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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE

139th MEETING

(ACNW)

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WEDNESDAY,

DECEMBER 18, 2002

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ROCKVILLE, MARYLAND

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The Advisory Committee on Nuclear Waste met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 1:00 p.m., Dr. George Hornberger, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

DR. GEORGE W. HORNBERGER, Chairman

DR. RAYMOND G. WYMER, Vice Chairman

DR. B. JOHN GARRICK, Member

DR. MILTON N. LEVENSON, Member

DR. MICHAEL T. RYAN, Member

ACNW STAFF PRESENT:

SHER BADAHUR
Associate Director, ACRS/ACNW

HOWARD J. LARSON Special Assistant, ACRS.ACNW

NEIL COLEMAN
ACNW Staff

TIMOTHY KOBETZ
ACRS Staff

MICHAEL LEE ACRS Staff

RICHARD K. MAJOR ACNW Staff

A-G-E-N-D-A

P-R-O-C-E-E-D-I-N-G-S

(1:00 p.m.)

CHAIRMAN HORNBERGER: The meeting will come to order. We are continuing this afternoon. We are going to hear two presentations on Nuclear Waste Safety Research and Technical Assistance Programs, and the cognizant member for this part of the agenda is Ray Wymer, so I'll turn the meeting over to Ray.

VICE CHAIRMAN WYMER: By a tricky bit of footwork, I assumed the -- on our Chairman's part -- I have gotten the responsibility for the research activities of the ACNW, and it's good because I'm very much interested in it.

Our first presentation will be by William R. Ott, familiarly known as Bill, who will discuss the Radionuclide Transport Research Program: Progress and Plans. Bill, are you ready to roll?

MR. OTT: What I'm going to try and do today is give you an update on basically where we are in implementing the plan, Radionuclide Transport Research Plan which we've talked to you about before, and I'm also going to go into a little bit more detail on a few activities that are actually coming to fruition right now, like the NEA Sorption Project.

(Slide)

Basically, we've just completed a peer review with the Institute for Regulatory Science, and we went through a day's worth of presentations to them in which I presented an overview, and what I've done is I've essentially adapted the slides that I used for them.

The first five slides, six slides --

CHAIRMAN HORNBERGER: Bill, who is the Institute for Regulatory Science? Is that internal? Is that a consulting group?

MR. OTT: This is an external group that does a lot of peer reviews for government agencies, particularly for DOE and EPA.

CHAIRMAN HORNBERGER: Is it a private company?

MR. OTT: It's a private company, right. We went through an external contractor. They work through the -- well, all this will come out in the slides when you get to it.

MEMBER GARRICK: I can just barely hear you. Is it my ears? Are you wired?

MR. OTT: I'm using this one, I wasn't using the other one. Is that okay?

VICE CHAIRMAN WYMER: As long as you stay close.

MR. OTT: The first few slides essentially are the organization of the plan. In other words, the plan has four key elements listed in it, and each one of those elements we'll discuss a little bit about the products that we've had over the last year, and perhaps a few products from preceding years that form the basis for what we're doing now. For instance, in the area of materials, 4SIGHT is a product that's listed on the list of products. It's something that was produced a couple of years ago, but continues to be the basis for our continuing work in other aspects of concrete.

We'll also talk about some of the planned products we have coming up during this year, from a lot of new starts that we started last year.

I'll tell you briefly how we expect to use those projects and the principal staff and contractors that are involved. It will all be on these slides. They are color-coded so that the entries in blue refer to stuff that's completed either recently or in the near past, and the things in red are things that we are planning to do, some of them very imminent.

I'll also have a slide on miscellaneous activities, which are things that we don't really and they are more targets of opportunity or small things that just don't fit in the planning process very well, but quite often they are very significant things, so I'll have a slide on the miscellaneous activities.

I'll talk about the status of our MOU on R&D of Multimedia Environmental Models. I'll talk about the status of the NEA Sorption Project, and the peer review of the plan.

(Slide)

The key elements of the research plan, as listed in the plan, are release of radioactive material which is primarily a source term issue; engineered barriers, which is anything we might do to design and disposal or containment facility; transport -- and here I've digressed a little bit and I've used two slides, one which focuses on flow and one which focuses on transport, and in between them is a little diagram that I'll discuss with you for a few minutes; and then the last key element is performance assessment, there will be a slide on that.

(Slide)

For the source term work on release of radioactive material, we have three staff principally involved -- Phil Reed, Linda Veblen and Ed O'Donnell. The source term area is the one we discussed before that because we're a generic program, it's very difficult for us to justify work unless we can find an area that really addresses a fair number of licensees. We had this in the area of SDMP slag sites, and we did a significant amount of work both at PNNL with Phil Reed's contractors, and through Linda Veblen at Johns Hopkins University, to try and determine where these nuclides were in the slags themselves in terms of mineral content, and then how those slags degraded over time. And one of the most recent things we did was do a leeching model through University of Pittsburgh and Dr. Su (phonetic).

The slag leaching model is the last component. It was completed in September. We have a letter report on it. The reason it's red is that the formal publication will come through the confidential report on Linda's work on the slag demineralization.

How this stuff is used: Obviously, source term has to be used whenever you're doing any kind of form of success, you need to know solubilities, you need to know degradation rates. In this particular case, we actually have the NUREG 6632, which is one of Phil Reed's products out of PNNL referenced in the Molycorp license amendment. At present, we don't have anything additional planned in source term that we expect to fund this year.

Let me say one thing about funding this year, we're still under a Continuing Resolution, so we have a problem with anything that's constituted a new source, and I really don't have anything in here about those things which haven't started under that particular restriction yet. We do have a couple of new starts planned.

(Slide)

This is the engineered barriers work --

MR. LARSON: It's all low-level waste release, it's not spent fuel?

MR. OTT: It's not spent fuel, it's everything except -- it's not specifically low-level waste either, it's slag -- anything that's not high-level waste could be covered here. There isn't a demand from NMSS for us to do low-level waste work, although I noted that you discussed it some in the meeting this morning, and we have seen greater interest in discussions on low-level waste.

MR. LARSON: Okay.

MR. OTT: This is the engineered barriers work. There's been a little bit of a change here in terms of the amount of support that we're getting for it internally. In the past, I've come before you and told you that we were doing work in engineered barriers, and it wasn't supported by the Licensing Office. But, in particular, the entombment option for reactors has raised concerns about the long-term performance of those entombed structures, and we're actually being relied on now to provide some information on entombment for rulemaking activities that are coming up on that option.

The principal product in this area in the last three years has been 4SIGHT. We went through a number of years working on that, and then we tried to do some validation, and I think -- I believe I reported to you at one point the result of that validation work was that we couldn't -- we weren't able to find data that was sufficiently constrained to say that we could validate this model for out to 500 years, which is what they want to use when they are doing a performance assessment on, say, a low-level waste disposal facility. We couldn't nail down the initial conditions on older concretes enough that it wouldn't have been a fitting exercise. So the conclusion and what we reported to NMSS was that we can predict, and we think the model is pretty good, but you're going to have to monitor.

Now we're trying to apply 4SIGHT to an assessment and monitoring of entombments. We've also started a project which is looking at non-concrete barriers. This is with the Corps of Engineers, and they'll be using the centrifuge equipment at Vicksburg to look at things like the cracking of clays over time. One of the problems that's been observed at places like Savannah River is the clay covers they thought were going to last a long time are showing cracks due to desiccation, and it's a process that we think is important to look at.

The last item on here is a study that's being instituted by the National Research Council and the National Academies, to look at the state-of-the-art of engineered barrier technology. There are three agencies currently that have agreed to fund this. The NRC has agreed to fund it, the EPA and the Department of Energy. The current situation with dollars in the Federal Government in terms of budgets being passed means that they haven't received the money yet to do the study. They've got an initial increment from us, they're due another increment, but the other two agencies haven't contributed yet. So this may be delayed for a short period of time until funding appears from the other agencies.

CHAIRMAN HORNBERGER: Is that the Board on Radioactive Waste Management?

MR. OTT: I believe it's the Board on Earth Sciences that's doing it.

(Slide)

The groundwater flow part of the transport program is the environment where Tom Nicholson and Ralph Cady -- well, Tom Nicholson does most of his work, and Ralph Cady does a lot of work there as well. Principal contractors in the area presently are PNNL, ATBD for an RFP that's in progress. University of Arizona has been involved in the past, is not presently -- well, under a no-cost extension for a couple of months. That project is ending, the one on conceptual model uncertainty in the Agricultural Research Service. I believe Dr. Newman of the University of Arizona is the subcontractor to Phil Meyer at PNNL on his follow-on work on conceptual model and parameter uncertainty and scenario uncertainty.

The work here in blue indicates the work at PNNL in terms of parameter values and distributions which form the foundation for the changes that we made to D&D and RESRAD, very significant contribution viewed by the Licensing Office over the last few years. And the hydrologic database incorporating regional and national data also is incorporated into those changes.

Evaluation of instruments and methods for estimating infiltration was done at ARS in a cooperative effort between Ralph and Tom and the investigators there.

The RFP that I talked about is supposed to be developed in a robust field-tested -- I'm emphasizing these words because Tom gave them to me very specifically and said I've got to use these words -- robust field-tested methodology for unified -- and "unified" is in red because we haven't tried to unify the parameter conceptual model and scenario uncertainties yet. We've addressed the parameters and conceptual model separately.

VICE CHAIRMAN WYMER: What's implied by the word "unified"?

MR. OTT: That means you're trying to develop a strategy that encompasses all these uncertainties into one overall philosophy for addressing them in a systematic and concerted fashion rather than separately worrying about parameters, separately worrying about conceptual models, and separately worrying about scenarios. So we're trying to integrate all this work that we've done in the past.

That's the work that's being done PNNL, and that project just started, I believe, this summer.

The integrated monitoring strategy for performance confirmation and early warning is the project that's out on an RFP. Everything has actually been completed and we're in the process of making the final award now, but since there are still contract negotiations to go on, I can't reveal who the successful bidder was.

VICE CHAIRMAN WYMER: I hate to keep bugging you. I don't really understand what an integrated monitoring strategy is.

MR. OTT: If you look at a natural system and you do -- if you go in and try and do a conceptual model of it, and that conceptual model says that this flow path is the one that's important, and you say, okay, I'm going to monitor this flow path. And there's an alternative conceptual model that says, well, that's not the principal flow path, it's over here. And if you haven't considered both of those conceptual models in developing your monitoring program, you may wind up monitoring the wrong thing or the wrong place. This actually happened in one of the places that was being monitored after Chernobyl where they totally misdiagnosed the location where they expected contamination to go and they wound up monitoring the wrong place and said, well, there's nothing there. Well, it wasn't there because it went in a different direction.

So, the concept here is that we have to integrate all of this stuff, look at multiple conceptual models, and make sure that we've integrated everything.

VICE CHAIRMAN WYMER: Thanks.

CHAIRMAN HORNBERGER: Is your use of the word "performance confirmation" the same as the use in Part 63?

MR. OTT: It probably is, but it's not meant to be. In other words, NMSS doesn't like us to use --

MEMBER GARRICK: I was hoping you would say yes because then you could explain to us what it is.

MR. OTT: From our point of view, what we're trying to do is establish a basis for monitoring a system so that we can actually confirm the predictions that were made, whether they were right or whether they were wrong.

CHAIRMAN HORNBERGER: So that's a totally different use.

MEMBER RYAN: That sounds more like validation.

MR. OTT: Except that -- you know, validation would imply that you know that you're right for the entire period of performance, and monitoring doesn't necessarily require that. We're essentially -- if we predict the performance with regard to several different conceptual models and there are several different ways the system could perform, then we want to monitor all those potential ways of the system performing.

MR. LEE: Bill, I can understand why you can't say who got the contract award, but can you tell us who at least bid on the contracts?

MR. OTT: No.

MR. LEE: Oh, you can't?

MR. OTT: We don't know that until the actual negotiations are complete.

MR. LEE: Oh, okay.

MR. OTT: I don't even know it, as a matter of fact.

MR. LEE: Okay.

MR. OTT: Even if I wanted to tell you, I couldn't, which is perhaps one of the reasons why they do it that way.

The evaluation of uncertainties in recharge estimates is, again, a small out-project being done jointly between our staff and the Agricultural Research Service.

(Slide)

This is a diagram that I drew -- so don't accuse anybody that really knows what they're doing of making mistakes -- because I was trying to demonstrate what I thought was going on to the contractors that we had working in the area, and asked if they agreed with me. And one principal caveat I should make right here is that I'm only talking about the sorption process right here, and there are other processes that might occur in the environment that might hold up materials.

The reason I put this thing up here is that, traditionally, in the old KD approach, all you worried about was what was down in that bottom balloon in the middle, Distribution Coefficients, and, in reality, there are a lot of things that go into determining what the distribution of aqueous and soil phases are. So we've been working for a number of years trying to understand the mechanisms particularly for sorption, and right now I was trying to figure out -- last year, actually -- I was trying to figure out where we are. And basically we're at the point right now where we have a fairly good idea, at least for sorption, in many instances surface complexation models are a fairly good descriptor. They do describe a lot of what's going on.

