NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING BOARD MEETING

LESSONS LEARNED IN SITE ASSESSMENT FOR CRITICAL FACILITIES

SATURATED ZONE HYDROLOGY

SITE CHARACTERIZATION UPDATE

Reno, Nevada
April 12, 1994

BOARD MEMBERS PRESENT

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Dr. Clarence R. Allen, Session Chair
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Dr. Edward J. Cording, Member
Dr. Patrick A. Domenico, Member
Dr. Donald Langmuir, Member
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P R O C E E D I N G S

8:30 a.m.

DR. PATRICK DOMENICO: Good morning and welcome to the second day of this meeting of the Nuclear Waste Technical Review Board. My name is Pat Domenico, and I will be the moderator of today's session.

Today's agenda covers three basic areas. First, we'll hear from the State of Nevada about its concerns with environmental issues. Then we will turn our attention to the main topic of today's meeting, basically a review of the saturated zone at Yucca Mountain. Finally, we will hear some updates from the Department of Energy and its contractors on progress on the Yucca Mountain site characterization and repository design activities.

If you have an agenda or a schedule before you, forget it, there's been some changes. First of all, you may notice on the agenda that we have three discussions of the saturated zone at Yucca Mountain, one by Marty Mifflin, a second one by the Department of Energy, Russ Patterson will introduce those presenters, and a third by Linda Lehman who has an alternative approach to the saturated zone.

Unfortunately, Marty could not make it today, some personal problems, and so we have to get away with that, so if we had the Father, the Son and the Holy Ghost, the father is missing today and we'll try to get by with the other two. And the other change is at the beginning of the program, two representatives of the State of Nevada will talk to us. Bob Loux will give us a few words on his ideas of the interim storage in Nevada, and then Steve Frishman will talk on the National Environmental Policy Act for the State of Nevada.

So with that, I think my agenda says we should first call on Bob Loux for the initial statements on interim storage in Nevada. I think I got it right. Is that right? Bob, are you here?

MR. LOUX: Yes, I'm here. Thanks.

Thank you very much. Chairman, members of the Board, thank you for your invitation to be here. As I think Pat indicated, we've got a couple agenda items we'd like to talk about somewhat briefly this morning. One is the issue of the National Environmental Policy Act as it relates to the project and as it's embodied in the Nuclear Waste Policy Act, and Steve Frishman from our office will talk about that in a moment.

I was going to this morning, as indicated, talk a little bit about MRS and interim storage issues briefly. And, of course, I'd be happy to talk about any other issues of concern. I know that there's been some other recent developments in the program, both from our side and DOE's, that I'd be happy to comment on if that was of interest.

In preparation for today, Bill had talked to me about the MRS issue, and as a result, I had provided him with copies of a report completed by Jim Davenport, who is an attorney with the State of Nevada. And this was a report, or a minority report, that emanated from the recent NARUC dialogue on interim spent fuel that was conducted late last year and early this year, resulting in a NARUC report that was finalized at the recent meeting in February.

I mention this report because this is the report that Senator Johnson most recently in hearings held up as a call for renewed interest in legislation regarding MRS. And as you may know, the NARUC report attempted to make a case that on site storage, MRS, on an existing federal site had some particular merit. The minority report done by Jim Davenport that I distributed I think effectively countered nearly all of the arguments that NARUC, the small NARUC group made regarding the viability and desirability of using an existing federal site for an MRS or interim storage. And I provide that to you merely for background. I don't really mean to speak about it, but I'll be happy to answer any questions.

The State of Nevada has been involved with looking at the MRS or interim storage issue for a number of years, and the most formal policy that the state has on this issue is one that's been embodied in Western Governors Association Resolutions, which have been in place for the last three or four years relative to this issue.

Governor Miller sponsored a resolution which was adopted by the WGA first in November, 1991, which made a case against an MRS, especially an MRS in the west, citing principally the position that it's largely unneeded due to the advances that have been made in on site dry storage technology at reactor sites, as well as the NRC's waste confidence proceedings and their conclusions that on site dry storage at reactor sites incrementally up to perhaps as long as 100 plus years is a safe and viable alternative in the short-run for geologic disposal.

This resolution has been recently readopted, in fact June a year ago in Tucson led by Governor Andrews, the 20 or 21 western governors again unanimously adopted a resolution concluding that an MRS in this system is largely unnecessary from a number of vantage points. And so the State of Nevada's position has been reflected in these resolutions. Formally, it is the state's position that, first of all, MRS is unnecessary. There is, as I mentioned, demonstrated safe and economic on site dry storage, and of course the NRC and others' confidence that such storage is safe for many years.

The western governors, and particularly the governor of the state believes more importantly, I think, or as importantly, that an MRS, especially an MRS on a federal site in the west, is not only unfair from sort of an equity perspective, but creates a number of problems, especially in the transportation area, looking at the large numbers of states that could be affected via transportation to a western MRS, and especially even one in Nevada could impact as many as 45 states with transportation routes through them at one point or another.

I think in the same vein relative to transportation, and especially again as it relates to Southern Nevada, the issues associated with transport, especially in the rail transport area, are particularly acute with perhaps the need for a hundred or more miles of new rail line to the site at NTS or Yucca Mountain, since neither right now are served by rail. And the likelihood, as I recall from the environmental assessment that was done on Yucca Mountain by the Department of Energy in 1986, the Yucca Mountain site was, of all of the sites under consideration, was by far the worst from a transportation perspective.

Moreover, I think there's a number of technical or licensing problems that might arise relative to a Southern Nevada, either on the Nevada test site or Yucca Mountain, MRS seismic perhaps background, radioactive elements, and certainly some issues related to compatibility with the other emissions currently at the test site.

I guess lastly, let me conclude by telling you something that you know already, that first of all, an MRS or an interim storage facility is illegal under federal laws as currently drafted and in place, but as you know and has been announced in the press on numerous occasions recently, that Senator Johnson and others are contemplating legislation which may likely either remove that prohibition or in some way alter it, perhaps making it not illegal to have such a facility in the State of Nevada.

Moreover, let me remind you as I've been reminding others, that there is still an existing state law adopted by the Nevada legislature in 1989 which makes the storage or disposal of spent fuel or high level radioactive waste illegal under state law. And as you may recall, those laws came into play during the state's legal actions that were taken in late 1989, 1990 relative to our contention that the amendment had vetoed or had selected Yucca Mountain as a repository site. As you know, during that period of time, of course, the issue of permits for work relative to repository program came into play and were the subject of much discussion, both on the Hill and otherwise, and as you know, those issues were resolved in the spring of 1991, not only when our legal action was completed, but I think by and large when GAO concluded that the state's actions really had no bearing whatsoever on DOE's access to the site.

I only mentioned that issue because if in fact Congress does move next year, as been advertised, to either alter the law to remove the prohibition or in fact name an MRS site in Nevada, there are many in the state who would welcome the opportunity to get back into court on those issues, and we believe such action would make that litigation ripe again relative to the constitutional issues that the state does intend to raise, either on an MRS issue or on a repository issue, whichever comes first, so to speak. And likely the other issues that I spoke of relative to permits and others certainly may come into play as well.

So while there may be an attempt and perhaps even a successful one to remove the current prohibition about MRS in Nevada, the state law that was in place since 1989 becomes ripe, and as I mentioned, many are anxious to use that to get back into court with the Department and the federal government over the MRS issue.

But having said that, clearly the State believes, in conclusion, that the MRS is unnecessary from a number of perspectives. It certainly does not provide any advantages, as once been advertised that it might in the system, and clearly in our mind violates at this point in time both federal and state law.

With that, Mr. Chairman, those are the issues I really want to talk about with the MRS. I'd be happy to describe to you other activities that the State are involved with. I know that some of you have asked me more recently about a couple recent legal actions that the State has taken, and I'd be happy to talk about those if you would like, or we could move on to the other issues, depending on your preference.

DR. DOMENICO: Since we have a hole in the program, I think we would welcome those.

MR. LOUX: Let me address that and one other issue, and then I'll turn the presentation over to Steve Frishman.

The State, as you know, has been concerned for a number of years about the issues associated with deposits in Trench 14 as they related to various hypotheses put forward by Jerry Szymanski and others.

The lawsuit that was filed last week was, in actuality, an appeal of a decision by the district court in Reno regarding our interest in deposing scientists associated with concluding that such deposits were in fact deposited by rain water and not coming from the subsurface, as Jerry and other people have been maintaining.

We had an interest in deposing those members that served on the National Academy Panel that reached certain conclusions about those deposits, as well as certain DOE scientists, in order to preserve their testimony, preserve their findings for use in licensing and other legal proceedings at some future point in time. And our concern was that the program schedule and others is so extended that some of that testimony and some of that expertise may in fact not be available some years down the road, and it was our interest in deposing those scientists who had reached certain conclusions in order that we could in fact preserve that testimony.

That argument that we raised in the district court in Nevada was not acceptable to the court and, hence, we are now appealing, in a sense, that decision to the Ninth Circuit, which was the action that was done last week.

This week, the attorney general filed another original action with the Ninth Circuit relative to the same issue, if you would, in the Ninth Circuit court. And that case really is one which takes exception to the Department of Energy's viewpoint that in fact all of the issues relative to the Trench 14 controversy has reached a conclusion and, therefore, DOE does not intend to actually expend any additional resources investigating that area or the issues surrounding that, and in some sense, the State believes that DOE has reached certain conclusions of suitability regarding that issue and we intend to at least challenge that decision and see where that might lead relative to the courts as well.

What I anticipate may happen is that if either one of these are accepted by the Ninth Circuit, that they would likely be merged at some future point into the same lawsuit. But the most recent one has to do with, moreover, that DOE is not expending any additional resources on the issues associated with Trench 14, does not intend to, and by analogy, believe have reached certain conclusions about those that they don't intend to update, and in fact has reached some suitability determination relative to that particular narrow issue.

Those are the two issues that are currently pending right now in the court, and as I mentioned, if either one of them are acceptable, they likely would be merged into one lawsuit at some future point in time, I suspect.

Lastly, Mr. Chairman, let me just make one remark. DOE has been actively making presentations, not only to you but to others, both within the State and other locations, about options with the program that they envision may become necessary down the road, depending upon funding.

The State I guess is concerned about those issues and about a restructured program. It seems to us that every time the program reaches a certain point, a major either restructuring or reorganization, re-reviewing, comes along which ends up resulting in some new improved program to some degree. And I understand that the presentation you heard yesterday was substantially different than the one essentially the state and local government heard a week ago in Las Vegas.

Nonetheless, all of this appears to us to be symptomatic of a program that is in some chaos. For example, the Department is unsure right now how much money it's going to get in the future, whether it's going to get the revolving fund that it's asking for via legislation. It doesn't appear to us that it's going to happen this year, and may in fact be brought up next year. But certainly a great deal of uncertainty about how much money it's going to get. There appears to be a great deal of uncertainty about what kind of program they're running, whether they're really continuing to run a repository program, or at some point next year, maybe attempting to run an MRS program, or in fact maybe running an MPC program or an MRS program. And all of these appear to us to be very symptomatic of the kinds of problems that the program has been experiencing I guess since the major destruction of the 1982 Nuclear Waste Policy Act that occurred.

As you know, the 1987 selection or amendments act selecting Yucca Mountain as the only site to be studied was advertised by Senator Johnson then as the "we have finally solved the waste program" only to be found two years later by Secretary Watkins that the program was in such disarray it needed restructuring, and now we're hearing the Secretary indicate that major retooling of the program once more is needed, and various reviews of many kinds are currently either underway or contemplated to be done.

One is a review of the major criticisms that Professor Thurber did, which has been concluded but is yet to be really initiated, the financial management review that the Secretary has long talked about. And meanwhile the calls that not only you have made but we and others have made for major comprehensive review of the program continue to go unheeded, and more recently I heard that the Secretary indicated that she would not honor, once again, these calls.

So it appears to us that there's a great deal of uncertainty in the program. A year from now, we could be examining an MRS program and not a repository program is possible, could be examining a long-term interim storage program of some sort or a hybrid thereof with MPC's and a repository program of some nature. But, clearly, there's changes in the wind, and I guess like you, we will sit by and watch and see what emerges down the road.

But, Mr. Chairman, with that, I'd be happy to answer any questions and then turn the balance of our time over to Steve Frishman.

DR. DOMENICO: Thank you very much, Bob. I guess there's uncertainty in science and uncertainty in funding, but there's more certainty in the uncertainty of funding than in science.

I think that there's a different message given to us here than we might get from Steve, so I think at this time, I would like to open it up to any questions that the Board may have regarding this presentation. Staff?

MR. LOUX: I'm certainly glad to know that I was concise and accurate.

DR. DOMENICO: I'll try one more time. Does anyone out in the audience have a question here?

MR. BARNARD: Bill Barnard. Bob, does the State of Nevada have an official position on Scenario A?

MR. LOUX: Scenario A, as I recall, is the--you might refresh my memory--but is the scenario where they receive the same level of funding that they have been receiving; is that correct or not?

MR. BARNARD: No, how to get funding level of 532 million current schedule and license application by 2001.

MR. LOUX: Well, I guess it's often been remarked that I guess only in Washington would you reward a program that is being poorly managed, poorly run, has numerous calls for independent review, at least GAO has documented some percent of their money is actually being spent on studies and science. Only in Washington would you reward that kind of effort with more money and an accelerated schedule.

Obviously, we've got a great deal of problem with that kind of program. It certainly is putting the cart before the horse, and we believe that a thorough investigation of suitability, of concentrating I guess initially, and that we may part company here with the Board, but with initial suitability with an exhaustive look at surface base work, and then only going underground is the kind of program that ought to be looked at.

Moreover, we think that certain conclusions about site suitability can be and should have been reached already relative to the program, and clearly our view is that the site should and ought to have been disqualified under DOE siting guidelines sometime ago.

DR. DOMENICO: Bob, is it your position that the program does not need an MRS or does not need an MRS in Western United States?

MR. LOUX: We don't think that the program needs an MRS, period. It clearly doesn't need one in the Western United States. If anything, DOE, through the concentric circles logic in 1985, demonstrated that perhaps somewhere in the Eastern United States would be more logical if one was needed. But, clearly, from our perspective, one is not needed at all in the system.

One thing I failed to mention in the resolutions in the discussions that I know governors have had is that their fundamental premise, and it was also contained in the Jim

Davenport report, is that you simply cannot force facilities on states or locales that don't want them. Governors are unanimous in their viewpoint that any of these sorts of facilities, even if they do come to pass, cannot be done without the consent of the governor of those states. And I suspect that applies to the current situation in New Mexico as well.

DR. DOMENICO: Are there any other questions?

Thank you very much.

MR. LOUX: Thank you very much.

DR. DOMENICO: We'll now hear from Steve Frishman on the National Environmental Policy Act, State of Nevada issues and concerns.

MR. FRISHMAN: Thank you. I have so much to say, I have finally fallen into the pattern that all the rest of you are in with playing with viewgraphs.

The last time I did this, strangely enough was on the same subject, almost exactly four years ago to the day. If you recall--I didn't really recall, but I found in my notes, my file, that I had on some day in April, 1990 spoken to you on the subject of the National Environmental Policy Act. I think every four years is probably--it's probably healthier to remember that we still had some responsibilities there and so does the Department of Energy.

I want to go through with you something that has a lot of sort of basic principles involved, primarily because I don't think it's been discussed with the Board before and I think since it's a coming issue now, it might be good to at least present some of the basics relative to the way we see DOE's planning going, relative to some of the thoughts that the Board has put on paper, and just sort of leave some questions and some concerns.

We've been discussing this subject with the affected counties in Nevada and California, and some of the questions and concerns that we're raising here today have come out of our joint discussions on the subject of NEPA implementation with the program. So this is an ongoing discussion that we have. The affected counties are certainly interested. They certainly recognize that this is a major area for their participation should the program go forward to that point.

DR. DOMENICO: You see, you're not used to giving viewgraphs.

MR. FRISHMAN: Well, I hope it won't work. Then I'll make you read, because I'm not going to read it all to you. You can see my title page, so I don't have to do that.

The Nuclear Waste Policy Act, as we all know, has some specific directions about implementation of the National Environmental Policy Act, and also has some limitations. And those limitations tend to be overstated sometimes, so I think it's probably instructive if we look at what those limitations really are and see the extent to which they do apply.

If you look at Section 113(c), aside from the good stuff at the top that is forbidden discussion at this table probably, you see at the bottom underlined is the requirement for compliance with the National Environmental Policy Act. And that means the Department of Energy should be expending funds in the area of that compliance. And you heard, or one of your panels heard a couple weeks ago about the extent to which that is being done and not done relative to what some people consider is sufficient implementation of NEPA.

There's also some limitations, and if you look towards the bottom, "except that the secretary shall not be required in any such statement to consider the need for a repository, alternatives to geologic disposal, or alternatives to the Yucca Mountain site." That is the limitation, and that applies only to repository. It has nothing to do with the rest of the program whatsoever.

That limitation is again restated in the Amendments Act. It says essentially the same thing, and this is in Section 114(f), which is the actual NEPA implementation section.

Now, DOE has become a little bit more clear just very recently in its plans for NEPA implementation throughout the program, since the program is now getting more and more segmented into different activities that ultimately are supposed to fit together.

First of all, DOE has in its rules a NEPA implementation requirement. Under NEPA, the practice by federal agencies is they write their own implementation rules. This is a summary of the sequence of events under the DOE NEPA implementation rules, and this becomes important in the sense that at different times, we hear different plans that sometimes are in accord with this sequence of events, sometimes not.

Most recently, I'm happy to say, they have been becoming more and more in accord with their own rules for sequence of events. And you'll see my discussing some of these elements as we go along today.

I'm actually eliminating some of these that we don't have to go through. In Admiral Watkins' report to Congress in 1989 when he revamped the schedule of the program after determining that it couldn't be implemented the way it was developed, there was a sequence of events that involved NEPA implementation And the 1990 through '98 period, the site characterization period, spoke to developing information for an environmental impact statement, put scoping out beginning in 1997, draft an environmental impact statement in '99, final optimistically a year later. So that program that is right now about to be changed again had a NEPA implementation schedule.

The current program has sufficient parts where the Department is now discussing NEPA implementation in terms of three and maybe even four separate EIS's; one of them for the multi-purpose container that we'll talk about in a minute, one of them for the repository site recommendation which you saw required under the Act, one for rail access spur, and I'll show you a schedule that shows how all that fits together, and a final for an MRS if there is to be an MRS. And I'm just giving you the two citations from the Act for the requirement for an EIS, if the Secretary follows the procedure in the Act for the Department to select an MRS, or if the MRS is selected or is brought forward by the negotiator. So we're talking a possibility of three or four distinct EIS's for a program that has been more and more fragmented within the Department and also within the Department's thinking.

You saw a schedule that was an abbreviated version of this a couple times yesterday. And the things that are important to look at on here are the NEPA implementation points. If you--and we're looking at the administration funding proposal. If you look at the NEPA section down here, you see this is the notice of intent, which was the top of the list of sequence, so you draft EIS, final EIS coming at about the end of '99. And that's for the repository.

You see another one over here, a notice of intent. This is for the rail spur. And if you look up the line, you see that the notice of intent for a rail spur is just one year away from a construction authorization for the repository. So what we're doing is seeing more and more a sequencing of events through this process where you have separate EIS's and now we have even one more as of last Friday, the notice of intent for the MPC is now intended to be issued this fall in 1994. You heard yesterday notice of intent for the repository is to begin sometime in 1995.

Public scoping for the MPC to begin in late '94, public scoping for the repository to begin probably sometime late '95 or '96, as you were told yesterday, and for the MPC, we're looking at a draft EIS in the fall of '95, which is pretty quick, and a final EIS in '96 for the MPC.

So I think you're seeing the pattern that I've been watching develop and it's developing almost daily now.

So the big question comes up now in terms of the National Environmental Policy Act what's required under that Act relative to the program under the Nuclear Waste Policy Act. Sure, it's a very complicated program, it's a very large program, but at the same time, other large complicated programs have complied with the National Environmental Policy Act.

And so I think it's important for the Board maybe just this once in your whole life to go through what some of the basic requirements of NEPA are, and I've selected them in a way where we can relate them directly to the parts of this program. And I don't think it will be too burdensome, and the reason I don't think so is because from your own statement, I can see that you have something similar in mind, but maybe not in terms and maybe not thinking in terms of NEPA. And this is where you have been concerned for a very long time, and this is just the most recent statement that I think I've picked up, about the necessity for looking at the entire waste management system, and this is the same thing NEPA wants done, one way or another, look at the entire waste management system.

So I think your interests are compatible in the sense that ultimately what you're asking for is going to have to come to be, and ultimately if the program proceeds, what NEPA implementation is necessary is also going to have to come to be, and I think they converge at this point with your interests.

Now, I'm going to be throwing a fair number of just citations at you. I'm not going to read them out loud to you. I just want to show you that they're here and that there are words that are necessary in discussing NEPA, and that there is a regulatory basis surrounding each one of these words, and you'll see how they all sort of come together over the next very few minutes.

We're talking here about what is the scope of the inquiry that is necessary for an EIS, just what do we mean by scope. And we have to remember that the purpose of NEPA is for informed decision making. It doesn't make decisions; it informs. And it informs through various types of analyses and also very much through the participation of all of the interested parties, including the one proposing an activity.

Now, scope consists of a range of actions, so here's some discussion of what is meant by actions and different types of actions that have to be considered. You have the possibility of connected actions, those that might automatically trigger other actions that can't or won't proceed unless some actions are previously taken, or that are interdependent parts of a bigger action. You also have to consider cumulative actions and also the possibility of similar actions.

You also have to consider alternatives and you have to consider again three types of alternatives, the no action alternative, other reasonable courses of action, and mitigation measures or actions that aren't included within the proposed action. And we'll talk about these alternatives again in just a couple minutes.

The types of impacts that have to be looked at in an environmental impact statement; direct, indirect and cumulative. I think that's fairly obvious, although there is always controversy over the extent to which indirect actions or indirect impacts are considered in an EIS. There is always controversy over whether the cumulative impacts are adequately treated in an EIS.

Now, if we go back to scope for a minute, you see that in discussing relationship to other statements, we have a reference to the concept of tiering. So if you look at what the regulations say about tiering, you see that it's intended sometimes for very large projects when it's possible to look at the entire program and then start dropping EIS's out of it as it's appropriate either in time or in development of the program. But tiering allows for what is generally referred to as a programmatic EIS, and then you bring subsequent EIS's out of that. It sort of sets the principles of the program, looks at the general broad scope of alternatives, the general broad scope of impacts and so on.

And the regulations tell you when tiering is appropriate. You can go from a program plan down to actions or proposals of lesser scope, or if there's a timing sequence when you don't have such problems as interdependencies and so on. So tiering was expected by those who drafted both the Act and the regulations, and also at times is found appropriate for large national programs that have many components.

Now, in our discussions we've come up with a set of pretty general questions and concerns regarding the way the Department is proposing to implement NEPA in terms of its three or four EIS's, some of them even overlapping in time. In fact, most of them probably overlapping in time.

Now, if we go back to remember some of the elements under scope, if we go back to questions about the scope, are the three or four statements connected, meaning do they automatically trigger other actions, where they can't or won't proceed unless other actions are taken, or that they are interdependent parts only justifiable by a larger action. And these are questions that are going to be repeatedly asked about the Department's implementations plans relative to this large number of EIS's.

If you look at impacts, are the actions proposed in the three or four statements, are they cumulative? And if we go to another one of the criteria in the regs., do they have common timing or geography? And these are questions that I think all of us can in a general sense start to answer right away.

If we ask the question about tiering, taken together, they constitute a national program or plan appropriate for tiering, the sequence is from a general statement to statements of lesser scope, specific proposed actions are sequenced in time, for which supplemental statements may be preferred. I think you can start seeing the questions, the continual questions that are coming up, and our concerns about the way the Department so far has been describing their implementation approach to us.

Now, this is the first of I think three general concerns that we just want to leave with the board, let you know that we're thinking about them, and we think that probably you should be thinking about them as well in terms of your concerns with the program development and also the systems analysis of the program.

It's unclear how DOE's plans for environmental impact statement issued at different times on different elements of the system will adequately address national, regional and local transportation issues in a comprehensive manner. We can't find any way when you look at MPC, MRS, a separate one for a rail line, and also still the open question that to my knowledge has never been resolved about where the transportation responsibility lies within the program relative to NEPA. We see it at fixed points in the program now, but we don't see it on the map.

It's also unclear how the implementation as planned will permit comprehensive evaluation of alternatives and options within the overall waste management system that maximizes protection of health and safety on the environment. This is going back to the cumulative questions and some other questions that are associated as well.

This one is one that I know is somewhat controversial at this point within the Department. I'm not sure that it's even a major question to people outside because it's taken for granted, I believe, anyway by those of us on the outside of the program that the Department has got to consider a no action alternative. I think the question came up that there's nothing in the waste policy act that says the no action alternative is not to be considered. It does say what those limitations are, if you remember, and no action is not included.

And I think in your panel meeting a couple weeks ago, you heard from both a CEQ attorney and a DOE attorney that they believe that if the limitation extended to excluding consideration of the no action alternative, the Act probably would have said that.

So now if you have three or four different EIS's that are all pointing towards one way or another, either at or pointing towards a repository and you don't consider the no action alternative, then what you're doing is you are eliminating the required consideration of alternatives under the National Environmental Policy Act. And as we heard yesterday and we continually hear, Yucca Mountain is not a sure thing. So if you start out early with an MPC EIS, you've got to consider the fact that there will not be a repository, because right now if it isn't Yucca Mountain, there won't be one under the law.

So the no action alternative has got to be considered, and it's got to be considered programmatically, because if you recall, the goal of the Waste Policy Act is permanent isolation of the waste. The goal is not transportation, the goal is not storage; it's permanent isolation. And if you don't have a permanent isolation as defined under the current legislation, then all you have is no action. Now, how do you deal with that in the other planned EIS's? This is something the Department is going to have to come to terms with.

Now, just for a conclusion, since I don't want to be recommending the direction the Department ought to go with NEPA implementation because there are a number of different ways it could be done, and what I'm pointing out is, at this time anyway, is that I think that the way I see the Department's NEPA implementation program developing, it is not compatible with NEPA, but I think there are some configurations that might be, and I think there's room for discussion. This might be another place where we can all eat steak together and talk about something.

The answers to the fundamental NEPA implementation questions and concerns will be crucial to defining an integrated waste management system. NEPA may be the way they finally--the Department finally is forced to do the systems analysis that the Board believes is going to have to be done before this program can be developed, even conceptually on paper, to the extent that satisfies you in terms of yes, everything does lock together and, yes, you have looked at it in terms of optimization, in terms of trade-off, in terms of maximizing safety and so on.

And the selection of the alternatives should be based on a systems analysis, because I think with a system this complex, with this many variables in it, I agree with you, that's probably about the only way to assure yourself that in fact you are managing the decision trail in a way that is traceable and in fact reasonable and shows that you have analyzed all of the steps and that the trail that you have selected is an informed decision right back to NEPA and the Board's wanting a systems analysis, the convergence once again.

So I hope that this discussion has been helpful to you in terms of understanding maybe how some of your concerns fit into larger concerns that we and the affected governments here and in California have been concerned about. And at this point, I'll leave it with you I guess just to think about it and you may want to get farther into this subject at some time in the near future because if the Department's plans go as proposed, we're going to be looking at a notice of intent probably within about six months just on the MPC, and there are going to be some serious questions then that are essentially the same questions that I've raised here about scope, and what is an EIS for an MPC. The Department has decided that that EIS, that the timing of that EIS and the proposed action of that EIS will be the deployment and implementation schedule of the MPC. So what's included in all that and can it be done in isolation from the rest of the program if, in fact, the program is all about permanent isolation and the goal of the program is not storage and transportation.

So I leave that with you and I'm certainly available to discuss it more, and we are going to continue discussing it. I know the Department is going to need to continue discussing it, and as was pointed out yesterday, many of the problems in a program like this are procedural, and procedural problems relative to the National Environmental Policy Act often end up in litigation. And, once again, we're in the situation where the Department has some pretty well defined responsibilities relative to NEPA.

There's a lot of precedent out there relative to NEPA, and most of the time when the Department of Energy and other federal agencies get in trouble with NEPA is when they don't look at the questions as simply as I've just presented them to you, and they end up in procedural violations because it's very difficult for a court to deal with the exact substance and the data that goes in, but the agency is bound to live through and be able to substantiate their decisions relative to some of the simple questions that I've raised here that are directly out of the language of the regulations for the National Environmental Policy Act.

I'll be glad to answer any questions that you have, or we can go on to hydrology.

DR. DOMENICO: Thank you very much, Steve. Does the Board have any questions here? We've long been in support of the systems approach to the whole waste management system. I think you know that. Any questions from the staff?

DR. CHU: This is Woody Chu on the staff.

Steve, you said you didn't want to tell the Department of Energy as to what course of action it should follow. But on the other hand, you've made reference and hinted at several times on the programmatic EIS. If the Department were to produce a PEIS, would that allay some of your concerns?

MR. FRISHMAN: I guess the reason that I mentioned PEIS is because that's the obvious out of tiering. Now, PEIS is one approach that I think procedurally could be done in a way that was in compliance. But also I don't think that it's necessary to completely reject the concept of one EIS for the whole system. While that may not be very helpful to the Department's schedules as they read them right now, I don't think, given the realities of the program, that that concept needs to be rejected out of hand. So there's at least two possibilities.

The only reason PEIS or programmatic EIS and tiering came out is because tiering is an element that is available under the regulations and one that should be at least considered, just as doing it the simpler way, or procedurally the simpler way with one EIS, should also be considered.

I really do not have a preference at this point. But either one of them could be done in a manner that was sufficient under the regulations.

DR. CHU: So if it were to be one EIS for the entire program, and given the Department's schedule for the deployment of the MPC, then that one program, in your view, I would think needs to be begun in the fall of '94.

MR. FRISHMAN: It means that they have to start defining their program to the extent that is sufficient for a description of the proposed action in an environmental impact statement. It would be the first time in twelve years we heard anything like it.

