NUCLEAR WASTE TECHNICAL REVIEW BOARD

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PROGRESS WITH YUCCA MOUNTAIN EXPLORATION AND
TESTING AND THE UNDERGROUND REPOSITORY
CONCEPTUAL DESIGN

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Days Inn Crystal City
2000 Jefferson Davis Highway
Arlington, Virginia 22202

Wednesday, October 9, 1996

The Board met, pursuant to notice, at 8:30 a.m.

BEFORE:

EDWARD J. CORDING, ACTING CHAIRMAN
CLARENCE R. ALLEN, BOARD MEMBER
JOHN W. ARENDT, BOARD MEMBER
GARRY D. BREWER, BOARD MEMBER
JARED L. COHON, BOARD MEMBER
DONALD LANGMUIR, BOARD MEMBER
JOHN J. MCKETTA, BOARD MEMBER
JEFFREY J. WONG, BOARD MEMBER

PARTICIPANTS:

PATRICK A. DOMENICO, CONSULTANT
ELLIS D. VERINK, CONSULTANT
WILLIAM D. BARNARD, TECHNICAL STAFF
SHERWOOD CHU, TECHNICAL STAFF
CARL DIBELLA, TECHNICAL STAFF
DANIEL FEHREINGER, TECHNICAL STAFF
RUSSELL MCFARLAND, TECHNICAL STAFF
DANIEL METLAY, TECHNICAL STAFF
VICTOR PALCIAUSKAS, TECHNICAL STAFF
LEON REITER, TECHNICAL STAFF
MICHAEL CARROLL, STAFF
HELEN EINERSEN, STAFF
LINDA HIATT, STAFF
FRANK RANDALL, STAFF
VICTORIA REICH, STAFF

P R O C E E D I N G S

[8:30 a.m.]

DR. CORDING: We need to assemble so we can start our session this morning.

Good morning. My name is Edward Cording. I'll be chairing this session of the meeting this morning. It's my pleasure to welcome you to our fall meeting of the Nuclear Waste Technical Review Board.

Our Board Chairman, Dr. John Cantlon, who was also Chairman of the Woods Hall Research Center Board, was unable to join us today due to an overlap in his chairmanship duties. So I'll be chairing the session this morning and throughout the day.

I am pleased that you could join us and we're looking forward to some very interesting presentations. Much is happening in the program. We're looking forward to hearing about that and discussing that with the DOE.

A couple of announcements, administrative issues. There are passes for your automobiles that are available in the back of the room. So if you would put those on your cars so that they don't get towed. Apparently, the Days Inn requires you to have some sort of identification on your car. So these are available to all of you who have driven to the meeting today here and are parking in the Days Inn lot.

As most of you know, the Board was created by

Congress in 1987, in the 1987 amendments to the Nuclear Waste Policy Act. The Board is charged to independently assess the technical and scientific validity of DOE's effort in designing and developing the nation's spent fuel and high level radioactive waste management system, including site characterization at Yucca Mountain.

My field of expertise is in geo-engineering and I am Professor of Civil Engineering at the University of Illinois at Urbana-Champagne.

I'd like to introduce my colleagues on the Board. They're Clarence Allen, Professor Emeritus of Geology and Geophysics at California Institute of Technology. John Arendt is a specialist on the nuclear fuel cycle and transportation of radioactive materials. Garry Brewer, who is one of our Board members, will be joining us. He's Professor of Resource Policy and Management at the University of Michigan.

Jared Cohon is Dean of the School of Forestry and Environmental Studies at Yale University and he'll be joining us shortly, as well. We have Don Langmuir, Professor Emeritus of Geochemistry at the Colorado School of Mines. John McKetta, Joe C. Walter, Professor Emeritus of Chemical Engineering at University of Texas. Jeffrey Wong, Science Advisor to the President -- excuse me -- to the Director of the Department of Toxic Substance Control in the California Environmental Protection Agency.

Past Board members who are now serving as consultants pending their reappointment or replacement are Ellis Verink, also at the table, distinguished service Professor Emeritus of Metallurgy at the University of Florida. And Pat Domenico; Pat is David B. Harris Professor of Geology at Texas A&M and his specialty is hydrogeology.

Also with us is Richard Parizek, who is Professor of Geohydrology at Penn State, and he is a consultant to the Board.

Board staff is with us today and I'd like to introduce Bill Barnard, the Board's Executive Director, and others on the staff are with him.

Over the last years, we have witnesses considerable progress in the DOE program. We've seen the restructuring and focusing of the OCRWM program, the development of a program plan, with a goal to completing the viability assessment in 1998 and recommending the site to the President in 2001.

We are all aware that this is a time of political regulatory and funding uncertainty for the repository program. However, we are also in the midst of a very large increase, expansion of information, increase in the scientific data available to improve our understanding of the mountain, Yucca Mountain, and those processes that are critical to assessing its performance and assuring the safety of the repository, particularly in the areas of the structural characteristics of the mountain and the hydrologic implications of those characteristics.

Some of the things that have been accomplished and are underway. Tunneling has been extended down to repository level and north-south along the entire east side of the intended repository area. It's providing access to faults on the east side of the repository and a platform for further exploration across the site.

The thermal test area is being developed. The first phase of the initial testing, a bore-hole heater test, is underway. Results from isotopic studies, such as chlorine 36 sampling, are being obtained along the tunnels to evaluate the flux and flow characteristics of the mountain.

Today and tomorrow we will be concentrating on the engineering and scientific side of the program, particularly the plans for fiscal year 1997 and beyond. We'll be looking at the new scientific data, what it means for the program, and we'll be discussing some of the aspects of the design and operation of the repository.

We are very pleased to have with us this morning Dr. Dan Dreyfus, who is director of the program in the Office of Civilian Radioactive Waste Management program, and the architect of the changes to the program to which I've just referred.

Dr. Dreyfus will review plans and priorities for fiscal year 1997, the year which started approximately a week ago, and will be discussing aspects of the program, including the viability assessment.

Dr. Dreyfus, we're very pleased to have you with us. We're pleased to join you here in Washington and discuss with you issues that you are pursuing as director of the program.

DR. DREYFUS: Thank you. I'm always glad to have an opportunity to meet with the Board. And it's fortuitous when you meet in Washington because I find that it's hard for me to get out of Washington. I'm almost afraid to leave Washington these days.

I would like today to give you a brief update on the status of the program. You will be having a number of presentations from members of my staff about particular things you've asked us to talk about at this meeting, and then to address the concept of the viability assessment, which I think perhaps needs some additional discussion.

When I addressed the Board last, the program was indeed in transition, as Professor Cording has said. We very nearly lost the program, as we know it, a year ago with the budget reductions, and, of course, what we have been doing over the past year is figuring out how to restructure it in accordance with the obvious directions that we're getting from the policy community.

We have revised the program to manage the 1996 funding reduction and we have been able to regain a strategy that I think is consistent with the realities of the budget and with the Administration's policy for the program.

We published a revised program plan in May of 1996. It was supported by the President's fiscal year '97 budget request and it has now been endorsed by the Congress in the subsequent Appropriation Act that came out a few weeks ago.

During the past year, we were able to make substantial progress in constructing the exploratory studies facility and carrying out other aspects of the program at Yucca Mountain, despite the disruption of the large downsizing that was required to manage an unanticipated funding reduction.

Because of the reduction, we were not able to optimize the 1996 activity. During the year, we needed to manage cash flow on a daily basis. The Federal system does ont permit you to miss the funding goal on the upside. You can only miss it on a downside. It's a jailable offense to spend more than you've got and you can't go get some more.

So we had to watch that very closely and until well into the end of the fiscal year, we were not certain about termination costs or we were not certain about cash flow. So I would not call it a -- I would not call it an optimum year. We had to forego a delay of some expenditures that would have given us greater production at the mountain.

However, we didn't do bad. The tunnel boring machine passed through the repository are and the turn toward the south portal and is now on its way out of the mountain.

We will be gaining additional important information in what is the last mile of tunneling. That is, I call to the attention of the Board, an east-west direction, although not in the repository area itself.

A greater emphasis, I think, in the next year will be on the penetration of the Ghost Dance Fault, the heater tests and other aspects of data collection in the ESF, in the repository formation.

We began a small-scale single element heater test on August 26. Construction of the two alcoves that will give us access to and ultimately penetration of the Ghost Dance Fault has begun.

In the fiscal '97 appropriation, we received $382 for the program. The Congress stipulated that no oversight funds ought to be provided to the State of Nevada and the units of local government. The amount they gave us is 18 million less than our request, of which 11 is associated it the state and county oversight programs.

That leaves us with an incremental reduction of seven million out of the money that we anticipated having for program activities, internal program activities, and the Congress instructed that that remaining seven million be taken from program management and cooperative agreements. And we indeed will take it from program management. It will, therefore, not impact the program plan with regard to the work scope that needs to be done, but it will impact other aspects of our activities, such as institutional work and management type expenditures. We will have to manage the program with considerable less contractor support.

You should also be aware that contemporaneously, we are reducing Federal staff. We have been able to meet -- the Department has a restructuring plan that was put in place by the Secretary a couple years ago, I think, now, and we were able to meet our fiscal year 1996 staffing target without involuntary separations in this program. Well, there are sizeable involuntary separations going on as we speak in other aspects of the Department.

Meeting our lower targets for the end of fiscal year '97 will not be easy. We may not do it through attrition and buy-outs and we may, in fact, have involuntary reductions in the coming fiscal year. From the approved Federal employee target that we had a couple years ago, we've about a 20 percent reduction.

Congress, of course, adjourned without completing legislation addressing the near-term management of spent fuel. This means, of course, that there is no authorization for siting an interim storage facility. In my judgment, completed legislative action on that issue is unlikely in the first year of the next Congress.

However, any scenario of interim storage or disposal is going to require a national transportation effort. The program has an approach, and will continue to develop it, for market-driven waste acceptance, storage and transportation effort. This will rely on a major procurement to essentially acquire the services necessary to mount the transportation effort in its entirety when it is needed.

The procurement activity which we are starting will provide a forum to resolve policy and institutional issues that confront transportation. It will also, I think, enlighten the policy-makers about the realities of an unprecedented logistical undertaking.

We also will complete a topical safety analysis report for a generic interim storage facility of the type that has been discussed in pending legislation. That will be a facility without regard to a specific site. The interactions that we'll have with the Nuclear Regulatory Commission regarding that topical report will also provide another issue resolution forum in which we can discuss with the Commission what will be required to actually license such a facility if and when a site is selected.

Our 382 million spending level will allow us to meet all of the fiscal year '97 milestones that are in the program plan. I have listed several of them in my prepared presentation, which will be available.

Some of the key milestones, in my mind, would be to daylight the tunnel machine, which is more symbolic than programmatic, but nonetheless an important accomplishment; penetrating the Ghost Dance Fault, which I personally expect to be revealing in many respects. We will be re-initiating the environmental impact statement activity for the repository, which we had to suspend for budgetary reasons during '96. I think that will be the focus of a great deal of attention and an important undertaking.

And we will initiate a rule-making on repository siting guidelines, the Department's repository siting guidelines, which will make them consistent with the program plan.

I would now like to turn my attention and your attention to an aspect of the strategic plan that seems to require more discussion, and that's the concept of a viability assessment in 1998.

Those of you who know me know that I rarely, if ever, use visual aids. An early mentor of mine referred to them as the crutch of the inarticulate, and they tend to distract the audience from what you're saying. So this is a diversion, but there is enough complexity here that I think a visual may help.

This slide illustrates the decision process leading to the development of a repository, and it's a complicated slide because it's a complicated process and that's one of the points that I want to make. It is a complicated, not a simple process.

Most of the fundamental requirements for that process are now set forth in statutes. They are set forth in either the Nuclear Waste Policy Act, as amended, which is the charter for this program, or the more recent requirements of the fiscal year 1997 Energy and Water Appropriation Act.

The Nuclear Waste Policy Act designates the Secretary site recommendation to the President as the Department's principal programmatic decision point. This decision point is a final agency action. It requires a completed EIS and it will be subject to a lot of external review.

In our revised program plan, we expect to achieve the site recommendation milestone in 2001. It's important to remember that the Secretary's responsibility prior to that decision is to evaluate the site. The act provides that the director and, thus, the program shall carry out the Secretary's function in this regard.

The act also provides that, and I'm quoting from the act, "If the Secretary, at any time, determines Yucca Mountain site to be unsuitable for development as a repository, the Secretary shall terminate all site characterization activities at such site, notify the

Congress and the Governor and the legislator of Nevada of such determination and the reasons for such termination," that's the end of the quoted part, and the act goes on to say, and within six months, recommend a new path forward.

Now, although this diagram presumes continuing progress to a license application and beyond, the Secretary currently is in the position of being the first judge of site suitability and may stop the action with a negative decision at any point and without external concurrence, and this is a profound responsibility that I keep trying to communicate to my staff and to external audiences.

The Secretary does not become an advocate of the proposed repository until a positive site recommendation decision is made, and that is made here. So until that time, this program is in judgment of this proposal and not an advocate thereof.

Now, the act details the requirements for site recommendation and they are complicated. Prior to any decision or recommended Yucca Mountain site, the act requires that the Secretary hold public hearings in the vicinity of the site, notify the State of Nevada of the proposed recommendation. The act also requires the Secretary to provide a comprehensive statement of the basis for the site recommendation and specifies in the act the nature of the information that has to be submitted, and that information is submitted both to the President and made available to the public.

The information includes a description of the proposed repository and the waste form or packaging and a discussion of the data from site characterization relating to the safety of the site. The act also requires the formal participation of external parties. The Department must seek comments of Nevada and other affected governments. The Nuclear Regulatory Commission must provide a preliminary comment on the sufficiency of site characterization analysis.

The Department must also complete the public process for the development of a repository EIS consistent with both the National Environmental Policy Act and the modifying requirements of the Nuclear Waste Policy Act.

If, following the Secretary's recommendation, the President considers the Yucca Mountain site to be qualified, the President will submit a recommendation of the site to the Congress, along with the information provided by the Secretary.

The President's recommendation has a complex set of requirements leading to either the acquiescence of the Congress or rejection by the Congress through inaction.

Now, there are, of course, very specific requirements that must be met for the license application to be docketed by the Commission and the Commission will be the arbiter of the application's adequacy.

Subsequently, we will have to support our application throughout the licensing process, which is certain to include adversarial intervenors and probably litigation.

So it's pretty clear that the site recommendation represents a solid and consequential decision point. The adequacy of the judgment made and the required supporting documentation are not set forth by the standards of this program. They must also meet the requirements of the act and survive the scrutiny of external participants in what will be an extended public decision process.

Now, to go back to my first diagram, the viability assessment is not the same thing. If it were, we would simply make it earlier by 34 months and save the public $850 million of additional work. Rather, the viability assessment is a step along the way. It is a management tool for the program and it is a major informational input to the policy process.

Based upon what is now a decade or more of data-gathering, analysis and conceptualization, which we have already completed, by placing the emphasis for the next two years upon the most significant remaining issues, we can, by September of '98, compile a comprehensive description of the design and operational concept for the repository. We can assess the performance of that concept in the geologic setting, which we know a great deal more about today than we did at the outset of the program.

We can accompany that conceptual presentation with a cost estimate, which will be much more meaningful than any cost estimate that has been created to date. And we can present a plan for the remaining work necessary to complete a license application, which will, again, be based upon a comprehensive concept of what we intend to do rather than a judgment about the real estate.

In my opinion, such a comprehensive description and proposal is essential for rational completion of the site recommendation. I think it's a priori need and it's a need to have it prior to the completion of the work so that the work will be completed appropriately.

We will not be finished with our evaluation of the site in 1998 and the Secretary will not be in a position to make a positive site recommendation in 1998.

Now, on the other hand, consideration of a comprehensive concept will, for the very first time, put many singular data points into a comprehensive perspective that we do not now have. This could become the occasion for a negative decision by the Secretary. If the compilation of the viability assessment does not result in a negative decision, however, no decision will be made, except the decision that we make every morning to come back to work and continue the site investigation.

If you will hark back to my earlier remarks about the obligation and responsibility of this program, for all practical purposes, we decided this morning not to stop and we will decide the morning after the viability assessment whether to stop or not.

Now, I think in the process of compiling the viability assessment, we will become enlightened in ways that we are not enlightened in the normal Monday or Tuesday morning because we will try to assemble the data in a meaningful, comprehensive way, and we may find that there are things we don't know and can't do. But if we don't, we are not finished. We are simply continuing.

The viability assessment, however, will give all participants a better comprehension of this venture and the significance of the data that we then have. It will give policy-makers information about the probability that a repository is a viable undertaking in ways that we do not have.

The President has stated that this information should be available before the siting of an interim storage facility is decided upon. The Congress has required the viability assessment to be completed in September of 1998, as set forth in the revised program plan. It is required by the Appropriation Act and it lists in the Appropriation Act the four components that will be required to be presented to the Congress and the President on that date. They include a preliminary design concept, focused on the more important, significant underground aspects of the proposal; a total system performance assessment based on the data then available; a plan and cost estimate for the remaining work required to complete a license application; and, an estimate of the cost to construct and operate the repository in accordance with that concept.

Now, as we implement the program plan, it's important that the distinctions between the viability assessment and the site recommendation be appreciated and preserved. Each has a purpose and I think each can appropriately serve that purpose.

If, however, we turn the viability assessment into a final go/no go decision, which begins to look like the site recommendation, then either we will be making the site recommendation decision prematurely and based on insufficient data or else we will have to delay the viability assessment and it will not provide the information to the decision process that the Congress and the President expects.

There seems to be some psychological need on the part of many observers to convert the viability assessment into such a formal final decision. That has not been done in any formal document. It is not so in the program plan. It is not so in the President's communications to the Congress. It is not so in the Congressional documents.

It keeps recurring, however, in casual

conversation and in informal written commentary. Now, I think it's incumbent upon those of us who are obliged to spend serious time on this program to try to keep this distinction clear. If we do not, we may find that misunderstandings on the part of policy-makers have become expectations and that the expectations have begun to dictate the character of the decision process.

It's not our intention to change that decision process, but that can happen in the policy process inadvertently or through misunderstandings.

So one of my reasons, one of my intentions here today is to ask the Board's assistance, in its interactions with the grander and greater community of interests on this program, to help us to keep these distinctions clear.

The program has put forth and is implementing a credible plan. The plan maintains momentum towards a national decision on geologic disposal options. I think that's what this program has been charged to do and is doing at this time. The program has the charge to bring to conclusion a national decision on whether we are going to go forward with geologic disposal at Yucca Mountain.

With continued funding, which is now at a more modest rate than in any previous program plan, and with an updated regulatory framework for the site recommendation decision, which is consistent with the program plan and enlightened by the data we now have, we can meet the schedule we have established for that decision process.

I thank you for the opportunity to brief the Board. I'll be happy to answer questions and I intend to stay here until the lunch, so we'll proceed how you wish.

DR. CORDING: Thank you very much, Dan. We really appreciate that. We're not sure whether it was the visual or the spoken part of your presentation, but it certainly, I think, provided us with a very clear picture of how you are proceeding and I'm very appreciative of that.

We have time certainly for discussion and questions from the Board. John Arendt.

DR. ARENDT: Since interim storage has been discussed at great length or is being discussed, I'm wondering whether -- what I'd like to know on that -- if you could put the chart up there -- whether it might not be a good idea to indicate on there when interim storage construction starts.

This is a question that is in the minds of many, many, many people and when I look at that viewgraph and if I'm interested in interim storage, it doesn't tell me anything.

So I don't say that you ought to do it, but I'm particularly interested myself as to when does interim storage start. Does it start in the year 2001 or does it start in 1998 or somewhere in between or sometime later?

DR. DREYFUS: Well, there are a couple of reasons.

First of all, that chart is entirely consistent with the current statutory basis for the program, which does not include interim storage in any respect, except in the notion that there could be interim storage associated with the actual construction of the repository.

So the interim storage concept, as it has been debated over the last two years, has no statutory basis, isn't there at all. The program plan is consistent with the Administration position, which says that there ought not to be a siting decision made until the viability assessment information is available.

In the program plan, we have assumed, because one must assume something, that a site would be chosen after the viability assessment is available. So we are, in our non-Yucca Mountain work, presuming a choice of a site in 1999 and, therefore, preparing long lead-time work on what such a facility would look like, how it would be licensed and how one would mount the transportation initiative, assuming the choice of the site in '99.

Now, if there were a site chosen in '99, then depending on what the act says, you have a lead time after that to building the thing. I have said that under existing law and without any specific special provisions of the act with regard to environmental impact statements or licensing, it would take four years from the site selection to the first transportation of waste.

Given some of the work we're now doing on the procurement, transportation procurement, one might say three-and-a-half or something like that. Some of the act, some of the draft bills and partially enacted bills had in there special provisions for contemporary writing of EISs and licensing and construction, which might have shortened that lead time somewhat. But under existing law, it's a three-and-a-half to four year operation from the time of authorization and site selection to the first shipment of waste.

DR. ARENDT: One other question. The May 1996 program plan that you're currently using, are you using that exactly as written or has it been modified or does it need to be modified in order to meet your financial commitments?

DR. DREYFUS: Well, not much. That plan was put together contemporaneously with the development of the President's budget for '97. It's consistent with the budget for '97. We got the money essentially, as I say. We will take the $7 million hit in management, but we will carry out the work plan in that program plan in '97.

We have requested the amount we need for '98. That budget cycle is only beginning. So we probably don't have to make very many modifications.

Now, we will be having a programmatic strategic planning meeting in about three weeks, two or three weeks, at which point we're going to look at several things. First of all, what actually was accomplished in '96? We went into '96 in kind of a state of disarray, as you may recall, with an unanticipated $85 million reduction.

So what we did in '96 is not necessarily what we said we were going to do in that plan and we'll have to adjust the program accordingly. The other thing is the program, we get smarter, oddly enough, each year. There is a learning curve and we have other modifications. So the end of this month, we're going to look at just that question, to what extent do we have to amend the program plan.

I see no reason why any major dates or target dates, high level target dates would change. The work plan may very well change.

DR. CORDING: Other Board questions? Don Langmuir.

DR. LANGMUIR: Dan, the Board, for its own benefit, because we like the word suitability, we've been trained by the DOE to use.

DR. DREYFUS: Yes, I have, too, and now they're training me not to.

DR. LANGMUIR: But we defined it as, among ourselves, representing a time when there was a high probability that the site could be considered suitable, could isolate high level nuclear waste for -- use the word to define itself, right? That there was a high probability of being able to isolate nuclear waste for a long period of time.

The way I read your chart up here -- I'm sorry that I like charts like this, as a professor, but I do. That stands -- that represents the site recommendation point in your chart, to me. Is that how you would view the old-fashioned site suitability concept? Is that about where we would be? Same thing?

DR. DREYFUS: I think so. I considered it at one point, but didn't suggest to the chart-makers that we shade this thing, because what you really have is a continuum.

What you really have is a continuum, but we know something now. We know something right now about the probability of success. I think when we hit the viability assessment, we will have a step function in knowing that only because we will have aggregated, for the first time around, a particular proposal where the performance assessment be hard-wired to a specific design concept and a cost estimate.

But it's a step function in a continuum. At the site recommendation point, the Secretary has got to come down on a kind of a go/no go decision. At that point, the Secretary makes a formal statement that the site is okay and that, I think, is where that situation occurs.

But bearing in mind that we will then be putting together data that the Commission thinks is necessary to convince them and in constructing the repository, we're going to do 100 miles of tunneling. So it just doesn't end. But at the site recommendation, the Secretary has got to make a call. The President has got to agree or disagree with it.

So I think, yes, at that point, I would say you're making that decision. Prior to that point, I would say it remains an evaluation on the part of the program and the Secretary.

DR. LANGMUIR: I have one more.

DR. CORDING: Go ahead.

DR. LANGMUIR: Unrelated. I think the Board, as a whole, has been very concerned over the years that DOE maintain and enlarge its involvement of concerned parties, whether they be state people or others, and the Congress' decision that you should not have any funding to do so, I think, is a big concern for us.

I'm wondering what, if anything, you can do or feel you can do to bring in the larger audience, the concerned parties into the process of making decisions, as well as educating them along the way as you proceed, in spite of the Congress saying you can't have any money for it.

DR. DREYFUS: Well, we are, at the moment, in a lawsuit on that -- on part of that, because -- I don't know to what extent you've followed the action. I have requested the funding for the state -- the Nevada county and state funding. Each time last year we -- in '96, we requested it and we were told by the Congress in the report, the committee report, not to give it to the state.

We wrote a letter saying we intended to do it in any event and got a strong letter back from the House Appropriation Committee saying you do that and you'll wish you hadn't, and we didn't do it.

Now, we then, again, requested the information -- the money in '97 and in '97, when I appeared before the House committee, I said we asked for it, we're asking for it again, and this time they put it in the act. They put it in the statutory language of the '97 act, there will be no money to go to that purpose. And the state has sued for the '96 money, which they contend we had a statutory requirement to provide.

I don't know how that's going to come out. My own view is clearly stated and I've told it to the Congress at every opportunity in my prepared statements and in cross examination testimony. So I believe it's important to have the counties involved and I believe it's important to have the state involved.

I have a strong appreciation for the fact that the counties are simply unable to provide proactive involvement in this program if we don't fund it. The state may find it a burden, but the state is big enough to do it.

So I'm not happy about that. We do many things to provide opportunities for people to participate and the problem gets to be do they have the capability to participate, do they have the capacity; for example, do our directors program reviews open, people from the state and counties do come and participate, they're pretty much everything that's going on. We do provide them with briefings.

So as far as the Nevada side of the program, we do as much as we can, but I understand, if you don't fund them, they have a very hard time participating. They don't have full-time employees. They don't have the kind of funds that they need.

I don't know what the answer to that is. We will be going into a NEPA process on the repository, which will provide another opportunity for involvement, for public information. So that just is one of the imponderables, but if told me to cut cooperative agreements, which we have with many interested groups, we cut them by two-thirds last year, I'm going to try not to do much more of that. Two-thirds is a pretty heavy hit.

So the clear intention of the Congressional policy is that we not spend a lot of money on that and we'll try to figure out ways to do it without spending a lot of money on it. We have home pages and that sort of thing which are voluminous and available and we're using every technique we know of to make everything available to anybody who wants it. The question is do they have the capability to access it without support. That's a real question.

We will request the funding again and we will approach the new Congress, which will be new in many respects, on that account and seek to clarify that policy next year.

DR. CORDING: Clarence Allen.

DR. ALLEN: It must be disconcerting to the public and to political leaders that, although perhaps no surprise at all to scientists and engineers, that this late in the program, after many years of study and then millions of dollars being spent, that we are making discoveries, and I speak particularly of the chlorine 36 situation, that may have significant effect upon the design, on strategy, on the aspects of the strategy.

I guess my question is how can we or how can you best and how can we best, if it seems appropriate, assure the public that it's realistic to make a viability assessment within two years and the other milestones on down the line in the light of scientific findings that indicate we don't understand the principal technical concern -- namely, the hydrologic flow through the site -- as well as we thought we did, and yet that remains the most critical technical issue, in most of our minds, I think.

DR. DREYFUS: Well, we're talking about the analytical approach to this and I think in any analytical or scientific pursuit, you have some phases to go through. The first phase is clearly to collect data almost at random and begin to sort of understand the terrain. And then as the data begins to inform you, you begin to systematize or sub-systematize, I think I understand this part or that part, and some things become more important and some things become less important.

There has to be this evolution and eventually you have got to come to the point where you have a working hypothesis of what you specifically intend to do and you are beginning to measure new information against that working hypothesis, not just viewing new information as interesting information that changes one of your subsystem concepts.

I think that's what the viability assessment is aimed at. It's aimed at saying, okay, this is what my working hypothesis of this repository is in its setting and from here on out, I'm looking to see if the new information refutes, changes, confuses that working hypothesis.

If, after almost 15 years of kicking rocks at Yucca Mountain and the tunnel through the repository formation itself, we cannot come up with a working hypothesis that says here's one way we could build a repository and here's what we think its behavior would be, then I'm afraid I must say to you the country is not going to hang around. You're going to wind up with long-term surface managed storage and we aren't do geologic disposal.

Now, that's a personal opinion. It reflects no judgment by the Secretary or the President, but it's the way I read the tea leaves, and I've been reading them in this town since 1960, so I have an independent ability to make that statement. Sooner or later, you have to fish or cut bait. We have to say this is what we would build if we were going to build it now.

Now, you can then say it's very sensitive to this parameter and I've got to keep looking at that parameter until I sort it out and you can change your mind with new data, but you can't hold all options open forever.

And the other thing is there's no way I know to focus the science except to look at what it is we're trying to prove. So I think it's entirely logical to have that kind of a working hypothesis. I think we probably should be working more on one right now, a much more informative one, of course, but one of the things that's troubled me right along in this program is I don't have what I consider to be a definitive enough working hypothesis for me to know whether chlorine 36 is truly a threat or not.

I was asked that by the Commission the other day, is this a showstopper, and I said, well, I don't think it is, but it could be. It could be. Well, how could it be? It could be if I'm resting my working hypothesis on a humidity situation that is refuted by this data point. I don't know. I should know. I think you can get there by '98. I don't think we're there now. Wouldn't try to pull that together today. I haven't tried to pull it together over the last four years.

I think we can pull it together by '98 and it will be meaningful. It will not be definitive, but it will be meaningful and we will know whether the evidence of the chlorine is, in fact, a serious enough change in our expectations that we don't know a way around it or it just means a design change, and that's basically the issue.

DR. CORDING: It seems to me that certainly at the time of that viability assessment, it's the time -- you really are assessing where you are and what remains, and I think you made that comment in your presentation. I appreciated it.

As I understand it, you're going to be saying, at this point, here's the way forward to our site recommendation, here are the things that we want to continue doing. We haven't completed our work, but we're going to be continuing the investigation to get to the point of making a recommendation on suitability to the President.

And it seems to me that that's an extremely important part of this, because it really is something that I see the DOE being asked to provide that sort of information. I see us, as a Board, being asked to respond to questions regarding that.

At this point, what is the way forward or what is necessary? Are we going in the same direction? Do we feel that we're going to get to the point with the remaining portion of the investigations?

That was sort of a statement, but I was really asking you for your perspective on that.

DR. DREYFUS: Well, in the plan, there is clearly several hundred millions of dollars and two years of work between those two points. So we wouldn't be there if we thought we were finished at the viability assessment.

