UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

Summer Board Meeting
Exploration and Testing Activities
Past and Future Climates and Hydrology at Yucca Mountain

Red Lion Hotel
Denver, Colorado
July 10, 1996

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Mr. John W. Arendt
Dr. Garry D. Brewer, Round-Table Moderator
Dr. Jared L. Cohon
Dr. Edward J. Cording
Dr. Donald Langmuir
Dr. John McKetta
Dr. Jeffrey J. Wong

CONSULTANTS

Dr. Patrick Domenico, Session Chair
Dr. Ellis D. Verink
Dr. Richard Grundy
Dr. Richard Parizek

SENIOR PROFESSIONAL STAFF

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

NWTRB STAFF

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Michael Carrol, Director of Administration
Ms. Nancy Derr, Director of Publications
Ms. Paula Alford, Director of External Affairs
Mr. Frank Randall, Assistant, External Affairs
Ms. Karyn Severson, Congressional Liaison
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

PAGE NO.

P R O C E E D I N G S

DR. DOMENICO: Good morning. Can we take our seats, please?

Welcome to the second day of the summer meeting of the Nuclear Waste Technical Review Board, and we're going to start off today with Ike Winograd, with his presentation on the paleoclimate, particularly, his work at Devils Hole in the Amargosa Desert, and the implications for Yucca Mountain.

Rick Forester of USGS will follow with a description and analysis of work being conducted by the project on paleoclimate.

We will then make a switch to paleohydrology, particularly, the isotope studies in the ESF. Stan Davis will start that section with his presentation on that issue, followed by Zell Peterman and Jim Paces of the USGS, who will update us on the isotope studies of calcite and opal fracture coatings in the ESF.

June Fabryka-Martin of Los Alamos National Lab will then present the latest results of interpretations of chlorine-36 studies she's conducting in the ESF.

After lunch, we will launch into a discussion of future climate modeling. Tom Wigley will offer us his perspective on the uses and limitations of climate modeling, followed by Starley Thompson of the National Center for

Atmospheric Research, who will update us on modeling studies, the studies of future climate at Yucca Mountain.

We will end the session with a presentation by Mike Wilson of Sandia National Lab, who will help us understand the significance of all this information with respect to repository performance, and a wrap-up by Sheryl Morris of the DOE.

Following that last presentation, we will have a round-table discussion devoted to climate and hydrology, but you will hear more about that later from Garry Brewer, who will serve as a moderator.

As usual, at the end of the day, there will be time for questions and comments from the audience, so, with that, I'll turn it over to Ike.

MR. WINOGRAD: Good morning.

Being the leadoff speaker in this morning's paleoclimate session, I want to take a few minutes to introduce the Panel to this relatively young field of endeavor. Although earth scientists have pondered the causation of the ice ages for nearly 150 years, such studies have grown exponentially in the past twenty or so years.

When I first started working in this field, about a dozen years ago, one of the leading journals, Paleoceanography, did not exist, and two other leading journals in this field were less than a decade old. Today, paleoclimatology is recognized as a major branch of earth science.

Because of the explosive growth of activity in this field, data is pouring in, and major surprises have appeared in a period of a few years. To illustrate the dynamic nature of this field, I begin by citing several major new findings that have come to light just in the past four years.

Many of you will recognize this plot as the SPECMAP marine oxygen 18 fluctuations in global ice volume during the past 600,000 years. For those of you who are not familiar with this plot, the major peaks on this time series represent interglaciations, and the deepest troughs, glaciations, with approximately 100,000 years separating each cycle. Let's look at a blowup of the last 200,000 years of this ice-volume record.

For the past 40 years, the picture we have had from this time series and from its predecessors has been of a relatively rapid deglaciation which occurred within 10,000 years, followed by a slow build-up of ice over tens of thousands of years, culminating in the full glacial climates about 18-20,000 years ago.

However, just four years ago, we learned that the actual picture for the past 80,000 years is considerably different. Major shifts in temperature, and possibly, also, in ice volume occurred between 80,000 and 10,000 years ago.

In this slide, we show the oxygen 18 from one of the two now famous ice cores obtained from Summit, Greenland in the early 1990s. The oxygen 18 in this time series is a proxy for temperature.

The thing to note throughout the period 80-10 ka is that the fluctuations in oxygen 18; that is, in temperature, are equal in magnitude to two-thirds of the eventual change that occurred between full glacial and the Holocene values.

Similar shifts have since been looked for and found in high resolution marine records from the mid- to high latitudes of the Atlantic Ocean. The smooth buildup in ice volume indicated by SPECMAP, the marine oxygen 18 standard that you saw in the previous slide, gave no indication of these rapid shifts in climate.