MR. WILLIAMS: Steve, I'm not a lawyer. Bob Williams from EPRI.

MR. FRISHMAN: I'm not either.

MR. WILLIAMS: Forgive me. Could you explain in simple terms your view of what role the generic EIS for transportation, I think it was NUREG 0170, it covered about 1,200 megawatts of capacity and the transportation that went into that? And then, secondly, what role the generic environmental impact statement for Mine Geologic Disposal served? It seemed to me that these both cover the broad questions that you allude to and the issues related to Yucca Mountain and to the MPC are relative nits in that larger scheme of things.

MR. FRISHMAN: Okay, not being a lawyer, I'll explain them to you the way I see them. NUREG-0170 became a center point in a legal action back in the early Eighties called the Pacific Inland Waterways case, and at that time, the judge determined that that EIS was no longer valid for use. And the general statement was that an EIS more than about ten years old, and especially a generic EIS more than about ten years old, should not be considered valid. So 0170, I considered to be essentially off the books, and that case was about the transportation of spent fuel coming back from Taiwan into the Port of Seattle, I think, and the court found that the EIS was just simply not valid for the action.

In terms of the 1980 generic EIS, the purpose of that EIS was to select a means for permanent isolation or disposal, and the action out of that was the selection of the policy of deep geologic disposal. It covers a lot of territory, but it is not sufficient to analyze a system for managing and disposing of waste. It's sole purpose was to analyze the alternatives that are available for what to do on a permanent basis with spent fuel and high level waste.

Also, if you look at the thing, if you want to glean anything out of it that might be useful, in addition, you find out that the procedures involved in--or the suggested procedures associated with what was the preferred action, which is deep geologic disposal, virtually every one of those procedures has been violated. If you recall, the 1982 Act was, you know, in a lot of ways compatible with that procedure, and all of that procedure is gone now.

I'll take you back to even some of the procedures that were rejected. For instance, that 1980 EIS said that there should be an EIS for site characterization, and that was rejected in the development of the '82 Act. It also talked about having to look at multiple sites. We no longer have that. We have, at least from the Department's perspective, a question of whether no action even needs to be looked at.

So I think that EIS was written for a different purpose. It may have some useful information in it, but I don't think it has anything that is procedurally useful to the Department at this point. And, in fact, there's ultimately through time, if we all keep going the way we have been, it may be that that finding of a preferred action is no longer even valid.

DR. DOMENICO: Any statements from the Department of Energy on this?

MR. BROCOUM: Steve Brocoum, DOE. I just want to make one comment. Steve, only an assumption for the presentation, we will have three or four EIS's. That hasn't been decided at this point. We are looking at that right now. So there are other options to have a single EIS, EIS supplements later on. So I just wanted to make that clear. We haven't decided how many EIS's.

MR. FRISHMAN: I guess what got me most interested in this was I saw a schedule and I also heard on Friday that scoping for the MPC/EIS is imminent.

MR. BECHTEL: Dennis Bechtel from Clark County. I'm also a representative of the Assistant Secretary, Grumbly's advisory board on DOE weapon sites cleanup.

Just one comment. EM has been kind of touted as something that DOE is doing right. And I know in our work on the advisory board, one of the things we're working with is a programmatic environmental impact statement, and we are looking at issues such as transportation, waste options, things like that. So I think that's an example of what DOE is perhaps doing right, and looking at a very complex issue programmatically. So I just wanted to add that for the record.

DR. DOMENICO: I guess that's in support of what is going on. Are there any other questions from anyone else?

Steve, thank you very much.

MR. FRISHMAN: Okay. Well, thank you.

DR. DOMENICO: My list says here that the next several whatever that means, presentations will be given by the Department of Energy and its contractors. Russell Patterson of the Yucca Mountain Site will I guess give us a short introduction to set the stage for these presentations and tell us just how many are included in the several that we have here.

MR. PATTERSON: Thank you. Can everybody hear me okay?

To answer your question, several what we have planned before the break was myself, Dick Luckey and Kenzi Karasaki of LBL. So I want to thank you for having this opportunity to give a quick overview and introduction of the following speakers. And with that, I'll go ahead and get started.

First is the slide so you can all find the handouts. And now I want to ask a basic question that's always asked, is why do we study saturated zone hydrology at an unsaturated zone site. My believe is the study of the saturated zone provides information required to address many of the issues which will be used to determine the suitability of Yucca Mountain as a high level nuclear waste repository.

Some of these issues which I wanted to just put up here is performance issues, such as groundwater protection, to identify the aquifers within 5 kilometers of the controlled area, to determine the aquifer vulnerability to contamination, groundwater travel time, the time for the water to move from the repository horizon to the accessible environment, and what path the groundwater might follow, total system performance, such as water transport of radionuclides to the accessible environment, water movement away from the repository, thermal effects on water movement. These are all some of the issues that we're looking at.

There's also design issues such as configuration of the underground facilities, what's expected fluctuation in the potentiometric surface beneath the repository, and what's the wetness of the rock around the repository excavation. We also have preclosure design and technical feasibilities, probability of perched water and possible inflow rates into the repository openings, and such regulatory issues as pre-emplacement groundwater travel time and prediction of radionuclide releases to the accessible environment.

The interface of the saturated zone program with other characterization programs is quite intense. We interface with the geochemistry program, the rock characteristics program, thermal and mechanical rock properties program, plus others, and we of course feed information to performance assessment and data, as do the other programs.

I wanted to go into that just a little bit more with ties between the site characterization program, not just the saturated zone program, but the whole site characterization program. The site program collects and interprets data, constructs 1, 2 and 3-dimensional site models, and regional models in our case, test, verifies and documents site models, and provides these site models to performance assessment. Performance assessment performs the sensitivity studies using site models, and from them, determines what's needed for TSPA, and then communicates their requirements or refinements back to the site characterization program.

What I wanted to impress with you at this slide is that we're looking at a very small portion of the overall geohydrology program. When we're looking at the site saturated zone program or the saturated zone program in general, we have the regional hydrology investigations and we have the site unsaturated zone hydrology investigations, which include a whole other large program. If you have a week sometime, we could go through the whole unsaturated zone and saturated zone programs with you.

What we're going to be hearing about some today will be actually further down in here, which is characterization of site saturated zone hydrochemistry, saturated zone hydrologic system synthesis and modeling, and some of the actual detailed studies that we are looking at down in this area.

What we look at in the saturated zone hydrology program--I don't know how this one got capitalized--regional hydrology, where the recharge areas and discharge areas for the saturated zone, and what does the regional flow system look, and how might these change over time.

In the site hydrology program, we look at how does water move through the saturated zone, how fast does it move, and how might it change over time.

For those of you that may not know out there, the regional system actually is a little bit larger than this to the north up this way, and a little bit further down south, but that's the approximate area, this being the pork chop right there. And the site, what we refer to as the site area, as far as for saturated zone studies, is basically this area.

The regional investigations are actually broken into two pieces as far as hydrology investigations. I hate to say broken, but it comes in two different study plans, and these are both handled by the U. S. Geological Survey. One is the characterization of the regional groundwater flow system, the other is the regional hydrologic system synthesis and modeling.

And the site saturated zone program actually comes into a number of ten studies under the old SCP, and I've broken that basically out into four general studies that I like to call the saturated zone hydrologic modeling, hydrochemical characterization, fractured rock test and hydrologic monitoring.

Up here, we have both the USGS and the Lawrence Berkeley Labs. Under hydrochemical characterization, the USGS is our participant. Along here, we have both USGS and Las Alamos, Las Alamos doing reactive tracer tests at the C-wells and at other sites throughout the area, and the USGS doing the hydrologic monitoring.

With that, that's a fairly quick overview. Once again, I want to impress that what you're going to be hearing is some very detailed information, which you requested, on some pieces of the studies, and it's not by any means the whole program. So with that, I'll turn it over to Dick Luckey from the USGS to go into more details on the saturated zone.

DR. DOMENICO: Thank you, Russ.

MR. LUCKEY: This should help find your handout. It's the one with the green cover. That's the way I have to find everything, is by color instead of by words.

I'm going to continue on with what Russ Patterson did and give you my perspective of studies of the saturated zone by the USGS, Las Alamos National Laboratory, Lawrence Berkeley Laboratory. This is kind of an organization of what I'm going to talk about. Don't worry about me really talking about this for 45 minutes. I'm going to be giving half of my time to Kenzi Karasaki from Lawrence Berkeley Laboratory. So I'll be covering this in about 20 minutes.

When the original schedule was put together, there was some discussion about having Ike Winograd talk about the water levels in Devil's Hole. Ike doesn't work for the program and has other duties that he had to attend to, and he's sorry he couldn't make it, but he has talked to the Board before. He did send a reprint from a recent article on the water levels in Devil's Hole, which I've provided to the board.

I'm going to do some of the same things that Russ did. I'll talk about from my perspective why study the saturated zone, why do regional studies, do kind of an overview of regional and then site studies. I'm going to be spending more time on an overview of the various modeling studies and how those fit together, because I think there's some concern there that I'd like to talk about, and then we'll go on to the next speakers.

Russ introduced this question very well, why study the saturated zone when we're concerned with an unsaturated site. The saturated zone is described as the last barrier before the accessible environment. It's not the only barrier, it's not the best barrier, but at least it is a barrier and it does need to be recognized as such.

Early in this program, the saturated zone was probably written off too soon. The unsaturated zone peer review and the board itself has pointed out that probably this was a mistake to write it off too completely. The saturated zone can be studied using widely accepted techniques. It's not quite the state of the art that the unsaturated zone is. The level of uncertainty in our studies of the saturated zone may not be as large as our level of uncertainty in the unsaturated zone.

The cost of study in the saturated zone may be significantly smaller than the unsaturated zone. So sometimes we need to do studies based on how much information we can get for the dollar spent. The studies can be done using standard surface-based techniques, and these studies can contribute to our overall "scientific confidence" in the site. This is kind of a term that we frequently use. It's more of a warm fuzzy feeling about how well we understand the site. It's not pointed at any specific regulation, any licensing issue. It's whether we really feel like we understand the physics of the system and how it operates.

And last, but certainly not least, the saturated zone is obviously the lower boundary of the unsaturated zone. So we at least need to know where it's at.

Regional hydrology has generally been downplayed in this program as something that's kind of fun to look at but not really appropriate for the program. I'd like to make the pitch that we do need to pay attention to regional hydrology. It's certainly not going to answer all of our questions. We don't need to put all of our money in that, but it's something that we certainly cannot ignore.

The region provides the appropriate hydrologic framework for our site studies. It allows us to acquire understanding from various previous studies that have been made of the region, and it allows us to link with concurrent studies that are going on in the region. There's a large DOE study over on the weapons site of the program, the environmental cleanup of the site. We need to take into account those studies. Las Vegas Valley Water District is looking at the region, as is the National Parks Service, based on their interest in Death Valley National Monument. So we need to understand what's going on in those studies.

There are some processes going on in the region that are somewhat masked at Yucca Mountain. By looking at regional studies, we can begin to understand some processes that aren't quite so obvious right at Yucca Mountain. Again, this contributes to this "scientific confidence" that we have in the site, and these regional studies allow us to collect information at a very low cost. Generally, we're tagging on to other studies and just acquiring data that someone else paid to collect.

This is, again, just an overview of the program. I'm going to use this just to emphasize what we're not going to be talking about. We're going to be talking about bits and pieces of the entire saturated zone program. I already hit on regional hydrology. Russ pointed out that there are a couple of studies under that investigation. This is the usual four tier diagram where we have the hydrology program at the top, various investigations at the next level, various studies at the next level, and various activities at the final level. And each one of these investigations can be broken down into these various blocks, and each one of these studies can.

In this particular example, we've just gone through the site saturated zone investigation, the characterization of the site saturated zone study, and then its eight activities.

This is just a listing of the various activities under the regional saturated zone studies as described in the SCP and the two study plans that talk about this. There's the assessment of the existing regional data, and then a regional potentiometric and hydrologic framework study. These two studies together generally acquire and put together the information that is needed by regional modeling.

Then there's the Fortymile Wash recharge study which is very close to Yucca Mountain and has provided us some very interesting data over the last couple of years. There's an evapotranspiration study down at Franklin Lake, Alkali Flat, depending on who you talk to. It's the same place. That's another area very close to Yucca Mountain that's sort of a local discharge area.

And then under these regional studies, there's various modeling studies. I'm going to try to talk about those all together in the end.

In the site saturated zone studies as described in the SCP, there are eight activities. I'm going to go over those quickly as to what we intend to get out of those. The first one is the potentiometric level studies. It was originally designed to determine the direction of groundwater flow in the saturated zone away from the repository. That's been accomplished to a greater or lesser degree. This study is to look at the cause of the large hydraulic gradient to the north of Yucca Mountain that everybody seems so interested in, and the moderate hydraulic gradient west of Yucca Mountain that people seem less interested in. This study is to determine the stability of the water table over time as the lower boundary to the unsaturated zone and this study will also provide estimates of hydraulic properties that will be used for the site model.

The C-well hydraulic tests that have begun, pumping has not begun but the instrumentation is already in the hole. The primary purpose of this study is to develop methods for estimating hydraulic properties. The secondary purpose of this study is to characterize those hydraulic properties at one site at Yucca Mountain, namely the C-well sites. As I mentioned, the packers and the instrumentation are already in place for this study. Pumping of those wells will begin soon, and we're already actively doing prototype work at a Raymond, California site, and I think Kenzi may say a word or two about that when he does his presentation.

The next step at the C-well complex is to do conservative tracers. Again, the primary purpose at this site is to develop the techniques for doing tracer tests in fractured rock. The secondary purpose of this is to characterize the transport characteristics at this one site at Yucca Mountain, and we're also doing prototype work with LBL at this site, at the Raymond site.

This is the revised potentiometric surface map that was recently published as a result of the potentiometric level study. This is an update of the map that was published in 1985 by Jim Robison. This map covers just the small hydraulic gradient area which was not covered in sufficient detail by the previous report. The large hydraulic gradient off to the north in the pattern area is not well enough understood to contour in this sort of detail at this point in time.

The moderate hydraulic gradient off to the west of Solitario Canyon and at Well H-5 is also not understood well enough at this point in time to contour in detail. Note that this map uses quarter meter contours, so it would be very difficult to do detailed studies of the large and moderate gradients. You'll note that the potentiometric surface slopes very strongly to the east on this map towards Fortymile Wash. There's some indications that there's a similar slope to the west towards Fortymile Wash off from Jackass Flats, but we certainly don't have the detail to prepare this sort of map off to the east.

Yes, Pat?

DR. DOMENICO: Why does this differ from the original maps where flow was to the south? What new information is here that has not been on the piezometric surfaces that I've viewed before? We had a definite southward flow before.

MR. LUCKEY: The previous map contoured the large hydraulic gradient, and if you'll look at the patterned area up in the north where the potentiometric surface altitude is about 10:30, and down in this area, it's about 7:30, we see a very strong north-south component. The original map had only one contour through the what we call the small hydraulic gradient area, that had a 7:30 contour that just sort of wound around through this small hydraulic gradient. And so the perspective of the map was very much dominated by the large hydraulic gradient in the north. So in this map where we don't contour the large hydraulic gradient, you don't get that north-south perspective. It's just based on what we're contouring here.

DR. DOMENICO: In other words, it's not additional information, it's a different interpretation of the same information.

MR. LUCKEY: It's slightly more accurate information than Jim Robison had to work with. He could not contour the small hydraulic gradient area because the errors in the water levels were larger than the gradient in that area. So it was impossible to contour, so basically he ended up contouring the large hydraulic gradient area into the small hydraulic gradient area. And so this map needs to be superimposed on the previous map, although they don't fit very well.

DR. DOMENICO: No, they don't.

MR. LUCKEY: But if you compare this map and Jim

Robison's 730 contour, you'll find a very good match. So it's what's contoured here matches to a fair degree what's not contoured, leaves a different impression, but there's still a very strong north-south slope component to the system.

Okay, to go on with the three remaining studies, the site saturated zone, there's the reactive tracer tests at the C-well site to be conducted by Las Alamos. Again, this is primarily a methods development area. The secondary purpose is to characterize reactive transport at this single site. At this point within the SCP and the study plans, we come to a decision point; that's how to take what we've learned at the C-well complex and extend it throughout the site.

This decision point, as described in the SCP, has one of two courses of action. If we find a single well method that seems appropriate, then the course of action is to take this method and apply it to the various wells throughout Yucca Mountain. If we don't find a single well site, then we go out and drill a second tracer test site. Some of the early stuff talks about the southern tracer test. We're not sure where that would be located. In reality, those are kind of two end members. We may end up wanting to drill a second well paired with an existing well and possibly do two well sites. It's too early in the process to tell exactly where these final two activities go, and it's hard to tell with Scenario A what sort of timing we're going to have for these two activities.

To go on to some other saturated zone studies, the Solitario Canyon Fault study is to investigate the moderate hydraulic gradient that exists to the west of Yucca Mountain. This is a future study that will determine if the Solitario Canyon fault is a conduit or barrier to flow or if more likely it's a conduit in places and a barrier in places, just sort of characterize that. As an indirect result of this study, we will learn something about the hydraulic characteristics of the Ghost Dance Fault, and less likely, but there's still a potential, we may learn something about the hydraulic characteristics of other faults throughout the site.

There's a whole study plan on saturated zone hydrochemistry. There are various things that are going to be done in this study plan. The first is the assessment of the existing regional hydrochemical data set. This assessment is essentially completed. This data set has been compiled and looked at. It's completed in the sense that it's been compiled. This sort of an assessment is never completed in the sense that each time we begin to use this data, we need to look at it whether it's useful for the particular purpose that we have in mind.

We're going to characterize the regional hydrochemistry of the Death Valley region, looking at potential flow paths, do a detailed characterization of the site hydrochemistry, evaluate the interface between the unsaturated zone and the saturated zone, and try to provide some additional information on fluxes across that boundary. And this site hydrochemistry study will provide some of the detailed data needed for retardation studies.

And then on the site saturated zone studies, there's also modeling activities there which I'm going to try to cover next.

First of all, I'd like to talk about why do we even need to construct a model. The first two bullets on this diagram are the fairly obvious ones. We need to construct a model to determine whether the site can meet the regulations, do the kinds of performance assessment calculations that are required. Obviously, you need a model for this kind of work.

The other bullets are not so obvious, but these are reasons to construct a model, whether or not we need a model for performance assessment. In any scientific study, we need to construct some sort of a model, maybe not as formal as we are constructing here, but we need to put our data together and synthesize it somehow. When we're doing that, we're actually constructing a model.

We need to look to see if the data sets are internally consistent. If we understand the processes, there's nothing like a model to tell you how little you know about the system. Very frequently, you think you know an awful lot about the system until you put all of your data together and put it through a model and your model tells you that the data are not internally consistent, that there are areas where you really don't understand what processes are going on.

And, finally, we need models to tell us if our current data set is adequate, and if further data is needed, where is it needed.

In this program, as most programs, we have a bad habit of thinking that there is but one model, and we use the word model in a very broad sense, in the same way we use the word data. It's difficult to tie down what we're really talking about. So here, I'm trying to just talk about some of the various kinds of models that we have, and that they are all different kinds of models. They all have their place. We have geologic models, climate models, flow and transport models, and performance assessment models. We're not going to have just one model of the system. It wouldn't be appropriate to have just one model. So when we start talking about models, we need to talk about which model we're talking about, what kind of model.

For saturated zone flow and transport modeling, we need to talk about a number of issues. Is the flow in fractures or can it be represented as a porous media equivalent. We know obviously at a very small scale it's fracture dominated flow. We also know at a very large scale, that porous media equivalent has worked. The site scale is kind of an in between scale. We're not sure whether it acts more as discrete flow or whether we can represent that as porous media equivalent.

What sort of discrete features are we going to have to include in our model? Only important faults, or are we going to have to include minor features? These are issues yet to be resolved. Is the flow predominantly two dimension or do we need to include the third dimension? Obviously, it's a three dimensional system. Some of our previous modeling has been done at two dimensions, and we believe that we need to move into three dimensional modeling.

Is the flow in steady state or are we in some sort of a transient phase? Again, this depends on the time scale. Our data indicates that over the scale of the study so far, that it looks like the system is very steady. There are no long-term trends. When we start looking at longer periods of time, this may very well break down, probably will break down if you look at Ike Winograd's paper.

What are the models' boundaries? We're only interested in detail at the site scale, although our site scale boundaries are probably going--our site scale model boundaries are probably going to have to reach out very far to get away from boundary effects, and the old question of what's the appropriate level of detail.

I've tried to depict here how some of these models fit together. Because we're talking about a site saturated zone process model, it seems to be the center of the universe. I think wherever we're standing, we believe that that's the center of the universe. That's what this diagram looks like. I don't believe that the site saturated zone model really is the center of the universe.

I've tried to put some of the links in this model together. The darker arrows are stronger links, so there are strong links between the site saturated zone process model and the PA model, as is there a strong link between the site unsaturated zone process model and the PA model. There's an obvious link between the saturated zone process model and the unsaturated zone process model, although I've shown this as a fairly weak link because the flux through the unsaturated zone is small compared to the flux through the saturated zone, and so a change in that flux's model and the UZ model is not going to change the saturated zone model to any great degree.

By the same reasoning, the change in the position of the water table probably is not going to impact the unsaturated zone model to a major degree.

Now, let's go on to what I term a hydrology potpourri. We're not going to be looking at the full saturated zone program. We're going to be getting just bits and pieces of the program that seem to be interesting at this point. It's not going to look like we have a terribly integrated program, because we are talking about bits and pieces.

As Russ indicated, if we wanted to spend a week on it, we could talk about all of the pieces. We'd bore you to death with it within a couple of days I'm sure. So we're trying to hit just some of the most interesting pieces.

First of all, Kenzi Karasaki from Lawrence Berkeley Laboratory is going to be talking about the TRINET fracture model. I want to emphasize that this is just a piece of the modeling. I should have pointed out on the previous slide how this fits into our saturated zone modeling, but it's a piece of the modeling picture, a very important piece. We're going to get a lot of understanding from it, but again it's just a piece of it.

Bill Steinkampf is then going to talk about the hydrochemical data that we've recently acquired from the unsaturated zone, perched water in UZ-14. This is just a piece of the hydrochemistry data, and in this particular case, we're talking about perched water and not the main saturated zone. But I think we have some interesting information here. I get real tired of hearing about plans and want to talk about some real data, so that's what Bill is going to do.

Then Zell Peterman is going to follow up on isotopes. This is a piece of the hydrochemistry picture, but again he has results and not plans, so he can talk about some real data.

Thank you.

DR. DOMENICO: Does the Board have any questions? Don?

DR. LANGMUIR: Don Langmuir, Board.

Dick, you showed us a revised piezometric map which suggested that--or showed clearly that you had parallelism between the piezometric contours and the Solitario Canyon fault. I wonder if you'd comment on what you think the significance of this revision is and how the subparallelism tied into your concept of groundwater flow, what you think it is right now around Yucca Mountain.

MR. LUCKEY: Well, let me start by saying it's parallel to the Solitario Canyon fault, as you pointed out. It's also parallel to Fortymile Wash. So these are probably two significant boundaries in the system. It's certainly not parallel, as Pat pointed out, to the large hydraulic gradient to the north.

I guess I see this trying to tell us, and I'm not sure that this is the correct information, but I see a certain amount of leakage across the Solitario Canyon fault moving eastward, and then Fortymile Wash being a drain for the system. This is perhaps telling us that the flux from the north across the large hydraulic gradient is small compared with the flux across the moderate hydraulic gradient.

I think this is a fairly well constrained map in the area that is mapped in this small hydraulic gradient. We probably need to look at a somewhat bigger picture to get the full picture of what's happening.

I would also caution that although the potentiometric contours have a very strong eastward component, that does not necessarily mean that flow is eastward. Because we have dominant faults and fractures that probably have a north-south orientation, we probably have a strong degree of anisotropy here. And so while Fortymile Wash may be the low in the potentiometric surface, there may be a much more southerly component of flow only because we have much higher permeability in the south.

Much of this is just speculation on my part. We have a lot of site characterization work to do. But that's how I interpret this map.

DR. LANGMUIR: Dick, you mentioned that reactive and conservative tracers were going to be used, and I was involved in that a long time ago and I'm curious which specific tracers you have in mind for the reactive choices and the conservative choices.

MR. LUCKEY: Right now, we have a series of tracers, a half a dozen conservative tracers, and I'm not sure that I could tell you all of the tracers, but the bromides, there's benzoic acid, which is a whole family of tracers that will be our conservative tracers that we use initially.

For the reactive tracers, the one that I'm most familiar with is the Las Alamos study, but they're going to be using the polystyrene microspheres to try to look at how colloids might move through the system. And I think for the other reactive tracers, maybe Russ can help me out if you know what they are, Russ. I know that Las Alamos is trying to select those. This is a study that is more than a year in the future, and so I don't know that they have selected them at this point. Lithium bromide is one of the conservative traces.

MR. PATTERSON: That's a reactive tracer.

MR. LUCKEY: Oh, lithium is reactive.

DR. DOMENICO: Dick, I have a few questions. You mentioned these activities, mostly hydraulic testing and tracer testing. How much of these activities are truly active today on the site? With the budget constraints and everything else, just how much is really going on on the saturated zone hydrogeology?

MR. LUCKEY: We've started work at the C-well complex more than a year ago. We're not actively pumping, but we do have packers and transducers in the three C-wells at the C-well complex and are collecting background data. We're currently in the process of constructing the discharge line, the spreading pit, acquiring all of the electrical equipment and the pumps needed to pump the C-wells. So there's not any water coming out of the ground at this point. I think we're currently looking in the mid to late summer to actually turn on the pumps. We hoped to be pumping by this point, but everything has gone slower than anticipated.

The other thing that is on the horizon but has not yet been done is we're looking at a cleaning out of the WT wells as a precursor to doing detailed hydrochemical characterization. The WT holes were drilled and left, and the program got bogged down before we actually went back and cleaned out all the drilling fluids from these holes and did detailed hydrochemical sampling. So this is something that we believe we can do in the near future. While we're cleaning out these holes, we're going to be doing a single well hydraulic test and at least get some information on the hydraulic characteristics of the system during this. But again, this is something that is late this year, early next year, depending on availability of resources.

DR. DOMENICO: The last question is I know the M&O is trying to get all the modeling under one roof largely through Intera. Does the USGS directly plan to do any modeling on this site? Is there any overlap with what is going on with the people at Intera or Sandia or anywhere else?

MR. LUCKEY: Well, the USGS is actively doing modeling on this site right now. We have--you've all heard about John Czarnecki's two dimensional model that's been around several years.

DR. DOMENICO: Several years, yeah.

MR. LUCKEY: I think since 1985, it was published. It's really been around longer than that. We now have a three dimensional model that covers the entire Death Valley region. It has, if I recall, something in the order of 76,000 actives nodes in that model. It's a fully three dimensional model. It goes about 6 kilometers deep to the base of the flow system. This is something that is going on right now. This is giving us our boundary conditions for our site model. We're very actively compiling the information that we need for our site model. That's part of what this revised potentiometric surface is all about. The previous potentiometric surface map was certainly not adequate for modeling at the scale of the site.

So, yes, we are doing modeling. We're doing modeling very aggressively. We are interacting with Intera and Sandia on performance assessment, sharing information with them so our models are somewhat consistent with each other. Most of that is done on a somewhat informal basis by reviewing each other's models and reviewing each other's reports. The Survey did a very comprehensive review of the Sandia saturated zone performance assessment modeling.

As I tried to point out on the one slide, whether or not performance assessment even existed, the Survey would have to do modeling. You can't collect data and pass it off to somebody else to put it in a model and have any meaningful data collection or any meaningful modeling. The person collecting the data has to do the modeling to be able to see if the data collection is appropriate. And once we do our modeling, our process modeling, then we can pass those sort of concepts off to PA and they can incorporate it into their models. But you can't just have one model. That was a major point that I was trying to make.

DR. DOMENICO: Does Bill Nelson have anything to say about that?

MR. NELSON: I believe the way that we have been functioning, and it's been functioning very well, is that the, for example, on the saturated, the USGS has most of the characterization responsibilities. The synthesis of those models, testing of those, documentation and otherwise, and then as they're completed, then there's the transfer that we'll interface with PA and go on. And that's worked out very well in the unsaturated zone. I appreciate the cooperation that we're getting here on the saturated zone. Quite frankly, the saturated zone, as Dick has pointed out, hasn't been analyzed to the extent there's been a period of time when it wasn't considered to the same degree. We're further behind, but we're making good progress.

I'd mention also in the transport area, that the responsibility for that is largely in the unsaturated zone has been with LANL, and in a similar way, it's the idea that the models are--the characterization occurs, the models are built, they're tested, they're documented, and then there's a transfer to PA. And it's working there very well.

DR. DOMENICO: I think you should introduce your group--whoever is next. You have three presenters, is that what I heard?

MR. LUCKEY: I'll start out by introducing Kenzi Karasaki from Lawrence Berkeley Laboratories, who will talk about some of the modeling that they have done, and then I'll let Kenzi introduce the next speaker and we can go on.

DR. DOMENICO: Okay. If we have any questions, we can come back to you or the three people following you. Thank you very much.

We will have a break after this speaker. We're pretty much getting back to schedule.

MR. KARASAKI: Working on Yucca Mountain project gives you a lot of uncertainties, and you work with a lot of uncertainties. I flew in from San Francisco, the first plane that left San Francisco, first thing in the morning. I arrived in one piece, but my luggage didn't arrive and I'm trying to catch the 11 o'clock, or a little after 11 o'clock flight out of here--out of San Francisco at 1 o'clock to Japan, and I'm uncertain if my luggage arrives there. Then I'll be without all those changes.

So I'm going to talk about saturated zone fracture flow and transport modeling. But this title is a little bit misleading, and Dick put it very nicely that modeling really cannot stand by itself. And my talk basically I would like to stress the point or talk about how modeling cannot stand by itself and has to be approached otherwise. People look at modeling just numerically and look at it as a dependent entity, but we have a little different approach.

