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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON NUCLEAR WASTE

September 21, 2005

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

163rd MEETING

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WEDNESDAY,

SEPTEMBER 21, 2005

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LAS VEGAS, NEVADA

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The Advisory Committee met at 8:30 a.m. at Pacific Enterprise Plaza Building One, 3250 Pepper Lane, Las Vegas, Nevada, Dr. Michael T. Ryan, Chairman, presiding.

MEMBERS PRESENT:

MICHAEL T. RYAN, Chairman

ALLEN G. CROFF, Vice Chairman

JAMES H. CLARKE, Member

WILLIAM J. HINZE, Member

RUTH F. WEINER, Member

ACNW STAFF PRESENT:

NEIL M. COLEMAN, ACNW Staff

JOHN FLACK, ACNW/ACRS Staff

ACNW STAFF PRESENT (Continued):

LATIF HAMDAN, ACNW Staff

MICHELE KELTON, ACNW Staff

JOHN T. LARKINS, Executive Director, ACNW/ACRS Staff

MICHAEL LEE, ACNW Staff, Designated Federal Official

RICHARD K. MAJOR, ACNW Staff

RICHARD SAVIO, ACNW Staff

MICHAEL SCOTT, ACNW/ACRS Staff

SHARON A. STEELE, ACNW Staff

ASHOK THADANI, ACNW/ACRS Staff

ALSO PRESENT:

MICK APTED, Monitor Scientific

CHARITY BARBER, Greenburg Traurig

JO ANN BIGGS, Hunton & Williams

CHRIS BINZER, Robison/Seidler

RAY CLARK, EPA

RICHARD CODELL, NMSS

ROBERT FRI, Resources for the Future

STEVE FRISHMAN, State of Nevada

CAROL HANLON, DOE/ORD

GEORGE HELLSTROM, DOE

NORM HENDERSON, BSC

CHRISTIN HITIRIS, NMSS/HLWRS

ALSO PRESENT (Continued):

DONALD HOOPER, CNWRA

MATTHEW HUBER, Purdue University

JOHN KESSLER, EPRI

MATT KOZAK, Monitor Scientific

BRUCE MARSH, Johns Hopkins University

ROD McCULLEN, NEI

JACOB PAZ, SEL EMV

GENE PETERS, NMSS/HLWRS

FRED PHILLIPS, New Mexico Institute of Mining and Technology

MAGGIE PLASTER, City of Las Vegas

MYRLE RICE, Lincoln/White Pine Counties

WALTER SCHALK, NOAA ARL/SORD

SOLEDAD SIFUENTES, Cogema Engineering

JUDY TREICHEL, Nevada Nuclear Waste Task Force

ABE VAN LUIK, DOE

MARYLA WASIOLEK, BSC

CONTENTS

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

(8:33 a.m.)

CHAIRMAN RYAN: The meeting will come to order.

This is the second day of the 163rd meeting of the Advisory Committee on Nuclear Waste.

My name is Michael Ryan, Chairman of the ACNW.

The other members of the committee present are Allen Croff, Vice Chair; Ruth Weiner; James Clarke; and William Hinze.

Today the committee will hear from Mr. Robert Fri of the Resources for the Future and Dr. Fred Phillips of the New Mexico Institute of Mining and Technology on the National Academy of Science's 1995 recommendation for the Yucca Mountain Standards and the 2005 court decision vacating a 10,000 year time period of regulatory compliance in 40 CFR Part 197.

Mr. Fri is participating via video conference, and Dr. Phillips is here in person.

The committee will hear a review by Dr. Mark Huber of Purdue University on the evolution of climate in the Yucca Mountain area over the next million years.

The committee will be briefed by Dr. Bruce Marsh, an ACNW consultant from the Johns Hopkins University, on an approach to the modeling of magma-repository interactions.

And we'll hear a briefing from Ms. Leah Spradley, an ACNW summer intern, on the modeling of a volcanic ash plume using the HYSPLIT computer code.

We will hear a briefing from ACNW members who have participated in the August 2005 visit to the Savannah River site and the Barnwell low level waste disposal site.

We'll continue preparation of potential ACNW letters and reports and discuss matters related to to conduct of ACNW activities.

We will also conduct a public outreach meeting this evening later on in the agenda.

Mike Lee is the designated federal official for today's session.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. We have received requests for time to make oral statements from members of the public, including Mr. Danny Kaufman and staff from Congressman Givens' office.

Yesterday we also arranged for Steve

Frishman to make some comments after a couple of this morning's presentations.

Should anyone else wish to address the committee, please make your wishes known to one of the committee staff. There's also a sign-up sheet in the back of the room for those wishing to address the committee.

It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so they can be readily heard.

It is also requested that if you have cell phones or pagers, kindly turn them off while in the meeting room.

Thank you very much.

I'd ask to take special attention to using the microphone as close as you can so everybody can hear you. There's a little problem with acoustics in this room and hearing folks. It is difficult unless you take full advantage of the microphones.

So if we could do that, that would be a big help. So thank you very much.

For this morning's session, I'm going to turn the meeting over to Professor Hinze, a committee member who is going to lead us through this morning's session.

Bill.

MR. HINZE: Thank you very much, Mike.

We have an interesting morning. We are going to be, as Mike has mentioned, we will be having three presentations that will provide us with background as we review the draft revision of 63, of 10 CFR 63, that is reacting to the proposed change in 197 as a result of the court remand of the time of compliance in the Yucca Mountain Standards.

The basis of this is that the 1992 Energy Policy Act stated that the EPA was supposed to prepare their standards for Yucca Mountain in a consistent fashion with the technical basis standards as established by a National Academy of Science panel.

We are fortunate to have two of those panelists with us today to discuss the results of the panel's efforts. We are hoping that they will provide us background on the basis for their decisions on establishing the standards, how they went about doing their work so we have some idea of how they reach their decisions, and we also are interested in the crosscutting issues, such as the dose factors, the infiltration, the climate change, and all of these other issues that impinge upon the time of compliance.

With that, I will ask Dr. Fri, Robert Fri, who is with the Resources for the Future to provide us with his view of the panel's work.

Dr. Fri, I recall that I introduced you to this committee. I believe it was about a decade ago when we held the workshop on time of compliance, and it seems to me that my recollection is that the subject matter was pretty much the same, and so we're anxious to have you reenlighten us and provide whatever information you can to the committee that will help us do the best possible review of 10 CFR 63.

With that, it's yours.

DR. FRI: Thank you very much, and thank you for the opportunity to appear electronically. It does wonders for my schedule.

I remember ten years or so ago when we had that meeting, and I even have still in my files the report of the ACNW on what came out of that meeting, on what you thought about all of this at the time. It was a very good report. So we might just all dig that stuff and save ourselves a lot of time.

Let me spend some time talking about the report and focusing on some of the aspects of it that bear on the standard as it has evolved over the last few years since our report was written.

As was said, I was the chair of the study that performed that report with the oversight of the Board of Radioactive Waste Management here at the National Research Council, and I want to stress that after the committee finished its report, it disbanded. Although the board has come back to this subject from time to time, I certainly have not studied it in detail, and I think Brad, while he was very instrumental in some of the technical considerations that went into the report at the time, his interests often lie elsewhere as well.

So we'll try to do the best we can within the confines of what the committee had to say in its report.

Let me first address a couple of aspects of the form of the standard that the committee recommended in the Yucca Mountain standard report. The Yucca Mountain standard abbreviation to TYMS, and I may use the term "TYMS report" or "TYMS committee" for shorthand as we go through this presentation.

First of all, as to the form of the standard, although the Energy Policy Act stipulated that EPA should develop a standard that prescribed dose equivalence, that was actually in the statute itself. Our report recommended that EPA develop a standard that sets a limit on risk to individuals of adverse health effects from release from the repository. In other words, state the standard in terms of risk rather than dose.

There were a couple of reasons for that. One is a technical reason, and that is since the risk, the dose-response relationship has been known to change over time, the dose that preserves a specific level of risk might change over time, and it seemed to us easier to set this standard in the form of risk.

The other, it occurred to us that it might be more understandable to the public. As you know, EPA has elected to set the standard in terms of dose, and that, of course, was within their prerogative.

The second issue that had to be addressed by the committee is the level of protection afforded by the standard, that is, what level of risk would be appropriate, and our report noted that the level of protection was a policy decision that needed to be established and would be established through the rulemaking process.

We said that science can provide some guidance in this matter, but at the end of the day, the level of protection that the public wants is up to them, and since the level of protection of dose allowed is now handled in a different way by EPA, I think it's important to note that we did not suggest that there was a strong scientific basis one way or the other for a specific level of risk.

We did point out that a number of other sources have set risk levels in certain ranges, and that that was a good starting place for EPA policy, but we didn't try to recommend a specific level of risk because we felt that was a social decision.

Well, with those two background ideas from the report about the level, about the form of the standard, let me then turn to the issue of the time of compliance and the evolution of the standard over the last ten years, its remand by the D.C. Circuit Court of Appeals and so forth.

As you know, the difference between the standard proposed by EPA several years ago and the recommendation of the TYMS Committee were greatest in the area of how to assess whether the repository will comply with the radiation standard that EPA sets, and of course, it's on this issue of time of compliance that the Court of Appeals remanded the proposed standard to EPA last year.

Now, I don't need to go through this for this particular audience in any detail, but let me just remind you that what we're dealing with is a process whereby material is stored in the repository over time. The canisters degrade. Radioactive material leaves the site and spreads in a plume throughout the immediate vicinity. That process can be modeled. Then that gives you some idea of what the source term is going to be for exposure to humans.

Then you have to have some kind of scenario whereby humans come into contact with that radiation that's being in the groundwater, and then you have to decide who is going to be protected, and that sequence of logic is the structure I'm going to talk a little bit about the standards.

So first the question is how long do you model this process in order to decide when you're going to test the standard.

The TYMS report concluded that there is no scientific basis for limiting the compliance assessment period to 10,000 years. That's the principal recommendation and conclusion on time of compliance; that there is no basis for limiting it to 10,000 years.

And of course, this is the issue that the D.C. Circuit sort of remanded the standard on really by saying, look, the one black letter thing the committee said was there's no basis for 10,000 years, and you limited it to 10,000 years, and that doesn't seem like it's consistent with what the committee said.

Having said that, the committee recommended that the compliance assessment be conducted for the time up to which the greatest risk of exposure to radiation from Yucca Mountain occurs within the limits imposed by the long-term stability of the geologic environment.

So that's kind of the second step in the committee's recommendation on how long. The first was 10,000 years has no particular basis. The second, it makes sense to go out to the time of greatest risk within the limits of geologic stability.

And finally, the report concluded that the geological formations at Yucca Mountain were sufficiently stable to permit modeling of physical processes that controlled the movement of radioactive waste from the repository for up to a million years. So that's the third step in the logic.

Fred will talk a little bit more, I think, about the reasoning behind that final step.

Let me just say it's important to

understand that this conclusion does not necessarily suggest that we can predict what's going to happen a million years from now. What it does is to say that the modeling of the physical processes that result in radioactive waste movement out of the repository is not likely in the judgment of the committee, not likely to be distorted by changes in geological conditions during that period.

So in this sense I understand that the committee's conclusions say that modeling physical processes for up to a million years is not really that much more difficult than modeling it for 10,000 years, and the longer time horizon provides more time for the radioactive waste to be released, that is released from the repository, to migrate to distant locations where it is more likely to come into contact with humans.

I go into all of that in some detail because I think it's important to understand what the committee actually said about this "how long" question. There are three parts to it. Ten thousand years doesn't hold up scientifically. It's best to go to the point of maximum risk, limited by the geologic stability of the formations of Yucca Mountain.

Thirdly, the committee felt that for

modeling purposes the stability was adequate to run the models up for a million years.

Okay. The second element then is the exposure scenario. The exposure scenario describes the means by which humans are exposed to the radioactive material from Yucca Mountain chiefly through the extraction of groundwater for growing foodstuffs or for drinking.

The TYMS report concluded that there is no scientific basis for predicting the societal factors that are required to establish exposure scenarios, and so we recommended that such scenarios be established through the rulemaking process, and the practical consequence of this recommendation is to rely on the knowledge of current human activity around the site rather than to speculate on what people might do in the future.

In other words, we said there was no scientific basis for predicting future human behavior. So you'd better use the only good information you have, which is what you know today.

Finally, there's the question of then who is protected. Who is going to get exposed to this material by the scenario that's developed to rulemaking?

And the TYMS report recommended that EPA apply the standards to a critical group representative of those individuals who based on cautious but reasonable assumptions have the highest risk resulting from repository releases.

Now, this turns out to be a somewhat complicated concept, but basically the purpose of it was to avoid the accumulation of overly conservative assumptions. In particular, Yucca Mountain was selected because of its isolation and the expectation that that would reduce the likelihood that some individual would come in contact with the groundwater that is contaminated with radioactive material from the repository.

And the committee felt and concluded that this isolation should be taken into account in compliance assessment and so recommended that the probability of people being present be taken into account when selecting the critical group.

And as I'll suggest in a moment, it's that probabilistic approach that turns out to be very important. Okay. That's what the committee recommended about, in general at least, about the time of compliance issue.

Now, going back to the standard that EPA issued and then was remanded last year by the Court of Appeals, the inconsistency lies in the different treatment of the time horizon for compliance assessment and in different treatment of the definition of who is to be protected.

The court decision didn't talk about the latter point. The fact is a substantial difference between, in my judgment, the way EPA approached this and the way the committee approached it. The TYMS committee elected to carry time horizon out to the point of greatest risk to the public which is almost certainly more than 10,000 years.

EPA limited its time of compliance to 10,000 years, and the question of who's protected, as I indicated earlier, the committee recommended a probabilistic identification of the credible group that would account for the isolation of Yucca Mountain. Now, you know, that basically means that it's not a dead certainty that some individual is going to come into contact with the worst possible case of radioactive material in the groundwater. You have to consider it probabilistically. That was the committee's view.

EPA, on the other hand, proposed to protect what it defined as a reasonably maximally exposed individual. This individual was assumed to live above groundwater that does contain the highest concentration of radioactive contamination from Yucca Mountain, and eats food and drinks water that contains this contamination.

In other words, the reasonably maximally exposed individual is a deterministic concept. There is no doubt that this person will counter the most contaminated water from the repository.

Now, at this point I need a visual. Fred, do you have that?

DR. PHILLIPS: No. Well, I have a copy on my computer, but I wasn't aware I was supposed to show it. It did not get through.

DR. FRI: Okay. Here it comes. I guess we're going to do it the old fashioned way.

This, incidentally, behind it is Kevin Crowley, who is the Director of the Board on Nuclear and Radiation Studies here at the Academy.

If you can see this chart, it illustrates these differences and the approach of the committee and EPA. The vertical axis represents the time horizon. This is the shorter time, compliance time, say, 10,000 years, and this is the longer compliance time that the committee recommended.

The horizontal axis represents the degree to which the person to be protected is selected on a probabilistic or deterministic basis. This is the probabilistic box, and this is the deterministic box.

And as you can see, the committee and the EPA were at diametrically opposed ends of this representation. EPA had a short compliance period and a deterministic scenario. The committee recommended a longer compliance period and a probabilistic scenario.

Now, the appeals court concluded that EPA had not set a standard that was based on and consistent with the findings and recommendations of the National Academy of Sciences because EPA didn't follow the committee's advice on the compliance period, but that's all the court addressed.

But if EPA were to have taken the course of proposing a new standard in response to the court's direction only changing the time horizon without reevaluating the use of the reasonably maximally exposed individual in the standard, there would have been a problem that the committee wanted to avoid.

The problem is that the specification of the time horizon and selection of the person to be protected are intimately connected. So if EPA wanted to extend the time horizon but retain the deterministic selection of the person to be protected, the resulting standard would show up in the upper left-hand corner over here, deterministic exposure and a longer time horizon.

But that is a place that the committee specifically did not want to be, and we know this because one member of the committee did want to combine a long time horizon with the deterministic selection, and he outlined that process and that recommendation in some detail in the report.

So the committee spent a lot of time considering that option and concluded that this would run the risk of excessive conservatism. As I wrote in the report in response to that committee member's proposal, "the standard should avoid an extreme case defined by unreasonable assumptions affecting those in risk."

Some members of the committee believed that the approach advocated by this dissenting member could become such an extreme case. So up in that corner is a place the committee consciously didn't want to go.

So in revising the standard, EPA, after the remake, EPA could have looked at what combination of time horizon and selection of the person to be protected would create a reasonable case that is consistent with the court's opinion and the recommendations of the academy. It could have tried, for example, to show that the protection afforded to the public by its remanded standard is functionally equivalent to the TYMS committee approach and that there were good policy reasons for going ahead with their approach, or it could have accepted the longer time horizon, but selected the individual risk in a less deterministic way, thus avoiding an overly conservative approach.

I don't know which of those might have worked. The committee went out of its way not to try and figure out whether the standard could be complied with. We didn't want to do those calculations, but there were ways of doing it.

But what EPA did do, as I understand this most recent proposal, is to change yet another variable, and that is the level of risk or dose itself. It retained the 10,000 year standard and the reasonably maximally exposed individual as the person at risk and then added a post 10,000 year all pathway standard that applies to the time of peak dose up to a period of a million years.

The numerical value of that added standard is 350 millirem, which is considerably higher than the dose allowed for the 10,000 year standard. That does release the constraint, I suspect, but it's difficult to say whether EPA's proposed standard is consistent with the TYMS report, which only provided, as I said earlier risk ranges for starting points for EPA's analysis.

I'd note, however, that the committee recognized that EPA properly has considerable discretion in applying policy considerations outside the scope of our study to the development of the health standard for Yucca Mountain, and so I think my view of the new proposal has gone as the mission changed as an area in which the committee did not take a stand because we felt it was not basically a scientific question, but rather a societal question of determining what risk is acceptable.

Well, I hope that bring some clarity to what is a complicated situation, and, Mr. Chairman, I'd either be happy to have Fred go ahead and talk about some of these scientific and technical and background of all of this or answer a few questions now. It's up to you.

MR. HINZE: Well, I thank you very much,

Dr. Fri, for an excellent review of the situation and how it impacts upon the decisions that were made by the EPA in their revised 197 standard.

I think that we'll allow questions at this time while all of this is fresh in our mind, and if you don't mind, what we'll do is go around the committee and see what questions there are for you.

Ruth, could I start off with you?

MS. WEINER: Well, I have quite a few, and I don't want to monopolize the time. The TYMS report says that -- and this is a direct quote -- that the related uncertainties in extending well past 10,000 years are "sufficiently boundable." Dr. Fri, what caused you to make that recommendation, to say that these uncertainties were sufficiently boundable?

DR. FRI: At this point, Fred Phillips, who is much better prepared to talk about the technical details than I am since I'm not a scientist, so I'm going to ask Fred to tackle that question.

DR. PHILLIPS: Do you want me to go ahead and address this now?

DR. FRI: Yes, I think so.

DR. PHILLIPS: I mean, basically what we did was to go through and consider the various potential causes of uncertainty and variability in the predictions or simulations. I don't want to use the word "prediction" here really.

I mean, they basically fall into two main categories, and those are climate variability and geological processes. And going through and looking at those, it did not appear that either one of those processes would vary a large amount more over a time period of a million years than they would be likely to or at least that we would seriously have to consider that they would over a period of 10,000 years.

MS. WEINER: But part of what we just heard and what the TYMS report is quite clear about is that part of the uncertainty is the probability of exposure, in other words, the probability that there will be people there, and that whatever they will be doing will result in exposure to releases.

That's the place where I wonder whether sufficiently boundable uncertainties were considered. In what sense would that be uncertainty related to exposure be sufficiently boundable?

DR. PHILLIPS: I think the committee's position was that we did not view that issue or the particular circumstances that are associated with exposure scenarios to be in any sense really predicable, and that what we recommended was essentially a stylized assessment of risk based on our current situation.

We did not recommend trying to come up with strange future scenarios for what people might be doing because we do not feel that that's really a valid area to speculate in.

So our position was that this sort of stylized approach to assessing risk would be equally -- I mean, it's equally applicable or equally inapplicable, depending on the viewpoint you want to take, in 10,000 or a million years.

MS. WEINER: In other words, you don't know.

DR. FRI: Let me add to that. Remember that the assignment of the committee was to look at the technical or the scientific basis for the standard at Yucca Mountain. So the question on the exposure scenario becomes: is there a scientific basis for creating a scenario that's different from the knowledge that we have today about behavior in the vicinity?

And the answer of the committee was, no, there is not such a technical basis, and we recommend using the information that you have today.

We apply the same principle to the

question of human intrusion in which there was a lot of studying going on about what was going to happen some time in the future about people inadvertently or on purpose drilling into one of the canisters and so on and so forth, which according to committee, which incidentally EPA adopted pretty much right down the line, was we can't make that prediction. The thing to do is, again, in Fred's term, to use a stylized approach. Just assume that somebody is going to drill a hole through one of these things and see what happens. And if it's a big problem, back to the drawing board. IF it works out, then that's fine.

And that's what we did. We just didn't see that there was a scientific or technical basis for predicting the future of humanity's activities either in human intrusion or exposure case.

MS. WEINER: It seems to me that what you've done is hand EPA a very, very difficult problem because you're asking -- EPA has to set a standard. that's what the law says they had to do. Did you look forward in your considerations -- and this is really a policy question -- did you look forward in your considerations to what EPA might do under these circumstances? Did you consider alternatives for EPA to take?

DR. FRI: No, we didn't. What we did, however, do was to recognize, I think, the points you were making, and that is that science and this issue can only take you so far. It can enlighten policy, in some cases, as in the case of saying that there's no scientific basis for limiting the standard to 10,000 years. It can foreclose some avenues of policy, but it can't in the end of the day make policy. That's a public policy issue. EPA is in that business. They do it by rulemaking.

And we noted frequently and consistently that there would be policy considerations that would shape the form of the standard over which EPA had control, and admittedly we didn't solve their problem for them. We left them plenty to do, but we felt that that was the appropriate place to draw the line.

MS. WEINER: Well, thank you.

I'm going to save the rest of my questions for Dr. Phillips since he answered the technical ones.

Thank you.

MR. HINZE: Thank you, Ruth.

Allen, questions?

VICE CHAIRMAN CROFF: I'm not entirely sure how to ask this, but the academy's report essentially recommended that the time of compliance be peak, I believe, risk.

MR. HINZE: Could you get a little closer, please?

VICE CHAIRMAN CROFF: The report recommended that the time of compliance be peak risk, I believe.

DR. FRI: That's correct.

VICE CHAIRMAN CROFF: Instead of 10,000 years or any other value which was said to be it's not a quote, but more or less arbitrary. Can you elaborate a little bit more on the scientific and technical basis for saying it should be peak risk or dose?

I can imagine radionuclide release profiles that at least have the potential to maybe make that not such a good choice, where there might be a peak at a shorter time and then a sustained release at a somewhat lower level, but over a much longer time that might warrant looking at other time selections.

Can you elaborate a little on how you got to specific determination?

DR. FRI: Well, let me start and then ask Fred to finish it off. What we said was essentially that the objective ought to be to find the time of peak risk to the exposed individual. So that means you've got a lot of moving parts in that calculation. The plume is moving over time, and its distribution of radionuclides changes over time. Of course, there's an exclusion area ignore, and you've got the probability that people are going to be on any specific place at any specific time.

And so what we did was to ask ourselves the question: is it plausible to say that the risk for those can be calculated given the situation with all of those moving parts?

And Fred and other members of the committee, and you'll find their piece in one of the appendices to the report, did work out an approach, a computational approach to dealing with that problem. It may not be the best one, but we were really at this point not interested in necessarily coming up with the most efficient solution to this problem, but rather simply an existence proof that there was a solution to it.

We convinced ourselves that technically you could do it, and os that's what we recommended.

Fred, do you want to add something to that? I'm sure you can.

DR. PHILLIPS: I mean, I don't have a whole lot to add. I would just say that what we recommended was a risk based standard and, therefore, the appropriate time to evaluate that seemed to be at the period of maximum risk, whenever that fell.

I mean, it sounds to me that perhaps you are thinking in terms of some risk integrated over time or something like that. I mean, that's certainly an option one could consider. That wasn't what we ended up recommending.

VICE CHAIRMAN CROFF: I wasn't necessarily suggesting that. I mean, it comes to mind, but I was more trying to get at, you know, what you're thinking was in saying peak dose as opposed to maybe looking at the dose profile around the peak or maybe even looking at least to some extent at even longer times where there might be somewhat lower doses or shorter times, where the doses might be somewhat lower, but much more sustained, and maybe saying, well if there's a high dose for 1,000 years and a somewhat lesser dose for 100,000 years, maybe it's more reasonable to focus on the somewhat lower 100,000 year problem.

Was there any discussion of these kinds of tradeoffs leading to your selection of the peak?

DR. PHILLIPS: Well, I mean, there was certainly discussion of it, which at this point I can't recall in detail, and after a lot of discussion, that was what we spent much of our early meetings on. We settled on a risk based standard and, therefore, I think if you accept that premise, then evaluating that at the point of peak risk is really the only acceptable time frame.

DR. FRI: I think it's safe to say that in our consideration, we recognize that it was a complicated thing. Yes, there were some higher dose rates early on. They attenuated, of course, over time. At the same time the geology might result in, you know, pooling of the waste material in certain spots which created a more likely exposure to a relatively high dose, and it was that whole complex set of movements that we felt needed to be captured by going out toward the time of peak risk.

MR. HINZE: Thank you, Allen.

Dr. Ryan.

CHAIRMAN RYAN: thanks, Bill.

Just a follow-up comment to Allen's question, and maybe you could respond to it. I think I see a slightly different picture that's in tune with your idea of a peak risk, and that is that if you recognize an individual where you've focused a scenario development recommendation, you know, it's where you're actually calculating dose or risk. That risk is pretty finite in time because you've got the individual's lifetime as the cap for the risk for that individual.

And then kind of moving that individual scenario across a longer time line seems to me to be what you've recommended.

DR. PHILLIPS: Yeah, I think that I'm essentially in agreement with you on that. The only thing that I would add is that we really didn't pose it in terms of an individual but rather in terms of a critical group.

CHAIRMAN RYAN: A critical group.

DR. PHILLIPS: But it would nevertheless be over the extent of a human lifetime.

CHAIRMAN RYAN: No, I understand it's the average memory of the critical group, and it's a little bit more formal construct there, but you know, again, you're talking about kind of individuals and sort of realistic characteristics of how an individual risk or dose would be calculated and then that evaluated over some longer time line is really where you made the recommendation.

DR. FRI: Yes, that's right.

CHAIRMAN RYAN: All right. Thanks.

MR. HINZE: Thank you very much.

Dr. Clarke.

DR. CLARKE: Excellent summary. No questions at this time. Thank you.

DR. FRI: Thank you.

MR. HINZE: Dr. Fri, Bob, I'd like to ask you a couple of questions. Many countries have a tiered approach, and as you will recall, the ACNW at one time suggested a tiered approach to the standards and the regulations.

In view of the uncertainties that your panel has recognized, did you consider a tiered approach with a variation in the standard as the uncertainties increase or move from a quantitative to a qualitative?

If you did consider this, on what basis did you reject it?

DR. FRI: Well, I think that we may have talked about it, but certainly the tiered approach was not in my memory prominent in the final discussions of what our recommendations would be. I think what we felt was that the modeling that we outlined pretty much along the lines that we have discussed was feasible, and then you would go ahead and calculate this time of peak risk and make your assessment at that point.

Now, if you argued that -- let me back off -- and the time frame of stability, the time frame over which you could do the calculations was sufficiently long that you'd pick up the time of peak risk; if you argued that the uncertainties are such that that's not going to happen, then I think you might be interested in looking at some other approach.

But we didn't think that was going to happen. So we didn't look at or we didn't recommend the alternative of a tiered approach.

Fred, do you want to add anything to that?

DR. PHILLIPS: Yeah. I mean, I believe that we spelled out at one point in the report here several issues that we had explicitly not dealt with, and one of those was trying to put any kind of societal weight, I guess you might say, on future consequences, and this may be similar.

I guess there are two levels of issues here that you could talk about. One is uncertainty in behavior of a system as time increases, and increasing uncertainty in that, and that's essentially a technical issue.

The other one is given that increasing uncertainty in both the technical issues and in the human issues that are involved, one could choose to weight less the consequences of future actions, and this is, in fact, what EPA has at this point fairly explicitly done by upping the level of the standard after 10,000 years.

So we said, you know, certainly that this option of saying that we want to give less weight to consequences after some long time period is one that should be considered, but that it's not within our purview.

MR. HINZE: Thank you.

Let me ask another question of you, Bob, if I may. Peak dose. Did your panel consider that there might be multiple peaks in the dose in the post 10,000 year period and that the uncertainties would make it untenable to predict which of those is really going to be the maximum peak dose and so rather than having the time of compliance be the peak dose, have a specified period of time like a million years?

In other words, why did you move to -- did you give thought to going to a specified period like a million years or 500,000 years or did you envision that the peak dose could be really predicted that well and thus specified?

DR. FRI: It was really the latter, I think. We looked at the -- the question is can you computationally deal with all of these moving parts, as I said earlier.