In order to make those work, the surfaces have to be understood. You have to know what the sites of sorption are. And you have to know what the reaction is between the sorbing site and the radionuclide. So when that box up there in the upper, left-hand corner talks about thermodynamic data for mineral/radionuclide pairs, you're talking about that reaction between that site and that radionuclide. So we're talking about essentially knowing what's in the soil or the rock in terms of what the sorbing minerals are, and then combining these two surface complexation models.

And the interesting thing here that I was very specific about was this arrow that goes from Surface Complexation Models directly to Concentrations because there is a potential for just totally doing away with using distribution constants in the actual calculation.

MEMBER GARRICK: You may have answered this, but how do you decide what minerals to use?

MR. OTT: You go in there and you

characterize the soil. You actually determine what the mineral composition -- and we've actually done this at the Naturita site. I'll discuss that a little bit on the next page.

VICE CHAIRMAN WYMER: Well, the Surface Complexation Model is really a surrogate distribution coefficient. I mean, there's very little difference.

MR. OTT: Well, there is difference because the -- actually, I ought to have a geochemist up here talking to you about this. I don't know if you've seen this --

VICE CHAIRMAN WYMER: Can't even see it now.

(Simultaneous discussion.)

MR. OTT: Geochemistry of soil radionuclides, it's Soil Science of America Special Publication No. 59, came out this year. It talks about a lot of application of Surface Complexation Models and the reactions involved. But basically, yes, the Surface Complexation Models are an intermediate step between the basic properties and the Distribution Coefficients, but they themselves deal more directly with the actual reactions involved and the reaction constants. So there's a much more direct connection to the science than there is with the bulk

KD.

The principal staff involved in this area are Ed O'Donnell, Linda Veblen --

CHAIRMAN HORNBERGER: This slide doesn't follow your color coding, right?

MR. OTT: No. I meant to say that earlier, but I forgot. This one does.

(Slide)

In here, sorption models sensitive to soil components and chemical conditions -- you know that we've been working at a demonstration project in Naturita, Colorado. The final report for that is being submitted right now, and it's under review. It shows here in blue that it's completed. It is not quite, but preliminary results seem to indicate that there's been a great deal of success in using surface complexation models to describe transport at that site in a fairly complex chemical environment.

The second item mentioned there, evaluation of the contribution of soil particle coatings to sorption processes, is actually a product of Sandia National Laboratory, who is working in conjunction with our USGS contractor at Naturita. And it turns out, in this particular case, that most of the sorption is occurring in soil coatings, not within the soil particles themselves.

Basically, though -- and Ray probably appreciates this more than anybody else -- if you look at that previous slide and that diagram, you realize that there's a different kind of data necessary to do that kind of modeling, and it's necessary to use a KD.

VICE CHAIRMAN WYMER: A lot more.

MR. OTT: And there's a lot more, but -- and at this time, we don't have enough to do it for more than a few radionuclides. We did the demonstration at Naturita because it's a uranium site and we thought we could do the demonstration and had enough information on uranium primarily due to previous work done by those principal investigators and others in Australia at the Alligator River's Analog Project, which is a multi-national study of sorption at the --

VICE CHAIRMAN WYMER: You've concentrated on uranium.

MR. OTT: So we concentrated on uranium as a proof of concept, just to prove that we could do it. It's a multivalent ion. If we can do it with a multivalent ion in a fairly complex environment, we ought to be able to do it with monovalent ions in simpler environments. And I'll talk some more about the NEA Sorption Project in a minute.

(Slide)

In the performance assessment area, I've only listed one past product. You're aware that for a number of years we were working independently on developing a framework model at Sandia National Laboratory, and eventually terminated that work because we couldn't support it. We didn't have the resources to do that kind of thing on our own.

We've moved to that and in concert with the other organizations that are in this MOU on Research and Development on Multimedia Modeling -- three of them, as a matter of fact -- Corps of Engineers and EPA and us -- are all working towards developing FRAMES as a more comprehensive modeling platform for dealing with complex sites. So the only product I've listed here is the RESRAD and RESRAD-BUILD models that were enhanced by us in the last couple of years.

In the new area, there's one here that might strike your interest because it relates to something you mentioned this morning, this first one, comprehensive assessment of parameters and assumptions of environmental pathway models. We had also looked at the end of the calculation.

We've been focusing on environmental transport and have not really been worrying about pathways up until now. When we looked at RESRAD and D&D and challenged the assumptions in there, we challenged the assumptions on the transport part of the problem, we didn't challenge the assumptions in the pathway models.

We've started to look at Pacific Northwest National Laboratory -- Phil Reed is the Project Manager on this one -- to go out and look at the pathways model, evaluate the assumptions, evaluate the parameter values, and evaluate the databases that support those parameter values. We should be getting a report from them probably in February on the first phase, which is this assessment of the models and where the holes are. And we intend to follow that up by choosing those areas that will give us the most benefit in doing further work to establish a sounder basis for the pathway models, very similar to what we did with D&D and RESRAD in terms of identifying assumptions, identifying when assumptions weren't soundly documented, and then going out and doing that.

The next one here is the dimension of FRAMES, which I've already talked about, and we've independently been working with the Corps of Engineers to bring some of their capabilities inhouse. Their groundwater modeling system has been observed to be a very powerful groundwater modeling tool, and we've now made that available to NMSS staff and had the Corps of Engineers come in and do training for the staff on it. Of course, the use of all this work is to improve our performance assessments of sites from simple to complex.

There is a note in here about new work on probabilistic RESRAD-OFFSITE and support for MARPAR. These are a couple of small things that we will probably do.

VICE CHAIRMAN WYMER: What MARPAR -- MARPAR, is that something Chrysler Motors --

MR. OTT: This is an interagency activity between ourselves and DOE and EPA, to try and agree on parameter values.

VICE CHAIRMAN WYMER: I didn't recognize the acronym.

(Slide)

I mentioned this Miscellaneous category before. Basically, there are things that come along which it is desirable to fund and usually give us a lot of payback, and I've just listed some of the things here and some of the people that were involved in doing this. A lot of our activities with the National Academies, such as that study on engineered barriers, is something that comes up. The National Academies attends the meetings, we're at the same meetings, and they get an idea to do a study, and then we are requested for funding, and we say, yeah, that's a good idea, and we do it. And in addition to that, we actually give some support to the National Academies Committee on Earth Sciences each year, which gets us to make presentations and get some review of our concepts as they're going along.

I've included on here the peer review contract we did with RSI, and a few other things. I'm not going to go into these in a lot of detail because I don't think we have a lot of time here.

The last thing I will mention on that slide, though, is that we are involved with both the NEA and the IAEA on things like the IG SC, which is an integrated group for the safety case. I'm afraid I don't know what the ISAM actual acronym is, and I couldn't get hold of Ralph today to ask him, but that's an IAEA activity which sort of parallels the NEA activity.

Nothing else is color-coded for the rest of the slides, so we don't have a time line to worry about here.

(Slide)

This is basically the status of the MOU. I believe I talked to you about this the last time I was here, so it's only been about six months. There haven't been a lot of changes. I think we either mentioned that we had sessions in planning or had just told them we had meetings. Actually, they were this summer, so they would have been afterwards. One was in the spring, one was in the summer. So there were two meetings held in conjunction with professional meetings during this year.

There's a workshop planned in January by the Working Group on Software System Design, and this particular Workshop is focused on developing a more efficient GIS interface for a lot of these multimedia models. The GIS is currently viewed as being rather large and cumbersome and difficult to really bring into the environmental models in an efficient way, so they're trying to develop a less robust way of dealing with GIS systems and bringing information into and out of the multimedia models. That's supposed to be held in January.

The Working Group on Uncertainty Analysis is planning an international workshop for August, and there's a third workshop that may be planned this year for the Reactive Transport Modeling Working Group. They provided a draft Phase II, which is a detailed operating plan to the Steering Committee at their meeting early this month, and expect to have the final to us at the meeting in February. When that's approved, then we'll know whether that meeting will actually occur this year or next year, but one of their first activities is planned to be a workshop there as well.

And the other fairly important thing to update is the fact that we actually have another member now. The Natural Resources Conservation Service has joined the MOU, so there are now seven Federal agencies involved in that particular set of cooperative efforts.

(Slide)

The NEA Sorption Project has been going on for a number of years, and basically it's a group of nations who are all involved in some form of nuclear waste disposal that have identified an improvement in this KC approach as an important thing to consider for improving their models.

There are 11 countries involved at this time. In some countries, like Japan, there are actually two different federal agencies that are participants in the project. In many of the countries, there are multiple working groups that actually did modeling tasks. In this last phase, they had a Technical Direction Team which selected, I believe, six problems, and then the 11 countries selected which of those problems they were going to work on. So each problem probably had from six to ten different analyses performed on the set of data that was involved.

And what I have seen is some very rough preliminary conclusions and lessons learned from this project, and what I did was I went in and I sort of excerpted them. These may look totally different when they actually come out of it, but I think these concepts will wind up in their final report.

They found that with all these different approaches -- and most of them focusing on surface complexation models -- they were getting good results in interpreting the data for single minerals and for more complex natural minerals. So they are confirming what we've actually seen at Naturita. They are being able to do this kind of modeling.

They found that they can interpret large ranges in observed behavior -- Rd 4 orders of magnitude, pH 4 from to 10. So they are finding success over a fairly wide range of chemical conditions. They found that they can handle complex aqueous chemistry, they can handle inorganic and organic complexants, a range of ionic strengths, they had good success with interpolation within the range of boundaries. They expressed some caution with regard to extrapolating outside of the range for which they had data.

VICE CHAIRMAN WYMER: Is this uranium again?

MR. OTT: No. Some of this was uranium, some of it was other radionuclides, but I can't tell you off the top of my head. There will be a report on this probably coming out in -- when you're working through the NEA, sometimes it takes a long time -- I would expect it by June, but the Technical Direction Team is actually putting it together.

(Slide)

They also had a section on Lessons Learned, and this is where I come back to that observation about data, and both the first and last entry in here talk about data. The first one is that few existing data sets are sufficiently complete to support this kind of approach to modeling. This conclusion probably come from the Technical Direction Team's effort to define problems for them to analyze because they had to find decent data sets, and didn't find a whole lot to choose from. They need time to get better data sets.

In the international community, there's not been the same -- or at least we have not observed the same -- thrust towards using distributions in probabilistic approaches. I think this second bullet here, or second item here, tends to indicate that at least in this project they have identified a need to look at uncertainties via measurement and support those uncertainties and measurement techniques in the codes, which I think would lead them to a more probabilistic approach.

There is a need to define exactly what essential data that you need.

Geochemical characterization is a pre-requisite to effective sorption modeling. What they are saying there is you need to know what the mineral substrate is because different clays act differently.

And, of course, the last one is the other side of the coin. If there isn't much data out there, then there isn't a database to support these models, but from their observations and the successful work they've had, they think it's time that the international community works together to provide a modeling database.

They made some observations about what should be done in the next stage. I'm not going to go through those because this is all going to wind up being negotiated as an international project. Essentially, all the participants will review the work scope and they'll look at what they think are the things that most need to be done in the next stage, and then they make a decision on whether to participate, and this will probably take anywhere from six to 18 months. So, if there is another stage to this project, it won't happen right away, it will take a little time to get it going.

(Slide)

Now, the last item I wanted to talk about is the peer review. We've been looking for peer review, stakeholder review, ever since we completed the plan, and we got inconsistent results. You noted it and suggested that we go out and get a peer review done. We found an organization that does it, and we went to them. They worked through or with the American Society of Mechanical Engineers -- not the American Institute, sorry about that, typo. They maintain a fairly large list of potential peer reviewers, and they will select a slate from that and they'll have it reviewed by the ASME Peer Review Board. So, RSI sort of works with ASME to actually select the reviewers. They aren't necessarily members of ASME, they are consultants and all that, and faculty members and other disciplines. The list of panelists is here. We were actually only familiar with one of them, and that was John Moore, who is knowledgeable to us from his past involvements in the IAH and AIH.

VICE CHAIRMAN WYMER: I know Joe Peterson. He's a heavy element chemist.

MR. OTT: I thought you probably would since he's down there at Tennessee. We gave them all the background that we had in terms of the plan. We met with them for about a day and a half, made presentations, answered questions, and all the rest of it.

(Slide)

When we made the contract with them, we gave them a set of criteria. Essentially, when we did this, we specified the criteria that we wanted them to look at, what questions do we want them to answer, and we came up with seven questions. They are listed here as criteria. And these are the ones that were addressed by the Peer Review Committee. The findings of their report will specifically address each one of these questions, usually with a fairly direct answer, and then with a bunch of text either giving caveats or indicating why they came to the conclusion they came to.

(Slide)

An observation that I'll pass on from Allen McGeesie (phonetic), who is the President of RSI, was that he felt that it was one of the most positive peer reviews they've had in recent years. He thought they were fairly pleased with what we've done.