This is my talk outline today; talk about fracture flow, what we know and what the problems are, and I would like to outline our approach to solving and attacking these difficulties. And also we have a prototype site in Raymond, California to put all these together as we progress in the C-holes and other saturated zone activities.

I'll start with fracture flow and transport. We know flow of fracture is discontinuous, heterogeneous, highly localized transmissivities, highly concentrated, and flow is usually channelized.

This is from a stripa report. We worked in stripa project as well. It is reported 94 per cent of our entire transmissivity observed in bore holes were concentrated in less than 4 per cent of length in bore hole.

Actually, if you look at the actual inflow length, it's much smaller than 4 per cent. And also from a preliminary analysis of past C-hole--here's the relative transmissivity distribution profile, and you can tell that again transmissivities are highly localized, concentrated. That's the characteristic of fracture flow.

And, again, people may have expected that I will be talking about fracture network modeling. Yes, we do fracture network modeling and we have models capable of doing that, but we have problems with that approach alone, having a network model standing by itself.

Too much emphasis on geometry; we have to put in size, distribution, orientation, density, shape, et cetera, and one would fit the function to those parameters and regurgitate or reproduce, randomize, and create fracture network. But the data for those geometry is very hard to obtain, and also data may not be relevant at all to actual flow and transport, because data from, again, classical network modeling sense, one would collect geometric data observed in bore holes that may not, again, have much to do with real hydrology.

So our approach here is actually use the model, forward model, which is flow and transport code, and data which we report, hydrologic data obtained by hydraulic and tracer tests, and use inversion scheme which is part of numerical code and analysis process. And our final objective is a predictive model that can predict hydrology and transport of the site.

And, again, with a model that heavily relies on geometric data alone, would not be able to--you may be lucky to come up with a model that can do a prediction, but we need to develop a model that is coherent and that explains data, hydraulic data, that we observe through direct testing.

And throughout my talk, I would walk you through what forward model we use and what kind of data, how we collect, and the inversion scheme, and also I will introduce to you the prototype that Dick Luckey briefly mentioned. Again, I would emphasize that the data we use to condition or--are hydraulic test data or I call it direct, done by direct measurements, tracer test data and other direct measurements include flow meter surveys, flow surveys within bore holes, again actual direct measurement of flow. And of course there are indirect measurements like fracture geometry, pavement maps, bore hole T.V. camera, televiewers and geophysical measurements such as seismic profiling.

They are useful, but again in terms of using it to condition a hydrologic model, we have to use it as a secondary information and direct data should be always that that conditions the model.

And the forward model part, we have developed TRINET. It's a three dimension model, but each element is a one dimensional line conduit mimicking channel flow. And because of high heterogeneity and high stress conditions in the tracer test, we expect a large variation in numbers, and the model allows for modeling shock front, shock wave front, to a very smooth dispersive front.

So a model would handle a three dimensional complicated network input like this. But, again, these inputs have to be conditioned and have to come from hydraulic test data and have to explain hydraulic and tracer test data we observe.

Now, how we do inversion scheme, inversions we can do, there are different ways of inverting and optimizing, coming up with the model that satisfies your criteria. Our approach is to use simulated annealing. Basically, to put it simply, if you are a hiker on a mountain, lost, you want to go to the bottom of the valley, and if you use gradient base search, which is a common way of searching, optimizing the function, what you do is you go downgrade all the time. You search--you find the downgrade all the time. But then what happens is you can get caught in a local valley that gets you no where. And simulated annealing lets you basically hop out of local minimum in search of global minimum.

And we can iterate on fracture patterns, single fracture or fracture patterns, fracture structure or cluster fractures, and again parameters associated with those. And the interpretation of the results, again, all these models will explain or satisfy your input hydrologic data, but still the nature of it is that you will never have enough data to uniquely construct the model. You will have multiple realizations and interpreting multiple results, we can do ensemble mean analysis where we can look at the backbone of those multiple realizations.

Simple, I can give you a cartoon of how simulated annealing or this inversion process works. Let's treat this as unknown fracture system. We drill several wells and observe pressure tangents. We do pressure tests and use this data and the target is, one would start with a model that doesn't have any prior conceptual model in it, and start with this model, make prediction. If it's different from this hydrologic behavior, you keep changing, reconfiguring this model until it fits perfectly. Actually this is synthetic data, but these bullets and curves are data and predictions. And what you come up with is this representation of this real fracture network that actually hydrologically conditioned and explains hydrology that goes on in this fracture network.

And to actually put the scheme and approach that I have outlined, we have a prototype site supported by the international program to do the prototyping of equipment as well as methodology and the modeling, which is the modeling and analysis that I just outlined, and also of course we work with the AECL and exchange knowledge. And this is at Raymond. We drilled wells in a fractured rock, nine wells in a radiating pattern like this to address scale issues as well as directional issues, heterogeneity. But again, the main purpose is to do prototyping.

USGS is using this packer string, the same make, same style, same model packer string that's being used in C-holes and will be more used in C-holes, are being tested at this site, and we have found our efforts resulted in design changes, new logistics. It's being very productive. We have developed automated tracer sampler. It does pretty much everything automatically, controlled by a PC, and the only thing that it doesn't do, and we're working on it, is to cap the bottles back, put the caps on the bottles back on themselves. And this data logger works on a 486 PC and inline fluorometer that gives us a real time count of our tracer.

We have again software that gives you graphics, graphics using interface, and again we can control test parameters from this computer. And this computer program and the data system is going to be used in C-holes, again as a prototyping.

I'll give you one example of what I mean, why we need this process and why this model doesn't stand alone, and the traditional approach in doing modeling for well test analysis is really limited. Here, in nine bore holes, we have total of about 30 observation points, packer zones. This is about the same number that's going to be observed in C-holes. C-holes are three wells, but there are going to be ten monitor zones. So there are about 30 intervals. Once you do pump tests, one pump test will give you 30 transient pumps, and traditional approach of using transparency and trying to do a type of--by hand doesn't work. And, again, as you can see, the data itself isn't fully mimicking nice--or porous medium flow.

We have done one tracer test. Again, this is all conservative tracer test, Las Alamos, and we put in Fluorescein, deuterium and bromide, and these are all supposedly conservative tracers. Actually, they arrived differently and we are very puzzled, but if these are truly conservative, their normalized arrival curve should lie on top of each other.

We have currently a conceptual model of the site and, again, this approach is going to happen parallel and a little bit in the delayed state in C-holes in Yucca Mountain. This is a prototype site and this is our best knowledge so far of this. And the issues here, again, together with actual prototyping, the relationship between fracture dispersivity and fracture geometry, averaging, we know we cannot test hydrologically throughout the mountain. We have to do some scale up averaging and also we have a concentrated effort at C-holes. That has, again, all the parameters will not go into our larger model, and that has to be simplified.

And again, we are looking at the way the transient curves actually do deviate from traditional or ideal curves, and that is telling you something, and we are working on how we get the information out of it, such as using inversion method. That is the automated way of applying the information out of transient curves. And another big thing that we are realizing is that there are test artifacts, and again when you drill bore holes in tight fractured rock, you create a big tunnel or a big tube, a hydraulic tube, that actually may interfere or give you a wrong signal or misleading signal of what the permeability of the rock is. Same thing with the storage. If the rock is very tight, your packed off zone may have more storage than the rock, original rock itself.

Tracer tests; when we deliver a tracer, we have to be really careful about the hydraulics in that injection zone. We want to know how the rock spreads contaminants and radionuclides, and we do tracer tests. But what happens is when you put tracers in the packed off zone, the bore hole hydraulics within the bore hole within the packed off zone can often influence the outcome of the tracer arrival that has nothing to do with the rock itself. So we have to be very careful about it and we are working on it, again, at this prototype site.

And, finally, we are addressing the issue of how much is enough. We have, again, at this small prototype site, nine bore holes, but again where do we know comfortably enough that we can do--we can say we have done enough, now it's about time we come up with a predictive model that can do prediction for the radionuclides.

So these are the issues we are addressing at this prototype site. And, again, to conclude, I just didn't explain to you what fracture model is, what our numerical fracture code is. It really cannot stand by itself. It has to really tightly integrate with the actual field testing, field testing design, result, analysis and feedback. And, again, at this prototype site, we're doing that and, again, at C-holes as soon as the pumping starts, we will be using all this knowledge that we have accumulated at this site.

And I would like to conclude, and actually a little bit after 11:00, I'd like to be excused. But I'm available for questions.

DR. DOMENICO: Thank you, Kenzi. Does the Board have any questions?

DR. LANGMUIR: I'm intrigued by your plot showing three conservative tracers that all behave differently. You mentioned that this behavior, the fact that they differ may reflect, did I hear you right, artifacts in the bore hole itself? Because if it's not that, then clearly you've got problems identifying what's going on.

MR. KARASAKI: Yes, we are in the process of looking at and trying to decipher what really happened here. One report I have seen from AECL says the Fluorescein actually multiplies or reacts and fluoresces more with the rock minerals or the water chemistry. And also deuterium can again--I originally thought that these are really common, and especially deuterium is like water, it's like water, it shouldn't react, but our isotope geochemist told me there's a possibility that it may react. But bore hole hydraulics is another thing.

We did mix on the surface very well, very nicely, and then injected it, and we tried to keep it agitated within the bore hole to keep it evenly mixed. But, again, the chemical inertness of deuterium may have just resulted in sinking down to the bottom of the zone, whereas Fluorescein may have been more active in mixing. And at this point, we don't know; this test was actually--again, when we do tracer tests, at least among my colleagues or the people in my field admit that when we do tracer tests, we have to do two, one to basically plan the concentration, roughly know the arrival time, and then do it again. This test was a very preliminary test, which tracers we can use, how much, how long it's going to take to arrive and all that scoping.

But, yes, it resulted in this interesting--and another possibility is actually this is in the order of molecular size, and even though I really don't believe in a short time frame like this, in ten hours, and the arrival time itself didn't change, it should be retarded, if it is diffusion into rock, matrix of some sort. So we are kicking the possibility around, but we haven't really pinpointed what.

DR. DOMENICO: Any other questions from the board or the staff?

DR. CORDING: You're describing some future tests in drill holes in the Yucca Mountain block; is that correct? Is that the C-site you're talking about?

MR. KARASAKI: I did probably mention it, but it was more from Dick Luckey. But what I explained here, the actual data was collected at Raymond prototype site, but we used the same equipment and data logger, same logistics, everything that's going to be used in C-holes in Yucca Mountain, yes.

DR. CORDING: Does part of these studies or your work or related work continue into the drill holes in the ESF where you're looking at flow through or across fault zones or other things that you discover there?

MR. KARASAKI: I would like to be contributing to that effort. That is, as I understand it, is more UZ effort, but I have been talking with PI in doing crosshole pneumatic tests, and he mentioned that he would like to use the data logger and program as is for his effort in ESF.

DR. BARNARD: Bill Barnard, Board Staff. I have a question for Russ.

One of your slides you showed that the interface of the saturated zone program with the other characterization programs. I was wondering if you could describe how that interface takes place? Do you have monthly meetings with other programs?

MR. PATTERSON: No, actually those are pretty informal sort of interfaces. Basically from the DOE standpoint, the DOE WBS managers, of course, are all located in close proximity. The WBS managers for those tests are also--were all within the scientific programs and we have our regular staff meetings and we talk about different things that are going on. There's also interfaces--I think there has been--

maybe I idealized it a little bit in that slide, maybe there hasn't been quite as much interaction as there should be, and I think that's being recognized and we're working to improve on that.

DR. DOMENICO: It's exactly 10:45. I think we'll have our 15 minute break. We're right on schedule and we'll come back and have another 40 minutes shared by two people, and then we'll adjourn for lunch. So we'll be back here at 11 o'clock.

(Whereupon, a 15 minute recess was taken.)

DR. DOMENICO: My understanding is the next 40 minutes will be shared by Zell Peterman and William Steinkampf. Is that correct?

MR. STEINKAMPF: Correct.

DR. DOMENICO: And that puts us on schedule. So the hole has been filled, so go ahead.

MR. STEINKAMPF: I'm Bill Steinkampf from the USGS, and my responsibility is saturated zone groundwater chemistry, description at the site and actually at the regional scale as appropriate. And what I'll do is I'll give you a quick run through of some of the things that we have done, or I have done to date. To paraphrase a George Thoroughgood song, I work alone. I don't have any staff. He drank alone. I also drink alone.

The first thing I'll tell you about is Dick alluded to the data set that we compiled that's everything we could find in Survey, EPA, relevant state and university publications for the hydrochemistry at the site. This has been put together. We took a quick look at it. There were something like 4,700 records that we pared down after we looked at errors, obvious errors, site errors, redundancies. I had a student do this, and it's in review now and when it gets through the USGS review and approval process, the data set is soon to be released. It provides a base of information for multiple uses. But as Dick pointed out, there are some strong caveats that we attached to it, and every use is not appropriate to the entire data set.

On a regional scale, one of the things that I felt that I could do with the funding that was available was to try to contribute to an understanding of the boundaries of the regional system. So because of the Park Service's interest in the ongoings at Yucca Mountain relative to the hydrologic system on a regional scale, they were nice enough to give me access to some numerous sites, some of which we have sampled within the monument.

If we look here, this is Yucca Mountain, Nevada-California state line, and this is Death Valley here. And these are the springs, essentially from the southern end of the monument up to the northern end of the monument that have been sampled. The idea here is to try to get some insight as to the extent to which fluid in the system as we know it in this area is able to move or does indeed move through, beneath or around the Funeral Mountains Grapevine Range and on down in here, the Black Mountains.

We know that there is a general direction of ground water moving in this direction to the south, and there's a large playa here that's obviously a significant discharge area for the system. And with some insight for the chemistry, hopefully we can get some idea as to a quantification of the relative discharge.

This is just a quick and dirty Piper diagram. The data is not all in from those sites. We're still waiting for a few bits and pieces, and we can see that there is a significant amount of variation, depending on where you are in the system. On a Piper diagram, generally volcanic rocks, things like the tuffs, when they interact with water, tend to yield groundwaters that are in this area. And the evolution tends to be from generally this area down toward a more sodic type water. These represent relative concentrations.

Limestone waters tend to be in this area, and things like evaporite waters and sea water tend to be over in this area. And, indeed, Salt Creek here is a discharge site out in the middle of the valley floor. Concentrations are relatively high and it has evolved to a more evaporitic looking water.

We've got a lot of isotopic data that is going to be derived from these samples, and I would suspect that, compiled with the information that we can assemble about the site geology for the region, which is very complex, will permit us to make some statements that will provide hopefully some information to the regional modelers who are again providing the boundaries for the site model.

In addition to--that's it for the regional work. There's also some opportunistic work that goes on as bore holes are encountered and become available through mining contacts.

On the site scale, we have information that currently exists and which dates from most recently about 1984, '85. These are existing bore holes in the site area and vicinity. I always kind of thought this looked more like a poorly trimmed strip sirloin than a pork chop, but that's neither here nor there. You can see where we are for frame of reference, J-13, J-12, J-11, and then out onto the area.

The data base that exists is for most of the sites that you see here, and is represented here on the Piper diagram, and you can see that lo and behold, there's no surprise. Everything kind of fits down here in this evolutionary path from less sodic to more sodic, bicarbonate waters. You see this everywhere in the world. There's nothing new here.

In looking at the data, however, at this data base, which is published and compiled in several locations, I came across some things that disturbed me a bit, one of which was the lithium concentrations that were reported. And if you look at the bar graph here, you'll see that there's a significant range of concentration variation. The J-holes, J-12 and J-13, here's the C-holes that Kenzi was talking about, and various other holes. VH-1 is off the site over in this area here. You can see a huge variation. P-1 is the Paleozoic, the single Paleozoic hole that we have at the site, one that goes through the tufts into the carbonates.

The purple line that is on here is the concentration at J-12 and J-13. J-13 is pumped quite a bit. It is probably the best guess for a representative water chemistry at the site. A lot of water in and out of that hole, and you'll notice that everything is above the J-13 concentration, except for H-1 and this little green one I'll get to in a bit. The green one is not the data that's been released in the past. And the reason for this is that lithium bromide and/or lithium chloride were used as drilling tracers in all these holes except H-1. Tracers came in after H-1 was drilled.

So it's clear that this data, from samples from these bore holes which represent intervals that range from about 150 to about 1,800 meters of saturated bore hole, are not indicative of the background chemistry at the site, which pointed out to me that we've got to go in and much more closely sample these holes as is possible, and new holes or additional sampling that will take place will address discretization of much smaller intervals as is appropriate or possible to try to give us as much vertical variation, as well as spacial variation, in the groundwater chemistry.

The green one here is data that's from UZ-14. UZ-14 is this uppermost little circle, and this other one that's highlighted here is G-1. G-1 is a geologic hole that was cored and drilled about 1980, '79, '80, '81, and during the course of drilling, something on the order of about 8 million liters of fluid were lost. This drilling fluid was a water based bentonite mud, I think largely in the upper part of the hole. Then they used detergent, air foam mix, and they also used a polymer based additive for drilling. So we've got three gross components of fluid loss.

UZ-14 is about 100 feet away from UZ-1, which was drilled in 1983. This was to be an unsaturated zone hole. Unfortunately, some 400, 500 feet above the water table, where the water table was anticipated, fluids were encountered in UZ-1, drilling was subsequently stopped, samples were collected and it was determined that a drilling fluid component was present in the fluids that were sampled.

In 1993, we revisited the site and started drilling

UZ-14, as I said, about 100 feet away from the site of UZ-1. The intent was to go down into the water table some 10, 20 meters. Lo and behold, within about--with a meter of the elevation at which fluid was encountered in UZ-1, fluid was encountered in UZ-14. Again we sampled.

Here's the chronology of what transpired, the altitude of the respective zones. What happened was fluid was encountered in the course of drilling about a 3 meter interval here. Bailer samples were collected, Samples A, A-1 and A-2. Another 3 meters or so was drilled, cored. Bailer Sample B was collected from that 6 meter interval, and then another 3 meters or so and we collected Sample C, and then we conducted pumping tests, 1, 2, 3, 4, and you see the samples that ensued thereon.

This indicates the extent to which this hole was subsequently cemented. Initially, the cementing program went in to try to seal this thing off here so that it could continue toward the objective of developing it as an unsaturated zone hole. Fluid was noted to be invading the hole, and an additional substantial cementing program. Again, this is not to scale. I just wanted to indicate that there is a big cement plug in there that was eventually reamed out essentially to the bore hole wall, and then drilling and reaming went on. Reaming stopped here and coring only has been going on here.

This is the approximate water level where the initial fluid was found, this initial perched fluid. This was another--the top of what appeared to be another saturated interval which was encountered. Drilling is ongoing as we speak at this hole.

I'll talk to you briefly about the samples that you can see, A-2, B, C and 4-1. Sample 4-1 was collected after about 6,000 gallons of total pumpage. Test 1 was about a gallon a minute. We found out that the piezometer to monitor the water levels was plugged. Started it over with Test 2, conducted a test for a day or two, something like that. Test 3 we upped from one gallon a minute to two gallons a minute, and found that the water levels were dropping. In this interval here, this was the base of the testing when we had about ten meters or so of saturated thickness, two gallons a minute, pulled it down too quickly, so we went back to one gallon a minute with pumping Test 4.

I collected a sample in the middle of 4, toward the end, I guess, after things had stabilized to my satisfaction, and I believe that Sample 4-2 represents a Bailer sample that was collected, a suite of Bailer samples that were collected after the pumping ceased.

Let's look at A, B and C first. And the first thing that was quite striking was the total organic carbon concentration. Total organic carbon concentration in J-13 is on the order of two-tenths of a milligram per liter. It could be .4 or it could be .3, could be .1, but something like about .2, and here we found 10, 12 and 490. 490 is a big number. I think you could fertilize with that, I'm not sure. That was quite an eye opener. I collected three samples during the pumping test and found that the three splits gave me numbers that were .4, .4 and .5. 4 is a magnitude lower than the lower most zone.

I'd point out that the water from the pumping test appeared to be largely coming from the central plug, roughly the center part of that 10 meter interval, corresponding roughly to where B was collected. But, again, these were bailed samples. This is a pumped sample. D is just a grab sample that was collected about three weeks ago, and the fluids that were coming into the hole during the course of drilling down to that depth had elevated pH's which we attributed to the cementing programs, the numerous cementing programs that went on.

Again, these three bailed samples, we did an extraction for an ethoxylated octylphenol component of the polymer based drilling mud, and we got concentrations, as you can see, somewhere between 1 and 3 for the uppermost sample, a tracer were comfortably detected in the lower most, and nothing in the sample with the highest TOC. I can contribute nothing to an explanation therein. And we can see that the pH's were not remarkable from those samples. There were no lithium analyses.

From the pumping test samples, pH stabilized around almost exactly at 8, some halfway into the pumping test. The lithium concentration is notable because it's half that of J-13, which would suggest two things; one, that we don't have just the drilling fluid component, because if it was just J-13 water which was used to make up the drilling fluid, we should see something that would be a little closer to the lithium concentration there. Yet we have definite indication of a contaminant from the G-1 drilling. So it would appear to me, based on that, that we have some sort of a mixed system, something that reflects a natural fluid as well as a contaminant. I don't think that we can quantify them at this state.

The bacteria information was quite intriguing. I stopped and talked to some people at the Harry Reed Center and picked up some sterile vials, and in the course of sampling from the pumping test, filled the vials from the UZ-14. On the way back in, I stopped the same day, within a matter of hours, at J-13 and filled those, and you can see that we've got at least a three order of magnitude difference in the concentration or the abundance of bacteria. This is all I can say about it right now, other than the bacteria do not resemble the populations that are present in springs in the Death Valley system right now.

Those look like windblown type organisms, and these organisms do not look like that. The only other thing I can say about it is that they do not look like someone's dirty hands. They do not appear to be an anthropogenic contaminant. Perhaps that explains to some extent the high TOC that was picked up in the lower part of the system. I don't know. Subsequent samples that we collect will include some sort of a bacterial or microbial sample.

I've got some C-14 data for, again, the suite of samples that were collected, pumping Test 1, 2 and 4. C-14 samples were not collected from pumping Test 3. You can see the range. J-13 is almost identical to the pumping Test 2 samples, about 29 per cent, something like that. I just put this up for general information. This doesn't cast a great deal of light on what has gone on. But, again, keep in mind that these were Bailer samples, they're far from being the most desirable type that we collect, but this was an opportunistic and expedient effort.

And that shoots my 15 minutes. I've got about four or five minutes. I think I can take questions.

DR. LANGMUIR: Don Langmuir, Board. Bill, we talked about this in Denver a couple months ago and I asked a question then and I'll ask it again. Has anybody looked at the suspended materials available that you collect on a filter? Since we're really trying to identify drilling mud specifically here, it's clearly suspended stuff, and at the time, I didn't get an answer that you analyzed those materials or really focused on them. And as an adjunct to this whole issue of mixing with drilling mud versus J-13 water and the perched issue, I'm still curious whether the suspended matter was collected and what it comprised.

MR. STEINKAMPF: Okay, we've got the filter elements. All the filtering that I did is preserved. And recently, I got some stuff from the bottom of J-13, again some bailed samples that are collected for another reason. And we're in the process of looking at the bulk chemistry as well as the mineralology of the entrained materials--I hesitate to call them suspended--from the bottom of the hole. Now, this is below the perched zone. This is some 400 feet below, but in the interval wherein we again encountered what appears to saturation.

The filters--when I collected the samples from the pumping test, there was essentially nothing that came out other than your friend iron, because the filter oranged up after a period of aging in the sunlight as we ran about 15 liters of water through, and that was 0.1 microns.

The suspended material that we would look at, or will look at and are looking at, is largely that which is drilling debris that's entrained in a Bailer sample. So I think it's going to be--again, remember that the water based, bentonite based mud was used largely, if not entirely in the uppermost section of the hole, and then they went to an air foam and this polymer, and they went back and forth between detergent and polymer, whatever worked. Beyond that, I can't provide you with--

DR. LANGMUIR: We're obviously very interested, and the Board would be, in the perched water issue and how it all ties together here. Maybe I'm anticipating what Zell is going to tell us and how the isotope ties into the detective work on that.

MR. PETERMAN: Can I just comment on the suspended material? The samples we collected or were collected for us, we prefer raw unfiltered samples and do the filtration in the laboratory. One of the samples of the P-1 test was especially gunky and we separated that material, and it was very heavy metal-rich. It had like 2,000 parts per million lead and 2,000 parts per million zinc, plus copper and molybdenum, and it looked very much like a material that was called liquid O-ring that we analyzed several years ago and supposedly was abandoned or not used any more as a pipe dope.

We analyzed the pipe dope that they're supposedly using and it's some sort of environmentally correct material, and it has no heavy metals in it at all. So this is a mystery. We went back and looked at one of the bailed samples which was also very turbid, and we saw a lot of rock, ground up rock, but we also saw significantly elevated concentrations of lead and zinc.

So we're puzzled, we don't know whether--you know, there are several possibilities. Maybe the environmentally correct pipe dope doesn't work very good and the drillers prefer the old stuff, maybe the drill stem was contaminated from the earlier pipe dope. We just don't know. But there is some strange solid material in the hole that's been added.

MR. STEINKAMPF: And to pose a problem there, this sample had essentially no lead and zinc in it. The pumped sample had lead and zinc at the detection limit, you know, it was plus or minus the value reported, plus or minus one microgram. So I don't know how that fits.

DR. DOMENICO: Well, I'm going to move it along. Zell, will you give us your presentation, please?

MR. PETERMAN: Thank you.

This follows along with Bill's presentation, and I'll describe the strontium, and if time, the uranium isotope data that we've acquired on samples from UZ-14. I'll try to address these topics as time permits. Bill has pretty much set the stage for the history of the well and the encounter of the water in UZ-14, and this pretty much says the same thing in terms of the history, except that this is from the collar of the hole.

So what we did was--let me add one thing here. This zone occurs at the top of the lower vitrophere in the Topopah Spring tuff, and this of course is the zone that regionally commonly shows the effects of past water - rock interaction. So we know that throughout the mountain at some time in the past anyway, it has been a wet zone.

Of course the importance of whether or not it is perched, I think the first question has been answered, it doesn't appear to be the water table. If it's perched, it would be desirable to know the extent and thickness of the zone. If it occurs in the repository block, of course it could be encountered in the ESF, mainly in the ramps that go down to the Calico Hills, and also if it is extensive throughout the repository block, I would think it would have to be taken into account in any thermal loading model. This would be a body of water then much closer to the repository itself than previously thought, and I suppose would have to be evaluated in terms of what it might say with regard to a fast pass scenario.

I'll first talk about the strontium data, and I have to give a little tutorial here. It's a system, isotopic system that has been applied to hydrology, but I need to just mention a couple things. The main utility of the strontium system is that strontium at these sorts of temperatures of course is a perfect geochemical analog for calcium. The utility of the system itself derives from the fact that strontium 87 is formed by the decay of rubidium 87, which has a very long half life, but it ends up that different materials in the earth's crust have different isotopic, strontium isotopic ratios, which is what makes it valuable as a natural tracer.

Since we can't measure absolute abundances on a mass spectrometer, we derive this equation here by strontium 86, which is a non-radiogenic isotope, and thus we measure the ratio today in rocks, we can measure this ratio. Because deviations are small, even though our precision is very great, we can measure ratios to 50 parts per million or better. It's convenient to express this ratio as a delta value from some standard, and we use modern sea water as the standard, and so this is a per-mil deviation from modern sea water.

I'd like to digress just a minute. This is not the first time that we've used this system at Yucca Mountain. Starting with the calcite silica issue, golly, several years ago now, and also in the climate program, we've been acquiring a rather broad data base for strontium isotopes in groundwaters of the region, both in the Paleozoic aquifer and in the Cenozoic aquifer, and part of it has been under the Paleo climate program and part of it under the calcite silica. And I don't expect you to make any sense out of these numbers. They're in your handout. If you want to try to contour them or anything else, you're welcome to do that.

I will point out one thing. There is an isotopic high right in here. Generally, the numbers in a very crude way increase from north to south. There's a isotopic high right here. The Funeral Range and the Precambrian rocks of the Funeral Range are just to the southwest of that isotopic high. If we look at the Paleozoic data set, which also has continued to grow over time here, we see these are the Death Valley springs, we see an isotopic high over here in Death Valley, which may indicate that indeed there is some flow beneath the Funeral Range from the Cenozoic aquifer into the Paleozoic aquifer, and that would explain these numbers very nicely.

I should also point out that there's a parallel data base being generated over in here by the Environmental Restoration Group, and specifically by the people at Lawrence Berkeley. So when we're all done, we're going to have a very nice regional data set to address some of the issue that both Russ and Dick mentioned with regard to, you know, where does the water come from, where is it going, are these things--is there any possibility that they're mixing, that sort of thing.

So we're still kind of in the data gathering stage there, but there's some very interesting trends and spacial variations that make us think that it's going to be a very useful data set.

Back to the UZ-14 now. As I say, that was kind of a digression. This is the pumping history of the perched water in UZ-14. Bill mentioned it. This just shows it graphically, a total of about 6,000 gallons were eventually pumped and sampled. A number of samples were taken for isotopic analyses during the course of the pumping.

Measurements were also made by the people who were out there, pH and specific conductance, and the main point here is the top one plots against time and the bottom one plots against volume, and the main point I want to make is there was a lot of noise early on in both these parameters and then a general plateauing as time went on, both in conductance and pH. I don't know the significance of these early variations with this strange material that we've found in the well. It obviously has been introduced at some time, whether it was drilling of UZ-14 or--I think it probably was, they can't conceive of this crud being transmitted along a fracture. If it was, it's certainly a major fracture down there. But with that sort of material in there, I would think that could certainly perturb both conductance and pH.

Okay, let me show you the strontium data to start with. The vertical axis is this delta strontium parameter that I showed earlier. Zero on that scale would be sea water. The top graph plots against date, because these are the bailed samples over here. The red dots are concentrations. The scale for that is over here. By and large, everything averages about .2 to .3 milligrams per liter of strontium, which is moderately high.