Major New Finding No. 2. Work by Kurt Cuffey and colleagues, published earlier this year in Science, has shown that the full glacial to interglacial temperature shift in central Greenland was 16° C, or twice the previous estimate, an estimate that went back about twenty years.

Major New Finding No. 3. The monumental CLIMAP Project study of oceanic temperatures during the last glacial maximum indicated that tropical and subtropical ocean temperatures either did not change, or perhaps were, at most, two degrees cooler than modern temperatures. However, a bunch of new data is suggesting that the oceans in these latitudes may have cooled as much as 5°C.

Major New Finding No. 4. Until the last issue of Science, it was accepted that about three-quarters of the oxygen 18 fluctuation in the marine record that you saw represented fluctuations in ice volume, with the remainder representing temperature. Strong evidence just published indicates that almost half of the fluctuation of this time series reflects not ice volume, but water temperature.

If time permitted, I could cite still two other major surprises. I cite these new developments not to knock the field of paleoclimatology, which I consider to be one of the most exciting in science, and which I feel privileged to be participating in. Rather, I do so in order to alert you to consider much of what you hear today, including pronouncements by Winograd, as tentative, at best.

Knowledge in the field of paleoclimatology is, at the moment, diverging, not converging. In the words of David Rind, a highly-respected climate modeler, "In this business, observations drive theory."

You will be hearing a lot today from me and others about paleoclimate inferences made from various proxy records, including proxies of global ice volume, sea surface temperature, land air temperature, paleo-plant life, water table elevation, effective moisture, et cetera. A few caveats about such records may be helpful to the Panel.

A proxy is just what the dictionary says: "Something serving to replace another thing; a substitute for," but I will add, not an exact copy of the object of interest. Keep in mind that some of the proxies you will hear discussed today may be recording--probably are recording--more than one climate parameter, and that these proxies incorporate, in varying degrees, local, regional, and global climate.

For example, as I just mentioned, the global ice volume curve we looked at, SPECMAP, which is obtained from the oxygen 18 of foraminifera, records not only ice volume, but also sea surface temperature, and, at some locations, sea water salinity as well.

Continental paleotemperature records, such as have been obtained from the Greenland and Antarctic ice cores, or from Devils Hole vein calcite, are the summation of cloud-base temperatures, changes in moisture sources, changes in isotopic content of the oceans.

Even if proxy records were unequivocal representatives of a single well-defined aspect of paleoclimate, we need to remember some other important things if we choose to compare two or more proxies.

First, different proxy records, even when obtained from the same test hole or location, typically record related events at different times, either because of causal relations between them, or because both are responding sequentially to a third, but as yet unidentified, factor. A good example is ice volume and sea surface temperature at Site 893A in the Santa Barbara Basin, about 420 kilometers from Devils Hole.

I show this data because Site 893A is the closest ocean drilling project program site to Yucca Mountain, at least the closest of the modern drilling, because it is an extremely high resolution marine record, having a sedimentation rate of 160 cm/kyr, and because we will demonstrate, in a forthcoming paper, that the temperature variations recorded by oxygen 18 in Devils Hole are nearly synchronous with the sea surface temperature at this site; synchronous both in timing and in magnitude.

The oxygen 18, in red, is an ice volume record, with sea surface temperatures shown in green. Both time series are tied to the same chronology, SPECMAP. Please note that prior to the last two deglaciations; that is, the penultimate deglaciation, and the last deglaciation, prior to both of them, sea surface temperature started rising about 10 kyr before the ice sheet started melting. In fact, the sea surface temperatures achieved half to two-thirds of their maximum value before the melting even began. So, which of these two proxies should one use to define the transition from full glacial to Holocene climates in the Great Basin?

To complicate matters further, the same proxy; for example, sea surface temperature, may record related events at different times, depending on latitude, even when all are tied to the same chronology. As you have seen, during deglaciation, sea surface temperature proceeded ice volume off of southern California; and, incidentally, also, in the Southern Ocean and in the equatorial Pacific, but, in the North Atlantic, sea surface temperature lags ice volume by thousands of years, as shown over a decade ago in the monumental CLIMAP Final Report.

Hence, comparing different proxies from different locations, as is commonly done, is very risky unless both proxies are equally well-dated, and unless the potential for spatial gradients are explicitly assessed.

To conclude these introductory remarks, proxy records are fascinating, but tricky to unravel, even when well-dated. To be certain of a paleoclimatic conclusion extracted from a proxy record, it is prudent to have at least one independent line of evidence in support one's favorite notion, especially when dealing with an endeavor receiving the scrutiny given to Yucca Mountain.