And we satisfied ourselves that that was possible. So we said that's the way we think would be the best technical way to go about it rather than set, you know, a specific time in the future at which the peak dose would occur.

And besides, you know, the dose if you mean -- well, if you mean dose by what's in the ground versus risk by which you mean the exposure scenario probabilistically applied, you've got even more moving parts, but we felt they could all be modeled.

MR. HINZE: Thank you.

Human intrusion was something that the TYMS panel had remarks about in terms of developing a specific scenario for it and dealing with it. Can you give us any insight into your thinking on that and where you ended up and so forth? Can you reach back?

DR. FRI: A little ways. Again, Fred should chip in after I make a few introductory comments.

We looked at human intrusion, and I remember that, in fact, Bob Budners (phonetic) did a terrific analysis of all of the kind of possible scenarios dealing with human intrusion and basically showed that trying to predict the future in any of these cases provided no useful information, and we concluded that we really couldn't predict what was going to happen.

On the other hand, the possibility of human intrusion is real. So rather than start creating scenarios about what might or might not happen over the next, you know, thousands of years, if not longer, as well as scenarios of the effectiveness of countermeasures that you take to avoid human intrusion, why don't you just pick, you know, one stylized scenario, which in our recommendation was essentially assumed that somebody for whatever reason comes along, drills a hole into the repository through one of the waste canisters, evaluate what happens.

And if that works out okay, fine. If not, you know you'll have to do something else, and that's essentially, I believe, the approach that EPA adopted.

MR. HINZE: Thank you.

With that I'll open the questioning to the staff. Latif.

DR. HAMDAN: I have one question concerning the groundwater standard, which is the standard in the EPA aggression (phonetic), and the question is: did the committee look at the groundwater standard beyond 10,000 years? And if they did not, why not?

DR. FRI: We did not look at the groundwater standard and on purpose. The conclusion of the committee was that we felt that a health standard, defensible standard that would protect public health could be set on the basis of individual risk or dose, preferably risk, and our assignment was simply to determine whether that was possible or not and give the basis for it, and we did.

The groundwater standard, you know, may or may not be redundant in that regard, but we felt it wasn't our job to look at it. We said that if possible, to protect the public health with a standard that protects individuals at the time of peak risk or peak dose and that was sufficient to protect the public health.

MR. HINZE: Mike.

MR. LEE: Yes, thank you.

I have two questions. The first one, Dr. Fri, going back to your earlier --

DR. FRI: Closer.

MR. LEE: -- opening remarks concerning geologic stability or the predictability of climate geology over 10,000 years versus a million years, in projecting future geologic events, could you elaborate on what the committee's views might have been in terms of the ability to make those predictions and what appropriate measures for making those predictions would be?

The existing standards rely on a 10,000 year time frame and prediction of events over that time can be applied. I think it's being proposed now to a million years. Do you have any views on that or could you elaborate on that?

DR. FRI: Well, let me start, but I think Fred is probably in a better position to answer that question. I think all I want to say is what the committee said was that the geologic considerations suggested there was enough stability there that one could conduct a modeling over an especially long period of time to find out what the peace risk to a probabilistically determined individual was.

It didn't say you were making predictions about what would happen geologically. We just said is it stable enough in order to undertake the probabilistic risk assessment that has to go forward over this time.

And our answer was yes, but that's all we said. That was sufficient under our purpose. We weren't trying to predict the future in any kind of detail.

Fred, you should comment on that.

DR. PHILLIPS: Sure. Could you be a little bit more specific about your question though?

MR. LEE: Currently, EPA is now proposing that the new --

MR. HINZE: Could you get closer to the mic, please?

MR. LEE: Oh, I'm sorry.

In the proposed revision to the EPA standard, EPA is now proposing that the projections of recurrence of certain features, events and processes over 10,000 years can be used in a million year analysis, and my question is: had the committee given any consideration to how those projections might be conducted or appropriate ways of doing those projections?

DR. PHILLIPS: Okay, and again, specifically what processes are you thinking of here?

MR. LEE: Geologic processes.

DR. PHILLIPS: I mean, if we're talking about things, I mean, basically the geologic processes that are relevant that we considered are things such as rates of tectonic displacement, rates of surficial erosion, rates of base and infilling. All of those are going to affect topography, and topography is the driving force for groundwater flow. They could potentially also affect the geologic framework through which the water flows, and those rates are reasonably well quantified at present, and there is no evidence to indicate that there is likely to be major changes in them in the future. In the million year time frame I should say.

And so if one can use those present data to predict changes in the configuration of the landscape or the hydrogeologic framework over the period of 10,000 years, there's no reason to think that they would not be also applicable with a somewhat larger bound of uncertainty at a million years.

DR. FRI: Is that it then?

MR. LEE: That's helpful.

My second question: has there any thought been given to commenting on the current standard? I know that the committee previously commented on the draft.

Has there been any discussion at the academy on that?

DR. FRI: The committee hasn't commented because there isn't a committee. I have no idea whether the Board on Nuclear Radiation Studies has any intention of saying anything or not.

Kevin Crowley is shaking his head no.

MR. LEE: Thank you.

DR. FRI: Consider that an authoritative response.

MR. HINZE: Thanks to Kevin.

Other questions? John Flack.

MR. FLACK: Yes. Just one question. On the consideration of the mean versus the median, on the implementation of the standard, whether or not it's a dose or the risk, was there any consideration of that and whether one should be preferable in dealing with the uncertainties over the other?

DR. FRI: That cropped up in the EPA stuff. I don't know whether we considered it or not.

Fred, do you?

DR. PHILLIPS: I'm afraid at this point I don't remember whether we discussed that.

MR. HINZE: Are there any questions from the audience or any comments?

Steve. Steve, introduce yourself and go to a microphone, please.

MR. FRISHMAN: I'm Steve Frishman with the State of Nevada.

I'm not here to discuss the merits of the report or of the EPA standard. We'll have plenty of time to talk about that in other venues. I do want to just make a fairly simple statement that someone last week much more notable than I said -- and remember it's established law --

MR. HINZE: Could you speak up just a bit, Steve, please? thanks.

MR. FRISHMAN: Okay. Remember it is established law, and I'm not sure whether any of you have actually read the court opinion on this or not. In fact, what I did was I copied out of that opinion the section on the 10,000 years to put in your records so that you can actually see what the court said about it over a space of about ten or 12 pages.

But the important point that got us in the situation that we're in right now is, first, the court said the 10,000 year compliance period selected by EPA violates Section 801 of the Energy Policy Act because it is not as EPA required or as the Energy Policy Act requires based upon and consistent with the findings and recommendations of the National Academy of Sciences.

That is the finding. The other thing that I think is probably of more importance to you at this point, and I have some interest in why this subject is even before you today, but the point that I think should be of interest to you is that the second finding of the court is that the Nuclear Regulatory Commission's licensing requirements are not unlawful, nor arbitrary and capricious, except to the extent that they incorporate EPA's 10,000 compliance period. That's it.

And now if you're looking for some further remedy, what the court said was it was the Congress that required EPA to rely on NAS' expert and scientific judgment, and given the serious risks that nuclear waste disposal poses to the health and welfare of the American people, it is up to Congress, not the EPA and not this court to authorize departures from the prevailing statutory scheme.

That's the situation you're in. I think the proposals that are out there take some liberties with that, but I think it's necessary to remember that we can all discuss and rediscuss the points that have been talked about this morning. We all have opinions on them, and they may not be the same now as they were in 1995. I know some of mine have changed in some experience with thinking about how you create and implement a rule.

But the position that we have right now that I think is possibly of greatest concern to you is advising the Commission on whether the rule that they proposed for Part 63 fits within the scheme of what the court found and what is realistic for a licensing process.

To go back and revisit what EPA was thinking, what Bob and Fred were thinking, and I remind you that Tot Pickford was thinking some things quite differently from what you've heard today, I'm not sure that that's anything more than sort of spinning of wheels.

If you really want to look at what your responsibility is to advise the Commission, then you should look pretty hard at what has been proposed for Part 63 and see whether it fits within the realm of a very, very simple court decision, even though it consumed 100 pages because there were lots and lots of other issues.

But I'll leave for you to look at the ten or 12 pages on the 10,000 year issue, and I urge you to look at it in its simplicity and straightforward approach to finding an answer on whether something is lawful or not.

So I encourage you to not take your

investigation out to where we were before 1995 because that is definitely behind us, and all we're doing now is trying to repair what EPA did the first time, and my guess is that we're going to be in a situation in a couple of years where we're going to be trying to repair what EPA did the second time.

MR. HINZE: Thank you, Steve.

I'm sorry, Judy. Would you go to the microphone? I couldn't hear you.

MS. TREICHEL: Can we get a copy of the graphic?

MR. HINZE: Bob, how do we get copies of the graph of the --

DR. FRI: I think Fred has got it on his computer, don't you?

DR. PHILLIPS: I do have one on my computer, and with a little bit of manipulation, we could get it transferred over.

MR. HINZE: Okay. If we could ask the staff to get that from Fred and make copies and make them available to the committee, the staff and the public, we'd very much appreciate it. Okay?

There's another hint here, if you would, sir. Did you have something to add?

DR. PAZ: Just like the other morning, I

was supposed to be on my way to Texas, but --

MR. HINZE: Would you introduce yourself, please?

DR. PAZ: Yes, my name is Dr. Jacob Paz.

And one comment which I have to say is the performance of the repository. There are too much emphasis on modeling, very little on large scale study and how the performance of the repository will be in the next 10,000 years or more, specifically there is no studies what is the competition between the heavy metals and (unintelligible) the absorption rate in the KE, and to make an assumption, it can lead very serious uncertainties.

For 10,000 years, I think this is -- the code say what it has to say, and either the code or the Congress has to address it, but I think the EPA went out of the boundary.

Other important questions is to look in the multi-level, and what is the heavy metal going to be deposited there? What is the risk to population? Ignored.

In the long term, is the issue of the actinide (phonetic) will be converted to lead. When it start to grow and grow, this also pose a serious problem.

Thank you.

MR. HINZE: Thank you very much. Certainly your remarks will be considered.

Any other questions?

If not, I would like to move directly to you, Fred, and Fred is a Professor of Geosciences in hydrology at New Mexico Institute of Mining and Technology and is a member of the committee and has already answered several of these questions.

But what I would like to do now is to move to more specific questions that might be designed towards the science that was used in reaching the decision regarding particularly the time of compliance.

And I wonder, Fred, on the basis of the comments that Bob made and the questions that have arisen here, do you have any comments that you'd like to make to start this off?

DR. PHILLIPS: No, I don't really, I think, have a lot to add. I actually sort of made the comments that I was going to make at the beginning of my presentation in response to one of the earliest questions. I was just going to say or I did describe how we went through sort of climatic and geologic factors that would cause the parameters within which some sort of probabilistic analysis would be conducted to become outside of the bounds that would be used, and our conclusion after doing that was that something on the order of a million years was a reasonable time frame for the extent, to the point at which one might speculate that changes would become so large that the whole scenario would significantly be altered.

And I will add that the million years was not intended as the result of a rigorous analysis. That was a suggestion of the general time frame that we thought was applicable.

MR. HINZE: Okay. With that, I would like to ask the committee and would like to go around the committee and make certain that we have all of our questions covered.

Ruth, can I start with you again?

MS. WEINER: I saved some questions for Dr. Phillips.

The committee suggested setting a standard in terms of risk rather than dose. What did you consider as far as uncertainties in the risk factor? What did you consider the risk factors to be and how did you incorporate uncertainties in the risk factor in your thinking?

I mean, what people frequently do is risk is risk. Risk for low doses is risk of cancer, and you take the dose, whatever it may be, multiplied by some conversion factor, assuming linearity, and come up with a risk. Is that what the panel did?

DR. PHILLIPS: Well, I mean, again, of course, it's important to recognize we weren't actually performing any risk analysis. We were merely thinking about the general procedures that might be used, and our recommendation was for a thoroughgoing risk or probability based analysis in which one would employ transport models that would be essentially Monte Carlo models that would consider variations in all of the natural parameters, that would govern transport, and that would include the geochemical aspects of it that would cause transport of radionuclides to be at different rates than water itself; and that then that would produce a probability distribution of concentration at any particular point within the system, within the area, right? And that probability distribution would be multiplied by the probability of a person being on the spot to consume the water and then the probability of the particular habits that would also influence the dose that they would receive.

MS. WEINER: I see. So you looked at

uncertainties in the dose calculations themselves.

DR. PHILLIPS: Yes, that's right. I mean, certainly much more than simply the dose to risk conversion factor.

MS. WEINER: Thank you.

DR. FRI: If I may, if I understand the question one of the issues is that the dose response relationship, our understanding of it changes over time, and that's one of the reasons we suggested a risk based standard. Because if societally you were either one in a million chances of mortality as a result of this is an acceptable societal standard, then the dose response relationship that gives rise to that risk can change without having to change the standard.

So we did recognize there were some uncertainties in that relationship, and to avoid complicating the standard, we said you've got to go with a risk based standard.

MS. WEINER: Thank you.

That is very helpful and very clarifying. The other technical question I have is since the maximum activity occurs very early on in the life of the repository, when you said look at the time of peak dose or to get back to Dr. Hinze's question, possibly several times of peak dose, were you considering disintegration of the waste package, mobility of the dominant actinides like Neptunium 237? Did all of that figure into your estimate that the peak dose would be somewhere out past 10,000 years?

Because if you look at the activity, it becomes flat, fairly flat. The total activity becomes fairly flat, and the dominant contributors are some of the actinides that have grown in.

Was that part of your consideration in saying that the peak risk occurs past 10,000 years?

DR. PHILLIPS: Yeah, I mean, our assessment in that regard was based on reports published by Sandia and Lawrence Livermore mainly, as I recollect, which -- and I'll say in addition that, of course, we were only considering transport outside of the exclusion zone. We were not concerned with things that were happening inside of it.

And those showed that several of the actinides would reach their peak levels in a time frame that was a great deal longer than 10,000 years.

MS. WEINER: So, yes.

DR. PHILLIPS: I mean, that was basically a result of a total system performance analysis.

MS. WEINER: Right. So you looked at the

performance assessment and said that because where the mobility of certain actinides becomes very pronounced.

DR. PHILLIPS: Correct.

MS. WEINER: And did you then look at the exposure as being through any particular pathway, ingestion, inhalation, or just general? How did you look at exposure of the critical group?

DR. PHILLIPS: I mean, again, we did not -- our viewpoint was that all significant pathways for exposure should be considered, but based on previous assessments, it appears that the one by ingestion through water would be the predominant one.

MS. WEINER: A final question. You outlined or Dr. Fri outlined the human intrusion recommendation. Isn't your human intrusion scenario deterministic rather than probabilistic?

DR. PHILLIPS: In a sense, I suppose so. We considered the option of doing a probabilistic scenario analysis on that, and we rejected that for the reasons that Bob gave.

And fundamentally, to boil it down to its simplest terms, the geologic environment and the performance of the engineering systems that are around the waste are things that are fundamentally analyzable on a scientific basis and which can be incorporated into a probabilistic analysis in some meaningful way.

Human society and human behavior, long periods into the future we did not feel fell into that category, and that is why we recommended a different approach for those.

MS. WEINER: And finally, this is a question that is difficult to phrase. Did you consider the impact that your recommendation, particularly the fact that you said that the 10,000 years has no scientific basis; did you look at the impact of what that might have on policy and regulation?

What kind of considerations did you give to that? That's really a question for Dr. Fri, I guess.

DR. PHILLIPS: I think that that's correct.

DR. FRI: Well, we didn't try to, as I recall the report, we didn't try to tease out what the substantive policy consequences would be. The report does, as I recall, say that we know that we're handing EPA a very complicated administrative and rule making chore, but that's about as far as we went.

There was also early on in the report a longish list of half a dozen or more things that we elected not to consider, and I don't have the report right in front of me. So I'm not going to try and read them all to you, but there were -- we considered a number of things pretty much off limits for our committee. We had enough trouble figuring out what the technical basis for the standard would be and recognizing that there are a lot of other issues that have to be dealt with.

MR. HINZE: Allen? Dr. Ryan? James?

DR. CLARKE: I just want to follow up on Ruth's first question of risk versus dose. I think, Dr. Fri, you said earlier that the committee recommended a risk based standard, but did not recommend a target risk level to that, we thought, should be decided by the public.

So I don't know if that was where you were going, Ruth, or not, but the other part of my understanding is that your knowledge that peak dose for certain radionuclides or peak travel time for certain radionuclides, peak dose would occur after 10,000 was based on modeling that was in progress and modeling studies that were being done by other.

So you really were not doing those kinds of calculations; is that correct?

DR. PHILLIPS: That's correct.

DR. FRI: The number is intentional, too. I mean, not only were we not necessarily equipped to do it, but we did not want to be in the position of knowing what the answer might be if you did a compliance assessment. We didn't want to be in the position of appearing to back-engineer anything. So we just took what data were already available from studies that were being done or had been completed at the time.

DR. CLARKE: Thank you.

MR. HINZE: Fred, I would like to ask a couple of questions, if I might, regarding your decisions regarding the time of compliance and stability.

Now, I was at a meeting recently where -- and this gets at the point of how you reach your decision -- I was at a meeting recently where a knowledgeable person was discussing the probabilistic volcanic hazard at the site, and the remark was made that 10,000 years was something that could be reasonably predicated -- I'm paraphrasing -- but that up to a million years seemed extremely improbable to that person.

And I guess what I'm getting at is I'm wondering what kind of -- we all have our areas of expertise, and I'm wondering what kind of information was brought in from the public and from the workers in the area towards understanding the long-term techtronic stability of the area.

DR. PHILLIPS: Well, I mean, we surveyed the -- a great deal of research, of course, has been done on Yucca Mountain and the vicinity because of the waste repository, proposed waste repository, and so we basically relied on the findings of that research for rates of geologic processes in climate change. I mean, I find it a little hard to --

MR. HINZE: Did you have presentations by the DOE staff on these topics?

DR. PHILLIPS: Yes, we had presentations from DOE staff and other research, you know, people that were also performing research on the area and from people who were funded by the State of Nevada to do research and so on. So we had a wide range of input on that.

MR. HINZE: And that has led you to the stability and the predictability.

CHAIRMAN RYAN: Bill, you're going to have to get into the microphone a little bit.

MR. HINZE: Okay. It's sliding. Thank you very much, mic.

Let me ask you as a geoscientist and as a member of the panel, as you were thinking about these, did you give any thought to the difference in characterization of the site for 10,000 versus some longer period of time, or did you give any thought to what kinds of information that one might need to consider the site for a long period of time rather than for 10,000 years?

DR. PHILLIPS: It would be helpful to me, I think, if you could give me some specifics there because --

MR. HINZE: Well, let me be very specific. Is there, as you've thought about this, is there any site characterization that you would deem advisable that would be useful for considering the time of compliance of a million years versus that of 10,000 years? Are there additional geological tectonic, igneous, seismic studies that would be germane for a one million rather than a 10,000 year time of compliance?

DR. PHILLIPS: That's an interesting question. You know, I think that the geological investigations that were associated with the site were not conducted by people who were thinking in terms of a 10,000 year time frame. They were not conducted by engineers who had a 10,000 year cutoff. Okay?

They were conducted by geologists who if they were studying the volcanic rocks they were interested in what happened in the Miocene, and that's a lot longer ago than 10,000 years, and so on.

So I really do think that the base of investigations is certainly there. One might want to try and interpret that data somewhat differently. So I reread or not reread, but I read some of the more recent documents that have come out on the performance assessment, and the basis for that in preparation for this meeting, and of course, all of them sort of cut off the evaluation. Well, here's what we can expect to happen over 10,000 years, and people are going to have to go back and redo those, looking at it in a longer time frame.

But, for example, a lot of the basis for the climate projections that were in those is on the Devil's Hole oxygen isotope curves, and those are a far longer time period than 10,000 years.

So the database is there, and I just think it needs to be used for a different time frame.

MR. HINZE: Any further questions? Latif.

DR. HAMDAN: Yes. Fred, one can understand that you want to evaluate the risk at the time of the big dose. That I can understand some of the -- one thing to do, technical stability and sciences for a million years or more, but what I was struck by this morning, you saying that when you came to the conclusion that one million years is reasonable.

And, frankly, I don't think it is. I don't think it's reasonable at all. We cannot predict for that many years. We don't have manmade structures that are millions years old. We can't make them. We cannot manage them. The economics are 4,000 years old. So what is the basis for coming to the conclusion that a million years is reasonable?

DR. PHILLIPS: The materials that are the basis for the prediction of the physical part of the system at any rate -- I won't necessarily say the engineering part -- but for the physical part or the system, those are materials many of which have been out there and in that environment for periods far longer than a million years. Most of the rocks that the water is going to be flowing through have been there for many multiples of millions of years.

And their behavior over those types of time periods is well understood. There's well over 100 years of geological and geochemical research into understanding how they behave over those sorts of time periods.

Similarly to tectonics, in the time frame of tectonics, a million years is a very short period. Only in areas of extremely high tectonic activity do you get significant variations. In general, a million years is too short a time to be very interesting to look at.

So why one would say that fundamentally what would happen in the environment over a million year time period in terms, again, not of a specific, exact prediction, but in terms of assessment of probabilities over that time period, I don't understand why one would say that it's not predictable.

With regard to the engineered systems, that' more problematical, but in fact, most of the changes and the degradation in the engineered systems that would be associated with the repository will be within the initial 10,000 year period. Those residual things that are going to happen after 10,000 years are going to be simply a continuation of that of the earlier period.

So if one can't say anything meaningful about what's going to happen over a million years, I don't see how one could say something similar about what's going to happen over 10,000 years with regard to those systems.

DR. FRI: Let me just stress something that Fred said because it's really important. The question the committee was addressing at that point is is the geology stable enough to do a reasonable compliance assessment out to the point of peak risk which may be as long as a million years.

The question was not can you predict what's going to happen in a million years or, for that matter, in 10,000 years. We're just trying to run a probabilistic risk compliance assessment, and the conclusion as Fred has pointed out clearly was that the geologic factors are sufficiently stable and known that you can run the model over a long enough period of time to find out where the plume is at the period of peak risk.

DR. PHILLIPS: Another significant factor here is that the area that we're talking about is one of quite considerable geological stability, and were it in a more tectonically active or even a climatically more erosive type of environment, you know, a million years might not be feasible.

But I feel fairly confident in saying that one could go back out to Yucca Mountain a million years from now and everything would still be very recognizable. It would have changed somewhat, but you would not no problem, you know, locating where you were with respect to Yucca Mountain.

I looked at some of the recent literature, you know, to sort of check the kind of numbers we used back ten years ago and really things have not changed very much, but basically according to the data that are currently available and are currently used in the system performance models -- and I extrapolated out the rates in there that are used over a million year time period -- one would expect the summit of the mountain to be somewhere between one and ten meters lower in elevation than it is presently due to erosion.

One would expect somewhere between ten and 50 meters more sediment to be deposited in the crater flat basin and the other basins that surround Yucca Mountain.

One would expect that faults would have displaced things somewhere between 50 and 100 meters over a large area. That's not a single fault. Displacements over a single fault would be on the order of one to 25 meters, something like that.

So, you know, there would be changes, but nothing drastic.

DR. HAMDAN: I really don't want to belabor the point, but I want to make the point that --

MR. HINZE: Latif, we can't hear you. Speak in, please.

DR. HAMDAN: I don't belabor the point. I just want to say I like the science and I like the arguments, and I like the exercise, but I feel that the context may be missing in this whole argument, meaning that what started all of this is if the framework for the time of compliance in a rule by the EPA, and that's the point that I've been trying to make.

MR. HINZE: Are there any further questions from the staff or from the public? Judy.

MR. TREICHEL: Just one sentence out of the bible that we've been discussing. On page 123 there is a sentence. Well, the bold says "use of mean value." The sentence says, "We recommend that the mean values of calculations be the basis for comparison with our recommended standards."

MR. HINZE: Thank you. I think that you previously remarked that you have no recollection of the discussion of the median versus the average and so forth, right?

PARTICIPANT: That's right.

MR. HINZE: Ruth.

MS. WEINER: If there's time I'd like to ask a follow-up question. Could I ask a follow-up question?

MR. HINZE: Please, please.

MS. WEINER: This is a follow-up to Dr. Hamdan's question. Is it correct then to say from your considerations of the geology of the site that this recommendation refers to this particular site or the particular geologic region in which this site is located and were there a different site, this recommendation could be different?

DR. PHILLIPS: Absolutely.

MS. WEINER: Is that appropriate?

Thank you.

MR. HINZE: Fred or Bob, do you have any final comments that you'd like to make to help the committee?

DR. FRI: Nothing that occurs to me, but of course, if you have other questions, we'd be happy to try to remember the answers to them.

MR. HINZE: Okay. Well, thank you very

much, and on behalf of the committee, I want to thank Dr. Crowley of the academy for making your appearances possible, and to both of you for your contributions. They've been very helpful, and we'll be very anxious to look at the transcripts and look at them and your remarks in detail.

And, Fred, we want you to stay around if you can for the rest of the meeting.

With that we'll take a 20 minute break until let's say 10:25, and we'll pick up with the next presentation on this topic.

Thank you.

(Whereupon, the foregoing matter went off the record at 10:05 a.m. and went back on the record at 10:33 a.m.)

MR. HINZE: Thank you very much.

We will proceed with Matt Huber's talk on the evolution of climate in the Yucca Mountain region over the next million years. Paper copies of his presentation, as well as the two subsequent presentations will be available for the public and the committee this afternoon. So paper copies are coming.

With that I would like to introduce Matt Huber, my colleague at Purdue University. Matt has been a research professor at Niels Bohr Institute in

Copenhagen and is currently a professor at Purdue University and focuses his effort on climate modeling.

He has many distinctions and awards, and I would mention specifically that he cooperated with EPRI in their recent report on long-term compliance, and working with them on the climate modifications that can be anticipated over the next million years or so.

He is also the co-chair of the Paleoclimate Working Group of NCAR, the National Center for Atmospheric Research, which speaks to his many accomplishments.

With that, Matt, it's yours.

DR. HUBER: Thanks, Bill.

Can people hear me now? Good.

So excuse me while I have to juggle a pointer, a microphone, and advancing this. So I'll try and not stumble around too much.

I'm a global climate modeler. The climate models that I used are based in the equations of physics. You start off with F equals ma, and you work from there. People have been using these models now originally for 40 years, and the current generation of models is really quite good and I'll hopefully help you see that today.

All right. So one of the interesting things about this problem from my perspective is the question of, well, weather, as you know, is very difficult to predict. Climate is difficult to predict. We live on this very variable world with nasty things like clouds and storms and hurricanes and ocean currents and vegetation and pesky things called people that live near the surface.

So one could ask the question how could you predict climate a million years from now when you can't even predict the weather next week, and that's an important question. It's one that I deal with on a regular basis because I've devoted my whole career to predicting what the climate was like 50 million years ago, 40 million years ago, 30 million years ago, and also into the future.

And hopefully I can convince you that we can tackle that problem in a pretty quantitative and realistic way.

So, again, with this issue of variability, this is satellite imagery of water vapor in the atmosphere. You can see this is a turbulence problem. There's mixing and stirring of water vapor which ends up raining out as precipitation in weather systems, and the ones that are of particular relevance to Yucca

Mountain is this bad boy right here, which often times gets set up and you pull in moisture from the eastern Pacific and occasionally will suck it up into this area.

Sometimes you get moisture that comes in and comes down around here. So if you want to understand, for example, infiltration in the hydrological situation in the Yucca Mountain region, you have to somehow include information about how weather is going to change in the future, and there's different approaches to doing that.

Now, this is a satellite map of the cryosphere and also the biosphere as a function of time over several years, and what you see is the beading of the seasonal cycle in the Southern Hemisphere, ice and snow growing and receding, and you see this repeated in the Southern Hemisphere.

Now, you see this over the course of a seasonal cycle, but you also see something that looks very similar, except that it deals with mean annual conditions over the course of glacial/interglacial cycles.

So this is the sort of thing we have to come to grips with if we're going to say anything about climate over the next million years. And there's, as I said, different approaches to dealing with that.

Now, you can take a very modern day mechanical approach as a starting point, which is simply to say let's go and look at precipitation and observe records here in this region in the past, say, 50 years and relate that to large scale patterns. The reason why you want to relate it to the large scale is ideally you can simplify the problem down to understanding the conditions in just a couple areas and then ask how might the conditions in those areas evolve as a function of time.

And what work is in this area has consistently come up with is that you can understand precipitation variability in this region by understanding really just three different precipitation modes, ones related to El Nino or the El Nino southern oscillation, which are called ENSO. The Pacific Decadel oscillation, the PDO, and what has historically been called the Atlantic Meridional oscillation, but which is probably reflective of a larger mode that's global in extent, and I'll show you what I mean by these in a second.

So there's a lot of published work that's been done on this, and what the Atlantic Meridional model looks like is if you look at these red colors, these are essentially sea surface temperatures. So what this mode looks like is a warming up of the Northern Hemisphere oceans and a cooling here along the Pacific, and associated with that mode are major changes in precipitation, including changes in drought frequency over the whole United States and especially in the Southwest.