The bottom graph plots against volume now, which then is this data right here, expanded to the pump test. And I want to point out two things. One is that although there's quite a bit of scatter here, the average of the bailed samples in terms of delta strontium is about 4.5. I have to mention one thing here, too. Early on in the pumping, it was necessary to fill the pipe with water. This was a low volume pump, and it was filled with J-13 water. Some of that got away and got back into the system. So there was some J-13 water added here during the pumping. What we see isotopically, this line down here represents J-13. We've got it very well characterized, at least the J-13 water that is being pumped today.

You can see that there was a lot of dispersion here in this isotopic parameter, a lot of scatter, and then with time there's very clearly a stabilization with one anomalous sample here. The mean of this what we call the stabilized signal is 4.45. So one conclusion you can make from this; first of all, there are at least two waters present in the hole, and there is a water then that's stabilized with pumping. It had a value here, it's quite distinct from J-13 water. This noise here could either be the result of J-13 water that leaked from G-1, or it could be J-13 water that was introduced during the pumping. We can't tell the difference. We think we've characterized here the isotopic composition of the indigenous water.

I want to compare that water signature with a couple of things now. There are calcite veins in the unsaturated zone. They're not terribly abundant, but they do exist, and over the years, we've been able to acquire a number of samples of these things. What's plotted here are calcite veins for a variety of drill holes, just as a distance of the water table at delta strontium along the X-axis.

This is what I'll call the stabilized UZ-14 water, and then these are the isotopic compositions of various calcites. You see the UZ water does fit in the range of these calcites. The average, excluding these G-2 calcites which we make another story on totally, the average of these calcites is about a delta strontium of 4, and we have a large data base for surficial calcite that evolved mainly through the calcite silica study that went on for several years, and I'll just show you that numerically.

Surficial carbonate, this would be pedogenic calcite, slow parallel calcite, vein calcite at the surface, Trench 14 calcite, whatever you want, it shows a very tight distribution, has a mean delta strontium of 4.43, very limited dispersion and we have a large data set.

The UZ fracture fillings have a much smaller data set. They show a much larger dispersion. UZ-14 water is virtually identical to the surficial calcite. We have 13 samples that we would say are on that stabilized plateau, and it's a very tight clustering of data.

The tertiary aquifer water, we don't have a whole lot of data from Yucca Mountain itself, but the J-12, J-13, JF-3, a couple other waters in there, have an average value of about 3. So the UZ-14 water is very distinct from anything in the saturated zone in the vicinity of Yucca Mountain. It's virtually identical to the surficial calcite and we would argue that a very plausible scenario, we think a very likely scenario is that this is indeed a perched zone. There's mixed water in it now. We don't know when the other component was introduced, whether it was ten years ago or last summer. The indigenous water has a unique isotopic composition. That composition is virtually identical to surficial calcite. We would say that it picked up its strontium signal at the surface and percolated downward and got trapped in that perched zone.

Now, uranium isotopes are a little more ambiguous. We can use the 234, 238 ratio as the tracer, just like we can strontium. Most times, the 234, 238 ratio is above unity in oxidizing waters. If it were in secular equilibrium, it would be one. We have a lot of data on groundwater from Yucca Mountain and the range is 1.1 to 7.

Again, just like the strontium, I'll show you this. The uranium data we have, which we have much less uranium data, shows a trend with time, and this is rather puzzling to us and we don't have an answer for this at the moment. The trend actually starts, the lowest value starts below J-13 waters, but with time and pumping, it appears to stabilize at a value of about 7.4, or something like that. So this is still a mystery. This could indicate that there's yet a third water in there, or the J-13 water possibly that they used ten or twelve years ago, or whenever it was, maybe J-13 has changed its isotopic composition with time for whatever reason.

All the uranium isotope ratios that we've done at Yucca Mountain or regionally, it's not just at Yucca Mountain, you see J-13 is a real end member here, and if you'll look at a map and plot these data up, you'll see there's a bull's eye around this J-13 value. Anyway, the perched water or the UZ-14 water is a couple orders of magnitude lower in uranium concentration, and I realize there are, you know, potential complications here with uranium precipitating out or being absorbed on iron oxides. We don't think that would change the isotopic composition, so the isotopic composition is up here at the extreme end and significantly different than the J-13 water.

So in conclusion, we can conclude that, one, there are at least two waters present in UZ-14. We also conclude that one is, on the basis of these isotopes, one is a real perched water and that we think it's probably more than coincidence that this zone that contains the perched water is also a zone regionally that shows the effects of water - rock interaction in the past.

So with that, I'll stop.

DR. DOMENICO: Thank you very much, Zell. Any questions from the Board? Don?

DR. LANGMUIR: Langmuir, Board. Bill Steinkampf showed us some what looked like uncorrected C-14 dates from the later pumping that showed that the apparent ages were getting older. I wondered if there was any corrected information which would give us some idea perhaps of the age of this perched water body. And that question has to do with what you think the size of it might be, from what information you have and what you could do next to establish that.

MR. STEINKAMPF: No, Don, there's no corrected numbers. We really don't have a means to do that realistically, I think, to come up with a number. We've got C-13. I've got a C-13 number for one sample, but I don't feel that that's sufficient to do a correction to meaningfully put an age on it.

DR. LANGMUIR: Can you at least bound it, given what information you have?

MR. STEINKAMPF: Yes, but I wouldn't be proud of the confidence in it.

One thing that I didn't mention was that on the slide, that Piper diagram that had a site data, if I can put this up for a second, you'll notice there a thing that looks like an oil well, there's an A, a B and a C. The oil well is the pumped sample that I collected, and the A, B and C represent that series of three bailed samples. And you can see that they are all conspicuously up on the higher end of the area, which strongly implies that they are much less chemically evolved than the rest of the volcanic waters. And this coincides, perhaps serendipitously, with the lower lithium concentration, which equally supports the less evolution, less water - rock interaction, less contact time than waters that we see from the saturated zone, even though these samples were not the best in the world.

DR. LANGMUIR: I've forgotten what the age date on J-13 is. What does C-14 tell us about J-13?

MR. STEINKAMPF: It's about 29.2 per cent modern, Don. I don't know what the numbers are. I think it's going to be on the order of 13,000 years, thereabouts.

DR. LANGMUIR: I guess the other question is for Zell, whether either one of you or anybody in the group now has a sense of how large this perched water body might be, what volumes of water we're dealing with and how we'll get a handle on that.

MR. PETERMAN: Yeah, there's a little bit of water in NRG-7 now. They're pulling up damp cores from SD-12, is it, or where's the LM-300 now? They describe it as damp core. So the answer is it's not known, the extent is not known. I would think it would be a critical thing to know.

MR. STEINKAMPF: But every place that the basal vitrophere or the Topopah has been penetrated has evinced significant alteration, healed fractures. That's Schon's magic zone. I believe that one core through the vitrophere in the vicinity of a fault showed some movement and that the fault plane did not appear to be altered, which should suggest something about the temporal nature of the occurrence of or arrival of fluid at that zone. I don't know if there is any information that reflects the presence of fluids adjacent to that altered zone, but I know that in the past, drilling tended to proceed at a fairly rapid rate, and that on occasion--or that there was less attention to detail perhaps paid that could have resulted in missing something of this nature.

DR. LANGMUIR: One more. When we talked about this at the Geologic Survey a month or two ago, Alan Flint presented a model at that time in which he suggested he could predict where perched water might occur at Yucca Mountain. How do you feel about his modeling effort and where do we stand on our comfort zone and those predictions at this point? This apparently was consistent, this occurrence, with his predictions.

MR. STEINKAMPF: It seems to make sense. The control appears to be the presence of more permeable unit, those below the top of the Tiva in these topographic rows in the washes, and that's essentially the correlation--correlation is not the right word. Those were the zones where Alan predicted higher water, and it appears those are the zones where we have indeed determined it. But, again, I don't think that we looked sufficiently closely at that contact, the top of the vitrophere, in the past, you know, the drilling in the Seventies and Eighties, to comfortably say we didn't hit perched fluid anywhere else. So as far as the distribution over the site, I think it's inconclusive at this stage.

MR. PETERMAN: That's right. The Paintbrush does crop out in the bottom of some of those draws in the northern part of the area, whereas it doesn't to the south.

DR. DOMENICO: Very good. I'm going to move on to the last presentation of the morning. Mike Wilson from SNL will talk to us about what might happen if we get a does standard.

MR. WILSON: Okay, we have a bit of a change of subject now. I'm going to talk about the role of the saturated zone in performance assessment, not exactly as Pat just said, having to do with a dose standard, but I am going to be talking about what effect that has quite a bit.

So in performance assessment, what we want to know is how good a barrier might the saturated zone be in reducing releases to the accessible environment, and the answer to that is it depends. And I want to point out two things that it especially depends on; the time period that you're interested in and the type of standard that you're interested in.

As far as time period, what we have found so far in preliminary modeling is that for a very short time period, say the thousand year ground water travel time criterion, the contribution by the saturated zone may be quite significant. For a 10,000 year time period like the old EPA standard, it's questionable. It may have a significant contribution, though most of our preliminary modeling shows that it's not very significant. For a long time period, 100,000 years or more, it becomes less and less important.

The reason we're considering these long time periods is we know that there's a new standard coming up and we don't know what it's going to be like, and we're concerned that there is a non-negligible chance that they may change the time period for the standard. So we are interested in what kind of effect that would have on our measures of performance.

The other thing that has some effect on how important you consider the saturated zone to be is what kind of standard you have. For a cumulative release type standard, at short times, as I said, the transport time may be significant. At long times, you can probably neglect the presence of the saturated zone entirely.

For an individual dose type standard, the amount of dilution in the saturated zone is a very key factor. And so something I want to really emphasize here is that with a cumulative release type standard, you can probably get away with neglecting the saturated zone if you want to. It may not be a wise thing to do for the reasons that Dick Luckey pointed out earlier, but you probably can.

For a dose standard, I really don't see how you can neglect it at all, because you're interested in what the concentration is in that saturated zone water when you calculate your doses.

For the rest of this talk, what I want to do is talk a little bit about some results we've gotten from our total system performance assessment study that we did last year and what we think we've learned from it as relates to the saturated zone.

First I'll talk about a three dimensional model of the saturated zone that we developed and did some flow and transport calculations with, and then I want to talk a little bit about effects of climate change on the saturated zone, and in particular as they affect dose calculations, and then at the end, I will talk about some of the items that we have found in our sensitivity studies to be particularly important and some of the activities that are planned that will help to give information on those.

For our total system performance assessment last year, George Barr developed a three dimensional, relatively local saturated zone model for our performance assessment calculations, and I wanted to point out one thing in particular, and that's that the calculations are done assuming an equivalent porous medium type modeling. So it's different from the discrete fracture network type thing that Kenzi Karasaki was talking about earlier. We're very interested in examining the implications of a fracture network model to performance, but we have not done so yet.

The reason for doing these models is because we want to know, for different kinds of assumptions, what the effect is on repository performance. And in particular, George developed two calibrated models of the saturated zone flow. The first one basically represents the same type of flow system as in the old Czarnecki model of ten years ago, and the second one is developed by putting in a coupling with the carbonate aquifer, as proposed by Fridrich, et al a couple, three years ago. And in both cases, the information that was shown earlier, the potentiometric information of Ervin, et al, was used for developing the boundary conditions, since it is a local model and there are not natural flow divisions at the boundaries.

This shows graphically the area that's being modeled. The actual model is a thin slice, only 200 meters thick, starting at the water table and going down, and it's roughly 8 kilometers on a side, and this shows you more or less how big 8 kilometers is for the Yucca Mountain region. And just so you can see it, this is what the numerical grid looks like. It has about 7,000 elements, and as is shown here, it has the region subdivided into five geologic units; Topopah Spring, Calico Hills, Prow Pass, Bullfrog and Tram. And that's not necessarily how it's divided for the material properties of flow, especially the hydraulic conductivity there. There was additional subdivision of these units as far as the material properties was concerned.

This shows the top of the grid and some of the important features. It shows where the repository is on there. The green dots are locations of existing wells where we have potentiometric information. Those are the points where the model is being fit to. The idea is to pick a set of hydraulic conductivities for the different material subdivisions and then vary them within the acceptable range to try to match the potentials at those points because that's the currently available information.

And some that George found in his experimenting is in hundreds of different attempts and variations of the hydraulic conductivities, he was unable to get a good match of the potentials at those points unless he made some assumptions, some additional assumptions. And he found that if he put in reduced conductivity at those two areas shown in blue, that he could get a good fit. And so the implication that he's drawing from the modeling he did is that he thinks it looks like along Solitario Canyon fault and along Drillhole Wash, that there's a reduced conductivity.

And this is not something that is by any means certain. It's a very poorly constrained problem. We don't have enough information to constrain the calculations enough to get a unique answer. And in fact, as I already mentioned, George developed two answers, and there's probably an infinite number of possible answers that you could get by varying these things.

But with putting the reduced conductivity in those areas, he was able to obtain a very good fit, and it's suggestive and it's something that's relatively easily testable, so that's something that should be easy enough to determine as we get more data.

This shows the results of the transport calculations that George did, and there's a couple of things that I want to point out. Number one, there's a range of transport times calculated, and it depends both on which of the two models you choose, the one with the drain to the carbonates or the one without the drain, and it also depends on the position within the repository area where you have your release point. And one thing that's of interest is that all these calculated transport times are shorter than ones we had used earlier, which were done with a two dimensional model that was developed by Czarnecki.

A good part of that may just be because of going to three dimensions instead of two dimensions. You have additional possibilities for going around low conductivity areas when you have higher dimensions.

Something that we did not expect is that in these calculations, the model with the drain has shorter transport times than the model without it. This is not what we expected. The concept of the drain model was that most of the water flow is going down to the carbonate, and what's left in the tuff is relatively slow moving. That isn't what happened in this model.

George thinks that if he were to increase the amount of flow that he put down the drain, that he may be able to obtain a calibrated model in which you get that concept better, and the transport times are already here somewhere, but that just shows you the amount of uncertainty there is in these calculations.

Okay, moving on now to climate change and dose calculations, there's basically three things you can think of that climate change may do to the saturated zone, and one is that the water table may go up. And Zell Peterman and the people who work with him have found evidence that the water table has been as much as 120 meters higher in the past. So it's not a negligible thing.

Something that also can have an effect on the calculated doses, as I will show a little later, is what time scale you're talking about for this water table to rise.

All right, secondly, is the water flux through the saturated zone affected by climate changes, or is it relatively stable over time. I don't think we have any information at all on that at this time.

When the water table goes up, does the mixing depth, that is, the depth, the vertical distance over which the radionuclides are mixed, would that increase as the saturated zone goes up, or does it just move up with the saturated zone? That's something that we would like to have some information about.

And, lastly, when the water table rises, you have a possibility of a completely different set of flow channels becoming active and changing the flow system considerably.

This is an illustration, it shows a typical dose calculation from our total system performance assessment, '93 calculations, and there are several things I want to point out about it. Number one, this is based on the concentration of saturated zone water at the accessible environment boundary. So it's water in the saturated zone 5 kilometers away from the repository, is what this is based on.

You can see that this shows a one million year period. That's the time period we chose to do our calculations for because, as I said, we were concerned that trying to get an idea of how longer time periods might affect our calculations, and it looked like a million years was enough time to see the peak calculated dose in most cases.

For this one, you can see the peak dose occurs at about a quarter of a million years.

The quasi periodic structure you see here arises from the assumptions that we made about how climate change affects the system. And these are by and large assumptions, because we don't have very certain knowledge at this time. Number one, we assumed that there was a regular 100,000 year period for the climate cycle. However, the amount of time within each cycle that was allocated to dry and wet climates was a variable, and that's why you get as much variation as you do in this calculation.

The second thing that's important in this, and that I'll talk a little bit more about, is that highest doses are occurring during what we assume are wet climate periods, and the low doses are coming from dry climate periods. And the last thing that is of interest I think is occasionally, such as right here and here and here, there are little spikes in the dose calculation, and that's coming from the assumptions we made about how the water table rises.

In our calculations, we simplified things and just said that when you change to a wet climate period, the water table goes up just instantaneously by some amount, and it was something that was varied in the calculations, but it could be, as I said, up to 120 meters in what we did. And what we assumed was that any radionuclides in that interval were then suddenly injected into the saturated zone flow, so that you get a spike in your calculated dose because of that. And it may be exaggerated because of the fact that we made it instantaneous rather than spreading it out over some amount of time.

This is an illustration of one of the effects I just talked about, and that's that the calculated doses are higher during the wet climate period. This shows what I'm calling potential dilution factor. This is the multiplier for the concentration being released from the waste packages. So that when this factor is lower, that means there's more dilution. So over this direction, you have higher dilution, and in the plot, the dilution is higher for the assumed dry periods than for the wet periods. And this seems the opposite of what you would think. In the wet period, there's more water, it should be diluted more. But it's because in the calculations, we did not make any changes in the saturated zone flow for the changes in climate. So that basically what happens is in the wet climate period, you get more radioactive material being leached out of the repository and dumped into the same amount of saturated zone flow. And so that's why it's the opposite of what you might have expected.

And then I also wanted to illustrate the effect of these sudden jumps in water table. The green calculation shows distribution of calculated doses if you turned off the changes in water table height so that you didn't have this sweeping out of material from the lower part of the unsaturated zone. And you can see that those water table rises are accounting for maybe a factor of three increase in the doses. So it's not big on a log plot, but it's something that would be nice to nail down a little better.

To wrap up this discussion of the examples, I think there's three important saturated zone issues for performance assessment; number one, what is the overall flux through the saturated zone. If you have a higher flux, then that means that your transport times are lower and it also means that your doses are higher.

Is the transport pretty well mixed over the area, or is it highly channeled? If it's channeled, presumably that means you have faster transport, so lower transport time. It's a little unclear what channeling would do to the dilution and, therefore, the doses. If you have a bunch of discrete fracture channels or other type of channels and the contaminants get more or less mixed among all the channels, then there's probably not much effect on the dilution and the doses. But if most of the radionuclides get into one of the channels and a lot of the rest of the saturated zone flow is clean, then the concentration in that one channel could be very high, and so you could get increased doses, depending on what happens when you withdraw it, whether you mix in water from the other channels when you withdraw the water.

And then lastly, how do climate changes affect all of these things.

And now I want to close by talking about several things that we've found within our sensitivity studies and our performance assessment models to be important. And in brackets after each one, we show some of the USGS studies that are planned that may provide the information on these areas, and I want to point out that many of these are planned but not necessarily going on at the moment, and when they will take place will depend on funding levels and what Scenario A does and all that.

So first of all, because of the modeling I showed that George Barr did, it looks like the Solitario Canyon and Drillhole Wash faults could be important control areas to the local saturated zone flow. So we think it would be important to characterize those areas.

And then next, the large hydraulic gradient; we really need to know what is going on there. Is it a drain to the carbonates like Fridrich suggested, or is it something else? We need to know what the implications are for repository performance from whatever it is that's going on there.

Next, the mixing, the dispersivity, whatever else you want to call it, needs to be characterized. Right now, we're pretty much making wild assumptions about what those things are because we don't have any real information.

And then lastly, climate effects, especially what the water table variations might have been in the past and, therefore, extrapolated into the future, both in magnitude and the time scale over which it happens and how it may affect the mixing and travel time.

Any questions?

DR. DOMENICO: Very good, Michael. I have a question.

When you said you had to lower the conductivity to recapture the known potentiometric surface, that really means that you probably treated it as a barrier of sorts; is that a fair statement?

MR. WILSON: Basically, yes. I can't remember how much you decrease it. It decreased it a couple orders of magnitude or something, the conductivity for those cells.

DR. DOMENICO: With the drain model in your flow analysis, could you capture that same piezometric surface by putting in the proposed drain, or did that not work?

MR. WILSON: No, it didn't work. He ended up having to put in those features in the drain model as well. But like I say, if you increased the amount of flow through the drain, maybe that would change the situation.

DR. DOMENICO: We've done some modeling. We couldn't get it to work with the drain model, and the only way we could reproduce it was with a barrier. It's been noted, I think, that a large gradient, you can almost make a case that it coincides with the boundaries of the calderas, so it very well could be that.

With regard to your transport modeling, how do you get dilution? Just recharge as it goes along? Because you don't have a hell of a lot of dilution, you don't have a lot of recharge in that system.

MR. WILSON: No, there's not a lot of dilution.

DR. DOMENICO: How do you get it? How do you put it into your model? Is it rainfall?

MR. WILSON: In the unsaturated zone, yes. In the saturated zone, we just assume there's some amount of water there, and so dilution will be that amount of water divided by however much water we assumed there was in the unsaturated zone. And that's coming from the amount we assume is percolating and also from how many waste packages there are contributing, which depending on the amount of water flow there is.

DR. DOMENICO: Could you make a dose standard?

MR. WILSON: Could we--

DR. DOMENICO: Could you meet a dose standard?

MR. WILSON: Our preliminary calculations show very high doses. I don't feel like that's necessarily right, because I think our source release calculations are very conservative right now, and so I don't feel like we can really answer the question right now.

DR. CANTLON: Cantlon, Board. Have you looked at all about Tom Buscheck's postulate of moving a fair amount of water up from the top of the saturated zone under heating and what that might do to input of isotopes down into the sat. zone?

MR. WILSON: We have not included that in our models yet. And something I did mean to point out at that one chart early on is that the thermal effects could have important impacts on these things, but we haven't included that. Buscheck has said there may be convection cells in the saturated zone, and there may be precipitation of calcites and things like that, and we have not looked into those yet.

DR. LANGMUIR: This is Langmuir, Board. This gets to an area that I was going to ask you about, too, Mike, and that was the source term. You had to assume a release rate, an event, some sort of failure scenario from somewhere within this program, which obviously has major uncertainties attached to it. Have you done sensitivity on how much that might affect what you're concluding here in terms of dose calculations over time? Obviously, it's going to have a major effect, what you assume.

MR. WILSON: I'm not quite sure I understand your question. But I think the point is that what assumptions you make about the source term and how water contacts containers are--in our sensitivity studies, we find that to be very important, and that's really where we think we need to put the most effort.

DR. LANGMUIR: Are we in the right order of magnitude at this point even? Do we know that much?

MR. WILSON: We are really forced right now to make some rather incredible assumptions, which as much as possible we try to make conservative. For example, when--we have an algorithm that we use, which may or may not be right, to calculate how much water contacts waste containers and how many waste containers get contacted. And then using corrosion models, we calculate that some of them fail, and at that point, we just pretty much evaporate the container and assume that water is contacting the waste and carrying it away. But if you corrode a few holes in the container, it may be very hard for water to get into the container and then for the contaminants to get back out. And so I think that's a very large area that we need to look into and maybe reduce the conservatism.

DR. LANGMUIR: One further question. You mentioned in passing that--and I should have caught this in previous meetings we've had--that there was evidence of the water table being 120 meters higher and that Zell had some things to say about that. My question is do we have any way of knowing when that 120 meters higher water table existed, and since then, have there been changes because of alteration in the rock which would affect its recurrence, or what would create that, from infiltration perhaps?

MR. PETERMAN: There are several lines of evidence that would argue for that 100 to 120 meters higher. Perhaps the most compelling evidence is the existence of the old spring deposits at the south end of Crater Flat and just north of

Highway 95. The best U-series ages we have from there indicate that they were wet between 15 and 20,000 years ago, and maybe, you know, several times prior to that. That's based on U-series ages mainly of root casts.

DR. LANGMUIR: But the hydrologic properties of the rock, alteration has been taking place through time, but could you infer that the rock properties have not changed since then?

MR. PETERMAN: Well, there, the water table is in the alluvial valley fill so, you know, that's pretty coarse stuff. Under Yucca Mountain, you see spotty alteration above the water table. Yes, certainly, you know, where that occurs, that could change the rock properties; right.

DR. DOMENICO: Any questions from the staff?

MR. REITER: Mike, I think this is a good example of what I believe the Board has been asking for, feedback from performance assessment and what kind of tests are really important. But let me take it a step further. Last January, we saw results from stuff that your cohorts in Sandia are doing, and they not only listed things which are important, but things which are less important and not so important. Could you give us some idea as to whether you'd be able to come up or give advice to the project office as to what kind of things are not as important as they might seem to be?

MR. WILSON: That's a mean question. I always have to give a couple of paragraphs of caveats before making any statement like that. I'm nervous because there's so many things left out of our models right now that we could be misled. But--do I want to say anything?

Let me just state that in what we--in our modeling, what we found to be most important is the water flux through the unsaturated zone and how it interacts with waste containers. And I guess anything else is less important. I'm not sure what to say.

DR. DOMENICO: Would you like some time to rehearse an answer so you can come back to us this afternoon?

MR. WILSON: Well, if you insist?

DR. DOMENICO: Any other questions from the staff?

We've got one minute, then we'll be at 20 after. Is there any one single question from the people out there?

We do have a discussion session I believe at 4 o'clock or so. You can bring it up then, so why don't we just break now and be back at 1:20. That's one hour. And don't be late, because we're going to hear about that extreme illusive site characteristic feature called groundwater travel time, whatever that might be.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

DR. DOMENICO: I'd like to introduce Jean Younker. She's going to talk to us about the evaluation of groundwater travel time, and Jean has talked to me about this and she's really, enthusiastically very reluctant to discuss this topic.

MS. YOUNKER: I am Jean Younker. I didn't put up the introductory slide. I guess I should, since I was going to make a couple of comments first.

One is, that this will be a little bit of a diversion now from the talks that you heard this morning, in that I won't talk, really, about the details of the hydrologic modeling that we'll do in order to evaluate groundwater travel time, but give you more a regulatory perspective, and that's the reason why I am here.

If you notice my title, I am the Regulatory and Technical Evaluation Manager for TRW, as the M&O contractor for DOE, and our job is to assist DOE and work with them in trying to figure out how we use the kind of information that you heard talked about this morning, to feed it into the regulatory process that the DOE has to do internally in terms of evaluating site suitability, as well as evaluating compliance with NRC's requirements.

Let's see, I was also going to mention that yesterday, one of the things that came through was the importance of continuity and history in the program, various programs that were talked about yesterday, and so I thought that between Rick Spengler and I--Rick's on the same panel here that I am--Rick's been here since '78, I've been here since '82, so I figure you're probably looking at some of the longer continuous service in the Yucca Mountain Project that we have, so we're trying to hold up our end of the continuity and historical aspects of the program for you.

I'm going to look at this groundwater travel time evaluation from both a Part 60 NRC perspective, as well as 960, recognizing that 960 is somewhat open to discussion right now in terms of how the DOE will use that to evaluate site suitability, or the way in which it will be used. So, that is a subject of discussion. I'm just going to make an assumption, as I talk to you, that we will use it in about the same way that it's been used in the past, just so that I can have that as a going-in assumption.

There have been a lot of recent discussions and comments about whether groundwater travel time represents a reasonable performance measure, or a good performance measure for evaluating an unsaturated zone, or perhaps any site, and given the time that's gone past since the regulation was promulgated by NRC, and since DOE put it into their siting guidelines, and so, the perspective that I'm going to give you is based on some fairly recent discussions and thinking that some of the people that are working in the issue resolution part of the project--we have, some of those people work for me--have been doing some thinking about, kind of assuming we are going to evaluate groundwater travel time, assuming the NRC doesn't decide to make any changes to their sub-system regulations--which we all know has been periodically a topic of discussion. I think Dr. Cantlon raised that as a question to the Commissioners, or at least a comment that maybe that was something that they should be re-thinking.

We're making an assumption here that we should go ahead and, you know, give them the process and the importance of the process. We should go ahead, build on what we've done before, and define an approach that gives us a reasonable performance measure for this criterion, so, for both 960 and 60, the way we're approaching it is to try to establish a performance measure, an interpretation of the regulation that is a reasonable performance measure that we think the DOE will be credible if they use it when they evaluate site suitability against their own disqualifying condition for groundwater travel time, as well as when the NRC has to look at the material that's provided.

So, the way I'll approach this will be, first of all, to talk about the little--very quickly, the regulatory perspective and background; then talk about some of the recent thinking that we've been doing on this, the background for the current approach that we have in mind, and then the general plan for evaluating travel time, and I want to make certain you understand.

This is an evolving process, and by no means am I presenting DOE's final preferred approach. This is something that the M&O staff, together with participants and the DOE technical managers are working together on, and it's evolving. We're just giving you a snapshot in time, so I want you to realize that what I'm saying may not be where we finally end up.

Let me give you the regulatory perspective first, and this one, I think, will be a review for many, many people in the room. As you know, we have two slightly different wordings that we're dealing with in this program. One is the NRC's groundwater travel time performance objective, and in this particular definition or requirement, the wording is:

"The repository shall be located so that pre-waste-emplacement groundwater travel time along the fastest path of likely radionuclide travel from the disturbed zone to the accessible environment shall be at least 1,000 years, or such other travel time as may be approved or specified by the Commission."

Now, if you look at DOE's Part 960 disqualifying condition, that is basically the same concept. "A site shall be disqualified if, once again, pre-waste-emplacement travel time from the disturbed zone to accessible environment is expected to be less than 1,000 years along any pathway of likely," important, "likely and significant radionuclide travel."

And one thing I had forgotten--and I'm not even sure I originally knew it--is that in the draft guidelines that DOE put out for review, they originally had wording--instead of the word "significant" in here, they originally had wording very similar to what the NRC has, and in the NRC's comments during concurrence process, review and concurrence process, NRC suggested to DOE that they should change that wording and perhaps not put this kind of a clause in, since the 960 purpose was to be a site screening or site evaluation tool, rather than the final judgment of compliance with a total system, which is clearly what NRC was getting at here, if you'll look at this.

This would lead anyone reading it to believe that the NRC people who wrote this thought that they might want to fall back on a total system perspective if they had some travel times that were shorter, but a total system that looked very good, total performance that looked like a safe site, that they intended to have a balance, or, potentially, a balance there.

So, what DOE did, then, in response to NRC's comments was to draft that clause, but they also added the word "significant," and by adding the word, "significant," it certainly, if you go back and read the statements of consideration and the comments, responses to comments that the DOE received, this word, "significant," was added kind of to imply the same basic thought process that is suggested by the NRC's words, meaning that not every pathway traveled by a water molecule may be as significant as another one, so I think that's the concept that we believe was intended.

