UNITED STATES NUCLEAR WASTE TECHNICAL REVIEW BOARD

1995 SPRING BOARD MEETING

THE EMERGING WASTE ISOLATION STRATEGY
THERMAL MANAGEMENT STRATEGY
ENGINEERED BARRIER SYSTEM DESIGN & RESEARCH

Las Vegas, Nevada
April 19, 1995

BOARD MEMBERS PRESENT

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

CONSULTANTS

Dr. Ellis D. Verink
Dr. Patrick A. Domenico
Dr. Dennis L. Price
Dr. Daniel Bullen
Mr. Richard Grundy

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Leon Reiter
Dr. Victor Palciauskas
Mr. Russell McFarland
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Sherwood Chu

NWTRB STAFF

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Dennis Condie, Deputy Executive Director
Ms. Nancy Derr, Director, Publications
Ms. Paula Alford, Director, External Affairs
Mr. Frank Randall, Assistant, External Affairs
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

PAGE NO.

INDEX - continued PAGE NO.

P R O C E E D I N G S

DR. CANTLON: This is the Spring Meeting of the Nuclear Waste Technical Review Board. My name is John Cantlon, and it's always a pleasure for me to get back to my home state to see how things are going here. As most of you know, we hold four meetings of the full Board each year, one in each of the seasons. We try to arrange to have at least two of our Board meetings in the State of Nevada, and this gives us an opportunity to find out how things are going here, since much of what's happening in high-level waste management really effects this locale. We last met last fall in Las Vegas. Last winter we met in Beattie, and here we are back in Las Vegas, so we've done three in a row here for the state. If there are any attendees from Beattie in the audience this morning, let me again thank them for the genuine friendliness and hospitality that we enjoyed while we were there.

The Nuclear Waste Technical Review Board was created by Congress in the 1987 amendments of the Nuclear Waste Policy Act, and the Board is charged to assess the technical and scientific validity of DOE's efforts in designing and managing the nation's spent fuel and high-level radioactive waste management system, including site characterization at Yucca Mountain as well as waste packaging and transportation. It is the Board's belief that activities such as these kinds of exchanges provide an open dialogue on technical and scientific dimensions of this that is good for the program.

Now for introductions. As I indicated, my name is John Cantlon. My field is environmental biology. I'm former vice president of research and graduate studies at Michigan State. I have served as chairman of the Board since April of '92. Clarence Allen here is a geologist and professor emeritus in geology and geophysics at Cal-Tech. Garry Brewer will join us later. He's a political scientist and professor of resource policy and management and dean of the School of Natural Resources and Environment at the University of Michigan. Ed Cording is a geotechnical engineer and is professor of civil engineering at the University of Illinois. Don Langmuir is a geochemist and a professor emeritus at the Colorado School of Mines. John McKetta is a chemical engineer and professor emeritus at the University of Texas.

Our Board is authorized to have eleven members. We currently have five vacancies, and these have been vacant for the past year. During this time, we've been fortunate to have several of the key disciplinary areas covered by former Board members serving as consultants. Pat Domenico is a geohydrologist and professor of geology at Texas A&M. Dennis Price is an industrial engineer and systems engineer and professor of industrial and systems engineering at Virginia Polytechnic Institute. Ellis Verink is a metallurgical engineer and professor emeritus at the University of Florida.

I'd like to introduce two new consultants. We're pleased to have secured the services of Daniel Bullen to help us with this meeting. Dan is associate professor of nuclear engineering at Iowa State, and he also has special expertise in material science. He will join us at the front table when we're discussing the Engineered Barrier System and will participate in the round-table discussions on both days. Also we have retained Richard Grundy, here at the side table, to assist on a part-time basis in the area of Congressional relations, very key at this particular juncture. He is an engineer by training. He has recently retired after 28 years of service as a Senate staffer, the last 18 of which were as a senior staff member of the Senate Energy Committee.

Now you will note at the top of our agenda, if you have it open, you'll see that we have three topics for the next two days. These topics are highly interrelated and build on themes from prior meetings. These topics are the DOE's emerging waste isolation strategy; second, the question of a thermal management strategy for our repository at Yucca Mountain; and third, some looks at the Engineered Barrier System.

We strongly believe that articulation of a coherent waste isolation strategy based on defense-in-depth is an absolute necessity for guiding the program for prioritizing the DOE's scientific efforts and for explaining the program to the scientific and technical communities and to the general public. We appreciate that this is a difficult task, and will take additional time to evolve and to mature fully.

We had a prior response from DOE about the strategy at our Fall Meeting, and at our Winter Meeting at Beattie, DOE presented a further step in the evolution of its strategy. Today we hope to hear more on the strategy. As you may know, we are particularly interested in the role of the Calico Hills formation that lies below the repository horizon and what it will be expected to play in the DOE waste isolation strategy.

Tomorrow we will be spending most of the day discussing engineered barriers. The EBS is on the agenda principally because the current version of the waste isolation strategy appears to us to elevate the role of the EBS considerably over the initial SCP expectation. The Board has long supported the use of a long-lived robust waste package in engineered barriers, but in combination with well-characterized natural geologic barriers. Our recommendation has been that this defense-in-depth would increase our confidence in the long-term performance of a geologic repository.

However, the apparent new emphasis on the EBS, with what appears to be somewhat less emphasis on the geologic isolation, raises several important questions that need to be answered. First, can this revised strategy provide adequate protection? Second, if the strategy does diminish reliance on geologic isolation, does this diminish important redundancy? And lastly, will there be ramifications from waiting until after the 1998 Site Suitability Determination and submission of a license application in 2001 before adequately demonstrating acceptable levels of natural barrier geologic isolation?

We continue to be intensely interested in the DOE's choice of a thermal management strategy, as we have been for several years. At our Winter Meeting in Beattie and at a November meeting put together by two of the Board's panels, we understood that the DOE intended to start with a low thermal loading with the hope or expectation of being able to amend the license application to a high thermal load before the repository opening date. This amendment would be based on results of thermal testing, including in situ testing at the repository level. The Board has had many questions and made comments about this strategy and about thermal management in general. Our comments were expressed orally at the meetings and were also in letters to Dr. Dreyfus in December on the 6th and on March the 3rd, as well as in our recently released eleventh report.

We have a session on thermal management scheduled for later today. Ed Cording will be chairing that session. Tomorrow our sessions on the Engineered Barrier System will include a discussion of criticality. I guarantee you that our initial draft agendas had criticality on the agenda before the Los Alamos scientist hypotheses appeared in the Sunday Times. In any case, we have since revised our agenda to allow time for the DOE to comment on their plans to address the hypotheses as they pertain to disposal of civilian spent nuclear fuel.

At the end of March, our eleventh report was released. It is a report for the entire calendar year and is the rather thick purple report on the back table. Please help yourself if you'd like a copy. If we run out of copies, please leave your name and address and as long as our supply lasts we'll try to provide them.

Now concerning today's agenda, we're trying a different twist on format. We have bullets or questions listed after most of the presentations. These are subtopics for which we are particularly interested in hearing comments by DOE. One point of procedure, we have asked each speaker to leave adequate time for questions.

After each talk, I or whoever happens to be chairing at the time will ask for questions and comments first from the Board members, then from our staff, and if time permits, we'll ask for brief questions from the floor.

I do want to point out, however, that as with all of our meetings, we have set time aside on the agenda at the end of each day for public questions and comments.

In order to encourage others to participate, we need to limit the time allowed to each individual in the public comments session, so please try to keep your remarks to a five-minute max. If there are many more people, we may have to reduce that. Those wishing to make comments are urged to sign the public comment register in the back at the sign-up table with Mrs. Einersen or Mrs. Hiatt. When you come to make your comment, please go to one of the microphones in the aisle and identify yourself and your affiliation.

And now I think we're ready to begin, and I'm very pleased that Dr. Daniel Dreyfus, director of the Office of Civilian Radioactive Waste Management, was able to join us and bring us up to date on the state of the program. Dr. Dreyfus, we appreciate your taking the time.

DR. DREYFUS: Thank you, Mr. Chairman, and members of the Board, I appreciate having an opportunity to speak to you today about the status of the program. Lake Barrett, my deputy, gave you a progress report several months ago in Beattie, and you will of course be hearing from several members of my staff on the issues that you chose for today's agenda. I will confine my remarks to some policy issues that

I knew were of particular interest to you and that I think Chairman Cantlon has just confirmed.

Before I begin, I think somewhere in the audience is Wes Barnes, the project manager of Yucca Mountain. Is he here or is he gone? I don't see a volunteer. He was here earlier, and I think he will be here--there he is, there he is. So I'm not going to ask him to come up here, but those of you that get a chance during the course of this time, you will be seeing more of him, I'm sure, all of you, over the next few years, so it's an opportunity when you're out here.

The new Program Approach that we implemented in 1994 relied, I think, in significant measure on the advice that has been received from this Board over several years. And we appreciate your continued support as we go through the efforts to refine the program plan and to respond to new developments. Later today and tomorrow, that dialogue will continue in several major technical aspects of the approach.

Now, a comprehensive approach to an undertaking of this complexity and with this kind of a time frame is not easily defined, and it's even less easily communicated to the thousands of participants and interested parties that are involved in this program. And furthermore, we are engaged in a dynamic planning process. Our knowledge of the Yucca Mountain site and our parallel comprehension of the institutional setting that we have to work in, which evolves along with our technical knowledge of the site. I think both of these have progressed considerably over the past years, but we are still very far from the point where we can set forth a definitive concept for the repository and contend that it will adequately address all of the demands and expectations of society. We now aspire to make formative conclusions about the technical issues in 1998. The stage we are at today, the program remains exploratory in nature. And I stress that, exploratory in nature. It's not simply designing something that will be built by engineers and put in place. We do not have, as I have said before, QA'd, as built drawings of Yucca Mountain, and we never will have. We will be continually revising our working hypotheses as we gain new understanding of both the technical aspects of the problem and the requirements of the policy setting, which I would point out are developing a pace this year and might have more impact on the program than we know. So we will appreciate the Board's advice and guidance as this Program Approach evolves.

We've been criticized by a number of our observers, including this Board, regarding the past effectiveness of program management. The Program Approach includes a rigorous performance measurement system to track progress against cost and accomplishment targets. The response to the system and the progress that we have measured thus far attest, I think, to a cultural shift in the program regarding the importance of achieving targets. It is possible that the published milestones and costs may need to be adjusted. But these targets should not be modified for convenience. Serious efforts have to first be made to meet or exceed the targets. Only when the realities require or new information concludes should those targets be modified, and then the consequences must be addressed. The Board's insights and comments regarding our plans and targets are valuable inputs into deciding when and how adjustments will be made.

The Board has also noted that in the past the program suffered from a lack of sufficient resources to carry on its legislative mandate. For Fiscal Year '95, we received a substantial increase in funding despite the severe government-wide budget constraints that applied. The Administration's FY '96 budget request for the program is $630 million. That amount will support the program plan, but it will not be easy to get Congressional approval of the funding. Pressure to reduce discretionary spending is severe in the Fiscal Year '96--Congress in fact has been revoking appropriations for '95--and it will grow more severe with time. We have to pursue an appropriate accounting treatment of the Nuclear Waste Fund if we aspire to have the kind of funding profile that we need to carry this program forward.

To address that issue, the Administration has submitted a legislative proposal to Congress which authorizes mandatory funding for the civilian portion of the program. It includes provisions for continued Appropriation Committee oversight, and in addition to those mandatory amounts it anticipates discretionary appropriations for the defense portion of the contribution.

The '96 budget request for Yucca Mountain is $472.1 million. It represents and increase of 96.8 million over '96, and it represents nearly all of the program increase that we would get if the proposal is approved. The details and the implications of that program funding you will be discussing with the staff later today.

We're also involved this year in a broader policy debate regarding the future direction of the program. A number of bills have been introduced and a hearing was held with the Senate Energy Committee on March 2nd. Clearly the discussion is going to be focused on interim storage. And it's certainly timely for Congress to readdress this issue. Interim storage has become a major policy concern of most of our interested parties, and something is going to have to be done about the current impasse. As a practical and political matter, this program needs Congressional guidance and probably new authority to define its role in the near-term management of commercial spent fuel. What's important to those of us that are involved in the program is the guidance and authority sets forth a feasible approach and provides the tools. And by that I mean the authority, the funding, and the personnel to pursue it. As the near-term objectives for the program are redefined, I hope that the long-term strategy of geologic disposal will not be lost or subverted. Otherwise, interim storage is going to become the nation's nuclear waste management strategy by default. And in my mind, that would represent a major failure of public policy. Decision to abandon permanent disposal will be profoundly important sociologically both here and throughout the world. And I would not like to see that happen as a part of an expedient response to a near-term problem.

We recognize that many of the technical strategies that address crosscutting aspects of this program are not fully developed. And your recent letters highlighted two recurrent themes: the need for a better articulated waste isolation strategy and for a better definition of thermal loading. A third issue has become more important--or more prominent, that is, with the New York Times publicity, and that's criticality at a long-term repository. I have only some general policy level remarks to make about these items. They're all on the agenda for more in-depth discussion later.

First of all, we have to recognize that in most cases definitive positions on crosscutting strategies have not yet been established. In our publications and briefings, we are still presenting working hypotheses which are being refined or revised as greater understanding is gained. We expect that we will have to modify our current strategies as new data are obtained and analyzed.

The Board has recognized that a coherent waste isolation strategy is essential for a credible disposal program, and indeed for a credible scientific program. Your observations have been that the strategy is not readily discernible in our program, and those are valid. The strategy embodied in the '88 Site Characterization Plan was difficult to discern, and the developments since that time have altered our approach significantly. I have assured the Nuclear Regulatory Commission that our current strategy, as well as that that was contained in the Site Characterization Plan, does rely upon multiple engineered barriers to limit the release of radionuclides to a natural barrier. We expect low liquid saturation and low aqueous flux to provide the long-term isolation. Current iteration does reflect a greater understanding of probably near-term environment waste package, and it also reflects a development of the multi-purpose canister.

We are developing an explicit statement of the strategy at a sufficient level of detail so that an informed observer can understand the rationale for design decisions and for our characterization activities. We'll use the strategy to focus site characterization activities on the key uncertainties that we face. The strategy will use--and I have told the Commission this--a defense-in-depth philosophy that is consistent with the Nuclear Regulatory Commission's regulations. The capabilities of the natural system as well as an engineered system will be utilized. Our goal now is to develop a waste package that will provide containment of the radionuclides for well in excess of 1,000 years, with a high degree of confidence and with gradual release thereafter. The greater integrity intended for the engineered system, which I think is consistent with this Board's recommendations, has led to some concern that we are de-emphasizing the comprehension of natural barriers. And I suppose if you emphasize engineering barriers then by definition you have de-emphasized in a relative sense concern with the natural barriers. But we are not de-emphasizing the natural barriers. Engineering solutions are not likely to replace reliance on a natural setting for isolation over the very long term.

The waste disposal concept we're developing now calls for in-drift emplacement of large, multi-purpose canisters with multi-barrier waste packages. We have not at this time progressed to the point where we can decide on a design thermal load. As the Board recommends, we intend to carry a number of alternative concepts forward. Later today you will be discussing a strategy proposed by our M&O contractor that calls for the evaluation of technical site suitability based on an assumed thermal loading near the lower end of the range. That program is still a proposal and we are still developing a position on this strategy.

Ultimately, we must achieve thermal loading that is compatible with the broad objectives of this program. The intent is to determine the suitability of a site for a repository with a capacity near the statutory expectation of 70,000 MTU. This must also be accomplished within rational cost and schedule constraints. And I mean rational cost and schedule constraints both for the evaluation itself and for the ultimate construction and operation of a repository. A decision to make the national investment in a geologic repository will certainly depend upon a showing that will substantially address the national need. Should the results of site characterization and related analyses indicate that the repository setting is only suitable for a design with a relatively low thermal load, then other strategies, such as the characterization of more emplacement area or technical options to manage the heat, will have to be explored. It is clear to me that the decision to propose construction of a repository must include more than simply having an appropriate licensing approach for some technical concept.

It also will require balancing the contribution that this project can make toward the national waste management requirement against the costs of building and operating.

The Los Alamos criticality debate and the recent New York Times "Sunday Supplement" treatment of it have raised that issue to national visibility. Criticality control, of course, has always been a consideration in our program. It is required by the regulations for the entire waste management cycle, including very long-term isolation in the repository after total containment. Criticality in the repository was the subject of studies as early as '78 and '81 by the predecessors of this program. It is not a newly discovered concept.

Certainly a criticality issue must be resolved in the design of a repository. As the New York Times editor somewhat cynically observed, the threat of a nuclear explosion in a repository could undermine public confidence and kill the proposal even if it does not enjoy any widespread scientific support. This is not an issue to be trifled with.

We intend to closely follow the scientific debate. Discussions thus far are focused primarily upon the risk involved in geologic disposal of weapons-grade plutonium in a vitrified situation. We intend to take seriously, however, the possible risk of nuclear explosions in the Yucca Mountain setting. The topic will be included in our evaluation of long-term criticality control. We will conduct whatever technical work is needed in our program to resolve the issue for our program. If it turns out that there is a non-negligible risk, we will evaluate it and we will act accordingly to assure protection of public health and safety and the environment.

Now, I would like to conclude on a philosophical note or two. First of all, I do not intend to let this program be driven to premature conclusions concerning any of the major strategic issues, such as waste isolation strategy, thermal loading, and criticality. It is not our role to in the program to arrive at rigid concepts early and then to adopt an inflexible defensive posture to justify them against criticism from outside.

We in the program are charged with the first line responsibility of deciding if the Yucca Mountain site is suitable, and indeed if the general concept of geologic storage is still useful for the United States. We must maintain a skeptical and objective viewpoint about all of the issues until we have satisfied ourselves. Then, if we are satisfied, we have a responsibility to design and propose the best possible project that we can conceive of and to describe it as objectively and clearly as possible so that a final judgement can be made where it should be made in the political and regulatory arena and so that it will be made as an informed judgement.

For our part, we expect to continue to develop solutions for the remaining technical issues. We'll strive to come to closure on a realistic time schedule as I believe society demands of us and within resource limitations that society can tolerate. And we consider our obligation to make the first call on the feasibility of this venture to be a public trust.

The proper relationship, I believe, between this program and its advisors and its regulators and the general public ought not to be one of an adversarial position. It ought to be one of collaboration, especially on this first determination that we must make, the technical site suitability in '98. That collaboration should help to ensure that we have considered the facts objectively and that we have reached a sound conclusion and that we have used the creativity that's available. The relationship ought not to be one that is adversarial, in which we try to make it work and the oversight bodies try to prove us wrong. Public interest deserves the constructive input of all knowledgeable participants in an undertaking of this consequence. There will be few enough people who understand in depth what is going on. I think they should all adopt the attitude of contributing, to enlightening the societal decision that has to be made, and not in fact to supporting a particular point of view or discrediting another one.

Thank you again for the opportunity to meet with you today. I don't think I touched on all of the issues on the agenda. I will be happy to respond to questions as the chairman wishes.

DR. CANTLON: Thank you, Dr. Dreyfus.

Questions from the Board? Don, Don Langmuir?

DR. LANGMUIR: Dr. Dreyfus, I see the real problem for you here in that you're pointing out that you don't want to draw premature conclusions regarding waste isolation strategy or thermal loading, and yet you need to have some sense of where you're going with them and have made some kind of decisions about them in order to plan the next several years towards suitability decision in '98. This seems to me a kind of problematic circle you're in. Would you comment on that?

DR. DREYFUS: Well, yes, sir, there is, and it is a problematic circuit that we're in. But I don't think it's one that's difficult from an analytical point of view. From an analytical point of view, we simply take the knowledge that we now have, draw a hypothetical approach based on that knowledge, and then test it against further data, further analysis, further thought. That's the approach that I intend to take. The problem we have is that there is an expectation often on the part of the community, the observant community, that as soon as you have drawn this first hypothetical stance it is incumbent upon you to protect it against attack. So when you say, "This is what I would like to do in the thermal loading situation, this will be my primary approach," and I may have contingency approaches, instantly it becomes a matter of justifying that somehow. Not so much against an alternative, it seems, but--and I've watched this dynamic now for two years, and as soon as you lay something on the table, you begin to start to defend it, defend it, defend it. What I would like rather is a dialogue in which you discuss the shortcomings and not, this is a shortcoming, so my people get driven into somehow plastering over it, somehow defending it, as opposed to saying, "Yeah, you've got a better idea? What do we do instead?"

Now internally I'm not going to let that happen. I'm simply not going to let it happen. We will change anything in this program when we find out it no longer makes sense or the data does not support it or that we got smarter. And I commend to those who are involved with us that they are going to have to get used to seeing those changes happen--I think you've seen a few--and not be appalled by the notion that, gee, the last time we were here they were telling us this, and now they're telling us that. The answer's yes, we got smarter, we learned more. We tried to describe something, and lo and behold it could not be described when you really got to it. These things are happening. I'm watching them happen from the inside. And several point out that my management approach will be to abandon a hypothesis as soon as it quits being able to be verified, whether it's being attacked from outside or not. But I would prefer not to be defending it all the time while I'm examining it myself.

And I think that's a problem we have. We do have a program that rests in an adversarial circumstance. Ordinarily people are not in that stance until they arrive at a regulatory process, but we are, and we have to understand that the political antecedents of this program created a built-in set of opponents who are intimately involved and who do not want to see it succeed. And that tends to drive us into a mentality that I don't want my people in. They are supposed to decide whether they like a hypothesis. They are supposed to decide whether they ought to recommend this program even next year, let alone in '98 or 2001, to continue towards its goals, and they can't do that if they feel themselves in a defensive mode against attack.

So I simply share with you the fact that I am not going to let them do that. As long as I am the director, we are not going to go into a defensive mode, we are going to change things, we are going to find problems, and we are going to tell you what they are. This Board, I think, more than many of our collaborators, is constructively disposed to help us find solutions. I commend you for that. I think you will find if you review the bidding that over at least the last year we've adopted a whole lot of what you've told us to do, and we'll be doing more. We need your help to look at things like thermal loading and help us figure out what the appropriate strategy should be as we go forward.

DR. CANTLON: Okay, other questions? Yes?

DR. ALLEN: Dr. Dreyfus, from your perspective--

DR. CANTLON: This is Clarence Allen.

DR. ALLEN: Clarence Allen. Could you bring us up to date on the status of the TBM operations? We understand things are going somewhat slower than was initially expected. And do you envisage this is going to have a serious impact on your schedule and achievement of site suitability determination?

DR. DREYFUS: Well, there are a lot of critical path aspects to the tunnel in terms of scientific programs and in terms of the design, and we know that and it is therefore a part of the program that stays right on the front burner. I mean, I get a written report on tunnel operations every morning in Washington as soon as I arrive, which means that somebody starts very early here. You will get a report as soon as I quit here from my engineering manager, and I think he's better prepared to tell you the technical aspects of what's going on.

I visited the tunnel yesterday, as I think the chairman did, and what basically is the situation is that we have two things going on at once. We are shaking down a new tunnel machine, and I don't think anybody that's ever done that will not understand that, you know, a new tunnel machine is a little bit of a kit from which you build a tunnel machine, so we're doing modifications as we find out more about this setting. We have also had bad rock, that simple. We've had fault lines, successive fault lines, which have been less consolidated and bigger than we had been led to believe, so the going is slow. Now, we're not seriously behind schedule at the moment, but if we continue to have the kind of conditions that we have at the heading right now repetitively over a very long stretch, we are going to have to develop some new methods. We have had some outside consultants aboard, recently we have some modification concepts to the machine that might help. We're going to have to deal with the situation. This, as I say, is an exploratory program. We have learned a whole lot about that mountain in the first couple thousand feet of that tunnel.

I think that two things that we know about this now is that I would not want to do this job without a tunnel. I think the hands-on aspect of being in the mountain has suddenly taken on a new reality for people who are somewhat skeptical about when we needed to be underground to do this job already. But on the other hand, this is very early days. This is shallow tunneling, this is an entrance, this has nothing to do with the repository formation as yet, and we are to understand that we have an engineering problem to get that tunnel down there to where we need the information. We're working it, and I'll ask Rick Craun to give you some more specifics surely.

DR. CANTLON: Other questions from the Board? Staff? Bill?

MR. GRUNDY: I think Bill was trying to get my attention.

DR. CANTLON: Oh, okay.

MR. GRUNDY: Dan, I think that you were quoted in the paper several times following your hearing before the Senate Energy Committee about--

DR. CANTLON: This is Richard Grundy.

MR. GRUNDY: I'm Richard Grundy on the staff. I'm used to asking questions of Dan. You made some quotes on probability of getting some decisions here, and I'd sort of like to ask for your clarification on two of these quotes and what your philosophy was behind them. One of them was you stated that it was about an 80 percent chance that you will find Yucca Mountain to be suitable and about a 50 percent chance of getting a license. To what extent were your comments based upon technical considerations in making those observations?

DR. DREYFUS: Well, let me start from the observation that it's patently ridiculous to make those kinds of statements. Having said that, let me put you in the picture that I tried for about four or five minutes of eye contact not to make that statement before I made it. But there is a desire on the part of people in the political arena to have these kinds of definitive remarks, and since this program is located in this area where you can get odds on just about anything, I figured that I ought to be able to give them odds. And there's obviously a sort of comprehensive product of your subliminal mind more than anything else. Of course, the other observation I would make is that a strict mathematician would multiply those together and say this is worse than a coin toss. But we won't do that.

What I observed is, from where we stand right now, and knowing what I know about the engineering ability to do business and knowing what I know about society's tolerance for uncertainty in many, many important societal and health and safety conditions, I'm reasonably confident that we can design and construct a repository that provides pretty good assurance that nothing untoward will happen over the life of the containment of that waste, or over the life of the mountain, for that matter.

On the other hand, when I look at the institutional arrangements within which we work and within which we do business, and again the attitude, which I would refer back to, that somehow it is the business of everyone but the program to try to demonstrate that it has not measured up. And I'll use the New York Times as an example. An individual writes a paper which one cannot get a copy of because it's not clear which paper he likes. Three or four drafts floating around the National Laboratory, a dialogue going on between him and his colleagues in which neither of them have written down anything they want to release. And a New York Times reporter, ostensibly a science reporter at that, finds this piece of paper. Not only does the editor of New York Times give him front page Sunday prominence, but he writes this thing up and he does the gestures of saying, "Well, there's not anybody much that agrees with this, but--" And I canceled a trip to Nevada and I spent a week dealing with that thing. Just dealing with it. I mean, not doing anything but just taking phone calls and explaining why it was that the secretary and I had not fully read that paper and informed the chairman of the committee when we testified the previous Wednesday. The reason was because I didn't even know about it until the Times reporter called me on Friday, and that was too late to tell the secretary. She didn't know about it till she read it in the paper.

And how many papers would you suppose the National Laboratory system has got in preliminary draft form that somebody scribbled down that are not in the main line of a problematic deliverable, they're just what the guy does when he's amusing himself? I mean, this is not a DOE tract product that was destined to be a part of the program. I mean, they're out there. Any one of them can derail a program if in fact the media and the public take it to heart. You have that kind of a situation.

In that kind of a situation we have to make a major political decision, which means that a secretary and a president and a governor of Nevada and a Congress at future point in time have got to buy into this thing and take a political stance, and then we go through a regulatory process with a commission none of whom have we yet met. Well, that's the situation, and I say it's a coin toss. Now, I say we can get a lot more sure about it, and I would point out to you that we are doing that at the same time we're doing science. When we go out with an Environmental Impact Statement, we will either wind up in court or we won't, and we will either win the court case or we won't. And if we don't, we will be recycled back into several years of additional work.

And we are working with the Commission on what will or will not constitute an acceptable license application. And by 1998, in addition to having enough convergence on our technical work to be able to talk in terms of firm estimates --or not firm estimates but solid estimates and solid judgements, hopefully not my guess at 80 percent, but something that a broad specter of people are willing to buy into. In addition to that, we'll have a lot of this institutional information, we'll know something. Right now, that's where I'm at.

DR. CANTLON: Okay, thank you, Dan. I think we better move ahead here. Our next speaker is Steven Kraft of the Nuclear Energy Institute--

DR. BARNARD: Oh, John, John, John, we were going to get an update on the ESF, the tunneling.

DR. CANTLON: Oh, I thought we were going to do that with Steve Brocoum when he comes up.

(Whereupon, there was inaudible casual conversation.)

MR. CRAUN: Can you hear me? Oh, there we go. Now I can, I can hear myself, too. I'm Richard Craun. I'm the assistant manager of engineering and field operations. I am here to give you an update on the TBM operations.

The first couple of slides are just an information issue. I wanted to share with you some of the topics that we're working with. For us, tunnel access and the control of access is important to safety. We do have confided working areas, many simultaneous operations taking place. I want to jump down here. We are currently experiencing about 14 visitors a day. Some of our peak visitor loading is about 28 to 30 visitors a day.

Several of you have been involved in trying to get access to the tunnel and get continued access. We have four categories of personnel access--unrestricted, restricted, escort and visitor. The first two categories really apply to those people that work in the tunnel at all times. The latter two apply to people that are wanting to visit the tunnel.

Escorted access, we're wanting to provide access to the tunnel on an as-requested basis. We realize that there's a need for people to gain information to perform their functions, to visit. Personnel not assigned to the ESF, we want them to--for example, Affected Units of Government, DOE, M&O personnel, NRC, NWTRB, county personnel--we want to provide access to those people that need to get into the tunnel on an as-needed basis so that they can gain whatever information they need to perform their function. That is the underpinnings of our access policy. We need to let people know that access may be restricted at times. If we are, for example, shotcreting in the tunnel, then in fact that evolution may require us to limit that access. We will work with the people very carefully to insure that access is provided as quickly as possible. Visitor access, those people that really don't have a function as defined to perform out there, we are looking at a policy of taking the visitors to the first alcove, and then it would require project manager approval to go beyond that. And the purpose of that is to provide access on an as-needed basis in a safe manner.

Now, to continue on with the TBM brief, currently we are averaging about five meters a day. Our '95 baseline was nine meters a day, so we are below our baseline. Our best day was on February 27th, and that was nineteen meters is what we were able to tunnel that day. And we do expect mid-July--and we are on schedule on that--to install the conveyor system, which will allow us to improve our tunneling rates at that time.

Now, this chart may take a little bit of explaining. Let's see if you can read it. The green curve is the baseline for '95. The red curve is our actual performance. As you could tell probably in this area, we were very happy, we were ahead of schedule. We were actually ahead of schedule and below cost, so that would have made this briefing even easier. At this time, we entered the current fault that we're in still yet this morning, and we have spent the last three weeks working our way through that fault.

Currently this morning we are at Station 5 plus 57.

We made about 1.9 meters yesterday. We have been installing select fill behind the grippers on the right side and in the front face. The fault that we penetrated transitioned across the tunnel path at about a 60-degree angle, so it first entered the left side of the machine, the left face, so the left gripper first started losing action, and then has transferred across the top of the machine to the right front face. The grippers are still in the fault zone itself. There's about twenty feet distance between the front of the face of the machine and where the grippers are, so until we get the grippers back into more competent ground, the machine will be going a little bit slower than we would like it to go.