There's the one that everybody is familiar with, El Nino. This is what a typical El Nino looks like. It's a large bolus of warm water in the eastern equatorial Pacific with an extension up here, and associated with that will be wetter conditions in the Southwest, and these are all things that are verifiable in the modern day, and we kind of understand them.

There's a Pacific meridional mode. Again, I personally think that there's only one mode. meridional just means north-south. It's a "jargony" term, and that mode is related to a shifting of warm water north of the equator, and you get this big thing of warm water off the coast of western North America, and associated with that is a big band of increased precipitation actually across the whole U.S., but with a focus right here in the Southwest.

There's another mode, the Pacific Decadal Oscillation, which again shows up as a big thing of warm water. It looks a lot like a long El Nino, but it's not actually a long El Nino, and it's correlated with big changes in precipitation here in the Southwest and in Texas, for example.

So the reason why it seems like somebody like me says, "Oh, El Nino causes warmer winters," and then an El Nino happens or -- sorry -- wetter winters, and then an El Nino happens and it's a dryer winter. It isn't because we're all idiots who are predicting these things. It's actually -- well, it may be. You could always take that attitude, but I would argue that it's because there isn't just one mode of variability. There's actually three or four and they're interacting, and so predicting the net can be quite difficult.

Now, you can do an even simpler exercise just to simply take a region, say, centered in the Yucca Mountain area and look at the events in which a lot of precipitation occurred and correlate them with temperatures all over the planet, and what emerges is an interesting pattern of increases in precipitation in the tropical Pacific, actually increases in this region, and a large scale increase in precipitation actually over much of the Northern Hemisphere. This is a large, global pattern. These are called teleconnection patterns.

Now, that pattern is not identical to the pattern which is known as El Nino, and that's a really important point. This is the pattern that you would get if you tried to pick out what's that just due to El Nino.

And you see something similar, say, in North America, as we just saw, except it only makes up a small part of the actual precipitation variability in North America and has a different spatial pattern. So there's actually, like I said, a combination of all these modes or what adds up to precipitation anomalies.

Now, it's interesting that a number of really prominent people, National Academy type people, have actually predicted that in a global warming world we'll actually lose what we would typically think of as the cold upwelling regions in the eastern equatorial Pacific that lead to El Nino, in other words, that conditions may become more like a permanent El Nino.

And if you think about the conditions that happen in this area in an El Nino, imagine those happening all the time. And you can look at what that would add up to, and that would lead to a substantial increase in precipitation, but nothing outside of the range of what's already been considered in a lot of these reports. It's just an additional source of increased precipitation.

Now, I'm going to step back and go back to the global problem again because in order to understand the local problem in the Yucca Mountain region, you have to relate it to changes of the global scale over the next million years, and of course, you can't just do the global. You have to come back down to the local.

But this is one of the areas that I work in. This is the global mean surface temperature record over the past 1,000 years. It shows bumps and wiggles and then right near the end of the record in the past 100 years, it shows this big increase. This is very well correlated with increases in carbon dioxide concentrations and human emissions. This is a thing we know as anthropogenic global warming.

Now, what has typically been assumed, and it's written into many of these documents is global warming may happen. We're not sure, but it may happen. The effects will be felt for about 2,000 years, and then we're just not going to think about it again.

One of the things that we used to project into the future are global climate models, and these I'll show a little bit more about them in a minute, but these include an atmospheric component, a land surface component and an ocean component. They frequently now include interactive vegetation, and they have implicit into them a human component because somehow you have to come up with scenarios for greenhouse gas emissions, and since we're the ones doing the emitting, we have to somehow include human beings into the model.

So this is a range of predictions. Again, you could think of these as stylized approaches. We choose different scenarios that basically have to do with how human beings behave, and try and predict how -- and then we feed the different inputs into different models, and that's what leads to this spread. This goes from 2000 to 2100, and you end up with global warming from anywhere from about two degrees up to about five.

And, again, these are quantitative predictions, but they are stylized in terms of how they deal with the human component of this problem.

A lot of this should sound familiar.

This work, this is from the Intergovernmental Panel on Climate Change. This work involves somewhere around 3,000 climate scientists who worked for five years and issue a report. Every single thing in that report has to be in press at least in a peer reviewed journal.

So there's an intense amount of scrutiny and the science is of uniformly high quality. The climate models that are being used have to somehow deal with the real world. So they have to include an ocean. They have to include land. They include river runoff. They include vegetation, soil, water, infiltration. They include just about everything, not quite everything, but we're always adding more bells and whistles, but they're pretty comprehensive, and you represent the earth as a series of grid cells, and the grid cell spacing is basically a function of how many runs you want to do and how fast a computer you have.

As I said, the models tend to have something like four different components to represent the major aspects of the earth system.

And ten years ago we were running models with a resolution that looked like this, and five years ago we were running models with a resolution that looked like this. The little grid boxes indicate the resolution of the model.

For the most recent IPCC report we are running at a resolution that looks like this. Well, you're seeing what I'm actually plotting is topography. So if you think about graphic effects on climate, that's represented here.

And four years from now, we're going to be doing all of our simulations at this resolution which actually starts looking pretty close to the real world, and that's just a function of how fast a computer we can get.

Let me go back. We can validate the models in the instrumental record period by simply taking these models and feeding into them the things that we know change. So in this case we take natural variability. So volcanoes and incoming cellular (phonetic) radiation variability due to the solar cycle, force the model just with that.

In this case we add greenhouse gases and nothing else, and in this case we add them both, and the key thing to take home from this is when you add them both, you get model records and observations, which are the red lines here that look remarkably similar to each other.

So that's an empirical verification that models get the right answer with the right forcing, and the don't get the right answer with the wrong forcing.

You can also use these same models and do paleoclimate, which is another way of verifying their validity, and I'll talk more about that.

There's a wider range of likely things that are going to happen in a global warming world. I list them here mostly for reference in your printed document. There's a lot to see there, but what we're pretty sure of is the Southwest is going to get a whole lot warmer. There are some results that will be coming out in the proceedings in the National Academy some time in the next couple of weeks, which I can't talk about, but you should definitely have a look at.

It's going to be a lot hotter here. Hydrological cycle predictions are more inherently uncertain because the models don't do as robust a job with that.

Now, the release of CO_2 depends on human behavior. So these are different profiles of likely carbon dioxide concentrations. They go from the modern day value, which is already higher than it has been in the past 400,000 years, and then you go ahead into the future.

And again, these are stylized because we have to somehow represent human behavior. I found it a very interesting statement that to represent human behavior from the National Academy perspective was somehow a statement of things will stay just how they are, and of course, the way things are is exponential growth of population.

So if you extrapolate from exponential growth of population you end up, of course, with one person per square foot of the entire Southwest in 100,000 or something. So, you know, it's an interesting statement.

In the global warming community, the way we've dealt with that is to take existing growth rates, make assumptions about how they will change or not change, not assume that population is staying constant, which it obviously isn't.

For a range of CO_2 releases, you get a range of sea level rises anywhere from about .2 of a meter to half a meter, and I'll just note that if you were to run these models out, you now know that you would get something like eight meters.

That's something that needs to be

considered in the Yucca Mountain process, how if this happens will sea levels change and how will that affect the local regional hydrological balance.

So other than just taking results from one particular climate model, this is December, January, February averaged temperature from a world with four times preindustrial CO_2. It's where we're going to be in 100 or 150 years, modern day model, and this is the temperature difference, and all you have to do is look at the temperature difference.

High latitudes are warmed by more than 12 degrees C. In this region, in this model, you're talking about a temperature change of somewhere between four and five in the winter. So warming and actually a pretty substantial warming in the summer.

MR. HINZE: What time period? Excuse me.

DR. HUBER: yes.

MR. HINZE: What time periods are those again?

DR. HUBER: Sir, this would correspond to where we're going to be in about 150 years, and this is today in the model.

Now, the question of are we going to continue warming past that or how long will that period. This four times CO_2 world less is a different one, which I'll get to.

But we start getting there in 150 years or so. So what we've gotten 3,000 scientists in the world to agree on and the National Academy to agree on and basically everybody to agree on is that unless something happens to change the rate at which greenhouse gases are being increased, well, they're just going to increase, and warming is going to continue as that happens.

Most of the feedbacks in the climate system that we know about are positive in the sense that if you melt back ice, that decreases the albedo of the earth, which just causes it to become warmer.

Other than geochemical processes that operate on ten to 100,000 year time scales, there's no known negative feedbacks in the climate system that have been vetted. So this looks like things are going to get warmer unless something that we don't know about happens.

Now, we also know, and I'll get to this, that greenhouse gases have changed the climate in the past, are a fundamental component of climate change in the past, and one of the things we might do is look to see how far back into the past we have to go to see the greenhouse gas concentrations we were putting in and look and see what the climate was like during that period.

Well, so that we can go back 400,000 years, we have ice scores and we connect -- if I had a newer figure, I could take this back a million and it wouldn't look any different. The top record in purple is CO_2 from ice scores. You see that it maxes out in this period at a little over 280 ppm, which is actually less than we're at today, and it has minimums around 180, and you'll see that there has been this gorgeous beading of climate in terms of temperature, ice volume, carbon dioxide, and methane, and a fairly regular or it's somewhat chaotic, but a fairly predictable pattern, and this can be very quantitatively tied to changes in earth's orbit and how that affects incoming solar radiation at the surface.

So records like that, in this case one could take the Devil's Hole record, which is similar in important respects, and has differences in some respects, but the general idea is the same, Owens Lake records or whatever local records, and you could assume, as has been assumed that we can take those records from the past 10,000 years or a million years or however long we have a record, and make some quantitative assumptions about which part of the records we think are good analogues, and then use these as bounds for understanding what the hydrological cycle shifts and climate shifts will be like in this area, and call that the standard approach, and on this I'll call it Method 1.

And what we've learned from that is that basically from this perspective glacials are the case we need to worry about because they tend to be wetter. I mean, it's much more complex than that, but that's the take-home message.

And those same methods have indicated that we're going to be heading into a ice age in the not too distant future from my perspective as somebody who studies deep time.

Now, another approach would be to actually do it like I said, look at the CO_2 that we're releasing, look at the warming that that should introduce, compare that with global climate models, and then go back to some period even further back in earth's past and there might be a better analogue, and use that period to assess what the hydrological and climate regions would be like in this region.

And then yet another one is -- and this one has been done -- is to use simplified climate models to retrodict the past, to verify that the models work, and then use those to go forward in the future.

So I'm going to discuss each one. Method one, there's been, you know, I've got a stack of papers this high on what has been done with that. People who want to read that can read that.

I already said the main thing that we've learned from that, which is that glacials are wet, which is bad. Occasionally another wet member can be the monsoonal intermediate case, but basically you can bound the uncertainty in terms of these methods by looking at glacials.

And the general idea is that they provide -- you can put error bars on these, and you can go and you can ever improve your estimates of the past change. The problem may be that you could keep doing this, but maybe the basic underlying assumption that the next 50,000, 500,000 years is going to be just like the past 50 to million years.

Maybe that's just not valid, and there's very good reasons to think that that's not the right approach at all, which I'll get to next.

So you could refine those estimates all you want, but maybe they're not relevant to the problem at hand.

So let's talk about Method 2, which has its own problems. Method 2 is to basically do a certain amount of hand waving and argument by analogue, and I'll show you what I mean in a second.

Okay. So if you take the anthropogenic CO_2 and plug that into a climate model, you end up with estimates of global warming of several degrees, say, five to ten depending on how far out you run them to equilibrium. You can take those estimates of global mean temperature change and we have a very good record of this, a paleoclimate record, and we can go back and you can just draw a line and you go back in time, and, oh, okay, the last time it was that warm was, say, 45 to 50 million years ago.

Again, this is just a different paleoclimate analogue. It's the same basic idea, and that would suggest that we're heading toward a climate that looks like the Eocene.

Now, what did the Eocene look like? Well, this is what the West in general looked like in the Eocene. It was a subtropical swamp, crocodiles, turtles, some of the thickest coal deposits in history were lain down during this period of time.

And you can plot those up on a map.

Everywhere that you see greens is basically corals in green happy things. The big orange crocodiles are crocodiles in the fossil record, and the little blue dots which maybe you can't see are lathyritic soils and kaolinite, and those tend to form under very warm conditions with seasonal moisture. They tend to form in the monsoonal regions today or in the high tropics.

And you'll see if you pick a latitude that's appropriate for where we are today, there are laterites and kaolinites there.

On the other hand, there's a big arid zone in the geological record. So it's unclear what to make of this. Now, there's an obvious problem with doing this, which is that the continents move around. Vegetation changes. The ocean currents change. Everything else changes.

So there's a reason why you should be really skeptical of using this approach, but nevertheless, it at least helps you to broaden your thinking when you say we've looked at the worse possible case is a glacial. Well, is it possible to at least think about the worst possible case being subtropical rain forest?

It would be simple enough to test. You just take one of your models and input conditions for

Costa Rica as a boundary condition. That wouldn't address the likelihood of that happening. It would just be an end member.

So now let's talk about what I think is close to being the right way to go ahead, and this is actually the standard way in the paleoclimate and future climate prediction business, although it wasn't used in the Yucca Mountain process, and that is to use physically based modeling, properly calibrated, verify it with paleoclimate, but then use it to go ahead in the future.

This isn't arguing by analogy. This is calibrating your model on the pass and using the equations of physics and looking into the future.

And what this basically assumes, like any other method, it assumes something. It assumes that most of what we need to know about climate is subsumed within earth's orbit, which is something you can, if you're a Serbian mathematician, you can sit down in a prison cell and write it out, as Milankovitch did, or if you're like you and me, you can sit down in class and write out the equations and predict how incoming solar radiation will change as a function of time.

That's an immanently knowable problem. You also have to include the carbon cycle because, as

I said, we have ample evidence that changes in greenhouse gas concentrations are fundamentally important, and in this case, in the carbon cycle we should really include human activity.

So you should also include some sort of knowledge of the carbon cycle or you could do it in a stylized way.

But if you add those two basic ingredients up, and what I'll show you is that if you just take those two basic ingredients, you can explain most climate transitions in the past 60 million years. That tells us that very basic level. We do understand climate and what causes it to change, and we can write down the equations and we can solve this problem.

If you look at the documents that were written by various organizations for Yucca Mountain, they say we can't do that and that's wrong.

Now, basically because of computational reasons most of the people who have been working on this use computationally efficient models, and they lack a three dimensional resolution, and part of what that ends up meaning is that those nice teleconnection patterns that I showed you that controlled precipitation locally here, they don't exist in those models.

So the main limitation of these simplified models isn't that the models are wrong. It's just we won't be able to go to the scale of interest for this problem with them, but they at least give us an indication what the global changes will be like, and I'll show you the next step at the end of the talk, and that will be four.

So earth's orbit is a knowable thing. The quantities of relevance to us are the eccentricity, so essentially the degree to which earth's orbit is elliptical changes as a function of time. I'm going to write down that equation, and interestingly, by dumb luck we happened -- well, maybe not dumb luck -- we happened to be founding our civilizations at a time where we're entering into a period of low ellipticity.

What that effectively means is a change in the seasonal cycle. There are other cycles having to do with precession and obliquity which I won't really talk about, although they're important. As we'll see, we get everything we need to know out of the eccentricity argument.

So this is work by Berger and Loutre, published in Science in 2002. Other people using other models published something similar in 2001 and 2000. This is time before present minus 200,000 years going into the future, 150,000 years. This is eccentricity, which again we know this. We can write the equation for it. It's an external forcing of the system.

And this is how it will change the amount of sunlight hitting the earth's surface at 65 degrees north. And we've known for almost 100 years now that that's the quantity that drives the timing of ice ages, and you can use this model to predict the volume of ice on the planet, and it shows actually exactly the right distribution of ice ages and interglacials in the past.

This is kind of a funny axis. This is ice volume here, where zero means no ice. So when this goes up, that means a warmer world.

Now, if you use the same model that's been calibrated to get the past just right and go into the future, it says for all intents and purposes almost no ice out to about 55, 60,000 years. Okay? So all of the documents that have been written involved in this project say we're going into an ice age some time between the next 1,000 to 10,000 years, and it's just not right.

Okay. Now, there are additional variables you can play with. One of them is to effectively add a little bit of CO_2. That pushes you up on that red line, and that absolutely gets rid of ice sheets. Even the little ones that are left around go away.

One of the interesting things is that we know with existing models if I were to take the Greenland ice sheet today and remove it and then try and grow another ice sheet, you couldn't grow it. It's not cold enough in the Greenland area today to actually grow an ice sheet.

That Greenland ice sheet is there as a remnant from the last glacial maximum. Okay? So if you melt these ice sheet, they're not coming back any time soon.

Didier Paillard published a nice paper. He had several on this subject. I just want to review what it says. We can expect, again, based on a calibrated model that the interglacial we're in right now is going to last at least 50,000 years, and claims that we're going into another ice age are simply incorrect, and he also raises the issue that as we add greenhouse gases, everything changes and we're really in the warm end member of things.

Now, you may say this is one scientist, this is two scientists. I mean, I'm going to show you yet another group of scientists completely independent people to give you an idea that this really is the consensus.

Dave Archer has recently published a paper. They had a calibrated model that's sensitive to orbital cyclicity and it has a carbon cycle component. So now we're going to bring in the carbon cycle interactively into this.

This starts in years before present. So this is the past going into the future, and this is the orbitally driven curve of incoming solar radiation. When that curve drops below this red line is when an ice age happens, boom, and that's what these little red lines are. Thee are model predicted ice ages, and their model predicts every single one with no difficulty.

Now, if you add carbon dioxide, this is another thing that as far as I know is incorrect in the existing Yucca Mountain literature. It's assumed that as we add carbon dioxide this will just go away before the next ice age. If you do carbon cycling modeling, you find that, yeah, most of it does go away. We're only left with about 17 percent 1,000 years from now, but it has this long exponential tail. We're left with ten percent at 10,000 years and seven percent at 100,000 years. So this carbon doesn't actually go away.

And the actual lifetime, if you wanted an e-folding lifetime, it's something like 30,000.

This is the same curve starting back 500,000 years in the past, going into 500,000 years in the future. This is the orbital insulation curve and then convoluted with the model, and these lines here, red line and blue line, are what happens when you add carbon dioxide in different concentrations in a stylized approach to this model, and the take-home point is that when this line crosses either the blue line or the red line is when you have a glacial.

So as you can see, save for the large carbon release, you don't get any glacials 500,000 years. So we can summarize this. These little green blebs (phonetic) here are interglacial periods predicted by the model for the past 500,000 years.

If we did nothing with CO_2, we would be in this green bleb, and we'd be in it for about 50,000 years, and then we'd have glacial/interglacial cycles not too much different than what this usually assumed. If we add a bit of CO_2, you end up interglacial all the time, and in this paper, Dave Archer says we should think about the fact that we're going to melt back all of the ice sheets and the world is going to start looking like the Eocene.

Now, that's the -- I'll tell you that's the probably most likely scenario. There's a scenario that's even worse from a global warming standpoint, but I'll get into why it might actually be better from a Yucca Mountain standpoint, and that's what happens if this warming causes a positive feedback in which we start releasing methane hydrates from the shelves of the ocean.

So methane hydrates is a rather bizarre chemical formula, but since they're a meta stable form of methane that exists in ocean sediments in these red dots basically all around the world, there is more carbon in methane hydrates than there is in the entire terrestrial biosphere. So if he burned everything on the planet, there's more carbon just stored in this methane.

It's meta stable. So it's sensitive to temperature and pressure changes. If you warm up the water, this stuff starts destabilizing, and we know of several time periods in earth's history that were global warming time periods. You crossed a threshold and you started releasing this stuff.

It's a massively powerful greenhouse gas, and it converts to carbon, which is another greenhouse gas. And the cool thing about it is you can hold it in your hand while it burns.

This is a record. Again, this is far back in time, but this is a very good record of the last time these methane hydrates went off. This is 56 million years ago coming towards 54 million years ago. This is a record of temperature.

So you see it was a fairly warm world. This is deep ocean temperatures of about eight degrees, and then boom, they spike up by five or six degrees, and then there's this exponential decay that takes about 200,000 years.

And associated with that, we have carbon isotopes, which the short version of it is this is why we know it's methane. There's only one thing it could be to explain that pattern. And in some sense this validates everything that I already showed you. There are very few negative feedbacks in the climate system. If you cause a warming, it tends to cause more warming, and there's very little to drag the system back.

What there is is geochemical weathering. Important to keep in mind this is weathering of rock and soils, the earth's surface, which feeds back to this issue of infiltration and the soil water that might be experienced here. But the only thing that brings us back from these periods is increased weathering, and that takes on the order of 100,000, 200,000 years.

And Archer has a nice, nifty little model, which I won't really talk about, but it just says there's a strong amplifying feedback. If human beings pushed the world to five degrees warmer than it is today, there'll be a certain amount of carbon release, but once we do that, we'll cross a threshold. The methane hydrates will degas, and then we'll double the amount of carbon and double down our bets basically.

And that carbon is going to stick around for a very, very long time. Okay.

So the results of that method indicate that even if nothing happens, it will be 50,000 years before the next ice age, and that maybe 400,000 years before the next one. In the meantime, lots of other things will change.

The ice age will melt. There will be sea level rise. Temperatures will warm rather drastically, and this may all be further fed back upon by methane release.

So whether by arguing that just based on model simulations and looking at the Eocene that, hey, maybe we're going to Eocene or using these other methods, they all say it's going to get quite warm and stay that way. So why is it that Method 1 predicated that we're sliding into an ice age? One could ask are Methods 2 and 3 incorrect.

I would argue that Method 1 is not considered a sufficient way of modeling the next 100 or 1,000 years by anybody in the climate change community. There's no reason to think it's an effective way of modeling a million years into the future. It's not based on any physics.

So if we want to move forward on this problem of actually predicting what climate will be like over the next million years, it's not up to me to decide whether people want to make that choice, but if they do, there's a very straightforward way to make progress, and that is to use fully coupled climate models that are validated in earth's past and use them to predict the future.

And if we do that, we can actually talk somewhat about accurate predictions of the future. So this is a record of global climate change over the past 60 million years. If we look at this curve, this is a record of deep ocean temperatures, warm climates of about 12 degrees C., deep ocean temperature at about 50 million years ago, and then eventually we get to the icy world that we live in today.

There's a major transition where we put on ice sheets for the first time right there, and that has been linked to changes in the carbon cycle.

This here is a record of atmospheric carbon dioxide. This is a modern day number here. The CO_2 in the past was something like four to ten times what it is today. So it looks like we can look at records like this and line them up with greenhouse gas changes and say, well, some of the major changes have been driven by greenhouse gases. So we have a world without ice sheets. We put one on, and somewhere in there, there's a change between a lot of CO_2 and low CO_2, but there's a lot of other interesting things that go on in between. But we're going to be focusing on the orbital part of this and on the CO_2 part of it.

So these are results that just came out, a record of atmospheric carbon dioxide that goes through the whole interval, and the modern day value is down in around here.

So the last time CO_2 was as high as what we're going to make it be was about 50 million years ago, and when that happened, there were no ice sheets on the planet. So it would be really interesting in terms of validating a model for the future to see if models can predict this kind of regime shift of going from a world without ice sheets to one with ice sheets with the right range of CO_2. And as I'll show you, orbital forcing is important.

These are awful figures. Paleocenaographers like them, and they're not much different. All core people create legal plots that look like this, and other people go to sleep, but the important thing is this is 35 million years ago, going to 31, and this is a record of ice volume. So not much ice, and then putting a bunch of ice on the planet. It's the first time the antarctic ice sheet existed right there.

This is a carbon cycle record here, and this is a record also of the carbon cycle. What these records in toto tell us is that coincident with placing that ice shield there's a major decrease in atmosphere at CO_2 and a very high resolution sense.

Also, in this same figure is the orbit of the earth, which like I said, this is calculable. We can back at least 60 million years with this number, and interestingly, this time period that shows up as having this major ice sheet is an unusual time period in earth's orbital history, but it also coincides with CO_2 changes.

So if you were to add up the results from this work, it says that the orbit had to be just right to put ice sheets on the planet, but also declining CO_2.

So the key is do we have models that if you put those inputs in, give us an ice sheet. So this is a climate model that was run by Deconto and Pollard, and this is effectively ice volume in their model starting off with very little and growing an ice sheet, and the key parameters that they used in their model were changing carbon dioxide, more or less the right amount as indicated by the data.

And what you're seeing here is ice sheets growing on Antarctica, and this is a three million year long simulation, is a fancy way they do this. There's some slight of hand, but you can run these models if you do it in an intelligent way for a million years. Not a problem. We can do this.

What you see is that as you cross the threshold o CO_2 you suddenly build an ice sheet, and the bopping up and down you see is the orbitally driven component.

Now I'm running it backwards in time for a good reason. This is what it would look like if we were to run this model into the future.

We can also pick other periods in earth's history, some of them closer like, say, last glacial maximum about 21,000 years ago, and see how well models do. Let me show you one result for last glacial maximum.

The red lines are simulated temperatures taken in a slice from the south Atlantic, the equatorial Atlantic, and the north Atlantic, and the red lines are the models. The little dots are data.

This is annual mean, winter, summer.

This is a fully coupled model. We have an interactive ocean component. That means we didn't -- there's nothing forced about the fact that this model gets exactly the right answer. The model does this all on its own, if you put in the right orbital parameters and the right carbon dioxide concentrations.

So we can go to all sorts of periods in earth's history, validate the models, and then project in the future. In the Paleoclimate Working Group that I'm co-chair of, we're doing this. We're doing this for LGM, including predictive vegetation, dust, aerosols, doing it for Holocene.

We're currently engaged in a run that will be 6,000 years long to go from the mid-Holocene to today. During this period of time 55 million years ago, 180 million years ago, we're doing it; we're validating the model all sorts of places.

The model is also freely available. You can download it off the Web. There are about 120 papers describing the results of the validation of this model that are also available for the IPPC report.

Now, what we can also do is do high resolution planet modeling. So you may think that the global models, yeah, those are great for large scale patterns, but what does that have to do with Yucca Mountain.

We now have the capacity to do simulations down to, say, one kilometer grid scale and drive those with the global climate model simulations. So we can also solve the scale problem. This is a simple problem to solve. And we can also validate those models using paleoclimate observations.

So this is just one simulation that I'm currently engaged in to try an take out some of these high CO_2 runs further out than they've been done before to see how hot it's going to get, and I'm just plotting a precipitation in this run versus modern day observations, and there's two quick things to see, which is that if I didn't tell you which was which you wouldn't immediately say, "Oh, yeah, well, okay. You know, the global warming world is a whole lot wetter or dryer or whatever." They look actually kind of similar.

Specifically in the Southwest, if anything, the model predicts a drying. Now, this is interesting. I don't say this is an accurate prediction. You would need a whole bunch of models. You need a lot of work, and a lot of people working on this to really make this an accurate prediction.

Well, on the other hand, I'll now look at several simulations, and they all show a drying in this area. What that would suggest is if global warming conditions are dryer in this area, there's actually a bit of a monsoon to the east of this area, and that actually leads to a moistening. So there is a monsoon pickup. It just is not here.

But if global warming leads to a drying and we're not likely to go into another ice age for 400,000 years or something, maybe we don't need to worry about the glacial end member in the hydrological cycle component of these assessments. Maybe.

What worries me about that statement is I know the models aren't perfect, and I know that one of the things that the models don't do very well is the hydrological cycle. Okay? So I just told you this whole spiel about how great the models are.

I also know the models do have problems with the hydrological site. So that's one of the many uncertainties that would have to be dealt with. But I think this is a doable thing if people want to do it. There's absolutely no challenge to moving forward on this other than time and resources.

Thank you.

MR. HINZE: Thank you very much, Matt. That was a very, very excellent presentation in terms of logical order and understandability, and certainly gives us some insight and gives us the insight into Yucca Mountain region that we're looking for.

I'll ask the committee if they have questions. Ruth, can we start with you again?

MS. WEINER: When you predict a monsoon or a larger rainfall in any region, do you also take into account the increase in vegetation and consequent increase in evapotranspiration? Is that included in the model?

DR. HUBER: You can. In the simulation

I'm show you, we don't have dynamic vegetation. So the vegetation distribution is fixed. You can flip a switch and you turn on interactive dynamic vegetation, and it includes everything from soil microbial respiration changes, soil moisture changes, vegetation, evapotranspiration changes.

It can get arbitrarily sophisticated very easily. The question then is making sure that you've validated that sophisticated model, and if you run this model, that dynamic veg. model for today, it tends to put too much vegetation in the Sahel (phonetic), for example. So it doesn't get it all wrong, but as with all of these things, it has a model bias.

MS. WEINER: You get precision without accuracy.

DR. HUBER: Yes, yes. Now, you could always handle that in a stylized sense. It's very easy to say, well, let's assume for whatever reason that at a subtropical rain forest there would that drag in a monsoon, and do a consistency check. That's the sort of thing I do all of the time. That's very easy to do. That's actually the least computationally expensive thing to do.