Let's see, one other comment on the DOE's disqualifying condition. That is the one disqualifying condition where, in Part 960, it says that this one you won't be able to evaluate as early in your site characterization program. Most of the disqualifying conditions were, at least the writers attempted to write them in a way that you would be able to evaluate them on the basis of less information than when you go to the evaluation of qualifying conditions, which are the other half of 960's--the guidelines.

The disqualifying conditions generally are a little more site-specific, a little bit more potential to evaluate them separate from the total system performance. This is the one where it clearly says in the text that this one won't be able to be evaluated as early in the site characterization process, so it kind of stands out as being a different type of disqualifying condition.

Okay. This will give you just a little bit more history, very briefly. At the time that the DOE issued the site characterization plan, there was a general strategy in the draft, in the consultation draft that was issued, and that strategy was then revised and, in fact, the comments from the NRC that I've summarized here probably represent kind of a merging of what they told us--what their comments on the consultation draft said, and what we then finally responded, and, as a matter of historical perspective, this view graph, or a very similar view graph was presented by Dave Dobson when he was the DOE Regulatory Division Manager in 1989, to the ACNW, so you can tell that--kind of the stream of information is there from around the SCP time.

Let me diverge for one minute and mention one other thing I wanted to say, and that was you know that we--the DOE had to evaluate groundwater travel time at a time the environmental assessment was issued in 1986, and so, there's already been one evaluation against the groundwater travel time disqualifying condition, and, if you recall, that was a very simple approach.

It was a stochastic model with just the idea that how many realizations, basically, out of a total number that we were able to run, would you get travel times less than a thousand years, given the site data properties that we understood at that time, and the assumption of--actually, I think we did sensitivities on this, but assumption of a certain flux value.

So, the history goes back pre- the time I'm starting, but I didn't think it was worth going back quite that far. So, the SCP strategy, then, which isn't very different than what I'm going to tell you we're thinking about today is, obviously, a 3-D hydrogeologic properties model is an essential underpinning for a groundwater travel time calculation; then, some kind of a travel time contour map; look for the regions of fastest paths; perform detailed sensitivity studies on those fastest paths; and then take those calculations into a compliance evaluation.

What the NRC told us about that approach was that they wanted to make sure--there was a hint in the consultation draft SCP that we might produce a cumulative groundwater travel time distribution such that you might not be able to identify the extremes for different alternative models, and so, one of the things that the NRC said was they wanted to make very sure that the DOE did not combine travel time distributions in such a way that you wouldn't see the extreme values.

They also said, obviously--they didn't--their comments were that we hadn't really identified all of the assumptions as clearly as they would have liked it in the evaluations that had been done to date, but I will add that the SCA comments, site characterization analysis that the NRC provided on the site characterization plan has no open comments related to groundwater travel time, so they basically accepted the approach that was presented, with the responses that were provided to their comments, and with the changes between the consultation draft and the final SCP.

Okay. Let's go on now what we've been thinking about and where we're heading. I don't think there's really been any question, if you look at the NRC comments on the SCP, it was always understood, I think, at least by some of the people in the staff and the DOE and the DOE contractors, that this concept of likely paths in both requirements, in both 960 and 60, implies a distribution of travel time as a probabilistic approach, although there's been--in the last few years, there's been a fair amount of what I kind of consider noise in the system that someone felt that these regulations had been written with the intent of a deterministic-type calculation of groundwater travel time.

I don't think within DOE and the contractor family, nor within the NRC staff that commented on the SCP, that there is any evidence of that. As I said earlier, the word "significant" was added to Part 960 to suggest that some pathways and travel times may not be important to performance, and, of course, that's what--in defining a reasonable approach to interpreting, or to evaluating groundwater travel time.

It's clear that the direction we believe we should have is toward the term or the word "significant," and, of course, the word "likely," but what does the travel time mean from the standpoint of significance to performance?

The phrase, once again, "...or such other travel time..." clearly implies, as I think I already said, that the Commission expected that you might want to look at system performance when you're evaluating this requirement.

Okay, the one kind of sticky issue that comes up here is the disturbed zone definition. We've had a fair amount of discussion about this by the issue resolution working group that's been thinking about this topic, and if you go back and look at the statements of consideration, look at the information that was--kind of defines what people were thinking about when they wrote these regulations, the original intent was to start this pre-waste-emplacement calculation outside of the zone of major coupled processes, outside the zone of major repository disturbance.

Well, clearly, since the time that was written, our understanding of where disturbances may extend to, depending on the thermal loading, has evolved quite a bit, so you get yourself into what could be--my personal opinion is--what could be a fairly not useful discussion if you start talking about having the disturbed zone go all the way to the saturated zone, you know, and that's one thing that people have suggested. "Well, gee, give them the kind of disturbances that we could be predicting." Maybe you don't have any unsaturated within--or outside of your disturbed zone.

A personal feeling, and I think the feeling of the group is that that's not a very reasonable approach to what was intended, so if you look at the actual words, both DOE and NRC's definitions, it's very clear that only a disturbance that has significant consequences on performance needs to be considered, so that you go back, then, to that word, "significance." What is the significant consequence to performance?

Once again, a personal opinion, I would like to see us not end up spending a lot of time and dollars driving site characterization studies for this purpose, of defining a disturbed zone, that we wouldn't have to do anyway. I personally think it would be--it just doesn't make sense. Let's look at the original intent. Let's make some kind of an assumption, and if we continue--which I'm assuming we will--with a groundwater travel time calculation, both for 960 and 60, let's define some kind of either mechanical disturbance or mechanical--some conservative boundary.

For the environment assessment, we used 50 meters. When I went back and asked Scott Sinnock, "Why'd we pick 50?", he said, "Well, it was bigger than anybody ever believed we'd need."

So, you know, I guess my impression right now is that it makes the most sense to pick some value. We can look at it from the standpoint of whether the changes in that zone could have a significant consequence as to post-closure performance by running sensitivities on it, but that seems to make the most sense from the standpoint of what the original intent of the disturbed zone, as the starting point for your calculation of groundwater travel time, was intended to be.

So, where are we now? Okay, the same basic thought process, but expressed a little differently, and I want to make sure I give credit where credit's due; Jim Duguid, Bob Andrews and some of the Sandia PA staff have helped with this presentation, so I'm not--I don't want to pretend that I've put it together myself.

The groundwater travel time can best be interpreted as particle transport, meaning not the average path length divided by the mean velocity, but a distribution of unretarded radionuclide transport times--and the "unretarded" was left out there.

The transport processes considered in the analysis would then include advection, of course, and then dispersion, and matrix diffusion. Now, all of the above are uncertain, spatially variable, and, therefore, we'll still have to use a stochastic process, as you would assume, given the nature of this requirement.

Okay, then carrying this to the next step, the particle arrival times at the accessible environment then depend on dispersion caused by the different paths followed by the different particles, meaning small-scale heterogeneities, matrix diffusion caused by particles entering the rock matrix where flow is slower, and the locations over the areal extent of the repository, particles starting at different locations there, and, depending on how you define accessible environment, a number of different pumping wells, if that's how we define it, or the envelope that you use to calculate the particle ending locations.

Then, this groundwater travel time distribution would define the likelihood of each particle reaching the accessible environment at a specific time, and, finally, the significance of a particular pathway or travel time could then be related to either the integral of the mass release along that pathway, or the peak mass concentration at the end of the pathway, and so you could--I mean, essentially, what we're thinking about is kind of the take the area under the curve that's below a thousand years, and compare that to a Table 1, whatever the Table 1 numbers look like to us when we get them from the revised EPA regulation.

So, in terms of the general plan, then, as I said, I think we should personally take a reasoned approach to establishing the disturbed zone boundary, determine the expected distribution of groundwater travel times, and speaking of it as a particle transport approach, from that disturbed zone through the unsaturated zone, and then run your sensitivity analyses to determine the effects of uncertainties and spatial variability in these properties; percolation flux, the hydraulic properties, alternative conceptual flow models, which, you understand, that's going to have to be one of the very strong sensitivities, both now and any time in the near future, matrix diffusion, and dispersion.

We'll move to the saturated zone. It's the very same approach; expected distribution of groundwater travel times in the saturated zone from points below the repository to the accessible environment, conduct sensitivity analyses on, once again, spatial variability, but also effects of uncertainty, spatial variability and hydraulic properties, boundary conditions, alternate models, matrix diffusion, and matrix dispersion.

And, our total travel time, then, sum the travel times through those two units, and, as I said earlier, evaluate the significance of the short travel times, the shortest travel times to performance to assess compliance with either Part 960 or 60, so we're trying to put this into a perspective where, I think, our view is that you want to not do something different for groundwater travel time that you wouldn't do anyway, except for not including chemical retardation, and that's really, if you look, that's the distinctive difference that--the only really distinctive difference that you have here from your transport calculations that you would do for total system assessments.

That's it. Thank you.

DR. DOMENICO: Any questions from the Board?

(No audible response.)

DR. DOMENICO: Jean, I've got one. That last slide--not the last one, the second to the last one, you know, you've completely re-interpreted groundwater travel--it's more than interpret. It's a change. You've actually changed it. You're going to use the same methodology that you use to calculate releases, same model, probably.

MS. YOUNKER: Right.

DR. DOMENICO: Diffusion--dispersion is going to send some stuff out of the advective front, but if you chalk up a little bit of matrix diffusion, and matrix diffusion and retardation, that's a retardation factor, and I thought the--I have no problem with this, because I think everybody knows what I feel about groundwater travel time, you know, but it seems that it's a complete change, you know, of that concept. It's not exactly pre-emplacement groundwater time. It's radionuclide migration.

MS. YOUNKER: Yeah. I didn't mention this before, but that is one, of course, of the debates, is: If you listen to what the questions that have been raised, is, does a pre-waste-emplacement travel time calculation really make sense in light of what we know?

DR. DOMENICO: Well, I think you know my position on that.

MS. YOUNKER: Yeah.

DR. DOMENICO: But if you can convince NRC of that, good.

I think the--I was told, anyway, that someone from the state may want to add a comment here. Carl or Steve?

MR. FRISHMAN: I'm Steve Frishman. I'm someone from the state.

There are a couple of different ways of looking at this, and I know we've been through this discussion in different places and at different times.

Let me start out in a way that relates a little bit to what Pat was saying, and also relates back to what Lake said yesterday, and that's the new focus of the program is on site suitability, and the guidelines are the standard for site suitability. The guidelines are required, under Section 112(a) of the Act, to establish factors which qualify and disqualify a site, and it also says that those factors shall be--or that geology shall be primary among those factors.

So, and now what you have done here, as Pat, I think, has observed well, is you have removed a geologic or hydrologic factor, and replaced it with the equivalent of a radionuclide factor, because you have the characteristics of the waste entering into the question of suitability, characteristics both in terms of what the waste is, and also, the thermal characteristics of the waste, and this comes back to such things as where is the edge of the disturbed zone, and so on.

So, if you're proposing to re-interpret this guideline in such a way that it becomes essentially a waste performance rather than a site performance characteristic, then I think what you need to do is reopen the guidelines, and see if that can prevail in a comprehensive review of the guidelines.

I don't believe that your rationale that you can interpret the guideline relative to what NRC has said in its comments about the guideline, relative to 10 CFR 60, I don't believe that that even plays. The question is whether you have satisfied the law in terms of writing a guideline, and a guideline that will qualify or disqualify the site. So, I think that's probably the key matter that's in front of us.

Whether it conforms to the NRC licensing standard or not, I think, is another problem that you have, and when you get to licensing, if you get to licensing, you have to figure out how to deal with 10 CFR 60. So, I would suggest that if you want to make an interpreted change to this extent, that we better go back and just announce that the guidelines are going to be reopened, put out a proposed amended rule, and let's go after it. Let's talk about what it really means, and we can even talk about such things as whether groundwater travel time, or whether a fast groundwater travel time is appropriate or not to an unsaturated site, or to any site, because, remember, these are still general guidelines.

So, what I see is a, first of all, a fogging of the suitability question, based, once again, on the licensability question, and we've been through discussing this distinction for years; and also, what I believe to be, essentially, an unauthorized re-interpretation of an existing rule that's been promulgated under the Administrative Procedures Act.

I also have some problem in your apparent interpretation of the implication of the meaning of such other travel time as may be approved or specified by the

Commission.

Now, there's two different ways of looking at that. One of them is, "other" could be another time, less or more than 1,000 years. "Other" could also be pre-emplacement groundwater travel time, or some other kind of groundwater travel time, not radionuclide travel time, groundwater travel time. So, I'm not sure that your interpretation of the implication of 10 CFR 60 is even correct in this instance.

I won't belabor this much more, but I think it's most important to recognize that whether the guideline, in fact, conforms or not to whatever you interpret 10 CFR 60 to mean is immaterial in a suitability determination.

MS. YOUNKER: All I'll comment is that I think the team that's been working this, I think we have some confidence that the original intent of "likely" and "significant" in DOE's guideline, at least, was pretty close to what we're interpreting, so I don't feel like as if we've moved that far away from the original intent.

From the standpoint of what should be done with 960, I'll have to defer to a DOE person. If you want to comment on that, Steve, or someone?

MR. BROCOUM: Steve Brocoum, DOE.

There's really not much to say, but we do have this public meeting on May 21st for the interested parties to come in and tell us what they think ought to be done, in Las

Vegas, in the Mirage Hotel, I believe, on the afternoon of May 21st. There's a notice of inquiry going out in the Federal Register to cover this.

DR. DOMENICO: Groundwater travel time, or--

MR. BROCOUM: No. It's the whole issue of how we should approach suitability, of which groundwater travel time is a part.

DR. DOMENICO: We have a minute or so. Any questions from the staff? I'm sorry, I neglected--any--Leon?

DR. VERINK: Jean, have you and the NRC reached any sort of agreement as to whether or not water vapor is to be included in groundwater travel time?

MS. YOUNKER: I am not aware of an agreement, but I do think that the NRC, at least certain representatives of the NRC, have made statements that that was never the intent for water vapor to be included, and I don't recall who said that. Maybe somebody in the audience can help me, who's closer to this, but I recall sometime fairly recently reading something where an NRC person was quoted--I don't know how far up in rank--was quoted to make that statement.

There are some NRC--I don't know. Seth, do you know? Seth Coplan, do you know who made that statement, by chance, or--

MR. COPLAN: No, I don't.

DR. DOMENICO: Thank you very much, Jean.

Our next speaker is Linda Lehman, who'll talk about alternative conceptual modeling of the saturated zone flow system.

MS. LEHMAN: Board members, and ladies and gentlemen, thank you for the opportunity to discuss with you an alternative conceptual model of the saturated zone at Yucca Mountain. I want to note that this work has been funded by the State of Nevada, and I want to acknowledge by co-author, Tim Brown. I'd also like to say that I'm very proud to have been with Nevada for ten years now, and was one of the first contractors hired by the State of Nevada.

Now, Carl Johnson likes to kid me about this, by proclaiming that I'm the oldest contractor that the State has, and I hope to some day live up to that proclamation.

This alternative conceptual model has really been built from a number of studies that we've done in the past. The concept didn't develop all of a sudden. It was actually a series of clues, basically, that we had from previous work, and work of other authors.

Two of these major pieces of work that have led to this conceptual model were presented to the Board earlier. A number of years ago, we presented what we called the Cosine Components in Water Levels, which I will be reviewing briefly. Also, our work with the INTRAVAL modeling group in the unsaturated zone has also led us to this fracture-controlled idea; and, lastly, we have just completed some work on Devil's Hole, only about two weeks ago, which became a major piece in this puzzle of fitting together what is a conceptual model for the site for the saturated zone.

In 1989, we analyzed the water table fluctuations for eight wells at the Yucca Mountain site. These wells are shown on this diagram. We wanted to examine the frequency of the oscillations to see if there was anything that could be correlated, or give us a clue about the behavior of the saturated zone system.

Since a normal transform analysis required a uniformly-spaced data input set, we developed, in-house, a procedure which we call FIT.M, which just simply fits a cosine curve to the water levels.

We also looked at the lags, as well as the periodicity, and what we concluded from that study was that, basically, Well WT-7 and WT-10 were responding at similar frequencies and similar lags. Likewise, Wells WT-16, WT-1 and WT-11 were also corresponding together, but at a somewhat different period and different lag.

What was interesting was that we first noted that these were all aligned in a north/south direction, primarily, or a northeast/southwest direction, and it indicated to us that this was parallel to some of the major fault systems, like Solitario Canyon Fault, and that one side of the mountain was behaving differently from the other side of the mountain, and that, perhaps, there was some sort of a hydraulic divide in between.

It also, in my opinion, established that we had some sort of connection in this direction with the correspondence of the water tables in the north/south direction.

Our work with INTRAVAL in the unsaturated zone led us to the conclusion that fracture flow probably also was a major component in the unsaturated zone. Our analysis for INTRAVAL seemed to--our water content profiles were matched easier with a fracture model, as opposed to a matrix for those flow models.

It also led us to investigate the work of Sass, who published this work in 1988. Now, we have modified these water table contours slightly. He published just the data points.

What this indicates is that it is not an isothermal system that we're dealing with. On the western side of Yucca Mountain, we find the hottest temperatures, 38°C, which happens to be very near the trace of the Solitario Canyon Fault, and also near the volcanos that are currently at the site.

The coldest water was moving down, we feel, from the north down through the south, and these cold temperatures are around 30°, 30.1, something like that, and the wells that we had tended to be coincident with the Ghost Dance Fault, which led us to believe that perhaps this fault is channeling this cold water moving in from the north.

We also see another cold water spike coming down the Fortymile Wash zone. In between, we have in between temperatures and in between these cold spikes, so, from this, we concluded that perhaps the role of the faults may be different. Not all of the faults, even though they are aligned in the same direction, may be providing conduits. Some of them may be barriers, but others could be conduits.

For example, on the west side, in Solitario Canyon, could very well be acting as some sort of a barrier to flow coming in across this direction, while this Ghost Dance Fault definitely, in my opinion, there's some reason for this low temperature spike to be there.

Now, we recently looked at Devil's Hole again. We looked at that several years ago when the Yucca Mountain Project went to water adjudication hearings over the use of water from J-13 to be used during site characterization, and the question that we were addressing, then, was: Does this increased pumping affect the water levels in Devil's Hole?

And Devil's Hole, historically, has been regulated because of the Devil's pup fish, Devil's Hole pup fish, and in the seventies, the water level was fixed that could not be exceeded, or you could not go lower than a certain level in this hole in order to protect the habitat of the Devil's Hole pup fish. So, we were looking at what the implications of additional pumpage could be on that water level, and, at that time, we found that there was a definite linear trend which was decreasing over time, and, without any additional pumpage, that that water level would decrease to the maximum allowable within the next ten years, without any additional pumpage.

We later went back to examine that to see what's happened, and over the last couple of years, we've had a lot of rainfall, so that level of decline, while it's still declining, is not declining at anywhere near the rate that it was previous to that. Now, maybe after this intensive rainfall period ends, we'll see another decline. However, these data that we used were provided to us by the National Park Service, and they are daily water level measurements that we used.

So, one of the things we were trying to do was see what were the correlations between other wells in the area and Devil's Hole, and what we found is that Devil's Hole was statistically correlated with AD-7 and AD-8, and some nearby springs. It did not seem, at this point, to correlate well with Army Well 1, which we had previously thought we saw a correlation.

What we did notice, though, was the response to Well AM-7, which is within a mile or so of Devil's Hole, responded in exactly the opposite direction. It had a negative correlation, so that, you know, made us curious, so we started looking into some of the reasons for that. Let me just show you the response of this water level.

What happened here is, during July, 1992, we had a series of earthquakes, which I will talk about later, but this response, this sharp response you see here in Devil's Hole was a result of that earthquake, and, as you can see here, Devil's Hole responded downward, while a nearby well responded upward.

So, we wondered what could have caused that, and I remember Ike Winograd's work a number of years ago, which showed that that spring discharge was probably controlled by a fault, and he hypothesized that this barrier may occur right through here. This has later been known as the Stewart Valley Fault, I believe, and maybe Rick can correct me if I'm wrong on the name of that.

So, this fault runs right through here, and the springs are in this general area. What we believe is that the difference in response was attributed to perhaps being located on the other side of the fault zone, or located in the fault zone itself of one of the wells, as opposed to the spring.

While we were looking at this, we also noted another couple of similarities between this region and Yucca Mountain itself. For one thing, there's another hydrologic gradient positioned right in this location, and the other thing was we had just recently gotten a copy, I guess an advance copy, of Rick Spengler's report, and we noted that this fault orientation was very, very similar to that of the Sundance Fault, and, hopefully, he will shed some more light on that later.

But, just in comparison, I put this slide in here just today, so it's not in your packets, but I felt I should show these similarities. This is from the Fridrich report, Journal of Hydrology, and you'll notice here this water table contour takes quite a dip here, and here's the hydraulic gradient, the large gradient, and there's a possibility that there could be movement down into this, what I'm calling a channel here, but what else is very coincident is I believe this lines up exactly with Rick Spengler's Sundance Fault zone.

Now, as I was saying, we were looking at magnitudes of earthquakes, and at the response of the wells to the earthquakes, and there were three earthquakes that occurred within a week, and that was the 7.5 magnitude earthquake in Landers, California; three hours later a second 6.6 magnitude earthquake occurred near Big Bear; and, the next day, a 5.6

magnitude earthquake occurred at Little Skull Mountain, located southeast of Yucca Mountain.

So, what we decided to do was look into our data base and examine the water table response to all of the wells that we had information on from DOE's data base. I want to say one thing about the data. We got these data from the DOE quarterly monitoring reports, and so, the data we were looking at was a monthly average, so that what we have is monthly average points over time, and, unfortunately, none of the wells that I showed you earlier that we analyzed with the cosine function were--are on that data base.

Apparently, DOE distinguishes between regional data base and site data base, and we don't have access to the site data base, basically, so we just looked at the regional picture.

And, what we found was that the responses could be grouped basically into four categories from these wells: First, wells that exhibited a temporary upward spike, and then returned to the previous water level; wells that exhibited a rapid upward change with an apparent long-term change, stabilizing at a higher level; then there were wells that showed a temporary downward spike; and, the fourth category were wells that showed a rapid downward change with an apparent long-term stabilization at a lower level.

I have some examples here I'll show you. This is a graph of AD-11, Well AD-11, and you can see before the earthquake, the water levels were here. After the earthquake, we jump here, and these dots are the measurements right before and after the earthquake, and with a higher--although it is declining somewhat it's still stayed up there. It hasn't come back down, and this difference here is 15 feet.

This one shows the opposite response. The Well RV-1 took a rapid drop and stayed stable at a lower level, also.

DR. ALLEN: Excuse me, Clarence Allen.

Is this before--is this associated with the Landers earthquake, or the Big Bear, or--

MS. LEHMAN: Well, you see, we're working with monthly, so we can't distinguish that, so it's really all of them lumped together.

DR. ALLEN: Okay.

MS. LEHMAN: Now, this is examples of some that have responded quickly and come back to their original levels.

So, what we did, then, was plot the most significant of those; in other words, wells that had only a sustained change of half a foot or more, and we plotted that coincident with the known shear, major shear zones, and some of the tensional faults in the region, and what we found was that a number of these--in fact, quite a few of them--line up along these shear zone faults, and the ones that are wells that respond in an upward fashion are shown in dots, and the ones that respond in a downward fashion are shown as triangles.

The magnitude is sort of indicated in the size of the dot, with the largest change in water level coming from AD-11, but you can see, right through this area, that they all line up nicely with this Stewart Valley shear zone.

Also, some of the water level declines line up nicely with other known fault zones in the area, and in the Crater Flat, we have a couple of water levels lining up with these north/south trending extensional features. So, while I believe Dr. Harris, that scientists shouldn't try to solve social problems, I also believe that hydrogeologists shouldn't solve tectonic problems, but that didn't keep us from speculating on a few mechanisms that might be at work here.

Now, one thing I wanted to show, though, was--before I get off of that--is I've put on here a normal fault. Actually, this is a transformed fault out of my structural geology book, and, to me, some of these patterns that I was seeing in here sort of took that zig-zag, not only with just the water table contours, but some of these faults as well, and this also shows the relationship, you know, you can have, in close proximity, a shear zone and an extensional one together, hooked together in such a transformed situation.

So, anyway, some potential mechanisms, we thought, could be that as this shear zone was adjusting to this earthquake, that perhaps the--I don't want to call it--the pore volume was compressed, which would then cause an upward movement of the water table, and, likewise, if a fault was undergoing extension, then the pore volumes would be increased and, therefore, you could cause a lowering.

Another possibility was that the compression was forcing water out of one region and up through a more transmissive pathway, and we also were curious about thermal flow rates brought on by the earthquake, and if they might also play some sort of role, and it would be, in our opinion, nice to see if there were any temperature changes.

So, basically, that's led us to conceptual model which is basically fault-controlled movement along not only shear zone faults, but extensional features as well, which can be highly influenced by the tectonic regime. We may have this running right through--these shear zones running right through the center of Yucca Mountain, so we need to think, what are some of the implications?

Obviously, we have monitoring implications. We have implications as far as the hydrologic gradients; for example, in areas that are really flat, if you have a 15-foot water level change in a well, then maybe your local gradients could be temporarily reversed, and the idea that we need more specific information along faults.

Right now, the Department of Energy wants to do some work along Solitario Canyon Fault, and along the Yucca Wash Fault, but these faults, as they showed, may be barriers. What about the ones that are conduits? To me, that makes more sense, to start looking at some of those as well, so I would like to see a different sort of monitoring program in place, an explorational program which would look at some of these features in detail, not just the Solitario Canyon Fault.

And, we also feel that we would really like to have some temperature measurements, more than are coming out of the program now, especially in the unsaturated zone, and, of course, access to the water levels on Yucca Mountain in the WT wells.

And that's it. Thank you.

DR. DOMENICO: Thank you, Linda. It's refreshing to see some data analysis on occasion.

Do we have any questions from the Board here? Clarence?

DR. ALLEN: You stated you had access only to the regional data base, but did not have access to the site data base. What do you mean by that?

MS. LEHMAN: Well, historically, the State has had a problem of getting data from the project, and, recently, the

M&O contractor did provide us a bunch of data that we had data requests outstanding for ten years, and we finally got caught up with some of that. However, we only now have water levels through 1989 on the WT wells, and we're hoping, you know, that we could be in the loop, you know, a little bit faster and, you know, within six months or a year to get some of this important data, but it just doesn't seem to happen.

DR. ALLEN: Does the DOE have any comment on that?

MS. NEUBURY: Claudia Neubury, DOE. I'm the Technical Data Manager for the program.

I've said this before, I'll say it again, that the State has access to all of our data through our technical data catalog that they get on a quarterly basis. The TRB also gets that quarterly data catalog. The NRC gets the data catalog. I think everybody in this country gets the data catalog, or at least it seems that way, and the State has a mechanism for requesting data.

The one thing the State doesn't have is any kind of protocol with us, so we can share data easily, and unless the State wants to enter into some kind of protocol agreement, they'll have to go through the data catalog and a formal request through the Project Manager.

DR. DOMENICO: Thank you, Claudia.

Any more questions; any other questions from the Board? Staff?

(No audible response.)

DR. DOMENICO: Thanks again.

We're going to change the topic slightly here. We're going to have an update on the faults at Yucca Mountain by Rick Spengler.

MR. SPENGLER: I'm going to be presenting to you today a review of some of the characteristics that we've discovered within the central block of Yucca Mountain. I guess I should thank Leon and Clarence for inviting me, and also giving me 40 minutes to speak. I rarely have 40 minutes.

The structural characteristics at Yucca Mountain, specifically, the mapping that we've done over the past two years, involves highly-detailed 1:240 mapping within the central block. This type of mapping is commonly done in open pit mines, and it's this type of mapping that, two years ago, we decided to apply to the central block.

Specifically, we were attempting to gather structural information to supply directly to the unsaturated zone modelers several discussions that we've had about what types of data needs that they had, or needed, and--but as we progressed in the over two years of mapping now, we find that there are applications or impacts on various phases of the overall program, which include investigations of the potential fast pathways, development of tectonic models for Yucca Mountain, design and construction of the ESF, as well as the potential repository area.

This is a map of the central block of Yucca Mountain, and what I'm attempting to show here are the structural features that have been mapped at 1:12,000, the Scott & Bonk map of 1984, and with this, I want to point out several features here displayed on the Scott & Bonk of 1984.

Some of the features on the Scott & Bonk include the Ghost Dance Fault structure cutting through almost the entire potential repository area. It dies out to the north here into an area of intense brecciation to the north here, but also, I want to point out that in Scott & Bonk mapping, there are several of these north, north/south, and northeast faults that have been mapped to Broken Limb Ridge right here.

This particular width of this set of north to northeast trending faults is about 1200 feet. I'd also like to point out, on the Scott & Bonk map, there are several features that have been mapped throughout this area, northwest trending features, such as these within the potential repository area, as well as directly outside, you've got this northwest trending set of fractures and faults here, as well as this set of north/south, or northwest trending faults here.

In FY92, we started our detailed mapping in this part of the potential repository area, one inch equals 20 foot mapping, and I'll be getting into some of the features that we've seen in this mapping, as well as extension of our mapping in FY93.

This is the grid system that we established out within the central block, utilizing the Nevada State coordinate grid system, and within that grid system, we established an internal grid system which allows us to expand our mapping in any particular direction within the central block.

We initiate our mapping in this part, as I mentioned earlier. Each one of these squares here represents a 200 x 200 foot area that we map. We then compile the map, or all these areas into one compilation. I should note the I'll be talking about Broken Limb Ridge to the south here. We've also mapped ridges, Whale Back Ridge toward the central part. The focus will be--much of the talk will focus on Antler Ridge here, and then some of our new mapping will include Live Yucca Ridge to the north. We've got about 135 200 x 200 foot areas that we've currently mapped.

DR. PRICE: Excuse me, Dennis Price.

What do the alphabetic letters mean? Because there's several with the same letter in the slide you just showed.

MR. SPENGLER: What we've done here is established, within each one of these blocks, 1,000 x 1,000 foot blocks within the Nevada State coordinates, we established an internal grid going this way within the block, and so, within any one block, we'll have this alphabetical order to the grid system.