Okay. I was invited here today to tell you what we've learned from Devils Hole that might bear on Yucca Mountain as a repository, so let me try.

For decades, geologists have been using tufas and travertines; that is, surficial carbonate rocks of ground water or lake perimeter origin, to infer paleo-lake levels, paleo-groundwater discharge points, and the altitude of such discharge, paleo-ecological changes, and the timing of such changes.

In Devils Hole, we had the opportunity to use not only tufas, but also, calcitic veins that record the upward flow of groundwater in fissures that fed the tufas. What do these calcitic tufas and veins look like in Devils Hole? I'm going to take you on a two-minute SCUBA tour of Devils Hole.

Here you see what Devils Hole looks like at the surface; not very impressive. It's a conical-shaped, collapsed feature into an open fault zone. Let's take a look at what the fault zone looks like in a northwest/southeast cross-section. This is a scale prepared by Alan Riggs of the fault zone dip, 70 to 80° to the southeast. It's open somewhere below, to a depth below 150 meters, and the opening is to scale. The average opening is just about two meters.

Let's look at the fault zone along strike; that is, northeast/southwest. This is an old slide prepared by the Parks Service. We have much more detail, but I like to use this because it's rather simple, but it shows the main features.

The saturated zone is shown in purplish blue. Look at Brown's Room in the upper right-hand quadrant. Brown's Room is a small room, nine meters high, that extends above the water table. It has not yet stoped its way to the surface. Some day, it'll do so. We'll talk about deposits in Brown's Room in a minute.

This is a shot taken by Ray Hoffman of the Survey, Carson City Office. Just below the entrance to Brown's Room, just below the water table, it shows you the typical roof of the opening. The deposits in the upper fifth of the slide, the sub-horizontal deposits, are called folia in the cave literature, in the spelunking literature, and these deposits mark the stands of former water tables.

The massive white-color material in the lower four-fifths of the slide are the vein calcite that you find, the dense vein calcite that you find lining all open fissures in the regional carbonate aquifer of southern Nevada remind you that the waters in the aquifer are supersaturated with respect to calcite.

This is a shot above water table in Brown's Room. This is Peter Kolesar, a carbonate petrologist, who works very closely with us. He's at Utah State University. The reason Peter is sweating is the relative humidity in Brown's Room is always 100 per cent. This is a beautiful shot of these folia. Again, they extend to the top of Brown's Room, nine meters above water table.

This is taken 40 feet below water table on a breakdown block. It shows the drilling rig that Alan Riggs constructed, using off-the-shelf items. It's an air motor, powered by a compressor at the surface. The air motor drives a core barrel. Everything is held in place by a strut that's anchored to the hanging wall and foot wall.

The next slide shows a close-up of the core barrel, and the strut holding the core barrel in place, but, more important, it shows the beautiful nature and the density of the mamillary calcite lining this open fault zone, and the last part of the tour shows the results of three days work by Alan Riggs and colleagues to get this core.

It took, as I said, three days, but most of those three days were spent decompressing, with just a few hours a day drilling. This 36 centimeter long core gave us the half million year record that we'll show in a moment.

Because the Devils Hole veins and tufas appear calcite, they are readily dated, using 230 thorium. I notice that there are a number of geochronologists in the audience, so I will just say, for your benefit, incidentally, using samples that we provided, the Devils Hole chronology has just been replicated by Larry Edwards at University of Minnesota, and M.T. Murrell of LANL, using a different isotope, and they were kind of enough to endorse the Devils Hole chronology in print. This took place at the spring AGU meeting.

The calcite in the veins lining the walls of Devils Hole records for us oxygen 18 and carbon-13 in upwelling groundwater that deposited the calcite.

The next slide shows the 500,000 year oxygen 18 time series recorded by the veined calcite from the core retrieved by Alan Riggs and his colleagues. We consider this time series to be principally a proxy of paleotemperature for reasons which I will be glad to recite, if asked to, during the question period. The barely visible dots mark 258 measurements of oxygen 18, while the vertical bars at the top of the slide show the location of the U-series dates, with two sigma error bars attached to them.

I show next an overlay of the Devils Hole and the marine oxygen 18 time series. The linear correlation, (r), not (r^2), (r), between these records is .86. No shifting of curves preceded the correlation analysis. Incidentally, I should add, we've now extended the Devils Hole record forward another 40,000 years. We've now come forward to 19,000 years. This was an older slide, and it started at 16.

Let's look next at a comparison of the Devils Hole record and the Vostok, Antarctica ice core paleotemperature record. The linear correlation of Devils Hole with the initial Vostok chronology, that of Lorius, et al., 1985, is 0.92. The correlation with the more recent EGT chronology, a chronology driven by a desire to be synchronous with the marine record, is 0.85.