The other point that I would make about this is that I can remember my first appearance before the Nuclear Regulatory Commission, when I was confirmed, Chairman Selin saying "I wish I knew better what it is you're trying to do so I could have a better ability to judge whether your investigation program is adequate."

And I think that's another point. Somewhere along the way, people have to say what is it you're trying to do so I can make my own judgment about whether you know enough about the flux and amount and you know enough about the rock mechanics in the east-west direction. What is the concept now sensitive to? Is it to these different parameters? And that, again, is why I think that viability assessment two years prior to the final cut will enlighten the thought process as to what truly is a sensitive issue that has to be resolved before you can make a site recommendation.

DR. CORDING: Thank you. Pat Domenico.

DR. DOMENICO: I never liked viability assessment. I kind of like it now. I don't really think it's that difficult. I mean, at least part of it, in the sense that performance assessment and underground design feed that assessment, given an EPA standard somehow defined in the future, the question is what is required from this site, in combination with its engineering, to comply with this.

DR. DREYFUS: Exactly.

DR. DOMENICO: Now, this is a quantitative question that can be addressed with models, and one outcome might be we are asking far too much from this site and its engineering to be able to comply, you fail the assessment.

The other outcome could be it's, from what we know, based on current knowledge, it's possible that what we're asking from the site to comply with this EPA standard is reasonable. Then you go forward to the next stage.

I mean, it doesn't seem -- if the scientists are honest in their modeling procedure and they're using models that are faithful, then I think it's a good stop in the program and it's a good assessment, if, again like I said, if people are totally honest in terms of what is required from this site to comply with that standard, whatever it is, then, to me, it's viable if you're not requiring that much from these rocks and the engineering.

I don't know if that was a question. Maybe there's a question in there someplace.

DR. DREYFUS: It clearly is going to be a first cut in what is possible and I think in gross terms, if it is clearly widely divergent from what is expected to be the standard and we don't know a way to make it better, you do come to a moment of truth. But I don't think you make decisions on narrow misses or hits at that point, but that's right. And sooner or later, you've got to make those kinds of preliminary judgments and decide what do I have to do now to fix it.

I mean, I would expect that that kind of an assessment is going to have some fairly broad bounding theories remaining in it. You will identify the ones that have to be narrowed and you have two years to narrow them. So all this is consistent with an orderly process of thought and study and I don't find it illogical.

DR. CORDING: Other questions?

[No response.]

DR. CORDING: Thank you very much. We appreciate very much your presentation and your continued participation with us this morning.

DR. DREYFUS: Thank you.

DR. CORDING: I want to proceed at this point to the sessions that we have established. The two areas that we're going to be discussing today and tomorrow morning are the Yucca Mountain program exploration and testing. So we're focusing there on an overview of the program and also then looking at the scientific aspects or the testing aspects of the program, and then going on later today, at 3:45 p.m., I believe, we'll be beginning the discussion of repository operations and continuing that through tomorrow.

In regard to the repository operations, there's been much work being done now to prepare for the viability assessment and developing a concept for repository design and operation, and in that regard, I believe it's been a major contribution that Dr. Dreyfus has made and the DOE with the M&O to establish an expert board to assist in evaluating and providing some input to the mains of actually accomplishing the repository design, excavation and design.

I think the use of these expert boards has been a very -- as I see it here, it's been very helpful in providing some guidance. I think it's already leading to ideas about how to make the operation and the construction more efficient and cost-effective, and I really want to congratulate the DOE, Dr. Dreyfus and the program for supporting what is not always easy to support, difficult very often to bring in the expertise, but to do that, I think it's been helpful and I see that as a very useful part of the program that they have established and look forward to a continuation of those sorts of efforts with experts.

As we go on, I just wanted to make a few administrative comments. We don't perhaps need to be as close as I try to get to this microphone right now, but we need to be within a foot or two of the microphone to be able to be picked up on the record. They would like us to do that.

We have requested in this program and we've set aside time for questions after each talk and we want to encourage our presenters to stay as much as possible within the time. We are going to take the time for the questions and at the same time try to keep on schedule, but we're not going to let the schedule overwhelm our need to have good discussions with the DOE and the presenters.

We will first ask for questions from the Board and then staff, if time permits, will be asking for questions from the floor. I want to indicate to you, as members of the audience here today, participants in this program, that if you are unable to ask questions of the presenters during the session, we have time for your own statements or questions and comments at the end of each day for a public comment. So if you would, sign up in the back with Helen Einersen and others of the NWTRB staff at the desk. There's a sign up there so that you can sign up for public comment, if you wish to make those.

It's important to us that we have that comment, that you be able to have opportunity to do that.

I'd like to continue on now with the discussions -- the presentation, rather, this morning. The topic, as I said, is really the Yucca Mountain program and this is a presentation that Steve Brocoum will be making. Steve is now the Assistant Project Manager for License Application and Site Recommendation and has responsibility for both science and engineering the performance assessment and construction at the mountain.

As I understand it, Steve, as you begin the program, you will be describing a little bit of the reorganization of the project office, so we look forward to that.

I think you've arranged to have an opportunity for us to break in the middle of your presentation. You're going to be on for quite a while, so we'll get a chance to have some discussion in the middle.

So, Steve, thank you and we look forward to the information you'll be presenting to us.

MR. BROCOUM: I'm a little embarrassed. I have a lot of viewgraphs here. I'm also supposed to use two projectors. I'll see if I can pull that off.

Now, the title here is Fiscal Year '97 Activities. The way the final briefing is, it's in two papers. The first one is overview to license application and the second one is fiscal year '96 accomplishments/fiscal year '97 overview.

It's all bound in one package in the back of the room for the audience, double-sided, so to save some paper.

The main goal here is to show that we have a comprehensive plan. In other words, Dan has had a series of strategic off-sites over the last few years. We have done, at the project, very detailed planning. We've done a long-range plan that takes us from today to the license applications. The program plan was updated and we did a very detailed plan for fiscal year '97. I'm going to kind of summarize some of that now and then in the second talk, I will go more into more detailed '97 activities.

The planning was started last year. In fact, it started, in a sense, at this meeting last October, when we, some of the project people under Dan, we started continuously planning to recover the program after that big cutback and get back to a program that leads us to a license application.

I have talked about all these things before. We've got a better understanding of the site conditions. We're working on our waste containment isolation strategy. We're trying to get the regulations streamlined. We're working our own regulation. The need to achieve viability assessment in '98 and, of course, more Federal management of the project. I'm going to try to show those things today.

We have worked very hard on integrating. So enhanced integration, I hope that shows through as I go through my presentation today. Very important.

We have iterated back and forth between the DOE management and our contractors to come up with an integrated plan. We have provided the detailed guidance. It was a top-down planning effort. We provided all the higher level milestones. Dan and Wes Barnes bought off on those milestones. We gave them to the M&O and told them to plan the details.

So we've had very proactive DOE involvement in this planning cycle. All the milestones and activities are logically tied, all the precursors, in fact, are precursors to successors. We hope that will come out in our presentation today.

So we had a long-range plan. It's like the five-year plan. We then did detailed '97 planning and, of course, it all is consistent with all the important milestones for the program plan. Our detailed plan has been baselined on September 30th and it's really the first time that I can recall that this program had a baseline in place for the year's activities in the beginning of the year.

The planning people love to talk about all the activities and how they tie together and so the long-range plan has 2,000 activities, with 4,000 logic ties. The detailed '97 plan has 5,000 activities, with about 7,000 logic ties. The point to an integrated plan that logically fits together. That's the main point I'm trying to make.

Now, I have to try and use two projectors. I was hoping there would be more room between the projectors to stand, but there aren't. I will move on the side here.

This is a high level diagram with 60 or 65 of the most important milestones. It's in your briefing, so if you can't read it. I am going to use this diagram repeatedly through my talk today as I talk about specific elements or strategic objectives of the program. It is broken up by year along the top. It has ESF and construction, core science, performance assessment, engineering design, and the regulatory framework, which is sub-broken down.

Then the light gray lines are some of the ties among the activities. If you look at this diagram, just as an example, that is the viability assessment, that little red diamond right here, and that is fed by the license application plan, the PA for the VA right here, that milestone right there, the engineering design, phase one design, and the cost estimate. Those are four key components of the viability assessment.

Another diagram. This is similar to the diagram in the program plan. We broke it up just a little more because it was kind of easy to talk to, but it's basically a diagram very similar to the program plan, where we're trying to show the key activities that support the objectives -- and I'm not sure the audience can see this, but it's in my talk. I'll put it up as high as I can here. And our long-term goal of repository operations.

The key objectives are updating the regulatory framework, the viability assessment, the EIS, the site recommendation, the license application.

I'm going to talk a little bit in this talk about updating the regulatory framework, the very first one, because that kind of sets the framework for the program to continue over the next few years.

We are planning to issue a proposed rule-making to amend 10 CFR 960 early this fiscal year. I think it's gone through the whole concurrence process at DOE. I think I'm correct in that statement. I think it's been concurred in by GC. So it's in the works.

There will be, obviously, a public comment period and there will be a hearing late this year and we hope to finalize this rule sometime during fiscal year '97.

We, of course, are very interested in the EPA standard. We will be interacting with EPA once they publish their draft standard. We're eagerly awaiting that and, of course, that could have a lot of impact on our program, how it goes. And we're equally interested, of course, in the NRC as they revise their standards. All these things have to happen before we do our site recommendation.

If one looks at history and sees how long it takes to do rule-making, we are watching this very carefully.

When NRC begins revising their rule, we will be providing our perspective to the NRC. We want to make sure that we can have a clear understanding of the reasonable assurance concept, because that leads you to how much is enough. We want to make sure that there are not overly prescriptive requirements. We want to clarify pre-closure and post-closure requirements, especially in the area of being able to use probablistic versus deterministic assessments, and there are some specific issues in various sections that concern us.

So the NRC has not indicated as to when they will revise their rule. I believe they're going to wait until the EPA issues their rule.

Dan went over this, the four components of the viability assessment. We have shown that to the Board several times. The design, the TSPA, the plan cost estimate for remaining work, and, of course, the overall cost to operate and close the repository. We're trying to pull all that together so that when we do issue the viability assessment, it's all integrated, the models that we use for the PA are, in fact, the models that are most current in science and engineering, the design aspects we use in the VA are the current design aspects. All that acts as a major integration effort.

The NEPA process has restarted. It's a new fiscal year. As you may recall, we started the NEPA process in fiscal year '95 and we stopped it in fiscal year '96, and we're restarting in fiscal year '97. We'll have a draft EIS in '99 and a final EIS in the year 2000.

Some key activities we'll be doing to support looking forward to the site recommendation. In 1999, we will submit to the NRC information for them to start their sufficiency of our site characterization for licensing. That will be -- most of that information, in our current thinking, will be captured in a project integrated safety assessment, the famous PISA.

We will prepare the documentation that is needed. Dan had a nice chart that showed all of that. NEPA clearly states what that documentation needs to include. Of course, if it's approved, the license application will be submitted in March of 2002 on our current schedule.

Management efficiency. We have implemented actions to be more efficient. Under the M&O, we have consolidated all the laboratories, all the national labs, and the M&O directs the work. We have awarded our technical support contract. That award was made recently. And we have reorganized and clarified the relationships between headquarters and the project to be able to work more efficiently.

The Yucca Mountain reorganization will not be effective until October 26.

Just to show you, this is where we are today. This is the current organization, with the six assistant managers. I'm sure you're familiar with that, but I have it in the package so we'd have something to compare.

This is our proposed new organization and this is where we're moving to start operating to on October 26. A major -- the four boxes up here are essentially direct reports to the project manager. A major new box is project control. We are elevating the role of project control. That used to be part of the administration function, and that's being broken out so we can improve the ability for costs and schedules and planning. Planning will be improved in project control.

The environmental, safety and health box, the functions remain about the same. The administration and asset management, those functions remain about the same as they've been in the past, other than we've pulled out project control.

These two boxes have new functions. The licensing box will include all the activities necessary to get the license application, including the VA on the way, the engineering, the science, and the performance assessment. All the work will be done under this box here.

This box here, since viability assessment is so important, it's going to be of high visibility for the next two years, we created a small organization to keep track of viability assessment, to help get it done, to help integrate across the program, and to help define the products.

So this is kind of a new organization that we're moving to. I will give you some of the names now. Project control will be Dick Spence; of course, chief counsel Susan Rives. Oh, this is also a new box. We have now back at headquarters a person that works for Wes Barnes, reports directly to the project manager, who helps work the interfaces between the project and headquarters, from the project's perspective. That is Linda DeSell. I'm not even sure she's here. I haven't seen her today.

Then we have the office of institutional affairs, Allen Benson, who recently moved out to the project from headquarters. Environmental safety and health is Wendy Dixon. Viability assessment is Rick Craun. Licensing is myself. Administration and asset management is Jerri Adams.

So for the first part of my presentation, just to close on that, we're trying to focus on site characteristics. We have a better understanding of site performance, a better understanding of the program, license application. We have a -- this is very important. We have a stable framework for moving forward. We have a long-range plan. We have a Congressional direction of what to do. We have our detailed plans. And presumably, as we move forward, we're not reinventing the program every year, which is what we kind of did in the last year.

Now, this was my break before going to the second presentation. So I could now either answer questions or start into the second presentation, however you would like to do it.

DR. CORDING: Why don't we take a few questions, Steve, if there are some at this point and we'll then continue. Jared Cohon.

DR. COHON: Could you put the big diagram back up? The big one. Has this been -- is this analyzed using something like a critical path method, that kind of thing?

DR. BROCOUM: There is currently a risk analysis being undertaken by some consultants and I think that will be reported to this new organization. That's happening right now. There is an activity that Mr. Barnes has started to kind of do a risk analysis of the schedule.

DR. COHON: Back to the diagram, I have some specific questions and some of them might simply be a result of difficulty in reading the copies. But some of the activities, like the heater test in alcove five under science, under core science, seems not ever to connect to any other activity. It just goes on through this period.

DR. BROCOUM: Okay. This was meant to be a summary diagram. In the actual detail plan, all these things are connected. In fact, when we reviewed these, we found some lines that were missing and to some degree, there's a degree of arbitrariness on these lines. But when you look at that activity, long-range or detailed plan, you can look up the predecessor and the successors. So when you actually look it up on your database, there are tables that show all of that. I don't have an example, but that's basically how the planning people do that.

So when you look at this, there will be, say, five are connected that have to occur before you can do this and then there are a bunch of activities after that depend on that.

So in reality, on a database, that is occurring, yes. A lot of effort is going into making sure that's happening.

DR. COHON: Well, let me try to put some words in your mouth then. I'll infer from what you just said that every core science activity is connected in some version of this to something outside of the core science block; that is, to performance assessment or something else.

I'm not asking you to demonstrate that, but --

DR. BROCOUM: The key thing in integration, the key thing in integration, in my view, is the process of going from the scientific and engineering data to the process models in each of those areas through the abstraction process and in the PA. That's where that integration will occur. Abe, in his talk, I think it's this afternoon, will talk about that in some detail. To me, that is the crux in the program of bringing together engineering, design, science and PA, that abstraction process, and I think Abe will describe the meetings we're having, workshops, I guess they're called -- I'm looking for a yes here -- workshops, thank you, Abe -- workshops that the various people get together for the abstraction process.

I talk about that some more in my second talk, but that, to me, is -- that's the one step we're doing, putting a high effort into, that we haven't done as well in the past, to try to bring in all of the -- to try and make a connection from the data in the field, the analysis of the data, the synthesis, the process models, abstraction, and PA. So I consider that very important in integration.

I notice that Dennis is standing at a microphone, so you may want to add something. Go ahead, Dennis.

MR. WILLIAMS: Dennis Williams, DOE. In regard to that question on heater test turned on in alcove number five. Now, you don't see any lines leading out from that. However, if you go across the chart to the right, you will see an item there, single heater test final report acceptance. That's really the outcome of that heater turn-on test and then you'll see the lines coming from that over to things like complete TSPA sensitivity analyses for license application, complete post-closure PA sensitivity for license application.

So really the turn on of that heater test wasn't the critical item. It's the report on that activity that is the critical item that is tied into PA and other things out there on the right of the chart.

DR. COHON: Thank you.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: All of us have been watching, obviously, the chlorine 36 information come in, which is -- I'm interested in knowing how it fits in all of this. I see some closure of concepts, models, whatever, end of the fiscal '97 period, under process models using SE site transport models acceptance.

I would assume that that represents a coming to closure on the uncertainties created by the chlorine 36 data and tying it into the unsat zone flow models with a package that gives us some confidence that we know what the distribution and amounts of flow are at that point in time. Is that how you view that?

DR. BROCOUM: It's our state of knowledge at that point in time. I wouldn't say it's closure. It's update, because we may have to update it in the future for the VA. I mean, we don't want to come up here and say we're closing and understanding everything. We just want to say to do the VA, we have to back up and we work very hard in our schedules to do that, you know, to integrate, to make sure we get the process models updated, and that's a very important thing that the science program is doing this year, with their latest information, and we have time to do the abstraction so it can feed the PA.

That has to happen. If that doesn't happen -- well, that has to happen to have a credible PA. That, to me, is what's going to raise the credibility of the PA. But that doesn't mean those are final models in terms of, say, LA. Those are the best we can do in the time we have with the data we have for our TSPA VA.

There's a reality here that you have to do that. You have to at some point decide you're going to take that information and put it in your PA. I know what the law says. The latest information, September 30th, 1998, but you can't do it in one day. So you have to kind of back off a little.

Although the PA people have been very good, I'm looking at Abe here, of doing runs and he's going to show you some today with the latest information in just a few days. So if something surprising comes in, they have the ability to incorporate that in a new calculation in a relatively short period of time.

DR. CORDING: One question regarding the workshops and perhaps more of that will come out later here today. But I'd be interested in hearing more about the topics that you're covering, workshop topics, for example, and the type of -- how you're carrying that out, what the composition is of those groups and what they're really doing.

DR. BROCOUM: I think Abe will be covering that in his talk today. So I think you'll find that interesting.

DR. CORDING: Okay. Leon Reiter.

DR. REITER: Yes. In Dan's very nice diagram, where he sort of showed a dividing line between DOE activities and then activities which involve external review, there's one item that you have that didn't quite appear in his, and that's the 10 CFR 960, the siting guidelines.

Looking through the program plan, they seem to indicate that siting guidelines will contain the criteria for determining site suitability and you have a compliance report in 1999.

Could you tell us how this fits in 960, in compliance with 960, what this all means in the rubric of progressive findings?

DR. BROCOUM: We do have -- the Nuclear Waste Policy Act requires the DOE to have guidelines. The DOE wrote 960 guidelines, I think it was in 1984. We are revising them. We're trying to make them more focused on total system performance assessment and overall system performance.

The act required DOE to have guidelines. Those are the guidelines that we created. We have to evaluate a site under the current laws, under those guidelines. That is an input that goes to the Secretary as she makes her decision. That's one of the inputs she will get. She will have a lot of inputs on this and this is one of them.

So we have to do that sometime and we, in our planning, have decided to do it in '99. That's how we set it up. So that's an internal Yucca Mountain or OCRWM activity to do that. At some point, we have to do that. That's all it is.

DR. REITER: So is that the internal decision on site suitability? Since the guidelines -- it says the guidelines are going to have site suitability criteria.

DR. BROCOUM: No. I don't think that's the internal decision on site suitability. I think the internal decision will be when we submit to Dan and Dan submits to the Secretary our recommendation on what the site should be. I think that will be the decision point. That's just one input. So I think that's kind of a fair way to say it.

DR. CORDING: Bill Barnard, Board staff.

DR. BARNARD: Steve, at the top of that chart, you show a completion of an east-west drift in 1999. Is that the east-west crossing of the whole site that the Board has advocated and, if so, have you decided to do that?

DR. BROCOUM: We have not decided what additional drifting we're going to do, but we do have a placeholder and we do have it budgeted for in the long-range plan. And as all this work goes on in science and design and PA, we will then decide what kind of additional drifting we need to do.

So we have it scheduled. We have funding, in a sense, identified. What exactly the east-west drift is we have not decided yet. I think Dan has made that pretty clear several times in the past. So we have the ability to do it. We don't know exactly what we're going to do yet.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: I was disappointed I didn't get to point that out, that Bill saw it before I did. But if you're going to complete it, when are you going to start it? If the intent is that if it's going to be done, it will be done early in '99. How long do you envision it will take to get it done and, therefore, when must you know when to start it?

DR. BROCOUM: I think we have all that in the long-range plan. I think there construction occurs fundamentally in '98 and if you're going to plan for it, you start the plan before that, which would be sometime in '97, I believe. So I think --

DR. LANGMUIR: So you'll have to decide definitively in '97 if you're going to do it.

DR. BROCOUM: I don't know if you have to decide, but you have to make the plans. There's also a lot of contracting issues, a lot of issues there. So I'm not sure exactly when you have to decide. Dan?

DR. DREYFUS: Maybe I should say something on that, because we anticipate, as we go forward, that we may have to do additional underground exploration, substantive underground exploration.

I'm not prepared yet to agree that that's an east-west drift or that we would know where to put it if we were going to have an east-west drift. I think we are going to be getting -- well, we have a lot of information that has not yet been digested from the tunnel. We have the Ghost Dance Fault penetration, which we will have preliminary information from very soon, at least in one alcove.

We are tunneling in an east-west direction outside the repository formation at the moment. As you say, there are some issues of the chlorine sampling and the fracturing in the tunnel proper that we have not yet totally digested.

So we have the funding latitude in the program to do underground work in the near term, if we decide that we now know enough to know what to do, and we're flexible and we're able to make that decision internally to the program once we're confident we're making the right decision.

So I don't think the date is all that definitive. It has to be somewhere in the plan, but I can tell you that I have the flexibility to do it sooner if I know what it is I'm doing.

I told the Commission I was not confident I knew what was necessary and at this point, that's where I am. Now, we may, in the next few months, come to some kind of a management decision about that. But we have the flexibility, we can do it and we can do it at the appropriate time. We can't do it yesterday, but we can do it from here on out, when we know what we're going to do.

DR. CORDING: One comment in that regard. I think -- the one thing I think that's -- in the program here, one looks for as much flexibility as possible and I've seen some very good examples of that underground where people have started to look at, for example, the moisture conditions, recognizing that a lot of the things that we would like to see regarding the ambient conditions are masked by ventilation, for example; that people in the program are thinking very seriously about that and we're going to hear more about that today, I believe, as to how they can try to understand that.

So there's been a lot of occasions in the program to do that and I think there's been a lot of response to what's being accomplished underground and observed underground that can modify in the program.

Just one comment in regard to further construction and tunneling. I see the -- the concern I have is what it takes to get a design done or a contract, particularly if it's a more -- not as standard an approach for contracting or getting the procurement, for example, to do those things.

It would seem to me that being able to move forward with some of those approaches of setting up to do it and then maintaining some flexibility as to how then one applies that approach, for example, more drifting, would be -- it would be desirable, I think, to be able to get that started, because the lead time is so great on some of these procurement type contracting issues. So that's been the concern I have as to what point do you need to start that process and still perhaps maintain a flexibility as to how you apply it or even if you apply it.

So that when you do get to the point of making a decision of what you wish to do, that you have it in place. I think that's the concern I've had.

Are we ready to proceed with the next part? Are you ready to just go right on, Steve, or are we set for -- are we supposed to have a break here somewhere?

DR. BROCOUM: This was going to be the break point.

DR. CORDING: Why don't we take a 15-minute break at this point, Steve, and give you a chance also to have a break. So we will reconvene at 10:20.

[Recess.]

DR. CORDING: Steve, we're ready to begin again with the fiscal year '96 accomplishments and fiscal year '97 overview.

DR. BROCOUM: I need to make a couple of comments here on my last talk. First of all, I did leave out an important person, a new position. Susan Jones is the Associate Deputy Project Manager.

DR. CORDING: Thank you. Steve, before you begin full bore here, I just want to ask the audience to resume their seats so we can listen to the presentation. So if you would finish the coffee break and get back, we're ready to begin. Thank you. You deserve to have them all sitting or most of them sitting. Thank you.

DR. BROCOUM: A couple of -- I think I ought to make a couple of comments on my first talk, since I walked out and various people whispered in my ear this and that, make sure I got it right.

I forgot to mention that Associate Deputy Project Manager, Walt White has that position now in the new organization. He will be retiring, I believe, in January. Susan Jones will be replacing him.

The other correction I was told by the planning people was that, in fact, the east-west drift, the construction would occur all in fiscal year '99 or whatever construction we do at that time in the planning. So the window is in '99, not in '98, as I said. So I just want to correct that. I'll leave this chart out in case I need it.

Okay. Second half here. This talk will talk about what we did in '96, in spite of all the constraints, what we plan for '97, what our key milestones are and how they support the viability assessment, the EIS, the site recommendation and license application.

I'll kind of do an overview of the activities, which is the left half of this chart. That bullet here refers to these kinds of activities on the left half of that chart and I will try to keep my charts in sync as I go through this.

What have we done in '96? We completed a concurrence draft of the waste containment isolation strategy. I always have a hard time with that, the WCIS, and we did put out a copy, if you remember, in the July meeting for the TRB.

We have drafted and it's in or finished concurrence of 960 and it's all part of the regulatory streamlining that we're hoping to have in the program. That includes the EPA standard and the NRC standard.

We've completed excavation of the main drift. We're now starting to excavate up the south ramp and we completed alcove four at the base of the non-welded Paintbrush Tuft.

We completed initial phases of the thermal test facility. As Dan said, we started the test on August 26. I even have a picture that shows them throwing the switch. We're moving towards a full-scale test towards the end of '97. The initial phase of the north Ghost Dance Fault alcove is complete. We completed the advance design, I think it was March of last year, the ACD. We published the third total system performance assessment, TSPA-95, we had '91, '93, '95. We'll have one in '98.

We've revised the whole program, revised the program plan, and we've baselined the long-range plan in July, I believe, and we baselined our detailed plan late in September.

Preliminary conceptual models were produced in the site area, these six models, which feed very important issues in PA, and these will be updated this year as we move into the abstraction process.

We continued field testing; for example, in alcove two, we started hydrologic testing in alcove three, the upper contact of the Paintbrush Tuft, lower contact, various pneumatic testing in the unsaturated zone and along the lower holds, along the ESF main drift in the north ramp. All that was going on this year despite all the cutbacks in our budget.

We completed processing of data, geophysics data in Crater Flats and there's been some discussion with the NRC on this. They've been also doing some data. We've completed the surface geologic mapping and that map is in review with the GS and will be published shortly. We started the C well testing, the saturated zone testing, using various tracers and transport through the saturated zone. We've said that already. The single heater element test was started and we started, in fiscal year '95 and finished in early fiscal year '96, I think it was 15, I believe was the number, 15 public scoping meetings for the EIS. So all that was done.

Now what I will do is I will show a bunch of pictures. Let me just get them all out and I'll just flip them on the other machine here. I'll stand here. The first picture, looking forward into the turn going up the north ramp, and I guess it doesn't show too well here, so many lights on, but we're looking around as we're turning into the south ramp from the main drift.

Looking back along the main drift, so that's the main drift down here, looking back. That's all completed. A big event when this occurs, when the ESF actually breaks through in the south portal, this is the area it will be breaking through right here and they're starting to grade and get ready and do whatever construction they need to do for the ESF to break out.

The alcove for the heater test under construction, various construction activities. Some more construction activities. This is the Ghost Dance Fault alcove, looking towards the Ghost Dance Fault in this direction. Looking back out from the Ghost Dance Fault, looking back out towards the main drift. I understand that's the conveyor up there.

Here is a picture of the C well tracer. They're injecting tracers into a well, which would be off to the left of the slide. Nobody in this room will notice he's not wearing a hard hat.

Here is the single element heater test, installed in the hole facing east, and these are various rock load cells to measure thermal mechanical stress when the heater test is turned on, I guess before they turned it on. Just a closeup of the same thing. That's a kind of a long element.

In some pictures, we have that, it's out and it's maybe 10 or 12 feet long.

Finally, without much fanfare, but it was done, they actually threw a switch and turned it on. Our understanding is that the heating up is occurring and the isotherms are moving out as predicted by modeling that was done before the switch was turned on, so far.

Last year we had $250 million out at the project, the share of the overall budget. This year we have 325. The actual breakdown of the numbers along the various WBSs is still being worked, so I'm not showing it on the viewgraph. I would prefer not to show a viewgraph because it's still not quite finalized.

Now, what we're going to do is go through the key '97 milestones supporting VA, EIS, site recommendation and LA. So first, the viability assessment. You can, on a chart like this, put in the tie lines that you think feed the viability assessment.

The key things we're trying to do in '97 are -- move it up a little -- develop a site description that uses all of the available data and the model results. One of the things we need to do is have a site description. That kind of pulls all the information together from the science program. Obviously, we want to provide robust site engineering system process models and we are integrating into the TSPA. We want to test the models and abstract them, and Abe will be talking about that this afternoon, and we want to start our peer review of the TSPA, which will be ongoing all -- not only through the viability assessment, but beyond the viability assessment. That peer review will provide information for the TSPA, help improve the TSPA LA.

In the design area, we want to focus on key design issues for the repository and waste package that have little or no NRC precedent and that are important to overall system performance. So that's where the folks in design will be. Of course, we want to develop the license application plan and we want to start developing the cost estimate.

We have to do enough design in all the elements, by the way, to be able to come up with a reasonable cost estimate. So even areas that have no precedent, there has to be some design at some level to allow you to actually do the costing; for example, the surface facilities.

And what this diagram here kind of shows you is in the P area and in the design area, these things feed into the viability assessment, as well as the license application plan. Remember the four key components? The license application plan, the TSPA, the design phase one, which will actually be done in fiscal year '97, and the cost estimate. So those are the four key components of the viability assessment.

With regard to the EIS, just a few words on that. We have restarted the EIS process. We have our EIS contractor on board. We will develop -- we had all these scoping meetings. We will develop a comment summary document and we will initiate consultations with other agencies for the EIS process. Again, this diagram shows you designs and performance will be feeding the EIS, and those are the tie lines there.

Site recommendation and license application. We want to complete the implementation of the document for the waste containment isolation strategy. I'm a little uncomfortable with the word complete here. I want to update. I keep thinking more in terms of update than complete. We obviously want to complete the five-mile loop and we want to develop an integrated tentacle engineering synthesis to support site recommendation, license application, EIS, as well as the VA.