MS. WEINER: The other question I have is completely disconnected from that one, and that is you show carbon dioxide cycles over very, very long -- over eons. Does your model include both an increase and a subsequent decrease in anthropogenic carbon dioxide?

In other words, can you carry this out to a time when there is no more anthropogenic CO_2.

DR. HUBER: For the future climate change predictions that have been done, partially because of intergovernmental mandates, it's a stylized approach. So you have a separate group of social/economic models as you model what the growth rate of the missions will be based on a whole variety of things. And then you use that as a static input into these models.

There is substantial work that's going on to actually link those two models so that as the Midwest turns into a dust bowl, people change their practices and that affects the carbon input.

That isn't to the level of having been vetted as this other work. People have emphasized the physical aspect of the system for 40 years. We're just bringing in the human component, but people are working on that.

MS. WEINER: Finally, do you think that these models are at a position where you can bound the uncertainties in predicting the climate in the Yucca Mountain region over the next million years?

DR. HUBER: Yeah, if you used stylized approaches. To me the major area of uncertainty is actually changes in the large scale sea surface temperature distribution that will affect things like how warm is eastern equatorial Pacific, which will affect weather patterns.

Now, if you tell me, "I have a theory and I think that that cold tongue and the warm pool are going to go away," now my fully coupled model might not support your theory, but I can just take my atmospheric model and take your theory and say, "Okay. We're going to get rid of the cold tongue and the warm pool. What would the implications be?"

So we can do stylized approaches and sensitivity tests, no problem. The basic physics of getting the water from Point A to Point B with the right boundary conditions is pretty straightforward. It wasn't 20 years ago. Now we can do that.

MS. WEINER: Thank you.

MR. HINZE: Allen.

VICE CHAIRMAN CROFF: No, thank you.

MR. HINZE: Mike.

CHAIRMAN RYAN: When I think about it from a performance assessment perspective, we're typically thinking about water, not so much --

MR. HINZE: We need --

CHAIRMAN RYAN: -- of how it gets to the Yucca Mountain area, but what happens to it once it lands. So precipitation rates and infiltration rates, the behavior in the near surface and subsurface water systems are kind of the key issue.

How do we couple your climate models to getting into the real specifics of infiltration, precipitation, those kinds of things? Does that fall out of your effort?

DR. HUBER: The models have pretty sophisticated representations with anywhere between four and 20 soil layers that handle infiltration, runoff, river routing. So the models already have in them a treatment of it.

Now, do they have the treatment that would be the most appropriate to this region? Probably not. What you would then do is use a high resolution regional type climate model and couple that with whatever infiltration model you felt would be best, and again, that's immanently doable.

CHAIRMAN RYAN: Great. That's a good answer. Thanks. Appreciate it.

MR. HINZE: James.

DR. CLARKE: A very interesting presentation. A couple of questions. Get a little more cord here.

You mentioned calibration several times, and you also then mentioned validation. I'm assuming you're using those pretty much in the same way. In other words, if the model has the ability to predict the past, that gives us confidence in its ability to predict the future.

DR. HUBER: In the more simplified models, the Method 3, those have these tunable parameters, and a lot of the physics is just all a function of these tunable parameters. So those ones usually what you end up doing is you tune them so that reproduce the observed time series over the past million years, and then you don't change anything. You go into the future.

So there that's what I mean by calibration. The kind of model that I'm really talking about, these fully coupled general circulation models, not to say there's no tuning, but the tuning is really of a completely different sort, and those yo would not retune them to get the glacial/interglacial transitions right. They either get it now or they don't. We are tuned for today.

And right now they do get the past right without retuning, and in that sense it's a verification and not a calibration, and so if they can get those transitions right in the past, I think that you can use them without any further jiggering into the future.

DR. CLARKE: So just to follow up on what you just said, I just want to hear you say it because this is an area of controversy out there.

It is your feeling that these models are sufficiently calibrated that they can be used --

DR. HUBER: Yes.

DR. CLARKE: -- to predict the future with confidence.

DR. HUBER: Yes.

DR. CLARKE: That's your feeling.

The other question and maybe asking Ruth's question a different way is you go from global to continental to North America to, you know, the West, to Nevada. How do the uncertainties play out as you go from large scale to the smaller scales?

And, again, as Ruth asked, you are sufficiently confident that you can predict at the much smaller scale.

DR. HUBER: Yeah, in general, I'll say one thing and then I'll care to go back. In general, the large scale distribution of the atmospheric highs and lows and these sorts of things govern the amount of water that will be input into the area and evaporate. Now, the one area where that's really not a justifiable statement is the Southwest monsoon, the one area that's relevant to this, where for a long time people couldn't get the Southwest monsoon right unless they actually put water in. They had to arbitrarily add water to the surface, and, oh, now we get the monsoon. It was the consistency argument.

In the past two years or so, models have gotten to the point where you can get a Southwest monsoon, for example, without adding the water. Now the models appear to be good enough to actually get that component right.

So I would say that, yes, we can actually do this scale argument across the scales and have things work basically right, especially in this region where the monsoon is not necessarily a dominant influence, but I think the model is actually good enough that if something were to change where the monsoon were to become more important, that the model would actually get that.

To me that's actually the key area of uncertainty in these models at the local scale, is where you can get the changes in the monsoon right.

DR. CLARKE: Okay. Thank you.

MR. HINZE: Bruce.

DR. MARSH: Matt, what about even during the glacial times? I mean, how extreme will the climates be? I mean, there's a lot of variability north-south, and you know, the odd thing about glacial time, everybody assumes it's very wet. I mean, you know, but there's a lot of dryness, too, a lot of arid conditions.

DR. HUBER: I mean, one of the things I skipped over in the interest of time was we can do things like predict where the storm tracks were in past periods of time, and this is a comparison of modern last glacial maximum, Eocene and Cretaceous of where the storm tracks are.

And that's something especially at LGM we can verify whether those predictions are correct or not, and so we can look at dust loading. If we have a model with interactive dust, we can actually see does the model put chlorite in ice quarters in Greenland in the right time, in the right place? So we can actually validate all of this.

There are so many prognostic variables in these models that we have almost an infinite room for verifying whether the models are good or not. So, yeah, you can actually get at that, and there's about four published papers on that LGM simulation comparing it with data, dust data, sea surface temperatures, land surface conditions.

One of the best tests is to run that model with interactive dynamic vegetation and then see if you can match the pollen record, and that's something that's being done.

MR. HINZE: Let me ask Dr. Clarke's question in a little different manner. We seem to be coming back to that, and that's the enhanced resolving power that you're achieving. And I think I heard you say that this was largely a function of computational efficiencies that you have today and will have even greater in the future.

I'm wondering about the data and the data resolution. How good are the data that permit you to get to the resolving power? Are we really fooling ourselves that we can do it at this kind of resolving power?

And what is that resolving power? Is it a degree or something like that?

DR. HUBER: Do you mean modern day observational data set or the paleo ones?

MR. HINZE: Well, and also predictive into the future. One of my next questions is you're entering a graphic effect into this, and we know that the elevations change with time. The Sierra Nevadas went up about 600 meter in a million to two million years.

Are you incorporating that kind of detail into these models so that we can get the resolving power that you're indicating?

DR. HUBER: To really resolve some of these range shadows is a difficult issue, but it's not a conceptually difficult one. It's simply do I have a computer that I have access to that I can model at that resolution.

MR. HINZE: Well, I'm going back even further than that. Are you getting or do you have access to the tectonic stability information that will permit us to do that because there are these uncertainties?

DR. HUBER: Yeah. Well, I mean, the issue of the uplift of the Sierra Nevada or actually of the laramide orogeny going further back, gosh, there are huge error bars on that.

I'm actually funded to do a study to look at the aridification of the West that happened in the Miocene. What you see from various proxy records is that regardless of whether you're on the up stream or the downstream side of the mountain in these records, they both have got more error in the Miocene.

Actually large parts of the West used to be much more moist regardless of what side of the mountain range they were on.

MR. HINZE: That was part of the Eocene.

DR. HUBER: Right. Well, it goes from the Cretaceous all the way to the Miocene. In the Miocene everything dries out and nobody knows why, and as far as we know, it has nothing to do with orography because it happens on the upstream side of the mountain and the downstream. Okay?

So I actually -- I mean, that's an area of active scientific research, but I think that the orography arguments for why some parts of the West are dry actually aren't right. I mean, if you look historically, you'll see that they dried out regardless of whether there was a mountain range there.

MR. HINZE: Another question, if I might. The gradient on the change is modest until you hit a glacial period. Is this, the rapid change that we see in temperatures, is this a feedback effect? What causes that very rapid change?

DR. HUBER: Yeah, as I said, almost all of the feedbacks in the climate system are positive. So you add a little ice, it has a little gold (phonetic).

On the other hand, there's a massive change in the carbon cycle right when you're putting on these ice sheets. Nobody knows why. There's apparently some kind of feedback going on with the carbon cycle to bury carbon.

MR. HINZE: Sequester it.

DR. HUBER: Yeah. Nobody knows why that happened. So that's another area of active research.

What's interesting though, again, is if you take a stylized approach and you choose a profile at CO_2, the model gets the transition, no problem.

MR. HINZE: Just another point though. I just want to make certain that we have it down, and that is that if we in some way mitigated the increase in the carbon in the atmosphere, carbon dioxide in the atmosphere, this consistency over the next 50,000 or 400,000 years is still there as a major factor.

DR. HUBER: Well, so if we were to mitigate and bring ourselves back down to a normal level, then it would be probably in a 50,000 year long interglacial and then glacials kicking in about 40-some odd thousand years from now.

MR. HINZE: About 50,000 years.

DR. HUBER: Yeah.

MR. HINZE: Okay.

DR. HUBER: So well, within the one million year.

MR. HINZE: Right. Okay. Can we open that up to additional questions? Mike Scott, please.

MR. SCOTT: Thank you.

With regard to the feedback mechanisms you were discussing, the press has carried various reports that warmer world means wetter world overall in a global scale, means more vegetation, means more sequestration of carbon. Is that not a significant negative mechanism?

DR. HUBER: Actually it's currently what is preventing CO_2 from rising at a much higher rate than we're releasing it. So, in other words, if you look today, there's a component of the CO_2 that we're releasing that's going into the ocean and a component that's going into terrestrial vegetation, and that's definitely there.

The thing is it's only a percentage of the amount that's being released. So year after year, this keeps on being more left in the atmosphere, and how much more of the terrestrial biosphere can continue to uptake is, again, one of these issues the people debate. All existing estimates are, if anything, conservative or optimistic in the sense that some of these simulations that have been done with interactive vegetation where, you know, the vegetation is allowed to say, "I'm being fertilized. This is great. I love CO_2"; if you take those models and you run them into the future, yeah, they draw down some of the carbon. Most of it still stays in the atmosphere.

The problem is, say, in one of these simulations that's been done is a change to a permanent El Nino in the tropical Pacific. I mean, you got to a permanent El Nino and you get rid of precipitation in the Amazon rain forest, and most of that dies back. So it's like you cut down the whole Amazon.

So these things all kind of feed back on each other, but none of the models that have been used projecting into the future show that the ability of the terrestrial biosphere to uptake carbon is going to be sufficient to uptake all of it.

Just taking, you know, attacks, if you

will, off of the amount that we're putting up there.

MR. SCOTT: I guess I was addressing the question from the perspective of your statement that there was all positive feedback or essentially all, and I'm wondering was this not a somewhat significant negative feedback mechanism.

DR. HUBER: Well, it's not a net feedback, no. It is taking up some of the carbon, yes. Is it drawing down more carbon than we're releasing? No.

So in other words, we're adding carbon, and regardless of whether this is taking it up, it's still going up. It's just a slightly lower amount.

MR. SCOTT: Okay. Thank you.

DR. HUBER: It doesn't change the prediction.

MR. HINZE: Was there a question over here? Neil.

MR. COLEMAN: Matt, what assumptions do you make or some of your colleagues make on the time of depletion, virtual depletion of fossil fuels on earth?

DR. HUBER: Oh, I don't make those arguments. I let other people decide when we're going to stop burning fossil fuels.

MR. COLEMAN: I mean with the presumption that they'll just all be used. I mean, that's important for knowing when the atmospheric CO_2 would reach an approximate peak and then start declining.

DR. HUBER: Well, I mean, the point in the diagrams that I was showing is that, say, if we switch from oil to coal to this, that, or the other thing, we're going to basically burn up so much CO_2 and add it to the atmosphere that that amount will be staying with us for 100,000 years.

If we completely switched and went to something else very rapidly, then that might not be the case, but so far I haven't seen anybody suggest we're going to stop burning fossil fuels altogether.

DR. MARSH: But even then it shuts off. It goes 30 or 40,000 years afterwards.

DR. HUBER: Yeah.

DR. MARSH: This dissipation.

DR. HUBER: Yeah, yeah. It's an efolding time scale. We're already committed to a fair amount of this, in other words.

MR. COLEMAN: But what number is actually used in the models? Is it 300 years, 400 years?

DR. HUBER: I could show you the emission scenarios. They're the IPCC-ESRES scenarios, and there's a variety of them. None of them involve going completely away from fossil fuels. So even the lowest emission scenario assumes that people are still burning wood and other things. So you keep adding carbon in all of the scenarios all the way out.

MR. COLEMAN: Another question. You didn't get into the issue of the effects of large scale ocean currents on the climate models, and there have been. I don't know how speculative those ideas have been. For example, brokers' commentary on the Gulf Stream and dramatic effects, actually dramatic cooling effects that would be possible in Europe due to global warming.

What's your take on those sorts of speculations?

DR. HUBER: They're blown entirely out of proportion. When you look at simulations that have been done of what the effect of that would be, they are smaller than the signal of global warming.

So, in other words, let's say you shut down the thermohaline circulation, and that leads by itself to a cooling of three degrees. Well, that's smaller than the warming due to CO_2.

One of the simulations I was showing, it actually has a thermohaline circulation slow-down, and there's little blurbs of cooling in the North

Atlantic, but the rest of the planet really doesn't care, and there's lots of rebuttals to Wally's arguments on this that have been published.

There's a couple of groups that really strongly believe this, but even if you look at those simulations where they've really hit the system with a hammer and shut down the thermohaline circulation and you look in western North America, it doesn't care.

MR. COLEMAN: Okay. My last question, a follow-up on the scavaging of CO_2 from the atmosphere. What are the best references that are available? Who's doing the best work in this area that you've seen?

DR. HUBER: Well, for the near term or for the long? Because, I mean, really there's a totally different community that's trying to model this 50,000 years from now than 100 years from now.

MR. COLEMAN: Longer term would probably be better.

DR. HUBER: Okay. Then the Archer references, which I have sent a bunch of them to Bill and Mike. So I'm sure we can hook you up with those. There are not many people who are actually trying to look at the carbon cycle that far into the future.

Dave Archer did his postdoc with Wally Broker and set the University of Chicago, one of the world's top three carbon cycle modelers, and his work is pretty canonical.

MR. HINZE: Okay. Let's move on.

DR. HUBER: Thank you.

MR. HINZE: Fred, you had a question.

DR. PHILLIPS: Yes. One was sort of a follow-up on Neil's first question here.

You showed a graph extending into the future with glacial initiations as a function of three different carbon level scenarios. One was essentially natural carbon extended on. Then you had a blue line and a red line.

How did those carbon inventories that were the basis for those simulations compare with the current anthropogenic carbon inventory in the atmosphere?

DR. HUBER: The 5,000 gigaton one is we burn all available fossil fuel reserves, and the 1,000 one, which I think shows an egglaciation in 100,000 or something, involves -- they correspond to different ESRES scenarios, which I could pick it out for you, but it involves one-fifth, if you will, of the total fossil fuel reserves.

As far as we can tell, those are very likely numbers in terms of people who try and model carbon use over the next two or 300 years.

MR. HINZE: I believe Abe Van Luik has a question. Abe.

CHAIRMAN RYAN: Can I get somebody to crank the lights up, please?

MR. VAN LUIK: Abe Van Luik, DOE.

In defense of the project, Saxton Sharp at UNR did our modeling. She used Method 3, and if you look at our EIS, she has a minor glacial coming in at about 40,000 years, one at 100,000, and then it follows the natural progression after that.

I asked her about the other modeling that was being -- I was just exposed in Europe to the European Union's model three years ago and said they're moving out the next glacial to about 400,000 years, and she said she was a peer reviewer on that work. She believed at that time -- and she may have changed her mind now -- that it was speculative, and she said, "Look. It's very self-serving to go to their model. For your project, your worst performance comes during those two early isglacial (phonetic) occurrences.

And if you look at our EIS, that's

correct. When we -- basically what she handed us was a deterministic model, you know, showing these peaks, and we put it in exactly the way she gave us.

Now, when we make some uncertainty bounds on the occurrence of these things and randomize it, it looks more like a long-term average, and so that's the stylization that we've gone to. Plus her model did not include the monsoon. So we're throwing the monsoon in as an expert judgment type of thing because we think that it's a real possibility that before a climate change you would have the monsoon.

Now, it looks like what has happened is that the climate modeling community has made a lot of progress in the last three years, and so we probably want to revisit some of these things.

Now, a fly in the ointment is I talked to Ike Winograd recently and said, "Ike, with all of this global modeling going on, all of these foresting functions seeming to pan out, what do you think of Devil's Hole?"

And he says, "Devil's Hole shows that there are local variations in ice ages that are not explainable by orbital parameters," and I was wondering what you thought of the Devil's Hole record.

DR. HUBER: First, for the Sharp report,

I mean, I've got the Sharp report and the most recent DOE report on my desk, and they both say we're going into a glacial within the next eight or 9,000 years.

So there may have been discussion at various points about these other models being correct, but what's in the document is actually very clear.

Yes, in 2002, 2001 I would say that the Berger and Loutre work was I won't call it speculative, but you know, you shouldn't believe what you see in Science, right? I mean, this is there because it's provocative and interesting and this, that, and the other thing, which is why I've actually previously steered clear from relying too much on it.

The fact that four other people who are really completely independent, especially Dave Archer, have reached the exact same conclusion, and it's one that you really can sit down with a pencil and paper and work out yourself.

I don't think it's -- I think it's fairly believable now. I agree. In 2002 I would not have hung my hat on it. Now, I would hang most of my hat on it.

And with regards to the Winograd comment, I found it very strange just having read the Sharp report and the DOE report that there is four important papers by David Lee that aren't mentioned, and for those of us in the paleoclimate community, I mean, I give lectures on this. David Lee shared in 2001 that the Devil's Hole record is very explainable in terms of orbital forcing, but it's an expression in the western Pacific. So that it's not 60 degrees north. It's a teleconnection to the western Pacific, which as I showed, that region is very much teleconnected to the western Pacific. That doesn't mean that glacial/interglacial cycles aren't -- I mean, glacial/interglacial cycles, you grow ice sheets at high latitudes. So those are orbital forcing at 65 degrees north.

But you can explain his record as orbital forcing of the western Pacific, and then a teleconnection there. So I agree that, you know, it's not all what's going on at high latitudes. You have to focus on the tropics, and that's what I'm trying to suggest with these teleconnection mats. We should really be thinking of how is the tropical Pacific especially going to be changing over the next million years. That's actually the key, large scale uncertainty.

MR. HINZE: Thanks very much, Matt, and thank you once again for a very excellently presented and very informative talk.

And with that, we'll take a break until 1:15; is that right?

CHAIRMAN RYAN: We will be adjourned until 1:15.

(Whereupon, at 11:57 a.m., the meeting was recessed for lunch, to reconvene at 1:15 p.m., the same day.)

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

(1:16 p.m.)

CHAIRMAN RYAN: It is time to get started, folks. We will reconvene and reopen the record, please.

Our next speaker is Professor Bruce Marsh from Johnson Hopkins University, who is a consultant to the ACNW.

Welcome again, Bruce, and he's going to provide us with what I think will be a very interesting talk in an approach to modeling of magma/repository interactions.

Welcome.

DR. MARSH: Some people have referred to me, in fact my past advisor, as an architect in the field of magma dynamics, good or bad, and at the same time though I augment that with the fact that I used to tell my mother-in-law all the time that nothing I ever did had any practical application, which I no longer can say. Little did we know that all of the work that we'd been doing in setting up a field in magma dynamics would actually be very useful for a human effort.

And it really comes to bear at Yucca Mountain in terms of understanding what magma does and how it behaves in an integrated system.

And a year ago, those of you who were at the meeting in Las Vegas, we talked in detail about what was needed, and one of the things we said what's needed is an understanding of the integratedness of the system, how it all fits together and works together.

I'd like to give you a little taste of that today in terms of understanding specific parts of the system and properties of the system that may be very, very critical to understanding the system as a whole, and one of these is the behavior of magma in the systems.

We're all familiar with this, and this is a very, at least the picture, it's a critical picture in many ways. Here we have the drifts, and a dike popping up through the system venting at the top and entering in the system here, and so it's a complicated process in many ways, and people who aren't familiar, let's say, with magma in detail almost don't know where to start on these things.

I'd like to give you some background today into it, and we'll start off by looking at a system that basically we know something about. This is the Island of Hawaii, Mauna Loa. Kilauea is the active part of the system, and you can see the lavas and things down here.

this is about a million years old. It's one million cubic kilometers of magma there, and of course you know there's a whole string. And the next volcano is under the water here, Luihi sea mount active now two and a half thousand feet under the water and growing up and to be a new chunk of real estate here in no time.

Well, one of the things that's interesting, and we've developed over the last, let's say, five years or ten years, is the system of a magmatic mush column, in talking about a system that has a plumbing structure to it that may be consistent and is consistent with seismology, geology, petrology, what we see in the system.

And in a big system like Hawaii or systems like even under Reunion Island, Yonmaon, other big systems in the world, we have what we call a system. It's an interconnected system of sheets and necks and things and all kinds of other detail and dikes and things in this system, and the important thing to realize is there are all different kinds of time scales in this, and what I mean by time scales, I mean thermal time scales, for example. There are spatial time scales, and related to those spatial time scales there are thermal relaxation times.

So something as large as this, buried as deep as this down miles in the earth, maybe 30 or 40 miles down will have a long thermal residence time, whereas things near the surface in flank eruptions and things have a much shorter time.

And how the system is accessed, how it's pumped, how it's forced is a great reflection of what you get on the surface. So, for example, you know when people work on your pipes in the street, your plumbing, afterwards if you turn the water on really hard you often gets sand and gravel and other things. If you turn it on real gently, you don't get things out of it like that. You get kind of clear water.

Volcanic systems, magmatic systems are just like that. They work the same way. The higher the flux of materials, like the higher flux of lava, you get all kinds of stuff in the system. You start bringing up deep seeded crystal out of these layers down here. There's layers we call cumit layers (phonetic), and you start bringing up that stuff, and it all comes out.

And from looking at that, we can actually learn a lot about the dynamics in the system, but there are a lot of other things in detail, and this is a coupled system together based on my model and Mike Ryan put together from Seismology, some of the seismicity in the region, and you see the character of the system now. We can actually get an idea of what it actually looks like at depth, and this is looking down quite a ways.

This is the mantle in the crust, and we're looking up further.

this shows the Kilauea area. This is the Halemaumau Fire Pit in Kilauea. In 1959, there was an eruption right here into this pit. The eruption actually was right over here, and one of the things that's very interesting, of course, in Yucca Mountain sometime is the effective topography and stress fields and the topography eruptions, and we have heard; in fact, it has been analyzed. DOE has analyzed some of the stresses in Yucca Mountain and things. They're small stresses, but they're also small here.

This is an open pit. There's an open pit from withdrawal of magma underneath it in a lava tube, and the whole thing sunk down almost like kind of a quicksand hole.

The eruption would have took place and not uncommon took place right on the height here, on the cliff, and you can see where the wind blew things around. But what happened is it fountained up a lot, and the lava went down in here and filled this up to over 100 meters, probably 135 to 40 meters deep. It made a lake basically, and we call these lava lakes.

And some people of the USGS had the foresight to actually go out on it after it was starting to solidify because one of the big problems we always have had dealing with magna is that we never get a sufficiently large pool to do experiments at. You can do experiments with little pieces of stuff in the laboratory, but it's not like actually a big system.

So this thing, we actually went out on it, drilled holes through it, did experiments in it. This shows drilling, when I was involved in it in the middle '70s even. This thing now is just still about 1,000 degrees in the center of it. So it's just getting solid, this lava lake, and it lasted for a very long time. It erupted and placed there in 1959.

So one of the remarkable things drilling into this, this is drilled out in the crust, is that you can actually -- that's the hole. So that's a drill hole. You can actually see the magmatic, for those of you up close, that little red spot down there. That's about 600 degrees, and the holes that annex about a two inch core going down in there.

And one of the things that's very surprising when you start drilling this thing is you could drill down 600 degrees at well below the solidness, in other words, the point at which the magna is solid, which is about 1,000 degree. You can drill out beyond 1,000 degrees and just keep on drilling. It sounded just like you were in a rock. You're drilling firmly in a rock.

Even now when you pull up the core, you're actually pulling up quenched magma, and it kept drilling. You can drill till you get out to about 50 percent crystals, and then you go through a transition where you can fee the drill stem is no longer drillable, and you can actually take the drill stem by hand and push it in. You can feel is mushy going through this stuff.

But at 50 percent crystals and higher, this is a rigid, solid material. Even though it has 50 percent liquid in it, these crystals are tacked together. And we know now that this tacking together starts at about 25 percent crystals and fits together like a chicken wire network and has strength, and the strength increases and increases until it gets up to a certain point.

Now, this is what you see when you look at this and you pull a core out. These are some of the big crystals I told you about that are pulled out in a big eruption, and they came out in the eruption. So that's not -- and those usually fall out to the bottom of the lava lake. A couple of them are trapped here because we're going to the surface, but this brown stuff is glass and those little, tiny small things are crystals nucleating, and they grow in little clusters. They're almost like little parasitic organizations. One crystal that needs this and this components will reject other components C, D, and E. Other crystals will grow nearby who eat C, D, and E, et cetera, and you get these little families, and you'll notice next to these things you get just ground glass growing, and then you get bigger and bigger.

This is titanium building up, and it gets real like tannish, real brownish. Suddenly when the iron-tame oxide is stabilized as a phase, it all disappears. Here the rock is whole crystalline just about down at 1,000 degrees.

So you see this remarkable transition that you can sample in a real system that's true in there.

Now, many metallurgical systems and

systems that people think about a lot are dendritic crystal growth. You take a bottle of wine. When people are late for dinner, you put it in the freezer, a bottle of white wine, and you forget about it. You pull it out and it's get these great big needles and things going through it, and people often think that's how magmas crystallize.

They don't. they don't at all. Those are dendritic systems where the fluid can easily circulate around, and you get a long range chemical exchange. Magma is out here. It comes in here. It circulates back and forth. That's not at all how magma crystallizes at all.

Magma has tiny, little crystals, and the crystal sizes reflect the rate of cooling, the nucleation rates, but they're within a bound. So when things start cooling, the salts especially, if you set a cooling rate, the salt will go to a whole crystalline material.

How does it do it? If it can't do it just through slow growing crystals, crystal growth is governed by diffusion. So if it can't keep up with the cooling rate, it just nucleates a lot of crystals, and so as any geologist knows, you go to the edge of a dike, a sill, a lava flow where it has been quenched. We call those chilled margins.

If you look at them in detail, they're full of tiny, tiny, little crystals, but you can see now these fronts as they move, and these are called solidification fronts. They're made up of a region out here that has nuclei, but not many crystals at all in it, and the crystals get larger and larger, and they have their own little pocket of liquid attached to all of these areas until you get in the back here, and it's all solid.

Now, remember we can drill out to right in the middle of this thing. You can drill it. You can land on it with helicopters. You can do all of these things. You're walking around in it. Out in here, this is a mush. So we call this the rigid crust. The middle section of much, this is called the suspension zone out here. And so these are very important to keep in mind geologically.

So here's how we have the divisions, and the crystallinity then, which is enormously important here, the crystallinity goes from zero to one in terms of fraction. What you see in here, this is the viscosity of the interstitial liquid.

The interstitial liquid changes its viscosity remarkably partly due to the cooling and temperature, but mostly due to the composition. The liquid composition is actually changing. The crystals that are being crystallized out have a composition different than the bulk material, and they what we call differentiate. It is still the interstitial liquid such that the material out here, of course, is very much basalt. This stuff in here is like a granite, the interstitial liquid, and this is the key to really understanding.

It's the simple process of separating these crystals from this liquid is what gave rise to the divisions of the earth and the continents and the oceans and basins, et cetera. This is very important, and we'll come back to this time and again, but this is very important in this thing.

So remember we get a chicken wire network setting out here of some strength, but we get back here at about 50 percent crystals and this thing is rigid. It's a rigid, drillable crust that has great strength.

Now, if we model materials that have -- if you just take an isothermal material, liquid, and start putting in solids, it's a very, very interesting problem mathematically. So here's the relative viscosity we start out with. So let's just say this is like if you had a fluid like water and put in a core label suspension and you kept bringing up the concentration, bringing up the concentration.

What you will see is these are a whole bunch of models that are used in the world. This is a very important process because in all kinds of factories we need to know how things could be transported like this, the paper industry, pulp, all kinds of different systems, any systems involving slurries, all kinds, emulsions, all kinds of things. We need this kind of information, but you'll see there's a .6 value here more or less, .5 to .6 in terms of this crystallinity where all of these models show the viscosity goes up without end. In other words, it basically goes infinite.