Please recall that Vostok is 113 degrees latitude south of Devils Hole, so that these correlations, achieved, mind you, without any shifting of the curves; that is, using the time scale as given by each source, this correlation is remarkable. How many linear correlations of natural phenomena are you aware of that exceed 0.8?

So, what do these slides tell us? I believe they show unequivocally--and I hope that's the strongest word that you hear me use today--I believe they show unequivocally that the major Pleistocene climate shifts recorded in the global marine ice volume record, and in paleotemperatures at Vostok, occurred as well in the Great Basin, as recorded by the Devils Hole oxygen 18 time series.

Now, clearly, there are differences in timing of some key events in these records, differences which some of you know have engendered eight published discussions of our 1992 paper, and, clearly, no one is claiming that the magnitude of temperature at, say, for example, Vostok and Devils Hole is similar, but these records are telling us that the southern Great Basin underwent the same dramatic climate shifts during the mid- to late Pleistocene as have been documented elsewhere on earth.

Well and good, but the Devils Hole oxygen 18 record is only a paleotemperature proxy, which tells us little about the subject you are most interested in from a Yucca Mountain perspective; namely, effective moisture, or paleo-effective moisture.

Were the full glacial climates of 20 to 30 thousand years ago cold and dry, cold and wet, or mild and wet? All three of these scenarios have appeared in the literature in the past 15-20 years.

That they were colder appears to be the case if you believe the Devils Hole oxygen 18 time series. That they were also wetter is seen from the Brown's Room 100,000-year water table hydrograph.

Please turn to the top illustration on the sixth page of my handout. Sorry, I don't have a slide of that. May we have the lights possible just for a minute? If not, I'll go on.

Anyway, this figure from Barney Szabo and colleagues, published in Quaternary Research a couple years ago, 1994. Recall the brachi-fungi looking calcite deposits that I showed in our instant SCUBA tour of Devils Hole. These deposits, again, called folia in the cave literature, are formed at the water table as the CO_2 out gasses from the upwelling groundwater, and they mark the stand of both modern and paleo-water tables in Brown's Room.

Szabo, et al., collected folia from levels up to nine meters above the modern water table, and they dated them using 230Th. They also used, incidentally, the calcite veins and flowstones, two other types of deposits, although these two only indicate whether the water table was above or below the level at which they were collected.

We see that the highest water table in this 115 kyr record occurred between 45,000 and 19,000 years ago. Since 19,000, the water table declined steadily to its modern value. So, clearly, this record, when used in conjunction with the oxygen 18 record obtained from the vein calcite, supports the cold and wet scenario for the latest Wisconsinan glaciation. Let's compare this record with another well-dated Great Basin proxy record of effective moisture.

Here I have plotted only the last 40 kyr of the Brown's Room hydrograph, along with the Lake Bonneville record, in order to illustrate a point I made at the start of my talk; that geographically separated proxy records, even when of similar phenomena--in this case, effective moisture--need not be coincidental, even when both records are well-dated, as these are. Much more could be said about these two records, if time allowed, and Rick, maybe later on, you and I can discuss these.

Okay. Does the water table rise of nine meters in Brown's Room during the past full glacial time have any transference value to Yucca Mountain? And, why is this rise so much smaller than other values reported in the literature? Values as high as 90 meters were recently published by Jay Quade and Marty Mifflin in the GSA bulletin.

A short answer is that the nine meter rise in Brown's Room is not transferable to Yucca Mountain because it occurs in a different aquifer, and is also in a different groundwater basin than Yucca Mountain. But, perhaps there is a more instructive lesson for Yucca Mountain from this modest nine meter late Pleistocene to modern water table shift.

As we heard yesterday, and as has been well-documented, not only at Yucca Mountain, but throughout the Great Basin, the complex structural and stratigraphic setting of this region results in an amazing distribution of modern water table depths. Depths to water table ranging from a few tens of meters to hundreds of meters below the surface occur within one to two kilometers of each other, even beneath a single bajada.

These modern differences in depth to water table reflect the structural disposition of aquifers and aquitards, facies changes, and the presence or absence of topographically low outlets for the aquifers. Paleo-water levels were, of course, also subject to the same tectonic, stratigraphic, and topographic controls as modern water levels, at least over a period of a few hundred thousand years.

If, as is likely, recharge increased during the last glacial period, then a highly transmissive aquifer with a topographically low outlet, for example, the regional carbonate aquifer at Ash Meadows, would be expected to show only a modest rise in water level, and such is the case in the Brown's Room of Devils Hole.