(Slide)

The last page is basically the recommendations. Now, the recommendations are sort of independent of the criteria, and they come out of the deliberations and opinions of the panel members. And you will note here that in some areas they actually overlap or repeat things that the ACNW had said to us in the past. For instance, No. 4. We asked them about the prioritization scheme, and they said they didn't have any reason to dispute our result or say that it was right or wrong, but they said that it might be better if it was more formally based. And I think you have said the same thing.

The agency is right now looking at a more uniform way of doing prioritization, so to a certain extent changes right now are kind of held up while the agency comes to grips with this on an agency-wide basis.

Their first one was a recommendation with regards to what they felt was a lacking in the plan. We had focused on the regulatory basis and hadn't really listed a lot of references in here that supported what we did in the plan. It wasn't because we couldn't, it was just because we didn't want to detract from the attention being given to the regulatory context, but it probably is a weakness in the plan, and we will remedy that in the next go-round.

The second one, the Project Team should perform an in-depth analysis of the relevant computer codes and identify any systematic errors. I think we do tend to do that. We're not going out and trying to do all of them, we're doing the ones that are primarily used in the community or are indicated to us to have a significant benefit. And we are not doing those that are proprietary. We can't go out and do this to, say, a code like -- that is sold over-thecounter or is being marketed by another organization.

The Project Team should pursue additional leveraging. They have an observation in there that they don't know how we can do everything that's in the plan with the kind of resources that we have. It's an observation that you've made as well. Their way of handling it is to say we ought to go out and cooperate with more and more people and do more and more leveraging. I think we are trying to do that through the MOU.

The last one, the Project Team should obtain input from the outside scientific community regarding the scoring of different projects with respect to the "issue support" attribute, which is somewhat different from what you recommended. You recommended we ought to go out and get somebody else to look at it in terms of a panel format or something like that. And they were saying specifically -- as you recall our prioritization system, we have this issue support criterion which basically assesses whether it's been supported internally by NMSS or the Commission or the Advisory Committee. If somebody says that something is really important, then it gets a fairly high score. If we can't get anybody else to support it but we still think technically it's a good sound thing to do, we have it listed as an intermediate priority, and if it doesn't get support from any of those places or us, it doesn't get any score at all. And they're saying that we actually ought to go out and have somebody look at that particular issue. It's an interesting recommendation. I don't know how we're going to deal with that right now. Obviously, outside input is something that a lot of people want us to have, and I think we're trying to get that.

Anyway, we are scheduled, I think, to come back in February, and at that time we'll actually have the -- I don't have the actual bound version of the report yet. The President of RSI said I could use anything in it, but the only thing I was certain wasn't going to change were the recommendations. And we would anticipate coming back and actually going through each one of the findings and each one of the recommendations, and giving you an idea of how we intend to proceed in regard to those, but we plan to do that in February.

Questions?

VICE CHAIRMAN WYMER: Thank you, Bill. I asked mine as you went along. I thought that this whole area was a very good one to support right from the very beginning, speaking as one member of the committee. Any questions from the table here? Mike, start with you?

MEMBER RYAN: I guess I have sort of a real basic question. What is your split between Yucca Mountain and high-level waste related activities and everything else?

MR. OTT: We don't do any high-level waste activities. Well, we don't do anything that's specifically related only to Yucca Mountain.

MEMBER RYAN: I gotcha. Okay. You made that point. A number of things obviously are, or can be.

MR. OTT: Yes. A lot of what we're doing -- I should make one observation, especially since you're new on the committee. We involve NMSS and the Center for Nuclear Waste Regulatory Analyses whenever we can. In particular, in the sorption project, there were modeling teams supported by the NRC. One was our USGS contractors, the other was a modeling team from the Center for Nuclear Waste Regulatory Analyses, and it was actually supported through NMSS.

On the MOU for R&D of multimedia environmental models, we went to NMSS and said this is an opportunity we think you ought to be involved in as well, and they have named working group members to each one of the active working groups under the MOU. So we're inputting in those areas where we can, into the people that are working on the high-level waste program, is I guess the best way I can --

MEMBER RYAN: That's a good answer. Thank you.

VICE CHAIRMAN WYMER: John, you're next.

MEMBER GARRICK: Well, how do you track the use of the results of your research?

MR. OTT: In terms of formally, I guess you wouldn't say that there's a formal way that we track it. In the user need letters that come to us -- there's not a firm schedule in which we get these things, we get them occasionally -- they will quite often state why they want something and how they'll use it, and then quite often we'll get involved in helping them in doing technology transfer and helping the staff that are going to use it understand how it can be used. For instance, we had NMSS staff come to us a few weeks ago and ask for information on monitoring, and what we did was we provided all the background information that we'd used in developing the RFP for our modeling project and the SOW for that project, and we sat down with the staff and discussed where we thought the state-of-the-art was in terms of developing monitoring programs. This was monitoring for, I believe, a decommissioning site somewhere out in the Midwest.

The staff works fairly closely with the NMSS staff, and I think that's how we know where the stuff is being used and when it's being used. Phil Reed essentially worked with the staff on the Molycorp license -- the people who were doing the molycorp license amendment, and they talked to him, and they wanted the data and he provided the data, and they mentioned to him that they had referenced it. So, it's primarily through two-way communications between the two staffs. And at the staff level, there is fairly close communication, I believe.

MEMBER GARRICK: Do you anticipate any change in funding levels?

MR. OTT: Our funding level has increased a little bit over the last couple of years. I think we went from -- well, we've bounced around over the last five years, from $2.5, 2.6, down to 2, and I think we're up around 2.7, but since the budget isn't approved, we don't know what it is finally for this year yet.

One other observation I probably ought to make is that when I talked about that pathways model, there were observations made. Everybody that's looked at it has looked at it and said this could eat up our entire budget. The first task is the only that we've funded, which was the one to look at the models that are out there and look at the assumptions and the parameter values, and look at the basis for them. A lot of that information is 20 years old. Some of it's 30 years old. And the science has advanced significantly in a number of those areas. If there are significant gaps in the data in there, it could -- there could be very expensive projects involved to remedy those holes, and we probably don't have the resources to do it. But I think just having the systematic analysis will be a long step towards identifying those gaps not only for ourselves, but for others.

MEMBER GARRICK: Is Research generally satisfied with the quality of the research?

MR. OTT: I think we are. The only project we weren't is the one at Sandia that we terminated. And there I'm not certain -- well, other agencies had been funding the work and dropped out, and with the funding that we were able to provide, there wasn't a sufficient critical mass. So, even

Sandia couldn't maintain the team, and they kept having to change people in and out, and you can't maintain a research program with that lack of continuity. So that was an example when we weren't satisfied with it.

I think in most of the other areas we're very satisfied with our PNNL contractors. Glen and Gia (phonetic) and Phil Meyer have been working with us for ten years or longer, and they are very solid. University of Arizona has worked with us for a long time. That direct association is currently coming to an end. No one can question the quality of the work that we've gotten out of Newman. Phil's contractors at PNNL have been sound. Our USGS contractor was sought out by the Nuclear Energy Agency to -- I think he's the Director of the Technical Direction Team from the NEA Sorption Project, in addition to being on our working group and our modeling team, so he's internationally recognized as being an expert in the area of surface complexation modeling.

MEMBER GARRICK: What about what you get out of the National Academies?

MR. OTT: What we have gotten out of the National Academies in terms of focused studies on research topics such as this one upcoming on engineered barriers has been very good. We have had a number of projects with the National Academies that have given us good results.

MEMBER GARRICK: They're a different kind of institution, it's not a research institution in the usual sense.

MR. OTT: One of the "disbenefits" of working with the National Academies is it usually takes them a long time to do anything, and that's one of the reasons -- when we went to do the peer review, we considered going to the National Academies, and we decided we need something faster than we'll get from the National Academies, and that's one of the reasons we actually went outside, is because it was something that could be done relatively quickly, and it was a group that was widely used by both EPA and DOE, had a large list of independent reviewers that they could bring into the process.

MEMBER GARRICK: Are most of your contractors other government agencies or not-for-profit organizations?

MR. OTT: Most of our contractors at this time are either other government agencies or National Laboratories. We do have work, cooperative work at NIST. I mean, it's another federal agency, but it is a cooperative project where they are actually putting funds in. The U.S. Geological Survey has actually been matching our contribution on the projects we've been doing at Naturita, so we put in $250,000 or so a year and they put in another $250,000 in terms of resources and laboratory facilities, and sometimes funds. So, we're getting matching contributions from the other agencies. When we go to the National Laboratories, we're footing the whole bill.

We do have the one RFP that is just being awarded, which will go to an independent contractor outside the government or National Labs.

MEMBER GARRICK: Is the reason you go to these institutions as opposed to maybe a private research institution because it's easier to get contracts resolved?

CHAIRMAN HORNBERGER: DOE labs are cheaper.

MR. OTT: I don't think so.

MEMBER GARRICK: No, I'm just curious.

MR. OTT: I'm answering George, not you.

MEMBER GARRICK: There are some very good private research institutions around, and I don't see much evidence of them being involved.

MR. OTT: They aren't, and I think to a

certain extent -- for instance, with Phil Meyer and Glen and Gia at PNNL, we became involved with them, and they have developed an expertise and knowledge of our program area that makes them very, very efficient and effective and very knowledgeable. And the fact that they're working on a DOE reservation with similar problems gives them a leg up to begin with. To a certain extent, National Laboratories, because of the problems they have already, tend to have expertise that is very, very apropos to what we need to have done.

MEMBER GARRICK: So you're generally satisfied that you're getting --

MR. OTT: We're generally satisfied we're getting --

MEMBER GARRICK: -- getting a quality that --

MR. OTT: And there are times when we look at it and we think there are -- this is a problem that we really think we ought to have somebody in academia look at, or somebody in the general -- which is this monitoring program that we've got the RFP on right now.

MEMBER GARRICK: Thank you.

VICE CHAIRMAN WYMER: George?

CHAIRMAN HORNBERGER: Bill, I'd like to first follow up just a little bit on John's question about how you gauge whether your research is effective. What do you have to do with respect to GPRA, or what have you agreed to do?

MR. OTT: I was going to say that I don't know that there's anything specific that we're doing to address GPRA within our small program.

CHAIRMAN HORNBERGER: So it doesn't come down that far, it's just sort of the Office of Research must have to do something.

MR. OTT: Yes. I was looking at Cheryl to see if she had anything else, and I don't think she does. Oh, she's going to go to a microphone.

MS. TROTTIER: I'll answer. I think that's what we do every year when we report what we've accomplished, so it's really an agency-wide report and we do it by arena. So whatever we accomplish is included in there, if that's what you're asking about.

CHAIRMAN HORNBERGER: Just, again, different agencies -- it filters down to different levels, and I didn't know if you had anything specific that you had to supply to the agency for their GPRA report.

MS. TROTTIER: Well, in effect, we do. If we can't talk about the outcomes of what we do, then we have a problem, but we don't do things that aren't going to have some positive outcome. So, that goes into the planning of it.

MR. OTT: The primary responsibility lies elsewhere, and they ask us the questions and we give them the answers. GPRA probably doesn't --

CHAIRMAN HORNBERGER: I'm much less interested in GPRA, and I was just wondering if you had a way that you could actually make it useful to you and not to OMB or whoever looks at the stuff.

Along those lines, though, I assume that now all of your NUREGs are on the Web, they are electronically available?

MR. OTT: There was a problem last year when they removed all the NUREGs, and we've been putting them back on slowly. Each one of them has to be examined to see if there's anything in it that has anything.

CHAIRMAN HORNBERGER: My point is in terms of one way you could measure how useful some of your research is -- again, all of these measures are imperfect -- would be to keep track of how many times these things were downloaded.

MR. OTT: That would be interesting.

CHAIRMAN HORNBERGER: It would be easy to implement and you could easily do the counting. It might give you some at least rough indication of which bits of your research were most widely accessed, presumably accessed from more than just NMSS staff.

MR. OTT: Yeah. That's an interesting idea. One thing I didn't point out when we were going through that slide on Miscellaneous activities, due to the way things worked out we wound up using a little bit of engineer money to develop two Web sites for the MOU on multimedia modeling. One of them is a public Web site. And the only problem there is that the Steering Committee has to authorize anything we put on it, but we're starting to put cross-references on there to NUREGs and things like that as well, and that Web site is actually getting a lot of action right now, apparently, from what Tom said.

There's a second Web site that we developed, which is actually an internal Web site for the working groups to use. Each working group has its own scoreboard site, and they are being used for interactive meetings and interactive document modification and things like that. That's another thing that we did this year to try and enhance our interactions with the MOU.

CHAIRMAN HORNBERGER: I just have one quick specific question. On this list you handed out, the NUREG/CR-6757, I'm sure you recall that one -- Large Scale Molecular Dynamic Simulations of Metal Sorption onto the Basal Surfaces of Clay Minerals. Who did that work for you, was that Sandia?

MR. OTT: Sandia.

CHAIRMAN HORNBERGER: That's what I thought. Okay.

MR. OTT: Sandia was very close in this project. They're going from doing purely theoretical stuff to try and coordinate their work with USGS, and I think it's proven to be very solid.