Talking to the tunneling people this morning, if we make progress today as we did yesterday, we should get the face of the machine into competent ground. More competent ground is a better way to state it. And then at that point we will be able to stop the process of overexcavation so that we will not be creating any additional voids above the machine, and that would allow us to start increasing the propel rate. And then as soon as the grippers get into the more competent ground, then we should be back to a tunneling rate that we were enjoying back at this time period.

Let me explain a little bit about this line here. This is a projection. This is the construction of the second alcove. We are late on that. Originally that was scheduled in this time period. What we have done is we've been able to look at that and been able to come up with methods of doing that concurrently. So we do now only anticipate from a TBM standpoint about a five-day outage. We are looking at also trying to start that evolution over the weekend so that we might even be able to reduce this down to a three-day outage of the TBM, which would allow us to get going again a little bit sooner.

Also, this platform here, we've been able to look at that. If one were to project this line up even higher, to the point where we will be tying in the conveyor system, we are now only projecting about a five- to seven-day outage of the machine. This currently is a six-week outage period. What we will do is install it in segments. We will bring the machine down for a brief period, make those installations, those tie-ins, bring the machine back up. So the effective result is that it appears to us to be very reasonable, and we've been able to lay out the job packages so that in fact we can make that conveyor tie in in more like a week, maybe ten days. So this will allow us to then hopefully in this time period, if we're able to reestablish our rate, projected rate, here, get the machine back in competent ground. If I'm successful today in getting the machine back in competent ground, this is what we consider a Level 2 Superstone. It's part of the program plan, and we are within three meters of meeting that, so we are very close to being on schedule.

Yes, sir, do you have a question? No? Okay.

So that's the explanation of--

DR. ALLEN: Well, yes, since you asked me.

MR. CRAUN: Okay. Well, I saw you lean back.

DR. ALLEN: The imbricate fault zone is shown as being quite wide on the maps that I'm familiar with, and therefore you say you're going to be out of it in a few days? I mean, I don't--

MR. CRAUN: Well, this specific fault, this specific fault, this specific fracture.

DR. ALLEN: Well, is there any reason to believe that there aren't a whole series of these?

MR. CRAUN: Well, the map that I looked at this morning showed approximately six between now and approximately Station 11. As I understand that, there may be six, there may be four, there may be eight, there may be ten, we don't know.

Let me go to the next slide and I'll help you with the answer to that. What we did do is as a result of taking three weeks to get through the current fault, we pulled together a team very quickly over the last three or four days. They've come up with a series of modifications, some a little more dramatic to the machine, some a little less dramatic to the machine. The top four I've personally checkmarked because those four have been installed on a similar machine up in Portland, and those modifications have greatly improved the machine's ability to both grip and to keep rubble from coming down in between the grippers on the top surface. We can go through these. Some of the bottom ones, I believe the Portland machine they actually reduced the rotation of the head of the machine. We want to look at that more before we go into that process or even implement that.

We are what I would consider in a very focused schedular manner. We are hoping to get these modifications through our process, through procurement, and possibly installed and operational on the machine within five to ten days. That's my goal. We may not make that. The DOE system in procurement, as you may be aware of, is lengthy. The team is working together to try to do that. So we are ambitious in trying to get these modifications to the machine. With those, we feel we will be able to improve our ability to grip.

Do you have a question, Ed? No? Okay.

In addition to that, because of what I would consider to be the successful results--in other words, the ideas that came out of that group of people that we pulled together very quickly--we've decided to pull together a Board of Consultants for tunneling and underground construction. The charter of that is being defined now, as we speak. We started that approximately three days ago. It's going to be chaired by Mr. Barnes, myself, Dale Foust and Bob Sandifer. And the participants for that panel are being defined now. We're looking around the United States for those people that can help us. Now, this panel may shift focus over time, so right now our focus is going to be primarily on the machine, to try to see what we can do, since it is a new design machine, and we are having some performance issues that we're wanting to improve in this more blocky ground. Our primary focus at this time will be on trying to improve the performance of that machine.

That was the presentation. If there are any questions, I'd be more than willing to try to answer any.

DR. CANTLON: Questions from the Board? Ed?

DR. CORDING: Rick, I'm pleased to see some of the things you're doing here and the modifications to the machine that you mention. I'm familiar with the Portland machine, and some of those were very helpful. And there's even more they would like to do up in Portland right now but haven't had access to the head to be able to, for example, extend the shield and get that forward to the front edge of the cutter head. But certainly some of those items are going to be helpful, and I'm sure that the people that you're working with have been able to bring in that experience. I'm very pleased to see that. Some of the grouting techniques they've been using in Portland are fairly interesting now, with a new fast-setting mix of cellular cement that they put up in the front, concrete, and that might also help in filling voids.

And I'm also again pleased to see the Board of Consultants. Certainly the issue you're dealing with is a very focused issue on the machine, and there may be people involved in that that you're bringing in a special group to handle that. The Board of Consultants may have somewhat different constitution or a broader constitution, but I think that certainly those types of groups can work together as problems arise. My experience with the Board of Consultants is that it will not work unless it has a champion in the project, somebody in the project that is really interested in utilizing that Board in a way to make their own management decisions. The Board is advisory, of course, and it's not making the decisions.

DR. CANTLON: Thank you. I think we better move ahead, we're running a little behind.

MR. CRAUN: Thank you.

DR. CANTLON: Our next speaker is Steven Kraft from the Nuclear Energy Institute. Rosa Yang is also going to follow.

MR. KRAFT: You know, you've got to determine if you have a right-handed tie or a left-handed tie at this thing.

Well, good morning. Thank you for the introduction, Dr. Cantlon, members of the Board. It's always a great pleasure and an honor to appear before this body and attend the meetings of the Rad Waste Club of America. We used to have one meeting a year and really fill the room, and now you go to two meetings a year and you don't fill the room quite as much. Given the money problems the Department is having, have you thought about tickets? We could sell memberships, we could sell sponsorships, we could have Coca-Cola, you know.

Well, once again, the industry is effected by delays created in the DOE program. We're ten minutes late, so we'll move right on.

The discussions we had at the last meeting were about suitability. Suitability is in our view part and parcel to licensing. You don't do one without the other. You could probably do the second without the first, which is part of what I want to talk about. So I thought that we would begin our discussion about alternate licensing concepts with reviewing where we ended in October. Rosa Yang from Electric Power Research Institute is with me today, and I will be calling on her shortly to carry on what will probably be the discussion of the most interest to this Board. But I want to use a point--I was going to make the point anyway, but the questions that Richard Grundy asked and Dan's response and the 80-50 percentages and things.

Everyone knows I go back to the very, very beginnings of this program, even before there was a program defined in the Nuclear Waste Policy Act, and the industry has been following and studying and recommending on this particular program for that length of time. And I just want to go back to repeating something that we tried to tell DOE many, many years ago that may have sunk in, may not have sunk in. When you say you think you can come to a determination that Yucca Mountain is suitable to develop further as a repository, whether you know it or not, you are in fact limiting your judgement to those technical and perhaps some not hard technical but softer science type of determinations that, you know, geology, hydrology, perhaps some of the less certain aspects of the project, such as predicting the climate and rainfall and all those sorts of things. But you are limiting it to those--let's call them scientific aspects of a determination, saying, "Yes, we can get forward to that point and say yes, we think we've got a suitable site." I mean, making no projections whether you would or you wouldn't.

But once you say that the site is licensable, whether you know it or not, you are saying, "I know I can get through the wickets of the legal process. I can take the science and I can go before any number of review boards, NRC, adjudicatory boards, and ultimately the courts and say I can convince people. I can take a judge or a three-judge panel or a full appeals court panel and probably the Supreme Court --and there's nothing in the law that says those people are geoscientists--and convince them that this was done correctly, NRC applied the rules correctly, and that license is in fact a good license." So you have to keep that in mind. And while I would agree with Dan it's ridiculous to try to predict percentages, it is in fact in my mind why you can predict a higher probability of success for suitability, but not as high a probability of success for licensability, because licensing involves all those other processes. And I want to talk about some of those procedural processes that are very important to success in this program that sometimes get ignored in the heat of scientific debate.

Last October, we suggested that the determinations of licensability be based on an integrated performance model that itself employs a realistic biosphere model. This is the matter that Rosa will be talking to you about in a few minutes. We have come to believe that perhaps after going through another six months of debate and deliberation on this matter that perhaps more than a biosphere model, perhaps a biosphere definition, is what's needed. And furthermore, NRC ought to revise 10 CFR Part 60 to reflect total system performance criteria. I added the word "only" to this slide from the October slide to make the point that there ought not be--this is not meant to say that we believe there ought to be a total system performance criteria on top of subsystem performance criteria, so that doesn't get interpreted.

Our view is that the licensing and regulatory changes contemplated by H.R. 1020, the Upton-Towns Bill currently before the House, and to some extent S-167, the Johnston Bill currently before the Senate, is in fact the alternate licensing concept or the beginnings of an alternate licensing concept that bears a great deal of attention and study and, in our view, the way we understand that piece of legislation, is pretty close to the right way we ought to be going. H.R. 1020 places sole jurisdiction in the hands of the NRC, requires NRC to modify its repository--oh, by the way, let me say, when you talk alternate licensing concepts, we also have alternate licensing concepts about interim storage facilities. They are also covered in H.R. 1020. I know we're talking about the repository here, but that ought not be forgotten as well. There are licensing concepts in H.R. 1020 that are on the critical path to waste acceptance that need to be dealt with, too, but I will not go into that here. It requires NRC to amend its repository licensing regulations to reflect the Program Approach. On March 2nd, in the one hearing that's been held in front of an authorizing committee on this program--Dan referenced it--NRC submitted a statement to the Senate Energy Committee reviewing the relevant pieces of legislation, H.R. 1020, S-167. If you have not read this, I commend it to your attention. The more I read it, the more I am convinced that this is a framework or a roadmap that NRC has put before Congress that says, "Here's how we think you can get from here to there," which is very, very important.

Now, let me just use that as a way to talk some more about how I interpret the words that NRC has to modify its regulations. NRC will play a very, very important role in determining what the national policy either continues to be or gets changed to in waste disposal. The Commission, as distinct from the staff, is set up to do that. There have been people on the Commission, particularly the outgoing chairman, who are very, very able policy thinkers and whose opinions are very highly regarded in all quarters that need to be listened to, and they will have their say as to how this ought to be done.

But once it's all determined, let's talk about the role of the NRC. The NRC has played a critical role in this entire program. In the early days of the program, well before anyone that has appeared before you in recent years was in charge of the program and DOE was in a period of time of trying to understand how you develop a program to meet a regulatory standard that they had never before ever had to do, the NRC people who were there at the time--many of them have retired by this point or gone off to other assignments--were in fact making some suggestions to DOE that were really quite helpful. And that shouldn't stop, and the law does contemplate that kind of relationship and there are various MOU's between the two agencies.

But my point is that when you read H.R. 1020, the way I interpret it, it says very explicitly, first of all, Congress would codify the program plan. Now, the word is "Program Approach" in the body of the Act, but the definition of program approach is in fact the program plan dated I think it was December 14th or something like that, '94, which is broader than the Program Approach. Touches not just site characterization, it touches waste acceptance, transportation, and a lot of other issues. However, the Act in itself goes forward and modifies the program plan by the nature of its terms, which as affects this discussion most directly, it eliminates suitability. Under the terms of H.R. 1020, suitability is no longer a project, it is no longer an issue, it is no longer something that needs to be dealt with.

There were earlier versions of H.R. 1020 that I saw that talk to replacing suitability with licensability. But I think the drafters of that legislation began to see something that we've been seeing all along. When you go to a bureaucracy and you say, "We want you to do X," the very first thing a bureaucracy does is it writes a procedure, identifies schedules, and determines decision points. Every time you do that, you open a program up to delays and to challenges that we don't think H.R. 1020 contemplates having this program subjected to. Our interpretation of these words in the way we read it now is very much like the way a private company approaches the question of whether or not we'll seek a license for a facility. You go talk to a utility that--do power for example--that has built a number of nuclear plants. There is nothing in the schedules or in the process where that company makes an explicit licensability determination at some point in time and says, "And now I will go forward." It is a fluid process. They believe that they can develop a license application and then design and build the plant to meet the regulations. The point in time where the company in essence says, "We think we've got this thing understood and we can get the license," is when the license application is submitted to NRC. There's an implicit decision, but it is not an explicit decision. And the way we read H.R. 1020, it's the desire on the top of that bill contemplating DOE act in that manner, and therefore the elimination of suitability.

Furthermore, the program plan would be modified if DOE thinks it ought to be modified in the future. And NRC must then change its regulations to be compatible with that program plan. So when I add all that up, what I think that means, what it means to me, is that DOE is running the program. Congress has told DOE to do certain things. Congress has made certain decisions already--location, need, timing, all those sorts of things. NRC's role in the licensing area is not to say whether or not DOE can or cannot do this project as a matter in and of itself. What they get to talk about and have the very important role of doing is saying, "Is it being done safely? And if it's not, here are some guidances to how you might meet our regulations so you can do it safely." And the only time NRC gets to say you can't go forward is if they conclude it is unsafe to go forward. I'm not saying NRC has not behaved in this manner before, and I make no accusations, I'm just pointing out this is the way I interpret H.R. 1020, because it affects the process, the legal adjudicatory, regulatory process.

H.R. 1020 establishes a three-step process for construction followed by license to emplace and a license amendment permitting loading and ultimately closing. But the timing is not in H.R. 1020. The timing is in fact in the program plan. And if DOE decides it needs to alter the program plan, NRC's regulations and NRC's timing has to change to reflect that. Again, it affects the process.

Another important aspect of H.R. 1020 is a provision that says should DOE determine--and again, through the program plan--that it wants to emplace waste earlier than

H.R. 1020 now contemplates, which is you get a license to emplace waste to a repository. For the sake of getting data to support future regulatory decisions, NRC would then be obligated to alter its regulations to allow that to happen and how it's going to be done safely. Whether or not DOE wants to do that or not is in DOE's discretion, not in NRC's discretion. So I think that's an important decision.

The key matter that I think you all are most interested in is the overall system performance standard, the health standard, what reads very much like a release standard or a dose standard. You know, all the debates that have gone on in the health standard reviews going on at the National Academy of Science. You could read it, and you probably have read it, dose to the average person in the population of the vicinity not to exceed one-third. NRC has said that is a perfectly doable, logical, consistent way to do business, and in fact they are--let's see, where am I here?--and in fact they are prepared to modify their regulations accordingly.

H.R. 1020--by the way, H.R. 1020 and S-167 are identical on that point, word for word. I mean, I've compared them, word for word. However, H.R. 1020 goes another step and says, "Here's how you go and evaluate NRC whether or not DOE has met the standard in the paragraph above." And it splits it into two time periods, 1,000 years and then 9,000 to 10,000 years, and then lays out a fairly hard and fast requirement for meeting the standard in the first 1,000 years that deterministic evaluation and etc. NRC has come back and in this review said there's uncertainties and deterministic evaluations as well that we need to deal with, that's certainly true. And I went back and I reread the relevant portions of H.R. 1020, and you know, it's funny the way these things turn on the placement of a word or the placement of a comma, and it has to do with the placement of the term "reasonable assurance" in H.R. 1020, and I think that the sponsors ought to be open to adjusting that correctly.

And I just go on to say here that given NRC's and DOE's critical role in doing this, if they see ways to make this happen that make it more logical, a better way to do business--and every agency knows this--there is an affirmative responsibility to go forward to Congress and say, "Hey, not quite. We're the experts, this is the way it ought to be done." And I know NRC is making those points.

In the second 9,000 years, however, there is some debate going on as to how the term "reasonable assurance" gets applied, what it means. Reasonable assurance is a terminology taken from reactoral licensing and has a huge case history associated with it that in our view H.R. 1020 takes the next step and says, "Well, okay, you apply that term, that concept, but you apply it in a way in the next

9,000 years, recognizing all the uncertainties inherent in there." And in our view, the term "reasonable assurance" isn't in itself sufficiently flexible to do that, and so when we were asked how we would go about doing this, you know, writing it down in statutory language is always hard to do, but how you would go about getting it across to the NRC in statutory directions, saying, "You're talking about likely compliance, not absolute compliance. You're talking about a very uncertain period of time." And again, our view that it needs to be done by an integrated performance model.

Again, I will repeat, and I'll come back to it at the end, clarifying the NRC's licensing action solely based on a finding of compliance of overall performance standard. And then another issue that was a very hot topic in the 1992 Energy Policy Act is NRC is required to assume that DOE's postclosure actions at the site will thwart human intrusion. Differs from S-167, which talks to actions after closure. Human intrusion, in our view, is not amenable to the kind of probabilistic predictions that you can make perhaps about harder sciences, and in the industry's work on the standards in the past, we have developed what we euphemistically call a "building code" type standard, where you go out and you take certain actions on the site that would prevent human intrusion by the nature of the location and design of the facility, not by active controls, which kind of stretch the imagination, I suppose.

Okay, one more slide and then Rosa. There's a lot of guidance given in H.R. 1020 on how the Commission will implement an overall standard, but how in fact they do that I would imagine there has to be some legislative history written as to how that's going to be done. Previously, EPRI developed a methodology for consideration by the National Academy Panel looking at the Yucca Mountain standard that we believe is an excellent method to implement these words in H.R. 1020.

And at that point, let me bring up Rosa, and then I will wrap up when she concludes. Rosa.

MS. YANG: Thank you. As Steve said, why I'm here is to report to you the work that we have done about a year ago, making a proposal to the National Academy of Science on the technical basis for Yucca Mountain standard, which we believe is very relevant to the success of the program. So I will give you a presentation on the work that we did.

As I said, the Committee was set up, and of course there is a strong interest from the utilities point of view to make our view known to the TYMS Committee. So EPRI was designated as the liaison to that committee, and we have participated in each of the meetings and made presentations. And at the end, we proposed a public health and safety standard for Yucca Mountain, and it is that standard that I want to present to you. And the sequence I'm going to follow is I'm going to present to you the thoughts that we have in leading to the proposal that we did. So I'm going to give you a brief view about what we proposed first, then I'm going to support it with some technical work that we did to explain to you why we came up with the standard we did.

The approach we took, is we used the performance assessment code developed by EPRI, called IMARC, which in the interest of time I'm not going to get into that. There have been presentations to this Board on IMARC, and there are reports available and we'll be happy to make them available to anybody interested. And we used that code to evaluate the performance of Yucca Mountain. We also analyzed the sensitivity to input parameters and scenarios and we tried to quantify the uncertainties.

So the standard that--excuse me. Before we get into it, we ask ourself, what would be the criteria for a Yucca Mountain standard? And there are three important aspects that we work toward. The first one is the most important one, of course, it has to assure the public health and safety into the far future, and you will see why we said that. And equally important is that it's got to be consistent with the scientific and the societal realities, because there are uncertainties involved and we've got to be scientifically credible. And the last one, but not the least important one, is they've got to be licensable. And we're hoping--like Dan said, there's an 80 percent and a 50 percent--we're hoping that the standard could be reasonable enough so the 50 percent could be as close to the 80 percent as possible. That's one key factor, as you will see, that we work toward.

So here's an overview first about what we proposed. What we proposed is reasonable assurance of sustained low health risk to an average individual in future local population groups. I underlined some of the key words used here: "reasonable assurance," "health risk," rather than releases, "average individual" versus maximally exposed individual. Another key feature is the two time frames. These are the factors that I will try to give you some insight about why we've come to what we've come up with.

The first one was "reasonable assurance". I think many of you know there are a lot of uncertainties involved in both the geology and, if you talk about dose, in the biosphere. And I just listed a few here, and I'm sure there are a lot more that you can list here. And the point we're trying to make, which is very familiar to all of you, is that you can always find a very few high consequence but the probability is very, very low type of scenario that could result in very high dose. Now, then, you ask yourself, is it reasonable to disqualify a site because you can either realistically or unrealistically imagine this kind of scenario that will result in high dose? And our thinking is the answer should be a very clear no. So how do you choose a standard to cover that sort of thing? So we think it's very reasonable to choose a risk-based standard which is very consistent with the concept of reasonable assurance which has been used by NRC in all the reactor regulations and licensing.

The next question is the current standard, the current EPA standard, is mostly a release-based standard, so we ask the question of release versus health-based standard. And our conclusion, based on the study, which I'll show you a couple of examples today, is that the release-based standard does not capture the true health risk, a health risk for all the scenarios that will be considered. For example, here's a calculation using our code of the released CCDF's at 10,000 years. We just picked two nuclides here, Carbon 14 and Technetium 99, and what is plotted here is the CCDF versus normalized EPA release. And here's the EPA limit that you're familiar with. And for the Carbon 14, which is about two orders of magnitude lower than the EPA limit, and for the Technetium 99 it's a bit lower. So that's a release. And if you look at the dose at the same time period for the same two nuclides--and we just picked an international standard, like ICRP 46, and you can pick any other criteria that you want to compare--for the Technetium 99, you can see it's about five or six orders of magnitude from a reasonable standard as opposed to the two orders of magnitude for the releases. And for Carbon 14, it doesn't even show up on the scale. It's off the scale to the left. So the point we're trying to make here is that if what you're trying to regulate is the health effect, then try to regulate to that parameter rather than another translated parameter like releases.

And I apologize for the next page. There is an important typo there. All of these sub ones really refer to the first bullet. The point we're trying to make is that if you take a Table 1 type of standard, like the EPA Table 1, each time you make any changes, you have to change the Table 1 value. We just give a few examples here. One of the most obvious ones is, in order to translate a health effect to the releases in Table 1, they assume a discharge to a big river, while the Yucca Mountain case is an enclosed basin. And there are quite a few others that each time you analyze a different scenario or have a change in the parameter, you almost have to go back and redevelop a Table 1 value. While on the other hand, if you look at the dose or the health effect, then it directly regulates to the things of interest to you. So we concluded that a risk-based or a dose-based standard is what you should regulate on rather than a release one.

Now, the next factor I want to address is what time frame. We all know all these nuclides last a long, long time. But again, I said earlier, we're not looking at nuclides, we're looking at dose or the health effect. So what we did here is look at the dose as the function of time. As you can see, the dose pretty much progresses with time and will probably peak at about several hundred thousand years. So this is a longtime issue. And similarly, we compare the individual nuclide that contributed to the dose. Here's a relative comparison we actually did. During the exercise, we compared our code calculation with a WISP code calculation, and we got very similar results, a little bit different here and there. But the whole point is that the dose doesn't peak until much longer into the future.

Now, do you set a standard, a million year standard, in the current regulatory environment? That's a question we asked ourselves, and we struggled with that a lot. What we come up with is because you really need to depend on the knowledge of the subsystem requirement and the expert judgement like the process that Dan talked about earlier. And the NRC will really need a general scientific consensus within the scientific community. So we think with the environment that we are in today, the regulatory environment we're in today, is reasonable to cut out a time period which is 1,000 years, which is the time frame that most feel can feel that we can predict the subsystem performance with reasonable certainty. And also we picked 10,000 years because the radionuclide inventory really dropped significantly over 1,000 years. So if you have a criteria or a system which you are very, very certain for the first 1,000 years, then you probably accomplish most of the purpose that you're trying to accomplish.

So here's just a curve again showing the radionuclide inventory as a function of time. As you can see, at 1,000 years, it drops the orders of magnitude from the emplacement activity. And as I said earlier, we think dose is the most appropriate standard in terms of Yucca Mountain. However, just to add added assurances, we think that we want a more stringent criteria for the first 1,000 years, because in terms of dose, there's very little dose for the first 1,000 years. So this is kind of like the concept of defense-in-depth. We want something much more stringent for the first 1,000 years so that we can develop a reasonably robust canister for the first 1,000 years.

Now, the last question about to whom. We think it's reasonable to say it shouldn't be to the maximally exposed individual, because no regulation is trying to protect everybody on earth. I think the airline industry is a good example, and there are all kinds of other examples. We're trying to protect the average person, not to protect everybody on earth. And we define what an average person is, and we're not trying to dilute the factor by average over the whole world population. The average person in a local population is the population in the immediate vicinity of Yucca Mountain and is an average in terms of age, health, diet, behavior. Those factors are important in the dose calculation. And we think the average person is most representative of the entire local population.

And we did an exercise to see what sort of dose you would get from an average person concept. I want to say that this is not a model. It's not a biosphere model. We're not saying this is the final word on what the model should be, we're just looking at examples. We look at a maximally exposed individual, which is something that you're very familiar with, which we call it a "straw in the plume" scenario. This is the worst case you can get, get all the drinking water from the contaminated plume, get all the food and live entire life right above there. And then we look at six other scenarios, and they include the different technologies, different population groups. And then we look at how different in terms of dose you would get.

This is a very busy chart. You can look at it in your leisure. But the point I'm trying to make is that you've got very different dose over this group. Therefore, it points out the importance about defining a biosphere that everybody can work toward.

So those are some of the technical studies we did. So let me wrap up on exactly what we proposed to the National Academy of Science a year ago. This is the same slide you've seen before. I think I developed the concept about reasonable assurance, developed why we prefer a risk standard versus a release standard or a stray dose standard. We looked at the average individual and the two time frames.

Let me just say a few more words about the two time frames. The first time frame we call the Engineered Barrier Period because it's a period that we really rely on a lot, if not 100 percent. I wouldn't say 100 percent because the geosphere always affects the performance of the Engineered Barrier System. But the focus for the first period is the engineer barrier. And we use the words "reasonable assurance" of substantially complete containment, and the measure is not 100 percent perfect canister at 1,000 years. You know, we want reasonable assurance of a good canister and we measure the performance of the Engineered Barrier System. So it does allow the credit for the overpack. And the repository will remain open for the first 100 to 300 years to do testing, to make sure what we think is going to work is still going to work, for retrievability if necessary. Institutional control is required for the first 300 years. We think this provides an added margin, because as you'll see, the second focus was really on the longer period, but we think this period provides added margin because there's no release and you're almost assured of no health effect and is a much more stricter standard than just the pure dose standard for the longer period.

Then, as I said earlier, for the consideration, you want to consider where the dose peak. And for this period, the Geologic Period, we have picked something, which we really haven't had a very fixed number to say, "You've got to be this," and we haven't really made a study on what it really ought to be, but we did take the ICRP 46 as a figure on merit for something that we're going to compare the results to. So we picked that as a design objective or figure of merit, but we don't think that should be a very quantitative licensing basis.

For the second period, we also think you should use probabilistic analysis similar to the NRC's policy on reactor safety. Another important factor here is that we don't believe human intrusion should be treated quantitatively for this period. We think you should design to make sure you minimize the possibility of human intrusion. You should use markers, you should use records, you should use everything possible to design it such that you minimize the frequency and also the impact of human intrusion. But other than that, we don't think it's reasonable to prescribe something artificial in the second period to include that in the calculation in the dose. And we also don't think it's reasonable--consistent with what Steve said, we think it should be total system performance. Look at the overall system. You shouldn't look at the subsystem criteria like currently in 10 CFR 60, because all of those subsystem criteria are already included in the performance model in the evaluation of the overall system performance. By requiring additional subsystem criteria, it could be counterproductive.

So let me conclude what I said. We proposed a standard, which the goal is to sustain low health risk to average individuals in the local population group. And we have two time frames. The first 1,000 years is reasonable assurance of substantially complete containment. Beyond 1,000 years we rely on both the engineer barrier and the geologic barrier to provide sustained, low health risk. We believe this approach is very consistent with scientific reality and acknowledge the reality and the uncertainties involved in both science and the current society, the practice we have today. We believe this approach enhances public acceptance and the licensing feasibility, because you have a very strict standard as a licensing basis, and then you do consider the risk into where the risk peaks. And we don't think the subsystem requirement is necessary.

Thank you. Steve.

MR. KRAFT: Thank you, Rosa, that was, as usual--really, at work, I've come to be very impressed with the work of Rosa and her staff, particularly John Kessler, who is here in the audience, and the folks they have under contract doing this work.

Let me put this in perspective. What you saw in the EPRI proposal, you can see how that led to some of the concepts in H.R. 1020, but not exactly. And I want to put some perspective on that. EPRI's methodology is a valid framework for applying the standard in H.R. 1020. It provides ways to get at how you make the two determinations in the first 1,000 years and the second 9,000 years, and it relies on internationally accepted standards. And I'll just go on to say the 100 millirem is consistent with international standards, ICRP 46, which is the repository standard, as well as others. But what's interesting is that even for periods beyond 10,000 years, a lot of people look at the 10,000-year limit as an artificial limit, and in some respects it is, in many respects it's not. I believe it is an artificial limit in the face of the life of radionuclides in the environment in this analysis. But it is a very real limit in the essence of how long can you run the calculations before it becomes just unrealistic to make decisions based on it, and I think that's an important point. When you go back to what I discussed at the beginning--and that's the real time regulatory process that is ultimately going to judge the quality of the work, and I think Rosa's presentation made it very clear that you still meet the international ICRP 46 standards.

Regardless of changes that may occur due to legislation, again, NRC needs to revise 10 CFR Part 60, which of course is directed in H.R. 1020, but even if it wasn't, to the total system performance criteria. We are very serious about that, and we think that the program is ultimately hurt by subsystem performance criteria, what Rosa calls "specious subsystem performance criteria." Go back in history--there are a couple people in the room who remember this, I certainly do--DOE fought the notion for years of a total performance analysis. And I've told the story before, but I'll tell it again. There was a meeting that I had with an individual who was responsible for this area in DOE, now long since retired, out of the program, after years of trying to convince DOE to pick up on this notion of total system performance criteria, the answer was, "I give up, we can't do it, we don't know how to do it." EPRI was brought into the picture. At that time, Rosa was not in the program. There were some other individuals, notably Bob Williams, who is also here in the audience, and the EPRI developed a very plausible model that was sort of the granddad of all the other models that DOE is now running in nine months for less than a million dollars. In DOE terms, that is no money and no time. Now, that doesn't mean it's the only model or the best model, it means it can be done.

But the issue here is not just performance of the repository, which is what gets into the second subbullet here, in a limited resource world, which we are all in and we'll always be in. That's the world. What a total system performance criteria backed up by an integrated performance model allows DOE to do--and this is where integrated performance models come into their own as management tools--allows DOE to do an ongoing project of assessing the performance of a projected repository given as the data rolls in for the analysts and determine where they need to be spending their money. If you're stuck with a need to meet a subsystem performance criteria and your analysis shows that that subsystem really has very little effect on ultimate dose, which is the issue, then why are you going to spend resources meeting that? Makes no sense and ultimately leads to a reduction in overall performance. The only time meeting a series of subsystem performance criteria equals meeting the optimum total dose criteria is in a resource unlimited world, which we don't have obviously.