In contrast, a sub-basin underlain by a thick aquitard might record a water table rise of tens of meters in response to the same climatically induced increase in recharge. My point is that for a proxy water table determination to be transferable to Yucca Mountain, it must not only be in the same basin and the same aquifer, but the aquifer must be in the same structural setting as the Topopah Spring formation beneath Yucca Mountain. If these conditions are not met, the paleo-water level proxy, however well-dated, may not represent water table change beneath Yucca Mountain.

This, for me, is the chief lesson to be learned from the modest nine meter glacial to Holocene water table shift, beautifully recorded in Szabo's 100,000-year hydrograph.

I turn next to another use to which the Devils Hole time series might be put in furtherance of assessment of Yucca Mountain as a repository. How long were the previous four interglaciations; and, consequently, how much longer might we expect Holocene climate to last?

Now, this can be approached by modeling, as we'll hear this afternoon. If you're a field-oriented person, as I am, you tend to look at the past, look at the record and see what the evidence we have tells us that we may use. Discussion of such a topic must begin with a few comments on what is your definition of an interglaciation?

You've seen this slide of the marine ice-volume before. Let's focus on two current definitions of the last interglaciation. The warm period between the dashed vertical lines is the preferred definition of some, perhaps many landlubber Quaternary geologists. This interval, which they refer to as the Sangamon, had a duration of approximately 56 kyr on the SPECMAP time scale.

In contrast, paleoceanographers define the last interglaciation as the 13 kyr interval bracketing the highest peak. They refer to this interval as marine isotope substage 5e. The approximate mid-points of the rising and falling limbs define the duration of the interglaciation under either of these definitions.

Now, we will use the paleoceanographers' definition today because it leads to a very conservative analysis; i.e., a minimum value for the likely duration of past and of the current interglacial climates, but keep in mind that the alternate definition for a much longer interglaciation is not without some supporting evidence, which I do not have time to get into today.

You should also be advised that the 13 kyr duration assigned to the last glaciation by the paleoceanographers is not based on radiometric dating, but rather on theoretical assumptions regarding the relationship of 20 and 40 kyr cycles in the marine record to precession and obliquity-controlled cycles in insolation. On the next slide, we apply this sensu stricto definition of an interglaciation to the Devils Hole oxygen 18 time series.

I show on this slide, with a blue overlay, the last four interglaciations at Devils Hole. They range in duration from 18,000 to 26,000 years, averaging 22,000 years, or nearly twice as long as in the SPECMAP marine record. The Vostok ice core also indicates that the last interglaciation was on the order of 20,000; in fact, it was the Vostok workers who first pointed out that, on continental records, the interglacial seemed to be twice as long as in the marine record.

Is there any other evidence regarding the duration of past interglaciations? Data for the high stand of the last interglacial sea level are especially interesting, and when I say high stand, talking about when sea level was at or above modern levels.

The red bars give the duration of the last interglacial high stand in six separate studies published in the past five years. For the benefit of the geochronologists in the audience, I must mention that all the U-series measurements in these studies were made with Tim's mass-spec methodology, and they met the strict criteria that the initial uranium ratio be equal to that of sea water, and I'm sure some of you recognize Barney Szabo and Dan's work and Ken's work, and others.

The top bar you may not have seen represents a synthesis of all the data by Claire Stirling. This appeared in the December, '95 issue of Earth and Planetary Science Letters. Her work indicates the duration of 12-13 kyr, but in using such data, please recall that the high stand data tell us nothing about the time that it took to reach and recede from these high stands, and when you consider such data, based on the sea level curve of Richard Fairbanks for the last 20,000 years, you cannot escape the conclusion that the sea level data, if you apply the same definition of an interglaciation, the mid-point of the rise and the decline, that the sea level data also support an interglaciation on the order of last interglaciation of at least 20 kyr.

Now, clearly, in this exercise, we have been comparing different proxy records, which you will recall I cautioned against at the start of my talk. Specifically, because they are different proxies, they are likely to occur and do occur at different times. Nevertheless, each of them, with the exception of the Marine O-18 records, suggests that the past interglaciations were of 20 kyr duration. Let's add still a further complication.

Because the paleoceanographic definition places the the interglacial boundaries at the mid-point of the rise and of the decline, it, perforce, includes climates considerably cooler than represented by the peak values. However, if we are interested in the duration of modern peak warm periods, then we need other information.

The marine oxygen 18 records shown on this slide suggest that these peaks lasted only a few kiloyears, two to three kiloyears. The Devils Hole record, in contrast, indicates peak durations on the order of 10 to 16 kyr, as seen on the next few slides, which are simply expanded scale plots of three of the past four interglaciations.