CHAIRMAN HORNBERGER: Do you have any sense as to how far along they've gotten into linking some of the molecular dynamics up to surface complexation?

MR. OTT: I think they've gotten pretty much -- you want to answer that, Ed? The Project Manager is right here.

MR. O'DONNELL: It's Ed O'Donnell. In answer to that, they looked at kalenite (phonetic) and the smectites with strip metallic ions. They are now ready to look at more complex oxides, the urinel (phonetic) ion being one of the ones. Also they are planning to look at the anion complexes, particularly technetium iodine, the problem ones. It's theoretical work which is also being done in conjunction with laboratory work, which is also being done in conjunction with field work that USGS stuff at the Naturita site. So, although this is one theoretical part, it's one piece of a bigger project.

MR. OTT: The final report on the Naturita project will include quite a bit of work in it that actually was done at Sandia.

CHAIRMAN HORNBERGER: Thank you.

VICE CHAIRMAN WYMER: Milt, any questions?

MEMBER LEVENSON: No.

VICE CHAIRMAN WYMER: Just looking down this list of products, we're at 2002, you and your folks have been pretty busy, haven't you?

MR. OTT: Well, yeah, that's what they pay us for.

VICE CHAIRMAN WYMER: That's a lot of production. Thanks, Bill. If there are no other questions, thanks for the update. I think you might want to reconsider whether or not you have a February presentation.

MR. OTT: You don't need it?

VICE CHAIRMAN WYMER: We're not going to

have a meeting in February.

MR. OTT: Well, last I heard you were going to not have one in January and have one in February.

CHAIRMAN HORNBERGER: Very recently we changed our mind and --

MEMBER GARRICK: We're going to have a super meeting in March.

MR. OTT: If you want us to come back, just let us know.

VICE CHAIRMAN WYMER: That's the way to leave it.

I guess it's obvious from Bill's presentation that it's a very broadbased research program the results of which will be applicable to many aspects of radioactive waste management.

The next presentation that we'll hear -- we call it directed research and they call it technical assistance because it's aimed at that specific sites, problems at specific sites or specific design requirements, and this presentation will be given by Budhi Sagar, from the Center for Nuclear Waste Regulatory Analyses. Budhi.

MS. SCHLUETER: I wanted to make just a couple of remarks. I'm Janet Schlueter, for anyone here that I haven't met. I'm the Branch Chief of the High-Level Waste Branch here at the NRC, and I wanted to make just a couple of comments to put the Center, their work, in perspective, if you will. For anyone who doesn't know, they were established by the NRC 15 years ago to support the High-Level Waste Program, so that is their highest priority. But in addition to doing that, they do support the NRC in other program areas in the waste-related arena, as well as any other that the staff or the Commission asks them to provide assistance on. And as a result, there's a dedicated conflict-free source of assistance to the NRC. They provide outstanding technical support to the staff, and have historically provided excellent service to us, and continue to do so, and manage the resources in difficult times as the budgets go up and down, as you know.

As far as the High-Level Waste Program, their primary role is to support the NRC in our step-wide licensing process for Yucca Mountain, and what I mean by that is that there's been an awful lot of work, as you're aware, from years ago up until now to get to the point of site recommendation. There's much, much work yet to be done when it comes to the Yucca Mountain Review Plan, putting a draft safety evaluation report strategy into place, resolving the KTIs, TSPA, yadda, yadda, yadda. There's lots of work to do to the point where we were to get a license application and move through the adjudicatory and hearing process, which will be three to four years. That, of course, will only be then passed into the next step of potential requests from DOE for receipt and possession and ultimately closure of the repository.

So, we are where we're at, but there's many steps yet likely to come, much work to do, and we see an extended role for the Center to continue to assist us in that regard. And I'll leave it to Budhi, and I apologize, I'll need to leave in a few minutes to go to another meeting, but that's certainly no reflection --

(Simultaneous discussion.)

MS. SCHLUETER: That's right, although I have heard it before.

MEMBER GARRICK: You can't convince us.

(Laughter.)

MR. SAGAR: Thank you, Janet, and thank you, Committee, for inviting us, giving us a chance to present this to you. Obviously, the work I'm presenting to you is a summary of main results or the significant results, I believe, we have obtained since we were down in San Antonio I think last August. I hope the Committee would find time to come back over there to listen to the workers themselves because while I can answer some questions at the upper level, if you go into real technical details I may have to take those questions back with me to be answered by the people who actually do the work.

In contrast to what Bill Ott just presented where they classify all the work that they do as generic, it's just the opposite with the work at the Center. None of what we do is generic. We are all related to some sort of site or particular design.

(Slide)

Briefly, my presentation outline would be the scope of the work we perform at the Center, a couple of charts on organization and funding level, and then basically significant results -- I say the last 12 months, but it's since last August, and it's hard to keep track of when we got certain things -- and I will try to cover the entire waterfront of work outside the repository program -- those are the two categories I have -- and then the repository program. Most of my focus is on the repository program because that's where most of the funding is and that's where most of the significant results are. Most others are licensing actions in which we are assisting the NRC staff.

(Slide)

The scope of technical assistance, as you can see from this slide, high-level waste, of course, is at the highest level. This is the highest priority work at the Center, and the main focus there is to identify and resolve any technical issues that we see before the license application comes in, and that relates both to the preclosure safety as well as the postclosure safety, and develop review tools, which means the YMP, Yucca Mountain Review Plan, or any software we need, or any other method or any other thing that we need in successfully doing the review.

Spent fuel storage: We spend a significant amount of effort at the private fuel storage facility licensing action and hearings, but most of the work is focused on the natural and human-initiated hazards -- that's the accident analysis -- and the operational safety.

Decommissioning: There are two main things you might notice, the work on soil reuse which is work on use of sewage sludge, and then work Bill described, the multimedia environmental modeling, the multiagency Memorandum of Understanding. We are participating together with the DWM staff on that task.

Environmental assessments, including the West Valley Project and then the incidental risk closure. The one that's going on right now is the tank closure at Idaho Engineering Labs.

(Slide)

Again, to emphasize that all of the work I will describe is site and/or design-specific. It's focused on pending licensing action, including high-level risk work, of course. We commonly have short timeframe for completion. Most of the high-level waste work, which is probably the longest duration, is still planned on an annual basis. There's nothing like three-year research projects. We had that when we were working for the Office of Research, but in the site-specific arena, problems arise and they have to be done in six months, or nine months, or whatever the time period is.

Risk-informed: Of course, we are working extremely hard to bring this factor into all of the work that we do, to the extent possible, and sometimes it's limited by the information one has to risk-inform the review tools, and to risk-inform any review in the prelicensing period that we are doing.

And the work that we do is typically to confirm something. There is some exploratory work, as I will explain as I go on, but it's basically if a question arises, there is some critical need, and we say, okay, let's do some laboratory work and let's go out in the field and do something, or do analysis, for that matter, or do all three together and try to come to an answer at the end.

(Slide)

Funding, I won't spend any time. The 2003, of course, is still up in the air, as you all know. We are in continuing resolution area, but you can see quickly that we expect it to remain about the same as last year.

I'm not sure if you know what IME stands for. It's the Industrial Mobilization Exemption under which we are given work that's not part of the charge. It is still within the special competency of the Center staff, but the idea is to maintain certain Center staff that will eventually be needed in the high-level waste program. So this is extra work. As you can see, the West Valley Development Project, the Mixed Oxide fuel, the site decommissioning, the uranium recovery, West Valley, and so on. But the total amount, dollar amount, in the IME work is a fraction of what we do in the total work.

CHAIRMAN HORNBERGER: Budhi, just one quick thumbnail thing. What does one FTE cost you, including overhead?

MR. SAGAR: Well, that's a very complex question to answer. I'll throw out a number and then it will get criticized heavily because do I add all the clerical labor? Do I add what FTE am I talking about? But I think it is the right number you get manhours, more or less.

CHAIRMAN HORNBERGER: Okay.

MR. SAGAR: But, I mean, if I count the actual manhours including support staff, including technicians, it's much larger. So, it's hard to answer that question.

MEMBER GARRICK: You could answer it by category.

MR. SAGAR: I could answer by category, yes, but that would take two slides.

(Slide)

Quickly, the management structure at the Center hasn't changed, you have seen this many times over. We try to maintain the structure. Unless you have questions, the only point I want to make is that the management level -- and that is what is shown here -- play a dual role at the Center. They are line managers as well as project managers. Even the directors have projects that they manage. So, we think that it's highly efficiently use of the number of people we have at the Center.

(Slide)

I have only two examples for the non-Repository Program. Quickly then, the first one is the work that we are just completing. We have submitted draft to the NRC on the soil reuse. The basic issue here is that there is slightly contaminated soil, under what conditions could one let it be reused for something -- for gardening, under the houses, foundations or whatever. And the idea was to develop a set of scenarios, consider 39 radionuclides that normally could be present in the soil, and then look at particle curie for gram concentration, what kind of doses may be obtained by a landscaper, by a rural resident -- of course, we have to define all the other sets of parameters that these people -- of the work they would be doing, and the inhalation dose, ingestion dose, et cetera, et cetera, all those combined together.

And we found, for example, that 21 of the

39 radionuclides give the maximum dose in millirem per year per unit concentration to the landscaper. So we are trying to decide what concentrations may be acceptable for the landscaper scenario, may be the one that's limiting.

MEMBER RYAN: I have to ask this question. Cobalt-60 is more important than plutonium.

MR. SAGAR: It gives more dose.

MEMBER RYAN: Yes, so it's more important.

MR. SAGAR: In this case.

MEMBER GARRICK: Is there any significance to the fact that the landscaper and the rural resident are essentially the same?

MR. SAGAR: The rural resident -- well, the 12 of the radionuclides out 39 show the maximum dose to the rural resident, and he is eating food grown on that contaminated soil, et cetera, et cetera. So there are different pathways in these different scenarios.

MEMBER GARRICK: The two get their dose by very different pathways, but they are comparable.

MR. SAGAR: Right. And we are trying to keep this similar to the work on the sewage sludge because this is a general question I proposed, a regulatory question as to how one might reuse -- how one might determine the safety of reuse of this lightly contaminated material.

(Slide)

My second example is part of the future work from the West Valley Demonstration Project. It's a very interesting technical problem for a hydrologist, for example. One of the critical issues at West Valley for the decommissioning part is the erosion rate, and that's pretty high in that area. And a rough calculation shows that certain areas may be exposed through erosion in a few hundred years, but there are some other areas that would take thousands of years. But the problem is that the modeling itself of the long-term estimate of the erosion process is not really an accepted process yet. I mean, there is the Frank's Creek here that eventually discharges into Lake Erie. So, it's the issue that we think, as a cooperating agency in the decommissioning EIS that we think we may have to do some independent work just to check that the erosion rates are estimated properly. But that's in the future because DOE is still doing some work that is required to feed into such erosion modeling.

(Slide)

Now, going on to the high-level waste

area, I decided not to structure my presentation along KTI at this time, so I did more of a process in the approach, in the sense I start with near-field environment, then go to engineered barriers, then go to far-field environment, then go to performance assessment nuclear safety because that's how they are logically connected anyway.

And I present these things through examples only. I mean, you may have questions on other stuff that I may try to answer, but in the near-field environment -- I think you guys made a comment this morning in the Commission briefing that the reactive transport or the coupled process is still an area that is not completely settled, and that will probably continue, and this is one example of something that would continue, for example, what I would put in the bucket of performance confirmation at some point because we do not see that these issues will be settled before or even at the time of review of the license application. These are complicated processes, and they will be studied as part of the "research" done for performance confirmation.

But before the first step will be made, these four green dots that you see on this graph are observed data of silica concentration in the pool water -- unsaturated for water. And what we were trying to do is have a coupled model which is the thermal, hydrologic and chemical reaction model, coupled all three ways, and try to see if we could simulate somehow the ambient conditions, ambient chemical conditions which happen to be this line at the end, by running this model in a transient mode. And this is what we call a semi-equivalent stage. These lines that you see are the thermodynamic equal, if for example the silica has to be an equal-variant with water at temperature. The main flow period because of the temperature come in a geothermal gradient here. Normally, we would have thought that this line should be similar to this line, but that's not the case. And therefore the question is, well, what makes this to be a semi-equivalent stage for Yucca Mountain. Obviously, this is not a closed system. Silica is being brought by the infiltrating water, and there are other issues. But the point here was that we were going to calibrate the model to the major data and then use it for extrapolation in the future when we put the report together. So we start the heating process and the chemical reaction rates will change, the kinetics will change, and then try to predict what the chemistry will look like.

VICE CHAIRMAN WYMER: Why did you choose silica?

MR. SAGAR: Well, I'm told by geochemists that that's the most common chemical constituent that they could track.

VICE CHAIRMAN WYMER: Is that because it reacts with something?

MR. SAGAR: It reacts, yes.

VICE CHAIRMAN WYMER: I mean, you're concerned about it reacting with uranium or plutonium?

MR. SAGAR: Well, there is no -- I mean, this is ambient conditions. There is no repository yet. There is no wave form yet. It's water and silica coming from top and reacting with whatever other minerals are there in the rock.