This, again, is what we refer to as our PISA in our current terminology. These are all necessary to support all the issues that Dan had on his chart that are required by the Nuclear Waste Policy Act. We have to describe the site, we have to describe the engineered barriers and how they interface. There are some requirements of the act. So this is here basically to make sure we have all that information.

So on the chart, on the right, we're showing you the key -- again, in a science, in the modeling area and the performance assessment area, in the design area, the second phase of the design, which will support the license application, all feeding down into the draft LA and then a final LA here. So in this area.

So, again, we're trying to show you how it all flows together.

If we look up more closely, in '97, what are the milestones for '97, which is the year we're in right now, these are some of the key milestones that we have to get done this year to stay on track.

So obviously we have to complete the TBM operations. We have to complete the Ghost Dance Fault. The models have to come together and the abstraction process has to occur. That's right in here. We have to complete the design and so on. We're also planning to issue the final 960, but it's a different color because it's a higher level milestone, and so on.

So this is basically, in a nutshell, the key activities and they emphasize ESF, they emphasize design, they emphasize core science and performance assessment, as you can see from that chart.

The one major thing in regulatory, of course, is the -- well, two major things. There's the 960 and the license application planning, because that's one of the components of the viability assessment.

Now, I will go through some of the more specific activities, to which we will talk in more detail a little later. The key ones I'm going to talk about are what we're going to do for the waste isolation containment strategy, the scientific overview. This is kind of an overview presentation. People will get into more detail. Testing, design and performance assessment.

I've got slides all over the place here. Okay. The waste containment isolation strategy gives our approach at the current time of how we're going to resolve post-closure performance issues. As it's updated, it incorporates new information and designs and realistic, more realistic as we get better understanding of performance predictions, and we try to also anticipate the kind of regulatory changes that are coming at us. The one major one we're anticipating now is some kind of a dose-based standard.

It helped us focus our science and design work to evaluate performance and it relies on the five key hypotheses that we've talked to the Board starting in January of '96, I believe, up in Beatty that time.

The highlights are just about ready to be issued as a DOE document. That's what we gave out a draft of in July. We've put together a -- the original waste isolation strategy was written by a small team of people. We've broadened the team, made it multi-disciplinary and included representatives from the M&O and the USGS and all national laboratories. There is a comprehensive draft that is now going to go into review.

But the process of writing this, which has been going on for at least a year now, has led to integration within the program, because as these people work on the document and as these people get involved in planning, that new knowledge they get from integrating gets forced into the planning.

So although we haven't finalized to the extent we might have thought we could when we started, I think we've gotten a lot of benefit out of it by the fact that people have been working together and knocking heads, if you like, to understand all the issues.

So I'm even hesitant to say we're finalizing. We're just updating.

Obviously, the scientific program will provide the process models. We have to be able to defend which process we have included, which we've excluded and why. We have to look at the models and compare the predictions from the models with the real world observations. We have to look at sensitivity and uncertainty analyses for all the parameters and all the assumptions and for those that have large uncertainties or have large consequences for performance, we may have to get more information, and we have to make sure we address alternative models that can observe those same observations.

So these are the kinds of things we worry about as we're going through this year.

In ESF, the overall goal is to understand how the unsaturated zone works and how seepage into drifts may occur. So we have to characterize the in situ conditions. Obviously, the thermal mechanical data collection, the thermal test was turned on, the first one. Somebody brought that up earlier today. We have to understand the effects of ESF ventilation on the evaporation and the whole issue of water mass balance.

There have been some suggestions, although it's not in the program right now, that we need to seal off an alcove and observe a sealed alcove, to make sure we fully understand how moisture may come into drifts, and that's something we're considering.

Obviously, the Ghost Dance Fault, we're just about there. I think they're probing right now to see exactly where it is, but all the key parameters for the Ghost Dance Fault have to be understood, and how it affects, again, flow through the repository block.

A lot of discussion will occur on this, I'm sure, this afternoon, but understanding the age of mineralization along fractures, the various isotopes, chlorine 36 and technetium 99 and iodine 129. Additional planning for additional unsaturated zone transport in rocks that are very much like the Calico Hills, there is activity this year to decide how that test and where that test will be done.

There are three options being considered. One is in the ESF, one is on the surface within the Calico Hills, and the other is in an existing other tunnel on the test site. So that activity will be happening this year.

In the surface-based -- I want to make a comment here. We're talking about surface-based ESF. What we're going to try to do is get away -- since we're integrating our program, getting away from talking about surface-based, ESF-based, or laboratory-based test. It's kind of one test program. But for speaking it's just easier to categorize them in those categories, but to some degree, they almost seem as being competitive in past years. So one of my goals is to get rid of that terminology. We have a test program that addresses specific issues.

But anyhow, obviously, with the new standard that may come out, doses to people at some distant repository, I understand the saturated zone has become more important in understanding the role it might have in diluting any releases from the repository, very important. C well is becoming very important. The monitoring I mentioned. We're going to initiate the large block test on Fran Ridge. That's been one of these things that's been kind of mulling for years. Some of the people think it's very important to understand the relationships between the hydrology and the heat in the rock, where you can observe it, and understand the boundary conditions very well. Of course, I mentioned the fracture coating and isotope analysis.

In the laboratory, they're going to help refine the zeolite stratigraphy. Zeolites may play a role in retarding radionuclides and understanding that stratigraphy and how it relates to the heating up of the repository, which I think will be talked about this afternoon with thermal loading, and not damaging the zeolites is an important issue because at about 90 or 100 degrees C, the zeolites could be irreversibly changed. That's an important issue.

Absorption tests, also important, again, particularly for neptunium. Finally, understanding couple processes in the thermally altered zone.

Kind of a repeat of something I said earlier, but, again, we want to concentrate on the zones that have little or no regulatory precedent in the NRC and which have a big potentially major impact on performance.

Schedule, constructability and cost, those areas. Okay. We have to -- you know, a few years ago, we had a multi-purpose canister. Now we have to make sure that on our waste handling operations, we're not -- it's not a multi-purpose canister word anymore. Again, to come up with accurate cost basis, some redesign of the source facility has to be done, enough to be able to get an accurate cost basis and enough to address anything, again, that is unprecedented. But the bulk of that and the bulk of the design will be focusing on the underground.

We have to evaluate the waste package to accommodate uncanistered spent fuel and look at these areas here and we have to do laboratory tests on the waste package material and waste forms to develop the process models which feed the PA. To the extent we can develop these models, just like a scientific model, these are the engineering models, the data, the more robust the PA will be. And, again, going from these process models and abstracting to the PA is an important issue.

There's probably three or four orders of magnitude of performance depending on how all of this comes out. If you look at all the different aspects of the engineered barrier, everything from back-fill to how the waste package corrodes, to whether you take credit for cladding and how the waste form dissolves and how it gets out of the waste package. So it's a very important area.

In the TSPA area, we're going to put teams of site people, engineering people and performance assessment people to support the abstraction process. We want to make sure we use current process models in the TSPA and we want to make sure we understand and bound the uncertainties of the process models. We want to do sensitivity analysis to quantify the effects of the uncertainties, especially those that have a big impact on performance, and see. Some that have a big impact on performance, we may have to do additional testing to reduce those uncertainties, or additional design, depending on the parameter or assumption you're talking about.

The TSPA will be peer reviewed by an external panel of experts, mainly experts of the various processes that we're worried about and experts in the various areas of TSPA. That detailed planning has gone and the work will start early this year. This year basically is an orientation to make them familiar with TSPA and all the process models and all the information flowing in.

Basically, though, the overall objective of the peer review is to provide recommendations for, in a sense, the one that's very important for the LA, the TSPA for LA. When you look at our schedules, the peer review will not be done in time for VA, but it certainly will be done in time for the next iteration of the PA for the LA.

So in a sense, by the end of 1997, we will have updated, not finished, updated our waste containment isolation strategy. We hope the final rule will be issued. That's not under our total control. We need to get, for example, concurrence from the NRC. Concurrence on the original rule in 1984 took nine months.

We will have completed the south ramp, TBM will have exited and the loop will be done and all the currently planned tested alcoves will be complete. We will have updated all the unsaturated and saturated and transport models. We'll have them updated for the TSPA VA. The probablistic seismic hazard assessment, which is just starting now, will be well underway. That is scheduled to be completed in January and that information will be feeding the design and, again, most of the design for the license application, and the first phase of the design for the VA will be completed.

We will hope to have the waste package material and the waste form degradation models, as I said earlier, very important because of the many orders of magnitude, about four or so. The TSPA panel will have been oriented to understand our process. They will have completed the site and the design process model abstraction workshops that Abe will talk about and scenario development will have started. Of course, the NEPA process will be underway and we will have completed the license application plan. The reason I have essentially there, that is due to us from the M&O on October 1st of '98. So it's next fiscal year, but essentially done.

I think we have a very interesting meeting for you because we're talking about very current activities and very current issues. We're going to be talking about the unsaturated zone processes and the models, the new information that's flowing in. One of the things that comes to mind to me is something that Dr. Cording said probably maybe two years ago, and that was once you get underground, you're going to get a vast amount of new information coming in. I still almost remember the day he said that.

That's what's happened to us. A lot of new information is coming in right now and we're sorting through it, working through it. So this is all work in progress and there are potential new alternative models coming out, but we're kind of -- I don't want to say overwhelmed, but we're -- lots of information coming in, is kind of a fair way to say it.

What I wanted to do here, and I wasn't successful, is I wanted to show all the process models, all the abstracted models, and the key performance assessment models, and I wanted to show them and say, now, look, we've talked about this, this and this today and next time we'll talk about this and this. By the time we go for like a year, we'll have talked about everything that feeds the PA, and that's still my goal is to be able, as we meet every quarter with you, to go through the whole picture.

The reason we don't have the models here is there's not total agreement exactly what the models are, and so we have to clarify that among ourselves before we can present it to you.

Then, of course, after we get done -- let me go back a second here. I didn't make a point here. I'll go back here a second.

So after Dennis Williams and Bo Bodvarsson finish talking about the hydrology in the sands, we will then move into Abe Van Luik, who will give us some results of sensitivity analysis TSPA has run, showing you what impact this new information has; again, very current. Then he will also talk about that abstraction process which we think is so important in integrating the program and he will also talk about expert elicitation, a few other issues that the staff said the Board was interested in. So that will all be in those talks.

On the engineering side, we're going to be talking about the concept of operations and I think Jack Bailey is doing that. We will be talking about the design status of the waste package and the emplacement drifts and Hugh Benton, of course, is doing that. Feasibility of technology and viability assessment and stability of the drifts will be talked by Alden Segrest, and repository thermal management will be talked by Dick Snell, and that's kind of the program for two days. Again, all very current topics as we move into this phase of the program of completing the viability assessment.

Thank you.

DR. CORDING: Thank you very much. We are looking forward to the remainder of the program, as you've been laying it out here, to see how these things are coming together and how you're looking at this extensive amount of information you're obtaining and evaluating.

We'll go to questions now for Steve. Don Langmuir.

DR. LANGMUIR: Steve, the sense I'm getting is that the TSPA provides the guts for the waste containment isolation strategy. Is this the way you view it? That provides the prioritization you need in order to decide how to focus in the strategy on different features of it.

DR. BROCOUM: It's a very important component.

DR. LANGMUIR: So it's the same people, presumably, involved in both activities, to an extent.

DR. BROCOUM: Yes, but it's an integration. We're trying to integrate between science, PA, and engineering. I don't want to say just PA, because it has to be that -- the PA won't mean much if the scientists get up and say my data wasn't used or wasn't used properly.

I mean, the goal, what we want here is the scientists and the engineers, when we're done, is to be able to say I participated, I understand how my data was used and my data or designs, whatever, were used correctly. That's kind of the goal, what we want, and give the robustness to the PA beyond just the numbers. So we want -- that's our goal, what we're getting, that's what we're working very hard on doing.

DR. LANGMUIR: So essentially you've got an internal committee of those who are involved in the process or reviewing it to see if they like or can agree with the conclusions.

That brings me to another question, which is I presume you've constituted or are close to constituting your expert exterior group that are going to evaluate this process. I'm wondering how you've done that, how you've picked them, and who they might be, if you know yet.

DR. BROCOUM: Abe is going to address that in his talk. Basically, we're going to send letters, I believe, to various scientific organizations asking for recommendations. We will pick from those based on their background and experience, but manage it within the program. There will be experts outside, but they'll be managed within the program. We're not going to go to another agency to pick the experts for us and do the peer review.

I think Abe will talk to that in a little more detail. I don't want to steal too much of his thunder.

DR. CORDING: Jared Cohon.

DR. COHON: Following up this question about peer view, though Abe will talk about it, from the bigger perspective. You said that the peer review of TSPA VA is really intended to support or to be used, the results of which will be used for TSPA LA and, in fact, the timing of the peer review is such that it can only be used for LA, because it's going to come really after the viability assessment milestone, in effect.

DR. BROCOUM: Again, when you -- I should not be absolute when I say these things. Okay.

DR. COHON: I understand. Timing is fuzzy and I understand that. But it leads to the question of what is intended in terms of review of the assessment itself, that is the viability assessment. Is there -- is that reviewable and do you intend to get it reviewed? I'm not talking about the TSPA --

DR. BROCOUM: I see what you're saying.

DR. COHON: -- VA itself, but rather your viability assessment determination. Is that going to be subject to review and comment?

DR. BROCOUM: As we envision the viability assessment, it will be a relatively -- do you want to say it? Do you want to talk, Dan? You're looking at me, so maybe I should defer to you.

DR. DREYFUS: The simple answer is no. The plan is what it says in the statute, pile up the documents and submit them to the President and the Congress, and, of course, the public will have them.

DR. BROCOUM: There is, if I can find it on here, there is a letter report, I think it's on this chart, I've seen it, PA. There it is. That letter report is from the peer review group giving us kind of a status at that time. So there will be some input from the peer review group in time for VA. So that's right here. But the peer review is not completed till fiscal year '99 in our current schedule.

DR. DREYFUS: Basically, the peer reviewers of the performance assessment will be making recommendations about the final performance assessment after the VA goes out. They will be in full cry, of course, when the VA goes out and may make a commentary on what they think about the performance assessment that's in the VA, informal letter or remarks about their review of that process.

But the VA itself is more than a performance assessment and the way that works is we make those documents public. I'm sure there will be considerable comment, discussion and introspective contemplation of them, but not in the program after they're out.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: Steve, a more specific question to details here, but of interest to me. On 33, page 33 of the overheads, you make the statement -- the statement is made that process models supporting TSPA VA with regard to waste package material and waste form degradation will be completed. I presume you don't mean actually completed in the sense that you fully understand what's going on yet, but that it's the status.

DR. BROCOUM: That went through my mind as I saw that viewgraph. Updated.

DR. LANGMUIR: Updated, not completed. I'm learning.

DR. BROCOUM: Right.

DR. LANGMUIR: It's updated.

DR. BROCOUM: Updated is the kind of word we like to use, because they're going to be updated again for the TSPA LA.

DR. LANGMUIR: Otherwise, I was going to say, how are you going to get all your --

DR. BROCOUM: Performance confirmation.

DR. LANGMUIR: How would you get all your results from the corrosion tests in the next 12 months or so when they just started sort of thing.

DR. BROCOUM: That's correct. A lot of work.

DR. CORDING: Jeffrey Wong.

DR. WONG: Steve, I'm looking at one of these charts, the one with the Big Dipper on it. I still don't understand the peer review. You're doing peer review. It looks like your OCRWM will make the -- or accept the viability assessment without peer view and then you do complete peer review before you complete the sensitivity analysis of the PA.

I wanted to know why you would stop peer review, not include peer review as you move into the sensitivity analysis, why you would not have peer review for that section also.

DR. BROCOUM: I'm going to turn it to Abe on that one, or do you want to wait till you're on later? He wants to wait till he's on later. This is how we have the schedule today. What I think we'll do -- I'm not sure what we're going to do way out in the year 2000 right now. So if that requires -- but, again, we don't want to be in a position, like Dan said, of doing a peer review that comes out -- we want the information and time to do a good TSPA LA, that's what we want, so we can improve the PA.

That's our goal. So that's kind of how the schedule is. But remember, all of this will get extensive review in the licensing process. If we get that far, every single assumption in the PA will -- the parameter will get extensive review.

So once you get into the licensing process with the NRC, it's going to be a very public and, you know, question and answer and all that. So I think it's going to -- it will be essentially peer reviewed, whether we have it on this chart or not, I guess is what I'm saying.

DR. CORDING: What do you see with regard to -- at the time of viability assessment, with regard to the statement about the plans continue to license application. For example, at that point, will the documents also include a description of the work that you will be carrying out to the license application?

DR. BROCOUM: Yes. I think one of Dan's important points is that the license application plan, which is right here, includes all the work necessary to get to the LA and the costs associated with it. So at that point in time, people understand what they're buying into, if they're buying into it.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: Steve, one of the big concerns of the Board and of me personally has been and still is how are you going to learn what you need to know about couple processes regarding the flow of fluids when you put a repository in there. Of course, you've got the heater test scheduled, with the rest to alcove five just starting right now, I guess, and then the large block test about to start, and then I see the drift scale test is scheduled for October '97.

My sense, if I'm right, is that the heater test in alcove five is strictly a study of the transfer of heat. It's a mechanical test. There will be no intention or effort made to measure fluid flow or couple processes in that heater test five, right?

DR. BROCOUM: That's my understanding, but the large block test, I think, does look at --

DR. LANGMUIR: The large block test --

DR. BROCOUM: They're shaking their heads here. Hold on a second. Dennis.

DR. CORDING: Dennis Williams.

MR. WILLIAMS: Dennis Williams, DOE. With regard to that single element thermal test, we do have a moisture monitoring component in that single element heater test. So we are -- it has mechanical elements that we are looking at, but it also has hydrologic elements that we are looking at.

DR. LANGMUIR: Okay. So you'll be looking at fluid vaporization and transport away from the heater.

MR. WILLIAMS: That's correct.

DR. LANGMUIR: But it's not designed to look at the couple processes that might result from that, right? The precipitation dissolution, sealing of transport pathways, that sort of thing. You can't look at that in that heater test.

MR. WILLIAMS: We probably won't get deeply into the couple -- those type of coupled processes on that test because of the size and the type of instrumentation that we have associated with it, but it would be the beginnings of an understanding of those types of things that then we would carry on the drift scale.

DR. LANGMUIR: What about the large block, though? I mean, that, I assume, from what I've been told, was intended to give you a sense of -- an ability to measure on the periphery of the block or into it couple processes.

MR. WILLIAMS: One of the advantages we see on the large block is to be able to introduce water; first off, drive the water off, reintroduce water into it, look at some of the chemical changes in that block, because after it's done, we will actually tear that block down. Of course, the large block gave us the advantage of having all the surfaces exposed such that we could really understand what was going on in that piece of rock.

So, again, that, too, will give us some beginnings of an understanding of some coupled processes, but the drift scale is the one that's probably going to roll a lot of that information together into the best test for coupled processes that we can do on a reasonable scale in the mountain at this time.

DR. LANGMUIR: Okay.

DR. CORDING: Steve, you've commented on the investigations to look at the absorption and the zeolites and the effect of temperature on the zeolites. At present, do you have a feeling for the type of credit that might be taken for the zeolites in the isolation strategy?

DR. BROCOUM: As I understand it, depending on the model of flow for the water, in some models, the zeolites are important because in some models the water gets diverted around the zeolites and they are not important. So depending on how -- which conceptual models we have to consider, zeolites can be either important because the water flows through them and, therefore, there's retardation and in some of them water is diverted in some of these new models, and I understand that's happening and, therefore, the zeolites would not be that important.

I think it depends on how the modeling and the understanding of hydrology comes out basically. But I think we have to cover all our bases and we cannot ignore them and I think that's what we're trying to do.

DR. CORDING: Right. Do you have any other information you might give us or some thoughts on -- you're talking about the further work on the unsaturated zone, non-welded Calico Hills or ESF, tunnel up the MTS. You mentioned that.

DR. BROCOUM: I'll just talk philosophically. It's important to understand not only how the percolation flushes the mountain, but whether actual drops come out, fluid comes into the drifts themselves, and I think a lot of the work is going to be in those areas.

So those two areas are interrelated and so as we get a better understanding of the hydrology through the mountain, the unsaturated zone, that will determine what additional work we've got to do. Dennis, I know, is walking to the mic, so I'll turn it over to Dennis.

MR. WILLIAMS: Dennis Williams, DOE, again. I think what Steve was referring to in the other tunnels on the test site, we've had a demonstration of applicability of a laboratory test which has to do with the UZ transport model, which has been in the plan for quite some time. Obviously, the best place to do that would be in the Calico Hills, but we don't have any present plans to go to the in situ Calico Hills in the repository area.

So we have looked at the possibility of using similar types of rock formations up in P tunnel on the test site. In addition, we have looked at a possibility of doing similar types of or tests in similar types of rock in alcove three of the ESF and we are also looking at the possibility of going out on the surface at an exposure of the Calico Hills.

We do have in the '97 plan funding to basically look at all these three options and try to come up with the best place to do this type of test, if this type of testing is warranted.

DR. CORDING: And the test itself would be -- you'd be looking at some tests like the permeability characteristics, some actual transport mechanisms. What do you see for the type of thing you would investigate with that?

MR. WILLIAMS: One of the things you'd really be looking at is the heterogeneities of that particular rock mass and the transport of radionuclides through that rock mass.

Now, this would be not an extremely large scale test, but I would envision this test would be something on the scale of several cubic meters.

DR. CORDING: I see.

MR. WILLIAMS: But, again, that's part of what the -- that's part of our -- and I think it was something like 230 K that we put into the program this year just to look at those -- the details of that plan, how we could possibly best field that particular test.

DR. CORDING: So you're basically looking for non-welded tuft, which is relatively massive and --

MR. WILLIAMS: That's correct.

DR. CORDING: -- not heavily fractured.

MR. WILLIAMS: And we know that the P tunnel isn't exactly the same. We know that alcove three isn't exactly the same. But we're looking for something that may serve as a suitable surrogate for that kind of testing, because to date, we're not down at the Calico Hills below the repository block.

DR. CORDING: Okay. Thank you. Pat Domenico.

DR. DOMENICO: When the process model is out -- I guess I've been making some noise for a few years now about one process where I thought it was a very good idea to get a peer review from experimental petrologists, mineralogists, as well as theoretical high temperature people in that field who have experience in geothermal regions, to take a look at that mineralogy and come to some peer review in terms of what they might anticipate based on what they've seen elsewhere.

I've never seen anything come out of the program in this area. Of course, you're going to make some observations with the experiments, but these are short-term effects compared to, I think, what knowledge some very good theoretical mineralogists and petrologists of high temperature can at least speculate.

Given that mineralogy, what might be the overall effects of the thermal load? Is that one of the process model peer review that has been considered or not?

DR. BROCOUM: I don't think we have explicit peer review in that area. We do look at natural analogues and if there is a natural analog -- I mean, we've approached it that way in the past. I don't know. Does anybody want to -- I'm not aware of a peer review that specifically goes to what you were just asking for right now.

DR. LANGMUIR: Ed, can I comment on that? We have had Board meetings where we have looked at geothermal analogues as an aspect of our concerns on the site and we've had people from that community of Pat's discussing what they think might happen with the thermal load.

Of course, the program itself, with Los Alamos people involved, Dave Bish and a number of others have looked at Yucca Mountain itself as its own analog because of the effects of heat from intrusions at Yucca Mountain in the past, which they can look at those effects and how they've influenced the transport of fluids and the precipitation of minerals. That's probably the best analog we've got is looking at those past performances of Yucca Mountain, which have been studied in some detail. That's the best evidence we've got of what would happen with the repository, I think.

DR. DOMENICO: So you're dismissing my concerns just like that.

DR. LANGMUIR: I think those people who are experts in that, both in and outside the program, should be involved in reviewing the program later on here.

DR. BROCOUM: Abe, do you want to say something?

DR. CORDING: Yes. Abe was going to the mic.

DR. VAN LUIK: Abe Van Luik, DOE. I was going to make a comment much along the lines of Don Langmuir, that we do have that expertise in-house and that one of our geothermal champions is Bo Bodvarsson, who is going to speak to us early this afternoon.

We have not explicitly identified the particular concerns that you were talking about as part of the peer review, but they will obviously be covered as part of the coverage of the important processes that we plan to put into the TSPA VA.

DR. CORDING: Thank you. Questions from staff? Sherwood Chu.

DR. CHU: I have a question on the EIS in your chart. It was hard to tell from the chart about the project topics as to where the issue of transportation of all of the waste would come into the NEPA process. Is the EIS work addressing only the repository side and the transportation issues will be addressed elsewhere?

DR. BROCOUM: I believe the routes to the repository will also be addressed in the EIS. Wendy has mentioned that. The details you have to go to Wendy for, but yes.

DR. CHU: Okay. So that EIS that you were showing is a program EIS rather than the project EIS, is that right?

DR. BROCOUM: I don't know.

DR. DREYFUS: That EIS for the repository encompasses most of the impacts, but it does -- and under the Nuclear Waste Policy Act, it is provided that that's the EIS. There are some constraints on what need not be looked at. Now, there is contemplated also in that body that if it was an interim storage facility, it would also have an EIS.

So depending on how this thing plays out, when the transportation takes place, I am not sure we have a definitive answer to what the NEPA documentation would be. We're anticipating looking at the generic issues of transportation in the repository EIS.

DR. CHU: And that would be part of that. Thank you.

DR. BROCOUM: There is an activity in the engineering area to look at potential routes in Nevada and to narrow them so that -- you know, in terms of less -- you know, the narrower you can make the potential routes, the less the impacts and the less you have to look at various impacts. So we're doing that for Wendy. That activity is being done for Wendy this year. So there is that activity in the engineering.

DR. CORDING: Other Board staff comments? Questions? Don.

DR. LANGMUIR: Don Langmuir. This is actually not my question, but one that John -- John Cantlon doesn't like to leave us without having something to say, so he's got like a memo with some possible questions to ask, and this one kind of intrigued me.

He pointed out that the five-mile safety envelope around the repository, why is it a sphere, why don't we make it elongated in the direction of groundwater flow and shorter, up gradient in the groundwater flow direction. Why don't we concern ourselves instead with the real direction that waste might take radionuclides and the distance rather than making it an envelope that's uniform?

DR. BROCOUM: In a sense, that will happen in the EPA standard, where they -- how they construct the standard. That is to the south, say, in the Valley Farm area, the envelope will be an elongated envelope, if you like, towards the south. So that's one of our concerns and the EPA is considering how and where that would be. So in the calculations that we do, we assume various distances to the south, and Abe will have some calculations to show you later and the various distances.

But in a sense, if it's more than five kilometers, then it will not be a sphere anymore, just as you suggested.

DR. LANGMUIR: Maybe it ought to be a time sphere, time of arrival sphere rather than a distance.

DR. CORDING: Questions from the audience?

[No response.]

DR. CORDING: In looking back on the fiscal year '96 with a reduced budget and all, to what extent are there items that you -- were you able to accomplish really what you wanted in '96 or what in part of that original '96 program have you deferred? I think we've had some discussion of that, but could you give me another statement on that or summary of what you're still working on or what you accomplished?

DR. BROCOUM: We stopped the suitability process that we had created, which was a very public step-by-step process for '96, that process stopped. We cut in half our interactions with the NRC and cut back a lot of the licensing work, in a sense, thinking ahead to the, say, license application.

Those areas were severely impacted last year. The suitability process has been replaced by another process. I think we would have liked to have more interactions. As we're going into this viability assessment, we want to make sure the NRC understands what we're doing and support us in a positive way when they're asked what they think.

So I think we need to have those interactions with the NRC. So in a sense, we lost a little bit, but we did as best we could under the constraints we had. So I feel we did a pretty good job last year. That's why I tried to show you the accomplishments of '96; not only doing things, but actually coming up with a new plan.

But we did have to cut back our NRC interactions, that was mandated, in a sense, so we did.

DR. CORDING: In terms of testing, you had to tighten up on some of the programs.

DR. BROCOUM: Yes. Dennis is standing there ready to help.

MR. WILLIAMS: Dennis Williams, DOE. Scientific programs, '96 started out to be a disaster for us. The 250 declining basically caused us to get into a mode of trying to capture all the information that we could, but shutting it down.

As things turned around and with an influx of some funding in some key areas, that led to a lot of our fracture coating data, led to a lot of the chlorine 36 information that we're getting, and at the renewed drive toward license application, I feel that we turned the scientific program around in '96.

We did have major cutbacks in like our service drilling program, but we were able to keep going with the C wells testing, which is in the saturated zone. We basically dropped the large block test out, but I think, as you've heard in the discussions, we've got the large block test back in now. We cut out a lot of our climate program in '96. We've got it back in now. In fact, we ought to be able to wrap the climate program up in '97.

So we took quite a hit and it was quite traumatic for a few months, but, again, with some funding that the director provided back directly into scientific programs and what we were able to do getting back on track to license application, I was, frankly, very pleased with the outcomes of the '96 program.

I think you will see some of the pieces of data that are coming out of '96 that we'll talk about later this afternoon.

DR. CORDING: Thank you very much. We're looking forward to that.

DR. BROCOUM: One more comment. The ACNW comes out and has a meeting once a year in the Las Vegas area. This year they had it in September and it was a very interesting meeting, lots of discussion, some of the same issues we'll be discussing today.

Last year, we were not able to support that meeting and I don't think the ACNW had that meeting in Las Vegas. That happened right after we got the budget cutbacks in September. So we weren't able to support that, but this year we were.

DR. CORDING: Well, we thank you very much, Steve. We're going to take our lunch break now and we are about 15 minutes even more ahead of schedule. So if you would, let's utilize this time in the afternoon. We'll take a few extra minutes for lunch, but if we could come back early, at 12:45 instead of 1:00 p.m., to begin the afternoon session, I think that will give us more time in the afternoon period.

So 12:45 to resume instead of 1:00. Thank you very much.

[Whereupon, at 11:27 a.m., the meeting was recessed for lunch, to reconvene at 12:45 p.m. this same day.]

AFTERNOON SESSION

[12:47 p.m.]

DR. CORDING: Our afternoon session is starting. The first presentation is on conceptual model of flow in the unsaturated zone, new insights. The presenter is Dennis Williams. He is Deputy Assistant Manager for Scientific Programs at the project office. I think he'll be also introducing Bo Bodvarsson, staff scientist from Lawrence Berkeley Lab, who will be participating in the presentation with him.

Dennis?

MR. WILLIAMS: From this morning, if visuals are the crutch of the incomprehensible or whatever the word was, get ready for the Tower of Babel, because there's a lot of visuals in this next presentation.