The rest of the whole world, and this is what I'm telling you about in terms of the solid build-up in a rock that's crystallizing, and the magma is crystallizing, these crystals not only touch and the viscosity goes up, but they actually tack together and weld together forming this.

I talked a lot about these. This model up here, this Roscoe model is probably the simplest of all, and I've adapted that and changed it really to fit rock systems some 20 years ago. It's used almost universally in the world today to model these things.

So when we look at a rock then in its crystallinization range, this is crystallinity. You don't have it going from zero to 100 percent. This is temperature. This is a Hawaiian Tholeiitic Bassalt. In this range out here, you can do things. In fact, if you really want material to flow very, very rapidly, you want to be out near what we call the liquidus, the liquidus beyond which everything is liquid, below which we start going crystals.

Processes that you want the magma to flow, you don't want to get near this boundary in here because in this region it's a rock for all intents and purposes. It still has to cool down and either quench or it's liquid out, but back in this point it is.

And this is what you see not only in the lava lakes. You see it under any rheological models, and it's very much a given.

Now, an absolutely interesting manifestation of this in the world, that if you take a plot up, for example, the crystallinity versus silica content in a rock, and as you know, basalt has about 50 percent silica. Rhyolite or granite has about 70 percent, and these are important factors, of course, in controlling the viscosity.

What do I mean by that? I just told you that we can increase the solid content, increase the viscosity, but we also can increase the silica content. The salts are very fluid. Grunetic (phonetic) rocks that have a high silica content on a very sticky, gooey and have a much higher viscosity, about 10,000 times to 100,000 times higher.

Now, the other observation when I first started doing this work 20 years ago or more is that you realize in the world there are no lavas that erupt out of any volcano in the world that has more than 50 percent crystals in it. I talked to an old volcanologist, and he said, "That's a mystery." He said, "We wondered about this."

I said, well, now we know what it is. When these things are at maximum packing, the materials is called a dilatent solid, and that means when you try to sheer the material, for the particles to move past each other they have to move out around each other. So the whole thing dilates.

And when you're in a volcanic neck and you sheer this to dilate, there's nowhere to dilate. The system is plugged. The volcano if it's near the maximum packing and you sheer it unreasonably hard, what happens? It explodes. It rips the top out of volcanos.

That's what you see on here, all of the bad actors. It's a little hard to see. You can see El Chichon and Mount Pele and all of these guys down in here. They're all near the critical crystallinity, 55 to 60 percent crystals.

The barrier does go down a little bit as we increase silica content.

These are basalts down here, and you see on here one, two, three, four, five percent water, et cetera, added to siliceous systems. Water is much more prevalent usually in the big siliceous systems. So this barrier moves on a bit, but this is a dramatic show that this barrier controls basically what we see coming out of volcanos.

It also controls the composition of materials. If you go to a lava lake, for example, in Hawaii and look at a phase diagram. This is a diopside, one kind of mineral. Another mineral in a silica, et cetera, SiO_2. You plot all of the compositions on here and you basically get right there. You get evolution down to that point, but it stops dead there. It doesn't go beyond.

That's because if you crush up the whole solidification front, that's where the point is right there. The interstitial liquid is in there, spans this range. It is interstitial, however, and you can't get it out by normal means. There are processes obviously we got it out because we grew the continents, but it's a dramatic also show, that if you go to Hawaii, you basically stop right there. None of this is ever shown, and that's another reflection of the solidification fronts, this dramatic change in viscosity as we go through this cooling range.

The other thing that happens in this range, of course, if you have a system and it has some water in it, as we get back into the system and we get crystallization, even though we don't have a lot of fluids out in here, it's dissolved in the system. We can get bubbles forming back in here, back in the system.

And magmas are like divers. When magmas come up from great pressure even though they have water dissolved in and the water is perfectly happy in there, one, two, three percent of high pressure means almost nothing, but as it comes up, as the pressure is decreased, the solubility goes to zero.

In other words, at room pressure and high temperatures, these vapors are insoluble in the magmus. So any vapor that's in it, water, CO_2 for example, SO_2, must come out at low pressures. It must go to a dry system because the solubility is zero.

But what happens at pressure then, when this thing starts to crystallize, you can actually get the saturation where you actually get bubbles forming back in the solidification front, and this can be important actually in modifying, mechanically pushing around the liquid, et cetera.

Now, the next important thing that we want to start to look at is the fact that the phase relations that I just showed you at one atmosphere down here, like an Hawaiian basalt, change as you go up in pressure.

This is an Aleutian Island basalt in the Aleutian Islands, and the pressure, here's 30 kilobars, which is equivalent to about 100 kilometers down in the earth, and as we go back in pressure, everything is liquid out here. You can see these are the various field, the various minerals.

The stability fields change as we go up in high pressure, and of course, if we wanted to put water in the system and we raised it up to high pressures, it would actually affect these phase relationships.

So there's a general kind of process here.

If we look at the phase relationships for a basalt, for example, under pressure under a dry system that has no vapors, no volatiles in it whatsoever, we have a positive slope to these, from liquids and solids in here.

Now, remember if we get out here at 50 percent crystals in the middle of this thing, this thing becomes an immobile body. If it's rising up to the earth's surface and it get to the point it's 50 percent crystals, it's over. It becomes a plutonic contribution to the earth's interior. It's no longer mobile.

In fact, the closer it gets to this barrier, the less mobile it gets. Really mobile stuff is out here on the edge, but this is a positive slope. When magmas come up from deep and they're starting to crystallize, they always want to rise out this way because they want to risk adiabatically. Adiabatically means they rise up and basically cool a little bit by expansion.

Now, if we take the same material, add some volatiles to it, two or three percent, four percent water, what happens is that material is saturated at low pressure. The melting points of those minerals, the phase relations are dramatically affected. It lowers the phase relations up to a point when this is saturated until a point when it's not saturated anymore, and then it resumes its natural progression up to high pressures.

But you will notice now when we have a magma that's here, for example, and is going to erupt on the surface, its temperature could be less than its solid temperature is on the earth's surface.

How does it get to the earth's surface? Well, as magma rises adiabatically and water dissolves out of it, it can rise up in its temperature a little bit. It can heat up a little bit, but it's a major problem in getting that magma out on the earth's surface. It can erupt explosively and things like this, but undergoes a lot of solidification because it is already cold. It's already colder than what it will be at the earth's surface.

Now, if you look at one of the basalts at Lathrop Wells, we see exactly these kind of relationships. this is the dry magma. We have good computational systems, and these are various phases you don't have to pay attention to, but it's all liquid out here. It's all solid back here, and here's ten kilobars. So that would be up to the base of the crust, and it has a liquidus about 1150, 1170, and it's solid at about 1,000 degrees. So it has that interval to work in.

Now, an actual piece of the Lathrop Wells was worked on by Mack Rutherford at Brown, and they were able to recreate the conditions, magmatic conditions that they thought typified that material, and they signaled this out. They found some hydrous minerals, and they published a paper showing that was the conditions there. It had something like 3.5 percent, 3.7 percent water in the system.

So the phase relationships of that then are like this. Up here it would actually go up like this again. So the preeruptive conditions are here.

Now, you'll notice that those conditions are actually at or below the one atmosphere solidification temperature. In fact, if you want to get that out in the earth's surface now, remember -- in fact, we make a plot. Here's the Hawaiian plot. Here's the Lathrop Wells plot of data, the same kind of crystallinity versus temperature. Here is the liquidus. So we're talking about an all solid and an intermediate temperature here of something like in between of 1090 or something like this.

And we put those boundaries on here. This is the region where it would be very fluid. There's the Lathrop Wells over here. It's fluid. It's quite fluid, but it has to actually get to the surface, and if it wants to erupt, there's a fluid, easy flowing magma, it has to move way out in here, which is impossible for it.

So it comes out basically as it starts erupting up. It loses a lot of volatile material, and this volatile material breaks it up into ash and tephra and things like this, and that vaporization phase propagates back down the column and dehydrates the system a bit and the magma things come up.

However, the system is cool. It's cool already. It's fairly cool. So lava can come out, but it can't come out in a very, very fluid way. And we see that very much.

So instead of having basalts that travel across the countryside like in Hawaii that start off at Kilauea and go for miles down the slope and off into the ocean, which is a thing you can do when you're a system like this, when you're a system like this you're rising up to the earth's surface. Any crystals that are in it, since it always tries to go and it burns up all of the crystals, it burns crystals all the time, and it rises up, and when it leaves at the earth's surface, it's very near its maximum temperature, about 1,200 degrees in Hawaii. Usually small loads of crystals, and it just flows fluidly down the slopes.

A lot of people have that in mind for volcanos in general, but we have this here in these alkalide basalts that we're looking at out there. Okay? So it's a very different situation, and that's why these guys don't go very far, and they're also small volumes of materials involved at the same time.

So when we're talking about a scenario like this now, these are kind of interesting factors to take into effect, and it's probably a good time to say a lot of the modeling I've seen in the dike-drift interaction, very nice modeling. Excellent calculations have been done, and some variable viscosity has been put in, but only in cooling, only as the temperature cools down, almost like pancake syrup you put in. It increases the viscosity a little bit as you cool, but none of the real strong effects of solidification is in.

So some of the things I'm mentioning today could be used to knit together already existing good pieces of research that have been done, and we could actually do a tighter job on, I think.

Instead of seeing what we just saw there of a large opening with a very thin little dike, for example, back and forth in the small volcano in the surface, perspective-wise we see many dikes, of course, that are one to two kilometers long or at least hundreds of meter long. And if we look at a drive that's five and a half meters wide, it's really a very, very tiny, little part of this system.

And magmatic systems, if they want to move, they're just like us. They want to do it in the least dissipation of energy. So they'll move up, and if they run into an obstacle, they just go around and keep on going, and we want to find out if there really is an obstacle there.

Now, dikes. Dikes in general, they're elastic cracks like you see in your windshield of your car at times, except they're overpressured with magma, and they move up and they do all kinds of dances and things as they come up.

So very, very commonly the leading edge will be broken up in a series of staves back and forth, and these guys propagate back and forth and the coalesce with depth, and because of the elastic theory, is they propagate around each other to do these kind of dances you can see.

Now, these are very local, delicate

features. I put in here to show you regional dikes forms. This is Hudson Bay. This is the Mackenzie dike swarm way up here that goes all the way down through Canada, and you can see how it's steered by the stress field in the continent, and this is not what we see out there. We'll see small, little dikelet areas. We see more -- you're not going to find that in your handout because I stuck that in at the last moment.

This is in Antarctica. This is a big seal, but this is some of the preexisting dikes you see. They're usually, you know, half a meter, a tenth of a meter up to a meter or two wide, not generally very large. You can see them propagating. We're looking down now. In Antarctica here, we're three to five kilometers down in the crust, beautifully exposed areas, nothing on it in terms of any vegetation or anything, and you can see these dikes as they propagate around each other moving back and forth.

They're not this infinitely fissure sheet that's coming up. So magma is moving around, trying to fit its way up, and here's a very nice one. It's a little hard to see here exactly, but you see these guys curving around each other over here, and the countryside is full of these things in some areas.

Small, and of course, they cool rapidly. A dike this side has about an hour to live before it's solid. It hits the 50 percent right away and it may move a little bit in the middle, and then it's done unless it is resupplied and has to propagate again. These guys die a thermal death rather rapidly.

These dikes can be made in a viscoelastic material. Here's me in a younger phase of my existence and experiments doing at Cal Tech. Here's Sven Mallo. We made a system of viscoelastic material, and we actually propagated. Unfortunately, you can't see it very well here, but it shows exactly the finger pattern that we saw before.

Just for historical purposes, three people who you'll probably never see standing shoulder to shoulder, Jerry Wassaberg, Don Anderson, Lee Silver. You know, Jerry and I don't want to be in the same room.

And the nice thing about using some of these, if you make it on Jello, you can eat it afterwards, and especially when you use whipped cream as the magma.

So dikes. Dikes undergo the same problem, the same phase, of course, except that they have two large fronts, and they have big cooling fronts, and this stuff propagates in from each side, and mainly when these are pushed, the material comes out of the middle of this thing where the material is the most fluid.

However, it is a very, very tenuous process of feeding the system as these fronts are moving in. Now, those well versed in physics will realize that this is a very interesting system because what you get here is the fluid is going at right angles orthogonal to the cooling field, and so because the fluid is flowing at right angles to it, the fluid flow, no matter how fast you flow it, it's not going to burn back the edges. The solidification front just keeps marching right in.

There are orthogonal vectors. It's like when you shoot a rifle bullet. You shoot a rifle bullet. It drops to the ground the same amount of time it takes you to drop it right here. Just the velocity takes it somewhere.

Well, these things have only a certain amount of time before they propagate in, and what happens with these things a lot is that if the system is being pumped, the dike will actually try to keep pushing out the walls open. It is over pressured. As the front comes in, it will try to push it open, push it open.

When the over pressure dies down and the eruption is over, the dikes actually may seal up. They actually may become much smaller. So when you look at them later, dikes and small cones and things, they look tiny. They might have been significantly larger, tens of meters larger, I mean, just slightly larger.

So when we're looking now at systems like in the solidification, we should think about what happens in the lava flows. What happens would be in the system of Yucca Mountain. We'd worry about the dikes going up, and these frosted areas on the outside are called thermal entry effects.

As soon as the magma starts going up through this cool rock to a larger mass at depth, it immediately starts to be quenched out in the margins. The further it goes, the more these guys propagate inward, and these thermal boundary layers on the margins get thicker and thicker with time. The actual active part of the dike is thinner and thinner.

So when a dike hits the repository, if it does, it will already have established by it, around it, some kind of a chilled margin. So it won't be just pancake syrup at a high temperature just zipping in immediately. You have to worry about these things.

As soon as it turns the corner and goes into the drift, we also have thermal boundary layers built up, and we also have the hole in that large sheet. So material is going to start in here, and on top of it, we're depressurizing the system. We're very close to the earth's surface now. So we depressured the system. This thing wants to be a solid. It's starting to crystallize and solidify enormously rapidly. So as soon as it hits the opening, it has released pressure even more. This thing will either go into a phase of forming tephra or ask or, if it has been degassed, it will start forming a very boldish, thick, viscous toothpaste-like extrusion that will start pushing its way into the front.

So this is what you see here, and I started this out. This is the thermal entry factor of well mixed tank, but it's not, of course. It comes in. It already is cooled somewhat, and then it starts hitting the system.

I've also shown you a system that will actually start and stop. We can actually see this in the rocks where a system will erupt for a while, stop, the fronts will go in and it will start up again, open up, and we can actually read this in the rocks, and this could actually be done at Lathrop Wells.

Now, what happens then is the system starts closing in. Instead of the flux of material going in via constant or increasing with time, because it starts closing up, this flux is shut off. It starts to shut itself down because the solidification effects are moving in all the time. This thing has a big bulbous front. It starts to plug itself.

It's a natural plugging material. It's like we were kids, teenagers group up and old timers would say, "You've got a hole in your radiator. Put in a raw egg."

What do you mean put in a raw egg?

Well, as soon as the raw egg gets into the opening, it's fried, it plugs the hole in your radiator, and your radiator is sticking up. This is kind of the raw egg treatment.

And you can see what I've done. I've taken a canister filled holes, and the part near the top is about three meters in diameter. These are various viscosities. This is very low, ten to the three, ten to the four, ten to the five. They could be ten to the six, ten to the seven, ten to eight poise. They could be even higher, which would mean the flux would drop off immediately over this time span of hours here.

And this is just a schematic now. I don't want you to take that too literally.

Now, to give you a feeling for what a flow looks like, a manageable flow like you might have seen at Crater Flats or Lathrop Wells, this is in Hawaii, for example. This is a flow front, and this moves along with a tractor tread. This thing is basically a solid. It's incandescent. It's probably five, six, seven, 800 degrees, but we're talking way below the solvency of this thing.

This is moving. It's being pushed from behind, and big blocks are falling off the front. It's like a tractor tread. It's moving slowly, pushing its way down through the vegetation things.

Okay. Now, if you're near a vent on the earth's surface like near Kilauea and this liquid magma is actually going through the air, I just want to show you this is spatter. This is magma. It hit on a tree, and it quenched on the tree. The tree quenched it.

This is an important characteristic. Magma is so hot it gets near anything and it will quench and grow a rind on it instantaneously. We even see this with human beings in Pompeii and things like this.

Dramatic, a tree. These are tree casts. A lava flow hit a tree in Hawaii. It just start quenching around the tree. Of course the tree catches fire, dries it out. It burns up the tree, but the column of magma, the column of lava stays there. So these are large. These things stick up and they're tree casts.

So this just shows you. You don't need something that's highly resistant in temperature. Any time magma hits any kind of cool surface into this room, anything, it starts quenching out. So the first thing it does when it hits one of these drifts, it quenches on everything that's around it. It starts quenching out, and what do I mean by "quenching out"? It becomes solid, and the motion has to go somewhere else.

So this is a flow front that you see at Lathrop Wells, and very, very similar to the one I showed you before, and this is the front. And you can see these big pieces of material had moved along, squeezing along on it.

And we can predict very, very nicely the cooling, the quenching time. This is the crust thickness, for example on lava flows and lava lakes. These are my calculated lines going through the data. This is square root time in hours. There are days here, and here's one day. You get a half a meter basically with one day of cooling time.

And of course, it is exponential. So if you stick something instantaneously into a vat, you get a rind on it instantaneously of a couple inches very, very quickly.

This is a dramatic case. this is an alkali basalt from the San Bernardino volcanic field in southern Arizona, very, very much like the stuff, almost identical to the stuff that come sup at Lathrop Wells and Crater Flats.

this thing in the middle is a piece of the upper mantle. This came from over 30 miles down, and you'll notice on the outside it has got a quench rind. This thing here was over 1,000 degrees. It got caught up. It's a piece of the upper mantle. It's a piece of prototype, but it has a beautiful quench rind around it.

I give this to students on an oral exam. Based on this kind of information you can calculate the original temperature of this thing, what it was in when it was dropped in.

So this erupting material from Great Gap picked this thing up. We call it zenalis. It was slightly colder than magma. The magma quenched around it and brought it on up. So this thing has a quenched rind around it. The rest of this is vasiculated, in other words, brought it all the way up near the earth's surface.

So quench lines are very, very important, and here's another one of these rubbly fronts that Lathrop Wells, and that's what a five a nd a half meter would more or less look, with a canister, would look like around this thing. In other words, to force this material into that opening would be a very job.

And this would be a quenched line after about an half hour and another one after another half hour, for example, and I just schematically put it on. We could actually do -- and I want to put out here to people -- we could actually do a very nice calculations here, numerical calculation, that would actually do this, calculate this and figure it out very, very nicely. We wouldn't have these little ears sticking down with fill-in, but look at the opening that you have to deal with in pushing material into this thing.

Now, we're just talking about material

entering into this or coming up even around a container. Quenching would be enormous. Now, the cooling off of these things, it doesn't really matter. I've put these in to show you how systems cool as opposed to really being on the earth's surface, being buried deep in the earth's surface. It doesn't really matter that much.

The important thing is, the incredible thing is magma is so much hotter than anything it encounters, it's such a foreign world for it to be on top of the earth's surface that it just quenches out everywhere it can possibly be.

It's a shame we can't see that brighter, but these are large intrusions. You'll have to take my word for it and look at it later, of antarctica that we can actually see where they propagated out and we can actually see the quenching around the margin of these. These are large. Even though these are 1,000 feet thick and kilometers long, we can see this phenomena happening there also.

And here. This is a large, integrated sheet sill, and not only does it quench out around the margins. These black areas quench around the margins, and I've simulated the magma where it would be in the bottom, and it goes along , and you actually don't get any more. It goes along. This piece goes out here, and this things goes then from about 300 meters thick out five kilometers, seven kilometers, down to about one centimeter thick where it's totally quenched out and stopped.

So to remind you then, these sumafication fronts, they're everywhere, and if we do any realistic calculations, we definitely want to take an account for these.

I want to end up also with coming back to what I said in the beginning about these thermal relaxation times in this, in moving magma from one place to another, and also of thinking of the system as an integrated system, but not just drawing your sheet at depth, but actually getting something that's integrated into the system.

Why? Because these all have different sumafication times. Different areas we look at, like I told before, have different regions in them where we have a hierarchy of cool-down times or sumafication time.

So, for example, if we took the DOE picture, for example, and we had the drift and had the main repository added, we would be able to do an analysis like this and lay this out in hierarchical terms of saying what's going to seal first, second, third, et cetera, in the system.

I want to touch on one last thing, and actually this comes up in these modelings by various folks, and that is the whole idea of convection in magmas. People say in some modeling people have convection in the drifts. I don't see it by DOE or EPRI, but I've seen some other group and done some modeling, and I want to show you a little bit about that.

It's been a very, very interesting topic because in big magma chambers, regions as big as this room or ever huge regions that may have a thousand cubic kilometers of magma and some systems we think had 500,000 cubic kilometers. The idea of convection comes up, and so I want to give you a little idea here.

This is kind of a different diagram. This is nondimensional time. This is time going off to the right. It's time scaled with thermal diffusivity in the link scale for the system. So you just think of this as time going to the right.

This is temperature. This region above here is what we calla super heated region. It's a region where the magma is actually above its first crystallization point.

We never see any magmas that are up there out of the earth above their liquidus, in other words, above their point of crystallization. It's always at or below it on earth.

However, there are metered impact sheets like the Sudbury sheet in Canada because of an extraterrestrial and large impact of something about 12 kilometers, it produced a sheet of magma 1,700 degrees, well above its liquidus.

And the cool down sequence has been very important for us. What we have found experimentally and I'll just show you in a minute, that one of magma's superheated actually convects rapidly. As soon as it gets to the liquidus, convection ceases immediately, and I'll show you some of this in a minute.

So we go on a range then. If we have a superheated system, and these up here we're looking at systems that are far from that. These systems are systems that can hardly get out of the earth because they have volatiles in them, but I wanted to show you one thing that we want to make sure.

So once it's in this range then, we actually talk about conduction cooling. It's all conductive cooling, and this makes your analysis so much easier. These are all basically linear analysis.

Here's a system that we've been using at Hopkins. This is a paraffin. This is in a decane. It's a paraffin that has actually a liquidus and a solidus in it, 25 square centimeter tank cubed, and what you see at the top is a solidification front growing in from the top. That real white area is the mushy zone. Right at the margins is a thin mushy zone, and the darker stuff is where it's all solid.

Now, you can see this is superheated, and so the ray number that tells you about convection is large to begin with and is within a few minutes of starting to cool, it's insulated everywhere else. We cool it strongly from above. It goes into very vigorous thermal convection.

Within an hour or so, you can see this thing. It is pumping out the superheat, and the convection is waning, and any plumes that are falling off are just falling off right at the roof there, and as we go on further with the system, it actually dies entirely after about four hours, and the whole system then takes about ten days further to crystallize down, in other words, this front to go all the way down to the floor.

Because it pumps out all of the superheat. Once it gets to the -- these little particles in here are just little buoyant, neutral particles. So we actually have a laser sheet going to the sides so that we can tell what's going on in the system.

So you can see that this thing actually becomes totally stagnant even though it has the viscosity of water. Okay? So there's no convection in this system. The system is not convecting this.

The last thing I wanted to say a couple of words about is that this is a kind of funny diagram where I talk about filling time for things, which is the flux of the eruption in times, the duration of the eruption. There's a couple of things on here. One is the eruptive flux.

People have estimated eruptive fluxes called large igneous provinces, provinces where they can get out in and they can date whole big sequences. So these are probably large fluxes.

But we're talking about here cubic kilometer per year, ten a year, 20 a year.

The thing that's also important is Tom Simpkins' analysis in the Smithsonian of how long, the duration that these things last. And you'll notice that the highest his stepping down here, the most common eruptions are from a tenth of a year to a year, and so you combine these together, these kind of rates, whatever you want to do, and I think we're at a rate that's probably way up in here, a very small rate. We can get an estimate for how much material is in the system, how big the either sills, which are horizontal sheets, or dikes will be in the system, and we get a real feeling for it.

So we can put these things on the system. We do know things that we can add in, take this cloth, and weave it together a little more.

So I want to leave you. When you look at a system like this, it's in antarctica where I've been working for the last 15 years and other places I've been working in the world. This is a system that's full of magma. These were large sheets of magma covering 10,000 square kilometers, for example. They were sheets that were injected, and there were about 1,000 feet thick. There are four or five of these big sheets going up and they're 180 million years old, and the continent has broken up. Dikes around, propagating edges, tips, all kinds of things. It's a wonderful laboratory for this kind of thing.

And so we've had seven expeditions here. It becomes so important in people's thinking that the

National Science Foundation let me take 25 scientists around the world down here last year, and you'll see an entire session at the HU on these kinds of processes that may be important. It's called "Magmatic Processes: Antarctic Perspective," which there will be 30 papers at AGU in the fall.

So the thing I want to leave you with, a couple of things. Convection is out in these. These are very sluggish systems. This magma is having a hard time to get up into the earth.

Solidification is enormously important, and it can be modeled. It can be handled, and we're at the point really with all of the work that has been done, I think, to do a little more careful modeling and really get to some firm, firm, I think, conclusions on some of these things.

So thanks very much.

CHAIRMAN RYAN: Thank you, Bruce. That's a fascinating talk.

I'll start with questions. Jim Clarke.

DR. CLARKE: Thanks, Bruce.

You've given us what I guess I would call a conceptual model supported by physics and analogues for what you think would happen if magma were, in fact, to reach a repository and interact with a waste package.

Can you design an experiment that would support that?

DR. MARSH: Yeah. There's actually a number of experiments that could be done. One is some numerical modeling on this setting up, using these geometries, using these real materials, and it's not that difficult anymore to do this kind of thing.

Secondly, we can do some scale analogue studies in small scale. In fact, we're doing some right now for a different process. These processes we're talking about where these solidification fronts move in and laterally when magma is flowing is very fundamental to how crystals are distributed in the systems.

And so I have a graduate student, for example, who is as part of her project working on one of these big systems in antarctica.

We could do this on a small scale with the right materials. There are solidification experiments that actually use paraffins, tubes, sheets, and things like this, and we can actually do this, I think on an analogue, small, scaled down system, and we can also do some large scale things, I think on some of these systems.

So without a huge amount of labor, I think some clever experiments, things could be done.

DR. CLARKE: Thank you.

CHAIRMAN RYAN: Bill Hinze.

MR. HINZE: Bruce, what are the implications of the lack of convection to the repository?

DR. MARSH: The fact that these things don't convect at all, well, they're so sluggish they can't convect, is that it makes the whole system much, much easier to treat, but it also says that the thermal relaxation time -- it goes right into a solidification state very, very quickly. There's no way you can have, for example, material coming into the dike, circulating into the drift and back. That would never ever happen in the systems at all, or heaven forbid, this material going into the drift and then sitting in there and convecting and stewing on the canister, eating on the canisters.

As soon as this stuff encounters the canister, the canister is a big lollipop. It just quenches out all around this thing.

And there are analyses. You know, the canister probably won't be moved by this. These are so heavy the effective density is heavier than the magma. The modeling I've done using all of the data I can get on the canisters, and they're 15 tons.

And if you actually work up even with the air inside and everything, they are heavier, much denser, significantly denser than the magma. So they're not really going to be moved by this stuff.

MR. HINZE: A parallel question. We see these sills extending for kilometers. Why can't the lava extend for a few hundred meters down a drift?

DR. MARSH: Yeah. The ones I've shown you in Antarctica are down five kilometers in the earth. there's a large amount of material. We're talking about 10,000 cubic kilometers, for example, or something like this in magma, not small little batches.

The batches of stuff we see in these kinds of regions, these small cinder cone regions, they're up in a very foreign part of the world with very small amounts of magma relatively speaking, and it is solidifying rapidly. So we get these small, small dikes and --

MR. HINZE: The thermal reservoir isn't there?

DR. MARSH: Yeah, the thermal reservoir isn't there to keep these guys alive. this thing in

Antarctica was a continental rift, of course, as the continents were moving apart. So material was part of what was going to become an ocean reef system, in effect an infinite bank account there to work with.

MR. HINZE: Thank you.

CHAIRMAN RYAN: Bruce, it's a fascinating picture you've created for how the magma would intersection waste packages and so forth. Is there any way to think about a secondary process where a new magma would come up and somehow intruse into this now quenched material and reattack the waste packages? Once it's isolated in that quench material, it's over?

DR. MARSH: Yeah, that's a very good question, Mike. One of the things we find in these systems is when magma has come in and solidified, what I call it it trusses up the system. It basically and even in the Antarctic case, those sills that we see in Antarctica, there's one that came in that was kind of in the middle of the package, large. It looked down. It took 1,000 years to cool down. It basically trussed up the crust. It put an I beam through the crust.

Other bodies coming in had to basically -- they're influenced by this strongly. So the short answer is you get this material into the system. It basically puts I beam constructions in the system, and this is where magnum won't go again. It will go somewhere else. The material out there, the trough is much easier to propagate a dike in than whatever propagated in this stuff.