CHAIRMAN HORNBERGER: They're basically silicate minerals in the rock?

MR. SAGAR: Yes.

(Simultaneous discussion.)

MR. SAGAR: The pollution precipitation processes are included in the model.

MR. PATRICK: I think I got your question right. Wes Patrick, Center. That sets the boundary condition. This is what the water is like that's going to come in, so this is the ambient case, and it then is used to drive the subsequent thermal calculations. So, we want to make sure that the model is getting the chemistry correct for the infiltrating water.

VICE CHAIRMAN WYMER: But unless the silica is -- the silica is important to the chemistry, then I don't see what this is.

MR. SAGAR: I have that here, the real geochemist.

MR. LESLIE: Brett Leslie, NRC staff. One of the issues that the State of Nevada has brought up is that the flux -- refluxing of silica will drastically change the permeability and porosity structure at Yucca Mountain, and the purpose of the Center's calculations really are we want to make sure that we can model the ambient conditions with this type of code, so that it can be used as an input to assess once you add that heat, that thermal condition, do you actually see what people have said from laboratory experiments.

VICE CHAIRMAN WYMER: Okay. So, it's whether or not you're plugging up the holes.

MR. SAGAR: Thank you, Brett.

(Slide)

Another part of the near-field environment is the question at what relative humidity does corrosion begin, corrosion is initiated. And the most common assumption in most of the performance assessment model is that the deliquescence point -- that is, the point at which the given salt gets saturated, fully saturated with water, is the point at which the corrosion will begin. And here in this graph, for example, the yellow triangles, we did an experiment in a humidity chamber, and these yellow dots here show you the evolution of humidity that was controlled by the experimenter from increasing and then decreasing here. And we had two metals here, the carbon steel and the stainless steel. The carbon steel is shown here in black and blue here, and the stainless here. The idea was to -- and then we had a multielectrode sensor to tell us when corrosion is initiated.

As you can see for the carbon steel here, up to this point, for example, of relative humidity, nothing happens, there's no corrosion, and then it is initiated. So, at this point then we say this is the critical relative humidity at which corrosion would be initiated.

MEMBER GARRICK: Is this work, Budhi, giving you any insight as to the viability of the DOE diffusive transport model? They have a threshold for -- a relative humidity threshold.

MR. SAGAR: That's correct.

MEMBER GARRICK: Is this --

MR. SAGAR: This is to verify that hypothesis.

MEMBER GARRICK: This is verifying that hypothesis?

MR. SAGAR: Yes. And what we show here -- and for steel, for example, the critical humidity is higher because the stainless steel is more passive or more resistive of corrosion. But the point here is that the humidity or the critical humidity at which corrosion will be initiated is a function of the metal itself, and then of the environment, the nature of the soil and so on.

I think the point we are making to DOE, or we have discussed with them, are two. One is that the deliquescence point is not necessarily the critical humidity. The critical humidity at which initiation occurs can be lower. Corrosion can be initiated at a lower humidity than the deliquescence point.

MEMBER GARRICK: And there's also the question of can it be sustained for long periods of time.

MR. SAGAR: Sure, but the initiation is one, and then the corrosion rate is another.

MEMBER GARRICK: Right.

MR. SAGAR: Right. But then the second point we are trying to discuss with them is that a single salt -- even if you agree, even if you believe the deliquescence point is good enough for initiation, a mixture of salt -- and we have done those experiments -- the deliquescence point for a mixture is different from a single salt.

MEMBER GARRICK: Right.

MR. SAGAR: So you have to do some experiment to give us both to any assumption you make in your model.

(Slide)

This is a work that we are starting right now, again, to look at the chemistry -- various types of chemistries of the dripping water on let's say the drip shield or the waste package if the drip shield fails and, as you can see, there is a different kind of pH range depending on the evaporation condensation cycle. And what we are doing -- we are doing it two ways. We are setting up a lab experiment which would actually take samples and measure the chemistry of the function of time. We would have a heater included in the experiment. And the second one is to do some, again, coupled modeling to see if we could figure out what kind of chemistry would evolve. So this is something that, for example, we are starting now. It may be another ten months or 12 months before we see some preliminary results.

(Slide)

Here is an example of another near-field phenomena which is the distribution of thermal loads. As we all know, there would be adjuvant in the sense that the temperature will not be uniform in a drift. The risk packages close to the edge of the drift will be cooler than the risk packages in the middle of a drift, obviously, because there is more heat loss on the edge. And then the question is would that thermal gradient transport moisture to the cooler edge, and if that is true, what would be the chemistry of that transported moisture, at what rate would the humidity change faster or earlier at the edge risk packages than in the middle of the drift. And, again, this is mainly modeling. This is not experimental data here. We are trying to see what kind of thermal gradient to expect. This last figure here shows you as a function of time the thermal gradient, the fraction of the drift with temperature gradient -- that is, the nonuniform temperatures -- and, as you can see, almost 80 percent of the drift could be affected by temperature gradient and, therefore, there could be moisture movement in this 80 percent complement of various drifts.

So, again, you may ask me -- and I'm surprised you haven't yet -- what is the risk significance of this. I'm sure John will do that pretty soon. But we haven't carried this all the way to the dose calculation, to answer that question, but my point here is that we have to do this kind of detailed analysis to feed it into a simplified model at some point, and see what effect, if any, this might have. And at some point, it may drop off that data screen if there is no effect.

MEMBER GARRICK: Are you doing anything to look at different concentration gradients, radionuclide concentration gradients, and how they would affect the diffusive transport out from the inside of the waste package?

MR. SAGAR: Yes. This is not the slide, though. Yes, we are doing that. In fact, there are two parts to that. One is the evolution of the chemistry inside the waste package itself, and then based on the concentration gradient created the diffusive flux that would be generated because of those gradients. And there is some modeling that has been done.

(Slide)

The Repository Scale Thermohydrologic Model was again -- as I said before in the beginning, most of these activities are undertaken because DOE is doing something and we have to respond or we have to review, and if we think something is important enough for us to actually do a simulation to see whether we can look at DOE results or not, then we do that, and this is an example of that.

The Cold Trap Process is the same as I described previously. This simply means that if some parts of the drift are cooler than other parts, then the cold part can trap moisture, that's all it means, Cold Trap Process. And this kind of multiscale thermohydrologic modeling -- what this scale simply means is that the repository scale is 2 kilometers wide, and the resolution we need in the modeling to look at the thermal effects and the moisture movement, there is not -- and somebody was talking of many flops in the morning -- we don't have access to those huge computers and it's just not possible to look at the scale uniformly that we can study all those effects.

So, we do multiscale, in the sense we break it up into three or four different parts or different scales -- you know, this scale modeling is first done at the final scale. The output from that feeds into a room scale, then a mountain scale, and so on and so forth. That's all the multiscaling. And here are some results of the temperature versus time depending on the effective thermal conductivity assumed for the rock.

(Slide)

Okay. So the near-field environment is -- that impacts the performance of the engineered barriers, and there are two barriers, of course, the drift shield and the waste package. And one of the questions in the waste package area is the effect of fabrication processes on localized corrosion, as I think has been more or less agreed that Alloy 22, the effect of corrosion rate is pretty small, the uniform corrosion rate is pretty small. Would there be any localized corrosion that may be faster and make holes in the waste package, that's the issue.

And welding, because you will be introducing a filler material and you will be changing the temperature of the metal, would that change the corrosion locally where the welding is. So, we have done some experiments which are mostly in the electrochemical cells, and the important thing to notice here is the repassivation potential, that's on the Y-axis of this graph, is actually a measure of how resistive the metal is to corrosion. The higher -- again, the metal and the near-field environment, of course, go together in determining that. The higher the repassivation potential is, the more resistant the metal is. So the higher repassivation potential is good, and lower is not so good. So, if you see any line, any point going down here, that's degrading the potential performance of that particular metal.

And here are the three particular elements that are displayed here -- the mill annealed sample that we did -- the mill annealed and aged for 4 minutes at 870 degrees C, this is the welding part, and the mill annealed plus welded. And you can see that the worst part is this TA, the mill annealed and the aged sample which are these red dots here, that the repassivation potential for this sample was the lowest of all that we tested. And all of the results are preliminary in the sense that these need to be verified, of course. And this line here shows you the corrosion potential, so if the corrosion potential is greater than the repassivation potential, there is a driving force to initiate the corrosion. And so long as it remains greater than the repassivation potential, the corrosion will continue. So, if you are below this point, I think there's something to worry about. If you are above this point, no localized corrosion would initiate or continue.

So the idea again is to look what would be the effect of the various fabrication processes on the localized corrosion process.

(Slide)

And then I think the several questions have been raised that we are more conservative or very conservative in our calculations. There is, for example, nitrate present at Yucca Mountain that may inhibit or reduce the rate of corrosion, so this is another experiment to try to look at the inhibition properties of nitrate on corrosion. Again, the repassivation potential is on the Y-axis and the nitrate-to-chloride ratio is on the X-axis. And these points here indicate that the repassivation potential is high as the nitrate-to-chloride concentration goes different from zero. At zero it is low. If there was no nitrate present, the repassivation potential is low. If nitrate is present, the repassivation potential is high. So, indeed, the presence of nitrate does inhibit the corrosion process. This triangle here is single experimental value where nitrate was introduced after crevice corrosion started, and then once the nitrate was introduced, it was arrested, the corrosion process was arrested. So, indeed, we were trying to verify if nitrate indeed has the effect that has been reported in the literature.

(Slide)

Well, the experiment of course needs to be checked, and I think one of the main issues in any long-term use of the model -- of results of the model is how mechanistic that model is. And for corrosion prophesies, as I said, so far we use the repassivation potential, the electrochemical potentials, as the main thermodynamic variable that is modeled as a function of time, pH, nitrate, chloride, et cetera, that are present in the environment.

Here is another effort at trying to do a mechanistic model which is based on point defect model, which is the defects of the fluid/solid boundary diffused in a layer. And this is at a very preliminary scale. We actually did experiments which is the red line, and you can see the experimental variation here, and the smooth curve in the middle is predicted by this new model that's being published.

Again, I would put this more in a category of long-term -- longer-term, not long-term -- longer-term work where if tested further this is found to be a viable model, and this is something we would use during the performance confirmation period as time goes on and as we get more data, more observations on actual material in the repository.

CHAIRMAN HORNBERGER: What causes the spikes?

MR. SAGAR: These are experimental. I think these are just variations in the experimental setup.

CHAIRMAN HORNBERGER: Those first three at the bottom look like they are 24, 48 and 72 hours.

MR. SAGAR: I could tell you what --

MEMBER GARRICK: You said something about Weible?

MR. SAGAR: Viable, yes.

MEMBER GARRICK: Did I hear you say that?

MR. SAGAR: Not Weible distribution, viable -- v-i-a-b-l-e, viable. If this model is verified and we say, yeah, this is a good one --

(Simultaneous discussion.)

MEMBER GARRICK: Oh, I see. I'm sorry.

MR. AHN: Tae Ahn of NRC staff. Regarding your testing time, this practice is to assure what DOE is measuring in the long-term testing for five years, even longer, until the repositories are closed. Also, this modeling effort assures the current understanding from of the corrosion process from the short-term testing like this is correct.

Also, we will incorporate the analog, for instance, as well to validate the short-term testing. So, all three or four efforts will be put together to assess the long-term integrity of the passive films.

MR. SAGAR: Thank you, Tae.

(Slide)

Another study that we're doing is to look at the natural analogs for the container material, and the two that are being studied are meteoric iron and Josephinite. And this apparently is a sample we got in Josephine Creek in Oregon, although the exact date -- this is approximate ages -- the exact date is not known. Of course, anytime you do a natural analog, the questions always arise how do you know under what condition this has existed for so long, et cetera, et cetera. So, there are a lot of assumptions you end up making even in the natural analogs, so there are no sure-shot answers, but it does give you some insights into the processes that these may have undergone, plus some qualitative comfort that if these things have existed for a long period, then there is some environment at least under which these metals would exist for a long time.

And we are also looking at the archeological, which is the pillar in New Delhi in India which is about 3,000 years old and still holds.

The industrial -- apparently there's lots more data on this, but this is a very short period. We're not talking about even hundreds of years like here. And sometimes the value of natural analogs of course is the scales, time scales especially, because then you can say, well, if something lasted for X-number of years, then there is some qualitative evidence that things would last for a long time.

I have already repeated this, but this Josephinite work will continue, for example, next year. We still plan to actually do more analysis of the components of the sample that we have, as well as do some modeling, to try to get some handle on what process this particular sample may have undergone.

(Slide)

This is very interesting, again, a very preliminary result that I'm showing you. But one of the concerns in the near-field is the drift stability.

It is not just during the preclosure period where there will be ground support and there will be maintenance, so I think even though that part of the report would be reviewed, this particular slide relates to the postclosure. And the basic idea here is that in the middle unit which is a fracture, which is about 25 percent of the drift location, large size rock blocks -- about 1 meter cube rock blocks -- could fall onto the drip shield. As you now know, the drip shield has dual purpose -- of course, the purpose of protecting the waste package from drips, also it has the purpose now of structurally protecting the waste package from any rockloads that may fall.