This is a composite of our mapping to date. I don't know if that shows up very well in the back of the room. Basically, we've mapped about--we have a relief within the area that's roughly a 200 foot relief. This exposes a large part of the densely welded Tiva Canyon tuff, from the hackly on up to the upper cliff.

Right within the middle of our study area is the trace of the Ghost Dance Fault. You see offsets here. What we see into the south here, at Broken Limb Ridge, we've got offsets on the order of about 45 feet vertical separation. As we go northward to Whale Back Ridge, we get a displacement on the order of about 70 feet of displacement. Heading north, Antler Ridge, South Antler Ridge, as well as the north face of Antler Ridge, we have a offset, or a vertical separation along the Ghost Dance Fault of about 47 feet, and then to the north, Live Yucca Ridge, we have abrupt decrease in the displacement of the Ghost Dance Fault to five feet.

What I'd also like to point out in this particular composite is we have a number of north/south trending features that we've been able to map on either side of the main trace of the Ghost Dance Fault. This detailed mapping allows us to map offsets on the order of two or three feet, and, therefore, we are able to track from south to north several faults on the other side of the Ghost Dance. We get the--what we refer to here as the West Hinge Fault, we have the West Fault, that's about 100-150 feet separation from the Ghost Dance Fault, and we have several other faults here, one of which we can trace here, the--what we refer to as the East Fault.

Much of our work has been concentrated over the past few years at near the central part of our study area, the south facing slope of Antler Ridge. Here, we have an exposure, very good exposure of not only the main trace of the Ghost Dance Fault, but also, these other faults that we see on the down-thrown as well as the up-thrown side of the Ghost Dance Fault. We have the West Fault again over on this side, and the East Fault, and some other faults to the east.

Now, what we've managed to do with this detailed mapping, the fundamental parts of this mapping is to map the distribution of breccia that we see within the study area, as well as the offsets of lithostratigraphic units of the Tiva Canyon tuff.

Many of you have been to this exposure at Antler Ridge. This is toward the base of Antler Ridge, the south facing slope of Antler Ridge. This is the main trace of the Ghost Dance Fault. You have this, what we refer to as a Type 1 breccia, granulated breccia at this particular exposure that's a couple of feet in width.

Part of our mapping is to trace wherever we see this breccia up the slope, and in this case, this is a close-up of this particular breccia zone, this characteristic of the main trace. We've opened up parts of this particular exposure I'll be getting into a little bit more, but at this particular case, here again is the main trace of the Ghost Dance Fault. The breccia that's now uncovered is several tens of feet in width right across here. There's a scale here of about three feet, so it widens out. You've got the main trace coming on through here, and you actually have breccia extending over into the up-thrown side, as well as the hanging wall side of the main trace.

I mentioned that we map these breccia zones as we trace them upward, and in our FY92 mapping, we saw that we are tracing these breccia pods that have a linearity to them along the slope, that seem to have a north to northeast trend, and there appears to be some west or left stepping of these breccias.

In our earlier mapping, this suggested to us that, in fact, we were dealing with a northwest-trending structural fabric older than the north/south trending features, and that this left stepping may be due to some of the faults stepping along these pre-existing planes of weakness.

Right here, we also have been mapping a breccia zone. We refer to this as the West Fault. There are these covered areas, and here is another breccia that we'll be uncovering and examining a little bit later.

Our study has three components to it. One is the actual mapping. Number two component is the verification or validation of our mapping that continues, and, thirdly, is the planning for future mapping and validation.

This is part of our initial validation of this mapping technique. In FY92 we were mapping these features along the Ghost Dance Fault. Some people believed that they were present, other people believed they were not, so part of the validation was to open up along Antler Ridge here, to not only open up right along the main trace of the Ghost Dance Fault here, but extend the pavement out to the west and to east to verify some of the features that we were seeing in our mapping.

This particular pavement we refer to as Antler Ridge Pavement-1, and I'll be discussing in two parts. I'll be looking at detail of this part of the pavement, and then, also, the western part of the pavement. The pavement is about 40 feet wide at this end and this end. It widens up very close to the West Fault right in through here. It's about 75 feet in width, and it covers a width, along with the main trace of the Ghost Dance Fault, right about in the middle. It covers roughly 500 foot.

This is the eastern part of that particular pavement, part of our validation, and what we see with this is, number one, we have a contact here that separates the hackly. The lower part is our hackly unit of the Tiva Canyon tuff from that exposure here of the lower lithophysal zone of the Tiva Canyon tuff. We just get a hint here in the covered area of getting into the overlying unit, which we refer to as the clinkstone unit.

What we see here are several features. One, we see these northwest-trending features, one here, one here, the contact coming along this bench, a downward dropping, a slight downward dropping along this bench. You can come along this bench here, and then, also, there is the disappearance of these benches over in this area. So, from this, we have a north/south trending feature exposed here. We have a north/south trending feature exposed on this pavement here. This is the main trace of the Ghost Dance Fault.

I want to show a close-up of several of these features, but to focus in, number one, on this particular northwest-trending feature cutting through, or this zone of brecciation cutting through here. We actually have another one up here, and then I'll show you a close-up of this particular north/south feature, where we have this particular offset.

This is one of our northwest-trending shear zones on the pavement. Here we have good bedrock of the lower lithophysal zone, and on the western side, we have this intensely broken up rock on the west side. This north/south trending feature is located right here, heavily coated with calcium carbonate. You can't see from this particular angle, but this is where we have that slight down to the west offset along this north/south trending feature.

This is most of our effort to date, or this fiscal year, is not concentrated on any additional mapping. We hope to continue our mapping in the latter half of this fiscal year. Most of our efforts are concentrated on the validation or verification of the mapping that was done, basically, in FY92, so there is this lag, but, basically, this is what we're doing to date.

This is the eastern part, again, but this is our breccia--what we call our breccia intensity mapping that we're doing. Here again, we have this one contact between the hackly and the lower lithophysal expressed here, but, in red, and in oranges, we're showing the highest intensity of brecciation that we see along this east side of the Antler Ridge Pavement. Again, we have this particular in the north/south features. We have a number, we have an offset here on one of the northwest-trending features shown right here. We have this intensely brecciated zone. Again, we have this brecciated zone, and so on, throughout this particular East Antler Ridge Pavement.

Now, looking over to the west, the validation, the dominant feature over on the western side of the pavement is this particular feature, which we refer to as the West Fault within this zone. The other thing to point out, I don't know if you can see it from back there, but there is a very strong northwesterly trend to the fractures and brecciation along this part of the pavement.

We continue to map in detail. We're mapping these pavements at 1:60, or one inch equal five feet, very detailed work, and tried to determine some of the jostling within this particular zone.

This, again, is a close-up of the West Fault that we see on the west side of the Antler Ridge pavement. Through our mapping, we had indicated this offset of roughly, or near-vertical separation of roughly 15-20 feet. This is the separation between the, in this particular case, is the upper contact between the clinkstone and the upper lithophysal zone.

Again, one of the more intensely brecciated parts of this pavement is right here along the West Fault. We have brecciation that we can see throughout this area that occurs over a width of several tens of feet.

I'd like to now focus on what we were doing in

FY93. What we wanted to do is not only verify the existence of these features from our FY92 mapping, but we also wanted to extend out in what we refer to as our wing mapping, to try to get a handle on the width of this particular system or zone, or whatever you want to call it.

Basically, I indicated earlier that in the Scott & Bonk map, it's roughly 1200 feet width here of these particular sets of faults, and, therefore, we wanted to extend our mapping to the east and west. We also wanted to extend our mapping along the Ghost Dance Fault to continue mapping northward.

This is the eastern part of our mapping in FY93. We extended to the east along Antler Ridge, mostly on the southeast, or the southern side of Antler Ridge, but extending over to the other part of the ridge. This is roughly 1800 feet in length. It covers about a 400-foot width.

In this mapping, we're mapping the units or the contacts between the clinkstone, the upper lithophysal zone, and, in this case, one of the depositional units of the Tiva Canyon, the upper cliff caprock facies. And, in this mapping along this part of Antler Ridge, we came across a series of down to the east faults scattered throughout all this area, some on the order of several tens of feet of down to the east displacement. All our other faulting, to date, along the

Ghost Dance Fault structure or system, were down to the west. These were down the east features.

This is a view of this particular part of our map area, and, in looking at this, you can pick out that one of the depositional units of the Tiva Canyon tuff, and that's this upper cliff unit here. So what I have just shown you, a map that covers roughly this particular area, and one of the things that you can see from this particular vantage point is that there seems to be an absence of at least part of the upper cliff unit in this part of the section.

Not only were we able to see these down to the east displacement, but other characteristics of this particular interval--by the way, this is H-4 location. This is one of the deep hydrologic holes that went down about 4,000 feet, and over here are some of the structures connected with the UZ-16 pad over in this general location.

This is looking off to the northwest, and in our mapping, we've seen several other things connected with this. One is that the upper cliff is thicker over here. It's about 75 feet here. Within this unit, it's about 45 feet thick, so there's an abrupt thickening over on this side of this particular interval.

We also see that the foliation within the Tiva Canyon near the upper cliff is about three degree dip in through here. As we get over to this side, it's got an abrupt increase to about 20°.

If we step back now to another ridge--I'm heading out to the southeast now--there's several other features, and I pointed those out, that they were mapped on the Scott & Bonk map. This is one of these northwest-trending features. This particular feature is a down to the east fault scarp right through here. The area that I was just looking at is right in through here, so it is actually--can be projected right in this particular location here.

Not all of these features that were mapped by Scott & Bonk were down to the east, but at least a few of them are down to the east; others, down to the west.

Now, I'd like to direct your attention going northward, again, along the Ghost Dance Fault. Right here, I have pointed out that on the south side of Antler Ridge, over in this area--this is the north side of Antler Ridge--there's another relationship that came into play as we were doing this detailed mapping, and that is that the Ghost Dance Fault, right in through here, has a vertical separation of about 45-47 feet.

As we extend northward, we cross Split Wash, located right here, and we are not only able to trace some of these other subtle features, like the East Fault here, and, also, what we're referring to informally as the West Fault and West Hinge, we can trace them northward, but as we trace them northward, they appear to be offset in the right lateral sense on this part of Live Yucca Ridge.

Not only are they displaced in a right lateral sense by about 170 feet, but also, as we cross over this particular inferred structure, the Ghost Dance Fault decreases, or abruptly decreases in displacement to five feet.

Also, on the north side of Antler Ridge, in this particular area, we were able to map in detail the Ghost Dance Fault north/south trending feature here, and it appears as though we have to move 50 feet off to the east in order to pick up that same structure, displaying an offset of around 47 feet.

In order to get that abrupt right step to this particular Ghost Dance Fault, we infer some type of fracture or fault that appears to be covered up with debris in between this right stepping feature.

This is a, looking northward to Live Yucca Ridge, I was pointing out features on this map just previously that were over here on the north side of Antler Ridge, and this is Split Wash. There are several features that I wanted to move towards along Live Yucca Ridge.

One, is that I'm going to be visiting a location right here near the drainage level. I'm going to actually first display a typical fault or a fault that we see in this particular part of the area. I might also note that we took full advantage of these runoff drainages on this particular slope. They provided very good natural exposures that would have been costly if we would have had to remove that.

The other thing is that you can barely see in this photo. There is an area right here that's referred to in the mid-eighties as Pavement 100, and I'll be discussing in detail Pavement 100 right at the very top of Live Yucca Ridge.

This is one of the typical features that we see along the south-facing slope of Live Yucca Ridge. This particular feature here is interpreted as a cooling joint. It does have brecciation associated with it, and it has a north 20, north 30 west orientation. We see numerous features such as this along the south-facing slope of Antler Ridge, in addition to these subtle offsets of the lithostratigraphic units.

Toward the base of Live Yucca Ridge, we have this particular feature exposed. It's a lineament that runs clear up along the ridge crest here, and I'm going to show a close-up of this particular location near the drainage level.

At this location, we've got about three to four feet of brecciated rock or breccia within this zone. On some of these exposures, we were able to identify slickensides that demonstrate near horizontal displacements or separation.

Moving up to the top of the ridge, this is the actual pavement that was constructed in the mid-eighties. Some of it is overgrown, but the thing that I want to point out is if we look to the northwest, where it is this particular feature that was mapped by Scott in 1984 as being a lineament that he identified on the aerial photographs, cutting through this particular ridge, where we see a cutout or a possible erosional cutout of some of the upper cliff.

If we go--I'm not going to descend this particular slope, but in this particular area, we also have an increase in brecciation that was mapped by Scott & Bonk.

We're going to also take a look at this particular pavement as it looked shortly after it was exposed in the mid-eighties. This is the arrow that was used. It's about three meters in length. It's pointing out in this direction. This is north, and so we're kind of looking out in a kind of a southeasterly direction here.

Things I'd point out, I'm going to be showing you a fractured trace map done by Barton of this particular Pavement 100 on Live Yucca Ridge. The main thing to point out are these very conspicuous northeast-trending cooling joints. They're cut across this part of this pavement.

To orient yourself, this particular curved fracture in through here, and this arrow, I'm going to turn you around a bit with this particular trace map. Here we have the north arrow again, so this is pointing up to north. These are these northeast-trending dominant cooling features mapped by Barton. This is actually published in one of the guide books that came out for the Geological Congress in 1989.

The other thing to point out--and this is in your handouts--these particular subtle features. These northeast-trending cooling joints are cut by a series of northwest-trending features. Although they're subtle, and they only offset these high-angle northeast-trending features by a few feet, they are present at the top of the ridge in through this zone, and there are some other offsets in through here.

In addition to our mapping of the breccia, and also the offsets, the sub-units of the Tiva Canyon tuff, to provide data to the unsaturated zone modeling effort, we also mapped fractures, and, to date, with our mapping in FY92 and FY93, we've mapped a total of 1557 fractures that can be entered into that unsaturated zone data base.

These fractures, in order to make progress in our mapping, we had to have some criteria. The cutoff on the fractures was fractures that were greater than six feet in length, or sets of fractures greater than six feet in length. This particular stereogram indicates that there are very strong preferred northwest, north to northwest trend to the fracture system within our mapped area.

Another type of compilation that we use is to take each one of our 200 x 200 foot areas and, within that particular area, any and all fractures that we had mapped in that area, we then plot up to use to determine the anisotropy of our study area.

In blue, we've kind of plotted up things that have a general north/south trend to them all along the Ghost Dance Fault. We do have fairly numerous areas showing a north/south trend. However, there are these other features or areas that we see throughout our mapped area that indicate a preferred northwesterly trend different from the north/south along the Ghost Dance Fault; pointing out these particular features here, northwesterly trend in this part of Antler Ridge, northwesterly trend in this part of Live Yucca Ridge, and also, through some of our pavement area.

Going to the south here, on the south-facing slope of Whale Back Ridge, we have a strong preferred, or preferred north/northwest trend to the fracture system, and also down in through here near Broken Limb Ridge, we have another series of northwest trending fractures.

Back to this slide of Scott & Bonk, again, these series of, I pointed out these family or set of northwest-trending features. This is our interpretation. This is where we've mapped along Antler Ridge. We've mapped, also, here in Live Yucca Ridge, and shown in red here, or orange, are some of our interpretation of connecting the dots, and this is where we come up with our Ghost Dance Fault and Ghost Dance shear zone.

We also have inferred that this may extend across the area where Scott & Bonk has mapped this particular lineament from aerial photographs, and, also, the breccia that he has mapped up to the northwest here, and also here.

To date, we haven't done any reconnaissance mapping, so we don't know exactly the field relationships of extending this off to the southeast.

Through our review process, our verification and review process, we've had a number of people, number of field trips out to Yucca Mountain to quality check this work, and, also, we're in the process of quality checking or reviewing the maps. Part of that review system has led one reviewer to an alternative hypothesis.

MR. FEHRINGER: Excuse me, Rick. Before you said that that northwest/southeast trending was the Ghost Dance. You mean the Sundance, don't you?

MR. SPENGLER: I meant the Sundance. I'm sorry.

This particular alternative structural model that will also be evaluated as we extend out our mapping would have this particular feature here, instead of having an offset, or right stepping of the Ghost Dance Fault on the north side of Antler Ridge, it would be a series of splays of Ghost Dance, and then there would be a cross structure. This would be termed the Sundance. This cross structure would then connect up with this particular feature that we have in this part of Yucca Mountain.

Now, in order to buy into this particular alternative structural model, what you have to do is basically dispute the mapping of the southeastern part of Antler Ridge, right in through here, so this is, again, part of the verification, and perhaps, maybe we need additional pavements to cover this particular area, and also Live Yucca Ridge.

To get to some of the characterization that has been proposed, there are some serious debates going on within the project right now regarding these particular activities. The debate revolves around whether or not we should be continuing on with 1:240 mapping. One of the disadvantages of this type of detailed mapping is that it is extremely time consuming. Right now, we have a crew of three people that have been working on this effort, not including our review team, for the past two years.

We know, from previous experience, and now, with the complications that we've gotten through this mapping, that we can only map about 100 of these 200 x 200 foot areas in any given fiscal year, unless, of course, we increase the effort so that we have more mappers. However, one of the difficulties there is that you just don't advertise for a mapper mapping in the Yucca Mountain area and expect he or she to be mapping within the next week or so. This is highly specialized mapping that takes some lag time in order to get up to speed with the very detailed stratigraphy out at Yucca Mountain.

We continue with our verification, and, in fact, what I just mentioned, we might need additional verification, the validation of some of these other mapped areas, and perhaps what we're doing now is strictly in the verification stage, using our mapping team to map the pavement. We intend to do some additional mapping toward the end of this fiscal year; however, if all things go right, we can only map approximately 30 additional areas this year.

So, our effort is being consumed right now in this verification effort. I suggest that perhaps not only is verification important, but perhaps it should be done in concert with additional mapping.

The other discussion revolves around whether or not we should--well, whether we should abandon the 1:240 mapping, and perhaps cover much more areas by going to more of a reconnaissance scale, and mapping one inch equals 200 feet is a scale that, if we were to go into this particular effort, might cover quite a bit of ground.

So, the debate rages in these particular areas as far as where we're going in the future of our detailed mapping effort.

There are some other efforts that will be done in concert with whatever mapping effort is eventually decided. We have our surface-based program, or before I go into that, let me go one step ahead here, and indicate that in FY92, before I leave the mapping, we had proposed that we start mapping out these wings. Our Group 1 was to continue our mapping to the north--

DR. ALLEN: Could I interrupt for a moment, please? This is Clarence Allen.

Before we go into this, you've submerged us in so much detail that I don't think people here really understand what your gross conclusions are. Could you summarize in two or three sentences what the gross relationships are, in your opinion, between the Ghost Dance and Sundance Fault; what your confidence is in this; and why it makes any difference, possibly, in terms of site suitability? I think the detail here has submerged your conclusions completely.

MR. SPENGLER: Okay. Let me back up.

To summarize, as far as the mapping effort, we have uncovered or recognized several additional features along either side of the Ghost Dance Fault, and the discovery of another feature that we are now referring to as the Sundance Fault, or fault system.

The field relationships suggest that, number one, we've been mapping--and the mapping is strictly within the 12.5 million year old Tiva Canyon, and, therefore, there is a very good possibility, or, actually, we cannot make the inference right now that these particular features are, in fact, older than the 12--or older than the tuffs, or they are older than the tuffs, but what I'm getting at is that they're not young features.

What we see with these stepping relationships here, not only the left stepping, but also the right stepping, suggests that there are these complications where we're seeing both, and that, in fact, there may have been jostling of the mountain that would accommodate both types of utilizing, pre-existing structures, and, at the same time, offsetting some of these structures.

I think the significance of these particular features is that, number one, they were not previously recognized. Number two, I don't know, personally, the significance to the repository or to the studies that will be going on as far as the hydrologic studies. I think that remains to be seen.

Some people would debate that these particular features are insignificant. I think with our early stages of our mapping, I personally do not feel that the significance of these features can be assumed or can be weighed and tested at this time.

You've got me a little off track here, Clarence. You've got several questions here that you have asked. Did I answer at least some of them, or did you have several others?

DR. ALLEN: Well, not really, but go ahead.

DR. DOMENICO: Rick, you're running a bit over. Do you have much more to present?

MR. SPENGLER: No. I'm just about finished here.

Part of the effort here is also to do some subsurface work. There is activities as far as the borehole, UZ-7A. This particular unsaturated zone hole is intended to penetrate the Ghost Dance Fault just north of Broken Limb Ridge. Part of John Whitney's efforts will be actually to do some age determination along these particular structures that we've identified in our mapping effort.

These include cutting a trench on Whale Back Ridge, deepening or exposing more of the trench in Split Wash, and also, that north/south trending feature that hooked up with the Sundance Fault, to also deepen that trench in Drillhole Wash. Also, part of the efforts to determine the age of some of these features, John Whitney's group also intends to examine northwest-trending features, such as the one that's cutting through to the north in Pagany Wash.

And then, obviously, part of the effort once we get underground is to investigate these particular structures along the Ghost Dance and along the Sundance.

I think I'll stop there.

DR. DOMENICO: Thank you, Rick.

Any questions from the Board?

DR. CANTLON: Cantlon; Board.

Based on what you know about other faults, what would you predict down underneath here? Would you expect these things to converge, or to go down in parallel with the main faults?

MR. SPENGLER: These features that we've been mapping are high-angle features. Some people have argued, when we first displayed or illustrated some of this work, that what we are mapping within our 200 feet of exposure along the ridge was just superficial. However, there's also zones that are pretty well exposed, for instance, along Busted Butte, which gives you much greater relief, and these particular features, at least the preliminary mapping done by Scott & Bonk of Busted Butte, does not indicate that these, at least in the relief that we have available at Busted Butte, that there is any decrease in the width of these particular zones in that particular area.

Others have suggested that perhaps we should examine in detail areas like Busted Butte to try to answer that particular question. Right now, I don't believe that that can be answered.

DR. CANTLON: It would seem that if the faults are major lines of infiltration, if they coalesce, then the collection basin for collecting water into a fault that gets into the repository is very much expanded in total collection area, so it would seem to me very significant to find out whether these things coalesce into the main fault line.

MR. SPENGLER: I agree, but with 200 feet of exposure, we don't have enough relief to make that assessment, and, therefore, we have to rely on the underground workings to try to get a grasp of those issues.

DR. CANTLON: But the likelihood of the tunnel intersecting in the appropriate direction would be a semi-miracle.

MR. SPENGLER: But, as I understand the plans now, if they actually have a underground working on the west side of the main trace of the Ghost Dance Fault, there are provisions available to go ahead and penetrate into the Ghost Dance Fault.

DR. LANGMUIR: Langmuir; Board.

The photographs suggested that some of the features you were looking at had caliche-type deposits within them. They were expanded, perhaps. Did you focus on those in any way? Did you identify those to the people interested? I'm particularly concerned that what you're doing is not tied into the rest of the program very well at this point, in terms of its interplay with the--you mentioned unsat zone studies, but, certainly, the issue of upwelling versus soil-created caliche materials is quite relevant.

Did you identify those areas in particular, and have they been looked at by others?

MR. SPENGLER: They have not been looked at as of yet. In fact, there are studies that will be initiated within the next few months regarding that particular issue, particularly in the pavement, the Antler Ridge pavement, where you have very extensive exposure of these different types of calcium carbonate deposits.

Now, if you're referring to the different types of brecciation that we see along the pavement, one of the parameters or characteristics that we are measuring or documenting is the characteristics of that calcium carbonate cement, and, also, the fragments associated, or within that breccia.

DR. CORDING: Cording; Board.

Just one question on this north of Split Wash. Are you indicating if this is--if the Sundance is tied into the Ghost Dance south, where the Ghost Dance, you know, as the Ghost Dance comes up to the north, and the much of the activity comes across on the Sundance, are you saying that the Ghost Dance to the north is not as significant as we might have thought originally, it's not part of the same trend, or--I mean, you've got it mapped there at the surface.

Do you have enough information up there north of Split Wash to know what the Ghost Dance is doing up there?

MR. SPENGLER: We know along Live Yucca Ridge, just to the north of Split Wash, that we can still trace the set of north/south trending features on either side of the Ghost Dance. We also know, or what I had mentioned was that the Ghost Dance Fault abruptly decreases in vertical separation from about 47 feet, North Antler Ridge, to about five feet on Live Yucca Ridge.

Now, the other peculiar thing that we see on Live Yucca Ridge, or the south-facing slope of Live Yucca Ridge is that the Ghost Dance Fault, or what we think is the Ghost Dance Fault, does not have the--of that family of north/south trending faults, is not the fault of greatest displacement. In fact, the one fault that's directly to the east, what we referred to as the East Fault, has a greater amount of displacement.

If that is, in fact, the Ghost Dance Fault, and that we haven't extended our mapping far enough to the east to actually capture the other East Fault, then there's even greater amount of right separation that can be demonstrated.

DR. CORDING: Could you pick up some of that with kind of a reconnaissance-type work that you've described there as Item 3? You might be able to get some of that information with that type of exploration, 1:2400?

MR. SPENGLER: We probably can pick it up by mapping the ridges as 1:200. However, if we go to a 1:200 type of mapping, one inch equal 200 feet, our basic fracture data base will drop out of any of the study.

DR. DOMENICO: Thank you.

Let's take a fifteen-minute break.

(Whereupon, a brief recess was taken.)

DR. DOMENICO: Our next presentation will be an update on the exploratory studies facility by Bob Sandifer.

MR. SANDIFER: This is a status report on the exploratory studies facility. I will do it in three parts. First, I'll cover the status of the current ESF layout. If you will recall, I made a presentation on an improved, or an expanded layout, a proposal which I will give you the status of that now; the progress since the completion of the starter tunnel; and I will have a discussion of ramp extensions versus the east/west cross drift, which is what was in the previously-baselined concept.

The current status of the enhanced layout is that it's been baselined at Level 3. A change request has been prepared and submitted to Level 2 of the Project, and we do have approval of that, and that's currently being implemented into our baseline.

This is what we have adopted, if you will. This is the enhanced layout. This shows that the 2.1 per cent grade on the north ramp, to 2.6 per cent grade on the south ramp, and a .5 to 2.6 grade on the Topopah Springs main drift. This shows the Imbricate drift, if you will, over to the fault zone. It shows the north ramp extension going down and getting in at Solitario Canyon Fault, and, if you will, the south ramp extension.

You have also seen this. This is the conceptual layout that I used in a previous presentation. This shows the primary or upper emplacement block, and the potential expansion block of the lower block.

The cross section, again, shows the previous baselined horizon, if you will, the SCP emplacement tunnel, and this is the upper block or the primary block, and this is the, if you will, the potential expansion area here.

This is one of the concepts, one of the working concepts that we're looking at. This is a lower thermal loading, where it takes the primary block as well as the lower emplacement block to handle the loading.

I'll move now to ESF progress since October, or since September. Package 1 previously consisted of 1A, 1B, 1C and 1D. Due to budget constraints, that was reassessed, both construction and design, for Fiscal Year '94 and '95. We now have a Package 1E, which consists of those things that are not required to either turn the TBM initially this fall, or keep it turning going into Fiscal Year '95, and the principal elements of Package 1E are the warehouse and the operations building, and we've now pushed that 90 per cent design review into Fiscal Year '95.

In this time period, Package 1C, the surface utilities, completed its 90 per cent design review. Package 2A, which basically is the modifications to the tunnel to permit launching the TBM, was baselined at Level 2 and issued for construction. Package 2B, which is basically tradeoff studies for the north ramp, we completed our 90 per cent design review, and it's pending submission to Level 2 CCB.

Package 2C, which is the north ramp, the design products that the constructor needs to construct the north ramp, that 90 per cent design review is currently scheduled for the first week in May. We would expect to mail our packages in that time frame, and, two weeks later, have our initial briefing.

The first alcove was excavated. That alcove is intended to support radial borehole and hydrochemistry testing. The delivery has begun on the TBM. We have several shipments on site, and we've begun actual assembly. The pad itself for the TBM has been formed and poured.

The 69kV power line is complete. The installation of the buried utilities on the south end of the portal pad are going well, and there's been significant progress in getting the water line from the J-13 well to the pad.

I have a series of photographs that I'll show you, starting with the front end of the TBM in the manufacturer's facility. You can see the cutterhead's here, and you can see the portion that was under assembly at that time. This is a closer view of the cutterheads. This is looking from the rear of the TBM, where you can see the, if you will, the conveyor belt here, and out the front end of the TBM here.

This is an artist's rendition of the complete TBM. The portion you were looking at a moment ago is in this vicinity right here. TBM is currently on schedule. We are still anticipating turning the TBM in early August. It's possible that it could be a little bit earlier, a little bit later, but, currently, things are encouraging.

This is a picture of the utilities, the trenching for the utilities. I have a view that shows more of that. The next picture shows more of what is going on with the pad versus what is going on at the portal.

This shows the Jumbo in position in the alcove. This photograph shows the completed alcove. Finally, there's a photograph of the--a broader view of the pad, and the portal itself.

I'll talk briefly about the ramp extensions. This is the north ramp extension, if you will, in the recently baselined approach, versus the east-west cross drift approach that was used in the previous approach.

First of all, the east-west cross drifts, total length of 2,063 meters. It allowed an examination of approximately two-thirds of the vertical extent of the TSw2, and, clearly, this ran right through the center of the potential repository block, which, to the designer, is a disadvantage. If it goes through the block where he's going to do his initial design work, he either has to accommodate that tunneling, or he has to avoid it.

The enhanced layout, again, this is the north ramp extension. This particular Imbricate Drift takes you up to that fault zone, and then the south ramp extension. Previously, the cross drift, the east-west cross drifts ran across, above the Calico Hills main drift, and if you recall, the Topopah Springs main drift came around and basically came out normal to the Calico Hills main drift.

The next is a cross section, if you will, going back to the east-west cross drift. This shows--this being the east crossed, and this being the west, and this being the Topopah Springs drift, normal to this drift, and, as you can see, you're staying in the upper two-thirds all the way across. This section, you're not looking at.