CHAIRMAN RYAN: So from a fluid flow standpoint, that first shot of magma into a system really creates a higher resistance to flow so that it has to find another path.

DR. MARSH: That's right. It would plug up the system.

CHAIRMAN RYAN: The second part of the question is, you know, people have suggested explosive kinds of events. How does that fit into your view of this?

DR. MARSH: Well, underground, when we're talking about underground, the first thing I might say is that a volcano is an attempt to cap a fountain of magma near the surface. We've all heard of these Red Adaire (phonetic) stories of capping run away oil wells that are on fire. They go in with a big weight and drop it on them. That's what volcanoes are actually.

They work up a mound and mound and mound until they cap themselves, and if they cool down and there's no more magma coming, that's it. It's over.

If there is a new charge, like in large volcanoes like in the Cascades or Hawaii or something like this, it has to come out again at the same point. It will reactivate, and this is where we get explosive eruptions because the magma that's in it gets near the critical crystallinity point.

So this is a major factor then in thinking about these systems. So how about underground? The magma is going up to the surface. Let's say it hasn't reached the surface yet for some strange reason. If you think about a dike oriented out there, a dike of any consequence, any length, it's going to venting in the valley before it vents anywhere, and that's where most or all of those things are going to bleed off immediately.

But let's say for argument's sake that it goes up through the mountain. It hasn't propagated anywhere else until then, and it hits the repository first. So what happens is it's going to start a volcano in the drift, and it's going to start with pyroplastic materials, tephra materials which are like popcorn sized, gravelly. It's going to build up in angle of repose. It's going to be coming into this thing. It's going to pile up this.

It's also hot. It tacks together. It forms a solid block, again. The early scenarios you heard some group saying that we could have a dike propagate up and we could have a shock wave going into it.

That's not going to happen. Shock waves are when you build up something. You have a membrane, you break the membrane, and you can actually have a shock wave.

All of the dikes that we see, as I showed you also, and even these bill sills start out as little, tiny cracks, and they go for a couple hundred meters. There's a little crack, opening stronger and stronger and stronger until it opens up. So it's a slight wedge opening up in this thing. So basically it would dissipate anything like that at all.

So what would you get? You would get a local little volcano build up in the five and a half or seven meter drift, and that would basically in this case where you didn't have any lava yet, you'd pile up this pile. The heat in there would weld this material together and plug up the opening, and the magma would certainly go around it and go somewhere else.

CHAIRMAN RYAN: Okay. Thanks, Bruce.

Allen.

VICE CHAIRMAN CROFF: Thanks.

To display my lack of knowledge in some of this area, what is the source of the water that is in these magmas and why does it vary so much among magmas?

DR. MARSH: There's water everywhere in the earth, strange as it may be, and the ultimate origin of some of this water is from probably the plate tectonic cycle where the ocean plate goes back down inside the earth and it carries hydrated minerals, minerals that have the hydroxyl radical in them.

And then once they get trapped inside the earth, it's in there and sometimes it's in there locked up in a mineral or if it goes to real high pressures, sometimes it is in there as some sort of defect or dislocation structure.

When any melting takes place, any volatiles that are in the mantle scream into the melt because the partitioning, partition cultures, enormously partitions this stuff into the melt. So it scavenges anything around.

So we call normal mantle like under Hawaii for very normal mantle material. Those things are very, very dry. They carry less than a quarter of a percent water. The ocean ridges in the world are also like that.

Alkalide basalts, which can come from quite a number of different kinds of sources depending on where they are in terms of old lithospheric material, et cetera, older earth that's not entirely devolatilized, any number of sources melting at high pressures can give you.

Now, these are not a huge amount of water. You can get one or two percent by weight. A hornblende crystal, a crystal of hornblende normal mineral that has a hydroxyl mineral, it has two percent water in it. So it's not as if there are huge amounts.

As we get the siliceous material, like the Pompeii type eruptions in some of these Mount St. Helen's eruptions, you get some silicic material. That material can contain a lot of water, and when you bring this up and you undergo the diver's bins, this material goes up and releases. It's like shaking up a bottle of Coca-Cola. You take the cap off and it really froths out, and that's exactly what happens in some of these real silicious things.

We don't have that here. We don't have

these. These are basalts. Basalts don't form ash flows.

Yucca Mountain itself is made of ash flows. That is one of these things. It flows out as a beer bottle froth at 1,000 degrees, collapses. Air goes out. It welds together in place, turns into a rock in place. So that's the fascinating aspect of that.

VICE CHAIRMAN CROFF: Thanks.

CHAIRMAN RYAN: Ruth.

MS. WEINER: Bruce, thank you for a great talk.

How much variation is there in the heterogeneity or homogeneity of magma around the earth, that is, the water content, the physical behavior, and so on?

DR. MARSH: Well, there are classes of magma, and they seem to hold together based on their tectonic locations, for example, island arc magmas, ocean ridge magmas, isolated hot spot magmas, et cetera.

This stuff that we're seeing here is in a class that we would call in the alkali basalts cinder cones isolated areas, and for a basalt, these are some of the more volatile rich. For a basalt, it may have one to three and a half percent. It's a little bit of you want to check more than one.

We have one analysis basically on this kind of thing, and it's a little dicey, a little bit, about how you estimate the volatile contents, but we do know that there are more volatiles in this stuff just because of the style of eruptions, for example, the big tephra piles and things like this that come out.

So it's a lot volatile driven, and of course, very deep in the earth CO_2, we get CO_2 around, and CO_2 is less soluble in magma than water. So it comes out more rapidly.

MS. WEINER: What I'm getting at is to what extent can you predict the behavior of one kind of magma from another kind of magma.

DR. MARSH: Yeah.

MS. WEINER: Provided the volatile content is similar.

DR. MARSH: Yeah. For example, cinder cones. You don't see many big tephra cinder cone sheet explosions in Hawaii. These are docile magmas, by and large, and that's because they have a low volatile content in general, and they're not. They're out in this trend. They have very low crystal entities. They're very hot. They're near 1,200 degrees, and they can flow a long way before they cool down to their critical crystallinity.

These things that we see out here because of their eruptive style and because of looking at the phase equilibria, they have more volatiles in them, and it reflects that.

However, instead of making and thinking of them as being more dangerous in the earth surface, it's more difficult for the magma to get out of the earth's surface because their temperature as they approach the earth's surface may be less than what it needs to be to actually be a lava flow on the surface, which is really an unappreciated fact a lot in modeling.

CHAIRMAN RYAN: The other question I have is how much pressure is exerted, would be exerted on a canister if some magma flowed around it and solidified and --

DR. MARSH: Very little. There are some contractions due to just the thermal cool-down, but not much pressure would be due except for the weight of the material on it.

The canister will heat up because there's air in it. It may actually rent. It may actually tear open a seam, but what would happen is you'd form a vesicle, a bubble or something nearby in the magma, and the magma would quench into that also.

I mean, you can't imagine a magma ever going in and dripping around or anything. So it would actually quench into the opening rapidly.

MS. WEINER: Thank you.

CHAIRMAN RYAN: Other questions? Latif.

DR. HAMDAN: Bruce, as you know, this is not only agreement as to what consequences can take place with the magma plus the drift. Can you based on your tremendous experience very briefly identify elements of the magma drift interactions that you think scientists can agree on, should agree on, likely to agreement, and elements of the interaction that they might not and require further confirmation maybe?

DR. MARSH: Well, I think that these kind of problems we're talking about are something that everyone can get a hold of and agree on. I think the things that you might want to think about a little bit is the angles which may be a propagating dike would hit the repository.

In other words, I show one five and a half meter, but we're looking at a field, a farm of these things out there, and whether it hits it at right angles, hits it here, hits it there, this is something that we can do a probabilistic risk estimate on based on the regional stresses and how much stuff is available, things like this, but I think it would be easy once people see this to get all on the same page and to come up through this same kind of level of experience on these things.

You know, as it stands, I think there are certain things we can rule out very strongly, and that is like thermal convection and things like this, but it is important, I think, for everyone to get on the same page in terms of the fluid that you're using to model with and what magma really is like.

Now, you hear people talk about how difficult it is to handle these problems, but they're actually not that difficult because you deal with it as a solid when it is immobile at 55 percent crystals or less or more, and beyond that you deal with a very viscous fluid with solids in it and things.

Now, what you see, often there's very nice modeling in DOE's reports and EPRI's reports and things. There are certain points, however, they get to when, in fact, they either do not use the results in the future or don't knit them together like this. Like I said, they use a fluid in the dike drift interaction report. they use a fluid that has a viscosity that increases with cooling, but no solidification effects, where it actually just becomes a solid snap through.

So these are little areas I think that could be smoothed up a lot with all of the researchers, and as I say, it's taking pieces of a cloth and knitting them together.

A lot of the stuff is there. It's just there are little enhancements that could be done.

DR. HAMDAN: So you do see the light at the end of the tunnel?

DR. MARSH: Oh, yeah.

DR. HAMDAN: Thank you.

DR. MARSH: Yeah, and it's not magma.

CHAIRMAN RYAN: Other questions? Yes, John Flack.

MR. FLACK: Yeah, Bruce. I'm thinking of the relationship between igneous activities and seismic events. Do you see this as a different kind of situation having an igneous event be preceded by a seismic event or do you believe the models could still accommodate that type of situation?

DR. MARSH: Well, there's an intimate relationship at some scales between seismicity in an event and magma. For example, there's hardly any magma that moves if it's in a well documented system, for example, in Iceland.

Magma often comes up in these big systems and propagates out, is distributed out as dikes that propagate horizontally. Some of these in Iceland you can actually watch them propagate down over a several day period, over 100 kilometers as they come down through.

And how do you know? You can see that seismic spreading down, and you can see the eruptions start happening out of these fissures. so you can see this happening. So that's one aspect of it.

In other words, when you're cracking open the earth, it's a seismic event. No other way around it. In active systems that are sitting there, in big systems, we get things like harmonic tremor and all kinds of unusual where the system seems to go into just an oscillation mode, for example, and these are now, we realize, we've coupled these together with this mush column system. These are open conduits where we basically get acoustic waves bouncing back and forth, and it resonates out of this thing, and there are certain styles of seismicity now that you can actually identify with these things that actually tell you that there's a reservoir of some limited extent perhaps, but there's a reservoir there, and it is told by this harmonic tremor.

So it's getting close and closer together, but it's precursors certainly in an event, even Mount St. Helen's, for example. The volcano started to enlarge. There was seismic activity. There was nothing on the surface until we started seeing over steepening, some steam and things like this.

MR. FLACK: So how it behaves will be certainly a function of the seismic activity that precedes the event.

DR. MARSH: Well, they go hand in hand.

MR. FLACK: Yeah.

CHAIRMAN RYAN: Any other questions?

Just identify yourself and tell us.

MR. APTED: Mick Apted at Monitor Scientific.

Bruce, I've seen proponents of the ideas of very low viscosity, basaltic magma traveling very far in these kind of intersected drifts, in a sense, I think, arguing against solidification as an important process.

But one of the things they point to as an analogue is this is like lava tubes and so on that they see in Hawaii. Maybe you could comment on what you think of those as appropriate analogues to this kind of situation that you've been describing.

DR. MARSH: Yeah, the key there, Mick, is the incredible difference in the Hawaiian system over what we see here, and it's a fundamental, and it comes down to this guy here a lot. If you don't appreciate this kind of diagram -- anyway, you can see it pretty much.

Here's the Hawaiian system. So anything that you can see on the surface there it's right at its liquidus. It's the most watery system, has a viscosity of about 50 poise, ten to the two perhaps. It is the most fluid stuff of all.

And if you don't appreciate this fact, and I don't think many people appreciate this, the fact is that this material is starting out down here. It's at or below its temperature. It's actually a dramatic region.

The trajectory of coming to the earth's surface, we could calculate that in more detail. All of the thermal properties are available now. It's a thermodynamic calculation even without heat losses. So that's the big factor.

If you actually do not understand the

difference between these two and realizing these systems, how different it is in its preeruptive state, in fact, that's a general statement I would make, is the initial conditions for the problems that are solved are very important in what you get for the outcome, of course.

And I would say if anything, for everybody to try to get the most realistic initial conditions and to make sure that they have those and actually worry a lot about their initial conditions before they do the modeling.

That's primarily what happens. The whole shock tube story, that was set up. The problem was done perfectly fine, but it was set up as an initial condition to generate a shock. You couldn't get to there with the magma that way.

So it's the initial conditions in these things, and that's -- like I say, these are subtle things, but absolutely critical in understanding how the system is going to work.

CHAIRMAN RYAN: There was one last question. We were heading a little bit past schedule.

Well, Bruce, thanks again for a real enlightening talk. We appreciate your insights.

Next up is Ms. Leah Spradley from

Vanderbilt University, a summer intern at NRC, and she's going to report on her project of modeling the volcanic ash plume.

Leah, welcome. We'll take a couple of minutes to let her get set up.

MS. SPRADLEY: Hopefully everyone has a hard copy, too, and they can follow along if you can't see very clearly.

My name is Leah Spradley, and I'm currently a Ph.D. candidate at Vanderbilt University, studying risk and reliability for an environmental management systems, and I'm enrolled in two different programs, VCEMS, the Vanderbilt Center for Environmental Management Studies, and also the Risk and Reliability Studies.

I'd like to take this opportunity to thank the ACNW and the NRC, in general, for granting me the opportunity to intern there this summer. I believe even though I had a short period of time there I learned a lot and met a lot of really good people. So thank you for that.

Today I'm going to be discussing how to use the HYSPLIT model to model the ash plume and dispersion for a potential igneous event at Yucca Mountain.

I'd like to acknowledge the following people. Excuse me. I have some animation on my slides. So I'm probably going to be standing here.

To give you some background, igneous activity has been identified as potentially significant to contributing to risk for Yucca Mountain, risk modeling, and especially the deposition at the RMEI location and also in the Fortymile Wash basin is of interest.

The HYSPLIT model has the potential to incorporate more atmospheric realism into the ash plume modeling that's currently being done.

To give you an idea of the event that we are trying to model here, this shows you the mean values of the parameters that we sampled, and this is the mean over about 1,000 different realizations. So you can see that we sampled the power and the duration and also the diameter mean size distribution for the ash particles, and from those you can calculate the height and emission rate and mass ejected.

My objectives for the summer were to explore the alternative ash model and then determine potential importance of the phenomena included in HYSPLIT that's not included in current models, such as wet deposition, and then compare these results to the current model.

To give you an overview of today's presentation, I'm going to discuss previous NRC models, the key differences between HYSPLIT and the current model called TEPHRA, my main simulation, and then a separate wet deposition simulation, and summarize the results.

So previous NRC models used an empirical plume model or semi-empirical plume model with the wind always blowing south towards the RMEI. So it was a constant direction.

And then current models include a redistribution of the ash, and they use a stratified wind field.

The HYSPLIT model is called hybrid single particle Lagrangian integrated trajectory model, sort of a mouthful, but it was developed by NOAA and the Air Research Laboratory there, and used at the Nevada test site to forecast airborne transport of potential plumes.

And it also makes use of the extensive meteorological resources, the RAMS data that is available.

To summarize the key differences between TEPHRA and HYSPLIT -- sorry. My animation is different -- first you have the data. HYSPLIT has hourly data available while TEPHRA has data based on 12-hour increments.

There are 24 elevation bins for HYSPLIT and ten elevation bins for TEPHRA.

The forecasting data is initialized from multiple weather stations in the HYSPLIT, and TEPHRA uses data from one weather center at the Desert Rock Airstrip.

HYSPLIT also incorporates precipitation data, whereas TEPHRA does not have any precipitation data.

The dispersion, the way the dispersion is calculated is also different. HYSPLIT does not assume a Gaussian plume, whereas TEPHRA does, and HYSPLIT uses three dimensional time dependent wind field, and TEPHRA only takes the wind field at the point of release.

HYSPLIT incorporates wet deposition, as I said earlier, as well as dry, and TEPHRA does not. And HYSPLIT uses discrete sizes for ash particle sizes and reports the depositions of all these sizes separately. TEPHRA uses a continuous size distribution, but only reports the total deposition.

So for my main simulation I used HYSPLIT as the transport model, and I tried to make the same assumptions that are used in the TEPHRA model based on the information that we had. I ran approximately 1,000 Monte Carlo realizations, and I randomly sampled the starting day and starting time of the igneous event within a year's window data that I had.

And then finally I calculated the deposition at the RMEI location and then all in the Fortymile Wash basin area.

This shows you the area of the Fortymile Wash basin that we used for HYSPLIT. All of these dots represent approximately 400, over 400 stations where I recorded the concentration after the event.

In TEPHRA, only this area between the black outline of the basin and 20 kilometers from the source was used as the capture window. Just to give you a reference point, this is Yucca Mountain, the center of Yucca Mountain, where the point source was located.

So the main idea of this slide is that we had a larger potential capture area for HYSPLIT.

I'm going to show you two measures of comparison. One is the ash mass deposited, and the other is the average surface concentration in the Fortymile Wash basin.

This slide shows you that the average mass deposited in TEPHRA was larger. You can see that for some of the runs here this is the PDF. It's a histogram of the mass versus the probability of that amount of mass being deposited for each run, and here you see the CDF of the mass.

So you can see that in some of the TEPHRA runs a much larger mass was deposited in the basin area, and you can see that with these graphs.

MR. HINZE: Leah, was that because of the size of the levitation of 20 kilometers?

MS. SPRADLEY: It's really too early to tell the exact reasons why a lot of these differences occurred. Like I said, I only had six weeks to perform these experiments, and I'll get to that in a couple of slides, potential reasons for these differences.

I wanted to point out, too, that these probability axes are different. So it's more fair to look at these graphs for comparison.

So, in summary, more mass was deposited using the TEPHRA model.

I apologize. My animation wasn't like this before.

So here's the second measure of comparison that shows the concentration in the Fortymile Wash basin. You can see that concentration of TEPHRA was smaller. I mean the concentration in HYSPLIT was smaller, and again, I want to point out the difference in these probability axes. It's more fair to compare the CDFs here.

You can see that the CDFs are fairly comparable in shape. It's just that the mean value using HYSPLIT was smaller.

MS. WEINER: Leah.

MS. SPRADLEY: Yes.

MS. WEINER: On that last slide it's concentrations in?

MS. SPRADLEY: Kilogram per kilometer squared. Sorry.

So in summary, the total mass deposited in HYSPLIT was found to be less than predicted by TEPHRA despite the fact that HYSPLIT had that larger potential capture area that we are looking at.

However, the differences are not fully understood. Like I said, the inputs to the model, the power and the duration of the event that we sampled were as similar as we could make them, depending on the information that we had at the time available to us.

Also, the conceptual models could have been different. there could have been simplifications in either model that were not fully understood.

This shows you the concentration of ash at the RMEI location. So the slides we were looking at before were the concentration comparisons in the Fortymile Wash basin. Now, out of all of the runs, approximately, 1,000 for each, the frequency of deposition at the RMEI location was comparable for both. About 30 percent of the time you found deposition at the RMEI location.

Here you can see that the mean for HYSPLIT was slightly larger, but they're pretty much the same. You can see that the HYSPLIT showed some large outliers, and there was more variance near zero deposition for TEPHRA, and that has to do with the way that the deposition is calculated for TEPHRA.

The next three slides I'm going to show you the behavior of the relative ash sizes, where they fell in comparison to the source, and like I said before, HYSPLIT models deterministic sizes of ashes. So it has binned in two different sizes.

And there were seven different sizes we used. They range anywhere from the mean of .02 microns to about 3,000 microns.

This slide shows you the contours of ash deposition by size for the Fortymile Wash basin, and the main point of this slide is that you can see the first four ash sizes that are the smallest ash sizes behaved very similarly -- I apologize if it's hard to see on the printed handout because it's not in color -- but these graphs all look very similar, and as you get to larger sizes, you see that the difference in behavior grows.

This last plot is a plot of the total sum of all seven ash sizes, and this shows you the behavior of ask sizes for where they fell or which ash sizes were more frequent, frequently fell at the RMEI. You can see that the ash size six, which has a mean diameter of approximately 500 microns, was the most frequent to fall at the RMEI location.

I also did an experiment finding out the effects of wet deposition on the results, and for this experiment I found days with abnormally high rainfall and then I fixed the power and the duration and the mean diameter for all of the runs, and I just varied the start day and the start time so that it would start correspondingly with those days of abnormally high rainfall.

And then I ran the HYSPLIT model with and without wet deposition and compared the results.

So here you can see that wet deposition affected the smaller ash sizes much more than it affected the larger ash sizes. The horizontal axis here is percent decrease in concentration when the wet deposition is turned off, and for the smaller ash sizes it changed the concentration almost 100 percent, and that's a result of there being no concentration in certain locations, and then the wet deposition causing concentration to be in those locations.

So, again, it caused a larger effect on the smaller ash sizes, and this is apparent in these contour plots as well. This is one day, February 3rd, that had a high amount of rainfall, and this is another day that had a high amount of rainfall.

Here is the source, and you can see with wet deposition, a lot of the smaller ash sizes were brought down closer to the source, and without wet deposition the wind carried these smaller sizes farther away. And you can see the same thing on February 21st. This is the year 2004, by the way.

So to summarize, wet deposition appears to cause a significant difference, especially for the smaller sizes, but given that Yucca Mountain is relatively dry, we don't think that this will lead to a significant or it will be a significant contributor to risk.

So in summary, the HYSPLIT model has potential for more realistic forecasting because it uses this three dimensional time dependent data. It relies less of empiricism for the dispersion calculations, and it can simulate the impacts of wet deposition.

However, there are a lot of uncertainties still, and HYSPLIT like most other plume models does not take into account volcanic momentum entrainment or buoyancy, which can be very important in calculating dispersion.

Also, the behavior of the plume models is generally oversimplified.

And finally, volcanoes can have a significant effect on the ambient meteorology, and that's not currently included in the model.

So to continue this research, I think it's important to incorporate the radionuclides into the ash, and also modify the existing simulation environment by coupling a vertical column source with the model instead of using just a single point source.

And increasing the number of realizations in the Monte Carlo simulation. Also determining if there are systematic differences in the HYSPLIT and TEPHRA models.

Are there any questions?

CHAIRMAN RYAN: Thank you.

Ruth, do you want to start?

MS. WEINER: Thank you, Leah. That was very good.

And having played with the HYSPLIT model myself, I can appreciate your problems with all of the inputs.

On your Slide 9, which is the one with all the colors --

MS. SPRADLEY: The contour slide.

MS. WEINER: The contour slide. I'll wait until you get it up.

MS. SPRADLEY: This one.

MS. WEINER: Okay. Was this a predominant wind direction? What was the wind pattern for these contours?

MS. SPRADLEY: Okay. It's important to keep in mind this shape is the shape of the stations at which I recorded the concentration on every run. So I have virtually no information about the concentrations in this white space. So it may seem that the wind is blowing north here. As you can see, if you remember the wet versus without wet deposition slide, sometimes the wind would cause the plume to go in both directions from the source.

So you can't tell. Even though this is averaged over all of the realizations, you can't tell if it was causing the deposition to form here and here as well as up here.

MS. WEINER: Yes. That's a very good explanation. When you first see that slide, it looks like the wind.

MS. SPRADLEY: It looks like the wind is always going north or on average going north. That would be more to add to the future research, to increase the number of stations and get more of a realistic wind rose.

MS. WEINER: How close in to the source do you get on a HYSPLIT model?

MS. SPRADLEY: Well, if you go back to the slide where I show you where I'm recording all of the concentrations, here, this one. Here's the sources.

MS. WEINER: Yeah. I can't tell the size of your grid from here.

MS. SPRADLEY: Oh, okay, okay. Well, here is the source. So we are getting very close to the source in all directions, but we don't get very far from the source in this direction.

Does that answer your question?

MS. WEINER: What kind of distance is "very close"?

MS. SPRADLEY: Well, this gives you a reference for distance. This circle is a 20 mile-kilometer radius away from the sources. So here's 20 kilometers away from the sources.

MS. WEINER: So close in is a kilometer or so?

MS. SPRADLEY: Yes. The stations here that are farthest away from the source in this direction are only a couple of kilometers at most.

MS. WEINER: Is there any difference between how close to the source you can get with HYSPLIT and how close you can get with TEPHRA? Do you know?

MS. SPRADLEY: I'd have to defer that question to somebody that is more experienced with TEPHRA.

MS. WEINER: Yes, it was just a curiosity question.

MS. SPRADLEY: I think Dick might be able to answer that question.

MR. CODELL: I'm Dick Codell from NRC.

Essentially with the HYSPLIT model you can get right on top of the source, but it doesn't really mean very much. We're more interested in the deposition over the whole basin for subsequent models, and at the RMEI location, which is 18 kilometers away from the event, we're only interested at that point, and so we're not really using any more information even though theoretically you could calculate it.

The TEPHRA model, from what I understand of it, you can do essentially the same thing. It's problematic though because these are just models that are looking at ambient transport of ash and tephra from a vent, and as you get very close to the vent, of course, the conditions toward your model assumptions don't apply anymore because you have the momentum and buoyancy and everything else that's going on very close to the vent.

MS. WEINER: Okay. Thanks.

Thank you, Leah.

CHAIRMAN RYAN: Allen, any questions?

VICE CHAIRMAN CROFF: No, thank you.

CHAIRMAN RYAN: Leah, just let me catch one on the way by here. When you look at your future work assumptions, one thought struck me, and I'd like your thoughts. You said incorporate radionuclides into the ash.

You know, thinking down the road when you want to calculate a dose, I guess, are you thinking about different distribution of models? For example, biometric incorporation independent of particle size, some sort of a biased model where you're looking at radioactive material associate with smaller sizes or bigger sizes or you're looking at a range of how you'll make that distribution of the radioactive material into the ash?

MS. SPRADLEY: For a short answer, I'd say more of a range. Right now I believe they're just using a fraction of the ash that has radioactive material in it. I think there are a number of different options that can be done for incorporating the radionuclides into the ash, and I'll be discussing those options with Dick Codell and others as far as how to move forward.

CHAIRMAN RYAN: That's kind of critical because that will drive your restorable fraction. If you get more radioactivity or less in there by one model or another, that can be a big driver of estimated dose. So that's kind of a key one to me.

That was really my only question. Thanks.

MS. SPRADLEY: Thank you.

CHAIRMAN RYAN: Nice job.

Bill Hinze.

MR. HINZE: A brief question. What's NOAA's experience with this code? Have they validated it, such as people have attempted to do with TEPHRA and Saranegro (phonetic)?

MS. SPRADLEY: Yes. I have all of the HYSPLIT documentation with me, and there are a number of examples that they've used to validate the code.

We actually have a representative from ARL in the audience. I don't know if he has anything to add.

MR. HINZE: Well, one of the things I'd be interested in is this being validated not only with respect to the total thickness, but also in terms of the size, distribution.

MS. SPRADLEY: Well, I'd be happy to show you the examples of the validation in the documentation that I brought along after the presentation.

MR. HINZE: Okay.

MR. SCHALK: I'm Walt Schalk from NOAA Air Resources Lab.

It was developed in Washington by Roland Draxler. He's kind of the guru on the whole thing, and the model has been around for quite some time and validated against numerous real world events, tracer studies, in the Gulf War, the Chernobyl event, and things like that for its transport and diffusion, and the build in to use the model wind fields as Leah was using with the RAMS model.

It has also been recently incorporated into the NOAA responsibility that they do ash proof forecasting for the whole United States. It was another code, but it's within the last two years been moved over into that capability.

So it has a wide breadth, and it has been used for quite some time by NOAA, probably at least ten years.

MR. HINZE: May I ask have you considered flocculation as part of the concern with respect to the distribution of the size of the particles?

MR. SCHALK: No, I don't believe that's included in the model.

MR. HINZE: Is that a factor in the wet case? Does flocculation -- is that part of the process of the wet condition or is this just simply the particles being caught up in the raindrops?

MR. SCHALK: I believe it's the particles getting caught in the raindrops and being washed out basically.

MR. HINZE: Thank you.

CHAIRMAN RYAN: Jim.

DR. CLARKE: Nice job, Leah. No questions.

MS. SPRADLEY: Thank you.

CHAIRMAN RYAN: Any other questions?

DR. LARKINS: Just a comments on Dr. Hinze's question. There's been a lot of experiments that have been done, both dry and wet, to measure the different modes or methods of agglomeration from the amount of moisture in the system.

MR. HINZE: Where is that material?

DR. LARKINS: I can get you some references.

MR. HINZE: Okay, great.

CHAIRMAN RYAN: Yes, Ashok Thadani.

MR. THADANI: Let me answer what John said. There's also been considerable work in other countries, and in particular in Russia, in terms of accidents with high energies and different aerosol sizes carrying certain radionuclides. You might want to take a look at that.

CHAIRMAN RYAN: Okay. Thank you.

Again, I think all of those comments sort of summarize into realism for what particles have what radioactive material and how they persist in the respirable range over time. That's a mouthful, but that's certainly what we're reaching for in all of these thoughts, I think.

But thanks, again, for a great presentation.

MS. SPRADLEY: Thank you.