Ignore the top time series. It's the Carbon-13 record from Devils Hole, which I have not even mentioned today, because it would take me a minimum of an hour to try to make some sense of it.

The O-18 record we've been talking about is in the lower part, and the yellow overlay simply brackets that part of the record where the O-18 varied less than ± .15 per mil; essentially, no variation.

This is, in paleoceanographic terminology, substage 11.3, which for you is four interglaciations ago; again, 16,000 year period of relative quiet. Going back three interglacials ago, which they term substage 9.3, we see a quiet period of ten kiloyears, and now coming to substage

5.5, which is another terminology for 5e, that is the last interglaciation. We have 10 kiloyears.

Temperature data from Antarctica, deduced from the oxygen 18 data from six cores, shows an 11 kyr long Holocene peak as seen on the next slide, supporting the Devils Hole findings of a 10 kyr or longer warm period. I could show you an identical record from Greenland.

Speaking of Greenland, at the last AGU, Richard Alley of Penn State showed some interesting data that suggested that around 8200 years ago, there was a temperature shift of perhaps two to three degrees Centigrade, so the Holocene was not without variations, but compared to other things, it was very quiet.

I have tried in the past few minutes to summarize a lengthy manuscript on the duration of the past four interglaciations as a guide to the future. The bottom line of that paper is that the present interglacial may be over, or could last another 10 kyr, dependent on which proxy record one wishes to use, and when you believe the Holocene began, another matter which is also proxy-dependent, as you saw in one of my first slides, the one comparing global ice volume and sea surface temperature off of southern California.

Alternatively, if one's concern is solely with the possible duration of current peak Holocene warmth, it may be over, or could last another 5 kyr, and, needless to say, none of the proxy data I've shown you take into account possible anthropogenic alteration of climate.

Does the impending end of the Holocene-type climates, whether in one or in ten kiloyears, mean that we will enter an ice age? Not necessarily so. As mentioned at the start of my talk, new evidence just published in Science, indicates that almost--actually, that's not fair. The authors published a shorter version in Paleoceanography about two years.

Okay, anyway, the new evidence indicates that almost half of the marine O-18 signal is temperature, not ice volume, as believed for the past 23 years. This new finding, in turn, greatly helps to explain why sea level remained above modern levels 115,000 years ago, as shown in several well-defined sea level records by Barney and others; remember, the Hawaii problem they pointed out?

At that time, 115,000 years ago, sea level was above modern, but the marine curve, ice volume curve indicated that sea level was 50-60 meters below modern; that is, that there was a considerable build-up of ice volume. Apparently, the marine O-18 curve at that time was recording a drop in temperature rather than a build-up in ice volume.

Additionally, there is strong evidence that two-thirds of the last ice sheet build-up may have occurred, may have occurred in the closing 15-20 kyr of the "100,000 year cycle."

The bottom line is that the marine O-18 record should not be routinely read as ice volume. At the same time, we need to know what past climates were like during the first, let's say, 10 to 40 kyr after the end of the previous interglaciations.

Okay. Some conclusions of this rather rambling presentation.

I. Due to the large number of startling findings in the field of paleoclimatology in the past four years, it appears prudent to have at least two independent lines of evidence in support of any paleoclimatic notion that one favors, especially in endeavors receiving the scrutiny given always to Yucca Mountain.

II. The global glacial-interglacial cycles of the Pleistocene clearly occurred in the southern Great Basin. The last glacial maximum was cold and wet, not a surprise to most of you.

III. Extrapolation--and this may be trivial, but I think it's worth repeating. Extrapolation of proxy water table altitudes to Yucca Mountain from distant sites is risky, even if the levels are well-dated, and the record is from the same groundwater basin. Needed are paleo-water table data in the same structural block as the Topopah Spring formation at Yucca Mountain.

IV. An examination of a large data base indicates that the past four interglaciations lasted on the order of 20 kyr. Based on these records, the present interglacial is unlikely to persist for more than 10 kyr into the future, and perhaps for much less time, barring major anthropogenic effects on climate.

V. The end of the present interglacial need not necessarily mean rapid growth of high latitude ice sheets, but, rather, cold climates in the Great Basin. Considerable more knowledge is needed on the paleoclimatology of the transitional periods between the peak interglacials and the cooler stadials that followed them.

Last, and perhaps most important, during major climatic transitions; i.e., glacial to interglacial, different proxy records are commonly offset from one another because they are marching to different drummers. The offset can be as much as 10 kyr; hence, the common assumption that changes seen in one's favorite continental proxy are in lock step with the global marine ice volume record may not be correct.

Thank you for your patience.