Let me conclude in kind of a broader term and say as we address national problems generally--I don't care whether you're talking waste disposal or welfare reform--you have to deal with a number of issues, many of which are discomforting to people who like to deal in the scientific. I'm an engineer; it's discomforting to me. But they are nonetheless real. Public health and safety, technical feasibility are the issues we're talking about here, but you cannot ignore political feasibility, public acceptability, the regulatory framework and the economics. And I ran through those quickly, but I think they should be pretty obvious on the face.

Give you an example that really talks to the regulatory workability. When EPRI first proposed to us this two-tiered way to look at performance the first 1,000 years and the second 1,000 years and Rosa had, you know, all the CCDF curves and you had the limit. The thing that struck me is, when you look at that page, the ICRP limit is always way up in the right-hand corner, and the performance in the first 10,000 years, doesn't even get on the chart at the origin of the curves. So the natural reaction is, why in the world do you need anything other than something like an ICRP 46 standard? We argued about that for days when we first were putting together the response. And what it comes back to is this, a judgement being made by people involved in the industry and the regulatory areas, is that it is unreasonable for us to suggest that there ought to be nothing more but a standard that is so far out on the chart that you can meet it without a whole lot of effort and without a whole lot of proof when your releases in the first 1,000 years are going to be predicted to be so low. So that's how we came up with using, even though we believe very much in dose standards, a release standard for the first 1,000 years, to give some traction to the system so you've got confidence in defense-in-depth kind of activity. That is regulatory reality. That's the reason why we ended up there.

Lastly, and I will conclude with this, is that we have been discussing the first two factors on that list. The rest of those factors will be determined by Congress, by the public generally, in the courts, many other bodies. And it's almost impossible to know how that's going to come out, but I go back to our very beginning statement. H.R. 1020, in our view, properly balances technical considerations with all these other considerations, is properly protective of the public health and safety and will lead to, if Yucca Mountain is ultimately licensable, protecting the public health and safety now and in the distant future.

Thank you, that concludes our presentation.

DR. CANTLON: Thank you, Steve. We have run late for our time. Let's take our break now. We have the panel discussion at 2:45. You can collar either of these two speakers to raise questions that are hot on your mind now during the break. Let's cut the break to ten minutes and get back here in ten minutes, at half past.

(Whereupon, a break was taken.)

DR. CANTLON: All right, our next speaker is Steve Brocoum.

(Whereupon, there was inaudible casual conversation.)

DR. CANTLON: Let me ask that all of the conversations in the back of the room, if you have to continue the conversations, please move out to the hall.

DR. BROCOUM: Today I'm giving a brief update on the waste containment isolation strategy. We discussed this in quite a bit of detail in Beatty I think it was in January. We've also presented it to the ACNW about a month ago, and I think we recently presented it to the NRC several weeks ago.

I will briefly review the strategy. We will pass on discussion on the importance of the different barriers related to the strategy at the key milestones of '98, 2001 and 2008 and some of the ongoing activities right now to evolve and mature the strategy.

I think what I'll do here is put this chart on that viewgraph and I can talk from this chart. Now, the strategy has five key elements. The very first one is an environment that's very favorable for the waste package that's provided by the unsaturated rock. We're assuming that there is low ambient flux and saturations are low or not fully saturated and so that the waste package can perform very well in that environment. A major change we made since the SCP is we have robust waste packages designed to address any uncertainties that remain in the environment even though we think it's a very favorable environment. Limited mobilization. Again, a lot of this depends on the fact that there is very little flux and low saturation of water. When the waste package eventually fails--remember, the waste packages have a substantially complete containment for at least 1,000 years, or well in excess of 1,000 years. When the waste package fails, limited mobilization of radionuclides within the waste packages. Again, because not too much water to dissolve them. When they finally do dissolve, slow release of radionuclides through the engineered barrier and, you know, as the strategy evolves, we're considering backfills and diffusion barriers and those kinds of things, which may even enhance the engineered barriers. And finally, after the waste package fails and after the nuclides mobilize, after they get through the engineered barriers and they finally get into the geosphere, again, because of the low ambient flux and saturation, slow migration through the geosphere, through the water table, out to the accessible environment, through the biosphere, depending on the regulation, it ends up to individuals or populations.

DR. PRICE: This is Dennis Price. What does invert mean on this drawing here?

DR. BROCOUM: In the case of this today--what it will be in the repository I'm not sure--today the inverts are the concrete platforms on which the equipment behind the TBM is sitting on. I don't know if the design is well enough advanced, but that will be whatever surface you have on the bottom of the tunnels to allow the movement of the waste package MPC's in and out. I don't think that's been decided as to what that material is.

The strategy utilized a multi-barrier and defense-in-depth approach to increase our confidence in postclosure performance. Numerous barriers, essentially the five that I mentioned, defense-in-depth, we think that the unsaturated environment and the engineered barriers are more or less equally important in the near-field area, again, because the environment allows the engineered barriers in the waste package to work very well. The natural barriers add confidence that the long-term waste isolation will be achieved. After you fail the waste package, after you mobilize the radionuclides, after you get them through the engineered barrier, then you have the natural barriers. And, of course, the key thing to site characterization is understanding all of these elements and understanding uncertainties involved with all these elements.

Now, the next viewgraph attempts to tell where we think we will be at various key milestones. This, of course, as Dan said earlier today, is work in progress. This is subject to change. This is how we see it today. And even within the project, if you ask different people, they might see it somewhat differently. We think that at the 1998 stage we'll have, assuming we can complete all our tunneling and all, fairly good understanding of the environment, of the hydrology coming into the mountain and in the vicinity that would have any potential of getting under waste packages. So we gave it three checks, we can put full reliance on that. With regard to the waste package, we're showing that for Technical Site Suitability we'll understand it well enough to have it realistically bounded, we'll have models, we'll have some information, maybe not all the information. With regard to radionuclide mobilization, that's because we haven't completed designing of the waste package at this time. Same with radionuclide mobilization, all the testing for dissolution of radionuclides will not be completed. The engineered system and diffusion barrier, we'll have less information. We will have models but not as much information, and therefore we'll have to take conservative bounded cases because we don't have as much information. And the same with regard to natural barriers, especially in the area of thermal, where we will just be beginning or not have had long-term tests yet for 1998.

As we move up to 2001, we will have full confidence, if you like, in a low flux environment. The waste package will have been designed. It will provide a substantially complete containment for 1,000 years. We will have more or less, I think, completed all the testing on dissolution of radionuclides. We also have a very well designed robust waste package. We'll have confidence in that. The engineered barriers will have more information so we can realistically bound them, we'll understand the models and have them realistically bounded. Natural boundaries the same.

Finally, between 2001 and 2008, as we're collecting more information during performance confirmation, we will update our license application, and at that point we will be able to put full reliance in all the barriers. In a sense, we will have achieved the multi-barrier, defense-in-depth approach by 1998.

The issue here is how to present under the current regulatory regime, to the NRC, a case that you can make reasonable assurance at this point in time. I think the way you need to do that is not only be able to say that during the operational period of the repository you have a lot of confidence in these three, or certainly the top two during the rest of the operational period, but have plans in place, adequately defined, so that the NRC can see that you will be able to get to this point with the plans you have presented them for performance confirmation at this time. So the goal is to give them an application that has enough information for them to be able to reach construction authorization in approximately the year 2004 to proceed.

Some update on the activities. We're analyzing the linkages from the key uncertainties which you presented in January to the site and engineering plans, and this is all part of our rebaselining effort and planning effort for the last half of '95 and into '96. We believe to--I don't want to use the word "finalize"--but to draft finalize the waste isolation containment strategy. We need to understand and integrate the thermal strategy, which has been getting a lot of attention the last few months. And we're in the midst right now of planning the rest of '95 and starting on the '96 planning process. I will be telling you something about that this afternoon. But basically looking at it all, see what kind of changes in our testing and engineering plans we need to do to move on.

Key areas in the Site Program highlighted by the strategy. Obviously testing to characterize for potential fast flow-paths. Of course the underground tunneling is very important. We're looking very carefully at the thermal testing and the data needs as relates to Site Suitability and licensing. We're starting to think more seriously about addressing, of course, the benefits that we can get for backfill. An engineer needs to look at this to see, you know, the benefits versus the liabilities. The National Academy's report is not out yet. It will be out, we hope, by summer or late spring. But anticipating that we might be going with some kind of a risk-based or a dose-based standard, we have to start thinking more of a saturated zone. And, of course, we need to make sure that we--and I think to succeed in the process that I laid out--have a performance-confirmation program, you know, throughout the period of operations of the repository.

Some of the things we will be talking about, that the engineering program needs to address, is interface between the waste package and the MPC's and the repository design--there's a lot of activity going on here; considering the robust waste package and the multi-purpose canister as a waste form or a canisterized, if you like, waste form; different options for backfills; and extended retrievability. A lot of these different issues will be discussed and are related. We have a waste containment and isolation strategy. We'll be talking about thermal management and thermal testing. Later on we'll be talking about the system study on Calico Hills. Tomorrow they will be talking about engineered barriers, corrosion, waste package design, the criticality issue, and of course the concept of operations of the repository, how it interfaces with the multi-purpose canister.

That's kind of the status of waste isolation and containment strategy.

DR. CANTLON: Thank you, Steve. Questions from the Board? Dennis?

DR. PRICE: I notice in your topics, Steve, that the issue of operations does not seem to be there. It would seem to me it would be very easy to compromise your waste isolation if the operations do not have the proper quality control and don't bing things and bang things and can be accomplished in the hostile environment that you're talking about. There's a lot of operational issues we don't hear much about.

DR. BROCOUM: I think there is a presentation tomorrow on the concept of operations. That will be given by--I'm not sure who the author is for that. Is that you?

UNIDENTIFIED SPEAKER: Kal Bhattacharyya.

DR. BROCOUM: Yes, so you will hear--

DR. PRICE: Okay, I'll wait. And the question about the unsaturated environment and the engineered barriers being of equal importance and so forth, maybe you could help me understand, does that mean that if it turns out a period of wetness can come down and things can get pretty wet at this particular site, if that turns out to be expected for some particular reason, that since your full reliance on an unsaturated environment, that would not be an acceptable site?

DR. BROCOUM: I think it depends on probably a lot of issues, but how wet, how much water, geochemistry. You know, the design of the waste package. It's kind of hard to give you a simple answer on that. But we have a much more robust waste package. They are considering overpacks; you know, corrosion resistance and corrosion. A lot of issues here going on, so I think to probably design you'll have to decide how much water you can accommodate, if you like, and I think that will have to be considered in coming up with the final design of the repository.

DR. PRICE: And the final question I've got is you had said the natural barriers for long term. Do you have a concept in mind for the engineered barrier? How long is that reliance dependent upon?

DR. BROCOUM: Well, I think at least for the waste package, the substantially complete containment is for a goal well in excess of 1,000 years. Currently, we also have another systems requirement for the release rate and the engineered barriers will have to meet the requirements for that release rate, which would mean very slow leakage out, 1 to 10 to the 5 per year of the inventory, I think, in 1,000 years out of the engineered barriers, you know, after 1,000 years. So you will have to design for that under the current regulatory environment with the subsystem requirements. So I think that the engineered barrier will play an important role for a period of time.

DR. CANTLON: Don?

DR. LANGMUIR: Langmuir, Board. I'm pleased to see backfill in the picture, but I realize that DOE has been very concerned historically about the insulating properties of backfill. And I've also understood that perhaps the assumptions were based on perhaps faulty calculations of that effect. Do you have people working currently on the effect of backfill as an insulator on the thermal performance of the repository? Second side of that, I'm assuming that isn't going to even appear until after the retrievability period perhaps is past.

DR. BROCOUM: I probably will turn to Rick. Is Rick in the room? Well, let me just say a few words. We're looking first of all to see if backfill can enhance performance, and TSPA 95 is addressing that issue. Now, if backfill can truly enhance performance, then we've got to say is the enhancement worth the tradeoffs of installing it and all this? And I think, from an engineering perspective, a lot of the engineers are very concerned that backfill, first, will be very difficult to install, and secondly, in terms of thermal and insulation properties, you have a lot of issues there.

But it's kind of--and I was going to talk about it in thermal--you have to balance all of these potential pluses and minuses to come to the best decision. Now, I don't know, I need to see if Rick--does Rick want to say anything? He's got his hand up.

UNIDENTIFIED SPEAKER: I think Hugh would be best--

DR. BROCOUM: Okay, Hugh. I didn't even see you in the audience.

MR. BENTON: With the expectation now that the preclosure period will last 100 years, we would expect that the waste packages would have cooled sufficiently so that we could accommodate backfill without exceeding our thermal limit. However, if it does kick up the temperature, it can have an adverse effect on the long-term performance. That is being studied and we'll continue to work on that.

DR. LANGMUIR: Another question, a very different one. Looking at your Overhead 5, where you show the progression, which we would all hope one could obtain, from conservative bounded through full reliance, this is the various barriers key milestone issue, what if you get along there one or two steps in and it doesn't work and there's a reversal? For example, we all have been hearing recently a lot about the fast pathway issue in the mountain, the unsat zone. What if it turns out that looks far worse than it even looks now when we get more age dates from waters and fracture zones and in the perched waters and that sort of thing? Where do we go?

DR. BROCOUM: That's kind of the--if you want to call it the danger of putting up a chart like this. This is kind of how we see it today, but we're predicting essentially no surprises in coming up with some of these checks. I mean, there may well be surprises, and when there is a surprise or a change in our concept, then I think we have to go back and look at this and still see if we can meet a criteria of defense-in-depth and multiple barriers, you know, before we go for the license to operate the repository. I mean, that's probably the best answer I can give you. So I'm not meaning to preclude any surprises here, that's just how it appears to us right now.

DR. CANTLON: Questions from the staff? Leon? Or Bill?

DR. BARNARD: Bill Barnard. Steve, I have another question about the same chart here. Along the lower axis you have reliance on barrier. Does that imply an increased accumulation of data as you go from left to right? Is that where you get the reliance?

DR. BROCOUM: In my view of this arrow, you know, we can actually remove the arrow, it doesn't really change the slide, but it does. Yes, we are going to be getting more information. I think we've showed you charts in the past where the curve kind of went up like this, and it was trying to make a similar point. We are collecting information.

We'll have more information here, we'll have more information there. And in fact we'll be collecting information until the repository, assuming we get past all of this, is closed. There will be more information, so in making these evaluations, through time you'll have more information.

DR. BARNARD: I'm just wondering, you've got two areas here that deal with a geosphere, number 1, which is low flux environment, and number 5, which is natural barriers. In one case you have all the information you need by 1998, and in the other case you have another ten years to collect it.

DR. BROCOUM: The low flux is the water kind of coming into the system, coming into the repository and rising, contacting the waste packages, which leads to corrosion which leads to dissolution which leads to transport. This is when you kind of put it all together and you kind of model it and you try to understand how the transport mechanism and hydrologic and thermal mechanisms work. It's a very complicated area we call coupled processes, and that's why the difference. This is limited to the water. This takes in all aspects of the natural barriers.

DR. CANTLON: Leon?

DR. REITER: Steve, a question on the same chart. And maybe it's just a matter of choice of words, but if you had labeled that, you know, "Little Reliance," "Moderate Reliance" and "Full Reliance," I could understand it. But somehow you've chosen the words "conservative bounded" and "realistic bounded." I mean, the Board has had some questions whether you really can make conservative bounded arguments so early, but if you really could, let's say you could, aside from design purposes, why go any further?

DR. BROCOUM: Aside from design purposes what?

DR. REITER: Yes. Why go any further? If you can make a really strong conservative bounded argument, aside from modifying design, why go any further?

DR. BROCOUM: Because even with a conservative bounded there may be a little information, there may be large uncertainties.

DR. REITER: Yes, but--

DR. BROCOUM: There may be a lot of expert judgement, for example, in doing those kinds of things.

DR. REITER: I assume the conservative bounded argument, the word "conservative" means you take those uncertainties into account.

DR. BROCOUM: Perhaps, yes. And also, I mean, as you know, you may not be able to proceed. You may not be able to meet that kind of--if you make everything very conservative, you may not be able to succeed in finding the site either suitable or proceeding. That's kind of a trap you can fall into.

DR. CANTLON: Let's take one more question. Don?

DR. LANGMUIR: Steve, sorry, the same overhead again. The low flux environment issue, I didn't hear you talk about what could be created in terms of a flux which could be far greater by the high thermal loading strategy. In other words, the strategy itself of thermal loading will influence the flux.

DR. BROCOUM: That's correct.

DR. LANGMUIR: And the flux you're describing here, which we have great appreciation of in '98, is the natural flux, hopefully.

DR. BROCOUM: Yes, it's more--

DR. LANGMUIR: But if we vary the thermal loading, we have a fairly unknown, don't we, an assessment of what that flux might become, particularly under high loading?

DR. BROCOUM: Yes, we had some discussions about that exact issue.

DR. LANGMUIR: You haven't got three checks anymore if it's a high loading strategy--

DR. BROCOUM: Yes, it obviously depends how you define the term. And we will here define the term for more or less ambient conditions.

DR. LANGMUIR: Yes.

DR. BROCOUM: Okay. And there's where the site exists today.

DR. CANTLON: I think we better get on.

DR. BROCOUM: Okay, thank you.

DR. CANTLON: Thank you, Steve.

Ed Cording will chair the final part, or the beginning of this next session, so Ed, it's over to you.

DR. CORDING: Our next three presentations--and Steve will be continuing with the first one--but our next three presentations will focus on the emerging thermal strategy. And I just wanted to note that certainly our understanding of the impacts of thermal loading on flow, vapor and fluid in a Yucca Mountain repository has evolved considerably, and one might say even dramatically, in the past five years, and we know there's much progress yet to be made. In 1989, heater tests were performed underground in G-Tunnel in Rainier Mesa at the test site in the welded tuft. Those tests and the resulting studies that have come from the review of the test results and further studies, these have led to a new understanding of thermal effects on flow in the unsaturated zone, and as a result to consideration of a wider range of thermal loading options and consideration of the impacts of these thermal loading options on repository design and performance.

Presentations today will provide an update on the developing thermal strategy and an update on the concurrent efforts to develop an understanding of thermal phenomena and site performance through the thermal testing, heater tests, in the ESF, these tests performed once underground access is achieved. And certainly of great concern to all of us is how these activities will be integrated into a thermal strategy and ultimately leading to the site suitability decisions and license application. So Steve will start our presentation, then we're continuing with Tom Geer and Tom Statton on thermal loading strategy and thermal test strategy, respectively. Steve on the emerging thermal loading strategy.

DR. BROCOUM: We've been talking about the emerging thermal loading strategy. The word "emerging" is getting a lot of use here.

The first viewgraph is what's the role of the thermal management strategy in meeting the program objectives, because we're talking about program objectives here. We're trying to design the repository system for timely disposal of the desired amount of waste at an acceptable cost, something that Dan said this morning. We're trying to establish a thermal loading range that is compatible with the preclosure and postclosure performance objectives, and we're trying to maintain flexibility to optimize design and performance during construction and performance confirmation. This is very important, we think, so we don't go down one path and find out for whatever reason ten or twenty years down the road we made a mistake and it would cost us more to recover, if you like. This flexibility may have a cost associated with it, but it's probably better to accept this cost today, and we'll be talking about that a little bit.

Some of the things that have evolved in coming up with a thermal loading strategy. First of all, of course, we had a decision to utilize the multi-purpose canisters, and much larger in a sense, waste package. We implemented the Program Approach. We're trying to phase our testing to manage our resources and to provide demonstrable measures of progress. Dan, again, talked about this this morning. We have implemented a step-wise site suitability evaluation.

While I'm talking about site suitability, let me mention that I think we're very close in implementing the contracts for the peer review with the National Academy any day now. As soon as that contract is implemented, we will send them the first technical basis report, which is ready to go.

And we're trying to provide increased confidence to support our licensing milestones. You know, the G-Tunnel and all the modeling done at Livermore and all the other work we've done, we've gotten a better understanding of some of the issues related to the thermal effects.

Some of the key topics that we'll talk about a little bit. First of all, we're trying to maintain the multiple hypotheses about the effects of thermal loading and we're not trying to narrow down the hypotheses and only test one. We're going to try to analyze a range of thermal loadings to support critical program milestones as opposed to a single one. And we're trying to prioritize and schedule the testing needed to evaluate the thermal effects.

Some more key areas. The different elements of the repository, some may behave better hot, some may behave better cold and so on. I have another viewgraph later that gets to some examples. But we need to balance all of these to obviously optimize the repository. We had, you know, decisions about the multi-purpose canister. That might effect some of the design of the repository, it may have some constraints. And obviously the issue that Dan brought up this morning, we need to have adequate repository capacity.

Now multiple hypotheses. The bounding cases may be the high loading on the one end with the possibility for extended dryout. The other case may be the low loading with the potential for more limited thermal disturbance. With the high loading, we probably cannot get the information in time on the type of schedule and try to deal with the low loading. For example, we probably cannot have adequate repository capacity. Our goal here is to maintain design flexibility--okay, and this is very important--to maintain design flexibility in the design so that we can increase our thermal loading to improve postclosure performance and cost-effectiveness, if it's supported by the test results. So what we intend to do as we go into site suitability evaluations, as we go into the license application, is just have in a sense a range of thermal loads that the design can accommodate. And if we can, we're going to keep that range constant from TSS on, and what will change in time is as information comes in, we can actually plan to operate the repository at a thermal load that is consistent with the information we're getting. So that's a little bit different than what we were thinking a few months ago of arguing low and then later on getting hotter. And in fact it even allows the potential to go down in time if for some reason the data showed us that might be a way we need to go. The key thing is being able to carry forth a flexible repository design that will work over that range that we're concerned about. That's the key thing. I think that comports with your letter of December 6th, when you recommended that the DOE carry three designs, or some level of design, a low, the SCP case and the high. In our view, this would meet that recommendation, because that range would cover that whole span.

So in terms of supporting the critical milestones of the program, for 1998 we would use the best available site and engineering data to evaluate suitability over the range of thermal loadings under consideration. That range, now, will go from low to high and will remain constant through the process. The uncertainties within that range may vary, depending on the information we have. For the 2001 license application, we'll evaluate performance for the range of loads that can be supported, again, with the available site and engineering data at this point and time. And we will maintain that design flexibility to operate somewhere within that range that we can make the case for it to our regulator.

The testing we're doing to evaluate the thermal effects. In our planning for '96 and in our planning for the rest of '95, we are looking at various options to access the repository horizon even earlier than our current plans for the inner thermal testing sooner. And it's very important to understand what the uncertainties are in the performance predictions over, again, that range from low to high.

Now, next viewgraph talks about the balancing of these objectives. For example--and these are examples, not meant to be a comprehensive list; I'm sure there will be many other issues we have to consider--the PA people and the modelers tell us, you know, the less you have to worry about coupled processes, the easier and less complex the modeling and the calculations are. In other words, if you're on the lower end of your thermal range, it's easier to do these evaluations. The waste package performance, however, may improve if you keep the environment dry in the near-field. And so therefore the waste package may be better off if it's hotter. Cost is lower if less repository area is utilized, so therefore, the hotter you could make it, the less it may cost. In fact, over the last several years on the engineering side of the house, they always made the case for a very hot repository for this reason here. This was a key reason. However, preclosure operations, that 100-year period of time, it may be easier to operate if you have lower thermal loadings, certainly during that period of time. And depending on how you calculate the groundwater travel time and if you have to worry about thermal and hydrologic effects, if you have less thermal disturbance, you may have a greater distance over which to calculate your groundwater travel time.

The MPC is an important consideration in evaluating thermal loading. The conceptual design and the specifications for the MPC did consider thermal constraints--rock wall temperatures and cladding and the center lying temperature of the MPC. Of course if you're going to put backfill in you may have to reconsider the assumptions going in on this. Basically, again, it's the balance issue. The impacts, you have to evaluate the impacts of the MPC on the overall design of the repository.

The issue about repository capacity. The lower end of the thermal loading range is likely to require a larger repository area. In our planning, we are preparing contingency plans for some characterization for expansion areas around the repository block. However, there are other design options also. You know, there is ventilation and there's aging and there's two-layer repositories that might allow you to get more into the repository.

Are these various options I think you're talking about, Mr. Geer. Are we talking about that?

MR. GEER: Not in any detail.

DR. BROCOUM: Not in detail, okay. That might allow us to take up to double the thermal loading with the same effects.

So what are we doing? We're in the process of developing a coherent thermal loading strategy which will become part of our waste isolation and containment strategy, which in turn will become part of our licensing strategy. The M&O, at our request, has produced a White Paper on Thermal Loading. That paper is under review. That paper defines the key technical issues, from their perspective, that need to be addressed. That paper attempted to establish an integrated approach and evaluate the options, gave the options and recommended approaches, and it identified the key information needs. So we need to review that paper.

The other thing I need to say is what that paper did is it brought together, in the coherent kind of way, many elements of the program to think about thermal loading in a highly interactive mode. E-mails were flying back and forth among various participants. It was, from an intellectual point, a very stimulating type of exercise. A lot of times in the past--and I think this is the problem the program has had, it's an integration problem--again, the engineers preferred a hot repository. The PA people, from their perspective, it was easier to do the calculations looking on the cool side. This paper, whatever one thinks of the paper, was very important in integrating and bringing in the people and making them talk to each other.

The other activities are evaluating and prioritizing the in situ tests that will advance understanding of thermal effects. I think you're talking about it later? Okay. And we are developing flexible design plans for the repository and the waste package that allow us, again, to accommodate the thermal range that we're considering.

Okay.

DR. CORDING: Let's proceed with the next two presentations, I think, and then we can have a question session at the end of that. This is Tom Geer with the M&O. He's the manager of systems engineering.

MR. GEER: Okay, as it's noted, my name's Tom Geer. I'm the systems engineering manager for the M&O here in Nevada working on the Yucca Mountain Project. What I intend to do in this presentation is to review for you what the features of the M&O's proposed thermal loading strategy are. My goal --I think Dan Dreyfus said it very well this morning--the goal in engaging in the discussions related to a proposed strategy is not to put a strategy up and then defend it to the death, but it's to engage the various interested parties in rational and reasonable dialogue so that we can exchange the various technical views so that we can understand and bring to light where weaknesses are in the various proposals and so that we can provide some clarity to our planning.

In establishing a proposed strategy, of course, we considered various alternatives, and I won't be discussing those in various lengths, but suffice it to say that we could have proposed that we go all out towards developing a low loaded repository or we could have gone all out towards developing a high loaded repository. What the proposal instead recommends is that we consider evaluating a range of options within there that we identify what's necessary in our planning and design processes specifically to keep those options open as more information becomes available.

Having said that, then, what we did was gathered a team of experts. It was a very wide team. There were three principle authors who assembled the proposed thermal strategy essentially to identify a roadmap to developing a thermal loading recommendation. What the strategy does is it describes the process needed to ultimately select the thermal loading, identifies and discusses the various alternatives that were considered, identifies the activities that are needed at each stage of the way and the associated timetable to make the various decisions. The strategy itself is based on our currently available information and our understanding of what that information is. What we have available to us for the various analyses that have been performed, our performance assessments, system studies which have been done, as well as the development of various thermohydrologic process models. We have testing results which were looked back at. Some of those have been mentioned earlier. We have information from surface based testing, testing which has been conducted in the laboratory, and information available from G-Tunnel. Also, a great deal of technical judgement went into proposing this strategy. And I want to emphasize that there is not a set of uniform technical judgements either within the program or within observers and commenters on the program right now, which caused and stimulated a great deal of debate on the various merits of some of the aspects of the proposal. Since we don't have unanimity of thought, the proposed program thermal strategy represents to us work in progress today. It's currently undergoing review by DOE as well as the rest of the program team, and we hope to evolve into a more solidified strategy as time goes on.

To review for a little bit on what the basis of our current understanding is, we've built various analytical models and conducted analyses. Results of those have indicated to us that, you know, thermohydrologic predictions are much more complicated or represent much more complicated phenomena than we had previously envisioned. It's important for us to note that the high thermal loads may produce large scale water movement in the mountain, but also the low thermal loads may produce water movement to some extent. There are various and differing waste package corrosion issues across the range of the thermal loading. And indeed we have some testing of information available, which I have mentioned before.

At the heart of the proposed strategy is the need to maintain design flexibility so that we can provide a phased approach to obtaining the necessary information to make our final decisions. An evaluation of the various thermal loading alternatives would be provided as part of the license application in 2001, and we'd provide an update to the selected thermal loading in 2008. The goals of the strategy are to meet the preclosure and postclosure requirements, to also meet the program's key milestones like Technical Site Suitability for the NEPA process and the license application and the license application updates. And the goal of the strategy is also to identify the activities needed to achieve those objectives.

So considering the information that we had available today and what we expect will probably be available at each of the major milestones, we established a set of strategy steps based on the consideration of the alternatives that were available to us. And essentially, at the first step, we would determine a sufficiently low thermal load for the Technical Site Suitability discussion for 1998 such that there would not be significant perturbations in the geologic setting at some distance from the emplacement drifts. We would evaluate various alternative loadings during the NEPA process. What we would do at license application or prior to license application is determine the Maximum Design Thermal Load that we're required by the regulations to identify. We would do that based on available information, a conservative --Steve mentioned the competing design objective--so we would select conservative design features where necessary and provide flexible design where that was appropriate. We would do that based on bounding analysis and expectations from the performance confirmation program. What we would do is essentially evaluate responses to alternative loadings from the low range established at Technical Site Suitability to the high of Maximum Design Thermal Load, which would be the one specified in the license application. Now, in the period from 2001 to 2008, we'd select from within the range of loadings from low to the MDTL, we would select an operating thermal loading for the emplacement. And that load would be less than or equal to what we had specified as the Maximum Design Thermal Load. And then we would continue to conduct a performance confirmation test for the thermal loading aspects of that program after the initial waste emplacement.

Okay, so I'll review for a little bit what would be available to us at each point.

For the Technical Site Suitability evaluation in '98, we would be relying largely on laboratory, the large block test, plus some limited in situ thermal testing results and our bounding analyses. The Site Suitability evaluation would be based largely on the test information gained from near-ambient conditions. So as a consequence of that, we would expect at this time the evaluation would be at the low end of the range of thermal loadings. The proposed strategy also calls for increased characterization of the expansion areas as that might be needed. Steve mentioned that we're considering contingency plans for that as part of evaluating the strategy.

Now, between 1998 and 2000, we would evaluate further thermal loading alternatives. We'd have additional results from the ongoing short-term in-situ heater tests.

And the purpose during this period would be to identify the appropriate range of loadings for the license application, particularly what the Maximum Design Thermal Loading is.

For the license application, then, we would have developed a flexible design capable of accommodating the range of thermal loadings which are presented in the LA. We'd identify what the Maximum Design Thermal Loading is and what the responses of the systems are and the natural system to that loading. The license application would provide an evaluation of repository responses for that range of loadings up to the MDTL. And we would also discuss the features of the performance confirmation testing plans that would be established.

Then, as part of the license application update, we would actually select the thermal loading that we desired for the initial waste emplacement. We would do that based on the additional results available from the performance confirmation testing that would have been completed to that point. We would also further describe the plans for ongoing performance confirmation testing, which would continue to be conducted after waste emplacement began.