CHAIRMAN RYAN: And for being with us today.

With that, we are at our scheduled break for 3:15 to 3:30. We're about on target. Well, actually we're ahead of schedule.

MR. COLEMAN: I have an announcement before anyone leaves. This is the first time we've used this facility, and we do apologize for the difficulty in seeing a lot of the graphics. We came up with this system to do a little better job of it, and because also we've had trouble getting as many handouts as we would like to have for you, I've placed a sign-up sheet in the back on the left, and we will provide CDs after the meeting with the presentation materials that were shown here because even some of the handouts are very difficult to read because of the resolution.

So please sign up if you want to get those CDs.

CHAIRMAN RYAN: Thank you, Neil.

We'll take a 15 minute break. I now have five minutes of three. So we'll start again at ten minutes after three.

Thank you.

(Whereupon, the foregoing matter went off the record at 2:54 p.m. and went back on the record at 3:18 p.m.)

CHAIRMAN RYAN: We'll go back on the record at this point and take up the next item on our agenda, which is a short report from the ACNW subcommittee report on the August 2005 visit to the Savannah River site and the Barnwell low level waste disposal site.

And Allen, why don't you lead us off on the Savannah River portion?

VICE CHAIRMAN CROFF: Thanks.

A group of three ACNW members visited the SRS and chem nuclear sites on August 10 and 11 of this year. We were accompanied by some ACNW staff members and one member of the public.

I'll try and summarize the highlights of what we learned at Savannah River, and then Mike is going to talk a little bit about the Barnwell site.

We toured the SRS facilities relevant to waste determination primarily and the mixed oxide fuel fabrication plant that's proposed down there, as well as the chem nuclear sites relevant to low level waste processing and disposal.

First, regarding the mixed oxide site, and I'll make this real brief, our interest in this was the waste handling from the plant, whether there might be problems with it backing up or being received because at one point there was a plan to transfer it to the Savannah River site per se away from the licensed mixed oxide facility for management.

What it appears down there is that is no longer the plan. They might still resurrect that, but this point they seem to be geared up to handle their own waste internally, which takes lot of that off the table, I think.

Moving on to the waste determination business, I'll just try to hit what I think are a few highlights here. First, it's not clear at this point how many waste determinations will be developed by DOE, which is another way of saying it's not clear how DOE will bundle the things that require a waste determination.

For example, will they submit a waste determination for one tank at a time, two, three, ten, 50? It's just not clear, and that sort of relates back to the potential work load and the potential number of issues that might come up.

The hints we got from them down there sort of indicate that we will probably bundle together fewer rather than more, on the theory that if they put ten or 15 together, if anyone had problems that would compromise the whole determination.

But I think that will be an ongoing deliberation, but that is the trend.

Some of the things that I think are important to think about is that more than tanks and the salt stone are the immobilized low level waste may eventually require a waste determination. Included in this are piping, facilities and equipment that generated the tank waste, such as some of the equipment in the old canning facilities, and facilities and equipment that have processed the tank waste, such as the DWPF, the vitrification facility, and some of the evaporators that they routinely use in managing the tank waste.

Savannah River at this point seems to have longer range plans for removal of key radionuclides such as cesium, strontium, and the transuranics to a fairly substantial extent. They're building a new facility for this purpose that's supposed to come on line in 2010, plus or minus I'd say at this point.

However, in the near term, due to limitations in capacity for waste storage in their tanks -- and this is limitations for storage in compliant tanks, meaning those that have double containment -- they are pursuing some interim processing of some of the waste that will result in greater concentrations of radionuclides going into the salt stone facility, in the low active waste stream.

And there has been some discussion there, and I expect an increase in interest in that particular topic as we go forward.

Class C limits continue to be important at Savannah River site. This sort of relates to our deliberations on low level waste that we'll see in the future. Such limits are self-imposed limits by DOE on what they can dispose of at the site. It's part of some of their compliance agreements with the state and in the new criteria they use for waste determinations being greater than Class C needs to the need for another plan on which the Nuclear Regulatory Commission must consult.

At this point they haven't proposed any greater than Class C, and it's not clear at all what this plan would be, but that's the way it stands at this point.

Retrieval to date from the tanks has been quite good. They've retrieved a number of tanks, and they've gotten the residual layer thickness down to very low levels, and most of them you can see bare spots in the bottom of the tank.

However, these retrievals to date are focused on what I'll call uncomplicated tanks, no internals and no other difficulties evident. As they move forward a substantial fraction of their tanks can be best viewed as having a forest, a verticle cooling coils inside that tend to be coated with the waste and make it very difficult for the retrieval equipment to maneuver. So we'll have to see how well they do on that, and it's something to think about.

Finally, I guess regarding monitoring, my sense in coming away is they see the need for it. Clearly, they're going to do it. What they're planning regarding monitoring just isn't really all that far along at this point. They just really haven't gotten serious about laying plans down about how they're going to do it and how far they're going to go in it.

MR. HINZE: Excuse me. Is that monitoring around the tanks then?

VICE CHAIRMAN CROFF: Yes, yes. Post closure monitoring, I guess, to be clear about it.

With that, I guess that's my side. Do you want to do your barnwell part?

CHAIRMAN RYAN: Sure, yeah. that's great. Thanks, Allen.

I think one last comment on the monitoring part of it. There's a pretty extensive environmental monitoring network, and unlike other sites they have a pretty good access to all of the history of monitoring. So at least they've got a basis, which I think they can move forward, but I agree with Allen. They haven't really developed that.

The second day of our trip we visited the chem nuclear low level waste disposal facility in Barnwell County, South Carolina. It was first licensed in 1969, with disposal commencing in '71. The land that's currently licensed, the 235 acres, was established by lease amendment in 1976. The decommissioning trust funds that are in place and used for decommissioning were established in '81, and then of course, the South Carolina history of being in a compact and out of a compact and back in another compact has all had an impact on the operation.

Their peak year of volume was about two and a half million cubic feet of low level waste in 1980, and currently they're receiving in the range of 35,000 cubic feet of low level waste. Most of the Class A waste is shifted to Envirocare, and Enviro now focuses no BNC waste, although they are licensed to take all three classes of waste.

Barnwell currently is in a compact with Connecticut and New Jersey, where out of compact generators will not be permitted at the current wave of thinking to take waste from outside the compact after 2008.

There is some, over a million, maybe even a couple of million cubic feet of disposal capacity and license that still remains. So there will be unused capacity at that juncture of 2008 that's fairly substantial.

The radioactive disposed has been, of course in the millions of curies. Two-thirds of their inventory is Cobalt 60, and then it falls off from there in terms of percentage by radionuclide. Most of the radioactivity is relatively short lived.

They have a pretty extensive environmental monitoring program and environmental modeling program with 240 groundwater monitoring wells on and off site a lot of indisposal cell standpipe monitoring for infiltration water and the like, and they've completed capping on seven or eight of the old disposal cell areas with the permanent multi-layered cap to shed essentially all of the surface water that hits the site so they can keep the disposal cells dry.

We had a thorough tour of the site, the laboratory facilities and other activities on the site. We also were afforded the chance to visit with the county council members, Barnwell city leaders and other members of the business development community, and so forth, and were pleased to learn that the community holds the company in high regard and, in fact, several times during our meeting said, you know, "Do whatever you can do to help keep the site open and in place here in Barnwell County because we think it's an asset to the community." They felt very strongly that it was an important contributor and a business that they understood and felt comfortable about.

And they concluded it's safe and needed, and they wanted to keep the facility open and running in their community.

So with that we finished that day's tour and traveled on from there. So that's the low level waste part.

I want to also add that Latif Hamdan was quite expert at getting our arrangements made for our visits, with our contacts, and he's developing a detailed trip report that will put all of the materials together and we'll have a detailed trip report for all of the members there.

Thank you, Latif, for all of your efforts. There were a lot of folks, a lot of moving parts, and a lot of places to go. So we appreciate your effort.

DR. HAMDAN: Thank you.

CHAIRMAN RYAN: You're welcome.

With that --

VICE CHAIRMAN CROFF: I wanted to add one thing on the Barnwell. In the discussions with the chem nuclear staff, I guess by way of preamble the site has two identifiable institutional control funds to watch the site after it's closed. One is held by a third party trustee and the other was held by the state.

And some years ago the state found itself a little bit short of change and took a fairly substantial amount of the fund that it held -- I think it was in the low hundred million and change, and they took 80 or 90 percent of it to help balance the budget.

Now, they are now on a course to reinstate that, but I think the message for the committee and for other sites decommissioning low level waste disposal is the structure of these institutional controls and the way they're protected is probably an important thing to keep in mind. It's just not enough to have a bucket of money someplace. It has to be shielded.

So there was I think a case in point here.

CHAIRMAN RYAN: Yeah, again, Allen, I appreciate you reminding me of that. That's a very important aspect.

The closure fund was untouched. That's the one that's using monies to cap as time goes along and as the site evolves. It was the long term care fund that Governor Hodges, who was in office at that time, moved all but $5 million of it, and it was more like $140 million, to the general fund.

The current governor has pledged a $25 million payback for the schedule to return the monies that were borrowed from the fund, and I agree with your comment.

The thought was that it was untouchable, but clearly that wasn't the case. So that's an important point to think about.

So thanks.

Any other comments or questions from members?

Well, with that report, we'll have a full trip report package that Latif will prepare and we'll be happy to answer any other questions at a future meeting.

Thanks.

With that item completed, our next task is to consider the continuation of our discussion of possible letters. We had left off with Allen going to discuss some of the major points from the working group. This is not the reading of the letter. This is Allen's summary of the information so that we can hear his views on major points and discuss those.

(Whereupon, the foregoing matter went off the record at 3:32 p.m. and went back on the record at 4:34 p.m.)

CHAIRMAN RYAN: We rearranged our schedule a bit this afternoon to leave an opportunity before we go off the record and take a break into our public meeting this evening. So if there are any folks who wish to make a comment to the committee at this time.

(No response.)

CHAIRMAN RYAN: All caught up.

Again, we appreciate your participation and will you be back this evening or no?

(Discussion was held away from the microphone.)

CHAIRMAN RYAN: Well, we'll be happy to have you even if it's a small group. We appreciate your participation today and your comments, as always.

With that if there's no other business for the open session and the on-the-record part of the meeting, we'll adjourn.

Any last items?

(No response.)

CHAIRMAN RYAN: We stand adjourned and the record is closed.

(Whereupon, the foregoing matter went off the record at 4:35 p.m. and went back on the record at 6:05 p.m.)

EVENING SESSION

(6:05 p.m.)

CHAIRMAN RYAN: All right. I'd like to call our evening session into order if I may.

This is the evening session of the ACNW, and we're here again this evening from a busy day today to receive a comment from members of the public that want to be with us.

I was explaining to one of our guests that we've had several folks who have participated during the meeting today, and we've afforded them enough opportunities to offer their comments during the day. They had satisfied their needs to do so. So we're on the way.

Dr. Larkins.

DR. LARKINS: Good evening. My name is John Larkins. I serve as the Executive Director of the Advisory Committee on Nuclear Waste (speaking from an unmicked location).

CHAIRMAN RYAN: You might need to flip it on and just hold it in front.

DR. LARKINS: Now I'll have to start all over again.

As I was saying, the NRC, one of its strategic goals is openness, and here we try to make the processes and the decision making of the agency transparent to the public. I think one of the vital roles that the Advisory Committee on Nuclear Waste plays is making some of the processes and the decision making of the Commission, particularly in the area of waste disposal and high level waste, transparent to the public.

I've been coming out to Las Vegas now for probably the last 12 years, and prior to that I had an opportunity to come out -- well, I served as a technical assistant for Chairman Lando Zech during the mid-'80s. I had several opportunities to come out and meet with representatives of the state and the governor and others and talk about the role of the NRC and waste management matters.

So I've been coming out here for the last, well, what is it? Seventeen and five, 22, 22 years on and off, and always manage to enjoy myself while I'm here.

As part of this outreach goal, we're having this public session this evening to provide an opportunity, a forum for anyone who wants to come in and make comments to go on the record. The ACNW uses those comments to formulate any advice or comments it wants to send to the Commission on how it might enhance its interactions with the public and also to highlight any issues that the public may want to raise during these outreach sessions.

What I'd like to do, first of all, is introduce the members of the ACNW. First is our Chairman, Dr. Michael T. Ryan. Mike has been on the ACNW now for three years. It will be four years this summer.

And to his left is Allen Crans -- Croff. Sorry about that, Allen. Vice Chairman. Allen joined the committee, I think, about a year, a year and a half ago, a little bit over a year. Allen has worked for Oak Ridge National Lab for a number of years.

I forgot to mention that Dr. Ryan has been in the waste management or waste disposal business, I guess, for 20?

CHAIRMAN RYAN: Twenty-five years.

DR. LARKINS: Twenty-five years. Brings a lot of experience to the business.

To the left of Allen Croff is Dr. Ruth Weiner. Dr. Weiner joined the committee what, three? Two and a half years, approximately two and a half years. Dr. Weiner is currently -- well, it says here retired. I thought you were still work at San --

MS. WEINER: I'm still working.

DR. LARKINS: Okay. It's a good thing I didn't read the script.

Ruth is working at Sandia National Labs. She's our resident expert on transportation issues and has been doing a lot of things in the area of risk analysis while at Sandia, and she also teaches at University of Michigan.

Okay. To my immediate right is Dr. Bill Hinze, William J. Hinze, Professor Emeritus at Purdue University, and Bill was our resident earth science expert. He handles everything from seismology to volcanology to a little bit of everything, hydrology included.

Bill formerly was on the committee for eight years and only recently came back to the ACNW this last year.

And to his immediate right is Dr. Jim Clarke, who is a full professor at Vanderbilt University and principally in the area of environmental analysis; is that right?

DR. CLARKE: That's correct.

DR. LARKINS: Good. I got the script correct.

And the rest of the people here are staff for the ACNW.

Let me just quickly mention the mission of the ACNW. It's up on the Board. It says to provide the NRC independent and timely technical advice on nuclear materials and waste management issues; to support the NRC in conducting an efficient and effective regulatory program that enables the nation to use nuclear materials in a safe manner for civilian purposes.

And the next viewgraph or chart tells how we accomplish our mission, and I won't go through all of the bullets, but basically the committee collects information through various forums, either meetings, workshops, and hears comments both from the NRC staff, licensees, applicants, industry, and others, and then reaches conclusions and provides technical advice to the Commission on this.

This is basically how the committee accomplishes its mission, and these things are done in the public, and in a generally very collegial manner.

Maybe I should turn this part over to Dr. Ryan, starting on page 5, the purpose of tonight's meeting.

Anyway, thank you.

CHAIRMAN RYAN: Thanks, Dr. Larkins.

The purpose of tonight's meeting is to

listen and consider comments from the public on matters related to the committee's activities and to support the committee in providing insights to the Commission on public comments and concerns.

Another purpose is to obtain information to support the advice to the Commission and opportunities to enhance involvement of stakeholders in the licensing and prelicensing activities.

Tonight's meeting is scheduled for two hours, and we've had one speaker arrive already. Other speakers will be invited to sign in, and then will be provided the opportunity to make statements to the committee.

And of course, we'd ask that we identify each of these folks so that we can create a thorough and complete record of the input that we receive tonight.

As I mentioned earlier, we did have two individuals, one from the State of Nevada and another citizen representing a citizens group who participated throughout the day with us and were afforded several opportunities to make comment, and they both indicated they had satisfied their needs during the day and didn't need to come back this evening.

We found that to be effective because it's helpful to get their comments at the time a particular topic is being discussed, and it made it more meaningful for them and also more insightful for us to hear it more as a timely dialogue rather than a comment made at the end of a long day.

The current ACNW activities include top priority activities, including of course the proposed Yucca Mountain repository and issues related to that; the risk informing approach that the NRC takes to its regulatory activities; decommissioning of nuclear facilities; health physics or radiation protection; and waste determination specific to materials that are at DOE facilities for which NRC will make statutorily required waste determination.

We also have a second tier of priority, including waste management research issues that are conducted by the Center for Nuclear Waste Research Analysis in San Antonio, Texas; radioactive materials transportation; low level radioactive waste, and fuel cycle facilities.

Specifically on Yucca Mountain our current involvement of the committee includes our continuing to interact with DOE and NRC staff during the pre-licensing phase; visits to the Center for Nuclear Waste Regulatory Analysis on discussion of volcanism issues, in particular; review of the DOE waste transportation activities.

We are following developments in the preclosure design and safety analysis. We're reviewing draft revised NRC Yucca Mountain regulations that are being developed under 10 CFR Part 63. We have observed workshops on the probabilistic volcanic hazard assessment work that's going on for the Yucca Mountain site in its vicinity, and we have provided or plan to provide advice to the Commission on some or all of these topics. That's our current work activities and work plan.

I think on the screen you'll see two Web sites. We certainly have paper copies of these handouts for those who wish to carry them away where you can download our letters to the Commission, our meeting agendas, our transcripts, our action plan, our charter, and member information that we reviewed briefly with you tonight.

Also, on a separate Web site is our most recent report and briefing to the Commission, which occurs approximately every six months or so. We might have two face-to-face reports to the Commission each year.

So those materials are available.

As all of the ACNW meetings, we conduct all of our meetings, including our letter writing sessions, in the public. We operate under the FACA rules for open public meetings. All of our information is gathered and discussed in public, and we appreciate this opportunity to have members of the community in Nevada and Las Vegas and Yucca Mountain area and Nevada as a whole to come and speak with us this evening. So we appreciate everybody's participation as we go through.

With that, it's your turn to speak.

Our first speaker, I believe, will be Mr. Mike Henderson, who works for the Office of Congressman Jim Givens who is from the Second District of Nevada, and without further ado, Mr. Henderson, please join us.

MR. HENDERSON: Thank you, Mr. Chairman, Mr. Larkins, Mr. Vice Chairman.

Mr. Vice Chairman, I have the advantage of having hospitality of Oak Ridge several years ago for a ten-day course called Nuclear Power and the Energy Crisis. It seems to me things have evolved only slightly since then.

On behalf of the Congressman, welcome to Las Vegas once more. The following is his statement.

Thank you, Chairman Michael T. Ryan, and Vice Chairman, Allen G. Croff, for allowing me the opportunity to submit these comments for the record.

I apologize for being unable to attend this hearing in person. However, I am currently in Washington, D.C., representing this great State of Nevada.

The Yucca Mountain project has been an issue that has always been of the utmost concern to me and to too many of my constituents. I represent every county in Nevada, including my county, which includes the Yucca Mountain Waste Repository.

While it should come as no surprise that the entire Nevada delegation is in strong opposition to Yucca Mountain, as an independent body, it is your mission to report and to advise the Nuclear Regulatory Commission on all aspects of nuclear waste management.

This includes objective analysis regarding the feasibility of the Yucca Mountain project as a deep geologic repository. It is extremely disturbing to see that since the birth of this project the Department of Energy has consistently failed to use science as its guide and has instead been blinded by its obsession to do anything and everything to rubber stamp this project so that it can be finished.

While this might be acceptable to the bureaucrats of the DOE, more than 2,400 miles away from here, it is completely unacceptable to the people throughout Nevada and this country.

When this project fails, and it is only a matter of time, who will be held accountable with the reality of a deadliest substance known to man contaminating our water supply, traveling our roads, and endangering our communities?

Last year the Federal Appeals Court ordered that the federal government needed to develop a plan for nuclear waste storage that protected the public against radiation releases beyond the proposed 10,000 years. As a result of the court's decision, the EPA needed to promulgate a new safety standard that can show compliance well beyond 10,000 years.

Many experts and scientists argued that the EPA could not realistically develop a plan that could insure public safety past 10,000 years. Unfortunately, many underestimated the extreme measures the proponents of this project would take to insure that the scientifically flawed project continues.

Instead of playing by the rules of the game, rules intended to protect public safety, the DOE and the EPA decided to simply change the game. In its most shockingly disturbing ruling yet, the EPA decided that it was scientifically reasonable to increase its radiation standard after 10,000 years from 15 millirems to 350 millirems. This means that the EPA has determined that once the clock hits 10,000 years in one day, it is completely reasonable for the radiation exposure to increase 23-fold.

I and my fellow Nevadans ardently disagree. The EPA has an obligation to protect public safety today, tomorrow, and in a million years. It should not speculate that a standard which is not deemed safe today could miraculously become a state standard in the future.

This decision was not based on any measure of public safety and instead just continues to highlight the means the DOE will go to in order to insure that the Yucca Mountain project continues.

As an independent Commission, you must closely review and scrutinize this illogical decision and show the DOE and EPA that just because you don't like the rules you cannot change the game.

In the next few days many of you will return to your homes thousands of miles away from Nevada, but for many of us here in this room, Nevada is our home. Nevadans are the ones who have to live here and be exposed to the deadly risk of the DOE's culture of ignoring science in favor of expediency in regard to this project.

And I remind you that we still have no plan for transporting this deadly waste through our communities for thousands of miles.

The safety of the American people along the transportation routes is in jeopardy due to this moving hazard that too easily could be a moving target. It is our hope that when you fully examine this project you fulfill your obligations as an independent Commission and ignore the pressures to rubber stamp this project.

It is our hope that you will see the flaws and the risks associated with opening Yucca Mountain and transporting high level nuclear waste. It is our hope that you will protect the people of Nevada and of this great nation.

I think you for your time today, and I respectfully request that these comments be introduced into the record.

Jim Givens, member of Congress, 2nd District, Nevada.

At this point I will be happy to entertain questions. I'm somewhat familiar with the Congressman's views on this issue. If I do not have the answers, I'll be happy to get them for you.

CHAIRMAN RYAN: Any questions?

I think not. Mike, I appreciate your coming here. Mr. Henderson, thank you for reading the statement into the record. We have the hard copy, and we have a transcript of it. So we appreciate your being with us tonight.

MR. HENDERSON: Thank you, Mr. Chairman.

CHAIRMAN RYAN: You're welcome to stay or depart as your pleasure takes you.

MR. HENDERSON: Thank you, sir.

CHAIRMAN RYAN: Thank you very much.

MR. HENDERSON: Thank you all.

CHAIRMAN RYAN: Any other commenters or questions?

(No response.)

CHAIRMAN RYAN: I guess we'll see if other folks arrive. So why don't we just kind of suspend the record for a moment, and when we have other presenters or speakers we'll reconvene.

Thank you.

(Whereupon, the foregoing matter went off the record at 6:24 p.m.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Nuclear Waste 163rd Meeting

Docket Number: n/a

Location: Las Vegas, NV

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

William Click
Official Reporter
Neal R. Gross & Co., Inc.

ADVISORY COMMITTEE ON NUCLEAR WASTE

Public Meeting

September 21, 2005

Michael Ryan, Chairman, ACNW

John Larkins, Executive Director, ACNW Staff

ACNW MISSION

- Provide the Nuclear Regulatory Commission independent and timely technical advice on nuclear materials and waste management issues to support the NRC in conducting an efficient and effective regulatory program that enables the Nation to use nuclear materials in a safe manner for civilian purposes

How Do We Accomplish Our Mission?

- We meet with and obtain guidance from the Commission on technical subjects on which they would like us to focus
- We use the Commission's guidance to develop an action plan to guide our activities
- We review licensee, applicant, industry, and NRC staff documents
- We hold public meetings to discuss technical issues and help us determine appropriate observations and recommendations on subjects under review

How Do We Accomplish Our Mission? (Continued)

- We listen to stakeholder input
- We take advantage of extensive Committee expertise and, where appropriate, expert consultants
- We collegially develop letters to the Commission and/or NRC staff to communicate our views and provide independent advice
- We review responses to our letters to determine whether the NRC staff is appropriately addressing our recommendations

Purposes of Meeting

- To listen to and consider comments from the public on matters related to the Committee's activities
- To support the Committee in providing insight to the Commission on public comments and concerns
- To obtain information to support advice to the Commission on opportunities to enhance involvement of stakeholders in licensing and prelicensing activities

Tonight's Meeting

- Scheduled for two hours
- Please sign in if you wish to speak (separate list from meeting attendance list)
- To allow all who wish to speak the opportunity to do so, please make your comments concise (5 minutes or less per speaker)
- Please identify yourself and use the provided microphone

Current ACNW Activities (Top Priority)

- Proposed Yucca Mountain Repository
- Risk-informing Regulatory Activities
- Decommissioning of Nuclear Facilities
- Health Physics
- Waste Determinations

Current ACNW Activities (Second Priority)

- Waste Management Research Review
- Radioactive Materials Transportation
- Low-Level Radioactive Waste
- Fuel Cycle Facilities

Current ACNW Involvement in Yucca Mountain Activities

- Continuing to interact with DOE and NRC staff during prelicensing phase
- Visited Center for Nuclear Waste Regulatory Analyses and discussed volcanism issues
- Reviewed DOE waste transportation activities
- Following developments in preclosure design and safety analysis
- Reviewing draft revised NRC Yucca Mountain Regulations (10 CFR Part 63)
- Observed workshops on probabilistic volcanic hazard assessment
- Have provided or plan to provide advice to Commission on some or all of the above subject areas

ACNW ON THE WEB

- You can download our letters, meeting agendas, transcripts, action plan, charter, and member information by visiting:

  http://www.nrc.gov/what-we-do/regulatory/advisory/ACNW.html

- A webcast of our most recent Commission briefing can be viewed at:

  http://video.nrc.gov:8383/nrc_webcast/archive.jsp

Public Comment Opportunity

- Your turn to speak!

VANDERBILT

Using HYSPLIT to Model Ash Plume Dispersion and Deposition for a Potential Igneous Event at Yucca Mountain

Leah Spradley
NRC Summer Intern 2005
163rd ACNW Meeting, Las Vegas
September 20-21, 2005

VCEMS
Vanderbilt Center for Environmental Management Systems
Risk and Reliability Studies

Acknowledgments

- Richard Codell
  - NMSS NRC
- Darryl Randerson
  - NOAA ARL Las Vegas
- Walter Schalk
  - NOAA ARL Las Vegas
- Roland Draxler
  - NOAA ARL Silver Springs

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2

Background

- Volcanic activity is potentially significant to risk.
- Deposition of ash at the RMEI location and in Fortymile Wash basin could be important in determining risk.
- HYSPLIT model has potential to incorporate more atmospheric realism in ash plume modeling.

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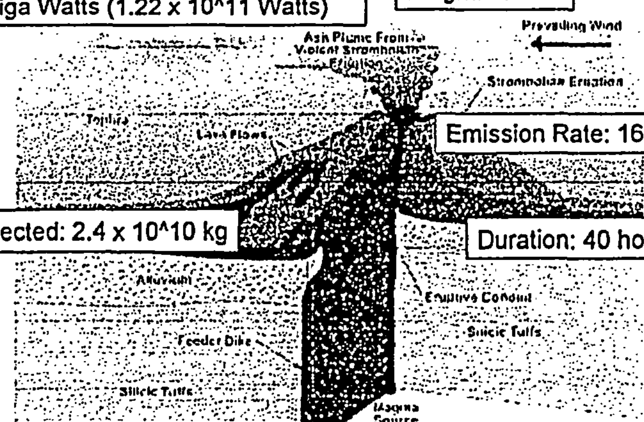
Mean Parameter Values of Event

Power: 122 Giga Watts (1.22 x 10^11 Watts)

Height: 4.8 km



Emission Rate: 166,000 (kg/s)

Mass Ejected: 2.4 x 10^10 kg

Duration: 40 hours

Diameter for Mean of Size Distribution: 1000 microns

Figure 3-1. General Conceptual Model of Basaltic Volcanism in the Yucca Mountain Region

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Objectives

- Explore alternative model for ash deposition.
- Determine potential importance of phenomena not included in current model (i.e., wet deposition).
- Compare results to current model and explain differences.

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Overview

- NRC Volcanic Plume Models
- Key Differences Between HYSPLIT and TEPHRA
- Main Simulation
- Wet Deposition Simulation
- Summary

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History of Volcanic Plume Models

- Previous NRC models used empirical plume model, with wind *always blowing south* toward the RMEI in lieu of redistribution by wind and water.
- NRC is evaluating new atmospheric transport model with stratified meteorology (TEPHRA) and fluvial/eolian redistribution model (ASHREMOB).

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HYSPLIT MODEL

Hybrid **S**ingle **P**article **L**agrangian **I**ntegrated **T**rajectory **M**odel

- Developed by NOAA (Air Research Laboratory).
- Used at Nevada Test Site (NTS) to forecast airborne transport of potential contaminant releases.
- Used at Nevada Test Site (NTS) and NOAA for forecasts of potential plumes.
- Makes use of extensive meteorological resources and atmospheric modeling studies (Regional Atmospheric Modeling System) available for NTS.