DR. DOMENICO: Thank you, Ike.

Any questions from Board members? Don Langmuir, Board.

DR. LANGMUIR: Ike, you've shown us, from Devils Hole, how we could use the carbonate precipitates, and I'm sure folks in this program have been looking for carbonate precipitates in voids and fractures below the repository horizon to the groundwater table, but I've never heard anything about it.

Are you aware of any such work, and does it tell us anything at all, if it exists, about paleo-water tables at Yucca Mountain?

MR. WINOGRAD: There's been a lot done, and two gentlemen here, Zell peterman and Jim Paces, I believe, are going to be talking about this this afternoon.

DR. LANGMUIR: Are they talking about paleo-water tables, or are they talking about infiltration waters coming downward in the ESF? Zell?

MR. PETERMAN: We'll be talking about infiltration. There are carbonate veins in the saturated zone. There's a zone around the water table, give or take maybe 100 meters, where calcite's pretty sparse. We've done a few analyses, just isotopic analyses. I don't think--well, I'll take that back. There are a few uranium series ages, maybe from saturated zone calcites, but it's not a large data set at all, and it's not something we're looking at right now.

DR. LANGMUIR: And do we have any sense of what the paleo-water table has been doing at the same time that Ike's Devils Hole data has been accumulating?

MR. PETERMAN: There have been arguments based upon mineralogy from the Los Alamos people, the arguments based upon the vitric transition zone in the rock mass from the standpoint that the vitric rock occurs above any zone that was saturated, and this is roughly, I don't know, 80-100 meters above the current water table.

We made an argument several years ago on the basis of strontium isotopes on some calcites from G-4, that they, isotopically, more resembled saturated zone water strontium than they did the other, the calcites, whose source was infiltrating from the surface.

I think Barbara Carlos wrote a paper several years ago looking at fracture-filling material, and I think she argued that there were some of the fracture mineralogy, something like, I don't remember these figures for sure, so 80-100 meters were more similar to what occurred in the saturated zone, so I think that's what exists at the rock mass itself.

DR. LANGMUIR: Do you guys have confidence that it's an 80-100 meter effect that we're dealing with at Yucca Mountain, as opposed to the 9-meter effect at Devils Hole? How certain are we of that?

MR. PETERMAN: Well, there is no direct age information on these features at Yucca Mountain itself.

DR. LANGMUIR: What would it take to get it?

MR. PETERMAN: Several--

DR. LANGMUIR: This is the critical issue; right? This is really the critical issue.

MR. PETERMAN: It's a critical issue, right. We do--and I think Rick Forester will address these things--there are the Old Spring deposits within a few kilometers or tens of kilometers of Yucca Mountain, and they're, you know, they're some sort of proxies for water discharge in the past, and there's a pretty good chronology emerging for those.

DR. LANGMUIR: I don't think the discharge is the issue.

DR. DOMENICO: Let's refocus on Ike here, because we're going to hear from Zell this afternoon.

DR. LANGMUIR: I appreciate that.

MR. WINOGRAD: These aren't dated; correct? They're not dated.

DR. DOMENICO: Anything further for Ike from Board members?

How about our other consultants? Do they have any questions of Ike; and staff? Leon Reiter, staff.

DR. REITER: Leon Reiter, staff.

Ike, I notice you did not mention the word Milankovich, and I guess that was carefully planned on your part, and I guess we will hear later on about an effort to invoke some of the orbital forcing functions in predicting what might be in the future.

What's your view of that? What do see from Devils Hole your view on those things?

MR. WINOGRAD: Okay. Let me give a two-minute answer. Is that all right? I can't do it in less than that, but I don't want to take longer.

First of all, Milankovich, the word Milankovich is used in two different, very different senses by different groups of people. The physical stratigraphers have a sub-field called cyclostratigraphy. The stratigraphers see cycles in their records, 20-40-100,000 year, 400,000 year, two million year, and they see this in sediments way back to the Triassic. There is no doubt that these cycles--well, it appears the cycles are real. They've been challenged at some sections, but not elsewhere. I accept them as real.

The cycles are real in the marine bracket. The cycles occur in Devils Hole. We said this in our first publication, and in the second publication. Cycles are real. In fact, John Embry, in his critique of our work, claims that the 20 and 40 and 100,000 year cycle are better developed in Devils Hole than the marine record. Well, okay. Anyway, they're there, so the cycles are there and are real.

But, as Olson and Kent of Lamont have pointed out, most recently in the latest issue of PQ, paleoceanography, paleoclimatology, and paleo-something else, but, anyway, they pointed out that they have these cycles in great strength in the Triassic during an ice-free world. So, the presence of the cycles in sediments, whether the continental or marine, or in vein calcite, is not a sufficient condition for northern hemisphere glaciation, so that's one way Milankovich is used, as cycles. They're real.