In order to actually maintain the flexibility, we had several options. We've already instituted a flexible approach to design. Subsurface designs are being put together that can accommodate the range of thermal loading.

Some of the features of that are you can accommodate the range of the loading either through your waste package, spacing within a drift, or perhaps in the number of drifts or the spacing of the drifts that you actually emplace waste in; planning for phased construction and development of the repository if needed, and that would allow for the use of expansion areas to be included as they were needed. We're also looking at various thermal management options related to providing ventilation during that preclosure period, providing sufficient lag storage for aging, and various other options. We're looking at flexible surface facility design as well, designs for facilities that can handle multiple MPC sizes, also what would be the size of the needed perhaps the lag storage facilities to handle more than just surge loads but to handle an aging function as well.

We continue in the waste package development area by looking at conservative waste package designs that would use robust materials suitable for the more challenging warm and humid conditions that we might expect at the low loadings, looking at the various waste package designs that might be suitable for the different MPC designs in planning for phased procurement of those so that as we learn more information we can minimize the investment made at any given point in time. We also need to evaluate various other waste acceptance considerations that might help us in the management or refinement of our thermal loading strategy, such as going after oldest fuel first and managing, to some extent, the receipt and throughput rates at the repository.

Tom Statton in a minute is going to talk in more detail about the testing, but I wanted to go for a minute over what some of the premises are surrounding the Minimal-Disturbance or the low loading concept versus the Extended-Dry Concept and why the strategy is time phased the way it is based on where we expect certain information to be available. In the '98 time frame, some of the premises, some of which are based on hypotheses, others of which are based on our understanding by having evaluated certain of our models, in '98 we would expect to be able to demonstrate that the ambient conditions are favorable, that the minimal disturbance concept would provide no significant perturbation to those conditions, and that through conservative waste package design we would have adequate waste package containment. And also from the limited amount of in situ heater tests in our thermomechanical effects would be acceptable.

The Extended-Dry Concept is a little more challenging, at least in the minds of those who put together the paper, in that we'd have to be concerned with does the Extended-Dry Concept provide the opportunity to focus flow in an adverse way back into the emplacement drifts or would we be able to actually use the Extended-Dry Concept to move the water away? Again, we have information related to--that should be "thermomechanical"--I don't know how many times we've had to change that--the thermomechanical effects also would be acceptable. But then from the ongoing tests that would become available prior to the license application we would have better assurance that we were achieving dryout in our local conditions, which would provide the more favorable environment for the waste package. And then at 2001 we would essentially use the results of scaling up our smaller scale tests and our laboratory tests coupled with our bounding analyses to make our arguments with respect to rewetting and condensate, and then we would have performance confirmation testing, essentially which was ongoing, to help prove those points.

Okay, so in summary, the M&O has completed a paper that provides a proposal for the program thermal strategy, and we believe that it's providing an important focus for ongoing discussion and ensuring that we are incorporating the necessary flexibility into the design and that through the ongoing discussions we'll have appropriate flexibility in the development of the program itself as it goes along.

Thank you.

DR. CORDING: Thank you very much. It might be good if there are questions at this point to have a few questions.

Pat Domenico on the Board.

DR. DOMENICO: There's two statements that I'm reading here, and one says thermal hydrologic predictions indicate more complex phenomena than previously was envisioned. And then on the next page I'm reading where "at this time it is expected that the evaluations on site suitability will be made at the low range of the thermal loading." So I suspect there's maybe a connection between site suitability and the thermal load that you select. And my question is, do you envision that there is some thermal load at which the site can no longer be deemed suitable? And when, if any time, would you be able to identify what that load is?

MR. GEER: I don't--

DR. DOMENICO: In terms of the data needs. I don't see that much difference in data between 1998 and the year 2001.

MR. GEER: Okay, I don't think that we've anticipated a maximum acceptable thermal load at this point. We've conducted various analyses which have focused us to low regions and high regions for practicality and testability, but I don't believe we've got data available to us or an indication that there would be an unacceptably high load.

DR. DOMENICO: I seem to recall one of the last meetings we had, when we talked about the effects of thermal loading on capacity, I think it was brought out that under low thermal loads the capacity is tremendously reduced. And I just wonder if that is the right scenario to select for a site suitability study.

MR. GEER: I'm sorry, could you repeat your question?

DR. DOMENICO: Yes. Under the low thermal loading, the low boiling, the capacity of the mountain is drastically reduced. I thought Steve brought that out in one meeting in the past. And I just wonder whether that is the right scenario to select for a site suitability examination, the low thermal loading one.

MR. GEER: At both the high end and the low end there are characteristics of either of those strategies that would cause you great difficulty. One of the ones at the low loading range is indeed the repository capacity issue. In the range of, you know, 36 MTU per acre, we need something around 2,600 acres for emplacement, which is why the strategy put forth, and it's under consideration, is recommending that we consider plans for characterizing expansion areas. You're not the only one to have made that observation, and it's bringing a lot of focus to that debate.

DR. DOMENICO: Thank you.

DR. CANTLON: Other Board questions?

(No response.)

DR. CANTLON: Staff? Leon Reiter?

DR. REITER: Leon Reiter. Just to continue on that line, and I understand this is an emerging strategy, but I see a sort of fundamental difference between what you proposed and what Steve proposed, and perhaps you can correct me if I'm wrong. You stated that you're going to start out for Technical Site Suitability at the low end of the range of thermal loadings, and one of Steve's slides adds, use the best available data to evaluate suitability over the range of thermal loading under consideration. That's a far more wide scoping and full ranging evaluation. To me, it implies not only looking at a much larger footprint than telling the people now who are preparing the technical basis reports that you're going to look at this wider footprint, but also telling the people who are doing the TSPA for site suitability they have to worry about a container that may be more prone to corrosion. On the other hand, if they're doing a TSPA or looking at hydrology for high thermal loading, they're going to worry about very complicated procedures. Is that a real difference or am I missing something here? But you're proposing what Steve has proposed?

MR. GEER: I don't know that there is a real difference. Maybe it was more in the way that I said it. We're going to go from the '98 beyond the '98 time frame for the next couple of years and evaluate in a range. We will have information--

DR. REITER: Right.

MR. GEER: --in '98 about the range, it's just the proposed strategy recognizes greater confidence at the lower load at that point.

DR. REITER: The question is, are you going to evaluate Technical Site Suitability over the range of thermal loadings or is the evaluation of Technical Site Suitability to be done at the low end in 1998?

MR. GEER: The proposal--go ahead.

DR. CANTLON: Steve Brocoum.

DR. BROCOUM: The proposal that DOE's putting on the table right now is what I said in my paper, over the full range. If you remember our development of Scenario A, which ended up being the program plan, in the original proposal in Scenario A, we were going to evaluate suitability over the full thermal. Then we went through a period where we were thinking more about single point type designs for the repository, and then we said, "How can we handle single point design?" At that point, we were thinking more low. Now, Rick is here and he's committed to doing a flexible design over a large thermal range that allows us to go back to our original strategy, which is to evaluate site suitability over the full thermal range. So what you're seeing here is a sequencing of events, and this report that Tom is referring, was finished a month or so ago, and we've been doing a lot of thinking since then.

DR. REITER: So is that correct, Steve, in your TSS you're going to show suitability over a much larger footprint than presently assumed?

DR. BROCOUM: I'm not ready to say that, but TSS will consider, for those guidelines, the thermal loading effects, we will consider the range of thermal loading. It's not in any guidelines.

DR. CANTLON: Russ McFarland, Board staff.

MR. MCFARLAND: Yes, Tom, in thinking about this session this afternoon and talking with you all, one hope was that some of our definitions, some of the words, some of our concepts had gelled a little more since our last meeting last fall. For example, on page 11 you make the comment, "ambient conditions favorable." Do we have a basis to make that statement? Have we yet defined what a significant perturbation is, or even what is an acceptable thermomechanical effect? Has the program evolved over the last several months where these terms can start being quantitative rather than purely qualitative as they have been in the past?

MR. GEER: My general impression is that they remain largely qualitative. The ambient conditions being favorable is one of the premises on which the middle disturbance concept rests. And Steve, I think, addressed some of the features of the site with its unsaturated nature and low ambient flux, etc., that go into that. Let me say it from my perspective and my understanding, because we don't have a definition. This means minimal disturbance in quantitative terms. There are differences about what that means to different people. So from my perspective, my understanding of the concept is the minimal disturbance concept. It is limited to those conditions where we don't have temperatures where bulk average temperature of the rock is above boiling. We don't have coalescence of the boiling front between the drifts. Movements of the water, significant perturbations of the water distribution, are limited to drift scale movements in those cases. So that's about as quantitative as I can express it.

MR. MCFARLAND: But these were the definitions we had last November. How can we better understand the thermal management arguments unless we have a common understanding of what these conditions of these two extremes are?

MR. GEER: I believe we're going to have to rely on some of the early test data to help us clarify that. And I think Larry wants to add something to help me out here.

MR. RICKERTSEN: I'm Larry Rickertsen with the M&O, one of the people that contributed to the writing of that paper. One of the things about the thermal loading strategy is that it is not a place where all those terms are defined. It's a step-wise plan, if you like, for arriving at definitions like that. Some of them might stay qualitative, such as "significant." On the other hand, we may go through a process. There is a study described in that plan at arriving at some notion of what that means. We're not ready to do that today. The paper describes a plan at when we would arrive at that. There are other terms, like what do you mean by "low loading"? What do you mean by "Maximum Design Thermal Loading"? That paper also describes or proposes a plan for getting at those terms as well.

MR. MCFARLAND: The paper you're speaking about is the White Paper?

MR. RICKERTSEN: The one that Tom was just describing.

MR. MCFARLAND: Oh, the one that Tom is.

DR. CORDING: I think one point of this, one example, is the definition of the term "ambient," and one says that we're working with ambient conditions and therefore minimal disturbance. And I know there has been discussion, some of the work has been done on looking at buoyant effects, and some of the effects of even lower thermal loading shows that there has been some potential impacts. And I think that's where one has to really get focused as to are we really in a situation where it's ambient or is there a lot of information we need to understand about even the low thermal loading or the thermal impact that has to be part of that loading level? And so I think that's one of the interests, is if we go with low thermal loading, do we have enough information, is there enough there, enough of an understanding of the low thermal loading environment in terms of the fluxes that are developed even under that condition due to the thermal effects? Is there enough understanding to be able to go forward, you know, with that option? And is it really an ambient condition, or how far from that is it?

Other questions? Dan Bullen, consultant to the Board.

DR. BULLEN: This is actually a question to both speakers. You wanted to keep flexibility in your design and you wanted to be able to accommodate low thermal loading, high thermal loading, different waste package designs. I guess the question I have is a follow-on to what Russ McFarland mentioned. What is the basis for the decisions that you're going to make? When you decide whether to go low or hot, do you look at total system performance and decide its dose to the public at X equals 100,000 years, or how do you make the hard decision? What's the basis that you're going to use for making those decisions with respect to both package design, thermal loading, waste acceptance criterias, whatever?

MR. GEER: I would defer to Jean for the TSPA perspective. We have to evaluate the options and the impact of performance of the mountain and engineering systems in the total system performance assessment.

DR. BROCOUM: I think the understanding, or maybe the bias that we have today is say we evaluate for licensing over the whole thermal range, that the uncertainties will be higher as you go to a higher range. Then you will have to decide at what point, at what level of uncertainty can you make a reasonable assurance case to the NRC. And that is the point that you go in with. But it may not be that simple. They may be thresholds, they may be reversed in some cases. So we're kind of going in with some degree of preconceived notion, and I think what we're proposing allows us to look at that and make sure that our current ideas are in fact valid. But to me it's balancing the uncertainties at a particular loading with the information you have.

DR. CORDING: Yes, Don Langmuir.

DR. LANGMUIR: Langmuir, Board. The word "uncertainty" bothers me a bit here, because you may have very low uncertainty at low loading, but that low uncertainty may be a very probable failure due to corrosion. So uncertainty alone is not the issue, it's the uncertainty about what.

DR. BROCOUM: Yes, I was assuming, perhaps obviously incorrectly, that yes, your performance is acceptable.

DR. CORDING: Okay, one question, Steve. In some of the materials it describes the potential for going to considering other levels of thermal loading, and in some cases it's not clear whether that's something that's occurring in the process to the 2001 date or subsequent to that. As you go forward with your investigations, is it a possibility that you would be selecting something other than the low, say, thermal loading option prior to the licensed decision?

DR. BROCOUM: Yes, in the simple word. Also, obviously this is a strategy in development. I said earlier we're looking at ways to accelerate the thermal testing to get more thermal testing earlier. There's a variety of opinion as to what you can do with just a little bit of thermal testing and can you eliminate or confirm or corroborate some hypotheses. At the other end of the spectrum, we're questioning whether we're going to lock in at a close final thermal load in 2008 or whether we ought to keep our options open in the performance confirmation period. We're going to get decades of data before we close up. In fact, when you close a repository is when you'll know the most about the phenomena. You'll have 100 years or more information. So we're looking at all that and we're trying to keep our design options flexible to be able to accommodate whatever direction we decided to go. Again, if you remember, the original proposal on Scenario A was not to make the final thermal load decision until after you began operating the repository. So it's kind of coming back more to our original concept.

DR. CORDING: But the loading that you do go forward with, you would be going forward with a specific thermal loading plan at licensing that would have a specific value and you would have enough area, you would have evaluated enough of the site to say that you can store the required tonnage of waste?

DR. BROCOUM: It will be a balance of thermal loading, area, characterization and confidence if you don't have all the information you have now and detailed plans to get the rest of the information, I think is the fairest way to say it. To actually state that we would have all the characterization done for the lowest possible case of thermal loading by license application may not be achievable, so I couldn't make that statement right now. So I think it's a balance, and it's detailed plans to get the additional information through the performance confirmation period.

DR. CORDING: One more, just one more quick one.

DR. DOMENICO: Pat Domenico. Is the paper that Tom referred to that suggested low thermal loading for site suitability, is that paper the same as the White Paper that you referred to?

DR. BROCOUM: It's the White Paper that was reduced by the M&O. I don't remember the date now, but it was about a month ago.

DR. DOMENICO: So they are indeed the same paper?

DR. BROCOUM: Same paper.

MR. GEER: Yes, they are.

DR. DOMENICO: Well, this is kind of strange, because out of Tom's presentation you did say that site suitability will employ a low load, and I think what you just said that's not true anymore.

DR. BROCOUM: Exactly, because we've been reviewing the paper, we've been debating it.

DR. DOMENICO: Okay.

DR. BROCOUM: Various people have had various reactions. My own person reaction to the paper, since I'm speaking for myself, I had several reactions. One, I was most worried about the case we would make in 2001. That was my own personal reaction. I also was worried in the paper because it seemed to imply--the way the paper was written, it didn't give you a status of knowledge. It seemed that we have to learn everything in the future. And finally, it was making decisions--in other words, it was saying we're going to have to go low here and so on here in advance, when we have the information. So those are the kind of things that I had questions about, but I still think that it was a very valuable paper, because it got all the people talking about it. Other people had different concerns. Some of the people had a lot of concern over the questions you are addressing on making the case low for site suitability. That was a major concern within the program. So that led to a lot of rethinking and some of the comments that I made today.

DR. CORDING: Okay, thank you very much. I think we'll proceed now with Tom Statton's presentation.

MR. STATTON: Steve, I thought, did a wonderful job of laying out the gauntlet, and perhaps the title of my paper isn't as revealing as it could be in that what we really want to do, I think, is see how and what the testing program is doing in stepping up to the schedule of events that Steve's laid out for us and indeed grow out of the program. And I think at first blush one wants to step back and say, "Where does this fit in the system?" and the answer is, we are to arrive at a 2001 date with a conceptual model of behavior of the site nominally tied, as I guess Pat was heading to, with performance assessment. But a conceptual model of behavior of that site that says it performs acceptably. And then take the testing program and map it through as an overlay on that process to see where the underpinnings of the various pieces of that understanding when they come into the system and how robust they are as a function of time, recognizing that for waste emplacement for the entirety of this mountain, we will not be able to conduct a demonstration test of that kind of scale, and hence our testing program will be tests of nominally smaller scale events.

By the way, I'm Tom Statton.

Starting off with what our goals are, nominally that's what I was trying to start with, is that our goals are in fact to provide the observational underpinnings of this conceptual behavioral model that we have that spans however many square miles we end up emplacing waste in. Recognizing that it is models that will indeed describe that behavior, and clearly, as in all things, those models become numerical models and computer codes and the like, with recognition that the testing program, per se, will not be able to test because of our lack of ability to deal with the demonstration scale issues. Our testing program, per se, will not be able to be in and of itself a discriminator that says the litmus paper says this is a high loading phenomena or a low loading phenomena.

One of the things in the testing program as it currently exists is that it nominally is trying to demonstrate behavior within the range of expected repository behavior ranges. Which is to say it is not a test program designed to describe the failure envelopes of various behavioral patterns, but in fact addresses itself to the operational range of behaviors that one expects to see. Clearly a broader and longer term testing program could try to define the various failure envelopes, but that is not where we are today.

Now, what I wanted to do is in sort of giving you this overlay of what the testing program is doing is, I guess to start, recognize that at first blush we have the underpinnings of an operational or test planning basis, and that was in a report that was prepared by Sandia as the head of a committee of various program participants. And that report, I think, is either out or very close to being out in terms of being published by the Department. I know that in talking to the staff that paper has been looked at through its preparation, and I don't know that it's been published yet in its final glossy form, but nonetheless, I think everyone's seen it. And that's clearly the first blush of an operational nugget of how to take a strategy and map it through testing.

In that paper and in our thinking, we have various test types. And the purpose of this is not to debate the fidelity of the absolute ranges or the absolute temperature values or the absolute number of days in duration, but it's to put into perspective the scale of what it is we're trying to accomplish. Today, we have laboratory tests, and nominally the scale of those tests is small. The temperature ranges have been broad in that we can deal with low and very high temperature ranges, including those maximum temperature ranges above those that we envision any operational state to be. And the time term for those is easy for us because it's contained within our laboratory.

As you've seen in that paper--and if you haven't seen it, I'm sure you will be seeing it--we described some small scale tests, and I think they're probably best characterized by the single element heater tests talked about in that paper. We talked about the large block test, which has sort of been on the boards for some time. We've talked about that, it is our current plan to in fact turn the heaters on in that test during the first part of calendar '96 such that we can march through and make a bunch of the observations of both thermal conductance, water movement, fractures, etc.

Intermediate scale tests are described in that paper, and nominally this is to let you know that they are significantly larger than these small scale, single element heater tests. And then clearly there's a large drift scale test, which I think approaches at least a demonstration test in the sense of certainly thermomechanical behavior of a specific volume around a drift that would certainly provide demonstrable underpinnings, one might say, to the preclosure behavior of the rock volume right around a heat source. And then the performance monitoring issue, where in fact we need to watch the response of rock to a very large heat source as heat is introduced into the mountain.

A schedule, and this schedule is intended to be, and I believe is, wholly compatible with the schedule in the first blush of this Sandia paper coming out for the various test elements of that program. Nominally, in terms of their scale, again with some reference back to the upper bound temperature ranges that are to be achieved, when those tests are to begin and how they are to travel through time.

Now, one of the things I wanted to sort of do, Steve laid out the gauntlet of where we are headed, and we have significant milestones within that, clearly 1998, clearly 2001, clearly the introduction of this LA update. We've got sort of two problems. That's a nice punctuation in terms of schedule, and yet in the intellectual sense we are in a continuum, where we begin to learn things through time, and what happens as they get portrayed on a viewgraph is they appear to start and stop, per se, when in fact that's not true, and we'll see as we get moving through the rest of them. But here is nominally the tests as they are laid out in time and those tests that are in fact to provide the underpinnings of this conceptual model as we lead ourselves to a license application.

I'm not sure this should be next or should be after we go through a suite of other charts, but at the time of TSS, to get back to Steve's punctuation marks in our process here, we're looking at not only preliminary process models, and as Tom alluded to, they will be based on not only our laboratory tests, some of the small scale in situ tests should begin to provide some information for us, we'll clearly extract from larger scale tests what one has been able to achieve to date, but we will have our empirical analyses, numerical simulations and analog studies. For example, information coming out of things that we're currently looking at, for example, down in New Zealand.

By license application, our intent is to have a significant refinement of those process models, and they will be based on observations from larger scale in situ tests underway and providing information out of the ESF.

By the LA update, which happens ahead of waste emplacement, the concept here is that we will have, at least in the preclosure sense, some large-scale demonstration, nearly emplacement scale demonstration tests, available to say that in the very near-field around my specific opening, under the heat load that I expect to be at the limits of what I intend to put in the mountain, behavior is in fact demonstrated to be compliant with this conceptual model that I came to licensing with that told you we had a high fidelity behavior pattern. And clearly the confirmation of some of the longer term phenomena, where we're looking at some coupled processes that deal with not only thermal mechanical but hydrological and geochemical processes, will be underway and we should be provided, at least in a much broader sample size, the underpinnings of information that indeed confirm the behavioral phenomena that we have gone to licensing with.

The next few viewgraphs were not to be--yours aren't in color. What I was trying to get at was a bit of the dichotomy between these punctuation marks that Steve alluded to as we go through, and in fact more of a continuum basis, which is our increasing knowledge basis, a function of time. So even though the computer lines up a left-hand margin, it is not that we are not going to know anything until 1998 regarding refinement of our mathematical models, but nominally those are our status of information as we travel through time. What we've done here in these is tried to break out in terms of the various model components of this behavioral model of the mountain the bits of information that we sort of expect to have in hand and as they travel through time. For example, the distribution of stress as a function of heat load is something that we can in fact learn something about early on, whereas--this was probably not a good example to do this with--but as we see in some of the following ones that there are different things that get learned from different scale tests as we march through time. And that was nominally here trying to tell us what our processes are and how they progress through time and the information sources or data sources for those functions, whereas here will be with laboratory tests and the large block test and the larger scale tests will provide information later in the system.

This one might have been a little better one to start with that example on. But here, with the idea of looking that we will have conduction based understandings with a convective overprint, where we'll begin to understand better convection processes as a function of larger scale tests and longer time tests in terms of the refinement of our heat transfer model as we look at mountain behavior as a function of both tests conducted and knowledge gained.

This trying to address itself to the movement of moisture in the mountain looking at sort of the underpinning observation that these in situ tests we can indeed validate or verify that in fact dryout zones are produced, what the roles of models are, when we will have both an understanding, conceptual understanding, of the roles of fractures and when we will have more modelable or discrete fracture flow models operative in the system. And again, here are the intended data sources, and I think you will find that they map not only back to this first blush of a paper by Sandia but they will map very clearly into an update of that that looks farther out into the longer term testing scheme.

And the last of these being the sort of the geochemical process models. Some of our work in addressing the coupled processes with geochemistry in fact take a little longer. That's a system that's a little more difficult to overdrive and believe that the phenomenology that one observes in fact is not a function of the driving rate but is in fact a function of reality. That's why these, I think, take a little bit longer to gain some understanding of that, and quite clearly, true good understanding of that is going to happen in the post 2001 time frame.

In terms of the coupled process models, what we were trying to convey here is that the early part of this testing program in fact focuses on--I don't want this one here, pretend it's not--what it focuses on is sort of paired relationships and how that is provided through the testing program as a function of time. And in fact that those paired relationships get to a full coupled process understanding only as a function of time and only as a function of preparing these building blocks that progressively confirm the behavior being predicted in our conceptual model as its being built, nominally leading to 2001. This is clearly recognition that some of that full coupling and the understanding or demonstration of that full coupling process is in fact not going to be available probably until after 2001, primarily because of time and scale.

Everybody needs a summary slide, so I guess the way you get a summary slide is you go back and say you just concluded you did what you said you were going to do. Nominally, this is the laying out of that program again within the confines of the punctuation that Steve has laid out as the challenge for the testing program, trying to capture what it is that will be well demonstrated by the various time frames of TSS, of license application, and indeed the LA update. And in fact, what it should be is a fourth bullet here that says as a function of performance confirmation, continued validation at larger and larger scales of this coupled process phenomena will be checked back to the predictive behavior that we go into licensing with.

DR. CORDING: Okay, thank you, Tom.

Board, questions?

(No response.)

DR. CORDING: Tom, one question, on these tests you describe the in situ small-scale tests and then intermediate scale tests. Could you briefly describe what those are, where they would be conducted?

MR. STATTON: Probably not. I don't know whether that means I can't--

DR. CORDING: Not briefly or not--

MR. STATTON: --describe them or not briefly. At present, the test location as described in the paper that's out at present envisions running those tests just after one makes the turn down into the repository block, sitting nominally at the higher sections of the repository horizon. That quite clearly is not the location as initially envisioned in the program baseline. So it is a proposal by a testing community as a mechanism to get started early enough. There are a variety of other things going on at present that would allow alternate locations to where that would be run to be perhaps distributed throughout the full thickness of the repository horizon as currently envisioned crossing the block.

DR. CORDING: The small tests, are those individual borehole type tests? Is that right?

MR. STATTON: Yes, and probably for a true, good definition I'd have Larry Costin describe that. But nominally they are single-element heater tests looking at heating a limited volume of rock around them, and then looking at the behavior in terms of driving water out, utilization of fractures, progress of a drying front, mechanical behaviors of induced stress as a function of thermal load. What did I miss, Larry?

(No audible response.)

MR. STATTON: But yes, and those are not only small scale enough that their absolute distribution across the mountain in fact is more readily obtained than some of the larger scale tests where we're looking at a full drift.

DR. CORDING: And the intermediate scale test, it's still a drift test, is that correct?

MR. STATTON: The intermediate scale test described in the paper at present looks at a heated source using multiple single elements, if one can envision that, to heat a larger volume or area, and it looks at trying to control some of the boundary conditions. For example, on a boundary taking a guard heater element that nominally mitigates some of the effect of heat sink on an edge to make it look as if we would have a larger area, one to look at stress variation as a function of temperature, etc.

Are we on the scale page? No.

DR. CORDING: I was just looking at you've described it on several pages. And then the large scale would be the--the drift scale starts when you start the large scale, is that right?

MR. STATTON: Yes. And not only the drift scale is to the degree we can to provide at least a demonstration scale test for those phenomena that we will be able to demonstrate. And clearly, in the thermomechanical sense, and given an understanding of our opening size, one ought to be able to provide a demonstration scale test that at least mechanically we understand and have shown that heat effects are as predicted.

DR. CORDING: The report by Sandia, is that the report that's coming out?

MR. STATTON: Yes, the report that's coming out in fact, I think, has been adopted by the Department, will be produced by the Department.

Yes, ma'am? I think Susan can address that.

MS. JONES: Susan Jones, DOE. The report we're referring to, as Tom indicated, was prepared by Sandia with the input from Livermore, Berkeley, USGS, performance assessment, engineering, and so on. It's gone through the DOE's internal management, technical and quality assurance reviews. The only thing left is a programmatic thing we have to do called a patent and classification review. This is the first time you'll ever hear me say that the probability is zero that it will be stopped for that reason. So we've sent it to the printer and it should be available within the next couple of weeks.

DR. CORDING: Okay, thank you. Yes, Don Langmuir?

DR. LANGMUIR: Couple of questions, Tom. Looking at the test types, I think as a geochemist and hydrologist I know that one of my biggest concerns is the coupled effects thing, which doesn't appear as having been adequately evaluated obviously for some time into the future. Looking at the test types, a couple of questions related to that test type overhead, which is number 3 on your list of things, of overheads. The very high temperatures, max rock temperatures listed there, the 250's, 200's, suggest to me you're talking about rock in contact with waste packages, and I wonder if that's a realistic assumption to begin with if we're going to put these things presumably in a void space.

MR. STATTON: Okay.

DR. LANGMUIR: How would this fit into reality?

MR. STATTON: I think that as one looks at operational limits and the operational limits--I don't want to make a direct translation to my MDTL, but nominally the upper bound condition under which we will operate. The calculations that are available to us today, and Steve alluded to them in his presentation, give us a rock temperature, rock wall temperature, of 200 degrees as a constraint. So nominally these 200 degrees are looking at that operational range constraint for those waste packages. I think that is, number one, consistent with the concept of the MPC and the amount of waste enclosed and it as a single-point heat source.

DR. LANGMUIR: Okay. Another question. The bottom line presumably is going to be the behavior fractures in the repository as conduits for heat, fluid, flow, and so on, and condensation. Looking at the test list you've got, I could see that it's going to be a long test that evaluates this as a phenomenon, I would--

MR. STATTON: That's correct.

DR. LANGMUIR: --assume. You've got small scale in situ tests one to eight years, but only one to two cubic meter blocks, which I would presume you'd have trouble finding a fracture in to study. Looking at other long-term tests, the large scale drift is about the first time you really have, this, right, that you're looking at this phenomena, and then performance monitoring? It's the last two things on your page; those are the two kind of tests in which you could look at condensation, refluxion, coupled effects with geochemistry, hydrology and mechanical?

MR. STATTON: That may or may not be correct. I think it is the conceptual intent that the intermediate scale test capture the role and behavior of fractures in both the driving and returning of water if indeed that happens. I should sort of premise where we are now that we've set this paper up and everybody's going to go order one. The paper lays out a conceptual testing program and sort of says, these are the things intended to be addressed by that program. At the time that paper was produced, all of the forward calculations required to say no, it's not really 25 meters, it's really 35 meters, or whatever, have not been completed. Clearly they are in the throes of being developed today, because these tests, if we are going to pull them forward in time to begin them quickly as ESF progresses, those forward calculations need to be made. And clearly it is the intent of the scientific programs of Yucca Mountain to do a forward calculation not only prior to design but prior to implementation of the tests such that we can then map results back to the outline that we've identified prior to testing, which is to say I expect in three months to see this zone of a test region dried out, I expect to see these kind of temperatures obtained, I expect to see water moving through fractures, or whatever.

DR. LANGMUIR: I have one other. Earlier we'd been talking about or heard about other sorts of input information on thermal effects unrelated to these tests. These have included, I presume, going back and looking at the G-Tunnel system and what data has been made available there, perhaps rethinking about it.

MR. STATTON: Yes.

DR. LANGMUIR: Another which is not integrated at all in any of this is how the New Zealand analog work will integrate and provide insights that help you reach these goals and where that comes in and how it comes in.

MR. STATTON: Okay. Where do I want to start? The G-Tunnel relook, if that's a way to characterize that, I think is in the throes of being conducted at present. There are people back looking at that information to try to help derive a test program. If I were to address myself to--boy, am I glad you're here, Will--the New Zealand work, I would probably have somebody like Will Clark or maybe Dale Wilder talk about that input. Don't jump up all at once, guys.

DR. CORDING: Okay, Will Clark.

MR. CLARK: Well, Don and I had this discussion, and I think the word "boondoggle" came up last night in the bar.

MR. STATTON: Thanks, Will.