And we have been discussing this for DOE. We eventually forced the Center staff to do some actual calculation rather than come and say, oh, this thing can happen, that thing can happen. And here are some calculations that were recently completed. This is what the shape of the drift may look like in less than 1,000 years, for example. So this has a lot of implications in the sense that whether or not there is engineered backfill, you will have a backfill. And if that is true -- and this is a nominal case, for example, if we believe that this result is okay -- then all the other calculations that we have done so far need to be changed. We need to then rethink about the water flow to the engineered barrier system, the temperature and humidity estimations ought to change, the igneous activity consequence calculations ought to be different, et cetera, et cetera. So, this kind of permeates into -- throughout different components of performance assessment. And as I said, this is really very preliminary, but one needs to take a look at this.

(Slide)

In the lower where 75 percent of the other drifts are located, they don't have large single blocks, but eventually the drifts are going to be filled up more or less. How long it takes is a question that ought to be settled, of course, through more modeling and so on, but we try to estimate, very preliminary estimate of would the drip shields, for example, under accumulated rockfall -- not by a single rockfall, but over time as the rockfall accumulates -- how much static load, for example, would be on the drip shield, and would they buckle, and so on and so forth. And it seems like the drift degradation time, which is filling of the drifts, is between 50 to 100 years. Some uncertainties were factored into these calculations. The load, the static load may vary between 40 to 160 tons, and that the drip shields -- there's a likelihood that the drip shields would buckle and may undergo plastic strain and, therefore, may rupture. If that's the case, some of this load would be transferred to the waste package, and then of course we have to look at T-22. I understand that T-22 can take lots more plastic strain than the titanium drip shield can, or it can sustain or it can deform much more than titanium before it ruptures, but still those calculations would have to be done.

MEMBER GARRICK: If these results are true, then this has a major impact on the assumptions that DOE are currently making about the time of failure of the drip shield.

MR. SAGAR: Well, I might as well indicate that one of the arguments DOE has is that the rocks falling down would form an arch, and things will not fall.

MEMBER GARRICK: So there was a self-supporting --

MR. SAGAR: Self-supporting, which may be possible. It is hard to get sufficient technical support through modeling otherwise that that would be sustained for 10,000 years. With earthquake motions and so on, would that still be there, even if it forms initially. So, it's an issue that needs to be discussed and more work needs to be done before we conclude anything. I'm not saying this is it.

MEMBER GARRICK: Right, because their supplemental performance assessment results show 20- to 30,000 years before there's a major contribution of advective flow into the waste package.

MR. SAGAR: That's correct. But we are saying in addition to corrosion, the mechanical failure needs to be looked at seriously.

MEMBER GARRICK: It would be very interesting to see what the difference would be in this with and without the drip shield. In other words, if this is true, they may get just as much performance out of the waste package without the drip shield.

MR. AHN: This is Tae Ahn of NRC staff. Actually, our sensitivity studies show the drip shield contribution to radionuclide transport is not very significant. The other issue is actually DOE's position, unlike the corrosion design, DOE believes the drip shield or waste package can have different design feature to be resistant to mechanical failure. They could modify it in many different ways. Also, again, current assessment presented here needed to be looked more carefully, especially in the manual performance assessment including probabilities.

MEMBER GARRICK: Very interesting.

MR. SAGAR: And this is something we will be doing this year, in 2003, is to take your question -- for example, what happens when that load is transferred, whether or not drip shield is there, to the waste package, and that load is then transferred to the supports here, and these are concentrated -- the stress concentration would go here because all of the load would be transferred to these two support pedestals, would there be stress concentration to the extent that there would be waste package crushing. We don't know, but that's a question we would want to investigate in the coming year.

(Slide)

Moving on the to the far-field environment, one of the issues, of course, in the far-field is the effect of the alluvium, and this is in the Nye County well where they encountered alluvium unexpectedly at depth. This wasn't really supposed to happen, this was supposed to be organic rock. And then we were trying to see -- we sent people into the field to correlate this stratigraphic with exposures on the surface, and we could do that. The geologists were able to correlate. And then this became the exposed -- the exposed areas became natural analogs for these at depth so that we could measure hydrologic properties in the exposed part and assign the same to the stratigraphic at depth. So this is our field work that we did to try to find the alluvium.

(Slide)

Another piece of the field work was to include the detailed hydrostratigraphic framework of alluvium into the geologic framework model that we used for all performance assessments and hydrologic modeling. So, again, this was field work done. The main question here that's being investigated is the assumption that is normally made, which is that the alluvium is homogenous and isotropic -- that is, the modeling assumption that's normally made, and we wanted to investigate if that is a good enough approximation or not, and at this particular scale the observation in the field indicates that this is a very heterogenous material, that there is strong horizontal line of anisotropy and that this ought to be at least factored into the model, in the detailed process model to see what effect, if any, such anisotropy and heterogeneity in the alluvium would have on the estimated dose.

(Slide)

More field work -- geologists love to go out and drill and get the rocks, so here is one of the staff members we hired developed a permeability probe, and she took that to the Bishop Tuff Inyo, California, which is an analog to the non-welded tuff at Yucca Mountain, and the reason we were doing this work is to see if there flow paths and transport paths in the unsaturated zone. And the hypothesis we had was that most of the flow paths were adjacent to existing faults, that when the faults slipped and so on, that it created a fracture zone to some lateral extent from the fault. Therefore, what we did was we set up this measurement regime, permeability measurements, in a direction perpendicular to a fault, to see how far the effect of the fault was found, as far as permeability was concerned.

CHAIRMAN HORNBERGER: I can't tell, is that just an air permeator? Where is the air source?

MR. SAGAR: Where is the --

CHAIRMAN HORNBERGER: What is the air source?

MR. SAGAR: The air is pumped -- there are two faults -- I didn't bring it with me, but it's a neat little gizmo. There are two coats, one the air is pumped in and the other one --

CHAIRMAN HORNBERGER: What do you use for a pump, though?

MR. SAGAR: I don't know. But pressure transportation is used in the other --

(Simultaneous discussion.)

MR. SAGAR: And this is the kind of results we are getting.

(Slide)

This is the primary fault, and then we basically measure the permeability in this direction. And there were two layers that we measured the permeability, in this layer here which is about a meter deep, and then this layer at the top, and the blue one is the bottom layer permeability variation as you go away from the fault, and the general trend is decreasing. So what it tells us is that there is a certain distance up to which the fault has an effect, after which it dies out. And if the flow path is going to exist, it's going to exist in this kind of proximity to an existing fault. So, this is again trying to connect field work with some laboratory work with the calculation to see if we could draw a conclusion as to how these pathways would exist.

(Slide)

Here is the modeling part. This is the saturated zone modeling, and this is at a regional scale, and we were trying to see how best we could calibrate a regional scale model. As the foundation of this hydrologic model, of course, the geologic framework model where the stratigraph is, the data in terms of let's say hydropic conductivity is never sufficient for modelers. They can always make the final, and there's no way you can measure permeability at all points, so in the end you end up calibrating -- that's what Case 1, Case 2 means -- make some assumptions, try and see if you can match the measured hydropic curves, change those assumptions, see how did you meet them better, and since there are quite a few measurements on hydropic heads, there is -- you do a release type of fit to see what gives you the least residual error, and this is the DOE base case where these points here represent the actual measurements of hydropic head.

(Slide)

And then we calculated the travel time by calibrating the model one way versus calibrating the model another way. The travel time is calculated by releasing certain fictitious particles and tracking them to the exit boundary. And the results can be quite different. Again, the idea was the model uncertainty could affect whatever your end results are, so you might have to try a few alternatives just to see what satisfies you, what fits in with the field data and the rest of the physics of the problem.

VICE CHAIRMAN WYMER: You said that was the travel time to the site boundaries?

MR. SAGAR: To the 18 km boundary.

(Slide)

On sorption, I think Bill Ott described a few activities in which we also participate, the NEA one. But this one is on colloids, and basically the colloids form certain radionuclides, and the colloids then move unimpeded. And the question was is the sorption of the colloids -- how irreversible is the sorption on colloids. And the basic -- this is a stochastic model that was developed and worked together with a consultant, was that as the reversibility -- irreversibility increases. If it was completely irreversible, there is a lots more transport with colloids than if it is reversible. So the next question is, well, how do we determine what sort of reversibility do we expect on colloids.

And in the PA model, the simplified model, of course, this is just a factor. X-percent of the radionuclides would be transported by colloids, and to provide some technical basis to whatever that X happened to be in the PA model, you have to do some experiments of the modeling exercise. This work is still continuing, by the way.

VICE CHAIRMAN WYMER: That's experimental work?

MR. SAGAR: This is model.

VICE CHAIRMAN WYMER: What did you assume was the mechanism for sorption of plutonium colloid?

MR. SAGAR: Don't know. Anybody?

CHAIRMAN HORNBERGER: I don't think it's a mechanism, as it says here it's a two-site kinetic model.

VICE CHAIRMAN WYMER: What does that mean?

CHAIRMAN HORNBERGER: Kinetic sorption just means that it's a rate-dependent process of sorption, and a two-site means that you have two different sorption sites, one with a rapid short kinetic time constant and the other with a slow one.

VICE CHAIRMAN WYMER: Doesn't mean much unless you really specify which one.

MR. SAGAR: Thank you, John. I now remember a little bit of it. But, yes, the long-time constant one is where the reversibility is long, and the short-time constant one is where the reversibility is -- where it is reversible. So, that basically is what is being studied here, what degree of reversibility would affect the result by what amount.

VICE CHAIRMAN WYMER: Okay.

MR. SAGAR: What the actual mechanism is I don't think is in this model. Again, if you have questions, I'll try to get you more answers by the people who actually did this work.

VICE CHAIRMAN WYMER: No, I know there's a lot of colloid work going on, I just wondered in this particular case what the assumptions were.

MR. SAGAR: And there's a paper I can get you that's been published on this work.

(Slide)

And the same basic idea is shown here, that as the irreversibility increases, the flux increases of plutonium, and there were three different kinds of sorption models used in the estimation here. This is all modeling. But this is still -- a lot of assumptions in this model, including the filtration and retardation because there's a lot of questions about the colloid size and if the size is large, what this difference allowed and therefore this would be a retention process rather than a transport process, and that could entirely happen. I cannot say personally this would necessarily get us to the final answer on colloids but, again, some of these items would probably continue to be worked at in the post-licensing period.

VICE CHAIRMAN WYMER: Each of those models has arbitrary constants. The answer depends entirely on what you select for those constants.

MR. SAGAR: But that is the beginning, and those constants of course -- that's what I'm saying -- those constants would somehow have to be provided some technical support.

MR. LESLIE: Budhi, can I add something? This is a perfect example where the acceptance criteria for assessing whatever the process is with alternate conceptual models kicks in. You can see that there's quite a big -- there might be quite a big difference between how you conceptualize colloid transport using these types of models, and I think that's what one of the things Budhi is trying to show, is that you have to think about these things because it could have major impacts.

VICE CHAIRMAN WYMER: So we have to really keep in mind that he's not trying to show what he thinks is going to happen, but he's going to show the range of things that might happen.

MR. SAGAR: I don't think anybody would know what's going to happen, so it's the range always, and the range could be narrow or the range could be wide, but that's about all you're going to get, realistically.

(Slide)

NEA Sorption Project -- again, Bill touched on this. By the way, they have eight experiments, Bill, if I remember right, and in fact the primary objective of this was to look at the capability of sorption models to produce reasonably accurate results and match with experimental data. These were not blind testing in the sense -- these modeling teams, and there are 16 or 17 of those teams, including from Research and us from NMSS, trying to model those selected experiments. Two of these models use data that was generated at the Center, so we were rather proud of that selection of those data sets at the international level. But, again, I think the KD value of the -- that's generally used in the PA models is, of course, maligned by all red-blooded geochemists because that's no good, but most of the capabilities of the performance assessment models cannot really -- at this point, cannot directly use the surface complexation models, although that's one of the objectives that Bill showed he would like to do. Of course, if you do this, why not go to molecular scale and hydrology. Those are all sort of questions, why this and why not something else.

So the approach we are using is that we would use the surface complexation model as a process level model, as a mechanistic model, and then try to drive the KD value that would be fed into the performance assessment model. But, again, the surface complexation model itself has seven or eight free constants, as you said today, or variables, and there's no way to get their value based on panoramic data. So, again, you have to do calibration, as I show in my next slide.

(Slide)

This is calibration of the model that we have. We've actually measured lab data. But the idea was if we can calibrate this once, can we then use the same calibrated model for extrapolation. And as you can see, the results are really good. Of course, there are some questions that this matching should be done without the investigator knowing what the experimental data is. But, again, my point would be this looks good, this gives confidence in the model.

This accuracy is not something that would be in the PA model, for example. The PA model would be much more rough and would have a bigger range.