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Summary of Key Differences

- HYSPLIT
  - Data
    - Hourly
    - 24 elevation bins
    - Multiple stations
    - Precipitation Data
  - Dispersion Calculation
    - Does not assume Gaussian plume
    - Uses 3-D time-dependent wind field
  - Deposition
    - Dry and Wet
  - Size
    - Uses discrete particle size bins and reports all 7 depositions
- TEPHRA
  - Data
    - 2x per day
    - 10 elevation bins
    - 1 weather center at Desert Rock Airstrip
    - No Precipitation Data
  - Dispersion Calculation
    - Assumes Gaussian plume
    - Only takes wind field at point of release
  - Deposition
    - Dry
  - Size
    - Uses continuous size distribution, but only the total deposition is reported

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9

Main Simulation

- Use HYSPLIT for ash transport.
- Make same assumptions as TEPHRA model for volcanic plume variables (e.g., power, duration, mean ash size).
- Approximately 1000 Monte Carlo realizations, sampling volcanic properties, and starting time within one-year data window (March 2003-March 2004).
- Calculate ash deposition at RMEI Location and in Fortymile Wash basin.
- Compare HYSPLIT and TEPHRA model results.

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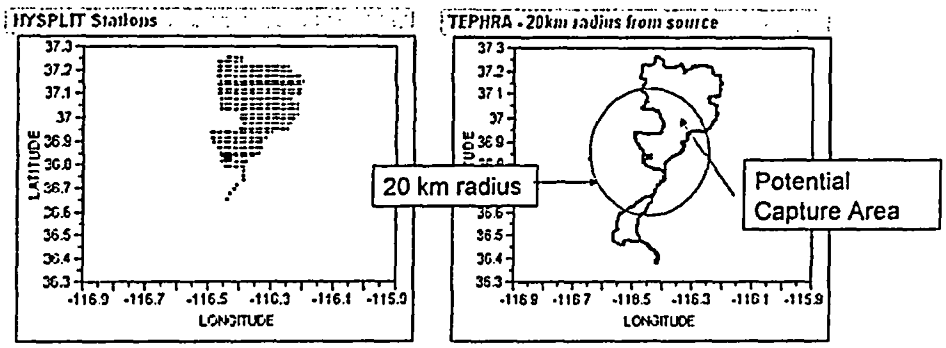
10

Data Sampling for HYSPLIT



Potential Capture Area of HYSPLIT runs is LARGER!

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Measures of Comparison

- Total Ash Mass Deposited
- Ash Surface Concentration

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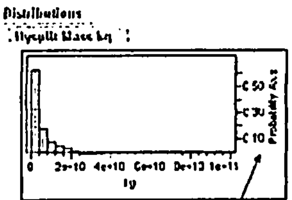
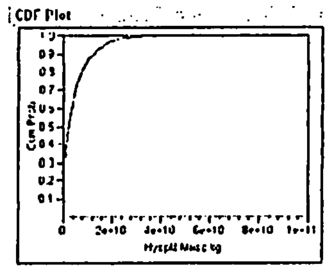
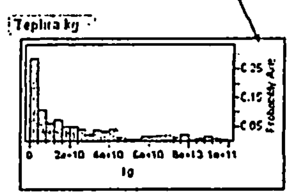
Total Mass Comparison (kg)

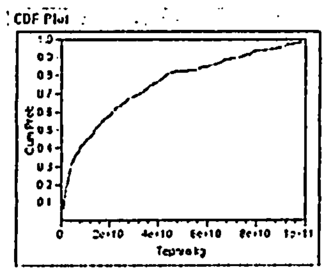
- HYSPLIT
  - Mean: 4.66e9 kg
  - Median: 2.44e9
  - Std. Dev: 5.29e9





- TEPHRA
  - Mean: 2.50e10 kg
  - Median: 1.4e10
  - Std Dev: 2.76e10





TEPHRA: More mass was deposited in smaller potential capture area.

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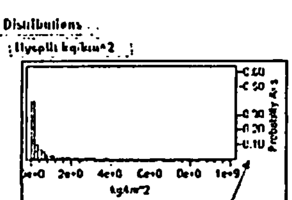
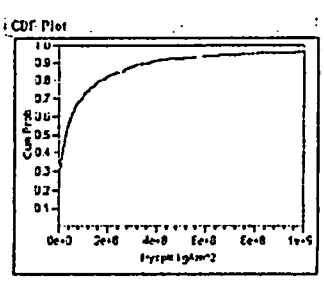
Concentration Comparison

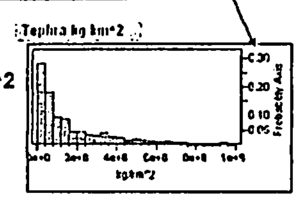
- HYSPLIT
  - Mean: 1.6e8 kg/km^2
  - Median: 3.0e7
  - Std. Dev: 4.65e8

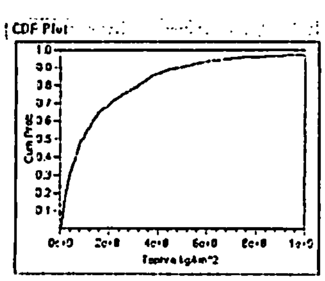




HYSPLIT resulted in Smaller Mean Concentration.

- TEPHRA
  - Mean: 2.0e8 kg/km^2
  - Median: 9.0e7
  - Std Dev: 3.16e8





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HYSPLIT Comparison Summary

- The *total mass* deposited in the HYSPLIT simulation was found to be *less than* predicted by TEPHRA despite the fact that HYSPLIT included a *larger potential ash capture area.*
- ***However*** - differences are **not fully understood**
  - Inputs to model the same?
  - Conceptual models the same?
  - Simplifications in the model?

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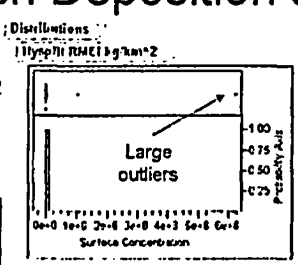
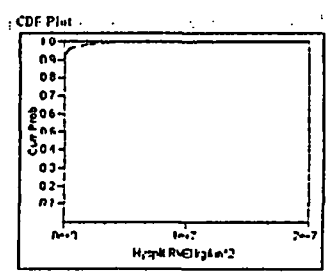
15

Initial Ash Deposition at the RMEI

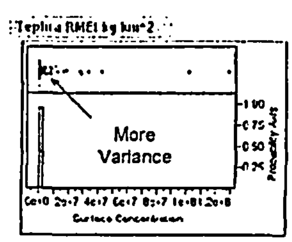
- HYSPLIT
  - Mean: 8.2e5 kg/km^2
  - Median: 0
  - Std. Dev: 2.0e7

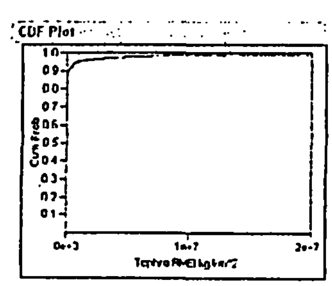




Frequency of significant deposition at RMEI was roughly 30% for both models.

- TEPHRA
  - Mean: 5.8e5 kg/km^2
  - Median: 13.9
  - Std. Dev: 5.7e6





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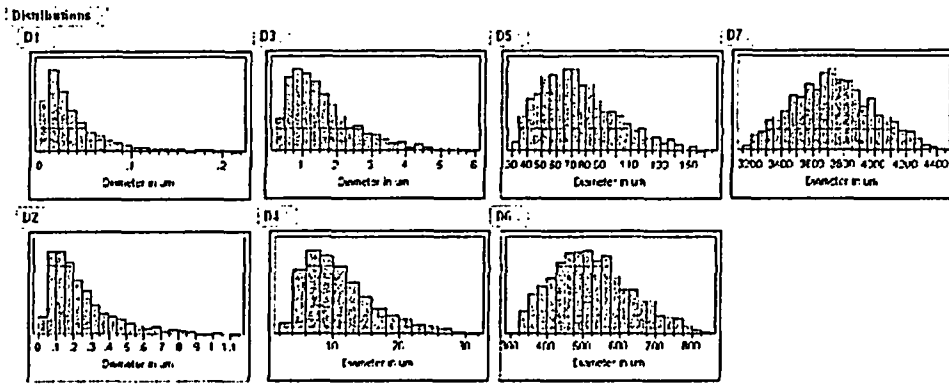
Ash Diameter Distribution Simulation Results



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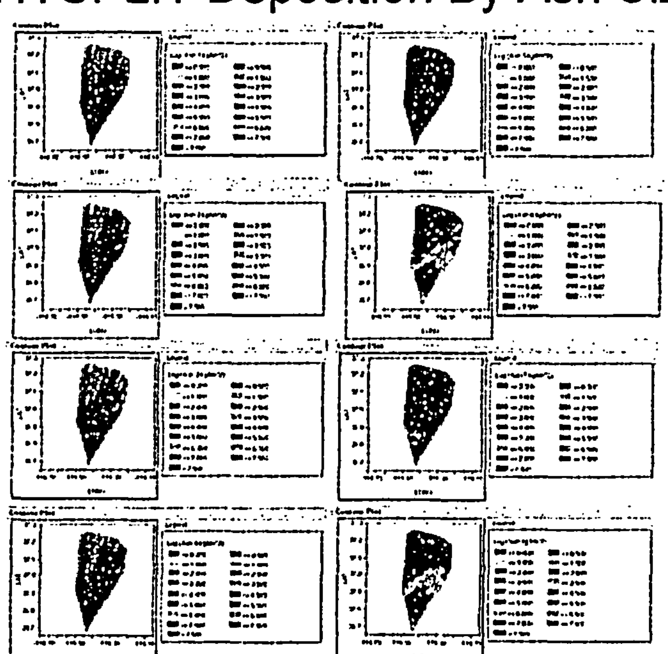
HYSPLIT Deposition By Ash Size



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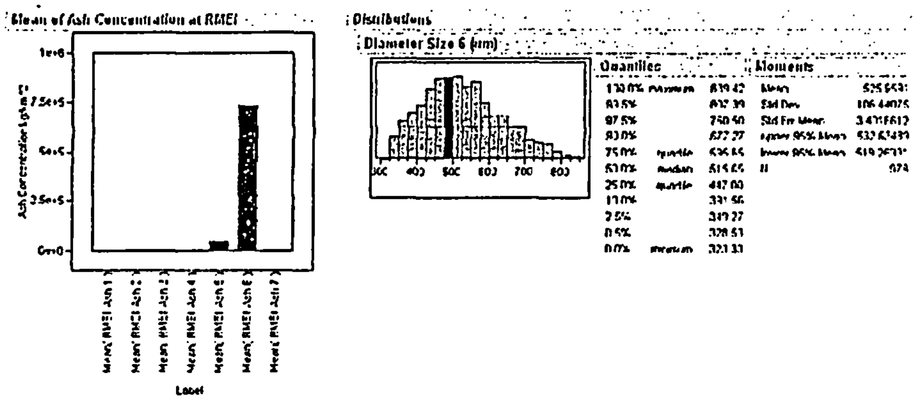
RMEI Concentration by Ash Size



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Effects of Precipitation (Wet Deposition)

- Find days with abnormally high rainfall.
- Fix power, duration, and mean diameter.
- Vary start day and start time to correspond with rainfall activity.
- Run the HYSPLIT model with and without wet deposition.

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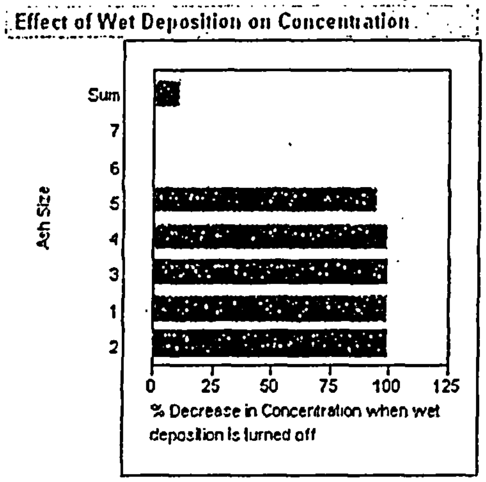
Mean Difference Caused by Wet Deposition



- Wet Deposition did not cause a change for the larger ash sizes (size 6 and 7).
- Caused a ***large*** effect on smaller ash sizes.

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Effects of Wet Deposition

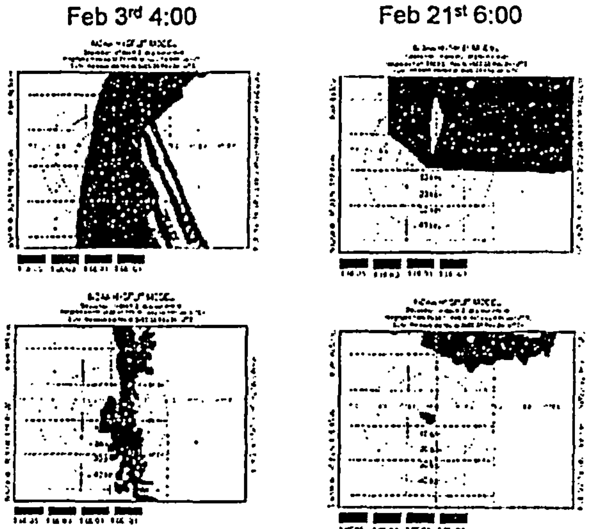
Feb 3rd 4:00 | Feb 21st 6:00

With Wet Deposition

Smaller ash sizes are deposited *closer* to the source.

Without Wet Deposition

Smaller ash sizes are carried *farther* from the source.



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Summary of Wet Deposition Impacts

Wet deposition appears
to cause a significant difference,
especially for ***smaller*** ash sizes.

However, given that the site is relatively **dry**, conditions that lead to wet deposition are **rare**. Therefore, wet deposition is *not* likely to be a significant contributor to risk.

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23

Summary

HYSPLIT model has potential for more realistic forecasting:

- Can use extensive 3D, time-dependent meteorological data
- Relies less on empiricism for atmospheric dispersion
- Can simulate the impacts of wet deposition

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24

Uncertainties

- HYSPLIT and most other volcanic plume models do not simulate regions where volcanic momentum, entrainment and buoyancy are important.
- Behavior of tephra in plume models is generally oversimplified.
- Volcanos may have a significant effect on the ambient meteorology that is not included in the model.

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25

Future Research

- Incorporate radionuclides into the ash
- Modify existing simulation environment
  - Vertical column source vs. point source
  - Increase the number of realizations
- Determine if there are *systematic differences* in the conceptual models used in HYSPLIT and TEPHRA

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26

Backup Slides

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27

Sampling Distributions

- Power,10^9 watts
  - LOGUNIFORM 9.9 500
- Duration, hours
  - LOGUNIFORM 24.00 71.94
- Mean diameter of ash, microns
  - LOGTRIANGULAR 100.0 1000.0 10000.0
- Starting day and time were chosen randomly within the data range

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28

Dispersion Calculations

- Vertical Diffusivity Profile
- Wind Shear
- Horizontal Deformation of Wind Field

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29

Deposition

- Dry Deposition
  - Gravitational settling velocity computed using the particle diameter and density
- Wet Depletion
  - Defined by a scavenging ratio *(l/l)* within the cloud and by and explicit scavenging coefficient (s^{-1}) for pollutants below the cloud base

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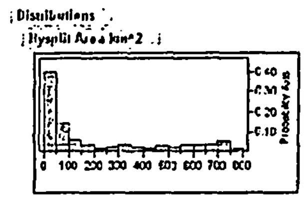
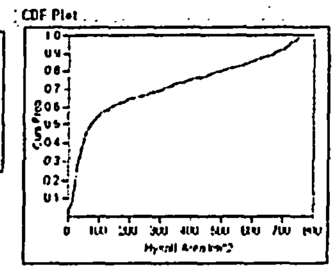
30

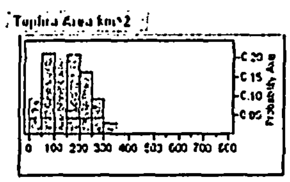
Deposition Area Comparison

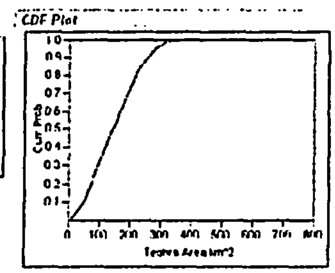
- HYSPLIT
  - Mean: 217.18 km^2
  - Median: 73.33
  - Std. Dev: 248.61





- TEPHRA
  - Mean: 148.99 km^2
  - Median: 146.00
  - Std. Dev: 77.56





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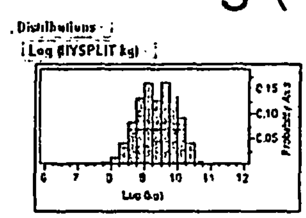
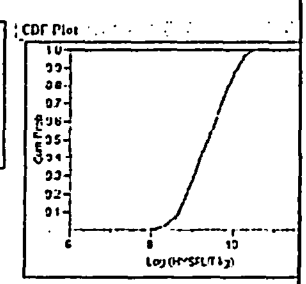
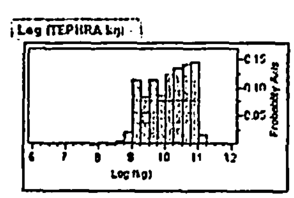
Total Mass Log (kg)

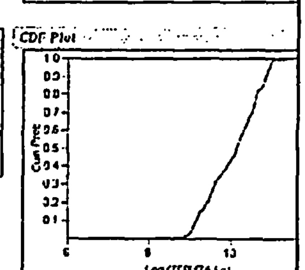
- HYSPLIT
  - mean is 9.36 log (kg)
  - median is 9.39 log (kg)





- TEPHRA
  - mean is 10.05 log (kg)
  - median is 10.15 log (kg)





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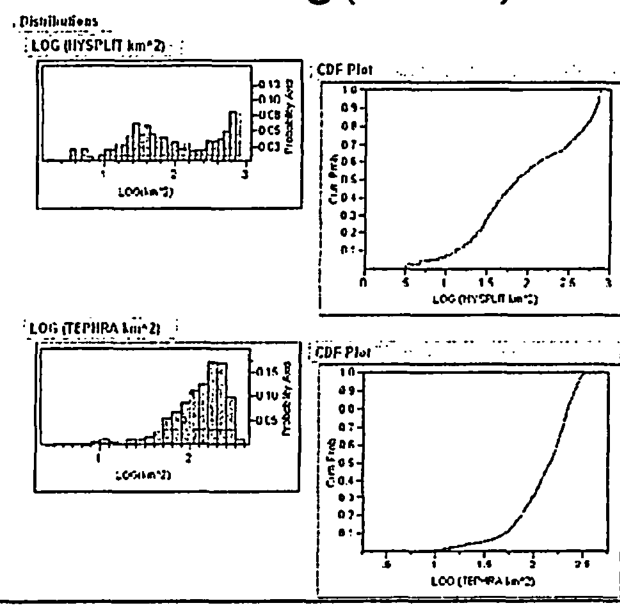
Total Area Log(km^2)



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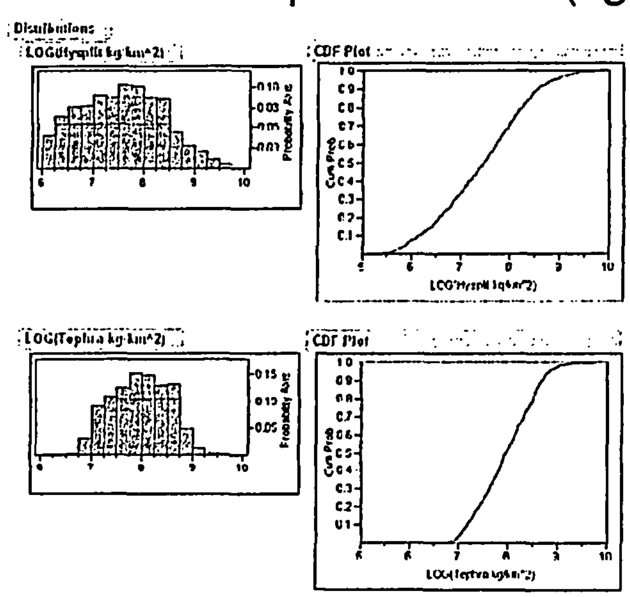
Concentration Comparison LOG(kg/km^2)



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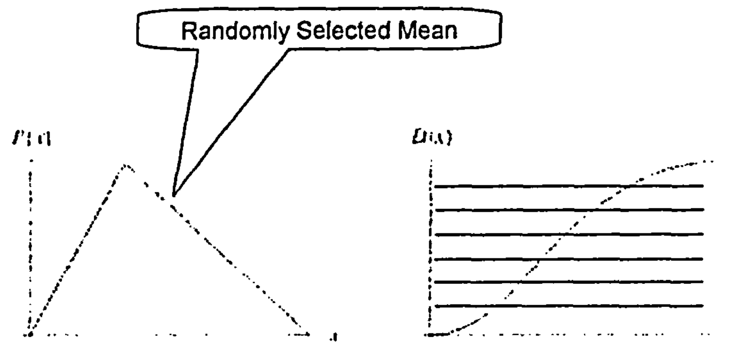
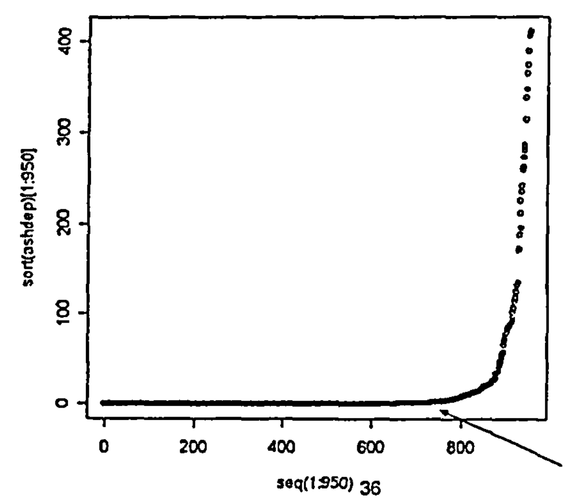
Ash Diameter Distribution



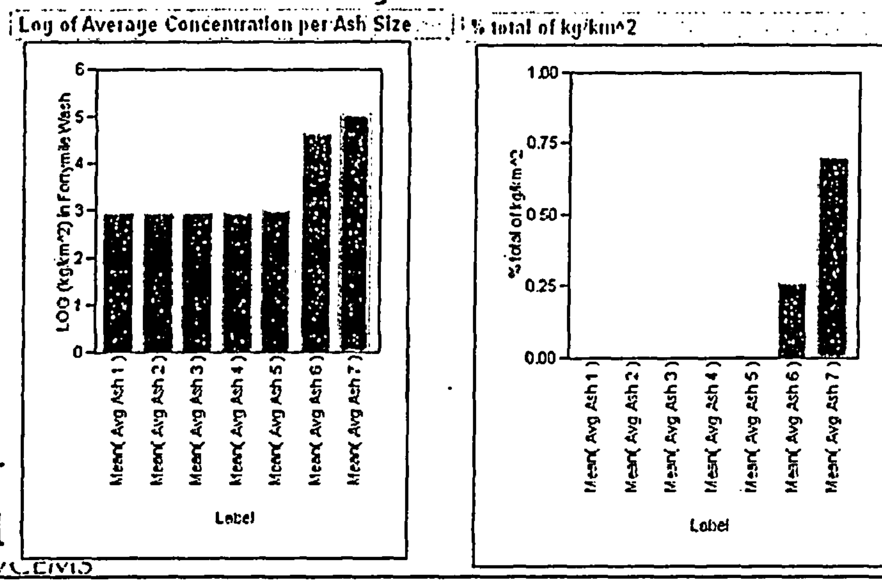
35

Deposition at RMEI in TEPHRA

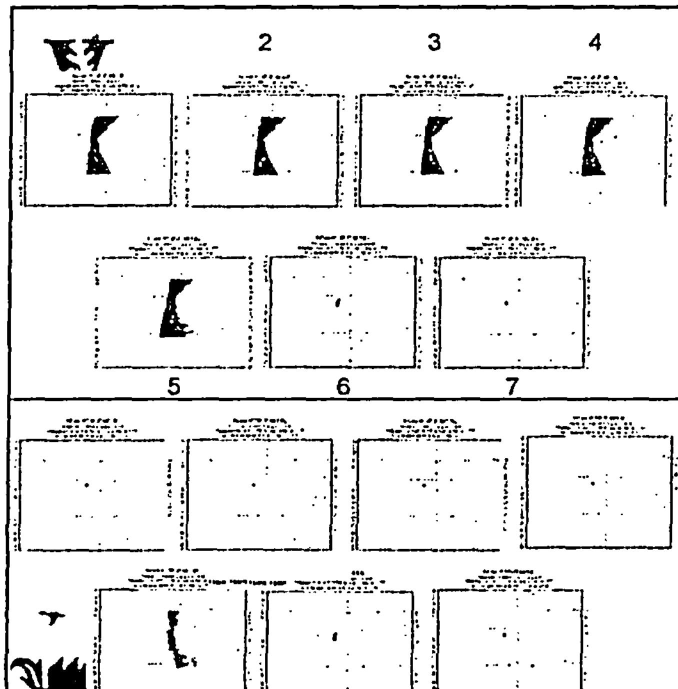


36

Fortymile Wash Concentration by Ash Size



Feb 3rd Release by Ash Size

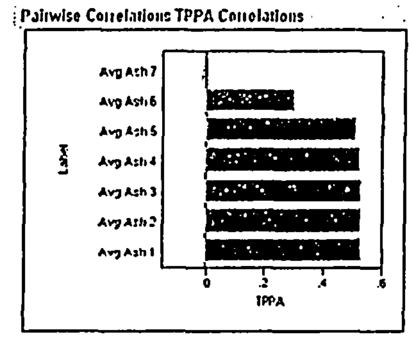


With Wet Deposition

Without Wet Deposition

Total Accumulated Precipitation (TPPA) Correlations

- Precipitation has a greater effect on the smaller sizes than the larger sizes.



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39

JIM GIBBONS
2ND DISTRICT, NEVADA

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Congressman Jim Gibbons
Statement Regarding Yucca Mountain Project
NRC Advisory Committee on Nuclear Waste
September 21, 2005

Thank you Chairman Michael T. Ryan and Vice Chairman Allen G. Croff for allowing me the opportunity to submit these comments for the record. I apologize for being unable to attend this hearing in person; however, I am currently in Washington DC representing this great state of Nevada.

The Yucca Mountain Project has been an issue that has always been of the utmost concern to me and to too many of my constituents. I represent every county in Nevada, including Nye County which includes the Yucca Mountain waste repository.

While it should come as no surprise that the entire Nevada delegation is in strong opposition to Yucca Mountain, as an independent body it is your mission to report to and advise the Nuclear Regulatory Commission (NRC) on all aspects of nuclear waste management. This includes objective analysis regarding the feasibility of the Yucca Mountain Project as a deep geologic repository.

It is extremely disturbing to see that since the birth of this project, the Department of Energy (DOE) has consistently failed to use science as its guide and has instead been blinded by its obsession to do anything and everything to rubber stamp this project so it can be finished. While this might be acceptable to the bureaucrats at the DOE more than 2,400 miles away from here, it is completely unacceptable to the people throughout Nevada and this country. When this project fails, and it is only a matter of time, who will be held accountable with the reality of the deadliest substance known to man contaminating our water supply, traveling our roads, and endangering our communities?

Last year the federal appeals court ordered that the federal government needed to develop a plan for nuclear waste storage that protected the public against radiation releases beyond the proposed 10,000 years. As a result of the court's decision, the EPA needed to promulgate a new safety standard that can show compliance well beyond 10,000 years. Many experts and scientists argued that the EPA could not realistically develop a plan that could ensure public safety past

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10,000 years. Unfortunately, many underestimated the extreme measures the proponents of this project would take to ensure that this scientifically flawed project continues. Instead of playing by the rules of the game, rules intended to protect public safety, the DOE and the EPA decided to simply change the game. In its most shockingly disturbing ruling yet, the EPA decided that it was scientifically reasonable to increase its radiation standard after 10,000 years from 15 millirem to 350 millirem. This means that the EPA has determined that once the clock hits 10,000 years and one day, it is completely reasonable for the radiation exposure to increase 23-fold. I and my fellow Nevadans ardently disagree.

The EPA has an obligation to protect public safety today, tomorrow, and in a million years. It should not speculate that a standard which is not deemed safe today could miraculously become a safe standard in the future. This decision was not based on any measure of public safety and instead just continues to highlight the means the DOE will go to in order to ensure that the Yucca Mountain Project continues. As an independent commission, you must closely review and scrutinize this illogical decision, and show the DOE and EPA that just because you don't like the rules you cannot change the game.

In the next few days many of you will return to your homes thousands of miles away from Nevada, but for many of us here in this room, Nevada is our home. Nevadans are the ones who have to live here and be exposed to the deadly risks of the DOE's culture of ignoring science in favor of expediency in regard to this project. And I remind you that we still have no plan for transporting this deadly waste through our communities for thousands of miles. The safety of the American people along the transportation routes is in jeopardy due to this moving hazard that too easily could be a moving target. It is our hope that when you fully examine this project, you fulfill your obligations as an independent commission and ignore the pressures to rubber stamp this project. It is our hope that you will see the flaws and the risks associated with opening Yucca Mountain and transporting high-level nuclear waste. It is our hope that you will protect the people of Nevada and of this great nation.

I thank you for you time today, and I respectfully request that these comments be introduced into the record.

Jim Gibbons

Jim Gibbons
Member of Congress
Second District
Nevada