This will be a presentation that's a little bit of a presentation within a presentation. We do have Bo who will come up and talk about some of the details of the unsaturated zone flow model. He's basically the guru of that particular exercise, so I will let him discuss that.

Here is an outline of our presentation, and I wanted to make some introductory comments with regard to that. Of course, I'll do the introduction, get into the conceptual model of the UZ, the Montazer and Wilson, some of the data collection activities that have been ongoing, and Steve pointed out -- he didn't want to say that we were overwhelmed by all the data coming in, but I can let you know that we're pretty close to being overwhelmed by all the data that's coming in.

Of course, Bo will come in on the data interpretation and the modeling. I'll go back to the podium for implications of the alternative conceptual model, which basically is our handoff to PA, talk about some uncertainties, plans for future work, and some conclusions.

But what I wanted you to know is that the presentation today will cover a very broad area, starting with a review of some of this field data that provides the input in the zone flow model. Much of this information was presented in the July meeting of this Board in Denver, so I will largely summarize it and put it into a setting for discussion in what we call the evolving or the alternative unsaturated zone flow model.

We've had a lot of discussion about whether this is a new model, whether it's an evolving model, whether it's an alternative model, and, very frankly, I would prefer not to get too involved in those semantic discussions. We've got a lot of information coming in. I use the simple term evolving to mean it's changing with time. That's basically the way I view it.

Bo will present how this data is incorporated into that unsaturated zone flow model and, of course, as I said, following that portion of the presentation, I'll return and discuss the implications, which is our handoff to PA, uncertainties, and some of the planning that we're doing into the next fiscal year.

While some aspects of the integration of the data from the field to the laboratory into the flow model will be obvious, the larger picture of an evolution of an entire integrated process of field and laboratory data flowing into a model, flowing into performance assessment, and then into the development of follow-up work, such as we have in '97 and out years, should not be overlooked.

Perhaps a few comments on percolation flux. This is from the Dennis Williams perspective on percolation flux. We'll talk about it a lot in these next presentations. What I want to make sure that everybody understands is that there is no direct measure of percolation flux. There are a lot of indicators. There's no meters to measure that. It's not like breaking concrete cylinders, which I did in my past. You have a calibrated machine to break the cylinder, you have an ASTM standard that tells you how to do it.

We don't have these types of things with the percolation flux. So you get a lot of indicators. These indicators kind of spread across the board, but I don't think any of us should go away today thinking that we are dealing with absolutes on percolation flux.

With that, I would like to review a little bit the Montazer and Wilson model from 1984. That was in the SCP.

We have it in our highlights of the waste isolation and containment strategy. It's been with us for a long time. There's a lot of things about it that were involved in its development. It probably didn't have a great deal of data, but it probably had a lot more geologic intuition that went into it.

Some of the key points. It had infiltration. Because of the presence of the PTN, it was thought that there would be lateral diversion; major faults running through it which would be conduits to lower portions of the rock mass; relatively dry in what would be the potential repository horizon; a water table way down below; and, some perched water.

Infiltration in these early years was considered to be somewhere between zero and five millimeters per year. As we got into collecting more data, it started to tell us that, hey, maybe there was some substance to this model because we could tell from looking at the rock mass up here it was fractured; obviously, it's going to have some kind of an infiltration flux in it.

When we got down here to the PTN with some of our bore holes, we started picking up saturations. We could see higher saturations along the upper surface of the PTN. We drilled down into this area, it was relatively dry. So some of those things were telling us, hey, maybe this model is really what it's all about.

So then the basic question becomes what other data do you collect to test this model and how do you refine it as you move forward in time.

That brings us to this cartoon that looks similar to the conceptual model cartoon of Montazer and Wilson. It has some of the same components, same rock types. We have the Tiva up on top, we have the PTN, we have the Topopah Springs level, we have the Calico Hills. This is basically to demonstrate where some of the data is coming from and what is really influencing our thoughts on this particular development of a concept of the mountain today.

We still have the infiltration. I'll talk a little bit about some of Allen's stuff on the temporal or the spacial distribution of the infiltration. We've got more bore holes into the mountain now, more holes that we can get temperature profiles out of. We actually have bore holes through the Ghost Dance Fault. We can get pneumatic information. We can get gas pressure information out of these holes.

We go down deep and we can see saturation water potential from our various core data. We're starting to look at the faults that we've gone across, but the big thing that is leading us to this development, this evolution of our understanding, is probably the ESF.

Two things that I feel are really important to the development of that understanding are we've been able to look at the delicate fractures for the fracture coatings, the Peterman & Paces of the GAS/DOF, and we'll talk more about that a little bit later.

In addition, we're starting to see the chlorine 36 bomb pulse coming into the mountain, and we'll talk about that quite a lot as we go through the rest of the presentation.

Briefly, some of the data pieces. The infiltration map from Alan Flint. The spacial distribution. Here we have the layout of the site area. You can see the ESF sit on here for reference, some referenceable bore holes on that, and our scale of infiltration varying from values approaching zero up to 15 millimeters per year in the upper corner, northwest corner of the site.

We see a lot of the higher infiltrations associated with the crest. That's probably because we have higher precipitation there. The average for the whole area, I think he calculates something like 4.5 millimeters per year. That's an average that's really not relevant to a large extent because more importantly is the matter of taking this actual data and feeding it into the modeling. So we know what's going on at depth in these particular areas.

The pneumatics. The pneumatic diffusion in the mountain at the repository horizon, we see conductivity along some of the major structures that we have known about for some time, the Bow Ridge Fault, the Drillhole Wash Fault, the Ghost Dance Fault. This is probably the first time I have ever seen the Dune Wash Fault behaving in this manner.

We've had the Dune Wash down to the south. We've had a bit of the Embercut Fault zone through here, we've had little bits and pieces of it, but now it's starting to look like we've got some continuity along that particular structure. This has been real important for Bo's work on the UZ model.

DR. ALLEN: Excuse me for a moment. Clarence Allen. Is that based on data or on modeling?

MR. WILLIAMS: I think it's got a database, but then it's a modeled presentation. Chlorine 36, the typical diagram that we've seen a lot of, the distribution along the ESF in the stationing, the ratios of the bomb pulse chlorine 36. We have our threshold at 1500. We have the light-colored boxes, which are feature-based samples, and we have the filled boxes, which are the systematic samples.

All of the systematic samples running down here below the threshold of 1500 times ten-to-the-minus-15, chlorine 36 ratios, and the hits of the bomb pulse above the threshold in key places like the Bow Ridge Fault, the Sundance Fault, here in the vicinity of the Drillhole Wash Fault.

Some very late data coming into this sample set --

in fact, it wasn't even in the deliverable that Los Alamos sent in at the end of August, is some of the iodine 129 bomb pulse data and the technetium 99 data. Corroborating data from other bomb pulse environmental isotopes that's helping us verify the bomb pulse chlorine 36.

DR. ALLEN: Excuse me once again, Dennis. This means then that there is no other way to explain the carbon or the chlorine 36 other than bomb pulse. Originally, they were talking about the other spallations.

MR. WILLIAMS: Yes. Other spallations, they've done calculations on that. It doesn't appear to be valid. There was always the contamination issue. That does not appear to be valid. Now we've got the corroborating data coming in from other bomb pulse indicators. It looks like it's real.

DR. ALLEN: I see.

MR. WILLIAMS: If we look at the spatial distribution, this is the map of the surface with the ESF on it and some of the structural features running through the area. The little circles here are where we have concentrations of those bomb pulse hits.

We do see it associated with some structural features and, of course, we've said these are associated with faults, fractures, cooling joints, those type of feature-based occurrences in the ESF. I want to point out this area here, where we have the Drillhole Wash running through or the Drillhole Wash Fault running through the ESF. We have bomb pulse chlorine 36 in the vicinity of that particular fault. However, we do not have any bomb pulse hits on that fault structure.

The bomb pulse hits that we have fractures in that area are on smaller fracture features oriented in a north-15 to north-30-east direction. So the actual features are oriented in this fashion.

We had the best geometries right here for determining that because we are cutting those features at right angles with the ESF, with that tunnel. Down in this area, we see a few features coming up on that north-east orientation, but they're more difficult to determine because of the orientation of the main running along that north-east orientation.

One of our working hypotheses for this year is to look at more north-east orientations to see if, in fact, we are getting a structural control in the north-east orientation on these chlorine 36 bomb pulse hits.

DR. DOMENICO: Excuse me, Dennis. What was the orientation again? I got north-15.

MR. WILLIAMS: North-15 to north-30-east. That's the bomb pulse part of chlorine 36. In addition, we're using chlorine 36 for other purposes, the non-bomb pulse, for mass balance of chlorine to determine percolation flux through, from a global sense, through the bulk of the repository. This is some information coming out of Los Alamos from various drill holes that are showing the averages or showing the database from several bore holes, the frequency of the samples over here, and basically the averages for units like the PTn, the non-welded Paintbrush Tuft, Calico Hills non-welded, and the Prow Pass, and we have numbers in the -- or averages, two millimeters per year, five millimeters per year, three millimeters per year.

Again, don't take these numbers to the bank. These are indications. This is another indication. There is nothing down here that we are actually measuring that value from directly off a meter.

I put the fault map up here from Warren-Day. These guys are always involved in all of our discussions because we feel that there's a lot of evidence that there is structural control on bomb pulse chlorine 36 and we've got the guys doing the maps on the surface working with the mappers in the underground, comparing surface structure to underground structure.

In addition, this year, we have specifically identified a task in the structural arena to look at the structural implications of chlorine 36. We were doing it last year. It wasn't formalized. It was an ad hoc thing. This year we have formalized that. We want to figure this problem out.

DR. ALLEN: Excuse me. The red versus the black here.

MR. WILLIAMS: The changes between the Scott and Bonk and the new mapping of Warren-Day. It's basically an update of the Scott and Bonk maps.

DR. ALLEN: In other words, some of the black lines are now thought to be wrong in their position.

MR. WILLIAMS: Well, maybe not exactly wrong, but depending on just exactly where it lies on the surface. Remember, Scott and Bonk was a pretty large-scale mapping exercise.

DR. CORDING: The red is the update.

MR. WILLIAMS: Update.

DR. CORDING: And the black is the original.

MR. WILLIAMS: Was the old, yes. Fracture coating data coming out of the U.S. Geological Survey. This is noted as preliminary data, Peterman and Paces. It's not going to be preliminary anymore because I got the report in yesterday, the final report on this, and it ends up being Paces and others with Peterman in the list of authors.

Basically what it's showing is the distribution of ages on calcite and opal from the ESF samples. Again, this is one of the big things that the ESF did for us. It allowed these guys to go in here and look at these fractures in place and find these delicate textures, such that they could strip off and date.

There are some very interesting things in that report that just came in, their September deliverable. One of them has to do with flux. Zell has calculated the total amount of calcite in the mountain, the total amount of opal in the mountain. He looks at the deposition on these fractures. He can see deposition, constant deposition over 12 million years on some fractures, continuous deposition, without breaks, is what he's telling us.

He uses this information plus what he's calculated to be the total volume of water deposited calcite and opal in the mountain, he comes up with percolation fluxes from that; for calcite, 2.1 millimeters per year; for opal, 0.3 millimeters per year. Again, that's what's in the report. I haven't double-checked his calculations. I probably wouldn't understand the calculation anyway, but it's another indicator. It's not something we take to the bank, but it's an indicator of what may be going on in the mountain.

DR. DOMENICO: One question. We recognize fast paths and slow paths, these kind of ages. Are we looking at the samples that were taken when we're looking at the slow percolation rates?

MR. WILLIAMS: We're looking at very slow percolation rates.

DR. DOMENICO: And you're still getting as much as you anticipated, 2.1 to 3 millimeters per year.

MR. WILLIAMS: 2.1 for calcite, 0.3 for opal.

DR. DOMENICO: For the presumed slow pathways.

MR. WILLIAMS: For the presumed slow pathways. Now, I think some of you were involved in a workshop that we had out at the ESF. One of the things that we do not see is a lot of -- we do not see fracture fillings in those fractures where we find bomb pulse chlorine 36.

So there's still something to be sorted out there of why. One of the things that Zell tells us is he feels that the fracture aperture has to be something on the order of five millimeters to have the head space for these fracture fillings to develop. The fracture apertures for the bomb pulse chlorine 36 are very tight, very small.

Another indicator, temperature, the geothermal gradient. Basically, the site area, the gradient at the repository horizon over the mountain, based on this set of bore holes. Over here we have the scale from 18 degrees up to 26 degrees Centigrade.

Over here we have a plot, a modeled plot showing potential fluxes as related to temperature, and this particular data set here is for a rather short bore hole UZ5, but it was out of one of Rousseau's reports. But here we see the -- I call it the dampening effect of the heat due to the percolation flux. That's my simple terminology for what's happening here. But we see these values coming in somewhere between that one and ten millimeters per year on percolation flux.

Again, a lot of discussion, a lot of argument about the validity of this approach, but it's another indicator. It's something else that we have to look at in more depth in order to understand exactly what we're dealing with here.

Perched water data. The lack of equilibrium between the fractures and the matrix. In large part, here, based on chlorine concentrations, and we have our perched water over here, showing relatively low chlorine concentrations; however, much higher up in the pore water samples, both up in the PTn and then down here in the Calico Hills.

Plus the fact that the perched water is coming in with an age of around 7,000 year old water. How were we getting 7,000 year old water through all this old matrix water up here, 200,000 year old water, unless something else is going on in the mountain that we really haven't got an understanding of yet.

Perched water. There's been some calculations on the perched water; what kind of fluxes are associated with the perched water. Our numbers range from zero to 0.3 millimeters per year, as minimum values. These are indicating down on the low range.

One of the difficulties of dealing with these kind of numbers is you're dealing with a perched water volume, a very illusive item to deal with from a volumetric standpoint, calculating that volume of water.

Just a couple more real quick ones on the perched water. The distribution of the perched water that was -- from a bore hole standpoint that was used in the modeling, they don't have SD/12 on this, an oversight. Striffler's interpretation of the structural controls potentially associated with perched water, possibly the top of the PTn, the Calico, possibly down in the Calico. It's basically some background on that.

And if we look at perched water from the north end to the south end, with the north-south main basically through this area here, looking at where the perched water resides with regard to the vitric-zeolitic boundary, we see the vitric-zeolitic boundary down here in the dotted. We've got the perched water sitting down here, very close to that particular boundary.

So at that point, I would like for Bo to join me and basically go into some of the details of how this data, these interpretations are used in the unsaturated zone flow model. Bo?

MR. BODVARSSON: Thank you, Dennis. Yes. I'm going to talk just a little bit more about the details of the modeling work that has shown us with respect to many of the things that Dennis has already mentioned. My outline is as follows.

I'm going to talk first a little bit about the UC model, UC meaning unsaturated zone flow model; the data that go into it; the model calibration; and then I'm going to end with some talks about percolation flux studies and what does all of this mean.

Then Dennis is going to talk later about the testing that we are planning to reduce uncertainties in all of these things.

It should be noted that the unsaturated zone is a big volume of rock and practically all the participants in the project, including all the labs and the survey, have been doing a lot of data-gathering that is useful to to the UC model, as I will show you a little bit later.

Another thing I want to point out for the Board, though, there's a series of reports that were issued, DOE milestone reports, that are available now. I think most of them have been approved by DOE; hopefully, most of them have been approved. And a lot of the stuff that I will be talking about is in the UC model report, the big milestone, and also some, of course, in June's milestones, as well as Zell's milestones.

I want to talk now a little bit about the model, just two brief viewgraphs. This is somebody else's, so I'm not going to talk about this one, although it actually looks pretty good.

I want to talk a little bit about the UC model and this is basically just a top view of the model that shows the area that we are considering. You know the ESF, that is coming around here. Currently we are located about here. You see the fine grid in the repository area where we expect to put the waste.

We have now extended the model to the west to be able to take into account the different bounding conditions for the Solitario Canyon Fault. We may have rainfall and infiltration in that area. And we have taken into account the location of all the wells, as well as most of the major faults in the area.

This is a three-dimensional model that is used to calibrate against moisture flow, against gas flow, against temperature.

Now, what is the purpose of this model? The purpose of this model is, in my view, first and foremost, the evaluation of percolation flux. And why is that? Dennis mentioned percolation flux about 20 times in his presentation and I'm going to try to beat that, at least 21 times. But the reason is as follows.

When you take a look at the waste isolation strategy that many of you have seen, there are five attributes to it that are listed here; from the seepage into drift, to the waste package environment, including the humidity environment, the waste mobilization, the radionuclide transport, both through the engineered barrier and the rocks, as well as dilution. Out of these five, four are strongly controlled by the percolation flux going through the mountain.

So this is unquestionably the most important parameter that we have to determine. What do we have to determine? We have to determine the spatial variability of it, number one, because we know it's not a uniform number in the repository horizon. You need to also determine how much of this percolation is actually going into the drift, because if it bypasses the drift and doesn't contact the waste or create an adverse environment in and around the canisters, that's fine.

So there are basically two problems or three problem that I'm concerned with in the modeling. First is the spatial and temporal distribution of the flux globally. The second one is how much of this goes into the drift through discreet features. Thirdly, then, flow path, of course, to the water table, because that affects the radionuclide transport issues.

So that's all I want to say about the model and I want to talk a little bit about the data. We get data from a lot of different people, a lot of different organizations, and it's kind of summarized in this viewgraph.

This is basically the UZ model box, which is a core operation between the Survey and LBL. Then what feeds the UZ sat scale model is geology and geophysics, matrix properties, fracture properties, infiltration, in situ thermodynamic conditions, environmental isotopes, the pneumatic gas data, the ESF moisture balance data, and all of this information has to be in the model to make it calibrated and make it the best model we can possibly make it.

As you see, almost all the participants that are involved in some way or another are feeding data into this model, and we appreciate that, of course. Then what comes out of this model, as was mentioned before, both in Steve's talk and other talks, this feeds directly into the transport model and, most importantly, into TSPA. I think Abe is going to give you a very good example this afternoon of how he used basically the output from our model milestone this year to do very quick TSPA calculations to see the impact in just a few days, which makes this a very good kind of integration.

Then we feed this into thermal modeling and then gas transport models.

I'm not going to talk any more about the data because Dennis told you all about the data so far. I'm going to talk now a little bit about the calibrations. How do we use this data? This is not in your package, but this is kind of the approach we are using in calibrating the UZ model. As I mentioned before, we have the three balances; the gas balance, the moisture balance, and the energy balance. All of them must be contained in the model because they are all coupled.

By far, the most important one, of course, is the water balance, because that controls the percolation flux, the spatial variability, the temporal variability, and flow into seeps. So that is strongly affected by the gas and the heat. So there are couplings in all of these.

We also must take into account the energy balance both for the thermal loading calculation and also to make sure that we have the proper heat transfer mechanism in the mountain, because you as know, the mountain is not isothermal; 33 degrees at the water table, 18 degrees at the surface.

Then the pneumatics. Pneumatics, as you know, when weather storms move past Yucca Mountain, the signal goes hundreds of meters into the mountain and we use that to estimate structural permeabilities of the Yucca Mountain rocks. That's very important, too.

With this approach, I'm now going to look in a little more detail about the percolation flux than what Dennis has been doing. Here is, as he mentioned many, many times, there is no unique way of determining percolation flux. But given the fact this is so important to determine, you must take into account all of these different areas and try to figure out where does it fit on here, on this percolation flux estimate.

I'm going to put them all together for you. It should be emphasized again that this is a continued evolution, our work, and none of these values are really absolute. We have to do some studies and tests to make sure that we are in the right ballpark.

Starting with infiltration, Dennis showed you this map here. What is actually interesting, two years ago, when we did the latest big model, UZ model, I used perc infiltration values ranging from .001 to 20 or 30, a huge range. If he can somehow bracket this range more, then we will have a lot more confidence in our performance assessment calculations rather than just go with tiny values and high values.

The infiltration studies by Alan Flint seem to be doing that. He is now converting what he believes is a very reasonable representation of the infiltration at the surface, which is basically in the repository area, ranging from about zero to 14 millimeters per year.

When I use the UZ model to calculate the average flux in this area, you get about seven to eight millimeters per year of flux due to infiltration, because you have the highest value at the ridge tops here, but then due to the tilting of the layers, this water moves down through the mountain and some of it spreads out in the Paintbrush, and then you get a large area, about seven to eight millimeters per year.

So we'll put this on our lower map here, on the right-hand side, and I'll put an "I" here that indicates estimated flow. Alan Flint indicates infiltration estimates percolation to be around to five to ten millimeters per year.

Going to the next one, which is the saturation and moisture tension data, as you know, we have about ten to 12 bore holes at Yucca Mountain, all of which give us this information here. They give us these dots that indicate saturation measured in cores and they give us these values that indicate also moisture tension measured in cores.

In addition to that, Joe Rousseau and his coworkers have obtained in situ moisture tension values that allow us to refine these estimates, because you see they are very variable.

You can use a model like the UZ model to see what infiltration rates best reflect these data from all ten to 12 bore holes. When we did this in the past, our conclusion was always that the moisture tension and saturation data favored low fluxes. Why was that? That was because we didn't have a detailed infiltration map like we have now, so we had to use uniform infiltration maps and then some of the bore holes that showed some very low saturations did not like it when we input high infiltration fluxes.

Now, with this information, our best estimate is that the moisture tension and saturation data is consistent with fluxes on the order of one millimeter or so, and I will put that right there, saturation.

The problem with these data, and they are very inaccurate, is that the rock matrix -- and Dennis mentioned this a little bit. The rock matrix, if you have transient pulses moving through the mountain, the rock matrix may not see those transient pulses. They may not impact into the matrix because they go so fast through the fractures. So this is only representative of long-term kind of slow fluxes, I would think.

Going right down, let's talk a little bit about the pneumatic data, and Dennis mentioned that also. I think the pneumatic program that I think DOE started like -- what is it -- three years ago, you think, Dennis -- has been a great success. It has told us tremendous amount of things about the mountain; not moisture flow, because this is gas, but also a lot about the features and fractures and faults.

This is just one example of a signal in -- well, this happens to be UZ7A and these are the sensors in the Topopah Springs area. What you have here is the calibration period which we used to calibrate our UZ model and the red line is the simulation and then the gray line is the data, and you see the calibration is very good, and then these are the predictions, because what we do is in order to establish a track record, you do blind predictions and then compare our model results to the actual data.

So what we do is we wait six months and then Joe Rousseau at the Survey sends us the surface signal moving through Yucca Mountain and the biometric pressures at the surface, and then we do our prediction for that period and send the data to Joe and he sends us the real data. So we really have a blind prediction and this is very well documented.

You can see the pneumatic is very well represented by the model and what this gives us is a very nice picture of gas permeability in situ. This is the map that Dennis showed you. There are two signals here. One is at the ground surface, the storms moving past Yucca Mountain. The other is the signal going from the ESF, because that has the same pressure boundary condition as atmosphere.

From the atmospheric or the surface signal, you get the vertical permeabilities, and from the ESF signal, you get the horizontal ones. So this really allows us to determine very accurately or reasonably accurately the permeabilities of the rock mass.

Now, to summarize for you what we have seen, we see basically in Tiva Canyon ten Darcys, horizontal and vertical permeabilities, roughly, all of Tiva Canyon, that's permeability of the gas flow, and this corresponds very well to Gary LeCain's Air-K data, and you would see this in bore holes.

When you go into Topopah Springs, you see a non-isotropic system, with a horizontal permeability on the order of ten Darcys -- a vertical on the order of ten

Darcys, horizontal on the order of two Darcys; still very permeable, very fractured, very permeable. And the fact is for every single bore hole, you see it going deep into the mountain. So there are always pathways in the fracture system. Very continuous fracture systems.

With respect to the faults -- let me -- the PTn in the middle is about 300 milli-Darcys, roughly. When you look at the faults, you see different permeabilities and much, much higher permeabilities for a lot of the faults. For example, when the ESF penetrated this fault here, which may be an extension of the Dune Wash Fault, that goes up to UC four and five, this is like a 500 meter distance and we saw it instantaneously and the model estimate is like 1,000 Darcys for this fault, very permeable, right through these bore holes.

But the other indication, this is also very interesting, too, is that you're matching a fault here intersecting these bore holes and you see the ESF provides the pathway and the surface signal much quicker than what goes from the ground surface. Right? Now, what does that tell you about the vertical permeability from the repository up to the surface? It must be low because it doesn't see this right from the start. It has to see it from the ESF.

This is the case for several other faults, too, like this one which connects to NRG6. It goes -- ESF hits right here and it goes down the fault here, we think, and then intersects with the bore hole, giving very high permeabilities of the fault zone.

The pneumatic, though, you always must keep in mind, does not give us any indication about percolation fluxes, just flow of gas. But it gives us indirect evidence because, for example, if there was no permeability in the fault from the repository to the surface, it may be water filled. But it's not. There is some permeabilities there.

Going now into environmental isotopes, and Dennis talked a lot about chloride 36, bomb pulse things, and I want to say a few words about that. The chloride 36 bomb pulses cannot be used to estimate percolation flux because we believe and I think everybody in the project believes that these are localized phenomena due to the fault going through the PTn, and you see that that's been very few areas. At best, you will estimate a localized flux for a single point in the repository horizon.

But what the Los Alamos people and Bruce and June have done is to try to use the non-bomb pulse chloride 36 that Dennis mentioned before. That is the one which is below the magic number of 1500 times ten-to-the-minus 15 in the ratio. What you see in the graph that Dennis showed is that most of the values are between 500 and 1200 in the repository horizons.

So what they have constructed is the past history of the boundary condition at the surface, the best they could, all the chloride 36 through chloride ratios in the past. We know that in about the last eight to ten years, it has been roughly 500 or so. Of course, with the bomb pulse, it was much, much higher. At the time, we got to zero here. But then they constructed these models, as well as Patrack Mitten's data that they get from fossils, that allows them to construct a history that looks something like that.

And what does it say? I says that about 10,000 years ago until about 40,000 years ago, it was roughly around 1,000 or so, this ratio, in the atmosphere at that time. Then over the last 10,000 years or so, it's much, much lower.

They're using this information and doing numerical simulations. They conclude that basically the flux should be somewhere on the order of one to five millimeters per year. Why is that? Let me explain that.

This is their one-dimensional simulation using station 35 and this is the geology. For one millimeter per year, they get these fracture versus matrix flow. That says basically matrix is the solid line and fracture is the broken line. This is basically in the PTn. Of course, we have all matrix flow. In the Topopah, we have mostly fracture flow. And then they did the simulation using this chloride, the source term on the surface, and calculate the profiles of chloride 36 going down through the mountain?

What do they find? That if the flux is very low, they find chloride ratios which are lower than 500, because, remember, from the source term, it was about 500 at the surface over the last 10,000 years or so. Actually, this was for -- this would take like 200,000 years from the surface to the repository horizon. So this would be the very, very, very old source signal. This is estimated to be lower than 500.

When you have like five millimeters per year, it takes only like 10,000 years or less or 10,000 or 20,000 years to go to the repository horizon and that's how you get these higher values of chloride that we're getting on the repository horizon, like 500 to 1000 in ratio.

So they conclude from this analysis, again, which is uncertain, like all of the analysis, that the flux would be somewhere in between one and five millimeters per year. So we will put that here. This would be environmental isotopes, one to five millimeters per year.

Now I'll go into fracture coatings, and, again, Dennis mentioned a little bit about this. This is just a little cartoon. This is work from Peterman and Paces, again. Remember, in the past, they used to do this analysis of a single fracture to estimate the percolation flux using a continuous depositional model and they came with something like ten-to-the-minus four millimeters per year in flux rate. But when they do this estimate of doing a global estimate of the total amount of calcite in place, calculating then the total amount of water needed to deposit the calcite, over 12 million years, they get the two millimeters per year that Dennis mentioned.

So this is a global calculation using the ESF and the calcite contents of the rock and extrapolating it over the entire mountain to get at the percolation flux rate, and that comes through about two. So we would put the F right there.

So moving right along, let's go to temperatures, right here. Temperature data is available for 20 wells -- from 30 wells at Yucca Mountain, a lot of wells, and the bore holes are shown in this viewgraph and this color scheme just shows basically the elevation.

Now, what I wanted to show with this color scheme is that most of the bore holes are located in the washes. They're not located on the crests. They're more in the washes.

When you look at the temperature data, you will find that the thermal conduction alone cannot explain the flow of heat from the saturated zone to the surface. Sass estimated that the total heat flow in the area is something like 40 to 50 milliwatts per meter squared. When you use that and the temperature gradient in the Topopah Springs, as well as the measured thermal conductivity in the Topopah Springs, you get only half of that, like 20 or 25 milliwatts per meter squared.

So something else, if these estimates are correct, must be carrying the energy from the saturated zone to the surface. There are two theories, both of which Sass mentioned in his paper. This is the 1988 paper that you probably have seen. One of them is gas collection. Gas comes in and since the humidity is low at the surface, it might be 30 percent, comes in, low in water content, then it gets to higher temperatures here, so the solubility of water in the gas is higher. It picks up the water here through evaporation process, brings it up here where the temperatures are lower, and then, because the temperatures are lower, the solubility of water in the gas in the air is lower, is three percent here, is about one percent here. So that water has to condense. It has to go out of solution. It can't stay in the gas phase because of thermodynamics.

What happens when it condenses? The latent heat evaporization for water is very high, very large. So that a small amount of water carried with the gas gives tremendous heat transfer up through the mountain. It has been estimated that it only takes like 0.2 millimeters per year to bring the energy from here to here through this proces. This is one possible explanation for the heat transfer.

The other one is that percolation. That's what we have looked at the details. If you introduce water through infiltration that is percolating through the mountain, there must be some energy taken to heat it from 18 degrees to 33 degrees, because it has to get heated. That energy comes from the heat flux through the mountain.

The interesting thing with this, when I started looking at this, I said let me look at a few wells and see if we get the consistent picture in these temperature gradients or if they are all over the place. And heaven behold, when you take a look at the gradients in the Topopah Springs, these are the temperature gradients in degrees C per kilometer, most all of the wells in the middle close to the crest have the same gradient in the Topopah Springs.

Now, what does this look like to you? This looks pretty much like the infiltration model, doesn't it? Because the highest infiltration that Alan always estimated was around the crest here. This is the same one Dennis showed and this is the calculation that Ed Kwicklis and Joe Rousseau showed and, again, they are looking at the gradient, but for a very shallow bore hole, and they showed for .1 millimeter per year infiltration, you have a gradient of 22 degrees per kilometer. When you go to ten, you have a gradient of only 17.