(Slide)

Bill also touched on the molecular simulation and one of our staff is doing some work in that area, and I always questioned him what good is this, but this again a longer-term project in the sense that the final objective is the same -- can we provide more mechanistic foundation to the sorption modeling? Can we take the molecular simulation model and go to the retardation factor eventually? Then you have some link between the science and the constant that you are using in your code. By the way, there was in the PA model, as far as sorption is concerned, is that the KD is not a constant anymore, we have made it a function of pH content, so that it varies as the chemistry of the water changes. So there are some changes to even the performance assessment model as far as sorption is concerned.

CHAIRMAN HORNBERGER: Budhi, are the people at the Center collaborating with these folks from Sandia?

MR. SAGAR: We cannot.

CHAIRMAN HORNBERGER: So this is -- you're doing this separately?

MR. SAGAR: Sandia is DOE.

CHAIRMAN HORNBERGER: Yes, I know.

MR. SAGAR: We read what they do, but collaboration, no.

CHAIRMAN HORNBERGER: It just strikes me as interesting that the NRC would support two different people to do the same thing.

MR. OTT: One of the advantages of the international arena such as the sorption project is that's an area where we can actually interact with people from DOE without running into these conflict differences problems. In this particular case, however, we haven't involved Sandia ourselves in the NEA project. Sandia has been involved with USGS. When we had a sorption modeling workshop up here, I guess it was 18 months ago, the Center was up here, our Sandia contractors were up here, USGS were up here, so there's been two-way flow between us and Sandia. The Sandia people that are working for us are not the ones that were working on the high-level waste program as well. So there's been two-way flow of information, it's just not been as direct and as frequently as might be desirable.

(Slide)

Okay. The igneous scenario -- of course, there are two parts to it, the probability and consequences, and I think you said to the Commission this morning they shouldn't be separated, although you do have to estimate them separately. You have to estimate the probability and whether or not that probability is less than or greater than 10^{-8} which is specified in Part 63. I think most people now agree that the probability is greater than 10^{-8} per year, and therefore has to be included in the scenario calculations into the consequence calculations. And I won't spend much time -- I think you talked about this part here, that there were anomalies in the aeromagnetic data and whether or not those are actually buried volcanoes. I'm told by the geologist the only way to be sure about that is to drill through them, which we don't know what the cost would be, but you can play modeling games, of course, try to estimate the probability if these were the ages of these anomalies and if X-number of them were actually volcanoes versus Y-number, and we find that the range really, as you said, doesn't really change a whole lot -- the probability range. So, it's probably not cost-effective to drill through all these 22 or how many anomalies here.

CHAIRMAN HORNBERGER: We were briefed by Brett, I think, at our last meeting, and of course the middle one there, he says under those circumstances you get almost an order of magnitude increase. So, yes, we've been briefed on that.

MR. SAGAR: Okay.

(Slide)

And the magma-repository interactions, as you guys said this morning, of course, there was an effort that we did -- and I thought it was a pretty good effort in the sense that this gave us some sense as to what kind of consequence modeling may be needed, and the short effect of course will be even less if we go back to the backfilling of the drift.

The only negative thing I found with the backfilling was the effect of any possible slip on existing faults. Apparently then even a little slip might crush a waste package. So there's nothing that happens here that is mostly positive or totally negative, there's always two sides to any effect. But we intend to work on developing a more realistic magma repository interaction model and consequence estimates in the coming year.

(Slide)

In the faulting scenario, there was a

faulting model, the fault structure that we built into the TPA code, the Total Performance Assessment Code, and this was the second methodology that the geologist came up with, which is that to look at the analog model -- the example given here is the Borah Peak in Idaho, I think between 6 to 7 magnitude of the record in 1983, and then to look at the distribution of the slip on not just the primary fault, but the secondary fault, and take this an analog for Yucca Mountain, give it this distribution, this type of statistical distribution of slip of secondary fault, and see what effect this might have on the repository, how many waste packages may be intercepted, et cetera. This, of course, may also depend upon if in the DOE design there is a setback distance for waste packages, for example, from all active faults that may change any calculation we do. But we find the calculations, very preliminary calculations that we have done, that if you multiply the mean peak, or peak mean conditional dose with this probability here, that the dose really that we estimate even from the distribution faulting is really small.

CHAIRMAN HORNBERGER: How does faulting cause failure -- package failure?

MR. SAGAR: We assume that the slip would have to be certain amount, like 1 meter, for example.

CHAIRMAN HORNBERGER: But is it through crushing because there is backfill around the canisters?

MR. SAGAR: We don't assume any -- this particular doesn't assume --

CHAIRMAN HORNBERGER: So how does the seismic event -- so it's a displacement -- in other words, there's a new fault through the --

MR. SAGAR: No, this is an existing fault --

CHAIRMAN HORNBERGER: An existing fault. So you're assuming that they are putting the canister lengthwise across an existing fault, and then you're getting slip along that fault. Okay.

MR. SAGAR: And, in fact, most of the intersections are on faults. I think one of the considerations here is that this work can be many hundreds of meters, that the setback distance probably from all faults is not possible, even if it's from primary fault, and therefore there will be intersections of risk that way.

MEMBER RYAN: I guess I'll jump in with the "so what" question here. Based on the dose of 70 micorems per year, it's not important, I would say.

(Simultaneous discussion.)

MR. PATRICK: If I could just to highlight what Dr. Ryan said, this is an example of where the study shows that from NRC's perspective, knowing what we know today, there's no need to continue to pursue this matter. It's a good place where risk insight has said we can close this one off, and those agreements with the DOE have been satisfied now.

The next step, the one Dr. Hornberger mentioned, the seismic issue remains open. We've got a report that we expect to see from DOE, and then based on what may come forward, we'll do a similar sort of an analysis.

MR. SAGAR: And that's very important, Dr. Ryan, because we're emphasizing to DOE that when they say this is not important, it's excluded, we say, well, you have to provide some basis why this is not important, and that's as important as things are important because when you look at there is a safety case, both have to make sense. You don't have to do as much work on the unimportant, but some work has to be done.

MEMBER RYAN: I appreciate that.

MR. SAGAR: The total performance assessment, I brought only one slide.

(Slide)

This is a recent completion of the sensitivity analysis, and the score in the right most column here, there are seven different methods that we use for sensitivity analysis, and they include estimation of local sensitivity which is more of a differential kind, and a global sensitivity as it is called which is across the range of the parameter name, and this is primarily parameter sensitivity, not model sensitivity.

As you know, there are -- I'm not quite sure how many total -- but a little over 1,000 total parameters in the TPA code, of which about 330, I think, are given probability distributions are sampled for performance assessment. And out of that, about ten at most are the parameters that really affect the final result. And, again, at one time, I think, following Dr. Garrick's advice, we were trying to develop a simpler model that would only use ten rather than the 350. We haven't really completely figured out how one would do that because then if you do these ten and you then say, oh, we gained something, what was the effect of that? Well, we can't do that with these ten because that may change something else, and something else may become sensitive, and so on and so forth. But, anyway, we use seven different methods, and seven out of seven said No. 1 is No. 1. So this was trying to get confidence that indeed the method itself did not change which parameter was sensitive, and that's essentially what's enumerated here.

MEMBER LEVENSON: Waste package failure does not show up among the ten most important?

MR. SAGAR: The drip shield failure time does show up.

MEMBER LEVENSON: But waste package failure does not?

MR. SAGAR: The waste package initial defect refraction shows up, but not the other. I think the range -- yes -- Tae knows the answer.

MR. AHN: Tae Ahn, NRC staff. This sensitivity analysis was performed with long waste package lifetime, so you see release either from the failure of a container within 10,000 years or very later time when container is gone. That's why you don't see the container lifetime effect here.

MEMBER LEVENSON: Well, I guess my follow-on question then is how can the drip shield failure be important if the canisters, the waste packages, haven't failed?

MR. AHN: Well, again, drip shield

controlled the water inflow to the initial failed container to release the radionuclide. Also, that's the main factors in this type of analysis.

MR. SAGAR: I would rather not -- I hate to contradict Tae -- but actually we do not assume waste package to the long-term. The waste package parameter, what I would determine their lifetime, have a distribution, and as you sample that distribution, they still last for 10,000 years. Therefore, they don't show up. And this is one caution that you have in any sensitivity analysis, that some things that don't show up, they don't show up because they did X and that X doesn't affect the dose.

MEMBER LEVENSON: I understand that, but the fact of a drip shield failure is that it allows water to go into a failed canister. If you don't have a failed canister --

CHAIRMAN HORNBERGER: They are juvenile failures.

MEMBER LEVENSON: What?

CHAIRMAN HORNBERGER: They are initial failures.

MR. SAGAR: The drip shield failure causes water to enter into these waste packages give you the dose in 10,000 years.

CHAIRMAN HORNBERGER: There are defects if there are juvenile failures, and so it's water getting onto them that gives you --

MR. SAGAR: What this is telling you is that in the first 10,000 years the dose is entirely due to initially defective containment, and anything that affects this --

MEMBER GARRICK: Yes, but maybe to clarify Milt's question, if you remove the 10,000 year requirement and look at it in the context of what's most important to eventually getting a release, you're still saying that the average mean annual infiltration is most important in terms of the gorilla having access to the waste package, namely, water, and eventually leading to a release.

MR. SAGAR: In the 100,000 year --

MEMBER GARRICK: And eventually leading to a dose.

MR. SAGAR: That's right. In the 100,000 year, for example, the waste package failure come up very high.

MEMBER RYAN: John, I think that's right, but doesn't it have to be coupled to the fact -- and to me the critical assumption is whatever the defective fraction assumption is. If you have no defective fraction, you have no release. The drip shield doesn't matter. So to me, it's interesting to see the scoring, I think that's a very effective way to do it. But when you start to sort through these and say what's important, what's not, what are the critical true assumptions versus some distributed -- distribution in sampling, I get down to the defective fraction assumption is really the critical assumption, and everything sitting on this ranking --

MR. SAGAR: If they were all initiative defective but no inflow, there would still be zero dose.

MEMBER RYAN: I take those together.

(Simultaneous discussion.)

MR. SAGAR: And I completely agree with you that anybody wants to interpret the sensitivity analysis, one has to be extremely careful and interpret what -- simply a table like this is not going to tell you.

MEMBER RYAN: A couple different assumptions, you could get a different ranking.

MR. SAGAR: Completely agree.

VICE CHAIRMAN WYMER: Neil, do you have a question?

MR. COLEMAN: Neil Coleman, ACNW staff.

Something to think about for the Performance Assessment Workshop, DOE recently sent in a product they are trying to resolve agreements on infiltration, and their basis for closing is that it was -- their results were very insensitive to infiltration. So we see there are difference in approaches.

MEMBER GARRICK: Well, again, they're talking about the 10,000 year compliance period.

MR. SAGAR: Right, plus knowing --

MEMBER GARRICK: But it certainly could have a major impact on the environmental issue having to do with time of the peak dose.

MR. SAGAR: Right.

VICE CHAIRMAN WYMER: Insensitive isn't a good reason, it's at what point it's insensitive.

MR. SAGAR: You could do that, too. I mean, it could indicate what point may become sensitive, and that's what I mean by global versus local. We could pinpoint that, too.

(Slide)

The two slides on the preclosure safety analysis. We're just taking the increased importance at the Center because as the anticipated date for the potential license application gets closer, this can become quite important, as important as postclosure as a matter of fact. And this basically lists the logical sequence of steps that you have to do in trying to estimate the preclosure operational risk.

(Slide)

And the slide on this side here indicates that existing software that we took from NRC, put them together with the executing module, so that we can calculate the preclosure consequences of accidental conditions both for the worker as well as for the member of the public, using the standard approaches of PRA really for the preclosure part. Just an example of the accident where a single BWR assembly falls off and releases some fraction as given by the national standards into the air, and what sort of doses to expect from that sort of an accident.

This is still pretty preliminary in the sense that even DOE is at the preliminary stage in developing the preclosure safety strategy, the identification of systems components important to safety, et cetera. So this work, in some sense, lags behind the rest of the work, and we are trying to catch up for the preclosure safety analysis.

(Slide)

In summary then, I don't need to repeat what I have said. I'll just point you to the third bullet, which is that some of the questions that I raised during my presentation will continue to haunt us during the review and beyond, and that perhaps what would go into -- some work will continue during that period, depending on the availability of resources, but that would be something in performance confirmation -- we may call them safety-related issues. Some of them may factor into the inspection program, it's not clear yet how that would be done.

And I thank you for your time. I took longer than I should.

VICE CHAIRMAN WYMER: We asked a lot of questions. Thank you, Budhi, that's -- my reaction to this is it's very impressive range or spectrum of the things you have going on, especially when you recognize that you've just been able to present a little snippet of the totality of what you're doing down there, these are just little samples.

Are there any questions? Mike, do you want to jump in again with any questions that you have of Budhi?

MEMBER RYAN: No, I think I asked a couple as he went along.

VICE CHAIRMAN WYMER: You asked your piercing questions. John?

MEMBER GARRICK: I'm just trying to satisfy in my own mind how you decided what to do. Slides 3 and 4 kind of beat around that bush, but -- and I'm always interested in knowing if the preclosure safety analysis and the work and the performance assessment work were the principal beacons for pointing you in the right direction, or was it something else?