MR. CLARK: So I was a little hesitant at this point. We have just put out a draft report. It's one of the better reports that I've seen come out of our organization in a long time because it's brief but very clear, concise, as to what we are doing in New Zealand, why New Zealand was picked over actually even more attractive sites around the world, and what we hope to--and why, by the way, we're not in Calistoga, right above Livermore, at the geysers up there, which by the way they would not let us into, and we did try. At New Zealand, we have an area that's been opened up to us. It occupies a range of liquid environments all the way from pH's in the 12 to 13 range down to 2. We also have access to 40 years of test data that they have developed, and we have a Hathi scientist assigned to us full-time. All this for 55K a year. What we do is we go down and we do geochemical studies over this whole range of environments. We also brought in metal samples of all the candidate alloys in this range of environments. We also now have gotten some excellent information from the concrete. The concrete cooling towers there have degraded an inch in the last five years due to microbiological activity. We've learned just here recently that the microbes have a unique ability to use synergism, which we're trying to do in this program, and that is they work together to change their pH from 12's to neutral to 2. And in doing so, things that should be innocuous are not over a period of time. All that is available to us in New Zealand.

Now, most of this work was started under the international program. We are now picking it up under the Altered Zone work and Tom and Susan Jones, of course, in the DOE. And in that, we send two technicians twice a year to collect the sample and the data that's available at that point, bring it back, and what we are doing is looking at the processes. Not at the data so much itself, but what are the processes involved in geochemical changes as a function of temperature, pH, and time over this whole range of environments that we're allowed to look at. And so that data is just now starting to become available. We completed the study plan which was required before we could actually legally start doing work on the program independent of the international effort, which the international effort is mainly devoted to the concrete work that's done there. So we have just started collecting that information and we are starting to put it out in reports now.

DR. CORDING: Thank you very much. John Cantlon?

DR. CANTLON: On your illustration number 4, your overhead number 4, I'd like to get you to talk about the locations of particularly the in situ tests that you referred to in 3, small-scale in situ tests. I presume in situ really means at repository level.

MR. STATTON: That's correct.

DR. CANTLON: And you have some alcove heating experiments starting up well above the repository level. Could you sort of place these tests physically? Where are they in the system? Because it looks like you've got them in here quicker than it looks like the TBM is going to be there.

MR. STATTON: All right, this schedule was optimized on, and again, taken off the overlay of the ESF advancement schedule.

DR. CANTLON: Yes.

MR. STATTON: And what it said was, as quickly as one gets into repository horizon rock, stop and run a test. Now, that didn't necessarily for those who are in the construction business, say stop construction, but it said testers get going, get your tests started immediately. Yes, some of the single element tests could very easily sit into a small alcove. The only remaining coordination function is, to do that, one is beginning to encroach upon repository real estate. Well, as Steve indicated this morning, repository real estate is of concern to us, hence what's back in the system is an evaluation of recognition of the time requirements to get this testing program going. But in deference to the space requirements, is there another way to within nominally the same time frame find a more acceptable place still in the repository horizon rocks to run those tests? So if you look in the Sandia paper, or this soon-to-be-out paper, one will find as you've headed down the ESF, turned left, there's a segment of ground right there that was sort of laid out by the testing community saying, "This is an acceptable place for us to run those." I think the systems approach says, "Let's make sure we optimize the place as well as the time." And consequently, I'd say that's probably not cast in concrete, stone, wherever we're headed to exactly field those, but clearly in the time frame we're trying to get that cast.

DR. CORDING: Okay, one other question, Tom. The process has been one, I think, of integrating the thermomechanical type studies that Sandia proposed a while back along with Lawrence Livermore thermohydrological type studies. Has that occurred, is that what we're going to see in the White Paper, that this has been put together and focus the paper on--

MR. STATTON: On one testing program.

DR. CORDING: --primary site suitability issues?

MR. STATTON: Yes. Where we are in that is the first blush of this, in the paper that's being produced, is more robust in the thermomechanical sense, primarily because in the nearer term those are the phenomena that are a little easier to observe. There will be an update of that paper which will include the entirety of the testing program. In other words, looking a little more into the postclosure issues than the preclosure issues which tend to get wrapped a little closer around the thermomechanical behavioral phenomena. The answer is, there is one testing program. This paper was the first bloom of trying to say what that looks like, and it talks about a test that deals with thermomechanical properties as well as geochemical changes and/or water movement. The update of this paper will take the program probably through its performance confirmation time, at least as we can today think of it, probably not in the emplacement monitoring sense. But it is one testing program, it is combined, it is a heater that serves both the thermomechanical testing requirements as well as the hydrological testing requirements.

DR. CORDING: The initiation of the hydrologic certainly has the strong priority for suitability type--

MR. STATTON: Yes.

DR. CORDING: --issues.

MR. STATTON: But part of this was to just get the overlay to see when it is possible and what information will be coming out, and we may have to look at thermohydrological coupling as not a fully coupled phenomena along with geochemistry changes. So we may be focusing some of these tests to look at water movement without necessarily being as sensitive as one would want to to geochemical changes. Because to get water movement, one may want to overdrive the system a little to initiate that phenomena, which in fact compromises, potentially, the observations we need to have in the geochemistry regime.

DR. CORDING: Okay, thank you. We're a little ahead of schedule here. I think what I would suggest is we break for lunch and meet back here at 1:30 instead of 1:45. If you would do that, we'll have our lunch break and continue with our discussion. Then we'll probably start a little bit early on the Calico Hills System Study as well.

(Whereupon, a lunch break was taken.)

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

DR. CORDING: If you would also rejoin us at the table, thank you. We're ready to start at this point. I did want to also comment that the session this afternoon is going to have some continued discussion between the Board and the speakers on the thermal management, and then we'll be going to the Calico Hills System Study with Richard Memory, who will be presenting that. Following that we'll have a presentation on '96 budget by Steve Brocoum, and then we'll have a time later in the session for public questions and comment.

And we've set that time aside specifically for the public comment, and we would appreciate it if you would sign the public comment register sheet at the sign-in table at the back of the room with Ms. Einersen and Ms. Hiatt. They're back in the back there at the desk. And when you do make comments, then please, if you would, use a microphone, identify yourself and your affiliation. That will be occurring at 3:55 p.m., and we are asking people to limit their time to five minutes maximum for those presentations.

Then if we would please now continue, we had been discussing the thermal management strategy. I think we had a few more comments from Board and staff in regard to the thermal testing program, and we'd like to ask the Board and staff if they have any other questions to provide at this point on the thermal testing.

Russ McFarland, did you have some comments on that?

MR. MCFARLAND: There was a question that I was hoping sometime to put to Steve. I don't know if now is appropriate. Steve, you made the comment earlier that some "site characterization" would be done on the expansion area prior to--I don't know if it was '98, but definitely prior to 2001. Could you define what this site characterization would consist of? We know, for example, it's not going to be drifting. What would you consider to be a meaningful minimum set of information to consider those sites suitable?

DR. BROCOUM: In our planning for '96 and out, we're starting to think about expansion areas, okay. I don't want to be too definitive, because we're going to basically ask the M&O to come back and tell us what they think. When I talk to you about my next talk on budget and planning, I'll be talking about the new relation between us and the M&O and how they integrate the program. But we certainly want to know if there's enough host rock, and so we want to use techniques like geophysics, boreholes, that kind of stuff. That's what we would envision, those kinds of techniques, and maybe drifting later. But we haven't really planned it out, so it's kind of a little premature for me to give you clear-cut answer.

MR. MCFARLAND: In 2000, you'll be declaring the site suitable, but yet the site will be undefined in terms of whether it's the basic footprint, the 1,200 acres or maybe even 3,000 acres. How do you address that question?

DR. BROCOUM: Well, I mean, we will have a lot of information, not only on the footprint itself, but we will have already a lot of information outside the exact repository block, a lot of boreholes, for example, and a lot of geophysical lines. So I think we'll have to, as we're getting near to the license application, and as we're getting a better understanding where we're going to come in on thermal loading, we will be making those decisions. In terms of the expansion areas, there is a design paper, an M&O design paper, that talks about expansion areas that was issued in August of '94, I think. Yes, I think it was August of '94. So there is a paper that talks about the amount of expansion areas versus the MTU loading you would do. But our plans aren't crystallized yet, so I can't really give you a more attentive answer.

MR. MCFARLAND: Thank you, Steve.

DR. CORDING: Okay, any other questions on the previous session?

MR. MCFARLAND: Ed, may I have one of Tom?

DR. CORDING: Yes.

MR. MCFARLAND: I wouldn't pass the opportunity up, Tom Statton. Tom, you discussed in some detail the test planning, particularly the thermal test plan that was laid out, and some prioritization was done, I would assume, in determining tests that are most important, tests that are least important. Some tests that within the whole hierarchy of testing were kind of put off and we would do these later, some were identified as very important and we will do these first. Priorities are usually established by a strategy, in this case a thermal management strategy. The program hasn't yet evolved a clearly defined thermal management strategy. How did you do this prioritization?

MR. STATTON: You give me too much credit. For openers, in the phenomenology sense, if we were to go back to Steve's presentation and a presentation even farther back, probably to you in Beattie, one would find that on the site performance sense, prioritization seems to have done itself to the point that we care a great deal about flux and we care a great deal about transport paths. Consequently, that tells one to begin to focus on, in terms of prioritization of thermal testing, the effects of heat on that water exchange, that water process. So, given that, one would say, "Well, then clearly the first test I want to go run is one that heats up water, moves water, and lets me take a look at that." In a sense, if there were a test that were the single test that looked at that, that clearly would probably be the first test we'd run.

But in fact what we're after is a phenomenology, and it's a phenomenology that needs some developing and understanding as a function of time. It needs access, it needs scale, and it needs time. In the testing program, as we begin to accommodate the lack of time in our equations--in other words, trying to make ten years look like one year, try to make 10,000 years look like ten years--there are other compromises in the system. Clearly one of those compromises, I think, finds its way home to the geochemistry program. As we begin to overdrive some things, other things don't happen or happen incorrectly. As we look at in situ testing, we're clearly not going to get underground to the point we have access to where one needs to test for, nominally, another year or so, somewhere in there--I don't want to argue about whether it's nine months or fifteen months. But I have a time gap to get there, I have a test that needs more than a month, more than two months, to take a look at this phenomenology of water movement. In the smaller scale, quicker tests, we're trying to take surrogates of that water movement and say it translates to the larger problem of water movement.

In the sequencing of events, it's a mechanical sequencing. It's a timing, not a prioritization of thermomechanical phenomena over a thermohydrological phenomena. It's simply a more complicated test, takes a little longer to set up, wants to be a larger scale to be translatable to the things we're after. So a theme that underlies this testing strategy is in fact focused very clearly on trying to get thermohydrologic properties, because today that appears, coming out of a presentation I think Jean made to you, to relate most to our flux issue. The fact that it is not the first test started simply says it takes longer to set up. The fact that it's not the first results coming back simply says, "I can't compress time enough to accommodate the volume over which I want to make an observation.

Did that help?

MR. MCFARLAND: It helped. Thank you.

DR. CORDING: Okay, thank you very much. Let's now proceed, then, to our next presentation, and that's the Calico Hills System Study. Richard Memory, who is manager of systems analysis and modeling with the M&O, will be presenting this topic on the Calico Hills, which is one of the barriers in the unsaturated zone below the repository level.

DR. BROCOUM: I was going to make a few comments on that.

DR. CORDING: Yes, Steve Brocoum.

DR. BROCOUM: I was going to make a few background comments. For those that have been around the program for a while, you might remember when we issued the draft SCP in 1987, we got several objections from the NRC, and one of them we had limited access at that time to the Calico Hills and they objected that we weren't doing adequate--I don't remember the exact words of the objection--characterization of the--we had to characterize Calico Hills, but they didn't tell us how. So to lift the objection, we committed to do a Risk/Benefit Analysis in the SCP that came out in 1988. That analysis was published in January of '91. We briefed the TRB at the time, I think it was March of '91. We looked at options for Calico Hills characterization, we used a multi-attribute utility approach, and that favored options that called for extensive drifting across the repository. That was a fairly complex study, as you might recall.

Now we're several years later, we have a new proposed Program Approach, so we decided that we ought to reevaluate the kind of information we need and different access options and to see how the various access options would satisfy our data needs. This study, which we asked the systems people to do, was to provide us the information to help us make the decision, and it wasn't study design to make the decision. So it's a study whose input will be used in the decision-making.

Thank you.

MR. MEMORY: Okay, thank you. So my name is Rick Memory, and I'm the manager of the systems analysis and modeling group within the systems engineering organization for the M&O. And I'll give you basically what is a status report on where the Calico Hills Study exists now. And as Steve just pointed out, it's scheduled for completion the end of May.

This is a list of the topics I intend to address.

So, first, the purpose of this study is to provide and evaluate some options for when, where, and how we might access Calico Hills either through boreholes and/or drifting. Our approach has been to initially develop a logical process for making the Calico Hills access decision. Secondly, identify Calico Hills' potential data needs based on suitability and licensing requirements. And then thirdly, given those data needs, develop methods of access and evaluate those methods against how well they provide the needed data, cost and schedule implications, test interference, and potential risk to site performance.

So the first thing I'll go over real briefly is this decision process that we've developed. And again, the purpose of this was to provide a logical process for making the Calico Hills access decision. And our assumption in this is that the waste isolation strategy does in fact utilize a defense-in-depth philosophy. I need to use a second screen here. This is not intended to be readable, but more to give you a glance of what the structure of the overall logic process looks like. And what I'll show here, without briefing each particular box, is basically I have a blowup of this section, and then I have another page that shows this section, a third page that shows that section. Even with this, this process then continues out past here. What we have on this is the Technical Site Suitability milestone.

So let me come back to this screen. What this process does is initially we ask a couple questions and provide answers to them relative to whether or not our current understanding of Calico Hills and its role in the groundwater travel time and the release standard is sufficient, and based on our preliminary site investigations, performance assessment calculations and licensing strategy. Well, actually, the question is, is there great uncertainty that does not allow us to move forward? If the answer is yes, then we look at ways right now on how to enhance our Calico Hills access prior to Technical Site Suitability. If the answer is no, that we're okay, then we come down and ask a question as to whether or not the Ghost Dance Fault needs to be accessed at the Calico Hills level prior to the ESF accesses. And that's done based on test interference and licensing strategy. If the answer is yes to that, then once again we go forward to developing a Calico Hills access plan. And what I'll get into in more detail later is this phase, which is developing pre-conceptual Calico Hills design approaches, and I'll actually go through these steps, the four boxes I've shown here.

So now what we've done is progressed along the logic chain to this point here. So what we're doing at this point, then, is continuing with the ESF construction, we're continuing with the borehole explorations as planned, we're updating our models to reflect what we're finding in the Calico Hills--I mean, through the borehole exploration and the ESF exploration, and we move forward to once we're in the ESF, we ask the question, are we finding water either in the ESF drift or do we find water in the Ghost Dance Fault, either the first or second access? If the answer is no, then we continue on our logic process along the dry path. If the answer is yes, then what we do is we update our models to reflect what we're finding in the ESF, and then the process models, and then update the TSPA and groundwater travel time calculations. Then we move forward in the logic chain.

And then on the third panel, we then ask the question, once we've updated the models and seen new results, are the TSPA and groundwater travel time performance calculations adequate for Technical Site Suitability? And that's based on the PA analysis and our licensing strategy.

If the answer is no, then we go down and look at how we might reallocate the performance to greater allocation to the Calico Hills barriers or to other barriers. If the answer is yes, then we can proceed with the high-level findings and go on and get past the Technical Site Suitability milestone without accessing Calico Hills. And then this logic continues, then, for the license application and then on out to the license update. This is provided just to give a feel for this decision process that's been developed.

So the next phase here is to identify the potential data needs. Our goal here, as I said, was to identify and prioritize the data needs in order to support suitability evaluation and licensing needs. Our approach was to utilize what we're calling conditional failure modes as in intermediate step to identify the data needs. Definition of a conditional failure mode is that it's a feature, condition, or property that could degrade the ability of the Calico Hills unit to adequately function as a geologic barrier. It's important to understand that this does not equate to a disqualifying condition in any sense. This was developed as an artifice to allow us to take an intermediate step. Instead of just jumping from the site suitability requirements down to data needs, it's an intermediate step allowing us to identify those data needs, and that's what the third bullet says.

The next chart, then, lists the six conditional failure modes that were identified. These are things, again, that have the potential for degrading the expected behavior of the Calico Hills unit. So they constitute the preferential flow and transport pathways that might exist through fractures and faults. We could get inadequate physical retardation, either through the lack of matrix diffusion or imbibition into the rock matrix. There may be inadequate retardation potential from a geochemical sense. We may get preferential flow and transport pathways that go through the rock matrix as opposed to the fractures and faults. There may be the potential for lateral diversion of the groundwater above the Calico Hills unit. And then finally, there may be a repository-induced alteration of the Calico Hills properties, such as thermal and chemical effects. So those are the conditional modes that we identified.

Then for each one of these conditional failure modes there is a set of data needs or data observations that we say we could then make that would help us identify if these sorts of things exist. And rather than going through all of those for all six, we'll just show you a single example of that. So this is an example of the potential data needs that are associated with one of those conditional failure modes, in particular the failure mode related to the inadequate physical retardation through matrix diffusion or imbibition into the rock matrix. So the data needs associated to that would be the rock-matrix hydrologic properties, rock-matrix hydrologic conditions, such as measures of in situ saturation and water potential, and then finally the interaction between the fractures and the matrix. And as I say, for each of the conditional failure modes we developed a set of these data needs. And I'll get back to that later as to how that helped us identify some potential access modes.

What I want to talk about now is just give a sample of some of the performance assessment work that has been done. The purpose here was to evaluate the impact of these conditional failure modes and related property uncertainties on the overall system performance. So we did that via sensitivity studies on TSPA and groundwater travel time. And the measures of performance that we've looked at are that the groundwater travel time as well as 10,000- and 100,000-year cumulative release and a 10,000- and million-year peak individual dose. For this presentation, these have all been completed at this point. For this presentation, I just want to give some sample results out of the 10,000-year cumulative release work that was done.

The first thing that I'd like to discuss is the matrix flow as the analysis condition. The basic TSPA model is the TSPA-1993 case. Versus the TSPA-93 base case, we looked at three conditions of the Calico Hills unit. One we're calling a Good Calico Hills, which is basically the 90th percentile values for the activity. We have high K_d's and high porosity, and we have Average, which is the 50th percentile case, and in that case we're using median K_d's and porosity, and then finally the Poor case is where we're doing the 10th percentile, low K_d's and porosity. And as another analysis condition, we're not considering the release of Carbon 14 in this case, because the gaseous Carbon 14 is not expected to be a player in the Calico Hills unit. The conditional failure modes, then, that this analysis responds to is the No. 3 and No. 6, which is the inadequate retardation potential and the repository-induced alteration of the Calico Hills properties.

So what you see out of this, this is kind of the standard TSPA/CCDF, you get the probability of exceeding this release versus the total normalized release with the standard shown over here. And what we get out of this is this is the TSPA winding its way through this envelope, and you see that the Good Calico Hills parameters are here and the Poor parameters here with the Average parameters somewhere in between. Up here at the Point 1 level you get maybe an order of magnitude or two difference, down here you get a less than 1 order of magnitude difference. So there's a fair amount of uncertainty, of course, in these calculations. Whether or not you consider these differences significant is, I guess, up to the reader. But this does not appear to be terribly significant, although you do see some kind of sensitivity. Then, of course, you see the standards where over here. And this is for just considering matrix flow, assuming matrix flow through the Calico Hills.

The next example is preferential pathway failure, and this is where we're assuming we're getting fracture flow and transport and we're using the TSPA-93 base case. Now, what we're assuming here is that we're getting 90 percent of the water that flows through the mountain, through the Calico Hills unit, goes through the fractures. And then this is a list of the conditional failure modes that we're gaining information on by doing this analysis. The conditions of the individual units that failed is we have this is the base case, where we're not getting a failed unit. That's the TSPA-93 case. This is the case where we get fracture flow in the saturated zone, and then we did a case where we get fracture flow in the Topopah Springs weld and the saturated zone. And then finally we did a case where we get fracture flow all the way through. And that's the purpose of seeing what the contribution of Calico Hills is.

So then, over 10,000 years, with a 10,000-year cumulative release, we get that the TSPA is here, which had no fracture flow, and then we have the saturated zone fracture being the furthest to the left. Then we get a combination of saturated zone and Topopah Springs weld fractures. And then there is a fairly significant looking jump when we include the fracture flow through the Calico Hills unit.

This was just two examples of the sorts of things that have been looked at in the study. This summarizes basically what I just talked about in the sense that the matrix flow does not seem to be terribly significant, and it implies that we might give low priority for the potential data needs that are associated with that matrix flow. But we did see that we do get fairly poor barrier performance if we have persistent fracture or fault flow and transport. And that was shown to be an issue, even though I didn't show it here to be an issue for groundwater travel time and 10,000-year release and dose performance. So that just indicates that there is a fair amount of significance to understanding this information. And then, as I say, we're still working on the implications of several other PA analyses that have been done.

Now shifting over to looking at the access options. By the access options, we consider three combinations or three things, either the boreholes by themselves, drifting by itself, or drifting plus boreholes. And this means boreholes that are not already planned.

Now, in developing the access option, we took one more step between the data needs and the access option, and that was to identify features and attributes that if we were to go provide access to these features would give us information on the potential data needs, which would then help us understand the failure modes. So this is a list of, then, the features and attributes that were deemed to be important about the Calico Hills, and it goes with the north-south distribution of features, properties, conditions, and then east-west distribution of the same thing, understanding the Ghost Dance Fault and that the flow in the Topopah Springs basal vitrophyre, imbricate fault zone, and so forth.

Then we developed basically three classes of excavation. One is a sort of what we call a minimum excavation, where you get minimal drifting in the Calico Hills and you basically take a look at one of the faults. And then we developed a moderate case called the modified base case, which gets you moderate drifting and multiple fault accesses. And then finally extensive excavation, where you basically target all the faults within or adjacent to the repository, you get extensive north-south drifting and significant, potentially extensive east-west drifting.

Okay, this is leading toward allowing us to evaluate and select an access option, or basically to give some kind of an evaluation of those access options. So what this chart does is, in a fairly subjective manner so we could discuss and argue about whether or not a box needs two, three, one or no checks in it. Along this column are the access modes, boreholes and then the three excavation cases. And then along here are the features and attributes that might be observable by using this access option, and then how much information you might get about these things if you do this sort of excavation.

DR. CANTLON: Excuse me, what is three checks, two checks?

MR. MEMORY: Three checks is the best. That means it gives you the most information, and no check means that it doesn't give you much information at all.

So, given that, we have some examples. These are simply examples that are almost to the point of being cartoons, but examples of each of the excavation cases that I've talked about. The dashed line here is the ESF with the North Ramp extension, and then the solid line is an approach for a modified base case access. So in this case we're coming off the North Ramp, cutting across the Sundance and going over and cutting across, then, over to the Ghost Dance Fault. So that's the modified base case.

A minimum excavation example would be one that starts off the south portal, independent of the ESF, comes down into Calico Hills, and then does one cut across over to the Ghost Dance Fault. Now, the value of this is that over some perhaps other access options is that it gives you independence from the ESF south portal, yet because you're at the south portal, you're able to use the surface facilities that exist to support the ESF as opposed to perhaps drifting from some other area, either the west or a southerly access.

Then finally the extensive excavation has the same entry through this independent south ramp, but as you see, it's much more extensive, cuts across to look at the major faults, and it does some east-west as well as north-south excavation to give you information on those features.

So, given this discussion, then, and what these things might provide to you in terms of information--let me turn this chart off--the cases that the study is looking at are basically these cases and evaluating them, is to look at what do boreholes only with no drifting do. That is an access option that's being evaluated. Looking at modified base case without new boreholes, and then just various combinations. Base case with boreholes, minimum excavation with boreholes. Two things we did look at that were slightly different would be the accessing Calico Hills before we look at Topopah Springs. That was put in here to provide an ability to ask the question, get some kind of considered answer as to whether or not that is necessary, what kind of data it provides to you, compare the sorts of data these other options give. And then the other thing that's a little different is to look at these excavation options and how well they might support a longer monitoring period so that you stay in the Calico Hills unit and continue collecting data for however long the repository is open for. So that was the set of cases that are in the process of being evaluated.

Now, that pretty much gets you up to date to what we've done so far. There has been a workshop held that was used to evaluate these options. Evaluation was done for understanding the amount of increase in our scientific understanding by going to these locations. And in order to do that, used a multi-attribute assessment approach to evaluate that, and as I say, we're currently compiling those results, and these will be out, at least provided to the DOE, by the end of May of this year.

And the final chart shows how we're going to combine this scientific understanding here with the other things that are important for each of those access modes, and that is cost, the potential adverse impacts that the Calico Hills drifting may have, and how it plays in the schedule, what's doable, when. So that all needs to be done, and then given those things, we can come up with an assessment, provide information on each of the access options in terms of ranking, perhaps.

Okay, that concludes my talk.

DR. CORDING: All right, thank you. Yes, Don Langmuir?

DR. LANGMUIR: I'm a little confused, Rick, in the sense that on overhead 7 and around that overhead you give your decision aiding process flow chart suggesting, at least at that point in the talk, that you had not made a decision whether to go for the Calico Hills or not. There's a yes and a no answer to the block. You're not sure whether you're going yes or no, whether it's appropriate to go for it or not, in other words. When you get to overhead 17--I'll wait for you to put that up.

MR. MEMORY: Okay.

DR. LANGMUIR: That's the normalized release rate chart. You pointed out that if fractures were important in the Calico Hills that there's a big effect, as you could see from that plot, the far right curve is much closer to violating health standards or release standards, standards you might adopt or have, so that fractures clearly, if they exist in the Calico Hills and cross-cut it, are a serious issue in site suitability. The answer then to me when I go back to chart 7 is yes, not no. Yes, we've got to go for Calico Hills. Then, of course, as you proceeded from there--and it sounded like a story of how one goes into the Calico Hills as if you're going to do it. Where am I in this? Is it a yes on chart 7? And where are we on the logic?

And a final little query for you, what have we learned from surface-based testing that bears on the characteristics of the Calico Hills? The sense I get from what we're hearing these days is that we learned almost nothing about it, it's a mystery, and the only way we're going to learn enough about it to decide any of these characteristics of it is by the underground test work. So I've left you a bunch of things to talk about.

MR. MEMORY: Yes, let me answer the first one. The answer to your first question is that the answer has not been determined is it yes or no. This is information. There are a number of caveats that we need to put along these kinds of calculations, and I, because the study's not done, haven't been able to put the whole picture together for you. And this is our work in the M&O that we eventually will hand to the DOE to make the final determination as to whether or not that answer is yes or no, and they may very well look at this chart and say the answer is yes, we've got to go to Calico Hills.

Now, to answer your second question, I'd like to call on Dwight Hoxie. I think he's the right person to answer the question about what we've learned from surface testing. Dwight?

MR. HOXIE: All right, thank you. This is Dwight Hoxie with USGS. What did we learn from surface-based testing with regard to the Calico Hills? You know, we had a drilling program we got started back in 1981 or so where we had a lot of boreholes that actually penetrated the Calico Hills--the H series holes, the G series holes--so we have some core data from those boreholes. Unfortunately, none of that is qualified core. That is, it's not been under the QA program. So since that time, however, we have gotten back to drilling again, and so now we have a number of boreholes, UZ 14, UZ 16, and so forth, that penetrate the Calico Hills. So we now have some data that we are acquiring. Unfortunately, one of the areas where we have a lot of uncertainty has to do with the vitric zeolitic interface. We have very little data on the southern part of the block itself. So this is an area where we need more data.

So I think that the answer is, is that we have quite a bit on the northern part of the block, we need more data to the south, which we could acquire from boreholes. The other thing that we need to do is to look at this transition at the base of the Topopah Spring where the basal vitrophyre occurs, where we have perched water occurring. So this is the kind of information that we can get from boreholes that we may not be able to get from drifting, for example. So maybe that answers your question in part.

DR. LANGMUIR: What are plans for doing the borehole work that you think is needed? How does that fit into the plans for the program?

MR. HOXIE: Well, I think that at least through 1999 we do have a program of completing a number of the so-called SD, systematic drilling program boreholes. And if we could complete those in the southern part and the western part of the block, where we have very little data, that will give us the lateral east-west kind of coverage that we really need. So I mean, I think if we complete the drilling program as it's currently scheduled, and we complete the SD holes as planned, I think that we would acquire a lot more information than we currently have. So I think there is a way to get there from here, put it that way, from the surface-based program.

The one thing I might mention, though, is that we aren't going to get from core, unfortunately, vertically, and that was something that you raised as going to be good information on fracturing. We just are not going to be able to get fracture density.

DR. CORDING: Yes, Dennis Price?

DR. PRICE: Maybe I could just summarize what I think I heard and ask for you to comment on what I think I heard. I heard a sort of a progress statement, that there were no results given, some examples of some results, but the work is in progress and no data and nothing of substance and of value to the Board except an indication of what you're doing.

MR. MEMORY: Well, I guess I'll let you decide the value. You are correct in that it's a work in progress, and I think you characterized it in terms of its status correctly.

DR. PRICE: I think as a Board we would have liked to have seen some data.

DR. BROCOUM: Just make a comment on this.

DR. CORDING: Yes, Steve Brocoum?

DR. BROCOUM: When we started interfacing with, you know, the staff members on this meeting, this was one of the studies that was ongoing of several system studies, and we thought it would be good to get it in front of the Board. The original intent of this was to show you the decision logic, because that was the part that was completed by February with the first part of his presentation, so he gave you an honest status. In the dry runs that we did, he did show more examples, and some of the comments were, "No, no, we don't need all those, let's take them all out." So a lot of the examples--in fact, all of them were going to be taken out. Then the other evening we did another dry run and we decided to put some as examples in. But the study will be done in the next few months, and at that time we'll have the whole study available, and we can come back, if you like, and give you a more detailed briefing with all the examples and have many different cases. He was just talking about a single case.

DR. CORDING: That would be helpful. Yes, Board staff Carl Di Bella?

DR. DI BELLA: Thank you. This is Carl Di Bella. On your very last slide, you talked about some other attributes to the decision-making process, cost and schedule. And since this study is going to be finished within a month or so, you probably have some pieces of data on the cost and amount of time it would take to do various options. I'm wondering if you could share that with us. How long would it take to mobilize and complete, say, your southern access drifting and how much would it cost?

MR. MEMORY: Okay, maybe Bob Saunders, do you think you could address that for us?

MR. SAUNDERS: At present time, we really don't have a lot of exact information on schedules or cost. Obviously the longer the drift the more it's going to cost. We're looking at probably a smaller TBM than we're using for the main loop of the ESF, which should cost a little less. And also, in terms of how it's going to be done, it could be done somewhat independently of the main tunneling contractor now. I would say it would probably take twelve months to get mobilized. Tunnel progress would be probably much the same as it is now or a little bit faster. Some of the things, like whether we're going to work under a rigorous QA program is to be decided. I think some aspects of the tunneling would have to follow some of the same constraints that are on the present program. It's really a little early to say in terms of total cost and schedule, but that's something that we are working on now.