So what does that mean? For our 18 to 19, this corresponds to a percolation flux of some five to ten millimeters per year using this approach. A very consistent picture. And if you compare this picture to the percolation flux that we get from the UZ model, unfortunately, these scales are not quite the same, but you see here, based on our model, about eight millimeters per year over an area that spreads out here and spreads out a little bit here in the same area. This may be five. This is calculated from the infiltration down to the repository.

So it looks very, very similar. So these indicate -- from this study, it indicates perhaps five or so millimeters per year infiltration. So we'll mark that like T. But don't forget that there is some alternative explanation that we are investigating, and that is the one with the gas flow. If this one turns out to be correct, the flux may be much, much lower, based on this analysis, but this is what the analysis shows so far.

There is one thing that may not make this model -- that we need to investigate this model, is that if this one is true, it takes very large velocities to go through the mountain, like 15 meters per second -- 15 meters per year was what Sass estimated. So all of this gas would have to be very young, at something like 50 to 100 years, and I'm not sure the data agrees with that. But we need to look at that.

The final thing is perched water. What does perched water tell us about percolation flux? Dennis quoted the north ramp report from the Survey that looked at the perched water around the UZ14 and they concluded basically that it's on the order of .3 millimeters per year and most of it through fractures, because the chloride content on the perched water is very low. It's like seven or eight milligrams per liter, much, much lower than that in the Topopah Springs.

We've done some modeling to try to get the perched water body and we get it at the right place, as you can see, around UZ14, NRG7, SD9. We even match the pump tests done in this perched water body to try to get a handle on the volume and things of that sort. And what we've come up with is like it would require like one or two or so millimeters per year or on the order of one millimeter per year to explain the perched water body. A lot of that flux should flow through the fracture because of the chloride contents. So this is perched water body.

So what does this mean? You take a look at this and you say, heaven behold, this all looks like one to ten millimeters per year. All of these methods seem to suggest that the range is something like one to ten millimeters per year. But it's certainly not conclusive. All of these methods are very uncertain. There are plans in place that Dennis will tell you about that are going to investigate and try to discriminate between all of these different approaches and try to get us a better handle on the percolation flux.

If I were to conclude, in my heart, what I thought personally based on this data, the percolation flux would be, I would say, somewhere around one or a little higher than one perhaps, based on this data and perhaps might be somewhere in this range. I would certainly not rule out this area here because you don't see any water coming into the drifts. Some of the data indicates lower fluxes. We know that the percolation flux varies spatially.

So there are all kinds of uncertainties and I think the important thing is, when Dennis tells you a little bit later about the tests that we are planning to do to look at this.

Now, what does it mean? What if the flux is so high? What will it tell us? Let's look at that a little bit. We have in our report investigated both the higher flux estimates as well as the lower flux estimates to give us an idea how the flow patterns are in the mountain, given a low flux and a high flux, because we are not ready to throw away the Montazer and Wilson low flux model to the repository yet. We need to study it. These are two alternative models that need to be investigated.

So we looked at this through the three-dimensional model and here you see what generally you get from these kinds of models. You see you prescribe some kind of infiltration flux and in this case, we used a very low flux. Then you get saturation profiles, velocities and flux rates and gas pressures and temperatures all through the mountain that you can look at.

Some of the things that are most important for us certainly is the saturations at the repository level and the fluxes at the repository level, and Abe will show a little bit about that in the TSPA calculations. The other thing certainly is the vertical mass flux at the water table. This started out with a uniform point, one millimeter per year on the surface, but you see how heterogeneous it is at the water table. It varies from nothing in many regions to large amounts that have accumulated because of flows, because of the vitric zones in the Calico Hills, which are more permeable than the zeolitic holes, zeolitic rocks and all of those kinds of things.

The important thing, though, to look at is what happens to the basic features of the mountain in terms of lateral flow, in terms of faults, in terms of ground water travel times, in terms of all of these things when you consider low fluxes and high fluxes, and the behavior is drastically different. We must look at this and then design a testing program to go after these teachers to tell us which one is the right model.

Take a look at this one. This happens to be a low flux going from UZ14, to east and west, and you remember -- I will show you this cross-section really quick. It's just basically a cross-section, east-west, in this region right there, UZ14. And when you look at this, you see one thing that stands out. For this low flux region, you get a tremendous amount of lateral flow through the PTn, over kilometers, you have a flux as low as .1 millimeter per year.

We can also have a tremendous amount of lateral flow on top of the zeolites, the low permeability zeolites in Calico Hills. But what does that mean? Some of these accumulate in faults and large features and move down pathways. The other case, this is Alan Flint's infiltration map, of five to ten millimeters per year, and, heaven behold, you don't get any lateral flow in the PTn whatsoever. You get lateral flow in the top of the zeolites to some extent in some of the simulations. In other simulations, you don't. They're not on top of this.

So the final slide here. The emerging alternate conceptual models that need to be investigated and looked at through a testing program, modeling, lab tests, whatever it takes. Here's the one that says we have a lot of -- this is the Montazer and Wilson, plus variations thereof. That says water comes in spatially, variable on the top, it flows laterally.

The other one says if you have higher fluxes, you have higher infiltration rates, there is no lateral flow, there is lateral flow in some of the zeolites that are the effects of faults below the repository. There is extreme complexity in the flow paths below here because you have perched water. You have vitric zones with high permeability. You have zeolitic zones with very low permeability. So the flow patterns are very complex here and need to be investigated, and we cannot discriminate between any of these potential flow paths because we don't have much data. But it suggests don't look at lateral flow in this region.

In this model here, perhaps we can look at the chemical changes in the PTn. If you have a huge lateral flow, that should show up in the fluid, because you would have very large resident ponds close to the pulse, for example.

So this is where I leave and Dennis is going to tell you what we are going to do to discriminate between those two.

DR. DOMENICO: One question. I think it's probably important to emphasize that in this total analysis, you did not consider the bomb pulse chlorine. Everything that you've done sort of pertains to what we have been referring to as slow pathways.

MR. BODVARSSON: Correct, yes.

DR. DOMENICO: That is correct. And we're getting numbers that are sometimes greater than one, but it's strictly restricted to the flow pathways, everything that you've addressed so far. Okay. Whatever it is, it's later. Not necessarily. Just that travel time might be more.

MR. BODVARSSON: With chloride 36, as far as I'm concerned, these just tell us that something is out there.

It could be a minor amount and it may not matter at all, because it's so small amount that there's no impact on flow into drifts or any of those things we are talking about, because it doesn't tell us anything about the volume. It just tells us something is out there.

DR. DOMENICO: That's correct.

MR. WILLIAMS: You guys are suffering through the unfortunate situation of having back-to-back long-winded guys in the same presentation. So I'll try to scoot along here for the rest of it and see if we can maintain some schedule.

I wanted to mention the implications of the -- I'll let Bo get out of the way here. I wanted to mention the implications of this evolving or alternative model, whatever kind of term we want to put on it. This is basically the hot potato we're handing off to PA, where Jay will talk about it. Higher percolation flux at the repository horizon. If we have the higher flux, we may be dealing with higher humidities and, obviously, we may have increased percolation flux to the water table.

Down in the transport area, below the repository, I mean, we've got quite a bit of understanding about what's -- or we're developing an understanding of what's going on between the surface and the repository. When we get down below the repository horizon, we -- it's a more difficult situation down there.

That's where the transport part of it is going to come into play. I guess I would be so bold as to suggest that maybe the Board needs to hear a presentation on the transport model, because we have a UZ transport model that's in -- that we deal with, as well.

In a future presentation at a future meeting, I think that we need to have that transport model discussed and talk about some of these other implications from a transport perspective.

The uncertainties associated with this, and we started off talking about some of the uncertainties. Don't take the numbers to the bank. However, we do know that this percolation flux is going to affect four of those five major attributes of the waste isolation containment strategy.

We're trying to use a variety of approaches to evaluate that global percolation flux and using as many corroborating lines of evidence as we possibly can.

There are certain uncertainties due to the techniques we actually use. The chlorine mass balance method was originally developed for soils. We're using it in a rock system. Of course, we are using things like the temperature, the chlorine 36, the fracture coatings, et cetera.

One of the things that's been mentioned about the bomb-pulse chlorine 36, we can get even an indirect measure of the percolation flux from that. However, from some of the modeling, we know that it's going to -- or we believe it will take percolation fluxes in excess of one millimeter per year in order to get those bomb pulses through the PTn.

So that wasn't mentioned, but that's potentially another data point that would fit on Bo's thermometer.

And the flow regime -- the uncertainty of the flow regime below the repository horizon. Again, we're down in that area, where we'll be talking about the transport. We don't know the extent of lateral flow in the Calico Hills, what the fracture matrix interactions are going to look like down there. We may be able to infer something with regard to the welded unit, but when we start getting down to those vitric units and those non-welded units, it's a different story, and what are we talking about as far as fast pathways to the water table.

The ESF provides us a location about halfway down. We believe we have fast pathways to that location. How much further down do they extend?

Plans for future work, FY-97. The big one is the utilization of the ESF. This has been one of the big advantages that has happened to us with regard to looking at the mountain. We've actually got this large diameter bore hole in the ground, running across it. We're going to continue the sample fracture coating information.

The work that Zell and others have done has been very valuable to us. We believe that there is a lot more that can be derived in that area.

The environmental isotopes, not only the chlorine 36, but try to get more in the iodine and the technetium on that.

The percolation flux test. We have a variety or proposals on a large scale percolation flux -- I shouldn't say a variety of proposals. I should say a proposal from a variety of people wanting us to do that. We're evaluating that this year.

Other possible hydrologic tests. We have proposals in on cutting small niches in the ESF to try to capture one of these transient pulses running through there. We're looking into that.

Continue the moisture monitoring in the ESF. This will probably be a lot more productive effort after we get the TBM out of there and can control the ventilation system a little bit better. And the continued study of the Ghost Dance Fault. Two alcoves going into the Ghost Dance Fault, I think that this is going to give a real hands-on look at something that may be the ultimate fast path going through the mountain.

Long-range plans, more of the same. Again, dealing with the ESF, trying to use it to maximum advantage. The Ghost Dance Fault alcoves, conduct the perc flux test, conduct the UZ transport test, the one that I talked a little bit about earlier during Steve's presentation, and then see if we can get a handle on some of these other things that we want to do with regard to hydrologic properties.

We've got contacts out there that we haven't done a whole lot of work on yet. Our funding profiles haven't allowed that. We can go back and look at the PTn, both the upper and the lower boundaries, and that's a fabulous opportunity to do some more work along that contact to understand better how water is moving through this mountain, including those fast paths.

In conclusion, what would I say here? Probably the first one Bo summarized at the end of his part. We may be in the five millimeter per year range. Some view that as potentially the upper boundary, but I think we have to recognize that. One to five, somewhere in there, that could very well be where we're at.

A lot of discussion on our alternative conceptual model, about de-emphasizing lateral flow at the PTn. I think that that depends on how the percolation rates are going to turn out. Faults as drains above the repository horizon, we didn't talk about that too much, but that's one of the implications of some of the modeling effort that we're looking at right now.

When we get down below the repository horizon, as I said, things become not only more complicated from the fact of trying to understand it, but how we're going to get at that information, because there's not a lot of good ways for us to get at that. Bore holes are possible, but you know what kind of information we've been able to derive from bore holes in the past. It's been good, but it hasn't been as comprehensive as we would have liked.

We mentioned the implications of the higher percolation flux and, finally, the long-range plan. We are trying to take into consideration all that we have learned to develop the working hypotheses, such as the north-east trending fractures, such as some of the infiltration data, development or the refinement of the models, plug that information -- or plug those concepts into our planning for '97 and the out years, take advantage of where we can get our best information, and just move forward in that particular fashion.

Thank you.

DR. CORDING: Thank you, Dennis and Bo. We're pleased to have the opportunity here to see some very interesting information and data as you're putting it together and I'm sure we're going to have some interesting discussions here, as well.

Don Langmuir.

DR. LANGMUIR: This is pretty exciting stuff. Looking at your lines of evidence for the infiltration flux that you're coming into a bound, at least, for low level value, some of that evidence, as it looks to me, is time integrated and would suggest to me -- and I'll jump now -- that maybe it doesn't matter what climate is going to do in the next million years, because you've integrated much of the past climate changes into these flux measurements and estimates that you've made by several different techniques that are time independent largely. They've integrated those times.

How dangerous is this to jump this far?

MR. WILLIAMS: I personally don't know how dangerous that is. However, I would like to point out something that was in the Paces, et al, including Peterman report on fracture coatings. They have a comment in there that over the last million years, 80 percent of which time has been colder and wetter, although they do not see a significant difference in the deposition rate of these fracture coatings over that period of time.

So I think that that's following along the same lines of what you just stated. It may not be relevant to what's going on down there and to be able to understand the mechanisms for why, I think we've got a long way to go to understand that, but that's what the data are indicating.

DR. LANGMUIR: One more. Bo had some lovely plots, and this is for either one of you, I guess, showing what you -- how you've been able to integrate your knowledge of -- on a map scale with a repository in the figure of what the infiltration rates apparently look like they're going to be.

Do you need an east-west crossing? Are you happy with what you think you know about the west side of the repository block without having to measure it? You've drawn those counters right across the block as if they were comfortable knowledge, using all these different techniques of determining infiltration.

MR. WILLIAMS: We have a lot of smilers out there in the audience. They know the question and they know me. I think I would just as soon set with the director's explanation of the plan for the east-west drift. I think at the appropriate point in time, we will discuss with him and provide him input on what we can achieve, potentially achieve with that type of an exploration, but I wouldn't --

DR. LANGMUIR: One of the things that you've got to do when you put this in your plan for next year is look at all the techniques that you're using for infiltration estimates and reduce the uncertainties in them. This would reduce some uncertainties, would it not, in a whole host of measurements that you're using for this exercise, or might it?

MR. WILLIAMS: I will take your comments into consideration and plan on some carefully considered remarks in the future. Bo might want to make some remarks.

MR. BODVARSSON: One comment. Going back to your last question first, really briefly. You are right. Some of these techniques are time integrated, but most of them are not. The only really time integrated one is the one on the fracture coatings, over 12 million years. The temperatures is more what we would call an instantaneous one, although temperatures take a long time to equilibrate, but we are talking about tens or hundreds of years, not something that will affect the climate change.

So these techniques, some of them integrate the climates, others do not. So I still think the climate issue is a really critical one.

With respect to the east-west drift, one of the things that is on the books or we are perhaps planning in terms of the percolation test is rather than an east-west drift, which is more of a what I would call like another ESF which covers just a line perhaps, one thing that DOE is thinking about is looking at an area like 100 meters or something like that, horizontal bore holes or some other ways where we can get the sampling over over a large area, because that -- basically, percolation flux is an area concept. It's not a line concept.

It's an area concept and with that, if you can get access to an area with all of these techniques, as you pointed out, Don, the chlorine 36 non-bomb pulse, the fracture coatings, the saturations and temperatures and all of those. Then I think, from my modeling standpoint, you have a better chance to get a better estimate of the percolation flux rather than with a line. Do you understand what I'm saying?

That's all I wanted to say. Sorry, Dennis.

DR. CORDING: Could I continue with that some more, Bo? Ed Cording, Board. Even though you're looking at area, you're still -- you're looking at conditions along, say lines of bore holes or however you orient it. You talked about putting a matrix, a ten-by-ten matrix or a ten meter spacing matrix over 100 meters of bore holes.

One of the things I was wondering is if -- I mean, you still are looking at conditions in the vicinity of those bore holes. There's a possibility that things will go by you in fractures between and one of the things I would -- my thought was that if you can do that or do a bore hole testing, maybe double bore holes or some other idea, like going in with the bore holes, but if you can do that in various areas where you have variations and known characteristics, for example, in the vicinity of faults, in the vicinity of cooling fractures and areas where there are fewer fractures or whatever, if you can pick several of those areas, using it almost as an exploration tool, maybe a little more enhanced than just a spot sampling, on other words, isn't that another way of looking at that type of sampling that you're talking about and that type of testing?

And is it really key here to get this area of 100 meters by 100 meters or is it something where you could go in and look at a ten meter or 20 meter wide strip at various locations throughout the facility in different geologies or different structural characteristics?

MR. BODVARSSON: I think you could do it either way, but I think this is the same problem as the chloride 36 problem. What June has elected to do or what DOE has elected to do, whichever one has elected to do it, they decided to have feature-driven as well as systematic. So I think their approach is to both of those.

MR. WILLIAMS: I think one of the things that we're going to be looking for in that percolation test, and that's why we've got the guys looking at it this year to see the best place, the best location for doing this, is to look at the representativeness of the fracture systems and we want to see all possible cases, would be the best.

DR. CORDING: Sure. And I think you'll see things in the vicinity of Ghost Dance and associated fracture systems that may be different in other places and all. I think -- and you may want to do some combinations of things that are spread out, more feature, and others that you're trying to pick kind of a representative volume or area type approach.

Clarence Allen.

DR. ALLEN: Clarence Allen, Board. Dennis, in virtually every viewgraph you've shown, you've emphasized the amount we have to understand, the challenges represented by these findings and so forth, and, as Don says, that's all very exciting and I agree.

But you are committed within two years now to make a viability assessment. How confident are you that we, within two years, can make a viability assessment on which you can have -- which is really meaningful and in which you have great confidence?

MR. WILLIAMS: I use, as that measure, how far -- or as a measure, how far we came in the last nine months, in a year that we admittedly started out as a disaster. I think we've got a lot of momentum right now. I think that we've got a lot of indicators that tell us where we're going to find the answers.

I think in that two-year period of time, I think we can develop a tremendous amount of confidence in the UZ model and the pieces that go into it and some of these techniques, some of these corroborating techniques, some of these different ways of measuring flux.

I'm confident that we can get to a reasonable understanding in a two-year timeframe to be able to support that viability assessment.

DR. ALLEN: Well, the past nine months have created more questions than answers, exciting questions.

MR. WILLIAMS: I don't think it's created more questions --

DR. ALLEN: Are the next going to be different?

MR. WILLIAMS: -- than answers, because I think that we see several lines of evidence starting to converge on the same type of conclusion with regard to flux, and flux is a very difficult measurement to deal with.

DR. DOMENICO: I have a question.

DR. CORDING: Don Domenico.

DR. DOMENICO: I have a question about the coating study. As I recall, how geochemists do that is they -- so many pore volumes go through rock, if you want to put it that way, with something being deposited. Now, if you can relate that to a flux, you -- I always thought you either have to assign it a velocity of going through or you have to know when it started.

So I'm a little bit confused on how the coating information was translated into some sort of flux, because I do believe that's the way it's done. You see equilibrium and the thing grows and it's -- so many pore volumes are sent through to make it go, and that's very mysterious to me, that stuff.

Does anybody know about how that is done?

MR. BODVARSSON: I think Dennis wants to answer this one.

MR. WILLIAMS: I do not want to answer that one, Bo. Why do you think we hire you?

DR. MARSHALL: Brian Marshall, USGS. I'm actually the one responsible -- I shouldn't even admit this -- for that flux calculation and it's really a very simple-minded calculation. Basically, it's taking the total amount of calcite and assuming that you have a mass of water that had a certain calcium concentration and just distributing that over the whole repository block or over the whole ESF, if you want to look at it that way. It doesn't really matter.

DR. DOMENICO: There's a sizeable margin of error, I guess, in such a calculation.

DR. MARSHALL: Yes. It's provisional or preliminary at this point and we are working on revising it, including geochemical models of how the water evolves as it moves down through the repository horizon.

DR. DOMENICO: Thank you. And the other point -- you know, I'm going through the facts that you brought out. The temperature. I believe the model that you're -- I won't say rejected right now, but the one that you're not favoring is Ed Weeks' model. Is that true? Did Ed Weeks publish on that some years ago? The moisture movement and the energy transfer due to convection actually.

MR. BODVARSSON: I'm not sure if Ed Weeks published on it. Sass has it in his paper and then Papadopoulos and some others did some analytical work on it. I'm not sure Ed Weeks actually worked on that specific problem, the heat transfer due to gas circulation.

DR. DOMENICO: But he did something similar where it was a circulating model. I'm looking specifically at that one. We have a lot of energy moving through the mountain, again, due to the moisture movement as opposed to the one that you're leaning toward.

MR. BODVARSSON: He looked certainly at the breathing of the mountain, the gas going in and out and the mass balance in it, plus the silica redistribution due to the gas flow, plus also a modeling of the gas ages both in the Tiva and the Topopah with Don Thorenson.

I don't think they did an energy calculation, unless I'm missing, but the reason we are certainly looking at that other model, Pat, and we are not going to reject it because it has a critical importance, which one is the right one. But if the age data that at least I am familiar with suggests 100 year old gas in Tiva and something like 10,000 year old gas in Topopah, and that would not be consistent with the estimate of 50 meters per year flow of gas in the mountain to get at the evaporation condensation rates that are required.

But we are certainly looking at that and like all of the methods, they are all subject to uncertainties.

DR. DOMENICO: I have one last question. This is probably to Dennis. I'm looking at the identification of fast path based on the distribution of elevated levels of chlorine and you said that the orientation seems to be north-15 to north-east-30. You guys have done a lot of pavement mapping there. You spent a lot of money washing off rock to get that pavement.

I just wondered if you had done some work on that to get some idea of the direction of the permeability to see if it correlates with what you're seeing there. Has anybody done that work with Jane Long's model or Jane Long herself?

MR. WILLIAMS: I know we've done a lot of work on the pavements. We've done a lot of fracture data work. But the piece that comes up high on my screen when I hear the structural geologists talk about it is the stress -- the tensional stress orientation in that vicinity, oriented towards the northwest, such that you have northwest-southeast tension.

Using that as a model, then, your youngest and most open fractures may be oriented in a north-10 to 30-east direction.

DR. DOMENICO: I ask this because I had a young student once apply Jane's model to one of your fracture pavements, one of your actual pavements, and we found the permeability direction to coincide more or less with what you just said, in a north -- I forget if it was northwest -- it was northeast or northwest, but I believe it was. It's been some time, you know, but I believe it was in that general direction.

MR. BODVARSSON: Just to add to Dennis' comments. Larry Ann at the USGS, his work actually involves taking the fracture mapping that was done both at the surface and within the ESF, doing exactly what you're talking about, the fracture network model. In this case, he uses FRACMAN to try to identify the connectivities, as well as the major direction of permeabilities.

DR. DOMENICO: Yes.

MR. WILLIAMS: But just for the reasons of that same question that you asked, that is why we identified as a identifiable task this year in our plan the structural implications of chlorine 36, taking all that data, sorting it out and see if there is a relationship between these fast paths and these structural orientations.

DR. DOMENICO: That's a very good idea. That's a very good idea.

DR. LANGMUIR: I've been trying, in my head, here and I'd guess I'd like your insights, to bring what we learned from the unsaturated zone long before the ESF into play in the analysis which you're finding right now in the ESF. In particular, I'm thinking, and I don't remember the exact numbers now, we had chlorine 36 data from the unsat zone from bore holes on the surface, which, as I remember, were 40, 50, 80,000 year kinds of dates.

We also had carbon 14 data, which presumably was impacted by the breathing of CO2 gas, and these numbers down to the repository horizon were in the vicinity, as I remember, of 30, 40,000 year kind of numbers.

We also had some tritium bumps, which presumably are like the chlorine 36 bumps we're seeing in the ESF. But apart from the tritium, does the data that we had for chlorine 36 and carbon 14 from shallower holes fit into the model you now have for the mountain? Is it consistent with the infiltration rates you're coming up with and your general concept as you now have it from the ESF?

MR. WILLIAMS: Why don't you go ahead, Bo.

MR. BODVARSSON: Well, the answer, in my view, is yes and no, because there is -- when you look at the chemistry of the mountain, there is a lot of uncertainties in the chemistry and there's a lot of issues that need to be addressed in the chemistry. For example, if you try to derive the perched water from the Paintbrush water, it's very difficult to do that, you know, just straight geochemistry, like a dilution or anything of that sort.

If you look at the Tiva chlorides, you have like five to ten, and you look at the perched water chlorides, you have like five to ten that indicate fracture flow all the way down through the mountain. When you look at the pore waters in the matrix, you get 70 to 80 milligrams per liter. The same things occur in some of the other isotopes you talk about. You have three that indicate old ages and then you go into UZ14 and you see 500 years in the middle of the Calico Hills.

So my feeling is that a lot of the pieces are starting to fit together, but there is still some important work that needs to be done on the hydrochemistry, understanding the hydrochemistry and the rock-water interaction, and that may actually be very much a key to the puzzle in the mountain.

DR. CORDING: Vic Palciauskas, Board staff.

DR. PALCIAUSKAS: Yes. My question is concerning the use of the temperature profiles in your work in estimating the percolation flux. I'm very happy to see that, because it can be definitive, whether you have colder water moving down or hot gases moving up.

But I was really surprised by the data that at least you showed in this picture here. It seems to penetrate only about 100 meters from the surface down and I was wondering whether there exists much more data and this data can be used in a more definitive manner and will there be more data collected in the next year for these purposes.

MR. BODVARSSON: Let me explain. The viewgraph that you're looking at is misleading, which is my fault, because the picture that you see on the left and side there is from a study that Ed Kwicklis and Joe Rousseau did looking at an individual wash and not looking at the percolation in the repository horizon, and they looked at UZ4 and UZ5 and they saw different temperature gradients, shallow in Tiva, where they tried to infer where the infiltrating water is going, if it was going in the middle of the wash or in the side slopes.

That was the purpose of their study and I put that figure to give some credit to their work. But the fact is, on the right-hand side, you have 15 to 20 bore holes, go all the way to the water table, that we use the gradients in the Topopah, which, I would say, some of them, like the older instrumented bore holes at UZ7A, NRG6 and 7, as well as SD12, are all within a fraction, because he instruments them very carefully and they correlate extremely well to Sass' data from 1988.

So I'm sorry about it, it's misleading, the one that you looked at. But we have a lot of data on temperatures.

DR. LANGMUIR: Ed, one more.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: I'm thinking of a hot repository emplaced in your new mountain with a lot more water coming into it. Do you have to rethink the possible consequences of a high loaded repository with this much more infiltration and how does Tom Busheck feel about it when he starts boiling it and running the fluids around and condensing it? Are we changing the possible effects we would see in a repository system with this much infiltration?

MR. WILLIAMS: Maybe I'll answer that question. Perhaps we ought to ask Tom Busheck that next time around.

DR. LANGMUIR: Bo's in the same lab with Tom Busheck. Bo, what he have to say?

MR. BODVARSSON: I think Tom is extremely happy right now. It gives him --

DR. LANGMUIR: Does that mean he has work to do?

MR. BODVARSSON: Yes. He has a lot of work to do. I want to -- you asked a question before and I wanted to address it briefly, and that was about the natural analog studies, and I think Pat Domenico mentioned that one. Actually, I wanted to mention that on the books this year, DOE has a task to look at the geothermal analogues with the report to look at the effects of mineralization from the geothermal system, as well as the heat effects to try to infer what's going to happen to Yucca Mountain. I just wanted to make sure that was one there.

With respect to the fluxes, I think if you read Tom Busheck's paper with respect to his drift scale modeling, that the flux cannot be a lot higher than five millimeters or more before you get some problems with the humidity conditions, but maybe it can be as high as ten. I don't know. Something like that for the drift scale study.

DR. DOMENICO: With regard to that question, Abe said that, of course, you were the geothermal expert. I didn't realize -- I've got a lot of respect for it, but I didn't realize you were a theoretical petrologist and literalogist as well. That's just a joke.

MR. BODVARSSON: Okay.

DR. CORDING: I'd like to talk a little more about some of your plans and particularly on the ambient conditions, trying to get a handle on flux from things like the drill holes pattern that you talk about.

Bo, were you talking about dry drilling those holes, the ten meter spaced holes? Would that be dry drilling?

MR. BODVARSSON: Yes. We're talking about dry drilling. Like Dennis said, this year, we will plan this test much better than we have in the past.

DR. CORDING: Would some of this be done and drilled and installed this year? Is that something that you would do? Is that part of the program?

MR. WILLIAMS: It's not in the plan for '97. What we specifically have identified is some of the planning exercise to evaluate the test to see how we would lay the test out to get as much as we can a consensus on the types of data we can collect and where we will collect that date.

It's very likely that we would not do any actual drilling until the '98 timeframe.

DR. CORDING: I know there was discussion also of even local drilling along the drift, along the main tunnel, as well as perhaps in the alcoves. But local drilling with dry drilling perhaps as a combined collection of samples for the isotopic studies and also some more passive monitoring, is that -- is there a plan to do some of that sort of work in the next year along the tunnels, dry drilling to collect samples, for example, or to monitor some of the ambient conditions back in the holes?

MR. WILLIAMS: I think most of the drilling that we have planned for '97 has to do with the work that we're doing in like the Ghost Dance alcoves. As far as a systematic drilling of shore bore holes along the tunnel alignment, we don't have those types of things in the plan.

DR. CORDING: Presently, the chlorine 36 collection or other isotopic studies, that will be done -- was that going to be continued in areas that you've already tunneled through or as you're continuing to advance the tunnel? What sort of sampling program do you have for that and is there a benefit to taking those samples back in the holes, some distance back from the wall as opposed to at the wall?

MR. WILLIAMS: With regard to like the environmental isotopes of chlorine 36 sampling, our program this year is very similar to the same funding level as last year. What we want to do is, of course, take samples on out the south ramp and then we want to work on one of these working hypotheses, as I laid out, of seeing if it's associated with those northeast-oriented structures.

Also, where we've got opportunities, like in the thermal test cross-over drift that's oriented in that northeasterly direction, possibly sample along the face of fractures to see what kind of lateral distribution we may have on chlorine 36.

As far as drilling holes into the wall to get a chlorine 36 sample, we don't feel that that's got a whole -- it's not a real good way to go. The reason you might want to do that is to avoid contamination, but we've shown that contamination is not an issue with chlorine 36. That's what June's report has shown.

So we don't want to do that. We don't want to go through the extra what we call uncertainty of trying to drill on a fracture that has chlorine 36 on it.

DR. CORDING: It's your feeling you don't have it -- you're convinced that you don't have any contamination issues to deal with.

MR. WILLIAMS: I'm convinced we don't have a contamination problem. I think we've got that pretty -- not pretty well -- that well settled and I really want to get into taking some samples on fractures identified in this working hypothesis and moving along some of those fractures so we can get a lateral distribution of chlorine 36, if possible.

