to put a gambling hall on that footprint, we'd be far more impacted on the turtle population than what you're proposing, even in your worst case scenario.

MR. O'FARRELL: Yes.

DR. BREWER: Well, good, thank you.

I think, in the interest of time, we should probably at least offer the invitation to Kent Ostler, if you would like to sort of sum up your views, feelings, whatever on this, and a non-decision, by the way, on the whole day, on the whole sort of panel, because you have been sitting there quietly watching this all go by.

One last chance, sir.

MR. OSTLER: I'm just a quiet person. I don't like to say much, but I really want to thank the Board, in particular, for bringing up these kind of issues and, you know, we have stressed something that Ron has said several times today, is our past studies have really looked at the effects of site characterization, and I think we're at the point right now where we can begin to phase back those efforts and look forward to the EIS process.

Those initial studies were not directed at the EIS process, and we realize that to model what the impacts are going to be in the future, that we will need to look at functional relationships that you guys have discussed so much, and we appreciate those comments and kind of directing where we think we should be going in this next effort, which is to look at what data would be required for the EIS.

DR. BREWER: Well, thanks are in order, again, to Wendy Dixon and all of her people for really preparing and allowing us to have a splendid two days. I think we've covered a lot of territory. I think the conversation, in terms of communication back and forth between this part of the Board, the Panel on Environment & Public Health probably is better than it was in November. It's good. There is some sense that there is constructive communication going back and forth, I think.

And, I wanted to express thanks on behalf of the entire Board to you and to everyone here at the Yucca Mountain Project. Thanks, also, to the public who came and sat here through the whole proceedings.

The Chairman of Chairmen says if anyone would like to say something, now is the time. Thank you very much.

I declare the meeting adjourned, and would the Board please stay assembled here. Everyone else is free to leave.

Thank you.

(Whereupon, at 4:15 p.m., the meeting was adjourned.)