MR. SAGAR: No, both are the beacons that point us in the direction of work, but there is a two-way feedback between the process level people and the performance assessment and the preclosure safety assessment people, and the topics can arise from either side, either the performance assessment person can say this doesn't look nice, or you have this factored into the model, what does it mean, give me some basis why this factor is such, can you do some work for me, does it need lab work, does it need modeling work, whatever, or it can come from the process level modelers, and the staff at the Center is mixed, of course, with people who do both performance assessment and same people do process level. For example, this recent example I gave you on rockfall came from the process level people. The PA people simply said how big a rock can fall and what can it do. They weren't thinking about accumulation of rock on the waste package. The process level people say, well, when we modeled it, this is what happened, or should you put that in here. So they developed a module that we are now going to put into the vehicle to see what the effect at the bottom line would be if we introduce this into the model.

So, it comes from both -- I cannot say it's all top-down honestly. More of it is top-down, but there is some that comes from -- and I personally, as a manager, I feel that's a good thing to do because it doesn't shut the door from anybody asking a question.

MR. LESLIE: Could I add to Budhi's comment, Dr. Garrick? It points back to a point that was raised in the Commission briefing -- I think Dr. Hornberger's presentation -- I can't remember, there were so many of them this morning -- you probably feel the same way -- whichever presentation it was, it was talking about the Risk Insights Task Force. It may have been yours, Dr. Garrick, I can't remember who covered that. But you emphasized --

MEMBER GARRICK: That's when you were asleep.

(Laughter.)

MR. LESLIE: To prove I wasn't, you made the point several times how important in that first step in the risk insights it was to get the staff talking, the PA experts talking with the subject matter experts. Budhi's point is well taken. And the way that worked in process -- and it was bloody at times, frankly -- was to have both sides open up to the fact that the technical -- the detail subject matter experts had to come to understand and be comfortable with getting to the bottom line, as folks like to say. On the one hand, if after a period of time one cannot show that these processes are important, it's time to close them out, to say we know enough about those, and on the other hand to have the PA people to come to understand that there may be things missing from the model, and that process level people can have those high experiences or bring in their own in-depth understandings of hydrology and rock mechanics and what have you, and then bring those into the model and test them and try them out, and I think that's what's made that communication step very important, and we're working hard to continue that on through. So, as Budhi says, both sides can raise the issues and, frankly, to make the point on the other side, the geologists were pretty well convinced that direct fault disruption was not an issue, but we had them put it into the fault module, work it through the risk calculation to establish/convince ourselves that it is down in the -- I don't know what it is -- femptilrem (phonetic) level, or whatever it ended up being.

MEMBER GARRICK: Well, of course, our position is not to close off any source of information that would suggest a direction the research ought to go, but you should certainly simultaneously be relying most heavily on those things that are deliberately and systematically trying to find the soft spots and the uncertainties associated with those soft spots, and I was just trying to get some insight on how influential that was.

CHAIRMAN HORNBERGER: I can't remember when it was, but I think it was Ray and I visited the Center and sort of explored this issue at length with Budhi and Wes, and I must say from my standpoint, and I think from -- I think that Ray agreed -- that we thought that your process was pretty good -- that is, it's pretty much as you describe it. You really are using all of the performance assessment insights, but you also bring a lot of personal knowledge and skepticism and everything else. In fact, I think we tried to capture some of it in the last letter we wrote about the NRC research.

So, I think that John does make a good point. We really pushed on it when we interacted with you, and at least Ray and I came away satisfied that you were doing it pretty well.

VICE CHAIRMAN WYMER: I think it's clear that you're not leaning back and saying, well, we've got it pretty well under control now, we can relax. Of course, it's in your interest to not say that, but still I think that's a proper attitude.

CHAIRMAN HORNBERGER: Milt?

MEMBER LEVENSON: I guess I've got a half a dozen questions, and that's maybe a reflection on the quality of the presentation because people who don't have anything to present, or nothing new, I never have questions to ask. So I've got a number of questions, I think it means there's some good things presented.

On Slide 8, if we could get that --

(Slide)

When I first looked at it, it looks like Cobalt-60 is the predominant source of radiation. But then I look at the bottom and the dose is millirem per year per picocurie per gram, and now I have no way to assess on that graph what dose is important because I don't know whether there's 100 times as many picocuries per gram of cobalt or plutonium. It's kind of strange unit, so I don't know what it means.

MR. SAGAR: Well, the point here was that if cobalt was 10 picocuries per gram, then the dose would be ten times. Again, remember the objective was to determine what kind of contamination may be acceptable for reuse of the soil. So, depending on what is acceptable, you can. So, so much of cobalt, so much of nickel, so much of plutonium would be okay.

MEMBER LEVENSON: So this is really just a conversion factor.

VICE CHAIRMAN WYMER: That's all it is.

VOICE: These kind of details are helpful when you're decommissioning. If you know your concentration, then you can very quickly --

MEMBER LEVENSON: Yeah, yeah. Okay. But that wasn't very clear.

MR. SAGAR: No real soil was being modeled. No entry was known.

MEMBER LEVENSON: On Slide 12 --

MR. SAGAR: Don't go too fast, that's what messes it up.

(Slide)

MEMBER LEVENSON: Can I infer from here that based on these measurements, at least for what was measured here, that while the corrosion rates increased for a while, they rather quickly -- in less than 40 days -- drop way off and corrosion will stop?

MR. SAGAR: Because the humidity was reduced in containment.

MEMBER LEVENSON: So that taking the initial high corrosion rate and multiplying that by long units of time is not a proper thing to do.

MR. SAGAR: No. You have to do the corrosion rate as a function of the environment, which is a function of the time, and if the humidity cannot be maintained at or above the critical, you don't have corrosion.

MEMBER LEVENSON: On Figure 14 --

(Slide)

-- in calculating humidity, were these calculations done assuming a gas type system? The USGS has measured the breathing rate of the mountain as a very high number. Was that taken into account in calculating relative humidity?

(Simultaneous discussion.)

MR. SAGAR: This one is temperature versus time, right? And this is a multiphase, so this is not gas-type. This is temperature, not humidity.

MEMBER LEVENSON: Okay. I was looking at the term "condensation". Sorry.

MR. SAGAR: Condensation plays a role in calculation of the temperature.

MEMBER LEVENSON: Yes, but the condensation that plays a role in the temperature is a function of the humidity.

MR. SAGAR: Sure, but I'm not showing humidity here.

MEMBER LEVENSON: No, no, no, I know. But in arriving at these, you did use humidity as part of the calculations.

MR. SAGAR: Definitely.

MEMBER LEVENSON: How did you get that humidity?

MR. SAGAR: Well, this is the water balance and the calculation of liquid water versus vapor is continuously tracked in the model. It's a multi-phase model.

MEMBER LEVENSON: So you're assuming a liquid phase on equilibrium at that temperature.

MR. SAGAR: Right.

MEMBER LEVENSON: On Slide 16, what is the relevance, or how would I convert the repassivation potential to a corrosion rate? In other words, between two points here that I'm looking at, one is plus-300, one is minus-200, that's a range of 500. Does that mean the corrosion rate is twice as much? Ten times as much? A hundred times?

(Slide)

MR. SAGAR: No, the repassivation potential is only an index to tell us when the corrosion is initiated, not the rate. It has no relation to the rate. When corrosion would be initiated and when corrosion can continue. If I was plotting rate, it will be on the Y-axis. That's what converts into the rate.

MR. PATRICK: Mr. Levenson -- Wes Patrick here from the Center. The reason that approach is taken, and it's only taken with respect to localized corrosion, I think it was Sugikawa in Japan who first came up with this repassivation potential. He convinced himself, and our data indicates that's the case, that in these nickel alloys the localized corrosion rates are so rapid that once you have onset of corrosion for material thicknesses of interest here, rate is unimportant. So the goal in designing and employing these materials is to stay out well above the corrosion potential, and that's the concept there and why they don't push to the repassive to the actual corrosion rate.

MEMBER LEVENSON: I understand that. My next is a comment on Slide 30, which is really relevant to the last couple also, and that is I understand what's been done, and I have no quarrel with it at all, but the point I wanted to make in this day and age of public communication is a number of these things are basically parametric studies, they are not projections, they are not rates. And I would hope that when you publish this information, you'd make that very clear. There's nothing wrong with doing it, I'm not quarreling, but I think there's a tremendous basis for misunderstanding.

MR. SAGAR: That's an excellent suggestion, and we'll try our best during the reprocess to make sure that happens.

VICE CHAIRMAN WYMER: Anybody else around the table have any point?

CHAIRMAN HORNBERGER: Budhi, this is one that will be easy for you, it's near and dear to your heart, I'm sure. You mentioned the studies on the alluvial deposits in Fortymile Wash, and you said that the conclusion was that you had a fairly substantial horizontal-to-vertical conductivity ratio. What is the scale? I mean, the picture you showed suggests that the scale -- you're talking about anisotropy that's due to heterogeneity that's on the order of meters thickness and units.

MR. SAGAR: We haven't actually gone and measured the alluvium. The earlier measurement I was showing you was on the Bishop Tuff.

CHAIRMAN HORNBERGER: That was your air permeability. Unless I misunderstood, you were talking about the need to consider the alluvium to be anisotropic, and I was just curious because -- the question is, is the anisotropy due to the heterogeneity, the horizontal heterogeneity, is that the scale that you're talking about?

MR. SAGAR: My understanding it is not because of the heterogeneity alone, that it is a much more systematic due to the sedimentation -- the way that the sedimentation or current form the alluvium layers, but it's more of a structural geology issue. It's a much larger scale than just a meter scale. The heterogeneity will give it's own anisotropy, but that's not the case here.

CHAIRMAN HORNBERGER: Thank you.

VICE CHAIRMAN WYMER: I'll ask again, are there questions around the table? Mike?

MR. LEE: Budhi, a number of the exhibits that you reviewed this afternoon speak to a couple of processes in the near-field. Do you have any general observations about the state of process knowledge, where we're at and how that translates into modeling for performance assessment?

MR. SAGAR: I will give you my take on that. I think the process is still at an early stage. The four process coupling is really done. We have gone up to the three processes -- the thermohydrologic and chemical, or we have done the thermal mechanical and hydrologic. And still there are a lot of issues if you really want to gain confidence whether the results of the simulation are any good. And that's why the calibration exercise. We wanted to see does a one-dimensional calibration on four points that we knew the data that we knew whether even that can be done, and that took a while. So, the state-of-the-art is advancing pretty rapidly I think, but it is still at the initial stage.

MR. LEE: So this potentially could be a ripe area for evaluation in the context of performance confirmation should it get to the licensing stage.

MR. SAGAR: I would say it would.

MR. LEE: The other question I had I guess reverts -- refers back to the last slide and the discussion of the -- I may characterize this the wrong way -- the integrated safety assessment or whatever you were calling preclosure safety.

We know that DOE is still kind of searching for proposed design in the context of funding and budget and things like that, and timetable. How amenable is the preclosure safety assessment tool to implementation in the context of an evolving design?

MR. SAGAR: It has significant flexibility, but I cannot stand here and say all changes that DOE may come up with can be accommodated. So there may have to be some changes made to our --

MR. LEE: I guess I'm kind of asking a leading question. I guess at some point in order for the tool to be implemented, there has to be a design to evaluate it against, and there's that lead time, if you will, for factoring that information into the tool before the tool can be exercised and a decision made. I see Wes shaking his head.

MR. SAGAR: I don't think -- personally, I don't think it's a great idea to box the DOE and say one and only one design needs to come out. I think they can carry forward whatever number of designs they want to carry forward, so long as they build safety cases for all those ultimate designs. And if they can do that by December 2004, then we can have a tool ready to do those. I don't think that's a practical thing for them to do. So there would be high thermal load, there will be low thermal load -- I'm sure those options would still be there. Can we accommodate those in this? Yes. Now they are talking about other stuff, a different kind of a transporter for the waste package --

MR. LEE: No rail.

MR. SAGAR: Well, we tried to imagine -- as soon as we heard it, we said, okay, what data exists on the safety of that transporter. Well, we couldn't find any. But in a safety case, in a preclosure safety case, they would have to come up with some safety data indicating why that transporter would be okay.

MR. LEE: Right. The only reason I raise it is in the context of any potential licensing review, once the license application comes in the clock starts ticking, and there's a need to exercise the tool and evaluate the implications of the analysis in the context of at least preclosure requirements and things like that. So, again, this is an area I think possibly ripe for some discussion with DOE sooner rather than later.

VICE CHAIRMAN WYMER: If there are no more questions, I'd like to turn it back to George to declare a break.

CHAIRMAN HORNBERGER: You've sort of tied my hands. Actually, that's exactly what we're going to do. I think that this will complete the recorded session. We won't need the Recorder. We'll go off the record. We will take a 15-minute break, and then we will reconvene and continue work on our reports.

(Whereupon, at 3:40 p.m., the recorded session of the meeting was concluded.)