DR. CORDING: And one other area, and you had some discussion of that. I think, Bo, you described the drift, the humidity conditions and the sealing off drifts, and the interesting part is that the drift itself if a boundary value type problem. It can shed water perhaps and that you won't even see it coming through. So you're not necessarily measuring actual flux coming through in an undisturbed free field of rock, but certainly you've got there a model really of the drifts themselves and it may be very useful to understand that as you -- you can go in and seal drifts off and observe the moisture conditions and the flow that might occur in that under controlled humidity conditions.

I was -- one of -- those sorts of problems, very often, you benefit from being off -- having a long enough drift so that you're away from other influences. So I was wondering what the thoughts were at this point on being able to seal off some of the, for example, ends of alcoves or back into the Ghost Dance looking at different conditions at different locations, what some of the thoughts are regarding that and if there are any plans in the program in fiscal '97 to either develop a program for that or to actually seal off a few things, a few alcoves and do some of that sort of testing.

MR. BODVARSSON: I can talk about some of the ideas and then you can talk about the planning. Like you pointed out very correctly, there are two or three aspects of the percolation flux. One is actually the spatial distribution and the values of the flux and the other one is how much goes into the drift and the third one is how does it go from the repository to the water table.

The test you were talking about before with the bore hole addresses the spatial variability of it and now you're talking about the drifts, and there are several things that have been proposed to do and I'm sure that DOE and others are going to be evaluating this year, has to do with the niches, like, for example, drilling in each and then closing it off.

And what we have been thinking of, Joe Wang and myself and others, is then also to introduce fluids on the top of it and see, under enhanced flux, how it's going to flow by instrumenting the needs of the site, because that's going to be fairly cheap and won't cost that much.

And the last issue that we thought about also is that the modeling that we have done with the higher fluxes seems to suggest that perhaps the permeability in the matrix in the repository may be higher than we get from this flux. Abe may talk about that a little bit later, but all our modeling with the inverse modeling seems to suggest that perhaps the permeability, matrix permeability in the repository area may be higher than what we measure with this low flux, and that can have tremendous implications because right now, the average that we estimate is roughly three times ten-to-the-minus-18 meters squared or three micro-Darcy, and that is sufficient to carry some one millimeters per year of flux, something like that.

Now, if you look at conductors in parallel, you know that the higher values of permeability are much more important than the low values. You can have nine of those and they don't matter, but the one that really matters. And if you get -- like I've seen measurements from Laurie Flint range all the way from ten-to-the-minus-18 up to maybe ten-to-the-minus-60, I don't remember exactly the numbers, but it substantially higher volumes there.

If you can only increase the matrix permeability by a factor of three or five, most of this big flux in the repository horizon may bypass the drifts. So this is something that we want to at least propose to DOE to look at through some kind of a simple measurement program, because this could be very critical.

DR. CORDING: Introducing the water, of course, gives you a feel -- gives you a picture of what happens in terms of the geometry, the boundary value characteristics of that hole and the fracture characteristics around it, but I would think you can also gain something from just the passive monitoring as well.

MR. BODVARSSON: Absolutely. That will certainly be considered.

MR. WILLIAMS: Yes. And as far as what we've seen in proposals that have come in, of course, we've got the -- I think it's the Flint and Joe Wang proposal on small niches in the ESF to close off and do some of these things.

We don't right now have that in the '97 plan, although we are taking it into consideration. '97, we're basically concentrating on three openings, other than getting the TBM out of the mountain. So we've got four headings that are going at the same time to go in and do some of these smaller niches, which isn't a real big job, but it causes the complication of developing another heading.

And, of course, as we do the two Ghost Dance alcoves, that will give us some kind of a feel for what we're dealing with possibly and there may be a possibility we can isolate portions of these for these purposes. So we're considering it. We know what the proposals are. We know what the concept is and I will say we're thinking about it, but we're giving it strong consideration.

DR. CORDING: Even in the thermal alcove, you have the side drift there. I know you're utilizing it at present for a heater test, single heater test, but perhaps that's something that could be closed off at some point, too. I don't know what other uses you have for it.

But being able to do that might be a potential --

MR. WILLIAMS: Yes. There might be some real interesting applications there, because theoretically, we will have heated it up and driven the water off, close the thing off, and now watch the water come back.

DR. CORDING: Sure.

MR. WILLIAMS: That could be very valuable to us. And any time we can use an existing opening for these kinds of benefits, it's always a lot better than going out and cutting anew.

DR. CORDING: Just cut off the end of an existing opening, certainly that would be taking advantage of those things and I think would be -- beats having to go on and drive more tunnels, certainly.

DR. BROCOUM: I just wanted to add, from a budget perspective, something to this discussion here. We planned a budget of 300 million this year and I showed a $325 million. So we have some contingency as these things are worked out. We have some flexibility on the budget side to incorporate some things if it's decided these are necessary. So it's not a matter necessarily of trading these off to other tests. I just wanted to let you know that, because I didn't make that clear earlier.

DR. CORDING: Good. It seems that some of these things -- you'd like to be doing these things as soon as you can. I mean, you're starting to see some real interesting possibilities here and I think having visited the project with some of you and looking in the tunnels at these things, I think there's a lot of people thinking in the same direction on a lot of this. So I think some very useful ideas are coming forth here. We're looking forward to seeing them getting actually installed or placed in the project.

One other item that you've been talking about a little bit, Dennis, was on the -- I got the impression that you were asking for a drift in the Calico Hills a little bit earlier. Perhaps not the east-west, but that was sort of the tenor of what I was hearing from you. Do you have any comment on that?

MR. WILLIAMS: I never ask for additional drifts.

DR. CORDING: Okay. Thank you.

MR. WILLIAMS: What I was just trying to point out is that when we're talking about the Calico Hills, we don't have a whole lot of tools to utilize in dealing with an understanding down there. Possibly for the UZ transport, using surrogates, and we always get the discussions of whether or not it's representative, but sometimes a surrogate or another analog is a reasonable way to go.

DR. CORDING: Well, it seems that this is another important aspect that I'm glad you're looking at, that aspect of potential isolation or delaying of the flow through the system. It's obviously an important part of it.

Any other questions from Board or staff, audience, consultants?

[No response.]

DR. CORDING: We're pretty close to being on schedule, and we'd like to thank you very much for your presentations.

MR. WILLIAMS: Thank you, sir.

DR. CORDING: And look forward to hearing more as plans progress.

We're going to continue now with our presentations. Abe Van Luik is going to be talking to us about the significance of alternative conceptualizations of an unsaturated flow to the system performance.

Abe has been the DOE team leader for looking at the performance assessment, synthesis of the -- is that correct -- suitability and licensing's technical synthesis team? Is that correct, Dave?

DR. VAN LUIK: That's correct, until October 26th when we change all names. Basically, I'm the team leader in charge of performance assessment. My objective today is to provide you a snapshot of the first preliminary evaluations of the system performance and the implications of one of the conceptualizations, and that's actually the most recently completely by the project.

I'd ask Bo not to leave the room because he's the one who gave us this realization.

What I'm going to tell you about is the unsaturated zone flow model. Basically, I'll be talking a little bit about the flow model case that we evaluated and one of the reasons that we keep saying preliminary results, preliminary interpretations is because the unsaturated zone flow model case itself was preliminary. Bo has since done a few more. And, also, the TSPA calculation to total system performance analysis that we did itself was preliminary.

We basically made modifications to TSPA-1995, the total system performance assessment, that we published early in 1996, which we have presented to the Board before. We made up three cases to look at, to look kind of at the range of uncertainty and we'll be giving you some preliminary results and preliminary interpretations.

Again, some caveats. This is work in progress and only a preliminary example is available at this time. Even as we speak, a second example has been worked up, but there just wasn't time to get it into this presentation.

We created from TSPA-95 a reasonably conservative case, a reasonably optimistic case, and a reasonably pessimistic case. The reason I use the word reasonably is because we didn't use the 50th percentile case and then 99th and a .01. I think all of these three cases span reasonable ranges of assumptions.

We took the representative columns from the same representative columns that we used in TSPA-95 out of the 1996 iteration of the UZ flow model, which you have just heard a lot about, with spatially variable infiltration. The average percolation flux at depth for the repository was seven millimeters per year and we used Bo's dual permeability model to define fracture matrix flux and velocity distributions.

We have an upside down -- no, it's nothing you can do anything about. It went upside down into the color xerox to put the heading on it. Some things I'm just not good at.

But I think the picture that you get here is these are the six points which we use in TSPA-95 as representative of six regions that were definable as having different stratigraphies and other properties.

If you look at the average flux covered by those six points, it's right around that seven millimeters per year average. The overall model, if you take the whole area that was modeled, is about four-and-a-half millimeters average. And as Bo and Dennis both pointed out, where the repository is is under the highest part of the mountain, which is where the flux is also highest.

The sensitivity cases that we ran were based on the TSPA-1995 model. That model is all set up. We can punch it and run it anytime we want, and it's relatively easy to modify certain aspects of it. The waste package degradation, waste form degradation, solubilities and retardation are all as it was in TSPA-1995. All of those, of course, will need to be revisited to do a comprehensive reevaluation of the mountain given the new fluxes.

We assumed 83 metric tons of uranium per acre, which is a mass loading about equivalent to the thermal loading. We calculated drinking water doses, two liters a day, at five kilometers, 20 kilometers, and 30 kilometers down gradients, and the primary differences in these sensitivity cases from TSPA-95 is that we used velocities for the water from the most recent conceptual model from Bo and we did not consider this time cyclic climate change. There just wasn't time to factor that into it. Although we did assume a pluvial case, which assumed continuously wet climate after repository closure. So it's kind of an extreme case.

We defined a pessimistic case, where 100 percent of the packages saw dripping water. I will explain later why this is pessimistic and this is what Bo was referring to a minute ago that I would get into, the sensitivity of this.

We assumed, for the pessimistic case, drips on the waste form and also that advective flow directly contacts the entire waste form after the first pit breakthrough. This is extremely conservative and if you read TSPA-95, you'll see that this was the normative case for TSPA-95. We have since rethought this issue and thought that this was a rather extremely pessimistic outlook.

We also, in this case, have flying iodine. It's one of these mystical things you have to take on faith. But iodine, chlorine and carbon all migrate through the engineered barrier system as gaseous species and when they hit the hose truck, they dissolve back into the water and come down with the flux.

We assumed for the pessimistic case a very low matrix diffusion and no back-fill. For the conservative case, we looked at 36 percent of the packages seeing dripping water. We used the drip zone waste package release model, but here we shifted and said that it's not correct to say that as soon as a pit penetrates, you have 100 percent of the waste contacted advective water.

So we had radionuclides moving through corrosion out of the engineered barrier system before it could contact advective flow. This time we forced iodine to keep its wings off and to come out as an aqueous specie, which we think is more realistic. And we used a relatively low matrix diffusion from fractures to matrix and no back-fill.

Next we will define the optimistic case. The optimistic case has four percent of the packages seeing dripping water and the four percent was based on the particular realization we got from Bo for this particular sensitivity study. So you can see that the 36 and the 100 were variations on a theme.

We assumed, for the first time, that 50 percent galvanic protection would be in effect for the waste packages, meaning half of the outer barrier would have to be gone before the inner barrier is susceptible to corrosion.

We invoked our cladding degradation model to reduce the release rates and we used the same release model, the same assumptions for chlorine. We have more moderate matrix diffusion from fractures to the matrix and in this case we assumed a back-fill. The only purpose that the back-fill served is to keep heat at the waste package surface a little bit longer.

The conservative case, pluvial climate, we only ran one case for the pluvial climate. We assumed that 53 percent of the packages saw dripping water. We assumed that the matrix flow and pore velocities increased by a factor of three; same assumption as in TSPA-95. We also assumed, and this may be a tad controversial, but we thought with this much flux, especially since flux goes higher as you go up gradient in the water shed, that the saturated zone flux would also increase by some amount, and all of the other assumptions are the same as for the other conservative case.

Next, I wanted to talk a little bit about why we picked five kilometers, 20 kilometers and 30 kilometers for the calculations. The five kilometer is the old 40 CFR 191 accessible environment boundary. That boundary, as someone pointed out this morning, would make more sense if it was elongated in the direction of groundwater flow, but the definition of 40 CFR 191 is five kilometers from the farthest extent of the repository boundary, and that's right in here somewhere.

We also chose 20 kilometers because that's the approximate fence line if you go down gradient, the best that our models indicate. It's about the fence line and there is actually some human habitation right about here, where there's a crossroads. And then we chose the 30 kilometers because that's actually where the Amargosa Farm area is here and where there is active pumping of groundwater for domestic and agricultural purposes.

And this is where we need to go to both at the same time. In case you remember TSPA-95 extremely well, you will realize that it did not have an optimistic case, the conservative case, and a pessimistic case. But we used the exact setup of TSPA-95, imposed the assumptions that you saw in the previous viewgraphs, and recalculated it to show these kinds of results.

Over here on the other viewgraph machine you see the results using the UZ flow model that was described by Bo a minute ago, and there we defined the optimistic case, conservative, pessimistic, and also we calculated the pluvial case at 20 kilometers only because we made some assumption that at 20 kilometers, the water table would be very near the surface.

If we look at the difference, for the optimistic case, you see that there is no difference between the two. If you look at the conservative case, you can see that there is an order of magnitude enhancement of the millirems per year calculated using the UZ flow model of 1996 versus TSPA-95, and then the pessimistic case, also, an order of magnitude increase.

I believe one of the things I should point out here is these are drinking water doses. We used ICRP-30 to convert the water concentrations of radionuclides to a drinking water dose. We did not do the correct assessment to look at the safe drinking water compliance because for that you have to use ICRP-2, which gives very different results. We also did not do a total dose which we believe may be required by the EPA if their new standards goes through, as we have heard that it contains those kinds of provisions.

So these were done strictly to allow this sensitivity analysis to take place. These were not calculations that looked either at compliance with the Safe Drinking Water Act or compliance with the EPA standard as we think it's going to come out, and it's good to keep that in mind because these numbers should not be used for any kind of compliance comparison.

Now, we have the same thing for 100,000 years. And I apologize. In all my proofreading of numbers on charts, it never occurred to me to proofread titles. But the one on this side with, unfortunately, the lower numbers is the TSPA-1995 case and that is the case using the UZ flow model.

As you can see, for 100,000 years, for the same distances, for the same stylized calculations of two liters a day using ICRP-30 dose conversion factors, we have very low doses for the optimistic case. We have probably a four-fold increase for the conservative case and we have about a three-fold increase for the pessimistic case, and then the pluvial case is, again, featured in here.

What we're going to do now is go through the dose history plots for each of these cases so that you can see what the actual effects of these things are. Again, this is a two-viewgraph thing for each one.

On the right, you will see the pessimistic case, TSPA-95, and on the left you will see the pessimistic case using the 1996 UZ flow model. You can see that there is a whale of a difference in the distance from five to 20 kilometers and not much of a difference in the distance from 20 to 30 kilometers. This, of course, is all dependent on the saturated zone flow model that you use. We use the same exact one as in TSPA-1995 for these cases, except for the pluvial case, as you'll see in a minute, and it is yet to be determined whether or not that is the correct model for our saturated zone.

The first version of the official project saturated zone flow model was just delivered to the project office a month ago and, of course, we will be abstracting and putting that into our TSPA next time.

But if you can look at these, you can see that in the -- using the higher fluxes brings the doses in earlier and somewhat higher, just as in the chart previously shown. The peak on the left side of the 1996 model is the technetium and iodine peak and then if you can imagine, neptunium coming up from the bottom sort of in this direction, neptunium takes over for the 100,000 years later.

If we can go to the next and look at the conservative case. I asked Bob Andrews, who was in charge of doing these calculations, what would you say about these and he looked at them and he said earlier and higher. So if I seem to be repeating myself, it's because each of these shows the same thing.

For the conservative case, you can see that we have dropped down quite a bit. It's still a large difference from five to 20 kilometers. From 20 to 30 kilometers is a smaller difference. The primary radionuclides contributing are technetium and iodine in the 10,000 year timeframe and neptunium coming in in the 40 to 50,000 year timeframe.

Again, with the new flow model, they come in earlier because of more rapid flow through the unsaturated zone largely and they come in somewhat higher.

We can go now to the optimistic case, my favorite case. Here we have exactly the same phenomena again, except the doses here are much lower for much longer times, because basically the release rate from the engineered system is much slower.

Now, what this tells us is if we -- if we want to make a case for the system, we need to pay some attention to the processes that we invoked for this optimistic model, because the optimistic model takes advantage of galvanic protection, which the engineers say is a real process, while we need to demonstrate that the process is real for the TSPA VA and LA.

We invoked our corrosion model for cladding. We will also have to demonstrate that. Basically, I believe and it's my gut feeling that the TSPA VA is going to come out somewhere between this case and the conservative case, because I do believe that we have reason to believe that the conservative case is conservative.

If we can go to the pluvial case, yes, and it's all by itself. I think we're done with the two projector thing now. The pluvial and non-pluvial case, it is of no great surprise that in the pluvial case, the radionuclides come in a little bit earlier. It is somewhat of a surprise that they don't come in really any higher. I wouldn't say there's any significant difference between those two peaks. And then when we see the neptunium peak in the pluvial case, it comes in much earlier and I would say there's also no significant difference between those two peaks.

So it's a matter of timing for these cases when all things are held the same except for the fluxes.

If we can go to the wrap-up. What's the significance of all this work? The case that we ran had an increased percolation flux and an increased bulk average matrix permeability. That is important. It's not just an increase in flux.

The increased percolation flux decreases the mean unsaturated zone advective travel time. I think that's obvious because things are coming in earlier. The higher flux may increase the percent of packages likely to encounter seepage. However, high permeability may decrease the percent of packages likely to encounter seepage because high flux is likely to stay in the matrix if the permeability is there to handle it.

The higher flux, as the question indicated before, may decrease time of reduced humidities. Thermal hydrology effects were not properly reevaluated for these cases. Another reason to call them preliminary.

The higher permeability may increase the time to initial breakthrough of radionuclides, depending on the percent of flux in fractures. Finally, as I said before, this is work in progress. It's a snapshot in time. We're not done yet.

Let me illustrate this point in the middle right here. For this particular case, with a very high matrix permeability, a fracture was modeled and a 28 millimeter per year pulse was put in and after 10,000 days, it was going around the opening which had 100 percent relative humidity. It was not dripping into the opening. That's for 28 millimeters a year pulse.

For the same exact conditions, if you want to see dripping, you have to push it up to a 180 millimeter a year pulse. This is an interesting sensitivity study on matrix permeability and how matrix permeability can determine whether or not you have fracture flow. Obviously, this points to something that we need to know and Bo pointed out also that one of the things that we need to get a handle on is what is the bulk matrix permeability.

So with this very optimistic viewgraph, I will leave you and, of course, you're speechless and have no questions.

DR. CORDING: Don Langmuir has one.

DR. LANGMUIR: Surprise. Looking at your cumulative dose plots, I'm going to get in some arguments with this, I gather, at Livermore in a couple of weeks, but I'm looking forward to them. My old friend neptunium. As I understand from what you're telling me, and I think I've known this before the discussion today, neptunium is going to come in at 100,000 years or plus or minus a few tens of thousands as the dominant contributor to dose and then carry the plots further on.

I gather, and I can't -- maybe I shouldn't say this yet, but I'm understanding that some work being done at Los Alamos suggests that the neptunium is at maybe ten-fold less soluble than these models are assuming as neptunium 5. The stuff I'm coming up with suggests that maybe it's three or four orders of magnitude even less soluble than that.

If you go ten-fold less soluble, do you drop the plot by one order of magnitude? Is it that simple? If you to 1,000 less soluble, do you drop it by four orders of magnitude, or is it much more complicated than that?

DR. VAN LUIK: We're all waiting for you to publish your book so we can cite it.

DR. LANGMUIR: Six weeks.

DR. VAN LUIK: Six weeks. Okay. The Los Alamos transport model, in fact, is not what we used here. We used the same transport model as in TSPA-1995. If we invoke their model with lower solubility and with a better picture of the transport properties of neptunium, in fact, that peak drops down to the point where the technetium/iodine peaks, which come very early, become the dominant peaks. You are correct.

This was based on TSPA-95, the way it was set up. And for TSPA VA, of course, we're going to be dealing very closely with the Los Alamos folks to make sure that we properly incorporate all of the parameters the way that they have determined them, and they will be reading your book as soon as it's published, I'm sure.

DR. LANGMUIR: One related question. Does the solubility of uranium have any influence at all on any of this dose stuff? I'm gathering it's not important enough, although I've read some TSPA studies which suggest that within 50,000 years or so, there was a uranium factor contributing significantly to dose.

I can knock it down by three for you, if that will help.

DR. VAN LUIK: The solubility of uranium does have a determination on the release of radionuclides that are congruently soluble with the uranium matrix, that's true. Of course, you would have, in the down side of these graphs, you would have a general lowering of all of them if you brought the uranium solubility down.

However, it doesn't seem to affect too much the technetium and iodine, which we model conservatively, I believe, as being solubility limited rather than matrix dissolution limited.

DR. CORDING: Other questions?

DR. LANGMUIR: I guess I had one other unrelated question. Of course, when you go from conservative to non-conservative to pluvial, you've got plots all over the map.

DR. VAN LUIK: Yes.

DR. LANGMUIR: And depending on what the standards are that we discover coming out of EPA shortly, we'll then have those in some perspective. But it made me think. Are we in a position or will we ever be in a position really to say we don't need defense-in-depth? The DOE is talking about cutting back on certain kinds of studies which provide the engineered barrier system defense that some of us thought we should have, including back-fills and that sort of thing, suggesting that they have enough.

And I worry that when there's this much kind of noise and this large set of uncertainties carried through, are we ready yet to say we don't need to have defense-in-depth and keep considering all the other ways to engineer our system to minimize release?

DR. VAN LUIK: You're asking for a -- Dr. Brocoum will address this question.

DR. BROCOUM: I assume some of this will come up in the engineering talks. We just completed our system study, which I just signed out, I think, yesterday or the day before, an engineered barrier system study and unfortunately that study used the lower flux rates of .1 millimeters per year.

So the end result of that study was that we're not going to, at this time, preclude, for example, back-fill or other engineered enhancements from the design. I'm not sure where you got your information that we're cutting these things out, because it's something that I'm not aware of.

DR. LANGMUIR: I guess the funding and the active effort. Maybe I'm not familiar with what's going on at the moment on that.

MR. SNELL: Dick Snell from the M&O. I think the study that Steve was referring to, I think there actually is 1.25, I believe, as the basis for the study. So it's not quite as far off as some of these numbers as you might expect.

But with regard to your question on abandoning a defense-in-depth approach, from my standpoint, given the vagaries, if you will, of all the work that's going on, at this point in time, I would say no, we don't want to abandon a defense-in-depth approach. I think the study that Steve referred to helps us because it begins to identify the priorities of various options we have from a design or engineering standpoint and it begins to tell us now where we can invest the funds with the most benefit to us in terms of performance.

So it's a worthwhile study and I'd say we're going to -- for the time being, we're retaining a defense-in-depth approach. Just me talking.

DR. CORDING: A question, Abe. The pluvial case with three times the flux, you use three times the flux.

DR. VAN LUIK: Yes, sir.

DR. CORDING: The pluvial case.

DR. VAN LUIK: It's 21 millimeters flux continuous, yes.

DR. CORDING: That's the number I was looking for. So 21. So you're really up -- okay. The seven was what you assumed for the standard case, which is the present -- much higher than -- present value much higher than what had been used several months ago.

DR. VAN LUIK: Yes.

DR. CORDING: We're ready, I think, for a break.

DR. VAN LUIK: There's two more questions over here.

DR. CORDING: Jeff Wong.

DR. WONG: Abe, your focus is on drinking water exposure. Have you done any calculations to include other potential routes of exposure other than drinking water? That is, non-drinking water use, such as irrigation or showering, washing clothes, et cetera?

DR. VAN LUIK: Yes. In fact, we have just completed, for our own internal look-see, a complete pathway analysis. We hope to have a more definitive look-see at that in the near future. But the answer is yes.

DR. CORDING: Leon Reiter.

DR. REITER: Abe, a quick question. I guess the thing -- I'm still not quite sure which knobs to turn which ways in all these tests. But there's one thing. When you presented the TSPA-95, a couple of times we asked the question, you know, at what point in the percolation flux do you start getting into trouble, and you said one to two millimeters per year.

Now we're jacking it up very high and we're not getting into trouble. Part of that may be due -- you also had a plot which showed in TSPA-95 that at one millimeter a year, you had, I think, like 45 percent of the packages were wet and that was sort of artificially limiting it.

So I'm a little confused here as to what's causing what and what's influencing what here.

DR. VAN LUIK: Well, the beauty of these new results is that with the increased percolation flux, we have an increased matrix permeability. If we have the increased flux and leave the permeability the same as it was last time, then all of the people in the ESF should be wearing slickers right now, because it would be coming through the fractures and the matrix just can't cope with it and it should be totally saturated in that mountain.

So you can't have one without the other and everything that we said previously about what point we get in trouble was assuming that our matrix permeability was fixed by a decree from on high. And it turns out, from Bo's model, that it is not fixed and that, in fact, he is doing sensitivity studies now on what matches the observations in the mountain best in terms of a bulk matrix permeability.

And as Bo also pointed out, when you measure permeability on the small plugs, you are getting a very true value, but that small plug is in a context of something that has hairline fractures, larger fractures, and has heterogeneities from place to place.

So I think what we're doing is waking up to reality of the mountain that you can't get by looking at the microscopic pieces of it.

DR. CORDING: Abe, looking at that picture there and your comments you just made about wearing slickers in a tunnel which is dry, what sort of flux would it take to be

-- and I know it depends on the concentration of flow and those sorts of things between fractures or among fractures and matrix and all, but what flux would it take for us to be seeing dripping in those tunnels with the present ventilation system? That's a calculation I would assume we can make.

DR. VAN LUIK: I believe it's a calculation that has been done, in fact, and I'm frantically searching for Bo, because these are the types of questions that we in PA have been asking of the site program and if Bo can answer the question. Did you hear the question, Bo?

MR. BODVARSSON: Yes. About the flow in the drift, how much ventilation is taken away?

DR. CORDING: Yes. If there was going to be -- under the present conditions, we are not seeing dripping. What wold it take -- what flux would it take to cause dripping under the present ventilation?

MR. BODVARSSON: Are you sure you want me to answer? That's a tough question.

DR. VAN LUIK: That's why I want you to answer it.

DR. CORDING: It must depend on how it's concentrated.

MR. BODVARSSON: Let me answer it this way. There was a study this year that DOE supported that Joe Wang at Livermore and Alan Flint from the survey jointly worked on that did the moisture balance in the ESF. They concluded from that study that the ventilation removes around 200 millimeters per year of water out of the tunnel.

They also concluded from the study that the amount of water introduced into the tunnel is also about 200 millimeters per year. So that there is not a lot of contamination from the water going into the tunnel, nor is it a lot of drying from the rock.

Talking to Joe, he's still trying to sort out how much of this could be seen. He thinks that if it is less than some 20 millimeters per year, he probably wouldn't see it. That was his gut feeling. But the data is not good enough to say conclusively now.

DR. CORDING: If it were concentrated in a few joints, if most of the flow were concentrated in a few joints and locally, it would be extremely high -- equivalent of an extremely high --

MR. BODVARSSON: Right.

DR. CORDING: -- flux that has to be picked up by the ventilation system.

MR. BODVARSSON: Right.

DR. CORDING: If it's spread out, you could have a lower flux that wouldn't be seen.

MR. BODVARSSON: Well, you know what they see in the tunnel, whenever they turn off, the humidity goes off and the ventilation goes off and you see water coming in through some of the rock faults and some of the holes they have there. But we cannot really conclusively answer this now. This thing is continuing next year, I know.

DR. CORDING: Thank you. Let's take our break. Abe will be back and as we all -- we'll be back here at 20 after.

[Recess.]

DR. CORDING: We're going to continue with the second part of Abe's presentation. This part is the path to an integrated TSPA VA.

DR. VAN LUIK: This is a similar presentation and you'll be able to tell that because I'll only use one viewgraph machine.

The path to an integrated TSPA VA and what I want to talk about is the approach to the TSPA VA, and there are three components to this approach, from my perspective as a DOE person managing this.

I want to talk about the abstraction process, the role of the TSPA VA peer review, all of the plan and proposed expert elicitations.

One point that I want to make is that a lot of the sensitivity analyses that were done last year, we just, I think, approved a document with a title called abstractions. A lot of the current and past, meaning this year's, sensitivity analyses are really preparatory to the TSPA VA. So we have already started working towards creating that product.

Now, the objective of the planning effort is to ensure that it captures the process level modeling -- I believe Steve made this point a couple of times this morning -- that comes from the site engineering environmental functions also for the biosphere model, which was asked about a while ago.

We want to involve external experts in a couple of areas. One area is through focused expert elicitations. The other one is through a more comprehensive peer review process.

We recently completed a TSPA VA plan. That plan is under DOE review right now and probably will be approved or sent back for revisions next week. But it defines the overall approach, the roles of the different organizations and their responsibilities.

It discusses the method of abstraction and for each process model to be abstracted, it lists the process models to be abstracted and for each one of them, it gives the current status of abstraction. As you can tell, TSPA-95 already took preliminary versions of some of these models and created abstracted versions of them. So it discusses that status.

It also discusses the 1996 work, which was to do testing just to get a handle on what's important in this process. It reviews NRC staff's treatment in their iterative performance assessment, too, or in recent communications, such as some of the things that they told us at the technical exchange on TSPA-95. It discusses what we believe at this point are the relevant uncertainties, the sources of information, the expected output from the abstraction process, the key personnel are suggested and a schedule is given. This is all in the TSPA VA plan.

The reason we did this is so that when we have our first meeting on a particular model, we are not starting out cold looking at each other and saying, okay, how do we begin.

We've basically already laid something out. The people coming to the meeting will have looked at it and we'll be ready to criticize it, because all of this, of course, is from a PA perspective. So there are many things probably that are relevant that we're aware of, but it's a way to get the ball rolling.

The worst thing is a meeting where nobody knows what's going on.

We will form abstraction and testing teams that will include process model development and performance assessment staff. It's important to have both of them in the same place.

Again, the reason is to ensure proper testing of these models and, in PA, a proper use of these models and the appropriate bounding of uncertainties.