DR. DI BELLA: Thank you.

DR. CORDING: Thank you. Russ McFarland?

MR. MCFARLAND: Bob, if I could ask a question on that. Since an access into the Calico Hills would not be in the GROA, geologic repository operation area, would that activity have to be controlled under a rigorous QA program?

MR. SAUNDERS: There were a number of accesses we were looking at. One was, as you see, from the south side, which would be an independent access. We were also looking at an access from the North Ramp, which would be within the GROA. There's also another access that Dick Bullock had proposed from the Solitario Canyon site. There are a number of concerns. And again, there's no final decision made on these as to whether we can use a large amount of diesel equipment, whether we would have to have the same control on the ground support program. Also, how long is this going to last for? If it's going to be a 100-year program, extensive testing over the life of the repository, there needs to be some degree of control there.

MR. MCFARLAND: Question to Rick.

MR. MEMORY: Yes.

MR. MCFARLAND: You made comment to a workshop. Is that one that you're intending to hold, is that one that was held?

MR. MEMORY: That was one that was held on March 30th and 31st with the team that's been working this issue.

MR. MCFARLAND: Okay, thank you.

DR. CORDING: Yes, Leon Reiter?

DR. REITER: Leon Reiter. I have two process kinds of questions. The first one is that I assume that in this model that you have there will be judgements that have to be made. I guess you could consider them expert judgements. And this is sort of a sensitive topic to the Board about the way this has been conducted in the past. Have you sort of laid out a protocol? For instance, one thing the Board has thought it's always refreshing always to include some external people. How did you address this issue?

MR. MEMORY: I'm sorry, I didn't get the last part of your question.

DR. REITER: Well, one of the things the Board has always felt the necessity to include expert judgement kinds of evaluations, people external to the program as this example. How did you go about doing this aspect of it?

MR. MEMORY: Well, at this point, since we're not in the process of making the final decision, we're in the process of developing information to assist the DOE in making the decision. We did leave it as an M&O--well, I mean, it's broader than the M&O, it was the M&O, the USGS and all the labs that may not be a part of the M&O yet. So it was broad participation with the participants of the program. I think that it would be perhaps up to the DOE when the final decision gets made to expand the participation.

I don't know, Steve, if you want to comment on that.

DR. BROCOUM: I don't really have a comment, I just want to make one other comment, though, on the previous question, what value it has in presenting a work in progress. I can't recall the Board ever saying that, but I do know the staff have told me that the Board would be interested in seeing work in progress to get input to our work before we complete it, so this is an attempt to present work in progress. So you can have either work in progress or you can have conclusions. Here we have work in progress, and it is in progress.

DR. PRICE: Yes. Dennis Price. Appreciate your comment, and I think the Board has always wanted work in progress. Didn't mean to squelch that. But I think also, especially my colleague, Warner North, has upon many occasions said, you know, don't give us a lot of generalities, get down to specifics and give us data. And I think if there's some data thus far available, we didn't see much out of this. And evidently there were, and maybe it got squelched, and that's something we need to work out.

DR. BROCOUM: Yes, there is a lot more data. Unfortunately, we may have had a poor judgement in putting the presentation together.

DR. CORDING: One comment on the probability of exceeding the graph that you have on normalized release. You describe failures. What assumptions are made in that failure?

MR. MEMORY: Are you talking about this chart?

DR. CORDING: Is it some sort of analysis of fracture flow, or do you just assume that something passes through? How do you approach that?

MR. MEMORY: Well, the assumption is that in the unit that fails you're getting 90 percent of the water flows through fractures. Dave Sevougian is the PA.

DR. CORDING: But you have 90 percent of the--

MR. MEMORY: Of the water flow through the failed unit is through the fracture.

DR. CORDING: Is through the fractures.

MR. MEMORY: Right.

DR. CORDING: Okay. And does that have some assumptions on sorption, those sorts of things, or is it assuming that all passes?

MR. SEVOUGIAN: Dave Sevougian, Entera, M&O. Yes, there's no sorption in the fractures, it's just K_d1 and 90 percent of the volumetric percolation is through fractures with a porosity of .001. There is sorption in the matrix, for the 10 percent that goes through the matrix.

DR. CORDING: Okay, Leon Reiter?

DR. REITER: Yes, I'm sorry, I didn't get a chance to finish. About the expert judgement, I understand that the DOE has to make the decision, but in decision aiding models, I think the Board has said that it might be useful to bring in people external to the program to help the DOE make a decision.

The second point I think has to do with a little bit of history, just permit me a second here. I'm just wondering whether similar mistakes are being made as have been made in the past. You know, the Calico Hills Risk/Benefit Study has interesting history. The original version of that was what's called a value of information study. And based on that study, they concluded that there was no need to go into the Calico Hills because the value of information would not be increased and it wouldn't really affect the safety. And then I guess DOE didn't like that decision and they decided to commission this multi-attribute utility analysis, which then, besides changing the model, weighed in enough consideration that they called that scientific confidence. Essentially, that's what at that point raised the import of the Calico Hills, namely that although the repository wouldn't change very much vis-a-vis safety, you would need this confidence, this credibility, to convince people that you really had done a good job. And I'm not sure how that particular aspect fits in here.

MR. MEMORY: Well, we're aware of and fully cognizant and a lot of people who are working on this worked on the CHRBA activity as well. I guess Jerry King is our common point between those two studies.

MR. KING: And I'll volunteer to try to tackle that question. We studied CHRBA very closely, Leon, and considered to what extent we wanted to model this study after CHRBA and to what extent we wanted to differ from it. And we simply didn't have the time or the money to do a study that was as wide ranging as the CHRBA was, and frankly didn't really think that the value that came out of it was worth, perhaps, the investment. So what we did on this study was conduct a multi-attribute utility analysis only on the value to scientific understanding to Calico Hills failure loads of different options. And we're currently processing those results. And what we will get out of that is a single scaler index that's a measure of how good of a job each option does at informing us about potential Calico Hill failure modes.

We did something different from CHRBA, we also elicited from our experts a utility function for the value of partial data sets. And this is going to enable us, with a lot of assumptions and judgement, to construct a projected time history of the improvement in scientific understanding that we would receive as we execute each option. Now, we intend to take these numbers and plot them versus the cost of each option and plot them versus the time associated with each option, and from that we can get cost versus time as well. Then we leave it to DOE to make the value judgement of, you know, how much scientific understanding is worth the cost. We didn't try to, like CHRBA did, put that into the multi-utility attribute analysis explicitly and come up with a single index. We are treating the other considerations, the potential impacts on waste isolation, the potential interference from test to test, something about some assessment of the projected regulatory reception of each option--I don't know how else to express it--and presenting those results quantitatively, just descriptively. So that's going to be the final product that will be delivered to the Department of Energy. But the value judgement, do we go, do we not go, how much is scientific understanding worth, we didn't explicitly try to rank that.

DR. CORDING: Yes, Pat Domenico?

DR. DOMENICO: There's a little smoke here. Come on, you know, the Calico Hills is taken as your main barrier. You have some idea--you said something about what the standard would be, and you know damned well that you can put whatever properties you want for that barrier to see what it takes to not meet the standard. So you know just how well the Calico Hills has to perform or how bad it has to perform for you not to meet the standard. And we've been asking for somebody stressing these models to failure for three years now, but no one shows us that. Even the CDF you had up there did not show failure, it shows approaching failure. You know, no one ever wants to see it go into that black spot for some reason. So you know what kind of properties it has to have in order for it not to make the standard. I mean, because it is your main barrier and you are assigning it properties, and as far as the last time I talked to you folks, you were not assigning properties to the saturated zone retardation at all. You were not giving it any credit until you found out what credit you basically needed. In other words, what sort of standard you had to meet. So I think, you know, there's just a little smoke. I thought I'd just say there was some smoke. There was no question there.

MR. MEMORY: Okay.

DR. CORDING: Thank you, Pat, we appreciate that.

MR. KING: I'd like to respond, if nobody else does, to that. There is obscurity here, but it isn't smoke. We struggled in trying to lay out this study because DOE wanted a recommendation, or wanted to know our opinion of do we or do we not need to go to the Calico Hills. And frankly, we decided we couldn't answer that question because we don't know how much performance has to be allocated to the Calico Hills. It depends on how much performance we get out of the EBS, it depends on how much performance we can assign to the saturated zone. So that's why in that flow chart that Rick showed we've got conditional decisions there. How much we need to rely on that Calico Hills depends on its place in the total system. And there's a lot of uncertainty in EBS performance and TSW performance due to thermal loading and thermal refluxing. We just frankly don't know how important the Calico Hills is yet, and we're not going to know for a couple of years. And yet DOE's going to have to make the decision anyway pretty soon of whether or not we're going to go, and it's going to be a decision made in the face of a lot of uncertainty.

DR. CORDING: Thank you. If one is looking at a multi-barrier and the systems which are redundant, when you assign percentages to things in looking at the possibilities, particularly at this stage, your total shouldn't necessarily add to 100 percent. We ought to be looking at the Calico Hills for what would happen if something else in the system fails. That's what I think we talked about with the multiple barriers. And so you can come up with performance assessments that will show very little effect if you cut it off somewhere else completely. And perhaps that's what one could do with certain engineered barriers, but you're also looking at geologic barriers, and the discussion today has been on combined systems. They're not downgrading the engineered to geologic barriers. I think that's what we're hearing. But we are looking at multiple barriers, which is part of what the Board has been recommending as well.

DR. LANGMUIR: Just on that same note, if you decide to evaluate whether the site would work without it and then come in later on and say, "Well, I guess we better look at it because it doesn't look too good," boy, you're going to get nailed by the public and the licensing people. You can't pick things up later on and bring them in to fill in gaps and be credible at all, so you're going to have to think about it early on if you want to use it all defensively.

DR. PRICE: Maybe smoke's not the word, maybe it's Pap, and I don't mean to smear Pap.

DR. CORDING: Okay, thank you. Other questions? Dan Metlay, Board staff?

DR. METLAY: Perhaps this should be directed to Steve. On the last page of the overheads, page 28, there's a sense of what is going to go into the assessment of the options, and each of the boxes for scientific understanding, cost, potential adverse impacts, and schedule, are sort of representative as roughly comparable. Of course if they're not, it really doesn't matter what some of those boxes might contain. In particular if cost is an overriding consideration, or schedule is an overriding consideration, it may not matter at all what the scientific understanding that you'll get from Calico Hills is, and I'm wondering if you are in a position to comment about how DOE thinks about weighing these various considerations.

DR. BROCOUM: Let me just start with this chart DOE only saw probably a week ago, so this information is almost as new for us as it is for you. So we haven't gotten the report yet. We have gotten the decision logic as intermediate deliverable. In other words, the first part of the presentation. Of course when you make decisions you do weigh these things, and as we make the decision, we'll weigh these things, and there may be other factors. This will be one input. This report will be one input into making that decision. This report itself does not make the decision. And my guess is that that decision will be made at the director's level.

DR. METLAY: I understand that this report will not make the decision. I guess what I'm trying to understand is, what is your sense of how these things are going to be balanced?

DR. BROCOUM: I guess the two most important boxes that I see on that chart are the adverse impacts and the scientific understanding.

DR. CORDING: Carl Di Bella?

DR. DI BELLA: Carl Di Bella again. Following up on Dan's question and my earlier schedule question, when do you think the latest is that DOE can make a decision to access Calico Hills by drifting, taking into account how long it's going to take to mobilize, how long it's going to take to allocate the money, how long it's going to take to draft writing the technical basis report and the guideline?

DR. BROCOUM: I think I indicated in my first presentation today, the one on waste--or is it thermal--I don't remember now--but that we are thinking very hard on how we can accelerate construction of the ESF and how we can both access, you know, across the block and into the Calico Hills. And I can't predict when that decision will be made, but we are working very hard on it right now.

DR. CORDING: Okay, thank you. Any other questions or comments? Okay. Thank you very much, and I think we'll proceed on before our break to the 1996 budget.

Is that appropriate, Steve?

DR. BROCOUM: Sure.

DR. CORDING: Steve Brocoum will be making that presentation. We're going to have a break following that, and following the break, then, we'll go to the public questions and the panel discussion.

DR. BROCOUM: Tom Statton just reminded me, he whispered in my ear, the fact that the Calico Hills is in the current program plan. I think it's for '99. Let me look at Robin; is that right? I think it's for year '99 right now. What we're really looking at is can we accelerate it at the moment. So it's currently in the baseline of the program is what I'm trying to say.

I think on the agenda this was called budget, and I changed the name just to talk about the planning process a little bit and the budget.

DR. CORDING: We're going to have our break following this next presentation, if you would all take your seats, I'd appreciate it, thank you.

DR. BROCOUM: What I'm going to talk about is an overview of the '96 budget and how we're planning for it, how we are rebaselining the program and the project to meet the program plan, what our '96 planning basis is, how it links to waste isolation strategy, we have some examples, and a summary.

I think Dan said this morning that DOE is requesting I think $630 million, of which about 472 million would be allocated to the project, about a 25 percent increase from Fiscal Year '95. That is the amount that would support the activities as we have in the OCRWM Program Plan from December of '94. And in fact, if you look in the back of that plan at the budget numbers, it is within a million or so dollars of what this number is right here.

This viewgraph gives an overview how we're planning. We produced our program plan in December and it feeds project rebaselining. We had our technical program review in February. That was a one-week meeting that we went over each of the site suitability buckets in a public meeting with many scientists. That led to some proposed changes in the site suitability schedule as part of this rebaselining. We are now engaged in a mid-year review. We have a formal meeting with the headquarter people on April 27th, and the results of that will also feed the project rebaselining. We expect to have the rebaselining done for the project at the end of May. That would then go to headquarters, then there would be an independent cost estimate by an outside contractor, then it would go to the Energy Systems Advisory Acquisition Board, I think is the name of it, ESAAB.

We've also started, based on a budget of $472 million, a detailed planning so we can be ready to go to work October 1st. There's a team of people being led by the M&O doing this baseline effort. One of the key people is sitting in the audience here, Robin St. Clair, in case we have any questions on the planning process. I asked her to come to the meeting, pulled her away from her replanning, so I thought I would mention it.

We are making a major change this year in how we do our planning and how we operate the project. And this viewgraph is trying to show that. In past years, when we did the planning, the detailed planning was done by each AM and their staff working with the participants. So, you know, the scientific programs would do their detailed planning and suitability and licensing would do their detail, engineering would do their detailed planning. We'd all meet periodically and argue with each other, and you know, eventually made it there. Nobody was very happy with the process. And I can see some smiles from some of the people that were involved in that last year. With the consolidation of the participants under the M&O, which is occurring right now--in other words, most of the participants will be consolidated in the M&O--the DOE is now asking the M&O to take the job of integrating the project and the program so that the detailed planning that cuts across the whole program will be done by the M&O, in concert with working with the participants that are now under the M&O or will soon be under the M&O umbrella. So that's what kind of a model. It's a major change for us. And, you know, some of us are a little apprehensive, it's the first time we're doing it, but we're kind of committed to going this way.

So in the past, the detailed planning was done by each AM in their area, and then it was coordinated across the AM's. The model we're using now is DOE has had its strategic planning retreats, many of them with Dan Dreyfus. We produced our program plan, we are producing relatively high-level guidance that we then send to the M&O. The M&O, working with this guidance, working with program requirements, working with draft tips that have been prepared, working with the participants, comes up with a detailed plan for Fiscal Year '96. That detailed plan is planned to be completed in July. I'm looking at Robin here for a yes. Okay. There is discussion back and forth between DOE and the M&O. As the plan is developed, we have numerous iterative meetings. When we finally agree that that's the right plan, DOE agrees, then we go into a worth authorization directive, I believe it's called--it's a contractual instrument--and we execute the plan. So this is a very new model for us to work, but we hope it gets rid of the stovepiping that there might have been some evidence of in the past. Stovepiping I mean suitability, licensing, science, engineering, environmental, and so on. So the M&O's responsibility is to balance all the needs of the program, and there are many needs, and in some cases they're competing, and come up with what they think is the best balance given our guidance. Now, are guidance comes out of the program plan.

The project rebaselining effort will further expand the detail in the program plan. We have produced a draft

Project Summary Schedule that has, I don't know, 1,500 milestones. I'm not sure the number of milestones, if somebody knows, but all integrated, all the feeds into the technical basis reports and into the regulatory compliance documents and so on. That Project Summary Schedule will be used to provide guidance to the planning, going on Fiscal Year '96, and that Project Summary Schedule incorporates the results that came out of the Technical Program Review. I told you, as the result of that Program Review, that we made some changes to suitability milestones. We moved some of the buckets around. Or we're proposing to move some of the buckets around would be a more accurate answer, because it hasn't yet been through the baselining process.

So the planning basis for '96 will be based on the Program Plan and the, as we're calling them, enhancements to the Program Plan that re reflected in the Project Summary Schedule. In other words, program planning came on in December, the Project Summary Schedule, I think the AM's agreed upon two or three weeks ago, and that's kind of the current thinking of the project, and so that will form part of the basis for '96 planning.

There are a lot of strategic issues affecting the '96 planning. There is, of course, the thermal loading strategy. As I said, we're thinking very hard of how we can accelerate these tests. We're starting to think about, as I said, potential characterization of expansion areas. We're thinking about the ESF drifting. If can drift faster than planned, that gives us more flexibility, and if we have more flexibility, we can look at different options for doing North Ramp or east-west and the Calico access. Of course, in all of this we have to balance the costs of ESF excavation and the cost of surfaced-based testing. That's one of the concerns. And historically in the program there have been some--I won't use the word "conflict," but there has been some tugging between the engineering side and the scientific side for the limited resources. Again, the M&O is asked to help us come up with this balance, because they're being asked to integrate the whole program.

Now, I have a series of viewgraphs trying to show you the kind of activities we will be doing in '96. I wasn't going to talk them, maybe use a few examples perhaps. I had some notes here. The first column are the elements of the barrier that we talked about earlier. The second column are the key uncertainties that we talked about in Beattie, somewhat modified and updated. And the third column are the activities that we will be doing, and what we've done is we've gone through the activities and left on these charts the activities that are either ongoing and will be continued in '96 or activities that will be started in '96. So this is a list of the type of testing and characterization activities for each of the elements related to the key uncertainties that we will be doing in '96. It's attempting to ask the question at this stage, when we haven't done our detailed planning yet, of what we will be doing in '96. Now, for example, characterizing fault geometries and hydrogeologic properties and faults as the ESF going down, characterizing the water chemistry and isotopic analysis of any samples we get under an ESF.

Jean Younker is here if we have a question on the uncertainties. Dennis Williams is here if you have any question on the activities.

This is some more examples here of some of the colloid investigations. The neptunium and technetium solubility experiments will be done in '96. The fusion rates, which are very important in terms of the release rate from the engineered barrier, and so on. With regard to migration in geosphere characterized in the fracture distribution and heterogeneity underground will be work going on. So rock properties testing as we go underground is ongoing work.

Anyway, this is meant to give you an idea, and as the people who are doing the detailed planning will take all these activities, will get them out, put them on networks, whatever the planning people are going to do. They've even moved into a separate facility so they can concentrate full-time on detailed planning. So, anyway, a few more examples, and that's continuing monitoring the seismic nets, the probabilistic seismic hazard evaluations, and so on, for tectonics. So, just some examples.

So in summary, the '96 planning is just being initiated. This planning will address activities needed to support the overall waste isolation strategy, as what the previous graphs are trying to show you, and we're going to obviously consider the recommendations we received from the Board, both in their report and in their December 6th letter.

The only other comment I need to make, I need to go back to an earlier viewgraph, is Dan said earlier today that funding, although we're planning for $472 million for the project, it may be very difficult to get that funding. And this is a problem we have every year when we do our planning, is that, you know, we won't probably know what our final mark, if you like, is in terms of funding until sometime in this time period here. So once we know what the actual number is, we can then finalize our planning. And then at that point you have to see if your schedules are correct and everything. So that's kind of where we are in our planning and budgeting process.

Okay.

DR. CORDING: Thank you. John Cantlon?

DR. CANTLON: Yes, Steve, you're certainly well aware that the program traditionally has mapped out a larger program than its been funded to deliver, the past year being one of the few exceptions.

DR. BROCOUM: That's correct.

DR. CANTLON: The question I have, and I've done enough budgets myself to know that one doesn't put up one will do if you get less money. That's not a very politic way to manage any program. But I presume you people do have this program prioritized so that if you were to get level funding or even less funding you have a way to have prioritized the operation to move forward in a kind of logical way.

DR. BROCOUM: One of the things that we didn't do in the Program Plan that we want to do the next time we update the Program Plan--I haven't shown the update because Dan has to give us the authorization to update it, but we do have plans to do that--is to consider what we call strategic alternatives. One of those will be if we don't get full funding. Another one would be if the NAS says no, this technical base report is inadequate. You know, there's a whole series of things like that. So we've identified about twenty strategic alternatives that we would like to specifically address what impact it would have on the program and the workaround you'd need to do that.

DR. CORDING: There was a description in the previous presentation on several alternatives for getting to the

Calico Hills. The question I had was, are you looking at possibilities that may be in addition to that at this point? Are there other options that you're working on?

DR. BROCOUM: I need to make a comment about these options. These were options to give you extensive drifting, moderate drifting and limited. They weren't meant to actually be real layouts. I need to say that. I knew when we put them up we'd get, "What about if you did this or you could turn it over here?" and so on.

DR. CORDING: That was sort of the way the ESF--

DR. BROCOUM: This is to show a method of a lot of faults, a few faults and one fault, basically, were the three options. We are looking at some options that are quite different than these three that were presented.

DR. CORDING: All right, thank you. Other questions or comments? Carl Di Bella, Board staff?

DR. DI BELLA: Thank you. Steve, you mentioned the technical program review in February a couple times, and I had the opportunity to attend parts of that review. I think it was a very productive experience, by the way, well put together. But I notice in a couple of cases, or maybe several cases, there appeared to be schedule disconnects between the schedule you had at the time for producing technical basis reports and the time that certain of the groups said that they could produce these reports. I think these were all in sort of the intermediate time frame, but you mentioned you might be making some changes as a result of that rebaselining. Is that part of the rebaselining specifically?

DR. BROCOUM: Yes. Let me give you some examples.

DR. DI BELLA: I just want to finish the question. And are you at the point where you have to renegotiate that schedule with the National Academy? And I wonder if you could give us an update on how things stand with the National Academy.

DR. BROCOUM: Okay, let me talk about the schedule. One of the big concerns from the participant technical program review was in preclosure rock characteristics. So what we decided to do is we're proposing combining the preclosure rock characteristics and the postclosure rock characteristics/geochemistry into one technical basis report and doing it later, when we were originally going to do the postclosure rock characteristics report. Since we're trying to do an iterative process, we're also planning to accelerate the preclosure radiological safety earlier, by about six months, and, I think kind of very significant, we're planning to ask the National Academy to do a peer review on our 1995 total system performance assessment so that when we have the 1997 one done it won't be the first time they have seen it and we'll have a chance to get comments from the National

Academy, fold them in, before we do the '97. The '97 one is the one we'll be using for TSS. These are the kind of things we'll be thinking about. Those have not been finalized yet, but the National Academy is aware of these and can support us in those things we want to do.

I said earlier that the contract is very close, but it's not there yet. We expect it to be completed in the next day or two, to be finalized and signed. At the moment it is signed, we will Federal Express them our first technical basis report, which is the one on surface processes. They expect to start that issue or call for nominations for peer reviewers in the next week and start the peer review on June 1st and complete the peer review on December 1st.

UNIDENTIFIED SPEAKER: December 1st?

DR. BROCOUM: December 1st. Six months. Is that right? Five months. No, six months, six full months, all of June through all of November. So that's where the National Academy is. But I wish I could say the contract was signed, but I can't.

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

I'd like to just comment on our procedures following the break. There will be the public question and comment session, and then following that will be a panel discussion. And I would like the speakers who have been present with us and speaking today to remain as part of the panel as well as the Board, and then we're going to be joined by some other individuals, included Dan Bullen, John Greeves, Stephen Hanauer, Carl Johnson and John Kessler. That will be our panel discussion following the public session.

We will break until 3:20, a fifteen-minute break. Thank you.

(Whereupon, a break was taken.)

DR. CANTLON: If you can have your seats, we'll start the public comment session. I'd like to make a few introductory remarks about the Board's public sessions comments. Those of you that have to continue your conversations, would you please move out to the hall so we can get underway here.

It's important, I think, that everyone recall that the Board was created by Congress as a scientific and technical review board. Our primary purpose in holding our sessions is to hear technical and scientific presentations from the people that are involved in developing this system and the scientific and technical critique from various other groups that are interested in the program. It is, of course, always useful to have during our sessions an open public period. Now, obviously if we're going to have a logically organized technical exchange between the people on our Board and our staff from a technical point of view, it is not compatible with moving ahead in a swift, logical way on very, very tight agendas to open up for public comment each speaker. This would be an incredibly time-consuming and diversionary way for our Board to operate. So we have no intention of modifying the way the Board operates and has operated in all of its existence to move in that direction.

Nevertheless, it is important in each one of our sessions to have a period after these technical exchanges have proceeded for individuals who are in the audience to make comments, hopefully on a technically focused aspect of it, because this is a technical exchange process. When one gets to basic policy, that is really outside of the Board's charge. And while we occasionally move outside of our charge when we think it's exceedingly important because of the technical ramifications of particular items, we do make such a move. But our Board is not a policy board.

So with that as a caveat to start, we have had one request for a public speaker, Mr. Thomas McGowan. Would you take the microphone there in the aisle?

MR. MCGOWAN: Honorable Mr. Chairman, esteemed members of the Board meeting, participants and attendees, my name is Tom McGowan. I'm an individual member of the public residing in Las Vegas, Nevada, and I do appreciate the opportunity to address the public record. And rather than engage in protracted dissertation, I'll attempt to limit my comments to candid summaries of what I consider the salient points at this juncture, with perhaps one allowable excursion in the verbal proliferation if I lose control.

Underground storage anywhere in the terrestrial domain, in my opinion, is unconscionable. That's a public opinion. It goes beyond scientific, technological, political, and legalistic concerns and approaches, in my opinion, the greater dimensional realm of ethics, morality, reason, integrity, and responsibility, which may or may not necessarily be in your Congressional mandate, or theirs for that matter. But it's in mine, and it may be on the agenda of some six to twelve billion people worldwide very shortly. I would enthusiastically encourage this Board and every person and entity and agency involved in nuclear pertinent activities, including but not limited to nuclear waste storage and disposition, to take very seriously that opinion and hopefully to adopt it as your own as well. We are human beings first, anything else second, and I believe that that's going to persist for the rest of human time, assuming there is any.

Second point, there is a compelling immediate need for an omni participant genuine national public consensus development process. There has never been such a process in this country or anywhere on this planet. There is none currently planned or potential as in pending, but you need one, I need one, we all need one. We need to unite as, first of all, a nation of unified people of one single mind, not simply an aggregate of competing interests sometimes redominating, sometimes not. We have a higher quest. That is to be utmost quality effective in terms of ethics, morality, reason, integrity, responsibility, in answer to a higher power than what we've been doing so far.

Third point, the entire site characterization process is fundamentally flawed on multiple grounds. I won't belabor your time or sensitivities explaining why I believe that, but I am firmly convinced, more so today than ever before. And I would make a levity based comment, if you don't mind, and with no personal denigration involved on anybody's part, to the Calico Hills incident, in which case probably runs a close second to the Dennis Fung incident. Any time somebody answers a question with the word "well, ellipse," case closed, verdict is in.

I wish everybody here the greatest encouragement for their respectable work. I tell you that I appreciate your work more than I can express. I hold every one of you in very high regard, and that's why I hold you to a higher standard. You are the finest scientific, technological, academic minds of our time. You need to set the standard, and the standard is nowhere in sight at this point. I know we can improve on this. And we cannot leave it for some future generation under the assumption that they will somehow be less quality deficient than we are. Chances are that will be just the opposite, if there is one. It is our responsibility. We must resolve this issue now, before we leave this planet. That gives you 30 to 50 years in some cases, in some cases a lot less time. But the initiative takes simply five seconds or less to question a personal decision making. You need to make that commitment that you will decide from here on out to do it right, not what's expedient or limit a special interest.

Regarding sovereign tribal nations, I am not a person of Indian ancestry. However, I know persons who were. I won't say some of my best friends are, because I don't know them that well yet, but I know for a fact they were here before I was, perhaps as long as 30,000 years before I was. And they have every right to have an equal representation and courtesy shown to them by government agencies that they have not had. I don't know when was the last time this Board or any other nuclear pertinent board took the time and trouble to voluntarily go to an Indian reservation and hold an important meeting that affected their interests as well as everyone else's, but if you have not done so, you need to explain why. And if someone has directed you not to do so, you need to identify who, and that person needs to be held responsible and accountable to the American people, because you will not use my name or anybody else's in basis for that kind of discriminatory activity to our Native American indigenous people. Thank you on that score.

I have a simple question. Regarding nuclear waste, why bury it? Why save it? Why protect it and preserve it? Why not store it for the requisite interim period only above ground pursuant to the drastic reduction, transelimination of it, completely and permanently, and the further deployment of the final disposition of the residual toxic byproducts be a space deployment, either sun targeted, blackhole targeted, distant planet targeted, and/or omni radially dispersed throughout the universe, and you can do that via the SHARP technology. I think you know that better than I do. There are still some remaining questions as to the aspect of potential reentry, but I say every baseball team has a catcher with a big mitt, and you ought to have one, too, if you haven't got one. You do have a pitcher, no question about it, called SHARP, Super High Altitude Research Project.

I would say also that for the process or reduction transelimination you have an item called ABC, Accelerated Base Conversion, which got a left-handed compliment of some kind earlier today from Dr. Dreyfus, without the opportunity to be here in person to respond or contribute to the dialogue. I would also remind you that that voluminous report is indeed a pre-final draft report. It was never intended for anything else. Neither was the Parks Report. So that dialogue is perhaps page 1 of a multivolume work.

The fact is, an underground repository, because of the axiomatic fact that a geophysical mass on a geologic time scale is a variable dynamic flux. It is not static and finite, cannot be made to be. Therefore, any limited incremental studies that you're conducting, particularly if they are selectively disciplined in terms of which uncertainties and for how long, can only be flawed. There is no way they can apply at each time loci in that continuum and under all circumstances of which variable. I would insist, if you don't mind, that you show a bit more of your higher calling than that.

And with that, Mr. Chairman, once again, thank you, sir, for your courtesy. I'm going to look forward to the opportunity to address this body briefly again tomorrow afternoon. Thank you.

DR. CANTLON: Other public comments? Larry?