On the north ramp side, this being the north ramp as you're coming down, and this being the north ramp extension, as you can see, it penetrates into the TSw2 and goes down, and does most of the vertical section of the TSw2.

The south ramp, similarly. This should be labeled, south ramp extension from here to here, and this is the south ramp, coming down, and then, as you see, it goes through the TSw2 here; essentially, the full vertical cut of that particular section.

The north and south ramp extensions total 3620 meters, or, roughly 1550 meters more than the east-west cross drift. It does allow examination of almost the entire vertical extent of TSw2, and they are located at the ends of the potential repository block. So, you have the ends, you're looking at the ends as well as the Topopah Springs main drift comes across the block.

The ramp extension advantages: More footage, more exposure, supposedly, more knowledge gained; more vertical exposure of TSw2, preservation of the primary potential repository block, or, if you want to look at it another way, it gives the designer more flexibility. He doesn't have to contend with the east-west cross drift.

Exposure of two distinct and separate areas. Correlation may or may not be good, but, clearly, you will learn something in either event, because you're on either extreme of that primary block.

Disadvantages, there is more cost, there is more excavation, and you don't get to see that part of the, if you will, the middle of the repository block, and therein, I guess, lies the debate. We feel that allowing the designer the flexibility is a much greater advantage than what we had in the east-west cross drift, clearly, of seeing the ends. If you use the north ramp, then you also are going across the entire block.

In summary, the first thing I'd like to say is this is clearly a cursory review. We have a great deal of difficulty discussing what we've done at the ESF periodically with as much work as is going on in an hour or 30 minutes, or whatever, so we would certainly suggest, and we would offer you separate sessions or whatever you may think is appropriate.

Also, I would point out that we are perfectly willing, with small groups, ones and twos, to have informal sessions; for example, Ed Cording and Russ McFarland were in our facility last week. We can discuss issues there, and we can do them in great detail, whatever happens to be bothering you, or information you need. So, I would certainly consider, I would ask--suggest you consider those.

There has been significant ESF design and construction work done in the last six months, and the next six months, as, I'm sure, you gathered, will include the initial start-up or turning of the TBM.

And that concludes my presentation.

DR. DOMENICO: Thank you. On time; even ahead of schedule.

Any questions from the Board?

DR. CORDING: Ed Cording.

Bob, in terms of the start-up in early August or around that time frame for the TBM, you have some other work that needs to be accomplished at that point to be able to start in terms of design; is that correct?

MR. SANDIFER: We have a tight schedule, both design and construction. We, at the present time, when we look at the integrated schedule, we see us making that. Now, to make promises at this point would be very foolhardy of me, but I would suggest that this fall is a good, solid time, as far as I'm concerned, that the TBM will turn. It may not be August the 12th, it may be August the 20th, but we are close enough now.

For example, the big package, Package 2C is essentially designed. We go into our 90 per cent design review in like two weeks, two or three weeks, so most of our design work is now done, depending on how well that package does. So, the point there, from a design perspective, is much of what we had complete control of, we now go from a mode of complete control to a whole lot less control. We certainly hope, with the improvements we've made in our design process and the way we intend to do business with this design review, that it'll go much smoother than some of the others have. Certainly, that was our intention, and we spent a lot of energy on that.

Construction, the utility work and the TBM work are critical. Right now, from a CTS standpoint, they see all the pieces arriving. They have to go together, and we can't really have any upsets or problems, and the surface utilities are the same way. It's a great deal of work going on out there concurrently.

So, we feel like we're going to make it. That's certainly our intention, and we feel like we're on target today.

DR. CORDING: In terms of once you've been able to start up, what's the present outlook for the funding and the operation of the TBM? Are you going to be able to operate it in such a way that it's being utilized 24 hours a day once you get rolling, or is that still a situation where you're not going to have it--

MR. SANDIFER: That's funding-dependent. It's dependent on us getting the kind of dollars that we discussed earlier for the program. You're talking a significant cost component of the total cost of the work going out there. Once we put this machine in service, there is a base--if you will, a baseline cost that you have to support if you're going to run it three shifts. That is built into the budget, and if we get the budget, we can run three shifts. If we don't run--if you don't get the budget, then you're talking about something less than that.

DR. DOMENICO: John?

DR. CANTLON: Cantlon; Board.

You've talked about the TBM being delivered on time. How about the muck removal apparatus?

MR. SANDIFER: Far as I know, it's on schedule.

DR. CANTLON: On time?

DR. DOMENICO: Any other questions from the Board? Staff?

DR. BARNARD: Bill Barnard, Board staff.

In the enhanced ESF layout diagram, you show the Calico Hills main drift at a significant angle to the Topopah Springs main drift. Is that a final design decision? Originally, they were on top of each other, as I recall.

MR. SANDIFER: The--

DR. BARNARD: The two drifts were running parallel on top of each other.

MR. SANDIFER: The Topopah Springs, that is, in essence, the change that was made in the enhanced ESF.

DR. BARNARD: Right. Do you plan on re-orienting the Calico Hills main drift?

MR. SANDIFER: We have not changed the orientation of Calico Hills, and, to my knowledge, we don't anticipate doing that. We've really not looked at it beyond getting this in place. Certainly, the next step is to look at things like that, but we essentially left it unchanged while we did the enhanced layout.

DR. DiBELLA: Bob, Carl DiBella, Board staff.

On your conceptual repository layout you show an upper and lower block. Do you recall what the acreages are for those two blocks, and if you're limited--if you use only the upper block for the total repository capacity, what thermal loading does that shoot you into?

MR. SANDIFER: I'm going to defer that to some of my--Kal, would you like to answer that?

DR. BHATTACHARYYA: I'm Kal Bhattacharyya with M&O. I happen to have the numbers in my hand here.

The upper block, Carl, its emplacement area is about 930 acres, which translates into about 73kw/a. If you took the other block on the east side of the Ghost Dance, that has about 220 emplacement acres, and you can achieve 59kw/a.

DR. DiBELLA: A further clarification here. Both blocks will allow--you have to have 59kw/a? Was that what you said, Kal?

DR. BHATTACHARYYA: If you had both blocks, if you used the primary block and the one that's on the lower side, if you had those two blocks, you could accommodate a repository which would have 59kw/a.

DR. DiBELLA: You couldn't accommodate the full capacity?

DR. BHATTACHARYYA: Yeah. The assumption is 70,000 MTU, and our assumption was that that translates into about 68,000kw total inventory.

DR. DOMENICO: Further questions from the staff?

DR. REITER: In the last talk, Rick Spengler talked about the Sundance Fault, and he talked--one of the things he had in the end was underground exploration, and I wonder to what--how and to what extent you interact with the kind of work that he does or other people do when determining what are targets that you want to look at there? Are you considering the Sundance Fault? And, if you have; if not, why?

MR. SANDIFER: Well, I don't know which one of these--in fact, it shows on some of the view graphs that I showed earlier, but, certainly, what we do underground is integrated with the scientific community. That's why we're there, so we certainly will be integrating with--

DR. REITER: So, have you discussed this with Rick Spengler?

MR. SANDIFER: I haven't, personally.

DR. REITER: I mean, is there interaction going on?

MR. SANDIFER: Kal, do you want to speak to that?

DR. BHATTACHARYYA: This is Kal Bhattacharyya again.

Yes, we are aware of that. We have talked to USGS on it. It's shown on this--on the view graphs that Bob showed. We are not quite sure yet is exactly, though, what significance of Sundance Fault is. As far as I know, this was traced in Scott & Bonk many, many years ago. It's just a more definition of the fault, so we are just waiting and talking to USGS just to see what the characteristic of the fault is, and, you know, whether displaced, and so forth.

So, we are aware of that. This layout preceded Rick Spengler's work, as a matter of fact, but, as you can see, it's there. We are looking at it.

DR. DOMENICO: Any further questions?

(No audible response.)

DR. DOMENICO: The last speaker today, Dean Stucker, talking on focused mined geological disposal system, advanced conceptual design status, and update on the thermal loading.

MR. STUCKER: My name's Dean Stucker, and I hope to provide a status of the focused MGDS, mined geologic disposal system, advanced conceptual design, and Dr. Saterlie will give an update on the thermal loading study as part of this presentation.

I wanted to, before I really get into the status, we recently went through a organizational change at Yucca Mountain, and I wanted to review that change with you as it relates to the engineering and field operations section, which Bill Simecka is now the assistant manager for. Before, we were working with directors and associate directors, and we've gone to assistant managers and lead teams.

The way we've divided it now is that we're looking at a project engineering group for the exploratory studies facility, surface-based testing facility, and we're looking for a team, a project engineering team for the repository waste package part of it, and for the implementing, we have a design team that does all the design, both ESF and repository; a construction that does all the construction at the site; and Win Wilson is the supervisor at the site office for the operations and maintenance part of it.

I thought it might be interesting just to--I get a lot of discussion as to why do we need repository waste package designs at this point in our site suitability evaluations, and I think it's interesting just to review that we are looking to see if the site is suitable for a repository, and, in doing that, we need to assure that the activities at the site, and the ESF and the surface-based testing are compatible with the potential repository design; and, secondly, that the repository waste package designs identify any impacts that might be at the site, or with the site conditions; and, thirdly, the designs, preliminary designs will provide the basis for any safety or waste evaluations needed in the site suitability part.

You'll hear me talk about the mined geologic disposal system, MGDS, and what we're referring to there is the waste package, the surface, sub-surface repository designs, and the site characterization exploratory studies facility and surface-based testing activities. We're really looking at the overall system when we refer to the MGDS.

I might skip a couple of these view graphs to catch up on time. If there's any question in the package related to them, they have to do with the phases of our design, and I'll just jump into the fact that, as you know, we had a major change in our program direction with the decision to go forward with the multi-purpose canister in February.

Of course, that is a single canister that goes through the storage, transportation and dispose elements of the program, and the storage must meet 10 CFR 72, transportation is 10 CFR 71, and to the extent that we can, 10 CFR 60, because of the uncertainties and the to be determined, to be resolved issues related to 10 CFR 60, we talk about being compatible with, to the extent we can at this point, with the design evolution.

And, because of the decision with the multi-purpose canister, we are looking at a need to replace the site characterization plan conceptual design with a new architecture that meets the baseline changes with the MPC. We are in the process now of looking at what those architectural design changes are, and these changes will support the site suitability interim evaluations, total life cycle costs, environmental impact development, and license application development.

If I were to put a cartoon up as to--I think I've showed something similar to this before. The approach that we've taken is this focused advanced conceptual design approach, which, really, if you look at the requirements, we have numerous to be determined criteria constraints to be resolved in order to meet 10 CFR 60 and other applicable requirements.

Some examples of these: Thermal loading would be up there. What we want to do is we want to make some technical judgments, and make assumptions related to each one of these TBDs, TBRs. We want to make them with the best available information we have at this point, and go forward with one concept that's focused, based on the MPC, and also carry alternative features to that design that are important to waste isolation, and, at the same time, look at the assumptions and substantiate those assumptions through the scientific basis, through the scientific community, and once we have substantiated them, go back in and do a change control to our requirements document to make it a permanent change. If we find out that we need to readjust our assumption, we'll go back and change our assumption.

The control design assumptions really are in three categories, and I'll have a view graph that goes into it a little bit further, but we're really looking at the requirements section assumptions that we need to make in that area. We're looking at concept, or operational concept assumptions, and even some data assumptions to go forward with a design.

This is a view graph showing the technical documents hierarchy for the program, the different elements of the program, and, of course, us, in the MGDS area, with the MPC baseline change, we have implemented changes to these documents that basically say that the MPC will be the primary concept that we go forward with. We're in the process now of revamping our project technical documents to trace back up to the changes with the MPC.

This shows the control design assumption document in the cartoon that I just laid out, where we will go forward and make numerous assumptions to focus the design and go forward.

This is where we are now as far as the process. We've identified well over 1,000 assumptions that we want to go forward on. We want to divide those into key assumptions. The key assumptions are those assumptions that are important or may affect other program elements that we want to take and form specialist panels to help us assure that we come up with the best judgment call on these assumptions.

The ones that are not key assumptions, the M&O design team will go forward, list those assumptions, and there will be a control design assumption document.

If you look at what the outline looks like for the controlled design assumption document, again, we have three major elements of this: The requirement assumptions area, concept of operations area, and technical data assumptions area, and then there will be appendixes to look at the rationale for assumptions, substantiation tracking areas, and traceability matrices.

I'll talk a little bit about the schedule and where we are in the schedule. If you look at the key activities of the design, I've put some of the MPC milestones that have been talked about earlier. This is our controlled design assumption document. We hope to have the draft out by May, and a Rev shortly after. If we're looking here at the advanced conceptual design summary reports, we want to have one in September, which is our initial report, which will undergo a review to the requirements and to the assumptions, an interim one in '95, and a final advanced conceptual design report in September of '96.

A key part to all of this is the substantiation of the assumptions. We want to look at some initial plans by May, and do status checks along the way to assure that the assumptions that we have made are consistent with the data that's coming in.

I'll turn a little bit now and talk about some of the concepts and sketches that have changed from the SCP days, and that we are focusing on. We're basically looking at two multi-purpose canister designs; a large one, 125-ton design, which would have a basic 21 PWR MPC container, an inner containment barrier, and an outer containment barrier for the waste package part, a corrosion resistance, and a corrosion--I've lost the terms--corrosion allowance.

And for the smaller MPC, a 12 PWR, basically, the same concept. We have four basic waste packages that we're focusing on; the two MPC, and the defense high-level waste package, which is similar; again, an inner barrier and an outer barrier, with the four defense high-level waste containers; very similar in dimensions to the large MPC, and the need to go forward with an uncanistered spent fuel package that we may receive at the repository, not in an MPC.

At this point in time, some of the key assumptions--I just put these up here--that we're focusing in on for materials and the possible thicknesses for the different waste packages, the inner containment barrier and the outer containment barrier, and weights. We're looking at, again, the 21 PWR, I think, is around 125-ton, 75-ton, and Bob showed some concepts of the underground.

One of the drivers for the change to the exploratory studies facility is clearly the fact that the repository concepts are changing because of the need to go to larger waste packages, and now, the decision to go with the MPCs. We're looking at assuring that we can have a design that uses the flatter slopes for the large rail-mounted MPCs. That's the reason for the ramp changes in the ESF, and the fact that we now want to try to avoid some of the major faults, and, based on the latest information.

Again, these are just working concepts as we go forward. I wanted to point out that this was the repository SCP concept layout that had been altered because of the ESF alternative study. I'll show that with an overlay of where we are now, and, again, the--what Bob showed is the current working concept, which has a main area of--and will contain 70,000 MTU, I think, at 73kw/a, and an expansion area, a lower expansion area that Bob pointed out, that would contain at a lower thermal loading. I think it's 59kw/a.

This shows the--it doesn't show up very well in the handout, but the green is the SCP layout, and the blue overlay is the new layout, with the current working drafts to accommodate our focused ACD. Bob showed that the SCP emplacement area was on a, I think around a 4 per cent slope. We wanted these flattened out for the large, rail-mounted waste packages. That was one of the main drivers for some of the changes.

And if you look at the concept that we're carrying forward right now, of course, this will--this is just early concepts. They will be enhanced and changed as we learn through the iterative process. This shows a 21 PWR waste package on a rail cart, and a 14-foot high emplacement drift.

This is an area, enlarged area that shows the main surface area, and what we refer to right now as a TBM launch main, where you could launch TBMs, drive the emplacement drifts, and then come back around and re-launch for the next drift. These are shielded doors, shielded areas for doors. These are the 14-foot diameter drifts, and the large service mains and launch mains are the 25-foot diameter drifts.

Our current working concept right now is that the large waste packages would be transported underground in a transport cask that fully shields the waste package. They would be turned on a turntable, lined up with the emplacement drift, and pushed into the drift. The shield door would be closed, and the transporter rotated and taken back to service for another one.

And, depending on the thermal loading, you could remotely take a locomotive, then, and place these cars at whatever distance you wanted in the emplacement drifts. Again, these are just our working concepts right now. They'll go through changes as we go forward.

I think that kind of concludes what I had. Are there some questions? Steve has the thermal loading part that he will continue on with.

DR. DOMENICO: We'll take some questions now. Any questions from the Board?

DR. PRICE: How many people will be working underground on this? Do you have a concept on that?

MR. STUCKER: I think we're in such an early working stage that we really haven't outlayed it to that extent. I might ask Kal if he has any working numbers.

DR. PRICE: And could I add, in addition to that, how do you cope with the heat with the people in there, and so forth?

DR. BHATTACHARYYA: Okay. This is Kal Bhattacharyya.

As Dean said, we haven't gotten down to the head counts yet. The concept that we are working with assumes that once you are in that emplacement area, it will be remotely operated, so nobody, no human being is going to be allowed into any of these emplacement drifts. Once that waste transporter comes in and rotates, from then on, we assume that everything is going to be automatic. The door will open automatically. The locomotive's pretty straightforward to pick it up automatically and push them back, close the door, and then, and then only, a person can come up.

The transporter itself would be fully shielded to a Department of Transportation-type shielding level, so that could go up and down the main ramp and up to the surface.

DR. PRICE: And protection from just heat?

DR. BHATTACHARYYA: The heat issue, that while we are emplacing, we are, of course, fully ventilating these emplacement drifts, so the temperature really shouldn't rise so fast. You are emplacing from the back on, and in the automatic mode, so no human being is in there.

Once one drift is emplaced, then you close the door. There is a setback distance of some 30 meters from the nearest emplacement, nearest--that launch main, you know, so temperature really should not ever rise very much into the access drift, as a matter of fact. You're look at a maximum rise of 50°C.

DR. PRICE: And this operating concept that you're describing now is--was that part of the life cycle costs that were involved in any system tradeoffs when you were looking at the MPC versus other types of operations?

DR. BHATTACHARYYA: No. We have not done any peer services under this concept.

DR. PRICE: Do you intend to do that kind of life cycle costing and tradeoff comparisons and system studies?

DR. BHATTACHARYYA: I believe we do because, you know, this ACD, one of the reasons for doing the ACD is to contribute to the TSLCC. [total system life cycle cost]

MR. STUCKER: The answer is, definitely, we will. If you look at the schedule that I had up, I didn't really get into the lower elements, the elements that we would impact, but, clearly, total life cycle costs would be re-looked at at the end of the advanced conceptual design when we have detailed cost estimates available.

DR. PRICE: In terms of tradeoffs with alternatives, alternative concepts of operations?

MR. STUCKER: From a systems point, I'm sure, along the way, we'll be looking at those tradeoff studies, yes.

DR. PRICE: But, at this point, that has not occurred?

MR. STUCKER: That has not occurred.

DR. PRICE: So that part of the systems studies is incomplete?

MR. STUCKER: I would--yes, it is, and the reason, we're in the very early stages of the advanced conceptual design, and we're limited, funding-wise, to a focused approach at this point. Next year's funding, we plan to look at the trade studies and expand on that.

DR. SATERLIE: Let me see if I can shed a little light on that.

DR. DOMENICO: Can you identify yourself?

DR. SATERLIE: This is Steve Saterlie, yes, and on the systems studies that we did last year, we had some preliminary estimates of cost of the various aspects as best we understood them at the time; the operational concepts, the costs of that part of it, and, admittedly, they were fairly crude.

Now, as we develop more details about those costs--and this year, in particular, we're factoring more of those details in, and we are, on the various options that we're looking at, we are comparing those costs, so some of that is getting done, but, admittedly, it's a maturing--we have until some of these concepts mature to get a good estimate of the cost.

DR. PRICE: Well, we, just by way of comment, felt you are off to a start, but that there was much more to do, and this is, perhaps, an indication of some of that much more that there is to do in systems studies.

DR. DOMENICO: Is that--

DR. PRICE: I have another question, if I may, while I've got hold of the mike, and then I can let go of it, and that is: Has this concept that you're presently describing considered the fillers issue and how it might be handled with respect to not only operating at the repository, but as it bumps up throughout the whole system?

MR. STUCKER: Currently, we're looking at the assumptions we would carry forward right now, that the--with the assumption that the design would have to allow to add filler materials at the repository, so a lot of the key assumptions that we're going forward with allow us those flexibilities until we come up with the trade studies along the design process, where we could back off. So, we're kind of bounding the cases on a lot of those areas.

DR. DOMENICO: Ed, you had a question?

DR. CORDING: Just a quick question on the TBM launch main. Would that be used to remove the muck, or would they be taking it out of the service main? You had a picture up there just before, Dean.

MR. STUCKER: Yeah, I sure did. In fact, I may have a--what we're looking at is--and I didn't bring other drawings to show that enhanced.

This shows the launching of a TBM, but these mains would be used, the service mains would be used to remove the muck, railed back to a central point, to a conveyor; a central point for conveyor removal.

DR. CORDING: Thanks.

DR. DOMENICO: Any further questions from the Board? Staff? Carl?

DR. DiBELLA: Carl DiBella, Staff.

I'm sort of curious under this concept how you're going to carry out a performance monitoring program, which is, we understand now, going to be 100 years in length, rather than 50, and how you're going to fix things when they go wrong in there. Could you explain a little bit?

MR. STUCKER: Yeah. I probably need to clarify that this--the focused ACD approach that you see here is based on a recent MPC decision, and it does not incorporate any changes that we might have if we do incorporate Scenario A. So, we need to look at what Scenario A impacts would have on this plan, but they are not incorporated in here.

I think they'd be minimal at this point in time, but things like retrieval, right now, we're looking at a 50-year retrieval period. If we went with Scenario A, we're looking at a 100-year retrieval period, and how would you monitor and look at that may have some impacts back on the design. They clearly affect the assumptions that we're going forward on.

DR. DOMENICO: Bill, do you have questions?

DR. BARNARD: Yeah. Bill Barnard; Board staff.

Dean, as I understand it, once the radiation doors are closed, you aren't ventilating the tunnel; is that correct?

MR. STUCKER: That's our current concept, is that once the door is closed and the waste package is emplaced, it would--we wouldn't open it back up unless you wanted to go in and retrieve, pull it back out.

DR. BARNARD: And could you retrieve that waste if the temperatures had increased to 200°C?

MR. STUCKER: We would have to look at what would be needed. Again, that's why I pointed out, this is very early conceptual, what we would have to do for blast cooling, or dropping the temperature down in a drift to go back in, we'd have to look at, and we'd have to look at--part of the assumptions that we're going forward on is developing a retrieval strategy. We are basically at the final part of developing a retrieval strategy. We need to look at what that strategy assumption would do with this concept, and how we might have to adjust the concept then.

DR. DOMENICO: Don?

DR. LANGMUIR: Langmuir; Board.

Related question, Dean. I would assume you must have some sort of remote monitoring for radioactivity set up. Once you close the door, there has to be some way to establish there's been a leak or not a leak if you're going to open it up again. I would assume that's being monitoring remotely. Is that part of your plan?

MR. STUCKER: I would assume that the design would have some monitoring. I don't know the details of what that monitoring is. Kal, you may have some idea of what the concept is at this point.

DR. BHATTACHARYYA: This is Kal Bhattacharyya.

Depending on the thermal loading, as a matter of fact--and Steve Saterlie is one of the people who is looking at the survivability of monitoring instruments, you know, in a given temperature, as a matter of fact. Assuming that the monitoring, assuming it will survive at the thermal load that they choose. Nothing says that it cannot actually do it. It can do it, you know, electrical cables, or whatever, around the door. The door itself doesn't have to be, you know, like a airtight, sealed type of door.

From my limited knowledge of radiation, I understand it is more of a shield as opposed to, you know, an airtight thing, so we could produce a mechanism, you know, which would allow you to go around or over, through some sort of conduit to the door, and monitor the emplacement, or the waste packages.

DR. DOMENICO: I think we'd better move on to the thermal loading part of this presentation.

DR. SATERLIE: All right. I want to thank you for this opportunity to give you a very cursory look at the update on the thermal loading study. Unfortunately, due to the lack of time, I haven't provided, really, any technical details of the study, and I'd certainly be happy to do that, if the Board is interested, at a later time.

So, this is the FY93 thermal loading study. The objectives of that study, as I think we talked about last July, are really to place bounds on there, if we could, to determine what was too hot, and possibly too cold; to grade or evaluate the thermal loading against performance criteria, and I talked to you about the SCP thermal goals and the reevaluation that we've done on those goals, and that was primarily what we'd used in those evaluations, although the total systems performance work was also incorporated.

In the system study, we wanted to, besides performance, we wanted to look at cost, safety, and operability, and, as I said, we did some work on the cost, but many of those concepts are still maturing, and we have to, you know, do more at getting the details of those costs.

Finally, we wanted to identify uncertainties, and translate that into what we need to do in future work, and, possibly, into the test programs.

The status of the program is that the work has--the study's been completed. The report is written. In fact, Russ McFarland has been given a copy of that report. I don't know if you've had a chance to see it, the draft report. It had a fairly extensive M&O technical and management review. DOE has completed their management review, and, at this point in time, we're ready to send it out to the participants for a broader review. We want their input.

Now, many of the folks that were involved specifically in the program and contributed, they saw their portions of it, but they didn't see the whole report, and that's what we want to do now, is give them, plus others, like USGS, an opportunity to comment, and we'll have a comment resolution process there.

The study integrated the activities of a wide variety of people. We managed the study, but involved in it and very integral to that activity were the design groups, the waste package people the subsurface, surface. Lawrence Livermore did some of the hydrothermal predictions for us. Sandia did some thermal predictions. Los Alamos looked at the geochemistry.

The point of this was that we wanted to make sure that all of these folks doing the predictions were using the same inputs, the same subsurface designs, waste package sizes, and the waste stream assumptions so that we could do comparisons across the board.

The study itself looked at a variety of parameters, three different waste package sizes, all the way from a 6 to a 21 PWR size capacity, although the BWRs I didn't put on here, but they're similar. I think it's 42 for the 21, and 21 for the 12, and 12 for the 6. Anyway, those size waste packages were looked at.

The three emplacement modes, vertical borehole, in-drift, and horizontal were looked at, and five thermal loads were chosen. These five thermal loads went from 24 to 111 MTU/acre. Primarily, the AML affects the post-closure, the mountain scale kinds of performance, and I've also put on here for the fuel that we used, the areal power density that it would be commensurate with, and that primarily affects the pre-closure or the near-field kinds of environment.

The lower thermal loads here, 36 and below, were chosen because the 36--above 36 becomes above boiling on the bulk average. The other choices we can talk about, if you're interested.

The evaluation itself, because of the kinds of calculations that were done, it turned out there was about eight of the thermal goals that we could evaluate this performance against. We couldn't do all of the goals. As you're aware, there's about 15 of them, because some of the boundary conditions were such that we couldn't make predictions that were accurate in that range, like the surface, or other aspects.

We also included some look at monitoring. We found some quantitative data about off-the shelf instruments, and where those might start to fail, so we included that into that issue as well.

The results. What we found when we did this comparison is that the 111 MTU/acre case, essentially all the goals except a couple were violated, and then when we did comparisons, the predictions at 83 and 111 were well-behaved enough that we could make interpolations between those, and we found that even down to about 100, all those same goals were violated, and so that was the reason for the choice of 100 as being too hot--above that as being too hot.

So, let me close with the key findings of the study. There was actually a number of findings, and I'd refer you to the report, or to a later briefing to discuss them, but, primarily, the study found that above 100 MTU/acre, that there was sufficient technical basis to say that that was too hot.

Looking at the geochemical aspect of the problem, Los Alamos concluded that as you increase the thermal load, the geochemical alterations, the uncertainty of those increases. That's probably not too startling.

Below boiling, on the bulk average in the repository, what was found is that for bulk permeabilities of a Darcy or below, that, essentially, there was negligible mountain-scale hydrologic perturbations. The water movement was negligible on a mountain scale. That isn't to say that there wasn't some local water movement that could occur in the drift scale.

Electronic components have high failure rates, it turns out, about 160° and above, the failures are dramatic, and so, monitoring was not really feasible or practical above about 100 MTU/acre. We're going to look at that in more detail this year, and we're going to get some more information on those particular aspects of it.

A number of uncertainties were determined. These still remain, and we're looking at those in more detail. In this year's study, we're trying to do some sensitivity analysis to determine where the break points really are on some of these parameters, and I guess, you know, many of these are no surprise. We've known these, in many cases, for some time, but we now have some technical basis.

For example, bulk permeability, we did a preliminary sensitivity study and found that above a Darcy, there are some significant changes, so we have to factor that into the test program to determine over what range we're going to be measuring these particular parameters.

Corrosion rates, of course, are still an uncertainty. Fuel variability is one thing that we want to look at. We just did the study with average fuel. We now have to look at the variations in the fuel that we're getting, and how hot--are we going to have hot spots and cold spots in this thing, and what does that imply about the performance?

Cost, we have to factor, as I said earlier, we have to factor in some of these maturing concepts and the cost of those particular aspects of the problem.

Geochemistry, percolation, and thermo-mechanical effects, we did a very preliminary thermo-mechanical analysis, and we're in the process of doing a better job on that.

There were some questions yesterday from the Board about ventilation and retrieval, and I just wanted to say that we have been working closely with the subsurface folks on the ventilation aspect. There was some preliminary work that's in the report on ventilation, and we plan to update that and improve that in this year's study.

This study, by the way, is--there's also a couple of other studies that are going on, and one of those system studies is a retrieval study this year, and so, the retrieval aspect and, hopefully, some, at least, maybe in a -- order, the costs of those retrieval aspects are going to be looked at for the different options.

So, more to follow, I guess, as the studies proceed, and at this point in time, I guess I'll quit and take questions.

DR. DOMENICO: Board questions?

DR. LANGMUIR: Langmuir; Board.

Just wondering what age of fuel, on average, would give you that 100 MTU/acre figure that now seems to be the maximum allowable?

DR. SATERLIE: Age. Well, of course, this study, the age was a 22.5-year for the PWR and it was 23.2, I think, for the BWR, years out of a reactor, and so what it was looking at was the density of how you place that fuel and place the package spacing and drift spacing, so I guess I'm not sure I understand your question, maybe.

DR. DOMENICO: Warner?

DR. NORTH: Warner North.