This morning, Professor Domenico said something about you have to have honest scientists who create a credible TSPA VA. I believe we have honest scientists. They keep bringing us the wrong results. And we also, within PA, looking at the way they work, you know, in every process model, like Bo's, there are hundreds of decisions that have to be made in putting a model together.

It is our concerted opinion, and it's only proper that almost every decision that is made that goes into that model is cautious and tends toward the conservative, never the optimistic side. And so this is an important point to keep in mind in all of this. But we want to appropriately bound the uncertainties, and that's one of the reasons that for certain specific models, we want to involve some outside experts to give us an opinion on whether we have appropriately bounded uncertainty or whether we're way too conservative or, in some cases, too optimistic.

That's the next bullet. We want to focus TSPA analyses on key attributes consistent with our previous experience, the waste containment and isolation strategy. I liked it better when it was just the waste isolation strategy, the WIS, and then the NRC's key technical issues that are appropriate to TSPA.

What's the schedule? The abstraction workshops, they start this month. We may be a couple of weeks behind, but they start very soon and they will end in the spring of next year. We will do sensitivity analyses concurrent with and after the abstraction workshops. We will document the abstractions late in '97, early '98. We will do the reference case analyses late '97, very early '98. The sensitivity cases, January to April of '98, and document all of the above and then the peer review will start calendar year '97 and run past the TSPA VA, and we will get into that later.

But you can see that PA people are going to be too busy to go to parties for the next little while. Don't invite them.

Why do we want to abstract? Well, TSPA results have to properly reflect results from the highly detailed and computational intensive site and engineered system process models. One of the criticisms that we deservedly have had for TSPA-91, 93, and, to some extent, 95, is that the work being done at Los Alamos, the work being done at LBL, and other process modeling, we looked at that, we interviewed the people doing it, and we built our models based on that. That is not how you build a defense-in-depth for a model.

But it is neither efficient nor reasonable, in our view, to incorporate all that complexity in each of these process models in a probablistic TSPA calculation. A lot of the runs that we do to create these dose histories, for example, they're either 100 or 1,000 runs. We'd have to start calculating now to do -- to incorporate all those models as they are.

So we use abstracted models as a surrogate for the more comprehensive process models, but the reason we are going through this formal abstraction process is because we have to maintain the essential elements of the process models, including the key interdependencies.

This is the challenge of the whole abstraction process and why it is so important to do it right, to document it and to have it reviewed.

The models that we are particularly interested in paying attention to in the abstraction process involve almost everything; waste form degradation and mobilization, waste package degradation, the near-field environments and all of the linked processes that go on in that environment, like the thermal hydrology, unsaturated zone flow, saturated zone flow and transport, and unsaturated zone transport, the biosphere model that leads us from these drinking water doses to a more proper total dose, and then also the disruptive events, low probability, potentially high results, volcanism, tectonism, and criticality.

The teams that we are going to set up will always have a TSPA core team, which is the particular analysts from TSPA that are involved, and, of course, management. I think Bob Andrews will be a part of every group.

The objective is to ensure the utility of the results for use in TSPA. This is a very utilitarian approach from PA's part; in fact, PA is insisting on this approach at the expense of work that could be done by the site and the engineered system people to improve their own models.

We want to integrate results from all abstraction testing activities. The abstraction core team has a performance assessment modeler, a representative of this core team, and then site and design representatives, and this is plural, depending on the model, we may have two or three representatives of that model.

And the core team is to plan and manage the abstraction and testing activity. The one thing we do not want to do is spawn a lot of sensitivity analyses that two teams are going to be doing simultaneously. We want to agree on what needs to be done and then assign that work out, so there is no duplication of effort and we will review each other's work to make sure that we agree that it was done properly.

The work scope, and the reason we're going through this in some detail is because it seems to be of great interest to you and I hope that everyone is staying awake. The preparation and planning is to -- I told you a minute ago about the current information that we've compiled. We want to expand and summarize that current information, including the perspective of the people doing the process level modeling. Up to now, this has all been done by PA people. Develop information in the current abstraction status of that process, select the workshop participants, and disseminate information from these activities to those participants, plan and schedule the workshop, and then synthesize comments and suggestions generated by the workshop.

Getting to the workshop. We will present to the workshop participants the current TSPA representation of the process, the current state of the process information. Then we will develop and prioritize a list of analyses to refine and enhance the TSPA model, and then the workshop will select analyses, schedule the activities to do those analyses, and define the resources required, and conduct the analyses.

It's important that the people doing all the developing, planning and selecting are the same people that do the analyses, I believe. Otherwise, we just have hierarchy upon hierarchy. These are workers.

What do we want from the different models? For the unsaturated zone hydrology -- and these are key outputs. There are more outputs in the actual document that describes this. We want percolation and seepage flux, a big discussion just a while ago, thermal hydrology, humidity and temperature over time, waste package degradation, we need to have some idea of containment time, time of failure, radionuclide mobilization, solubility, diffusive and advective flux, and this group here will be studying your book.

Unsaturated zone transport, advective velocity distribution, saturated zone hydrology, the dilution factor. As was pointed out, you have a lot of dilution from five kilometers to 20 kilometers, what's the basis for that. The bias is running the TSPA-95 saturated zone model perhaps out further than it has a real basis for, but we will fix that by incorporating properly the site program saturated zone hydrology model. Biosphere, give us the proper dose conversion factors, the proper boxes to fill in as far as what -- not everything in a generic biosphere model would be applicable to the Yucca Mountain site. For example, the fish pathway from lakes is not going to be applicable. And then look at the probabilities and the potential effects of disruptive features and events.

The flow diagram basically goes over what I just said, except for some people, it's easier to visualize this, but you can see that there is a logical place for all of this input. This -- you know, we have been jumping up and down saying this is not an easy process. But it's a very important process to make sure that the TSPA VA -- and this is a model for the TSPA LA -- that these two products reach right down into the basic work that was done by the project on the site and in the lab to make sure that the TSPA properly reflects that work, our understanding, and those results.

The next viewgraph is the workshop dates, and it says proposed. These are the ones that we are proposing in the TSPA VA plan to the participants in these particular workshops. They are not fixed by any means, but we would like to stick to somewhat a schedule that looks like this, and there is no sense for me to read this to you, but it's given you as a first look-see of what we're planning.

Moving right along to the second phase of ensuring that we have a TSPA VA that has some credibility. It will be reviewed in depth using a combination of expert elicitation to look at focused issues and a peer review to look at the general issues. The peer review, as was explained this morning, is to look at the TSPA VA process and then give us guidance for the development of the TSPA LA.

This morning the question was asked why do you have the TSPA peer review completed at this point when you're doing TSPA sensitivity studies later. The recommendations from the TSPA peer review panel, from looking at their experience with us in creating the TSPA VA, should include recommendations on where we need to do additional sensitivity analyses and to put additional resources to basically beef up the product. So that's why these lines are going straight from the TSPA peer review to the TSPA sensitivity analyses for the license application.

This is the way we have planned it so that this bridges from our experience in creating the VA to give us direction on how to do the LA in a more defensible manner. So this is actually planned this way on purpose.

We have four key phases for the peer review. This fiscal year, we will convene the panel and introduce the program in an orientation phase. We will introduce then, the following fiscal year, to -- they will have a lot of homework here. It's not like, you know, we're going to meet once and then we don't see them for a year. We will be meeting with them to introduce them basically to everything we've done so far and what the basis is for what we have done.

Then they will review the process models and the scenarios that are to be modeled for the TSPA VA. They will look at the results, they will look at probably a few of the abstraction workshops, and then look at the overall process and how we're folding that work into the PA models in the '98 timeframe, and then they will review the TSPA VA, they will give us a quick look in the middle of '98 on what their general impressions are, but their basic purpose is to prepare guidance for us to use to modify this process and beef it up in areas to produce in TSPA LA.

This is the schedule. The orientation phase starts very soon. Just this week, we are preparing to send out the first letters inviting people to nominate peer review panelists. The orientation phase, as I said, will take place during this fiscal year, to be followed by an abstraction phase the next fiscal year. The viability assessment comes in here. They are reviewing the product even before it is absolutely completed. So there is some time for them to make recommendations and, in fact, as they are watching this process, as they are looking at what has gone on in the past, unless I completely misjudge the type of people that we're going to use, they are bound to make recommendations and observations along the way that we can still implement in this process for the TSPA VA.

But the main purpose is to give us recommendations on how to do the TSPA LA, because after all, from our perspective, the TSPA VA is a dry run for the TSPA LA, which is the real thing.

What have we done so far? We have prepared letter requests to various professional organizations for nominations. We have defined technical specialties that are going to be needed and as soon as get replies from these letters, and I believe they will go out this week, some of them, we will select peer reviewers from the list of nominations considering their expertise, interests, and availability.

Then we will let contracts for the panel members and we will nominate and negotiate a chairperson, and according to our procedures, that chairperson, with us, develops a peer review plan that will implement all of the above.

I will describe very quickly the orientation phase. We will introduce into TSPA-91, particularly the way that it handled volcanism; TSPA-93 in the way that it handled the secondary effects of volcanism; TSPA-95 and the subsequent modeling activities, like the material that was just shown to you here, because we have done in 1996 a lot of subsequent modeling and a lot of sensitivity analyses and a lot of abstraction analyses.

They will review these modeling activities in detail and make preliminary observations on the modeling plans and the documentations approach and assumptions for TSPA VA. So they will be almost like a steering group in the very first phases to give us general observations on what they think we're doing right or wrong.

For the scenario and process model phase, we will introduce them to those models and we will have them -- we will help them review the current state of the process modeling. Then they will issue an interim letter report with their impressions on the TSPA VA and recommendations for the TSPA LA in the 6\98 timeframe.

For the abstraction phase, we will present them the updated process level models as they become available for TSPA VA. The panel will review these models and the abstraction process that converts these models into PA input, and that letter report that I mentioned a while ago will also include their impressions and recommendations for the LA.

The actual peer review phase, this is the classic peer review phase, they will, slightly ahead of the VA, look at the document as it is being pulled together and issue an interim letter report with their impressions at the same time that the report comes out. Then they will continue for quite some time and conclude with a final report with recommendations for the LA.

We will use that as guidance for the TSPA LA. And the reason we say guidance is that, you know, they may make 300 recommendations, of which there's only time, resources and realism enough to implement 200 or something. So we always run somewhat of a risk of having a peer review that either recommends too much or too little, but we will definitely use everything they give us as guidance.

Moving right along to the expert elicitation plan. The purpose is to quantify and document the uncertainties in the process model to strengthen the TSPA VA, and this is the issue that I was speaking of a while ago that we have great confidence in our process modelers, but we suspect that, at every turn, they are somewhat conservative.

We want to quantify the uncertainties that are introduced by the interpretation of the data all the way to the creation of the models. We want to focus only on those process models that are very significant to total system performance. So there's a limited number that we are proposing of these elicitations.

We want very small-scale focused elicitations approximately of six months duration each. The panels will have five to six experts and will include project experts and external experts.

We will follow the nine-step process outlined in the NRC's branch technical position on the use of expert elicitation. We think it's a fine document, a fine piece of work.

The approach is to complete the first elicitation and that will be on Bo's model, unsaturated zone process model, and then propose, actually in the middle of that, propose the additional elicitations, waste package degradation, waste form dissolution, drift scale thermal hydrology, unsaturated zone hydrology.

The unsaturated zone expert elicitation. We want to look at the spatial and temporal distribution of the percolation flux. We want to focus on infiltration, basically the work done by the USGS that was input to Bo's model. We want to look at methods to characterized unsaturated fractured rock. We want to look at the analysis and numerical modeling of fluid flow in variably saturated rock and then to quantify the data and modeling uncertainties.

This is an approximate schedule for this elicitation. We have already sent out letters asking for nominations to the panel. We hope to have our first workshops in November to discuss data needs, models and their interpretations, and then receive feedback. And the final report, of course, comes after the feedback, in the May timeframe. But we hope to be able to run with this feedback and start making changes in the model.

The status. We have developed, for the first one, the unsaturated zone expert elicitation, an implementation plan. It defines the panel selection criteria and the process to be followed. The letters went out and panel selection will begin as soon as we start receiving or as soon as we receive a critical number of returns to our letters.

This is a proposed expert judgment schedule. Unsaturated zone here, waste package degradation, thermal hydrology, waste form dissolution, saturated zone hydrology. Here is the viability assessment. As some of you who are astute may observe, there is not much time between the saturated zone hydrology panel and the viability assessment. These are still flexible and we may actually be able to either eliminate one or double up the schedule a little bit so that we have a little bit more time from the end of this assessment to the viability assessment.

But as I pointed out before, as soon as the panel is done and has verbally given us the recommendations, even though we give them three months to write up their final report, we basically have their input and we can start working with that input.

So I'm sure there are no questions, as it's very clear.

DR. CORDING: Thank you, Abe. Clarence Allen.

DR. ALLEN: I note that five of your workshop dates are scheduled for December. Do you think it's really possible to find outside peer reviewers who are going to be available that soon?

DR. VAN LUIK: The workshop dates that you're looking for were the abstraction workshop dates. Those are neither -- neither the peer review nor the expert elicitation will be part of that. This will be internal and we drive our people with whips. I mean, if we want to have five meetings in December, by God, they will be attended and held.

DR. ALLEN: Thank you.

DR. VAN LUIK: But as I said, this is a preliminary schedule, yet to be negotiated with the participants, and we really don't treat people that way.

DR. CORDING: Jared Cohon.

DR. COHON: I had a question that arose during Steve Brocoum's presentation which I thought you might have clarified, which is why I didn't ask it then, but I'll ask it now.

He showed something called a TSPA model hierarchy, which has at the top total system performance assessment model, performance assessment models, then the abstracted process. I don't understand the difference between the top two hierarchy levels, the performance assessment models below, total system performance assessment model. What's going on?

DR. VAN LUIK: What's going on there is that we have, for example, we use RIP as the overall total system performance assessment model. We could use TSA, as well, as we did in '91 and '93, but RIP, I believe, will be our model of choice for TSPA VA.

DR. COHON: And what does RIP stand for?

DR. VAN LUIK: That's the repository integration program, because otherwise it wouldn't be RIP, it would be RIM. But it's basically a model created by Golder Associates for DOE and it is an extremely complicated spreadsheet into which we abstract all of this information and put it in time phase and spatial phase and run the code basically the way it was demonstrated for TSPA-95.

Now, the inputs to that model come not only from process level models, but they also come from subsystem models, like YMIM to look at the -- I have to -- it's Yucca Mountain integrating model, which is a Livermore product, which can be used to look at nuances of the engineered system. We have ARREST-CT now available to us, which is a numerical version of the ARREST code, in which you actually look at geometric issues within the near-field environment and the engineered system. And those types of models really are not process level models at all. They are ones that integrate process level models to a next higher step for subsystem performance assessment. So that's what that second box meant. And it's a little confusing, I agree.

DR. COHON: Are all process models -- do they all wind up in one of those subsystem models or do some go directly, after abstraction, to the --

DR. VAN LUIK: Some go directly after abstraction into RIP.

DR. COHON: Okay.

DR. VAN LUIK: For example, in the TSPA calculations that I just showed you -- should I stop right there by just saying yes?

DR. COHON: Yes is good enough, because I have other questions. Could I?

DR. CORDING: Please, go ahead.

DR. COHON: You showed the table with the key outputs from the abstraction, from the various process models. For example, containment time. I assume that each of these is a function of some thing or some things. It's not just a number or even a set of numbers over time, but functions. Is that right?

DR. VAN LUIK: Yes. These are all functions and they will be input as functions, yes.

DR. COHON: All right. One of the things that I'm concerned about in the presentation, and I don't know if it's a real concern or because of the press of time you have to abstract from your process models, this thing goes in one direction. That is, you start with the process models and you wind up with TSPA and never did I see that you would ever go back and go back in two ways.

I mean, one is, okay, you now have this result from TSPA. A question that arises, should we believe this result in terms of specific processes, and the process models that you started with are better in answering that question than TSPA would be.

The other, kind of going back, though, is to take what you learn from the TSPA process and go back and do more work on the process models, which might be -- which might come out of the TSPA process itself. Can you comment on that?

DR. VAN LUIK: Yes. I'm glad you gave me the out you gave me right at the start. Of course, I left those out because of the press of time. But one of the functions of the whole abstraction process and the participatory thing is to delineate sensitivity studies of the process level model itself and its abstraction to make sure that their results are in sync and that the major processes have all been captured.

Then as we put that into the TSPA model, of course, we will again run that and run sensitivity cases to focus on the aspects that came from that process model and make sure that in two different levels, we have captured it appropriately. But that's what the whole abstraction give and take is all about, as you will see when you attend our December meetings.

DR. COHON: Yes, I'll be at every one. Actually, since you raised that, are these open to people other than the team members?

DR. VAN LUIK: As soon as I said that, I realized I stepped in something. These will be internal working meetings of the project and I would have to go to a reading, to Steve, I believe, to see. The abstraction process, in and of itself, is just a working process. Of course, the peer review and the expert elicitation are going to be open. We're going to ask people to nominate a person to follow that process, but we don't want a gallery at each one either.

DR. COHON: Sure.

DR. VAN LUIK: So it will be a -- but I never really considered whether the abstraction process meetings would be interesting enough for people to attend. I believe the orientations might be interesting for you to send a staff member to. I personally have no objection. I just don't know how we conduct business, because these are not -- these are working meetings. They're not show-and-tells

DR. COHON: I understand.

DR. BROCOUM: These are working meetings. It's not a public meeting. We're not going to notice the meeting. So they're working meetings. That's, I guess, the best way I can describe it right now. It's not a meeting like this meeting is here. It's really a meeting, an internal project meeting to get the work done.

DR. COHON: Let me go on. Just stop me when I run out of time, Ed. Abstraction core teams, are these going to be chaired by someone from the TSPA group? Is that the person who is sort of going to be pushing the train?

DR. VAN LUIK: My guess would be, in most cases, that would be the case, unless we have a volunteer from one of the other participants that wants to take a lead of it. Basically, as I said, this is being driven as a need from PA. So PA would want to be in charge.

DR. COHON: Could you tell us or provide to us the list of the professional organizations that you have requested nominations from?

DR. VAN LUIK: The list is about ten or 12 long and I was shown it in a flash to say is this okay. No. If I began rattling off some names, I would leave off a whole bunch and offend everybody in the room probably. But perhaps Jean can tell you.

DR. COHON: Another question about timing.

MS. YOUNKER: I can just say -- Jean Younker, the M&O. We can just get that list of -- I think it's in a formal letter, so we can just get a copy of the list for you, if you're interested in who we were requesting names from.

DR. COHON: Thank you. Back to this delicate timing issue you have with TSPA VA and VA itself. If your peer reviewers perform as you hope and they give you a lot of substantive things to follow up on and assuming that that's going to be part of the public documents that go along with the VA, how do you simultaneously claim credibility for TSPA VA which supports the VA determination and say, well, we've got these 300 or 150 things that we still have to do to make this thing really support decisions?

DR. VAN LUIK: It's precisely for that reason that we're asking them for two products in relation to the TSPA VA. One is their --

DR. COHON: The interim one.

DR. VAN LUIK: -- quick impressions, the interim letter report, and we run a risk if they debunk the product, there is hardly a way that we can recover from that. But that's a risk that we just run using this approach.

The later report comes out about nine months later and will give us, I hope, a list of things, concrete things that we can do to improve the product for the LA.

DR. COHON: One last question. In talking about the way you're going to use experts, you focused on the characterization of uncertainties in the process models. As we know, the abstraction process will introduce additional uncertainties and the TSPA model will introduce yet more uncertainties.

How do we deal -- what do you plan to do to deal with those additional uncertainties, to characterize them and quantify them?

DR. VAN LUIK: What we hope to get from the expert groups is the ranges of uncertainties for the key inputs to the TSPA VA. Once we have that range, we know how to mathematically propagate it through the analysis so that they will be properly convoluted in the outcome.

Without that, we would be basically one step back from having credibility. Part of the credibility argument, of course, is what are the uncertainties and if the groups -- and here, again, there's an element of risk here. If the groups feel that the uncertainties in the model currently do not capture the total band of uncertainty that they believe, in their expert judgment, is out there, then the expert judgment group will cause an expansion of the uncertainty in the total product.

But this is another reason that we appreciate having the TSPA VA as a dry run for the LA, because it does give us three years to fix a lot of holes that they see in our certainty or uncertainty.

DR. COHON: Thanks.

DR. CORDING: Board or staff, other comments?

[No response.]

DR. CORDING: I think, Abe, we're complete here, finished, at least at this point, and thank you very much.

The next topic and the last presentation today is on repository operations. It's basically an overview of the mine geologic disposal system operations. It's a presentation by Dick Snell, who is managing integration operations, and Jack Bailey, who is deputy manager in the same area.

We're going to be talking about the repository itself, the repository design, which includes the surface facilities where the waste is received and processed, the underground facility where the waste is disposed, and related elements such as waste package.

I believe, Dick, you're giving an introduction and summary on this. I'll turn it over to you.

MR. SNELL: Yes. Thank you. I'll just give a quick intro, I have the easy part this afternoon, and then Jack Bailey is going to take over and he will go into the first portion of it. I have one chart here to launch the thing.

What I wanted to do by way of introduction on this one chart is just point out that the material that's going to be covered by Jack in the next presentation, plus those that are going to follow tomorrow morning, are all of a piece. That is that Jack's initial item, the overview of the MGDS, including the design approach and the current status and the major technical issues, is just that. It covers the whole repository operation.

Included in that, he will talk about a fairly large number of technical issues. I think there are 13 that we've identified in Jack's presentation and those 13 issues are a distillation of a whole series of comments that we've received from the Board, from the NRC, and from other reviewers on the program, and they are those that we believe right now are important ones for us to address from an engineering and design standpoint. They're not necessarily the only ones, but they're important ones that we can see right now.

Then from that group, after Jack finishes that initial presentation, we've selected several which we think are representative and interesting at this point in time to give you a little bit more detail on. So the bullets you see there on retrievability, the waste package, remote handling, drift stability and thermal management are a subset, if you will, of that first presentation.

With that, I'll let Jack take over and go ahead with his material.

MR. BAILEY: Good afternoon and thank you. I'm going to provide an overview of the MGDS operations, and this is the basic format. I'm going to talk through the design phases, and I want to go back to Dr. Dreyfus' first slide of the morning. I really liked his slide because it showed the design stretching over the whole time period of the evaluation here where we look at a VA, a site recommendation or a license application, and that's exactly what the engineering department is trying to do is develop an engineered design throughout this timeframe, with focus first on VA, that for the portions that support the TSPA, that which goes on to the four criteria that Dr. Dreyfus talked about, and then finally to get us to the LA.

As such, I'm going to talk about the different phases. I'm going to give you some basics about the facility itself, size, layout and such, and the waste forms.

Then I'm going to ask you to indulge me and I'm going to try and walk you through the repository from the time fuel gets there until we emplace it, so you can see what a design looks like. Then I'm going to go back and show you what issues arise through those various phases and the 13 issues, as Dick alluded to. The issues that show up that we feel we need to resolve or at least come to some type of closure on so that for the viability assessment, we have a basis for a design that's analyzed, costed, and planned for in the viability assessment.

The repository design phases. Well, we've already been through the site characterization project conceptual design, 1987. You will notice the piece I wanted to point out was that that was shipment by truck and a vertical bore hole emplacement was the old design.

The advance conceptual design, which we put together in March of 1996, was a compilation of a good deal of design done since 1987, integrated during that first part of the fiscal year, and then was basically based on the use of the multipurpose container.

Our next effort is for the viability assessment design, which is in fiscal year '98. This concept will not rely on the multipurpose container. They individually handle the fuel elements, as you will see later. And we have to provide a design that provides a consistent basis to support the performance assessment, to be in lockstep, as Dr. Dreyfus said, demonstrate feasibility that the design is accomplishable.

We need to be able to estimate costs, as he said, and develop a licensing plan from that design. Our license application design, due out in fiscal year 02, is intended to have enough detail so that the NRC can make a determination with regard to the license application and be able to issue a construction authorization. And, of course, it has to reflect the latest scientific and performance assessment input. It has to be changed as we learn more about the mountain and as we learn more about the engineered facility.

And then finally I put a slide on for ongoing design, which, should we receive the construction authorization, is where we get down to the actual details of design, details of how you implement that design for the constructor to put into the mountain.

We have what we call the one-pass approach, which, as I said, harkens back to Dr. Dreyfus' slide. That says that we're going to start a design on the board. We're going to focus on the VA, but that design is going to be controlled and as we find we have to make changes through findings from the PA, through the scientific findings, through the model testing, we will make changes to that and we will continually update that design. There is not going to be another ACD, there is not going to be a design package in which we stop and start over. There will be a design which continues throughout, but we'll be able to status the completion of that design at any time.

The advance conceptual design is our point design. You'll see some of that as we go through this today. The reference design for VA, when you look at our scheduling, if you do, phase one is where we come through and try and find all of those inputs that are necessary for performance assessment. That happens basically at the end of fiscal year '97 and by that point, the TSPA should be satisfied with the engineering input.

We will provide updates throughout '98, but basically by the end of '97, we have to provide that information. And that's why phase two overlaps into TSPA design. That phase two will also provide some of the additional work that's necessary for the costing and the planning. For the LA, we finish the design during phase two and then we do some additional work during phase three to make sure that we fleshed it out and have enough detail for the license application.

And what are the repository physical characteristics? Well, we're looking at the disposal of the regulatory required 70,000 MTU and we look at around 11,000 five-and-a-half to six-foot diameter containers. We'll place that in 120 miles of 15 to 20-foot diameter tunnels and drifts, utilizing about 840 acres underground, anywhere from 200 to 400 meters below the surface, based on the topology.

The surface facilities, our current design says about 29 buildings, about 800,000 square feet of floor space in order to handle it, and our staffing is around 600 for the surface and sub-surface operations. Remember that we have to receive and, for number of years, we continue to excavate the underground. Then you can see the 300 for the underground drift excavation. These are numbers from the ACD which we'll be working on.

I will point out at this point that all of this is subject to change as we evolve the design.

This is the representative waste form data. It works in three blocks, as you can see the sideways brackets. What we receive is we receive waste in rail or we receive or we receive waste in trucks. It can come in in a spent nuclear fuel canister, one which had to be opened perhaps, or we may receive it from the cask or the truck which we could life directly the PWR, the pressurized water reactor or the boiling water reactor assemblies, and you can see that we have DOE spent nuclear fuel and we have Defense high level waste canisters that could be received. That's the basic fuel that we would get from the rail casks, the truck casks.

You'll notice in the peak units per year, down in the green line, you're looking at in excess of 10,000 units a year that may have to be handled. A great deal. This is the effect of going from the multipurpose canister design, where the fuel was all going to be encapsulated at the utilities, shipped to us and then all we had to do was place it into an overpack and emplace it. It all is still a lot of work, but now we're looking at handling all these individual items.

And what do we emplace? We have three basic canisters. We have the spent nuclear fuel, we have the Defense high level waste, and then we have a canister that combines the Defense high level waste and the DOE spent nuclear fuel.

As a basic, this is what the facility looks like. The north portal is associated with the receipt, canisterizing and emplacing of the fuel. The south portal is associated with the continuing excavation of the facility, since it's anticipated that we'll begin to emplace, as shown by the brown lines, while we continue to excavate it, as shown by the green lines.

You will see I placed the ventilation on there fairly prominently. I talk about that a couple of times, since there is 120 miles of emplacement drift and tunnels and it is underground ventilation is, of course, a key concern for certainly human performance.

I want to put this in for the current versus the ACD repository. The piece of interest -- what we have managed to accomplish is that we can place at least 70,000 metric tons in the upper block alone as opposed to the ACD design, which said we needed to use the upper and the lower block.

How did we do that? It's hard to see in the detail, but you see a phantom dim line across here. We had what we called a TBM launch drift, where we're using a mechanism whereby we had a tube and had to place the boring machines into this tube, which was a great waste of space which we couldn't use for emplacement.

As was suggested this morning by Professor Cording, we have been using some expert or some consulting panels with underground experience. They suggested that that was not really a necessary method, that the new generation of boring machines could, in fact, do without that, and we were able to recover along here around 40 meters per drift at each end and about 40 acres total.

In addition, we extend it slightly to the north and you can also see we managed to cut some corners based on the characterization. As such, we're able to put 70,000 metric tons at the 83 metric ton uranium loading into the upper block, with about a 15 percent margin for setoffs and such.

This is the surface facility as shown in the ACD. Clearly it's going to change. You can see that we have a radiological controlled area, where we handle the fuel. We have the support areas associated with those things that you have to do to run a large facility.

Up here I say it's going to change. All it is is a block right now, but the waste handling building will likely change since we now handle so many individual elements of fuel rather than the canisters. The waste treatment building may, in fact, change, since now we're handling bare fuel as opposed to canisters. We're likely to have a different quantity and mix of waste.

We have the cask maintenance facility here, which is shown as a very large item. That was because, at this time, this was the cask maintenance facility for the program. With the RSAs, for the transportation initiative, this would be a very small facility associated with just being able to put the cask back on line. So there's a good deal of design that goes into the surface facilities to match the new program plan, but it gives you the idea that there clearly are some specific functions that have to be handled throughout the facility.

What I'm going to talk through here is I'm going to take you on a walk through the repository, as we see it. It's, for the most part, how to take a walk. The first one is probably the only one that's not a walk, but it is to a certain extent.

What I have is two slides, which I will show over here if I can find my button. I'll show two slides over here that show the major activities or operations that have to occur in the repository and over on this screen I will try and show some of the specifics that go on inside each one of those areas. If you'll indulge me, I'll walk you through the facility.

First, over here, we have a nice little TBM which talks about the construction, the development and the disposal container fabrication. Once receiving a construction authorization, you can see you have to build your surface facilities. That's where you receive your fuel. So you have to get that piece done. You also have to start into your sub-surface development in order to lay out the arrangements that I've shown you previously.

You will notice that the sub-surface development goes on much longer than with the surface construction. We would be able to receive the fuel and when we had an appropriate amount of sub-surface development, begin emplacement and then move over to the -- we'd be able to continue to develop while we emplaced.

Down here, the disposal container fabrication would likely start sometime during the surface construction, so that we had a backlog of disposal containers available to place the fuel in once it was, in fact, received. Then once it was received, we would be able to emplace it, as you can see. We would have finished the excavation in advance of the emplacement and will hopefully finish buying disposal canisters before we finish emplacement.