MR. HAYES: Larry Hayes, USGS. Try to be constructive here. You know, there's a lot of discussion on the pros and cons of surface-based versus underground testing, and I think most of us accept and understand that each has its benefits relative to the other. I am afraid that there was some confusion left today about the benefits of surface-based testing and what we might learn about the Calico Hills from surface-based testing. Certainly we'll learn some things that are worthwhile, but we won't learn everything we need to know. Same thing goes with tunneling. We'll learn some things that we really need to know, but we won't learn everything we need to know. I think with DOE's blessing I'd like to send to DOE a statement, at least the survey perspective, on what it is we're going to learn from surface-based testing, and perhaps what we won't learn.

The second point I guess I'd like to make, and it goes back to Richard Memory's talks on the Calico Hills System Analysis, preferential pathways, Richard, you presented as perhaps a detriment to the site in your model, that that could perhaps lead towards failure. I believe there are some people that would believe under certain circumstances preferential pathways could be a benefit, perhaps acting as a natural drain system. I would ask that if you haven't put that kind of factoring into your model, perhaps it might be worth doing.

Thank you.

DR. CANTLON: Other comments? Martin?

MR. MIFFLIN: Marty Mifflin, State of Nevada. In the spirit of Dr. Dreyfus' talk this morning, I'd like to make a constructive recommendation at a timely point. And it relates to Tom Statton's presentation on the testing program and the timing and the various scales attached and so forth of what type of information would be coming forth. It's my perception that as long as the heavier thermal loads is maintained as a strategy or a possible loading scenario that site characterization should incorporate the type of studies that allows the analysis and the goodness or the poorness of the site as you go along in these next ten or fifteen years that seem to be contemplated. If you look carefully--and I would like the Board to look at this in particular, because the Board has recommended the ESF facility and the characterization from that strategy--if you look very carefully at the type of information that would be required to model a heavy thermal load with the mobilization of the matrix moisture, one of the major questions that will come forth in that modeling is, where does the mobilized moisture go? And in order to create the boundary conditions that have any meaning, one has to characterize the site with those gross site scale boundary conditions in mind. If it's a very heavy thermal load, or even a reference or SCP load, the question becomes, how much of that moisture gets driven out of the mountain, for example? Because on a condensation scenario, what you lose in the process of the thermal load as it ramps up to higher temperatures determines how much stays in and how much would be coming back to the repository horizon. And under the heavy thermal load scenarios that have been modeled, one sees that you would have a rather large envelope where you would mobilize the moisture.

Well, to go into licensing without a good idea of your major boundaries, that because you've only tested, so to speak, or characterized on the basis of localized borehole type data or at best tunnel scale data does not allow you to determine whether or not the better unit holds the moisture in or whether it goes out, leaks someplace and, as Dr. Domenico mentioned one time, have some steam coming out of the mountain. But it's important, if that steam is going to come out, that it may be very, very critical with respect to the overall performance of the site from the standpoint of how much condensation comes back into the repository.

For several years now I've tried to urge that the repository scale data bases should be established prior to the ESF, disrupting the apparent confined conditions that occur in the Topopah Springs. When I say confined, I mean confined from the perspective of pneumatic continuity. And the evidence has been available since the mid-'80's that there is a degree of confinement with the blowing and sucking of the wells and so forth.

The recent success and progress of the surface-based program in monitoring the barometric pressure changes demonstrate that the confinement's there. The degree of confinement becomes critical with respect to any type of above boiling thermal load, because it tells you where the moisture can go and cannot go in terms of a gas phase and how much is there in the cool down in the condensation phase and reentry into the repository.

So after wasting your time on this, I think it's very, very important to get the possible loading strategy hooked into the site characterization program. Because once you disrupt that confinement, you have no known strategy to determine major boundary conditions. And there's about a four- to six-month period here where there's a grace period to think that through. Now, if there's not going to be an above boiling thermal load application, then that becomes probably less important. But I'm saying that because of space requirements and costs, there's a real effort to try to find a fairly high density loading scenario to make the site fly. And therefore I think it behooves the Board to think this through a little bit.

Thank you.

DR. CANTLON: Thank you, Marty.

Other comments from the audience?

MR. WILLIAMS: I'm Jim Williams, and I have a question or a comment that's keyed to Mr. Dreyfus' statement this morning that the Yucca Mountain effort is keyed to funding suitability for a capacity near the statutory expectation of 70,000 metric tons and that if that doesn't work out--I'm paraphrasing here--that other strategies, such as characterization of significantly more emplacement area or other technical options would be necessary. So my questions are two. Does this imply that the characterization program defines space for 70,000 tons is likely to be or possibly significantly larger than the $6.3 billion program currently underway? That's number one. And number two, is the observation that 70,000 metric tons may not be the entire inventory that requires permanent disposal? The no new orders case has an additional 26,000, roughly, metric tons in it, and projections of defense waste may be higher and greater than Class C waste that's slated for permanent disposal. So are we looking at sort of another repository program of some sort here beyond the 70,000 tons?

DR. CANTLON: Steve, you want to comment?

DR. BROCOUM: Well, I want to make a correction. The program is not 6.3 billion, I think it's about 5. I don't know if it's 4.9 or 5. The Program Plan Approach is roughly a $5 billion site characterization program. Prior to that, the program was baselined in 1991 to a $6.3 billion program when we were going to go to license application in 2004.

The extent and the cost of characterization of the expansion areas hasn't been determined yet, but that's part of our planning, as I said earlier, in our strategic alternatives. In doing that, we will cost out and perhaps come up with several alternatives to characterize, you know, different amounts of expansion areas, for example, or have it done by different times. So I can't give a straight answer to that.

The question on the capacity and the excess needs from the operating reactors, I think we are to report to Congress between 2006 and 2009 on the need of a second repository. And we did a recent study, I think called the 803, that confirmed that that was correct. So from our perspective, the law is requiring us to make a report to Congress after 2006, you know, it's correct and still valid.

DR. CANTLON: Other comments?

MR. MEYERS: Good afternoon. My name is Calvin Meyers. I'm from the Moapa Band of Paiutes. My comments are not technical, they're more of I have a question that maybe you people can help me with, if you will. The Moapa Band of Paiutes' land will be traversed by the transportation of nuclear waste. I would like to ask the Board if they could send the information of any of that waste that comes across my lands, what am I looking at? Because I have asked my tribal chair if I can write a letter to Secretary Babbitt for affected status, and I've asked other people to help me write this letter. As of to date, we are not an affected status because we do not live on Yucca Mountain.

And the Department of Energy gives us a big runaround on questions that we ask them. I've heard comments from some people from the Department that are officials stating that "We can't work with the tribes because they change positions all the time." That's a bunch of bull. They do that more at the Department of Energy than we do. Besides, we live here, we have always been here. When I talked to Mr. Barnes, I stated that I lived here, and his comment was, "Well, I've had nine driver's licenses." That, to me, means that he doesn't know where he's going or where he's been. He's lost. I'm not lost. This is my land.

And this is real hard for me to come up here and ask you for this, but this, to me, is the only way that we are going to get anything done, that we, the people of this nation--I mean my nation, not yours, because this is my nation that I am talking about--we have to be heard. We have to be talked with in a government-to-government relationship, and that government-to-government relationship was started off by DOE. But I have not seen it work yet. So I would like to ask that, if you can, send me some information about what's going to be shipped, and if there's any danger to us, I would like to know, because I'm tired of being laughed out. The Moapa Band of Paiutes is not part of the public. You can tell because look at the affected units of the local government. You do not see the Moapa Band of Paiutes on there, but yet we are just as affected as anybody else. They're not going to come up and build protection for us when they start shipping. So we're just as affected as anybody else, and we would just like to have our right, just like any other human in the United States have their rights.

And that's all I'd like to say. Thank you.

DR. CANTLON: Thank you. Other comments?

(No response.)

DR. CANTLON: All right, Ed, it's over to you to get the discussion session going.

DR. CORDING: The panel discussion will include several individuals who are joining us in addition to the Board and the presenters at today's session, and they include John Greeves on my right. John Greeves is director of Division of Waste Management of the Nuclear Regulatory Commission. Stephen Hanauer, special assistant to the director of OCRWM. John Kessler as manager of High-Level Waste Disposal Division of EPRI. And Steve Frishman with the State of Nevada. Then on my left, Dan Bullen, who is associate professor of nuclear engineering in the Department of Mechanical Engineering at Iowa State University.

I suggest as we start this we will be discussing topics that have been presented today and focusing on waste isolation strategy. The discussion of the Calico Hills is a part of that strategy and a specific example of it. And then looking at the thermal strategy, also part of waste isolation strategy, and the integration of the test program with the thermal strategy and the bringing together of that as one approaches the decisions for site suitability and license application. Those will be our topics, and I'd like to invite the people that have joined us that have not had a chance to speak today to make a statement or any comments they would wish to make at this time. I'd suggest we start with John Greeves of the NRC.

John, the floor is yours.

DR. GREEVES: Okay, thank you. It's my time, I'll take it. First I'd like to compliment the Board for putting on this session. I enjoyed it, and you've got some hard-hitting issues here, and it's in fact helped me work through some of the thinking that NRC is going through.

Just to touch on, quickly, the legislation. Steve Kraft mentioned that this morning. There's a whole lot more in that legislation than just the issues of Yucca Mountain. This whole business of the MRS and adequate funding and integrating the spent fuel program is driving that process. However, it seems like there isn't a whole lot of agreement as to which way to go, so I'll stop with that.

NRC concerns are: Will there be adequate information for the license application? We are not focusing on this Technical Site Suitability issue. Our mission is license application, public health and safety, so that's where I come from. And not news to anybody in the room, this thermal loading strategy is a key problem. I've had some real concerns with it. There's, I think, some healthy tension associated with it. It's an evolving program, and one of your questions is, is the strategy clear and coherent? I say no, it isn't at the present time. It needs to come further forward. Every meeting I go to I learn a little something more. I heard about this Maximum Design Thermal Loading concept, and I have to tell you, I was pleased to see that, because I was not looking forward to a process where an application came in with a low load. We all look at that for things like groundwater travel time, other issues, and then four or five years later we look at some higher number. So I'm encouraged, although I haven't seen this White Paper, although I'm quite anxious to get ahold of it, as the rest of you are, and it helps ease a little bit some of the concerns that I have. The concerns that I have in large part are based on Dr. Dreyfus's statement of keeping his options open. I understand where he's coming from, but he makes my job very difficult by keeping his options open. The high load, the medium load, the low load, backfill, no backfill. I think Dr. Langmuir appreciates the difficulties of trying to figure out what the geochemical environment you're in when you have that confronting you. So I was concerned before I came to the meeting on those fronts, I still am, and we've been pressing to try and come up with a reference design. I think that's a healthy tension and it's somewhat different, I think, from what the Board has said, so I just share that with you.

Another key issue is this question of adequate real estate. Some of these commenters that just stood up hit on that. Many of the people here did. I think Dr. Dreyfus has said if you can't house any more than 20,000 metric tons, you don't have a repository. Well, DOE, I think, needs to come forward and tell us what is it that they can get. Maybe it isn't 70, but is it 50? And what does that do to you in the way of real estate that you need to acquire? It's an issue you've all addressed.

These issues associated with coupled effects, Tom Statton put up a chart showing, that data isn't going to be available until sometime late in the process. Puts a regulator in a real difficult position in terms of defining what's adequate assurance in that decision process. I'm just sharing with you the problems that I have. It also affects the groundwater travel time thing. I sit through so many meetings on that, and it's tied to the thermal issues. DOE made a presentation to NRC a week ago, and it very much affects their efforts on groundwater travel time.

I heard some discussion about the subsystem performance objective. Steve Kraft pointed out that the chairman provided testimony saying that we could live with an approach that had a single system. However, I've got a question that I don't think DOE would stop using this multiple barrier approaches. This Board is recommending that they use multiple barriers. I don't think DOE would stop. Maybe they would venture some discussion on that in the round-table discussion.

I've got just two more points. One is a problem that I have is we're trying to write a standard review plan. I told you how difficult it was to evaluate all these options that DOE wants to keep open. I don't have the resources to write the review plans for the high, the medium, the low, no backfill, backfill. It really gives me a problem, and we're forced to go to a new approach that time doesn't permit me to identify here. We'll call it a vertical slice approach of review, and maybe in another meeting we can talk about that.

And let me finish with Dr. Pat Domenico mentioned this question about can you define what the failure issue is? Don't be afraid to show these charts that exceed the standard. Any other arena I deal in, if you're going with an abounding approach, you frequently exceed the standard in your early evaluations, and most of these meetings I go to, somebody says, "Hey, look how far below the standard I am." I would urge don't be afraid to show you don't meet the standard. In fact, I think Dr. Domenico was pointing out if you do the analysis right, you can help yourself understand what criteria do I need to find with these test results? What's the litmus test here when I go underground? And then I can prove I'm better than that.

I've gone on here a little bit, but I thought those were important comments. Back to you.

DR. CORDING: Thank you very much, and I think we're going to want to come back certainly to a number of those. Let's continue with our other panel members here. Stephen Hanauer.

MR. HANAUER: Thank you, Mr. Chairman, I don't have an opening statement.

DR. CORDING: Okay, thank you. John Kessler?

DR. KESSLER: Just a couple comments on thermal loading. I just also want to point out that I do sense some sort of change in approach from what was presented in November. I hear words along the lines of preserved flexibility as opposed to the go low to begin with and then proceed to hot. That does seem to be somewhat, at least on the surface, a change in approach. And I agree there are definite down sides to preserving flexibility, but at this early stage that might not be such a terribly bad idea.

Also, I think that Russ McFArland attempted to ask the question earlier about the minimal disturbance premise of no significant perturbation to ambient. I also would like clarification as to how you're going to go about deciding what significant perturbation is. Even the lowest thermal loadings that are being considered are still 100 times the natural background heat flux, so you definitely need to spend some time thinking about what you mean when you say no significant perturbation.

Also, I keep hearing the word "boiling," and when I hear the word boiling, I think of this pot of water with these bubbles occasionally bumping up very rapidly and this very violent type of activity going on. I just don't think that that's what is a good analogy for what is happening at Yucca Mountain, or what will happen. I think we should think in terms of vaporization. And in that case, there's just a steady transition or increase in vaporization that occurs as you proceed through this 96 degree C number. Vaporization occurs below that number. The point I'm trying to make is that these processes continue below the boiling point. And my concern is that I hear a lot of talk about, "Well, if we just stay below boiling, then life is easier." We still have vaporization and we still have condensation. As I said before, I just am concerned that by staying below that arbitrary value you still have all the mechanisms in place, and I'm concerned that that may not necessarily make your licensing life easier, and that's what you're really after, is trying to get a license. You may have to address all those issues whether they are strongly there or whether they are somewhat less strong there. So that's my concern about this arbitrary decision to stay below boiling.

Another concern in terms of high versus low loading is, there was some discussion, and I think Dwight Hoxie jumped in with a few of the comments about the amount of data that we do or do not have for Calico Hills as of today. And that brings along the lines of how many wells do we have drilled, or how many holes do we have drilled to characterize right now the footprint that's under consideration. It seems like DOE has scaled back that amount. It may be difficult even to characterize the current footprint for any of these Calico Hills options that comes along. So then the question is, if you're going to expand to more real estate, it just seems like it's going to be very difficult to characterize all that much area you might need for some of these lower thermal loading strategies if your intent is to maintain the 70,000 metric tons.

And finally, as much as I hate to admit it, I think that perhaps what we heard about as far as the flow diagram approach that we heard as far as how to make a decision about whether to go forward with the Calico Hills study or not, perhaps something like that should be done for the thermal loading approach. There's a lot of conflicting, or at least opposing, characteristics as far as whether one should go high or low. At least if they were all written down in an attempt to be addressed in some sort of decision-making process it would help all of us understand a little bit better as to how DOE will work its way through this thermal loading strategy as the years go on here.

That's all I have for opening comments.

DR. CORDING: Thank you, Steve.

Steve Frishman?

MR. FRISHMAN: I'll keep it real short and sort of outside of the things that I think may come up when we start talking about the questions.

I guess the first thing that I need to repeat is a comment that I made to you once before, and that's that under the law the Secretary's site recommendation is a very important decision, and maybe the most important decision under the Act that the Department has to make. What I'm seeing as I watch the development of the Program Approach is that there is less and less attention being paid to what it takes to make that decision. That decision is more and more being pushed off to a hurdle that has to be jumped. Well, it's a hurdle for which the Department is accountable. And I guess I place this at the Board's feet more than anyplace else, and that's that you have a charge to look at the technical validity of the work that leads to decisions. And my question is, after what we've seen today and in past presentations of waste isolation strategy and other areas, can you come to a determination that a suitability determination appears to have technical validity when you know that the questions that are important to licensing have not been answered? And so I think it's a large question that's out there, and we see more and more evidence of what will and won't be known at various stages of decision. Technical Site Suitability also includes the Secretary's determination that at least from the Secretary's point of view the site meets the requirements of NRC licensing. And here we see a difference in the amount of information that is available to make that decision. And that's because the guidelines required them.

So I guess something for you to consider is this whole question of is a Technical Site Suitability determination a technically valid determination when you know that it contains less information than successive decisions, although there's an estimate within that Technical Site Suitability determination that the data will provide that next level of information as expected by the Department to meet our next set of standards? So I think that's a difficult question for you to have to deal with, but I think it's one that is very much within your charge, and because of the way the Department has put its program together, I think it is staring you square in the face right now.

Another point is that there's talk again--and this is the first time we've heard it in quite a while--about ambient. And I think we're seeing a Site Characterization Program that is getting farther and farther away from ever being able to define or at least give us confidence in what the ambient condition is. I think the program is tilted very heavily, and more and more so all the time, towards total system performance. Well, I question whether you can have a total system performance conclusion that has any validity or credibility if you don't know where you started, if you don't know what the initial conditions were.

And finally, built into this idea that is sort of flopping around about where the thermal load will be at various decision points, throughout there has always been the statement that the assumption is that low thermal load is somehow related to a lower uncertainty. Well, I've thought about that ever since the first time I heard it, and I'd just like to kind of lay out that I think that assumption needs to be questioned. I'm not sure that in fact a lower thermal load provides a lower uncertainty. I think in fact it may provide a level uncertainty with all thermal loading. It may in fact, as was mentioned, create some new uncertainties that actually jack up the overall uncertainty in performance. So I think the assumption needs to be questioned, and just as was said, we have to remember that under any thermal scenarios that I've seen and the data associated with them, we have significant perturbation, because we have the rock temperature getting high enough to rapidly vaporize water. So I think this idea of significant perturbation, any thermal load that is being considered right now using the MPC, and using fuel of the ages that are being discussed, I think you have a hard time saying was not a significant perturbation against ambient. And we'll probably have lots more to talk about.

Thanks.

DR. CORDING: Thank you. Dan Bullen?

DR. BULLEN: Well, I don't want to reiterate what was mentioned previously by the other panel members, but I have a couple of comments that I would like to make.

First, I want to compliment DOE and the M&O. At least they're applying some performance assessment methodologies to the decision-making process. Now, by doing this, they also raise a few questions. For example: How was the performance assessment used? What criteria were used for the selection? Did you do a peer review? Do you elicit expert judgement? And in fact what decisions were made with respect to Calico Hills or any of the other decisions that are made using the performance assessment? I think it's a big step forward because the utilization of PA may give you a direction in the way you want to go with respect to site characterization.

However, there are a couple of things that I question with respect to maintaining flexibility. For the thermal loading issue, you want to have flexibility with respect to loading, and I understand that, you know, Dr. Greeves has a problem with that because of the fact that he has to write criteria that you can evaluate it. But I think if you find that we're going to use a large MPC and we're going to have a total inventory of 70,000 metric tons of uranium, then we're going to have local conditions that are high. This addresses the issue of whether or not we have boiling or steam formation as Dr. Kessler mentioned previously. But the repository will be locally hot and you'll have to address the issues associated with corrosion and microbiologically influenced corrosion and waste package material selection. I'm looking forward to tomorrow, when we talk about waste packages and the corrosion testing requirements. But the waste package material selection is also going to be driven by the fact that you have a locally hot container, and you might want to consider that.

Now with respect to the waste isolation strategies, substantially complete containment I'm very interested in, particularly with respect to the uncertainties associated with the performance of each barrier. And I think it's a very important opportunity that you have to try and determine if you have much larger uncertainties at a longer period of time. I mean, if you predict performance of the barrier and you use that as a performance tool but your uncertainties are so large that the data don't mean anything, what have you gained?

And then finally, with respect to the Calico Hills, I'd just like to make an observation. We tried to define what the failure was, or failure mechanism was for Calico Hills, and it brings me back to the same questions I asked about performance assessment. How was the PA used? What criteria were developed for determining whether or not it failed? And I guess the numbers that were presented, essentially the 90-10 split for fracture matrix coupling, identified failure of the Calico Hills unit. Now, my personal experience with this has been, in using the PA codes, both the IMARC code and the RIP code for evaluation of a repository--one of my doctoral students is doing waste form evaluation for metallic and mineral waste forms for the IFR. He's funded by Argonne, and guess what, that's not too hard to figure out. But what we did find is that the critical parameter to map both the IMARC and RIP codes and make them behave similarly for each of the waste forms was fracture matrix coupling. And so that may be an issue that you would want to investigate with respect to your performance assessment tools and decide, is it really worthwhile to go down and look at Calico Hills?

I've just tossed these ideas out as a little fodder for the fire here, and I'll open it up to the other panel on the people, I guess.

DR. CORDING: Good. Thank you.

There's a number of topics to cover here, and we have some time, and I'd like perhaps to stay with the thermal issue and we can work our way back to some of the larger questions as well, but there's several people have brought up this issue of--I think Steve Kessler comments, what is significant perturbation? Some comments of Steve Frishman that maybe the low thermal loading is not the best, maybe there's more risk with the higher loadings. Many of us are hearing these sorts of comments. Perhaps we could at this point hear some further comment on the topic, and particularly of what really does this low thermal loading strategy involve and what do we need to do to evaluate it? I think that's the topic that perhaps we can start with here. We can work our way up to higher thermal loads, perhaps, as we continue our discussion. But the low thermal loading or ambient conditions has been sometimes described. What are we really talking about there and how should we be approaching it?

DR. LANGMUIR: Langmuir, Board. Maybe I could be more specific with some pieces of what Ed is suggesting we discuss. My sense from talking to Tom Buscheck some time ago was that no matter what waste load was put in the mountain, there would be some refluxion. We've talked about this, too, John mentioned this a moment ago, that "ambient," whatever you choose, if there's waste in there, there's a thermal gradient that exceeds ambient, there's evaporation, there's condensation. Presumably there's some movement of water vertically around in the system. And frankly--this is coming back to I think Steve's issue--low loading, you may be able to predict things better, but my guess is the prediction is much more corrosion and much shorter lifetimes for waste packages because of the amounts of water close to packages, less complete evaporation taking place, potentially more continuous refluxion through time. You don't move the water away from the system for a long period as you might with a higher loading.

This is fishing a little bit, but in terms of the test work that's proposed--and this goes, I guess, to Tom or someone in the audience--how are we getting a handle on these effects, this "ambient," the low temperature loading scenario, what it might mean? Because to me it also means you're still going to have fracture movement of fluids, condensation in those fractures giving secondary effects, coupled effects. All that could happen in low loading. Is the hope that those will happen early in those tests because we're starting to cook it? When are we going to see these effects sufficiently to evaluate their significance to performance of the repository in the test work?

MR. STATTON: I don't want to answer that part. If we were to take a look at the testing program as we can conduct one, as we have one laid out is also as we can conduct one. And we look at the scale/time reference in which we are capable of conducting tests, we're conducting tests that have a cycle to them in a licensing process that has to be measured in years. Not tens of years, not hundreds of years, because the legislation that was put together cannot have envisioned a process that requires a 25-year test to accept a licensing case that's going to be dealt with in a matter of a few years. So quite clearly what's embodied in this is a test frame that's a short-time test frame. We can argue whether it's three years or five years or seven years. I think that doesn't make much difference.

The volume of rock that one is capable of heating in a testing program, our testing program, any testing program I can envision, without overdriving the system, consistent with our point heat load is going to deal with meters of rock away from an opening. Well, when we look at applying a heat load to the mountain, the effects that we are, number one, capable of looking at, and number two, capable of inducing, are measured in meters away. What we're talking about with a high thermal load is really the coalescence of that. It's the extrapolation of a phenomena that we are going to test to some space that we're incapable of testing in either time or volume.

So I think the testing program per se is reasonably insensitive to the high and low thermal load issue. That's one of the points I was trying to get across in the viewgraphs. The phenomena we are going to look at is the near-term, near-space phenomena that we can drive a test to take a look at. I believe that is the low thermal loading case. And I believe it is an extrapolation or a leap of faith for us to extrapolate tens of meters to square miles. So in terms of the testing program, if that's really the question you asked, I believe the testing program focuses on the phenomenology that is common to both high and low thermal loads. It provides underpinnings for an extrapolation beyond that, but quite clearly we can't do a demonstration of that phenomena that's measured in square miles.

DR. BULLEN: Tom, can I follow up on that? I'm Dan Bullen from Iowa State. The large block test that's proposed, if you do it in an expeditious manner, should tell you a little bit about fracture matrix flow, how much water you can mobilize as a function of the driving force that you put behind it. And whether or not you overdrive it and destroy the rock or whatever might be a problem is sort of secondary to the fact that you can see how the water moves, how it condenses, if you get flow back down the fractures, and you get to see the behavior of the small-scale mountain, if you will. Could you address the goals of the large block test and say maybe in the short term how you expect these results to be able to be extrapolated through the performance assessment modeling to predict what the mountain response might be?

MR. STATTON: At the sake of losing where we are, chances are what I ought to do is ask for about a two-minute detailed description of that from Dale Wilder, who was hoping I wasn't going to do this.

DR. BULLEN: You're asking for a sidebar? What, you're Judge Ito here?

DR. CORDING: Is the judge losing control?

MR. WILDER: I was just taking a bite of my candy bar when you called my name. The large block test as it is currently designed will be focusing on essentially the thermohydrological aspects, looking at things like buoyant convection, whether or not we can create a refluxing zone--and we are designing it to where we will try to do that--and can that water which gets mobilized then pierce through a thermal zone. We will also have as an objective of the test to take the block apart after the test and try to evaluate the geochemistry, although we will not be able to monitor the geochemistry during the test, to look at issues of can there be hydrological property changes because of the introduction of heat, which we assume that there will be. We will also be trying to look at issues of condensate shedding, which is one of the big issues that have been floating around. We will monitor both above and below the heated zone.

Primarily what we're going to be trying to do is to build confidence that, number one, our models have incorporated the right kinds of physics so that we can then evaluate underground conditions. And secondly, to build some confidence in our ability to properly predict performance. And so we'll be looking both for the physics and also some pre-test, post-test calculations.

And then finally, we are going to try to look at a number of different approaches for trying to evaluate that thermohydrological regime to evaluate how sensitive some of the assumptions and the model assumptions are. I should say how sensitive the results are to those model assumptions and the input.

DR. CORDING: Dale, there are certainly different boundary conditions on the large block test. Is that going to pose some major problems in terms of being able to translate that information to what would be incurring in a larger rock mass?

MR. WILDER: Well, I think the first point is that while those boundary conditions are quite different, we are not trying to characterize the underground. We recognize we'll have to do that with an in situ test. But because the boundary conditions are as they are, it allows us to control those boundary conditions in a way that we can't in the in situ tests. And so we are not allowing moisture movement across the boundaries of the block except for the top and the bottom. We are controlling the temperatures so that we can get more typical of what we could model and what we can evaluate. And so we aren't trying to duplicate what we would see underground, and it does give us an opportunity to have better control.

DR. LANGMUIR: Langmuir. Do you have other fractures in that block and are they part of your experiment?

MR. WILDER: Yes, there are a number of fractures and it is a major part of the experiment. The block was located originally to take advantage of a number of fractures. We wanted to make sure we saw a complete suite of fractures, and so we did permeability testing before we determined where we would actually cut the sites of the block. And the mapping that has been done, I'm not sure how many fractures we have mapped, but I know it's up in the hundreds, from very tight fractures, which as you look at the block really look like nothing more than a pencil line, to very open fractures and very continuous fractures. We have our instrumentation designed to look at both the continuous fractures as well as the nonconnected fractures, and so we will be able to make a comparison between the response in those different domains and hopefully get a handle on the interconnectivity functions.

MR. STATTON: Yes, I think back to try to just very simply answer the question, the phenomenology we're after with a large block test is can we dry out rock as a function of heat, can we nominally, given our understanding today, observe the way it leaves, can we map where it goes, can we see how it comes back, and can we in this sort of off-site location observe any chemical changes or physical changes in pathways, for example? Can we observe those in such a way that they can be modeled in situ? The value of being off-site, the value of a large block test is more than being able to just control the boundary conditions, we can look at them. There's high value in a freestanding block in an ability to get a three-dimensional picture of what the fracture distribution is in there. An opportunity or a luxury that we are unlikely to get in the underground.

MR. WILDER: If I could just follow up on that just very briefly. That was one of the comments I was making when I said we've got four different approaches that we're trying to look at. Because we do see the fractures in three dimensions, we are now able to model can we take an approach where we really try to look at the actual fracture distribution. And we are looking at that with three-dimensional codes as well as looking at some of the layer cake and the vertical permeability structure and so forth, and we will also try to do some FRACMAN modeling, and then we can compare all of those to see how sensitive our results will be to understanding the full three-dimensional fracture system.

DR. BULLEN: This is Dan Bullen with just one quick question. Having completed the large block test and having gotten the data that Dale mentioned, could you then use it to make some decision on thermal loading? And when would you see that decision being made?

MR. STATTON: They're scared to death I'm going to answer that.

DR. BULLEN: How about a preliminary decision?

MR. STATTON: Again--and the analogy to a litmus test, I think, was started earlier--the large block test nor other heater tests are in fact litmus tests for thermal loading. The phenomenology we're looking at, the behavior space we're looking at that in, and the time frame we're looking at it in, is common to both high and low thermal loads. In the immediate vicinity of any emplacement opening, given an upper bound operative temperature at the skin of the rock of 200 degrees, we are going to look at the same phenomena within the next five or ten meters. So I think that the behavior patterns here help us in terms of our conceptual extrapolation to a coalescence of that behavior versus a localization of that behavior. It is that conceptual extrapolation that makes the decision on thermal loading, not the test results.

DR. CORDING: Steve Hanauer?

MR. HANAUER: I'd like to address this problem a little more generally. In all the safety analysis I've been associated with, which uses scenarios and analysis of things most of which have not yet occurred, and we hope never will, it is necessary to use calculations and modeling as well as physical data in order to answer a large number of questions that start with "What if" without doing a large number of experiments, some of which are either impossible or you really don't want to do. The result, then, is predictions of outcomes which have with them a certain amount of uncertainty. Now, it's obviously impossible to do a testing program before licensing that involves the time scales that Tom was talking about, 25 years, 100 years, 1,000 years, and it's equally obvious that a testing program on the size scale of Yucca Mountain is also impossible before licensing, partly because it would take so very long. What's necessary is to bring model development and the necessary physical data long together so that the extrapolation to decades or centuries or millennia and the extrapolation to the size of the mountain is not just a leap of faith, it's a scientific prediction which is subject to some uncertainty.