Could you clarify on the last slide, that I'm not sure you showed, evaluation of thermal criteria using system study results? There's a Footnote No. 1 there I'd like to understand.

DR. SATERLIE: That was a backup slide, by the way. The footnote right here is the goal for the vertical borehole, and the 6 PWR, and what it says is, we looked at some work that Sandia had done on a variety of different packages sizes, heat outputs, and found that to meet the goal of 200°C wall temperature, we had to have less than about a 5.2 kw package, which, for the fuel that we used, translates roughly to about a 9 PWR-sized package, so the 12 and 21 wouldn't meet this goal and, therefore, they weren't evaluated in the vertical borehole concept.

DR. NORTH: Thank you.

DR. SATERLIE: Okay. By the way, let me caution you, since you do have this in your package, please don't add those up to get a total value, because we did not--these goals don't necessarily have the same weight and, in fact, we have not assigned weights to those, so let me caution you not to add those up. The report is very clear about that.

DR. DOMENICO: Ed, do you have a question?

DR. CORDING: Ed Cording.

On your 111 loading there, I just was--my question is, the above boiling concept--I guess, let me rephrase it.

The long-term above boiling concept, the 10,000 years, and the long dryout period, is that concept going to require something like 111, and is that concept still one that's being considered?

DR. SATERLIE: Well, yeah. The predictions--of course, what we're talking about there is the predictions, the model predictions that indicate that if you put enough heat into the mountain, you can drive the moisture to a low enough level, somewhere around 10 per cent relative humidity or below, and that you can drive it away for a long enough time, and that when the heat decays, it takes even longer for the moisture, then, to come back in, and so, the issue there, or the belief is that you can, in fact, keep the moisture away from the packages for a significant period of time.

That's one of the things that we're looking at. The concerns we, of course, have is does the mountain behave like the models really say they behave? Do you get refluxing? Are there portions of the repository; for example, the edges, where you may, in fact, degrade faster, and all of these questions we're looking at in more detail as we go along and, of course, many of them are going to have to wait until we get some test data to really substantiate some of these aspects.

I don't know if that answered your question.

DR. CORDING: The other part of the question is--not entirely, because the other part of the question was if you go above 100, will that long-term boiling concept, or above boiling concept and long dryout period concept require you to go above 100 MTU/acre?

DR. SATERLIE: Okay, I'm sorry. The predictions that have been made show that at the 83 MTU/acre, that you can get substantial dryout, at least in the repository center, and so, you know, the 80 to 100 range is certainly a candidate for that kind of dryout.

DR. CORDING: I guess one other point was we had some discussions last year--I don't recall when, but the discussion was that there would be a possibility that some of the criteria for temperature at drift walls or borehole walls, things like that, were being reconsidered; that some of the criteria were felt not to be as critical as others, and I was wondering if that's been part of what you've been looking at, also?

DR. SATERLIE: Well, as I said, we used the criteria that were in that SCP thermal goals reevaluation report specifically, and we're now, when we did that effort with DOE sponsorship and support, there was the conscious decision that we should, as test data became available and more analysis became available, that we should routinely re-look at those thermal goals, and we've been having conversations with DOE about this, and there is the feeling that we're probably at the point where we ought to look at those again, and this coming fiscal year may be an opportune time to do that.

But, I can't answer any more than that, because I don't know if that will really be approved or not, and--but there is an attempt to do it.

DR. CORDING: You're working off of some modification to the SCP original plan. The original temperatures described previously, you're working off something, what, in the last two years? Is that--

DR. SATERLIE: Right. There was a reevaluation done, and many of those goals stayed the same, but there was a goal added about drift wall temperature less than 200°C. That was based on some thermo-mechanical work that had been done, and some borehole samples that had been put under stress.

There was another goal that's not on here that we couldn't evaluate; that we found that the surface temperature of 6°C had been probably established in error, or non-conservatively, and so we recommended that that be dropped to 2°C change in temperature, so there were some of those kinds of things, but, for the most part...

DR. DOMENICO: Dennis, did you have a question? Oh, did you? Well, go ahead.

DR. VERINK: In looking at the evaluation table, does the table suggest that the 83 MTU/acre is unsatisfactory, looking at the bottom part of the chart?

DR. SATERLIE: Looking at the bottom part of the chart, no, not necessarily. As I said, we have to make a determination of which of these thermal goals are more important.

What this just said is that this is a more challenging environment, and there are some questions about that, but if, for example, this TSw3, we find that that goal may not be really that important of a goal because of the zeolites are not--may not do the job that we had hoped they did, or that they may not be as uniformly distributed across the repository as we had hoped, then that goal may be of lesser consequence.

So, no, I don't think that's--all I want to say at this point in time is the study showed that above 100 is too hot, and it doesn't say that any in these range necessarily meet the requirements or exceed the requirements, but we're looking at that further.

DR. VERINK: That's 100C.

DR. SATERLIE: I'm sorry, Ellis. What was--

DR. VERINK: You said more than 100.

DR. SATERLIE: 100 MTU/acre.

DR. VERINK: MTU, okay.

DR. SATERLIE: I'm sorry, more than 100 MTU/acre.

DR. VERINK: Okay.

DR. DOMENICO: John?

DR. CANTLON: Yeah. Cantlon; Board.

Were there any elements in your calculations looking at surface variability? Tom Buscheck's models, as you're probably aware, indicate that if there is high permeability in fracture continuity areas, you're going to get big temperature changes at the surface, not the model 2° rise, but as much as 25° rise.

Any surface impacts looked at in any of this modeling?

DR. SATERLIE: Well, I discussed that specific question with both Tom Buscheck and Sandia, and the problem that we all had was that those models have a boundary condition at the surface which would not, in fact, allow us to determine what the temperature rise was there, or even, really, a meter below the surface.

Now, we can make some estimates, and we'll maybe look at that a little bit more this year, make some estimates from the gradients, the temperature gradients that we see in those upper layers, but it was not, as I said, we couldn't, from the models that we used, conclude anything about that surface temperature rise.

DR. CANTLON: A follow-up question. The temperature variability from point to point on the surface is one variable. The other element of his modeling is a long-term, 10,000-year possibility of major moisture transfer to the surface, which, in the desert, is a fairly interesting phenomenon ecologically.

DR. SATERLIE: Yeah. The predictions do show a significant transport of water, and then recondensation levels at--right below the Paintbrush Tuff layer for the permeabilities that we've chosen there, so that is certainly a possibility for the high thermal loads.

DR. CANTLON: Yeah. It may not be significant to the repository, but it's something, clearly, some of your critics are going to want to know something about.

DR. LANGMUIR: Langmuir; Board.

Just curious, Steve. If we go to the 100-year retrievability, is the only impact that you would see in your table deal with access drift temperature? Is that the only thing that's going to be affected in terms of evaluating the appropriateness of these choices of MTU?

DR. SATERLIE: Well, we would have to look more in depth at the ventilation concepts that would be able to achieve that retrievability. You know, if we're planning to retrieve a single drift, you'd get one answer. If you're planning to retrieve all of the waste, you'd have to have a more complex ventilation system, and that would have to be factored in there, so I don't know, did that--I maybe didn't answer your question exactly.

DR. DOMENICO: Dennis?

DR. PRICE: I wasn't going to ask this follow-up, but I think I need to.

On the first row, you've got 1 meter rock temperature in boreholes, and you answered it in terms of that Footnote 1, that came from a vertical emplacement study. Can 12 and 21 PWR be placed in-drift without violating the 1 meter rock temperature?

DR. SATERLIE: Okay. Well, for that one, we used this different goal, which is a drift wall temperature of 200°C, and the reason for that was to keep the temperature gradients in the wall below a point where the thermo-mechanical predictions were indicating that we might get some large-scale rock failure.

And so, yes, the 21 PWR at the various thermal loads, up through 83, as you see, meets that goal.

DR. PRICE: And if it were horizontal, we would be up on the top row again, and the footnote would prevail? 12 and 21 would probably not work in a horizontal?

DR. SATERLIE: If the horizontal drift were of a small size so that there's variable space, yeah. This was just--

meets this if you have a, either a 4.3 meter drift or 7 meter drift, either the 14 foot or the 25 foot.

DR. PRICE: And does drift filler make any difference with respect to that?

DR. SATERLIE: That is one assumption--we did not look at backfill in this thing, and that certainly changes things, because it will increase the temperatures in the waste package dramatically. You're going to exceed this 350°C temperature, most likely, for the typical types of waste that you have in there, and you could, over a short period of time, possibly exceed that temperature, but that is an aspect that we did not look at in this particular phase of the operation.

DR. PRICE: But that doesn't mean to imply that backfill is out of the picture if you go that way, but it means that at a later time, you would be able to backfill, or what does it mean?

DR. SATERLIE: Well, yeah, particularly now that if, in Scenario A, we're looking at a 100-year type of period, we might not backfill until 100 years, and, at that time, the waste would have decayed sufficiently; at least, you know, I suspect sufficiently. I've done some calculations that show it should be considerably cooler, and so that would cut that thermal spike out, and, yes, backfill is still a consideration. There's the possibility that it might be used as a drip shield or moisture barrier.

DR. DOMENICO: Ed, you had a question?

DR. CORDING: In regard to the below boiling loadings, the finding says that they produce negligible mountain-scale hydrologic perturbations. Is that conclusion such that we're--are you stretching that to say that there isn't significant moisture effects from heating that we need to investigate further at those lower temperatures? Have you--does this take into account the information that's been being generated, analyses on boiling effects, vapor flow and all those things, even at below boiling temperatures?

How much more do we need to know, in other words, about thermal effects, even at the below boiling levels?

DR. SATERLIE: We certainly need to obtain test data that will demonstrate or verify any of these conclusions. The buoyant, the convective scale processes seem to be working, according to the prediction, at higher permeabilities than the one Darcy, and that's why I qualified that statement with the one Darcy level.

What we're doing this year, which is, I believe, very important to do, is look at the drift scale water movement, because that part and its interaction with the packages and the corrosion part of it is something that we need to understand better, and so, we are looking at the drift scale water movement in those low thermal loads this year.

DR. DOMENICO: Any further Board questions? Staff?

(No audible response.)

DR. DOMENICO: Well, let's now open this up to public-- oh, thank you, first.

I presume we could open this up to public comment now, and I think the public can ask anything they want of anybody they have heard today.

Step up to the microphone and state your name.

MR. CHESTNUT: Dwayne Chestnut, Lawrence Livermore.

I'd just like to emphasize a point on this boiling convection question, since it seemed to capture quite a bit of attention.

I think it's very important to keep in mind that that's extremely sensitive to the average bulk permeability in the vertical direction of the mountain, and there are some data scattered around that suggest permeabilities considerably greater than one Darcy may, in fact, exist.

Some of this comes from the containment program of underground testing, where there have been direct measurements made of barometric pressure fluctuations, and these gives numbers of the order of 10 to 50 Darcies over significant vertical distances.

Some other information from the Calico Hills pump test data in the saturated zone, the modal permeability tends to be about two-tenths of a Darcy, but it's a log normal distribution, with a standard deviation of about two, which means that you have substantially high percentage of the total permeability measurements above 10 Darcies. So, I think it's important.

The other thing is that as far as the high thermal, or high mass loading density is concerned, we need to keep in mind that this study was done with a fixed age of fuel, and if you age the fuel longer, you can go to the higher thermal loadings, probably still meet some of these thermal goals, and still have the possibility of the extended dryout. So, I think the question's still open on a few of these questions.

MR. BLANCHARD: This is Max Blanchard with the Department of Energy, Yucca Mountain Project Office.

I'd like to ask the Board to begin focusing on the use of the word "suitability" with respect to future conversations and briefings that people present to the Board.

It may not be your perception, but at least it's my perception that what's been happening over the last year or so is two uses of the word "suitability;" one which focuses on the term, "suitability" with respect to Section 114 of the Nuclear Waste Policy Act; another with respect to the penultimate question, that being should the site be closed, which is a licensing question.

And, oftentimes people in conversations, sometimes in the hallways, sometimes here in the room and other places, although they seem to think they're talking to each other, they talk past each other. The point is, it would be of benefit to everybody, those who know this, and those who maybe knew it and forgot it, or haven't quite studied the law, to make sure that when we refer to suitability, we're referring to not the penultimate question, but something that's embodied in the Waste Policy Act that was meant to be a preliminary decision, a measure of increased confidence, that Congress should get prepared and be willing to spend the large amount of money to construct a repository.

The suitability issue is one where the Department Secretary makes that decision on his or her own basis, using a set of criteria that were called the guidelines. To be sure, in doing that, the Department will be seeking information from lots of oversight bodies, but, in the end, there's going to be a risk in that, because all of the answers to the questions that we need to have over the long term won't be there. There won't be any empirical information to determine whether or not the models we've used are really, indeed, valid.

To be sure, there's a question of suitability implied in licensing, but the licensing question comes in three parts: One, whether or not, not the Department makes the decision, but whether another federal agency, going through a formal licensing proceeding, can decide it's willing to grant an authorization to construct, and then, perhaps, ten years later, is that same agency willing to grant an authorization to receive and possess, and then, some 50-75 or 100 years later, is that same agency willing to grant an authorization to close?

At that point, there's a need for an answer for the penultimate question with respect to the 10,000-year suitability of the site, and it's not just the site, it's the engineered barriers, too. So, it seems to me there's an issue that we need to try to clarify in the conversations about site suitability, because I know that in some cases, conversations have drifted into discussing things about disqualifiers, or the suitability of the site, when, really, the penultimate question, as opposed to something which is a measure of increased confidence that indicates we should be spending the money to go ahead and construct.

In the area of disqualifiers, the disqualifiers are what they are in the guidelines, and, to be sure, other things could cause us to say, "This site's not suitable." Either the Department could, on its own, or, by the NRC saying, "We're not going to license this site given the conditions of the site, or the design of the engineered barriers." That also means the site is unsuitable.

But, it would be preferable if we refrain from using the word "disqualifiers" in a way other than that which pertains to 10 CFR 960, given the way the Waste Policy Act intended for 10 CFR 960 to be used.

I don't know how often you all have paid attention to this confusion, but I've noticed it drifting in over the last year or so, and in some arenas, not particularly this arena, but in other arenas, conversations are rather free-flowing with respect to confusing site suitability as a preliminary measure of confidence about moving forward, versus can you get a license on this site to close the repository.

DR. DOMENICO: Thank you, Max.

Does anybody on the Board wish to address that statement? Warner?

DR. NORTH: Warner North.

I think your point is very well taken. I think many of us are concerned about it, too. I think it would be useful if we reserve the word "suitability" to refer to 10 CFR 960, and use other words for the ability for the site to be approved for a license.

Now, given that we're beginning to go into discussion of phased licensing concepts, I'm no longer sure I know what that means, and I'm certainly as uncertain as everybody else as to what the new standard is going to be coming out of the National Academy review.

I have an even more difficult time thinking about a process 100 years from now that's going to lead to a decision to close the site. All of this leads me to reiterate my concern about the need to do an assessment of the risk that we may be wrong as we take the next steps. What is it going to cost, and what are the technical difficulties involved of retrieving waste, given that we put waste in Yucca Mountain?

It seems to me that issue really needs to be dealt with. I'm glad you're thinking about it, but I'm a bit disappointed that you don't have more to say about that issue at this point. I think that before we get into discussions of suitability against 10 CFR 960, you really need to answer those questions. Is there a way that, if we find something 50 years or 100 years from now that indicates the repository is not a good long-term place for disposal, that the waste can be retrieved with an acceptable level of cost and acceptable difficulty?

DR. DOMENICO: Dennis?

DR. PRICE: While we're talking semantics, it gives me the opportunity to make a comment about suitability with respect to some of the discussion yesterday.

I had a feeling inside me, and I didn't make the comment yesterday, but I'll make it today, that siting success was synonymous with building and operating a facility at a site. I would say the siting's success could occur when you reject a site, because the process was appropriate, and perhaps timely, and I think, also, as we look at this phased licensing, we have to be careful about chewing up a lot of time in which, maybe in the end, you come to a decision that this site should be rejected. That is just a caution. I think we could be successful by rejecting if it warrants it.

DR. DOMENICO: Does anybody in the audience wish to address--

MS. TREICHEL: I don't usually say anything in technical exchanges, but there was an exchange today that sort of connected what went on yesterday, and, also, what was going on today, and it was after Jean's presentation, when she was talking about groundwater travel time, and about site suitability, and so forth, and she and Steve Frishman had a discussion or an exchange concerning what her presentation was, and at the end of that, Steve Brocoum stood up and talked about the 960 guideline, and that that would be brought up at a May 21st meeting.

And I've been, for a long time, working on a very frustrating exercise concerning that May 21st meeting, which we were all told was going to be a large--well, Alan Benson referred to it as a, "Y'all come," and that was his way of saying it was a public stakeholder meeting, and we were sort of led to believe that during that meeting there would be a discussion about methodology for determining suitability--if we can use that word--about how the methodologies are used, about the process, about when it's appropriate to change, if it's appropriate to change, how you change guidelines.

And it seems that with what Steve Brocoum said today, that meeting suddenly is a new arena, and there's much more intent to interpret or re-interpret that 960 guideline, and to have an audience, because that meeting is consecutive to--or is lined up with the international conference, where you're going to have a tremendous number of people with the nuclear industry, with Department of Energy, with their consultants, with their contractors there. It's being held in the same place. It follows a Federal Register notice. The public has very little contact with the Federal Register; that there is going to be a 960 discussion at that meeting, and I'm not sure what will come back from that, but it may be used to create some sort of a mandate that would have to do with these guidelines, and have very little to do with public opinion or any sort of way that the public can be involved.

And there is, right now, a series of letters going back and forth between Alan Benson and myself concerning the public's right, or the public's ability to bring in consultants or representatives from public interest groups to be able to be at a meeting like that, because there is going to be a lot of representation on the other side.

And, so, I just want to bring that up to you because so many times, the public is seen as sort of a irritation element in this whole thing, and, from what we heard yesterday, that's real counterproductive, and it takes away trust and confidence, it makes all sorts of problems.

But these guidelines have been here, and they were what was used to sort of encourage and give Congress the impression that Yucca Mountain was the great place to go, and we constantly hear about the way we got to Yucca Mountain, with the 9, the 5, the 3, the 1 site, and those were the guidelines used, and it's been nine or ten years that those have been used, and suddenly, we're coming up to a meeting where I'm not sure how that meeting will go, but it's not, I don't think, what you would call a public stakeholder meeting in the way that we're seeing it coming together with the agenda.

Thank you.

DR. DOMENICO: Warner?

DR. NORTH: I think we'd want to recognize Senator Hickey.

SENATOR HICKEY: I suppose I would like to address the Board and Dean Stucker. This deals with the waste package design, and we've always talked about an integrated design and integrated management.

What I don't see, and I don't quite understand--perhaps you can address it--deals with the movement of this package, the delivery of this package to the site from wherever that may be, and why I think that perhaps should be addressed has to do with the exposure along the routes. It has to do with the travel of that package. It has to do with the safety. The weakest link in that package is the equipment that will be carrying it, and I don't see that addressed in this presentation. I wonder where you see that fitting in. Maybe it's a timing thing.

MR. STUCKER: I might put up a view graph here that would show the timing and the area of responsibility for that.

If we look at the technical document hierarchy that I showed earlier, clearly, there is a program element that's the transportation part of the program that has specific requirements that they are developing for the transportation aspect, and to answer your question, it really needs to be addressed by that part of the program.

For the MGDS part of the program, we do have the responsibility for the link within the State of Nevada, and there have been some preliminary studies done within the State of Nevada, but the overall transportation aspect is under the responsibility of the transportation part of the program.

SENATOR HICKEY: This may affect your whole program and size of the package, and I think it's a part of your delivery system, and that being a part of your delivery system deals with how fast this mobile unit will move at this weight. It should be an integrated part in the delivery of this package, not a separate system. That's all I'm addressing.

What you're addressing here, you're saying, "Well, it's part of a different system."

MR. STUCKER: It has to be part of an overall integrated system, and the studies that led to the MPC decision addressed parts of the transportation, storage, and store elements. I showed on one view graph that 10 CFR 71, 10 CFR 72 were addressed in those early studies that came to a conclusion that led to the MPC, and I agree with you, it has to be an overall system element that you look at.

SENATOR HICKEY: This could affect your, at least in my view, the weight that you're going to move it at, at what speed, and what--that has to be addressed, and what I don't see is that integration of people that have an understanding of movement of that kind of equipment, and that's why I bring it to your attention.

DR. DOMENICO: Don't go away, Dean.

MR. FRISHMAN: I think I know what's concerning Senator Hickey a little bit, because I picked it up, too, and that's that there clearly, to use Dean's word from his presentation, the SCP is being redesigned based on a decision to employ the MPS. That's the assumption on that redesign.

Well, my knowledge, and I believe Senator Hickey's knowledge, and a lot of other people's knowledge of the program is that the decision has not been made to deploy the MPS, or MPC. So, I think that's where the trap is coming, where you're redesigning the SCP, and you're, in fact, probably going to build it based to that redesign, if things go the way you want, and it's being redesigned on the basis of a decision that has not been made.

I was curious about that, too. Am I wrong? Has the MPC been adopted?

MR. STUCKER: The MPC decision to baseline our program for requirements to go forward on what the concepts would look like has been made. The decision to deploy it will be made at a later date. You need to look at what the concepts are, and if the overall system works before you make that final decision, but, clearly, we have made a decision to baseline our requirements that focus us to look at a MPC-based program.

MR. FRISHMAN: All right. So, the SCP baseline now is MPC?

MR. STUCKER: I would say the technical baseline has a primary MPC basis to it, and that's why we are going forward to replace the conceptual design in the site characterization plan, to update it to be consistent with what the program requirements are now.

MR. FRISHMAN: Are you doing any type of analysis with that that supposes, or that looks at the possibility that you do not have the MPC available, and you have re-done the SCP in accord with an MPC? I love speaking all these alphabets.

That was a question that occurred to me, you know. Do you lose or gain something relative, for instance, to vertical emplacement if you go to this new design and then don't use the MPC? It seems to me you've got a lot at stake in that design.

MR. STUCKER: There is definitely risk, and our program management has pointed out that there is risk, but that risk is manageable, and we feel that, in the overall picture, in the system's concept, that is the approach that we have to go forward on.

DR. DOMENICO: Did the Board have anything on this?

DR. PRICE: Just a follow-up on Senator Hickey's comment, and with a question to DOE.

As I understand Scenario A, and about April 15th, I want to call it Schedule A, but Scenario A does take a minimum funding approach to transportation for the near future. They're taking, actually, funds away from transportation and transportation studies for other purposes.

Is that a correct, a fair interpretation?

MR. BLANCHARD: This is Max Blanchard with the Department.

The evolution of the transportation system that goes with interim storage, using the MPCs, and delivery of the MPCs to the repository is not very well thought out yet, and how that's going to evolve into a much larger role in the program is yet to occur, and it's clear from a systems standpoint, that that's a piece of the component that needs increased funding and increased level of effort.

It's downstream as we mature the concept of so-called Scenario A with respect to how to address your question.

DR. NORTH: Not seeing anyone else in the audience who is eager to make a parting speech, I'll add a few words of my own to second what I've heard in the last few minutes, and try to tie it to yesterday's discussions.

I'll start by noting the last line on the last slide of Scenario A, abbreviated top level strategy at the bottom. "Conduct stakeholder interactions throughout process."

I think what we are now seeing are major changes in the basic strategy of the program that have been proposed, and seem to have some momentum toward being adopted. Maybe it is seen that this is basically the only way the program is going to make it, given what they are going to get in the way of funding and support from Congress in the near term.

Nonetheless, lots of representation has been made by the program in the past based on the old SCP and based on the system as it was conceived at that time. It seems to me utterly critical--and we had almost a full day of example yesterday--that the program interacts early and often with the affected public, particularly in the State of Nevada.

And, I must say, if the idea of the program and implementing the draft public involvement policy that we were given an opportunity to see and comment on on about Christmas time, is to hold one meeting on May 21st, at the time when all the folks in the nuclear waste community have come in for a conference, it strikes me that the Department is badly missing an opportunity to have much more substantive dialogue about these changes in the program that it sees highly desirable to make at this time.

So, I hope you're all listening, especially the management, and see this period of change as an opportunity for some real meaningful stakeholder interactions, not as a one-day event, but as a throughout the process event, and that you will take what you have heard on this subject, especially yesterday, quite seriously, and view it as a central part of the technical aspect of this program, because if your ideas aren't getting through, you're not understood, and you don't have the kind of integration that is persuasive to all those stakeholders out there looking on at what you're doing.

It's not just a matter of having the science right. You've got to be persuasive, and I would urge that you think about the funding that you need to allocate the time to do that, even in an era where the budgets are very tight. I think it's really critical for the future of the program.

End of my parting speech. Thank you.

DR. DOMENICO: Is there any further public comment? This will be the last one, and we'll turn the meeting over to John.

MR. BOAK: On a much simpler subject, I wanted to address a few questions that came up. I'm Jerry Boak from the U.S. Department of Energy.

I wanted to address a few questions that came up from the perspective of performance assessment. Perhaps the first one was the question to Rick about what is the significance of the Sundance Fault? There are two major questions with respect to suitability that are raised by the discovery, or by the amplification of what we think the Sundance Fault is from a series of fractures on the Scott & Bonk map, to, perhaps, a major fault zone.

The first is, of course, the question of available area, if we have to have offsets from this zone, and they have to be substantial, there are questions about reducing the area, and the Board's made it clear that they have concerns about how much area is available to us.

The second is the hydrologic question with respect to how that would behave as a hydrologic zone. It's really a specific case of a much more general question that we've been struggling with for a great deal of time, and the immediate follow-on to the completion of our total system performance assessment is, in fact, to look and see what we can do now to try and pull together everything we've learned in the past two years, from studies by Sandia, studies by the U.S. Geological Survey, to really make some better statements about fracture flow in the unsaturated zone.

We know it's important. We know it's a critical question for us. We think that the attempts that have been made to demonstrate it in the past have not been effective in really resolving questions about it. We have a lot of concerns about it that remain, but as far as the Sundance Fault being something unique in that regard, I think we certainly intend to follow the development of the understanding of the Sundance Fault, but it's more a part of a much larger question, that we are, indeed, we're pedaling frantically, as hard as we can, to study.

And that leads to a comment that I'd like to make, which is a little bit of a defense of performance assessment as a potential driver for this program. In my view, it's been involved in driving the program since before I came on board, but, in fact, it's done so in a rather stealthy way, and that's partly a problem that, as Ross Perot has said, the devil's in the details. There aren't a lot of people who know all the details of how we put together a performance assessment, and, simultaneously, are really intimately familiar with the site characterization program, and I mean very intimately.

It's surprising how detailed and how focused a set of data can completely transform your view of how things operate in the geologic system, and even people like Jean Younker have to admit sometimes that there are gaps in their knowledge of those two parts of the program.

Our way around that has been, predominantly in the past, to convene task forces, like the group that put together the site characterization program, the group that looked at the Calico Hills risk benefit analysis, the test prioritization program, and performance assessment was always deeply involved in those things. There were direct calculational inputs to ESSE and to the Calico Hills risk benefit analysis, and there was also direct participation of performance assessment people.

In essence, this constituted having a kind of top level performance assessment model which was run on a carbon-based computer. Sometimes, that was invisible, because, as has been noted before, it didn't have the--it was not blessed with the silicon sacrament of computation, and so, I think that I have seen--I have a hard time identifying any activity that we've engaged in in the project that was not either driven by our need to get forward on issues of suitability, and move towards demonstrating compliance, or else driven by concerns of the design, which was also necessary to put that case together.

I would like to have it more directly visible. I would like to have had more obvious credit for some of the things that performance assessment has been involved in. I do note--I want to make firm here the fact that the term, "pork chop" for that outline did come from a performance assessment person. It was coined by Maureen McGraw, so there is an important driver on the program there, at least. I think there are other places.

I think we have now moved to the point where performance assessment is providing much more direct input back to the site program, but there is always the problem of lag between the time we scope out a study, and the time we finish it, so the question about getting in some of Tom Buscheck's concerns about the saturated zone, well, Tom didn't run those calculations until well after we'd scoped the TSPA, and so there's a fair amount of that kind of thing going on.

It's pretty hard to keep up with Tom's calculations. We have a tough enough time just checking out some of the assumptions on his earlier calculations. That has been a problem for us. We've been constrained in a lot of ways in getting the information in performance assessment, pulling in what we have learned in the past few years into performance assessment. It's not an easy process. The abstraction process is one of the toughest parts of getting a reasonable performance assessment together.

We hope that our avid participation in the development of Scenario A, in terms of bodies and time, will help to make sure that as Scenario A gets defined, it will be defined with performance as a very strong driver in that.

Thank you.

DR. CANTLON: Well, let me close the session by thanking all of the participants, DOE, NRC, State of Nevada, our foreign visitors, who came in to share with us their views. I think Jerry's last remarks here are perhaps as good a tone to pull this two days of session together as anything.

I think, as we look at the Board's five years of oversight of the project, you will recall that we were highly critical in the early years of our perception that the system was not really pulled together in any kind of an operational way, and that the hiatus between the very high-quality science we were hearing, and the "So what?" question, where does it link into the safety of the repository, that was a very dominant thing.

Today, we heard a very excellent look at the surface geology when Dick Spengler talked about it. It's difficult for him, and probably even people in the program, to fill in the "So what?" question, because we haven't really gotten the ink dry on his maps yet. So, the element of frustration that oversight groups like the Board have is, in part, intrinsic in the nature of this very, very complex process that's going on, in which the whole operation is essentially building a prototype.

Nobody's built one of these before. Nobody's looked at that kind of a mountain in that kind of a detail for that kind of a purpose before. So, I think both the State of Nevada, the Board, and so on, that have these periods of frustration, have to recognize that what we're looking at is really a very, very complex process, almost without parallel, in the way science and engineering have been brought to bear in a context of extremely high public concern and emotion.

So, thank you all for bearing with us, and your critics were, I think, constructive in our intent, if not, sometimes, in our tone, and so thank you again, and we look forward to continuing the dialogue.

(Whereupon, at 5:30 p.m., the meeting was adjourned.)