This slide was intended to show that a scenario, a means by which we could go through it, depending upon how the program goes for the future years.

The next effort is in waste receipt. The waste has to come in. It has to be inspected to ensure that it's in adequate condition to be worked on. You'll notice I show some staging here. It's likely that we'll have to have somewhat of a backlog at the front end of the repository; no large one, of course, but we'll bring it in. It will come in on at least a couple of trains or at least a couple of canisters per train and we will then take that into removal preparation.

It's set up for transportation, with the controls for transportation events. Once we've received it on-site, then we can take it to the waste packaging.

The waste package says we have to unload it. It's still canisterized. We look at anywhere from our present plans of a canister of some 23 BWR type assemblies, PWR assemblies, fresh water assemblies in a canister to in excess of 40 assemblies of a boiling water reactor. So this is a large undertaking and that's a large mass to deal with.

We would look at unloading that. We would have to take it in. The casks are going to be filled with an inert gas and we have to prepare and get the cask ready for actual opening, be it -- here we show canister removal and opening, which, in fact, may be a weld or may be a bolted condition.

Then we go to actual individual assembly transfers and we take that from the disposal canister or from the transport canister into the disposal canister. Then we go on and we make a disposal canister weld and we then go to disposal container transfer so we can send it down the tunnel.

We now go to the waste emplacement and you can see we show a train, which is how we believe we wold take the canister out of the waste handling building, and we would take it down below. It has to be emplaced. We have to monitor it. Ventilation goes on throughout. At the end, we would seal and back-fill up the main drifts and, of course, if necessary, we'd go to retrieval before we went to the sealing and the back-fill. That would be the next step and I have some slides here of the specifics.

As you can see from over here, we go from haulage. We emplace. We have to caretake. We have to make sure and watch and do performance confirmation and ensure the repository is performing as expected. Retrieval may occur and then we go to the closure and decommissioning.

As you can see up here, I have a sketch of the transporter unloading the waste package. The waste comes down the transporter. It's pushed out on a cart, so that it can be picked up. Here we have a nice drawing. Again, you can see this cart in more detail as it's pushed out and is, in fact, attached to the transporter and the rail car here.

You can see what we've changed to from the ACD. If you looked at this at the ACD timeframe, each of these waste packages was placed on a rail car, which was pushed by a locomotive in and then abandoned in place, if you will, at the precise spot that it needed to be placed.

What we're looking at doing now is to take this canister in and you can see we have a gantry arrangement over here that's on rails and this gantry arrangement comes over and picks up the waste package and lifts it up, carries it down into the emplacement drift, places it on pedestals low in the emplacement drift, and then leaves it and then the gantry comes back for the next waste package.

So we've helped ourselves in several ways here. Most notably, in terms of retrievability, we've left no moving parts inside of the emplacement drifts at this point. The canister merely sits on some V-shaped wedges low in the emplacement drift and the gantry which carries it in is, in fact, maintainable, because we can bring it back out into a low radiation zone and reuse it and take it to a different drift if need be and do the maintenance associated with making it reliable and controlling it external to the hostile environment, the emplacement drift.

I have a slide here on performance confirmation as one suggested method of putting in an observation drift and taking a look at the different emplacement drifts to see how they are performing with a variety of instrumentation.

This is the type of thing that will go on during both the emplacement stage and the caretaker stage. This is not necessarily going to occur, but is meant pictorially to show that we have to deal with the performance confirmation of demonstrating that the packages are behaving as expected inside the drifts.

And I have a back-filling piece over here which shows that in the main drifts, not the emplacement drifts, but in the main drifts, we look at closing them up with the back-fill material, putting appropriate seals in in accordance with the regulations, and then continuing to seal. And, again, we can use the rails that are in place in the main drifts to perform that function at the time of closure and decommissioning.

I wanted to talk about ventilation for a minute, and this may be out of -- I'm not sure you have this slide in yours. This is mid-emplacement development. You'll notice that I have a piece on here that shows the emplaced area of the repository and another that shows the under-development area of the repository.

Our means of accomplishing ventilation and, of course, regulations require that we separate our systems, our development side from our emplacement side, our approach to this is that we exhaust from the emplacement side. And you'll notice that the air travels down from the north portal down the ramp into the area into the area. There is basically leakage, if you will, through the doors and we haven't determined how much, if it's controlled or if it's just leakage, but it comes through the drifts from either end and our intent is to place a ventilation drift below the repository and use raised bore holes to reach into each of the drifts so that we can have a ventilation path down each drift and out through the ventilation shaft. So we take a suction, exhaust from this side.

On the developmental side, we look at using fans up here by the portal, and, of course, we don't show the seal right here because the least path of resistance would be out. But basically we force air in through the south ramp. We distribute it through the developmental side. We use ducting and such to take it into the various areas that are under development by excavation, and then we exhaust through the development exhaust shaft.

And by doing that, we maintain a higher pressure on this side of the air locks, which we have to install between the development and the emplacement side, then on the emplacement side. You'll also notice that we show a couple of TBMs and they're going in different directions. So we've also, in the ACD design, we looked at only one direction and then pick up the TBM, carry it around and go through again. Here our intent is to drive through, partially disassemble it, bring it back and drive again, and we can do it from both directions. So I believe we've gained some efficiencies in the actual development of the repository.

And here you are in the caretaker phase and in this phase, you can see we're exhausting again at the emplacement exhaust. We bring the air down. You have leakage past doors at this point in time and it goes in and goes out in this direction, for the long-term ventilation of the facility.

That was the basic walk through the repository, what you would see if you went and took a look. Not a lot of detail in the design and we'll talk a little bit more about that tomorrow when we come to the individual issues, but it should give you an idea of what functionally has to occur inside the repository in order to get the waste down there.

Why is that important? Well, as you've heard all morning, the scientific and the performance assessment folks are looking at what the various characteristics of the mountain are, what the characteristics are of what we place down there. It becomes our job as engineering to make sure that what actually gets emplaced is in conformance with what all the analysis says has to be there.

So in order to do that, we went back and said, well, here's all the functions that we have to accomplish. What are the issues that are going to drive us for the VA? Remember that we have four things we're looking for in VA. One is a design, two is tied to the PA, three is we need to be able to make a reasonable cost estimate, and four is we have to be able to get to a license plan that says can we really do this over the next four to give years.

So we went through the various operations that had to occur and we went through and picked up what we believed to be the 13 issues that we need to work on. In actual fact, we came up with 90 or 90 to 100 different areas where there was a high level of work or interest that needed to be done, but when we sat down, these were the 13 that kind of popped up, we need to make some kind of a decision and move on.

This does not mean that these are final decisions. It doesn't mean that they're irrevocable. It just means that we're going to make a choice in order to move ahead for the viability assessment.

The first one is sub-surface mapping. Our question here is the extent and nature of the geological mapping of the emplacement drift wall surfaces, how much do we have to do, how much mapping do we have to do. It's an impact to us because of the selection of the ground control system. We're leaning towards a lined emplacement drift, what you saw in those previous pictures. Lined drifts solve a lot of problems for us and the ground support is going to be a specific talk tomorrow. But a lined drift solves a lot of problems for us.

Unfortunately, the current technology is one where you never see the wall of the tunnel before you line it. So we'd have to be looking at a little bit different technology or some changes based on how much mapping we have to do. So we're going to be working with the various people necessary to decide and set a requirement on how much mapping we believe we have to do in order to move ahead.

We have an issue on waste handling. We're looking at a production scale dry package -- dry packaging of spent fuel assemblies, around 11,000 annually. This is not a simple hot cell. This is a production hot cell. And we've done virtually no work on it. We've been working with the MPC. So we feel that we need to do some work to understand what we have to do to make sure that we can get there in licensing space and technologically, as well.

We, in fact, are going to spend some time and look at it for wet or dry; is wet really an option, should we be looking at bringing in and queuing up some assemblies so that we can do the thermal and the fissile material blending prior to loading or is that going to be an impact placed on the transporters.

It has an impact on our licensing, the cost, the waste generation, and, of course, NEPA as to what we do with it. In our study, we're going to do a study, we're going to look at the VA design, we're going to choose one early this year and go wet or dry and move out with that. And when we get to the LA design, we obviously will do more work on it.

Disposition of site waste. It says location. It's a question, in fact, of how much is there. The previous baseline, as I said before, was with the multipurpose canister. Now that we're handling individual assemblies, we're going to have more waste. We need to quantify what we think that waste is going to be and how to deal with it.

Our intent here is there is some cost data, NEPA may or may not be impacted, but we want to make sure we have that information under control. Our process is we're going to do a study at a recommended disposal strategy and it is our intent to reflect those studies in the VA.

Remote operations. It is interesting in that we're going to have a large application of remote handling. We're going to be handling very large waste, 60, 80 tons. It is radioactive and it is thermally hot, and we're going to do a great deal of it remotely and we feel that we need to make sure that we have established the applicable technologies, the methods, make sure that it's a licensable approach for handling all of these items in the remote operations area.

Performance confirmation also enters into it in that it is interesting that we want to get some information out of the various drifts, perhaps remotely and perhaps the remote means is the way to gather that. Again, the gantry is of interest to us. We could actually send a gantry in to a variety of drifts to gather information for us and bring it out. But the remote control of this and the ability to deal with the upset conditions associated with that, breaking down with the package in place, being derailed with the package in place, we need to be able to deal with that and work on those issues. And we'll be working on a preliminary design and this will get a little bit more discussion tomorrow, as well.

The issues you saw were generally surface-based. They were generally cost or schedule oriented. They're of interest to us. They are not our primary focus. They are things that we think we have to resolve and we have to know more about. These are more of the key design issues associated with the performance assessment and you can see they, as you would expect, tend to cluster around the emplacement of the waste in taking care of it. I have criticality control up. Abe Van Luik mentioned it as one of his disruptive events that has to be analyzed. We have that as an issue.

We also have the current disposal criticality regulation as a deterministically worded rule. It says criticalities are not permitted during isolation operations unless it leads to an unlikely independent concurrent sequential changes of conditions. Essentially, nuclear criticality safety. When you get into very long timeframes, it's hard to separate events and, as such, we believe that the reasonable approach for post-closure disposal criticality control is probablistic and it has a big impact on the waste package design and the loading of that waste package. You can't put as much fissile material in each package, which would cause more packages, greater area, greater cost.

Obviously, there is some work associated with criticality control, as well. That has to do with determining whether the likely probablistic type configurations that you are going to see, what are the effects of a criticality should one actually occur, and how is that handled via the performance assessment. Those activities are also being handled and, in fact, we are proceeding with the development of the risk-based approach that I just described and have a couple technical reports issued on that.

We have provided suggested word changes to try to deal with the language of the regulatory issue and with both of these, we have ongoing discussions with the NRC in regard to our technical reports. We have taken the approach of putting our technical reports in the hands of the NRC and looking for comments and having discussions with them. Our methodology has satisfied this. Clearly a PA issue in terms of disruptive event.

The engineered barrier system performance, this was alluded to this morning a little bit. You'll notice we called out the back-fill, the drip shield and invert material additives to enhance post-closure. The impacts -- well, the invert design might have to be different if we decide to place some type of material additive to it and the method of placing back-fill material to meet the performance requirements.

You will notice that the gantry approach lends itself to the back-fill requirement. It would be easy enough or hard enough that at least the capability is there to use the gantry as a means of getting back down the drift and applying back-fill, if, in fact, we need it. So the design as its evolving is being helpful to us.

The resolution process, our study, the EBS, as Steve Brocoum pointed out this morning, done at a lower percolation rate, said, well, do you really need it or not. Basically, you don't need it, but keep the option open for the invert additives. In actuality, the only way to keep the option open for back-fill is to design for it. So we're including the back-fill into the design and if we find that we need it, then we'll be readily available to do it.

The same goes for the additives and, in fact, we're looking at some drip shields and some ceramic applications should the drip question raise its head.

We're not placing a great deal of emphasis on that because we have an idea of its performance, but we'll keep the technology alive so that if the total system performance assessment says we need it, we'll be able to get back to it and incorporate it into the design.

Thermal management, hidden over here. Look at the effect of the thermal loading and the thermal management techniques on the overall performance, what thermal load do we want to put underground. The other piece that's of interest is how do we manage the thermal load that actually gets placed into the waste package and put underground, and there's a lot of techniques that actually will allow us to do that. Dick Snell is going to talk about that a bit tomorrow, so I won't get into a lot of detail.

But this impacts the size, the shape, the layout of the repository. It affects the ground control system with regard to structural aspects. Performance confirmation, design instrumentation and control. Again, it creates a hostile environment and how hot you make the packages is important.

The resolution process, we're going to look in the 80 to 100 MTU range as our aerial loading and we're going to work on the selected issues and work closely with TSPA to try and stay in touch with the question this morning of does the thermal load help us or hurt us with regard to the flux, what's the tradeoff there, and work through that. But we'll choose a thermal load and work through it this year for our reference design for the VA.

That burn-up credit, and it shows up over here, it's tied to criticality control. Criticality control, as I said, has to do with the likelihood of the criticality. The burn-up credit, on the other hand, is the process of accounting for the reduced physical content of the fuel. So that we can load more fuel into a package. The NRC hasn't approved that, per se, as yet, away from reactor applications. Again, we'd be limited to just a few assemblies and we're working through that approach with the

NRC in conjunction with the criticality. We separated them because part of it is the burn-up credit, which is one issue with the NRC, and the second part is how to deal probablistically with the results, which is the second issue of criticality.

Ground support, that which holds the drift wall up, has to be compatible with the engineered barrier system performance. Up to this point in time, we've been staying away from sedimentatious materials. We've put together a task team that is dealing with sedimentatious materials to make sure that we're able to do that. Clearly the ground control system, the layout, the ability to do retrievability, the ability to use the gantry crane, the very long timeframe associated with caretaker activities, all those things are enhanced by having a robust ground support system. And from an engineering point of view, that's what we'd like to do. Engineering is about tradeoffs and that's a tradeoff that we'd like to try and make with the TSPA, and we're working through that issue in order to do that.

Performance confirmation, which I've alluded to a couple of times -- and, in fact, ground support will be discussed tomorrow in some more detail.

The performance confirmation, we need to look at what we have to do in order to demonstrate that all these models that you're seeing are, in fact, behaving the way that we expect them to inside the repository area. It's important that we get the right kind of measurements. We need to know what the measurements are. We have to design them into the repository. And in some cases, we may have to develop some technology because of the long timeframe and the harsh conditions that are involved.

So we're working very hard trying to come up with some performance confirmation concepts, what the appropriate parameters are, and what instrumentation we can use and how to design it in without impacting the performance of the repository, and we're going to continue developing those through '97.

The issue of retrievability. The strategy for retrieval hasn't been fully developed. We don't have the credible off-normal retrieval scenarios clearly defined that we need to be able to deal with, is it a leaking package, is it a package that has rock falling on it, what are the bases for retrieval, is it economic recovery, what are the bases that we have there for why we want to do retrievability and how easy do we want to make it. If we want to make it real easy, then lined drifts are highly desirable. If we're wanting to mine it out, then it's much different.

So we need to establish a method and a set of requirements and criteria for the engineers to design to. And we have a study set up for this year, so that in about May of this year, we should have an answer as to what we believe the policy should be with regard to retrieval.

Seals are of interest, as you would expect. They are in Part 60. The material for the seals will have to be developed for the long-term performance anticipated or expected from the TSPA. We need to determine locations and types, how we're going to do it and how it interfaces with the back-fill. Again, we have to make sure that we have enough information and we believe that the past work done probably has enough information and we'll be working at this. This will probably be a '98 effort rather than a '97 effort. We don't believe there's a great deal of new work, but we need to compile what's already been done and make sure TSPA is using it.

Finally, I put up post-closure performance as an issue. This one I did a little bit differently because what it's about, in my mind, is integration. There needs to be an established standard. We've assumed one at this point. We have to have a defined performance allocation. We have to take the science, we have to take the performance assessment, and we have to decide how much the engineered systems have to do, what are their criteria. With that, we'll look at the needs to change the design.

There was a question earlier by Mr. Cohon, who suggested there were two ways that you ought to go back and look at the PA. I would submit there's a third one, and that is that you ought to look at it and see if there's changes to the engineering design that perhaps ought to be made as opposed to just the modeling or the adequacy of it, but, in fact, should you go back and look to see if the engineering should be changed to make it more robust or less robust as to have an effect on the TSPA.

I find it interesting that we're at the end of the day. The science folks have talked and the TSPA folks have talked and it's really all of us together getting to the bottom line of this thing. We're last because the concepts and the characterization of the site is clearly something that has to be done. When it comes down to it, we have to take all the things that they learn and the TSPA, go through this and come up with an engineered design.

That's our discussion for today. We're going to go through, as Mr. Snell said, five items for tomorrow and look at it in more depth. I'm happy to entertain questions about the design as it is.

DR. CORDING: Thank you. Let's go ahead with some questions on the overview. There's been a lot of progress here and I think it will be very interesting tomorrow to go into some of those specific issues and breaking this down into key issues I think is going to -- is a good way for us to, from our perspective, get a handle on what major concerns you have and how you're integrating this with other parts of the program and how you're continuing to investigate the design. I think it's interesting.

MR. BAILEY: If I may jump it. It would be our intent for future presentations to status you on what we showed, and that is to go through in more detail, other than the five that we're going to, as well as come back and report to you on what we found, what were the results of retrievability, what were the results of ground support, and talk about that in terms of the ongoing development of the engineering design.

DR. CORDING: So we could focus on specific issues at other meetings, and I think that is something that I think we look -- we would appreciate that approach. I think we do appreciate that approach.

John Arendt.

DR. ARENDT: A few questions. The use of the gantry, does it permit you to move packages over another or are you planning on using it in that way?

MR. BAILEY: At the current time, we have the space in the tunnel based on the size of the waste package, the size of the emplacement drifts, what we expect to see in the way of ground support. We have a few inches of clearance that still allows us to move packages over one another. That capability exists. If we start running into a more robust package or a different ground support system or we start closing our tolerances, then we're going to go through a decision process and determine whether or not we need to do that.

DR. ARENDT: Okay. I'll skip around a little bit, but have you -- do you know what the maximum temperature in the repository will be that the remotely operated equipment will see or will be operated in?

MR. BAILEY: We're looking at a design temperature of around 200 degrees C as a final temperature inside the repository drift, maximum temperature inside the repository drift.

DR. ARENDT: Do you know of any remotely operated equipment that operates in that kind of environment? This will be a first of a kind.

MR. BAILEY: That's right. That's one of the reasons that it's up there. It has to do with the environment that it has to operate in reliably.

DR. ARENDT: Are you going to do any -- will there be any prototype work or pre-operational testing or how do you know when you get the thing designed that it's going to operate and for how long it's going to operate? I'm sure that -- maybe it's too early, but you certainly have to take that into consideration. I guess the question is are there going to be prototypes.

MR. BAILEY: Well, it's clearly a developmental program to ensure that the capability exists and whether we do it on a full-scale basis or on a small-scale basis, we're clearly going to have to show that it will operate in that area, determine proper maintenance schedules, determine the materials of consideration that are most likely to fail.

DR. ARENDT: When will you know what's going to come to your door? With the market-driven approach, I can imagine most anything, or not quite that bad, but pretty much so, what you would have to handle at your receiving facility and when will you know what you're going to be required to handle?

MR. BAILEY: Well, we're making assumptions. We've made assumptions in the past in that regard and we're continuing to work with the people who work with the transportation initiative to try and define that. I can't give you a specific date as to when that's going to be known.

DR. ARENDT: Will there be any standards that you might be able to use or specifications that the people will have to use?

MR. BAILEY: Well, we have some interest certainly with regard to the fissile content and with the thermal aspects of it that we would put into the different packages. They in transportation also have some limits associated with the thermal and fissile content and it would be our intent, if possible, to live within the requirements that are being placed on it for transportation.

DR. ARENDT: I understand at Kijema, that if a package comes to the door and it doesn't meet the requirements, that they refuse the package and it has to go back, and I'm just wondering. I know this is a detailed -- I would hope you would do better than that.

MR. BAILEY: It's a detail that we, of course, are concerned about and we don't intend to have happen to us to where we reject what's brought to us.

DR. ARENDT: I think that's all I have.

MR. BAILEY: Rick Craun, I think, would like to add something.

DR. CRAUN: Richard Craun from DOE. I just wanted to add a clarification on the 200 degree limit that Jack indicated. It is a design limit. During the normal emplacement operations, those emplacement drifts will be ventilated. So the remote handling equipment will not be qualified to those types of temperatures. So it's a more complex answer than what you received.

DR. CORDING: Rick, if you were to go back into go in and use that same type of equipment for recovery, I'm not sure whether you're planning to do that or not, but you ventilate to try and get temperatures down or how would you approach that?

DR. CRAUN: The first thought would be, yes, to go ahead and introduce ventilation, cool the drift back down and then go back into entry. Like if you had a drift loaded and then you wanted to go in and do the retrieval later on, you would ventilate it and then go in.

DR. ARENDT: Ed, I have one more. The staging area looked rather small. How are you going to control the thermal loading in the repository, I guess, with such a small staging area or where is that control going to be accomplished, at each reactor site or at the repository?

MR. BAILEY: I presume you mean the staging associated from this drawing, which is out of the ACD, which was when we had the multipurpose canister.

DR. ARENDT: No, not there. I saw over there, I think I saw a few -- there are a few packages. I think, as I remember, it was on the viewgraph over there.

MR. BAILEY: That would have been a pictorial. Mr. Snell is going to talk to this tomorrow, but there are a series of strategies. You can deal with the thermal load as it comes from the reactor if you place it on the transportation. You can deal with it by queuing up the packages as they're received. You can do it by queuing up the individual assemblies before you load them into the package or you can put them into the package and then cool the package before you send it down the main drift into the emplacement drift. You can actually just cool it before you send it down.

DR. ARENDT: That hasn't been decided yet.

MR. BAILEY: There's a whole series of those strategies and, as I said, Mr. Snell is going to talk to that tomorrow. There's a whole series of strategies to accomplish that.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: You pointed out that you were looking at a very specific base case for waste disposal in the repository concept right now, with 70,000 metric tons of uranium in a certain load. If we're going to be doing a bunch of thermal load tests, which we are, that will suggest perhaps that a higher or a lower load is more appropriate, maybe this isn't going to happen right away, obviously it's going to be decades, will we still have the flexibility? Will we know enough about larger pieces of the mountain, for example? This is not a question for you, I guess, but if it's a lower load, we'll have to maybe take a bigger piece of a bigger repository site than is currently being looked at in detail.

If it's a higher load, it could be smaller, obviously. Are these kinds of flexibilities built into how you're viewing the design of the repository right now? Are you maintaining that sort of a larger view, with the option of changing what you do if thermal load is changed based upon some tests we're doing?

MR. BAILEY: Yes. And I was listening to your question while I looked for my slide. This lower block, which we showed only partially filled, that actually runs this area, is being maintained. It's being maintained in our interface drawings. So that this area is not used up. We have some margin yet in this. As I said, we have a 15 percent margin for a standoff distance. If we don't have to use it, then we have some more there. There is perhaps a little bit more land to the north that could be characterized. That's a tradeoff based on the underlying strata. So it's difficult to say exactly right there.

There have been some scoping studies that suggest that there are some other areas that could be used if we get into a very low thermal loading and if we have to go to that, then we would go and do the characterization of those areas. So the answer to your question is an emphatic yes, we are maintaining the ability to go to a different thermal load.

One of our requirements in engineering is to be able to maintain alternatives. We're not optimizing the design, for example, for 80 to 100 MTU. We're going to choose one in there so that we can show through the calculations that it will work and it will be feasible.

If we, in fact, were trying to optimize it for some value between 80 and 100 and ignore the other alternatives, then we, in fact, would have somewhat of a different design. So we are constrained by maintaining a number of alternatives throughout the process.

DR. CORDING: Carl DiBella, Board staff.

DR. DiBELLA: Can you put up that ventilation mid-emplacement overhead? You just had your hand on it a moment ago. I have a question about the ventilation. Yes, that's good. The last time that I recall that the Board had presentation on the repository design was over a year ago and at that time, after emplacement, there was planned to be no ventilation whatsoever. So this is a major change in that there will be some ventilation after emplacement.

My question is this. How much ventilation will there be and will the heat and perhaps the moisture, too, but principally the heat that is removed by that ventilation significantly affect the thermal loading basis?

MR. BAILEY: Mr. Snell, I believe, is going to address that, again, tomorrow. One of the pieces that we looked at in the thermal loading study was the forced ventilation of the drift to equalize the temperature throughout the drift and minimize hot spots, if you will, or, in fact, cool the drift so that the facility not necessarily would be driven to as high a temperature as it might be otherwise.

We have, for the reference design for VA, chosen not to implement that, to try and control the temperature, and what you see here in the ventilation is basically a leakage type ventilation that is meant for radionuclide control in accordance with the regulation. It's not meant as a thermal management means.

DR. DiBELLA: No. I'm asking whether there would be so much ventilation by the leakage. I mean, have you calculated how much leakage there will be? That's what I'm asking.

MR. BAILEY: No. We haven't calculated the leakage, but it's not our intent to try and do thermal management through that means. It's, in fact, a radiological type leakage, to make sure the flow is into the drifts, as opposed to a calculated and intended and controlled flow rate.

DR. CORDING: Is your approach with the layout here to be able to not only dry tunnel from both the west and east sides, but also to emplace waste from both sides? Is that correct?

MR. BAILEY: Yes. Because of the -- and it was actually a question that was asked before.

DR. CORDING: Or either side.

MR. BAILEY: Right. Because of the ventilation drift here in the middle, we have the ability, if we wanted, to do some retrieval to ventilate towards the center in either one. So we could actually emplace from either direction, if need be, or remove from either direction and only have half the tunnel distance to travel, as opposed to the old design where you basically had to empty out the entire drift to get to that package. Now you can actually go from either direction.

DR. CORDING: This is really a much more flexible system for you and it looks more efficient and looks more economic.

MR. BAILEY: Yes, I believe you're correct.

DR. CORDING: Woody Chu, Board staff.

DR. CHU: In the issue areas of remote handling and performance confirmation, both activities require things or instruments to operate routinely for a very long period of time in the hostile environment.

Now, in the issue resolution process, would you consider doing some kind of assessment of reliability, maintainability and availability as part of that resolution process?

MR. BAILEY: Yes. I probably went over that too quickly. Yes. The maintainability, the replaceability, perhaps the ability to send instruments in and bring them out remotely, all of that will have to go into it to ensure that we get the data that we need for the long period of time.

DR. CHU: And some sense of -- some feeling of mean time between failure.

MR. BAILEY: Yes.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: I guess I -- you were discussing -- you mentioned concrete as a possible material in here. Is the prestressed concrete liner concept still something that's viable in the program?

MR. BAILEY: Yes. We believe that that's a viable concept for lining the drifts.

DR. LANGMUIR: What's known about them at 200 degrees and plus, how they handle it? It's a hydrated series of minerals in concrete. They're not going to be very happy at 200 Celsius. I wonder if it isn't going to collapse around your waste packages. I presume that's the kind of thing you'd be testing.

MR. BAILEY: That's exactly the kind of thing that we'd be testing and trying to learn about here in the future. In fact, I think when Mr. Snell talks again tomorrow about thermal, that the drift scale test will do some testing to try and learn about the ground support and the temperature effects on that ground support.

DR. CORDING: We're up for thermal tomorrow, also.

MR. BAILEY: Yes.

DR. CORDING: The question of line load and point loads are things I think we want to find out where you're -- what your present thinking is on that.

Other questions? Staff? Any questions, comment from the audience?

DR. BUSSOD: Gilles Bussod, Los Alamos National Lab. I was looking at your ventilation drift that you're talking about that goes north-south and underneath the repository. Do you know how large a structure is that and how far below the repository horizon or below the repository is it? And if it is large, have you looked at the effect it would have on the natural barrier system?

MR. BAILEY: It's about ten meters below the repository horizon. I don't recall the diameter of it. It's about seven-and-a-half meters, I'm told from the audience, in diameter. I don't believe it impacts the natural barriers perhaps, the mineralogic type. We are talking to PA about the effects of placing it down below and that hasn't been fully evaluated. It is, in fact, a preliminary design.

DR. CORDING: I think we're nearing the end of our session. We want to thank you very much for your presentation and we'll look forward to going into more of the details on the various aspects of this tomorrow.

We are essentially in a public comment session and I'm not sure -- Helen, do we have any requests? No requests for the public comment. You had signs out. Are you sure you had everything set up? Okay.

DR. LANGMUIR: Ed, I'd like to ask a question of some earlier speakers, just a short one.

DR. CORDING: Okay. Don Langmuir wanted to have a public comment.

DR. LANGMUIR: I could move to the audience and it would look better. I wanted to apologize. It was brought to my -- I was reminded that in July, we heard from Alan Flint that the highest measured infiltration rates ever recorded at Yucca Mountain apparently occurred in '95 and some very high rates because of El Nino occurring a couple years previous to that, giving us potentially a pulse of water moving down through the mountain starting in '95, and it seems like it's a very appropriate pulse to be following presumably in the infiltration studies; an opportunity, a one-of-a-kind opportunity to see where the pulse is going and how fast it's moving.

The question is what is DOE doing, if anything, to take advantage of this major pulse and look at its chemistry and its hydrology as it moves on down through the mountain, its chlorine 36 performance and so on.

And Bo is back there talking to someone else. He's a potential answerer of the question.

DR. CORDING: Bo, did you hear that question?

DR. LANGMUIR: And Dennis Williams left. Anybody else? Larry?

DR. CORDING: Larry?

DR. HAYES: Larry Hayes, M&O. We're looking at that proposal of Alan's, evaluating whether or not we really believe that we can see that pulse. There is some various thought on whether or not the timeframe that we would be able to monitor would allow us to see something that would be worth putting money into that kind of study.

So it's one of those that are similar to what Dennis has discussed earlier. We're looking at it. We're going to evaluate whether or not that's something we want to fund.

DR. CORDING: Thank you. Any other comments from anyone in the room?

[No response.]

DR. CORDING: We want to thank you for the presentations today. We appreciate them very much, the effort that was put into this, and we look forward to tomorrow. Our session starts tomorrow at 8:30. It will be just a morning session, but it will be a long morning. We're going to run till -- I think it's about -- yes, it's 1:00. So we'll look forward to seeing you here tomorrow.

[Whereupon, at 5:10 p.m., the meeting was recessed, to reconvene at 8:30 a.m., Thursday, October 10, 1996.]