In our case, we have the advantage that if we ever build and load the repository, we can measure what's going on, and we are not in the realm of maximum credible accidents about which we may never get some data. But if the scientific predictions are sufficiently favorable and the uncertainties sufficiently small, we can go ahead with our predictions and we will have the virtually unique advantage in the safety and improbably accident field of eventual confirmation or not of our predictions and a time in which to do something.

Now, in this context, the thermal decision, which does not have to be made today but which has to be made sometime, appears in this general framework. The reason that we are all so frustrated with it today is that we have a large amount of strategizing, and in my opinion an insufficient amount of technical work, both in model development in application and, as we all know, in physical data. We have a program to acquire this. We are not going to take any given test and use it as a litmus test. Such a test doesn't exist. We have to take the available data and the available models and make the best predictions we can at any given time if a decision is needed and to evaluate realistically, which is very difficult, the uncertainties which are involved.

Now, I think the immediate future, besides the activities which have been described today which are necessary, also needs some additional technical work using the data available today and the models available today. I don't believe we have anything like serious scientific predictions of the available alternatives. We have something called low, which is not well defined, and which, as has been pointed out by others, is going to involve a perturbation of the ambient. We can't get around it. We have high, which is stated to involve extensive dryout, and the predictions involved in that have been taken in some detail and need further examination.

Other alternatives have been suggested, the isolated hot drift being one example. There are approximations in some of these scientific predictions which need to be removed. An obvious one is the present assumption that the surface boundary condition is 100 percent humidity, which is true in Nevada only very occasionally, and which doesn't allow for the transpiration of water vapor out of the mountain which many people think will take place.

I think that we can do a much better job scoping the thermal strategy problem than we have so far done. But I don't think it's necessary or even desirable that this be solved today or even in the next year. We're going to have to say something sensible about thermal loading in our Technical Site Suitability considerations, and I don't think we should try and decide today what this should be. We're going to have to say something not only sensible but provide reasonable assurance in our license application in the year 2001, or whenever it is, and we will continue to learn about thermal loading for a long time after that. I think the search for solutions today is probably counterproductive.

DR. CORDING: Okay, thank you. Steve?

MR. FRISHMAN: I don't know whether people have looked at it this way. Is it really the thermal load that we're worried about? Isn't it the resulting cool down condition out in the future that we're worried about? And, you know, whatever the heat of it is becomes a part of that consequence and it becomes a mechanical consequence, it has chemical consequences, it has hydrologic consequences. But for performance, isn't that what we're trying to get to, what is the consequence of the thermal load rather than the load itself?

DR. CORDING: Of course there's some discussion as to what levels you're talking about in terms of thermal loading, and certain of the models certainly you're talking about the refluxing taking place from hot and cold areas, so it's in space as well as in time.

Any other comments on that?

MR. STATTON: Yes, again I heard gasps. I think one of the things that accompanies this poor description of high and low loads is what is required to be an accompanying vision of volume. A low load indeed perturbs what ambient conditions are, but may perturb those conditions over a very small volume of rock, by comparison, and hence mobilize a very small volume of water, which allows in fact a very small volume, some percentage of a very small volume, to be available to return.

Part of the thing that came with this vision of coalescing heat in this entirety of the mountain in the scale of square miles was the quick calculation that said even at some low porosity and moderate saturation I could end up with acre feet of water if I extrapolate the area long enough so that I get this sense of large volume of water perturbed over a large area, but nonetheless a large volume available to return. So that as I focused the return in my mind, saying that I dried water off through the matrix and I bring it back through a single fracture, that large volume becomes problematic to me.

But I think that's part of the coupling in Tom's description of high and low load, is kind of this volume that's being perturbed, the volume of water that's indeed being mobilized. Unless we're looking at a current state in

Yucca Mountain that we consider to be totally out of equilibrium, a fairly small volume of water in the low disturbance sense to have to deal with.

Those are the two things that I think we need to couple as we talk about high and low load, is we need to think about both the area and the volume of water that accompany that.

MR. FRISHMAN: And that's the first step of the cool down, first step of consequence of cool down.

DR. CORDING: Dennis Price?

DR. PRICE: It occurs to me that if you start--Steve Brocoum maybe can comment on this--if you start with a low thermal and establish the footprint necessary to support the low thermal, then later, as you contemplate if you might do the switch to the high thermal, you've laid the foundation for a higher capacity repository. Similar to reracking a pool, you rerack the mountain. Has this occurred to DOE, and can you comment on it?

DR. BROCOUM: Yes, ideas like that have occurred to us, and we've talked about expansion areas for a long time. I even think we had it in the 1986 Environmental Assessment. So yes, the idea of expansion area is not a new idea. I mean, we've always contemplated we may have to go to expansion areas. But we're trying to get away from talking, as I indicated today, as we did a few months ago, from starting low and moving up. We're trying to evaluate the whole issue, come up with a strategy and come up with a range of design we'll cover, and then justify a thermal load based on the information we have at that time. So I wouldn't say it would be low or high.

DR. CORDING: John Kessler?

DR. KESSLER: Yes, getting back to the reduced volume for the lower thermal loadings, I think then the question that needs to be asked is, what licensing ease is there by reducing the volume? You've still got the effect going on. Now, okay, let's assume that it's a smaller volume over which this effect occurs for the lower thermal loadings.

The next question is, if that's part of your licensing strategy and you're going to that, it must be that the lower affected volume gives you licensing ease. So where is it? One option might be, okay, the boundary from which you have to start jumping off with your groundwater travel time calculations--and I don't even like the groundwater travel time criterion, but that's beside the point--there's one thing that perhaps you've gained by going to a lower thermal loading so you have a smaller affected volume. But I guess what I'm still focusing on is you still have to define the effect and you still have evaporation, condensation, refluxing going on at the higher and the lower, and trying to define that, quantify it to NRC's satisfaction is my concern about whether there really is that licensing ease that occurs when you go to lower thermal loading.

DR. CORDING: John Greeves?

DR. GREEVES: Yes, it comes a little bit to what I call a credibility issue also. As I opened with my remarks, I was somewhat encouraged to see this new concept of Maximum Design Thermal Load, because I was frankly having trouble coming in the door with a low load and, you know, everybody watching this process and switching to some higher load at a later point. I think you need to come in the door showing what the constraints are. If you really are thinking of going to that higher load, I think as a licensing entity I need to know that coming in the door. As much as I'd like to have DOE pinned down on some of that, I have to live with that a little bit, so that's a comment.

But as far as even with this low load concept, I think as Dan Bullen noted, when you get with these MPC concepts, you really have high loads in a point location. And with that, let me ask Tom a question. When will we have data for an MPC scale thermal loading test? I see your charts, but I can't read them, the scale is too crude for me. Because I think that's where the credibility starts coming forward, when you put in place about an MPC scale test and get real data from it. But where are we in the test taking process in terms of that kind of data?

MR. STATTON: An MPC scale test says, at least in the way I conceive of that--and I'm speaking for a significantly larger testing community--starts with an opening, nominally, of our emplacement scale and a heat source that will nominally drive us to some 200 or thereabouts degrees at least for the MTDL at those boundary conditions. Not only the test then needs to be measured in its duration in years, if we're taking a look at the total cycle, because of the volume that we're trying to heat.

So as rapidly as we can get access to create an excavation, create an excavation and import the heat load to begin that test is indeed part of what our strategy is about. I mean, that's precisely where we're headed. Given that that test in its complete cycle, even slightly overdriven, is going to take on the order of years, like five, six, seven years, to heat and then cool and see what goes on, the output of the cool down portion of that test is not in the near-term. One, I don't have access, two, I don't have the opening, three, I don't have the heat source, and four, I haven't started to heat some volume of rock.

I think the output of a test like that, however, in the vicinity of the 2001 license application time frame, can be available in terms of the heating part of that cycle. That does a number of things for us. It doesn't do everything we'd like to do with the reflux portion of the equation, but it does a great deal for us in the demonstration that an opening is one, stable under that heat load, two, behaves nominally as our conceptual model has predicted, and three, allows us to track, for example, the way our hypotheses are developed for the exit of water away from the heat source.

That's the kind of assurance, I think, that we bring to the table at that point in time. Will we have a full-scale emplacement drift test run by 2001 through the entire thermal cycle that we would like to run it without overdriving the system significantly such that serious things in other parts of that test might be compromised? I think that's not possible to do. But I do believe that a significant portion of that test will be available to underpin the model that is the description of that behavior by license application time.

DR. GREEVES: So I take it that what you're saying is the ramp up portion of it will be available. That's not a comment on an analysis of that date, that's just a comment on one of the datas available. And that's the difficulty I think the regulator has in evaluating this process, because the real full scale of an MPC type environment, that data probably is not going to come in and be analyzed until 2005, something like that.

MR. STATTON: Yes, but I think we want to be careful when we push off what we call the analysis too far. Given that in our scientific approach to that exercise there are forward calculations that predict behavior, that's a model. That says, given this heat source, I will now describe the behavioral patterns that we're going to map. Given that my observations throughout that process match that forward calculation, then I think we have much more than no analysis of the data as we've gotten it. It says the analysis that was made in the forward calculation in fact is correct analysis, or at least nominally results in a correct behavioral consequence. So I think no analysis is not the right arena to put that in.

DR. GREEVES: I understand what you're saying. Thank you.

DR. CORDING: Okay, Don Langmuir?

DR. LANGMUIR: Just a thought question for John Greeves. I sensed your reluctance and perhaps NRC's reluctance, and I can appreciate this, of accepting a license application which has several pieces to it or a continuum of suggestions to it in terms of options from low to high. And I can appreciate how complicated it would be to accept that as a submittal. But we all know how complicated the Yucca Mountain system is, that unlike any power plant you're dealing with a small fraction of the system that's engineered and so can be predicted as engineered, and a large piece that's geological and totally unpredictable and will remain that for some time, at least to a good extent. And I wonder just how flexible NRC can be to a proposal for licensing which may well be in pieces, parallel pieces if it works the way DOE would like it to, presumably all documented very well, but several parts and obviously a lot of work to deal with as an agency. My sense is that's what's perhaps going to be needed, that openness, that willingness to deal with that kind of a submittal. What are your thoughts on that?

DR. GREEVES: Well, I think you're sensing properly that I'm concerned about how to deal with that, and as you and everybody around this table has said, this is a unique process. It's unique in a number of ways. It's not like the reactor business. If you think about it, we're actually putting a tremendous investment in this thing before the license application.

Also, as Tom pointed out, the data is a continuum, it's not really punctuated the way you see these milestones on a chart. I'm a regulator, I've been a regulator for a number of years. It creates dilemmas for us as to how to think that process through, because there's going to be a lot more data in 2001 than there is in 2000, and they're going to start writing this license application at least a year before 2001. All these things make the regulator's life complicated, and I stress we need to have a credible process.

Let's put it all out there in front of us. Let's don't be switching things around at 2008 and come up with a new idea. If you've got that, let's get it out there, let's keep it credible. And I will commit to you that we are thinking about what is the flexibility, and there are no guideposts out there to help you. There are some bad experiences with piecemeal license application in other arenas. So I am aware of those, and there are bad experiences where you have large uncertainties, too. You present a licensing entity with a large uncertainty, and on occasion they tell you, "You didn't pass go," in other arenas.

So I think we have to come to meetings like this. The NRC has to do some unique activities. And again, I mentioned in my opening remarks that I don't think there's time here to discuss how we're going to do that. We've done that in another forum and I'd be happy to come back and talk to you about that later. But there's some unique regulatory challenges here. I think it's what I call a healthy tension. I try and get Steve Brocoum to tell me what the thermal loading strategy is to make my job easier, because I have to write these review plans. So I think this is a good forum format to tease these issues out.

I don't have a total answer to you. Are we going to be somewhat flexible? I'd say yes, we have to be. The reality is, as Tom Statton pointed out, we're not going to have the complete information on these what I call room-scale tests, which I think are going to be a key to the real decisions.

DR. CORDING: Thank you. Steve?

MR. FRISHMAN: I think we could probably all very quickly write down the list of reasons why this dilemma is even on the table. And maybe it comes back to a real simple question that was sort of behind my statement to the Board the last time we talked about this suggesting that two MPC's per acre would not be a reasonable proposal to go to Congress and ask $40 billion for. Now, maybe the Department, in the course of its thinking through this, needs to make some kind of a decision on its own from a policy standpoint on just what's the least amount of spent fuel capacity they would find feasible for Yucca Mountain. And once you know that number, you can adjust cool down, you can adjust whether you need to look at expansion areas. But you know, make the decision. How much is Yucca Mountain worth to you?

DR. CORDING: Steve Brocoum?

DR. BROCOUM: I'd like to answer two questions here, John and Steve's.

DR. CORDING: Please.

DR. BROCOUM: First of all, we have had several versions of the annotated outline. We just issued the latest version. The first one has the DOE name on it. Next January we'll issue the next version, which we have full passthrough. By the time we get to 2001, there should be no surprises on the NRC's part as to what's going to be in our license application, because that will turn into our license application.

My greatest concern, though, is, and has been for some time, as to exactly what our position will be in 2001, a lot of these issues that we're discussing around the table, and how we can make the case that the NRC can find with reasonable assurance. If one were inventing a new licensing process and we didn't have this one, we might want to think about phase licensing, because basically the hearings and a fundamental decision that it's okay to go forward happen, in this schedule, by 2004. So the NRC is essentially saying you're okay, and yet we're going to go for numerous years constructing and then another 100 years operating and we'll certainly have a lot more information at any of those steps than we will have in 2001, 2004. So you're making a key decision whether it's 2004 or 2010, before you're going to have a lot more information in the future. I mean, the real decision is at the end, when you decide to close it. But the licensing process isn't constructed that way.

The thing about what capacity makes a viable repository, I think Dan was pretty clear today in his statement. He said it has to substantially handle the problem we have at hand. He has said that before, and I asked him directly, how many metric tons is that? He didn't give me an answer, but I walked away from that conversation with him that it has to be near 70,000 metric tons. I think you got an answer from Dan this morning when he made his statement.

DR. CORDING: Yes, Tom Statton?

MR. STATTON: There's--and maybe it's just my perception--perhaps a misunderstanding when we talk about expansion areas, what that entails and what that entails in terms of data being available. There are formally expansion areas identified about the region of Yucca Mountain, adjacent to Yucca Mountain, and to my knowledge they're kind of pensioned off into individual expansion areas given numbers based on some criteria. But to my knowledge, there isn't a single one of those without data in them today. I mean, each one of those has an existing data set for it. Now, it is not a data set that was collected post approval of the DOE QA program in 1990-91. But nominally each one has drill holes in it, has regional geophysical data sets associated with it, has an understanding of the regional groundwater table because its still adjacent area in the same region, has a very clear understanding of the regional tectonics because its adjacent area in the tectonic framework. Those expansion areas are not blackholes of knowledge at all. There are deep drill holes, they exist in those.

I think part of what we're trying to say is there would be some additional data that might want to be collected under the existing QA program, but it nominally supplements a data set that exists within the system today. So this isn't launching off into virgin territory at all. One would like to have perhaps a more current set of geotechnical information that describes the framework that we're looking at, but we're looking at similar rocks, similar structure, similar region. We just happen to have some drill holes that were drilled prior to the approval of the Quality Assurance Program.

DR. CORDING: But the plan would be now to have additional testing and borings in those areas from perhaps what you would have anticipated for the porkchop proposal?

MR. STATTON: Yes, I think that's what Steve was alluding to when he said nominally we don't know whether there's that one or ten, but clearly it wouldn't exceed the data density in the region we're already looking at. And frankly, that data density is not all that great within the porkchop itself for a whole variety of reasons.

DR. CORDING: At this point, we've been broadening the discussion to some extent to the entire waste isolation strategy, but we might consider also the Calico Hills issue and any comments you'd like to make in regard to that.

I think one thing that I've been observing in the program is that when I think of site characterization, I can think of it in the narrow term of going in there and finding the index properties to the materials that are there and making sure that we know pretty much what the geology is. But the program on which we are embarked here is much more than that, it's much more than just characterizing and obtaining parameters for rock properties or groundwater flow properties, geochemical properties of the underground. And we're looking at trying to understand basic phenomena here in the unsaturated zone and learning a lot about what models are applicable. And so there's a tremendous amount of investigation that is having to go on concurrently with trying to evaluate or characterize a site. There's a tremendous amount of effort that has to go into trying to understand some of these basis phenomena.

I think a couple of the items that really seemed to me in terms of the geoscience issues control is the matrix fracture interaction. And there's been a tremendous change, I think, in thinking about that interaction in the last few years, and that's something that's superposed here on this process of trying to get the license application, is that you were still developing these models and trying to understand what happens. And then part of that in addition to that that's related to the flow in the Calico Hills but it's also related to this thermal refluxing issue and the last testing was in 1989, and that not only provided some information on thermoconductivities and parameters, but it provided some insight into behavior and into the models that there's been a lot of development of the models subsequent to that and trying to understand the thermal phenomenon.

I think that that's part of what's still having to be done here on this project, is in getting down and getting these tests started. It's not just to fill in the blanks in the few models, it's to really understand the behavior. And the models and the testing in the field have to go hand in hand to try to develop that. And looking at how much progress has been made in the thinking in the last five years, there's still, to me, a long way to go. And at the same time, we're trying to get to the point that John Greeves wants when he says, "Look, I want to have something that we can license." So you talk about a good tension here, I think that the program is focusing on primary issues, but there's a lot to be done here.

So perhaps now we could go and broaden our discussion to the discussion of the overall strategy and perhaps talk about the multiple barriers in the Calico Hills. So any comments on that before we close our session in perhaps the next fifteen minutes?

DR. BULLEN: This is actually a question for Richard that I saved from the previous--I'm Dan Bullen from Iowa State. You mentioned in one of your viewgraphs that you had done a PA study, the purpose of which was to evaluate the impact of Calico Hills' conditional failure modes and property uncertainties on the system performance, and you gave us a little it of data from the 10,000-year cumulative release results, but you alluded to the fact that maybe you had already done 100,000-year cumulative release, 10 and million-year individual doses, and the 1,000-year groundwater travel time. In light of the fact that you alluded to those types of data, do you have any viewgraphs ala Tom Buscheck hidden away that you might be able to pull out and share with us?

MR. MEMORY: Well, maybe Tom has those viewgraphs.

UNIDENTIFIED SPEAKER: He does.

MR. MEMORY: Yes, I'm sure Tom does. I don't have any with me. I can summarize very easily the 100,000-year results, and that is that with the 100,000-year releases, the difference between the good Calico Hills and the bad Calico Hills was made very small, it was reduced greatly. The dose results I don't remember. Dave Sevougian might be able to expand on that.

DR. BULLEN: Could you just explain to me how you came up with the criteria of what was good and what was bad in the Calico Hills determination? How did you go about that?

MR. MEMORY: Well, you mean how did we come up with the 90th percentile?

DR. BULLEN: Yes, how did you come up with 90-10? I'm very interested in the process, because I think the process can be an important tool. And I guess as such I'm asking you sort of the hard questions as to why did you pick this number, how did you do that?

MR. MEMORY: Why did we pick 90-10 versus something else? I better late Dave address that.

MR. SEVOUGIAN: Good question. Is Bob Anders here? Okay, sorry, Dave Sevougian. I'm with PA, M&O. We picked the 90th percentile and 10th percentile of the distributions of Calico Hills properties--as far as I remember it, it seemed like as good as anything to pick. I mean, we had a normal distribution, you know for the properties.

DR. BULLEN: No, I understand that, but I guess the follow on question was did you do any sensitivity analysis to see whether or not 70-30 gave you significantly different results or 50-50 gave you significantly different results?

MR. SEVOUGIAN: Well, 90-10 didn't show much difference on the releases, so that seems--I mean, maybe if we picked 95-5 it would have showed a little difference, I don't know. The tails of distribution didn't go out that much farther.

MR. MEMORY: We might have two questions going on here. One is the 90th percentile in terms of the parameters.

DR. BULLEN: Right. That's not the question I asked.
MR. MEMORY: Right. The other one is the fracture flow.
DR. CORDING: The ratio of fracture to matrix flow.
MR. SEVOUGIAN: Oh, okay, sorry.
DR. BULLEN: I'm very interested in that, because I think that will have a significant impact on the performance.
MR. SEVOUGIAN: Sorry.
DR. BULLEN: If you flush it all out real quick, then you're going to get a dose.
MR. SEVOUGIAN: Right.
DR. BULLEN: If you basically take into account that there may be some matrix fraction or a fraction of flow in the matrices, then you'll have a significantly different result.
MR. SEVOUGIAN: Right.
DR. BULLEN: And so the question is, how did you come up with, first, is it a critical parameter? And if it is, then how did you decide what the fraction was, and did you do a sensitivity analysis of that?
MR. SEVOUGIAN: I did not do a sensitivity analysis. I did not have time.
DR. BULLEN: Okay.
MR. SEVOUGIAN: I picked 90 as--I just picked it out of the air.
DR. BULLEN: Okay.

MR. SEVOUGIAN: I didn't do 100 or any other number. It might be useful to try that.

DR. CORDING: That's 90 percent of the flow that's coming into the--

MR. SEVOUGIAN: Into the Calico HILls.

DR. CORDING: --Calico Hills?

MR. SEVOUGIAN: Or into any of the other units. Ninety percent of the percolation, which let's say it's a half millimeter per year flux, then .45 was forced through fractures with a very small porosity. So it essentially goes through instantaneously on the scale of these performance assessments.

DR. BULLEN: So based on those results, would you say that it's important for us to know what fraction goes into the matrix and what fraction is in the fractures?

MR. SEVOUGIAN: Well, if it was ten percent, yes, it would give a lot different result if it was ten percent.

DR. BULLEN: I'm kind of leading you through this because I want to know if performance assessment is going to be a good tool to decide what experiments and what characterization we need to do. Do you think it will or won't?

MR. SEVOUGIAN: Can you expand on that a little bit?

DR. BULLEN: Well, basically what you've identified is a very important parameter, because when you had completely matrix flow, you didn't have a big effect. But when you made a partition between the fracture and the matrix flow, you had a big effect. And so doesn't that tell you something with respect to the usefulness of your performance assessment tool?

MR. SEVOUGIAN: Okay, we tried to make the Calico Hills fail. He said we just had a lot of smoke, but we tried our best to see, you know, if we could make it fail.

DR. BULLEN: I agree, and I think that's what you did, and I'd hoped you would have said that to say that "what we did was to pick parameters that told us something and looked bad so that maybe we should decide whether or not it's worthwhile to characterize Calico Hills." And I didn't see that in the presentation. If the presentation had said, "We want to do a performance assessment analysis and we want to look at the critical parameters, and, oh, by the way, one of the critical parameters might be fracture flow," and if fracture flow did indeed in our performance assessment come up to say that yes, it's an important parameter, then maybe we should go look at Calico Hills and see how much fracture there is.

I'm off on a tangent, I realize, but what I'd hoped was that in the presentations you would say, "I want to use this tool of PA, and then actually, after I've used the tool and I see the results, can then I direct where I'd go with my limited resources?"

DR. PRICE: I don't think that's a tangent. It got very much to the point.

MR. SEVOUGIAN: If you want to look at the dose, the dose was very significantly higher. The releases were still less than the remanded limits. The dose was very much higher. Then this one-third background thing. So if you're looking to make a case of the Calico Hills as an important barrier, defense-in-depth, then that would say to me that yes, we need to look at it, because if it fails to this extent, then we could be in trouble.

DR. CORDING: I'm just wondering just to what extent our--and we've had discussions of this before, but to what extent are the models that we're using in performance assessment adequately considering the fracture matrix interaction? And obviously the answer might be not enough, and it's very difficult to come up with any model that does that well. But it seems to me that that's an area that needs to be looked at when we're making these decisions, because I think in the last few years there's been a lot learned about fracture matrix interaction that wasn't even considered five years or so ago in the performance assessments. Any that were done at that time would have given you a very unrealistic view of the behavior. So I guess that's a question of mine, is to what extent are we going to be able to improve these models? First of all, the models that we're using that maybe aren't part of the performance assessment, but also then how do we integrate that into performance assessment? Where do we stand with that and how much more progress do we need to make?

DR. DOMENICO: This is Domenico on the Board. With regard to what Ed is saying, I recall perhaps a year ago DOE had some plans to go to NRC to check out, get their approval, if you like, on the utilization of these what we might call simple models that are used in performance assessment. Is there any result on that? I think I heard that from Jean Younker one time. Going to NRC to see if they would accept the results from the simple modeling that's going on, do you recall those discussions? You don't recall those discussions.

DR. LANGMUIR: Can I rephrase the question? Langmuir. You've used the WEEPS model and the composite porosity model in the TSPA analyses that were published in the last year or two, and those have focused on emphasizing fracture flow or matrix plus fracture flow. Are those basically the models you will go to NRC with at the time of licensing, or will you be modifying your approach, simplifying the model approach that you provide in the license application? Maybe that's a rephrasing of the question.

DR. BROCOUM: Is Abe Van Luik around? Abe Van Luik is the guy we need for this right now. I guess he's not here.

DR. YOUNKER: This is Jean Younker, M&O. I'm certainly not qualified to tell you how we expect those models to evolve before licensing, but what I would propose for you is to get either--I think Bob Anders is the right person to answer it. Maybe what we can do is during your open session tomorrow, if we could defer that question and have him just answer that particular question as a part of tomorrow's, I think that would probably be wise. I could give you my view of it, but I'd rather have Bob, who's really responsible for managing that, tell you what his concept of it is.

DR. BROCOUM: The reason I mentioned Abe is because we went past a fairly detailed guidance and performance area to get to the M&O for their planning next year. It really focuses more on the process models, the low level process models, so we can get those in place and try and make sure that each is under development and will be delivered as needed and we know who's accountable for it. He's not here.

DR. CORDING: All right, let's have that tomorrow, then.

All right, Don Langmuir had another question.

DR. LANGMUIR: Totally unrelated question which I'd be interested in answers from several people here on. We heard, I thought, an interesting presentation from Rosa Yang and John Kessler and from Steve Kraft, and one of the things that came out of it was the suggestion that the NRC consider a release standard that was up to 1,000 years, and then go to potentially a dose standard after 1,000 years as a basis for deciding on repository performance. And I wonder, this strikes me as probably total novice at this sort of thing, but it strikes me this would be a very original kind of a submittal if NRC was to receive two kinds of standards in one proposal for a license. Is there any precedence for this sort of a thing? Rosa's got her hand up here, too. Maybe she'd like to modify my question.

MS. YANG: If I can modify your question somewhat, I think we're really going for a dose standard. In fact, it's a risk standard. But we want the defense-in-depth, because our basis is you prove something with a very rigid standard in the licensing process, and you can't really use dose for that short a time period. So in effect we are really adding that to the substantial complete containment.

And if I could go off on a tangent a bit, I want to react a bit to Dr. Greeves' opening statement regarding the subsystem criteria versus the multi-barrier system. I think both Steve and I said again and again that we believe in defense-in-depth, and in fact the whole repository concept is multi-barrier. You have multi-engineering barriers and multi-geologic barriers. But we just don't think it's productive in the licensing arena to use subsystem criteria. It's not inconsistent with the multi-barrier concept, it's just, you know, in the licensing arena, you should look at the overall system, because that's what you're after is the public health and safety. It's not a groundwater travel time or any subsystem requirement like that. So that's a modification.

DR. CORDING: John and then Steve.

DR. GREEVES: Me first?

DR. CORDING: Please.

DR. GREEVES: It's interesting, I listened to the presentations this morning and there's this concern about using subsystems. I find it a little confusing, though, to see the approach bring back one of those subsystems, which is substantially complete containment. Can you have it both ways? But we don't need to talk about that. It just was a little bit interesting this morning.

Like I say, the total system performance assessment approach is one we are comfortable with. I personally would look for multi-barriers, and I think Rosa said that they support that approach, they just don't want to see it in licensing space. And I understand that. If the legislation were to come forward and say to go that way, the NRC would fall in line and go forward with that.

DR. LANGMUIR: I guess part of my question, though, was how comfortable are you with a license application in which there are two kinds of standards?

DR. GREEVES: You mean the deterministic approach?

DR. LANGMUIR: A release standard for the first 1,000 years and a dose-based standard after 1,000 years.

DR. GREEVES: I think Rosa clarified that they're both dose-based standards.

MS. YANG: Right.

DR. GREEVES: And yes. The only difference between the two is there's a prescription of the first one being a deterministic approach and the longer term being probabilistic.

DR. LANGMUIR: That's what happens when a geochemist messes up in things like this.

MR. HANAUER: I have something I wasn't going to say, but I guess Rosa Yang has pushed me into it. I'd like to point out a disconnect between two sentences in what she just said, which I think is important. And that is that the idea of using only total system performance assessment is directly contrary to the idea of defense-in-depth, and that you probably have got to allow yourself a certain inconsistency. My background till November was in nuclear power plant safety, and I'm a strong believer in defense-in-depth. This means that you acknowledge in your evaluation that you don't know everything and you provide echelons of defense to guard against failures, lack of knowledge and understanding of one or the other of your barriers.

Now, this is directly contrary to how you do total system performance assessment, in which you calculate the consequences of scenarios and calculate doses or risks depending on just how you do it, with the idea that everybody known it's true that you know everything. And if you do this very well and very honestly, you put in allowances for uncertainties.

Now, in fact, in this repository we are even less likely to know everything than in nuclear power plant safety, and there is one important lesson which occurred about sixteen years ago when the Three Mile Island Unit 2 was melted. The available total system performance assessment was called the Reactor Safety Study, and it modeled not Three Mile Island Unit 2 but another pressurized water reactor that was thought to be somewhat similar but in fact later was discovered not to be. When the sequence of the accident began to be understood, the practitioners of TSPA of that time, namely the Reactor Safety Study folks, said, "Oh, yes, that's sequence TMLB prime, it was in our study, and weren't we smart."

But in fact they weren't smart at all, because the events which occurred in the Three Mile Island accident, the inadequate procedure which resulted in the valves being left closed, the inadequate understanding by the plant staff which resulted in a correctly functioning emergency core cooling system being turned off, and various other errors of commission and omission which should be charged not to the plant operating staff but to people like me who in their offices didn't provide a system which was more oriented towards success. These occurrences were not modeled in the TSPA of the day, and therefore that TSPA didn't provide a matrix to understand that accident. Now, that tells us not that TSPA is bad, it's very good and very useful, but that it it is the beginning of understanding of safety and not the end and that failure to provide requirements for subsystems is throwing defense-in-depth out of the licensing process, contrary to assertions that were made this morning.

DR. CORDING: Thank you very much. We have, I think, reached the end of our period here. I appreciate the comments, and that's a good note, I think, to close on. I appreciate the presentations by the speakers today, they were helpful to us. And just would like to inform you that we will recess till 8:30 a.m. tomorrow morning. Thank you panel and speakers.

(Whereupon, the meeting was adjourned, to reconvene at 8:30 a.m. on Thursday, April 20, 1995.)