I think the way you're asking about, that people don't always distinguish, is Milankovich, the Milankovich hypothesis for the origin of northern hemisphere glaciation, and Milankovich himself, by the way, recognized--he was aware that there were cyclothems in the carboniferous rocks, and he was aware that insolation occurred way before the start of northern hemisphere glaciation, and so, he knew himself that insolation was not a sufficient condition, and the cycles were not a sufficient condition, and so he invoked the movement of the pole, of the North Pole.

Our view is that, again, Milankovich knew it himself. Salzman has said it more recently. Insolation itself is not a sufficient condition for the northern hemisphere glaciation, because it preceded northern hemisphere glaciation. We showed in our 1992 paper that insolation is not a necessary condition for deglaciations.

And one last thing, and that is that in response to the Devils Hole challenge, and in response to some very detailed sea level dating by Barney and others, and Sterling and Chin and on and on, it was recognized that some modification had to be made in the Milankovich theory, and, indeed, Tom Crowley and T.L. Kim published a paper in Science in 1994--and I have that paper with me if anyone wants the exact reference--they published a paper in which they made a major modification in the formulation of the Milankovich hypothesis, and I won't get into what modifications they made.

And this is exactly what should happen in science. If a hypothesis is challenged by some new data, the first thing is, is the data correct? And then, if you replicate the data, then a modification must be made to the hypothesis, and this is exactly what Crowley and Kim did.

That would be my two-minute answer; probably took more than two minutes.

DR. DOMENICO: I'm certain there are more questions, but we have a round-table later this afternoon, so, I think, in view of the schedule, we should get going.

Thank you very much, Ike.

MR. WINOGRAD: You're welcome.

DR. DOMENICO: The next presentation is by Rick Forester, who will talk about the paleoclimate records, implications for future climate change.

Rick?

MR. FORESTER: The purpose of our effort in the Yucca Mountain Climate Program is to look at past records in as much detail as we possibly can in order to provide information, at least a basis to discuss what the future may hold.

In today's talk, in 25 minutes, I can't go into all of the detail that we have. What I would like to do is to address three areas: first, climate forcing functions on a millenial time scale; second, climate records for the past 400,000 years, the calibration of those millenial time scales; and, thirdly, some information that we have from multiproxies during the last glacial cycle.

As Ike was starting to discuss, climate is a very complex process. There are many, many forcing functions that drive the climate system. Most of those forcing functions are clearly terrestrial. They are things that are working on the land, not in the skies. Indeed, climate forcing functions work on all time scales. Changes in solar variability, volcanic eruptions commonly operate on a short time scale. On a very long time scale, factors such as continental drift and tectonism; indeed, tectonism in the Yucca Mountain area transformed Pliocene wet, woodland-type climates into a semi arid desert.

On the time scales that we're most interested in--and, as Leon brought up, we are looking at the primary forcing function of orbital parameters. The orbital parameters that are import to driving insolation include eccentricity, the fact that the earth's orbit changes from a circle to an ellipse over time, and I'll use that in the rest of the talk. We also have obliquity, where the earth tilts on its axis, which, in the northern hemisphere, increases or decreases the size of the polar circle, and we have crescession, basically, a wobble as the earth rotates, such that it determines the seasonality of the earth's approach to the sun during the elliptical orbit, affects the amount of insolation.

If we simply look at the eccentricity curves, eccentricity spectra for the last 800,000 years, we notice a couple of things. If we look at where we are in the modern world here, and we go back in time, we see that we've got 400,000 year eccentricity cycles. The larger values on the curve represent a time in history when the earth's orbit is very elliptical; the smaller values represent a period in time when the earth's orbit is relatively circular.

And, I think one of the things that I've often wondered in terms of all of the CLIMAP studies is when you look at simply eccentricity--and eccentricity plays a major role in procession, in particular, in that in terms of insolation--these subcycles of 100,000 years in duration are quite different from each other. They aren't the same kinds of things. Therefore, I wouldn't intuitively expect those subcycles to be identical in the record, and as I'll show in a moment from the Owens Lake record, they appear not to be.

The impact of eccentricity and procession on insolation can be seen in this diagram; great deal of variation. Large ranges in variation represent times when we have an elliptical orbit, and small variation represent points in times when we have a relatively circular orbit.

This is an insolation curve simply calculated from a million years ago to a million years in the future. If we notice, in particular, from Year 0 forward in time over the next 100,000 years, insolation falls in the relatively small range, and represents a period in earth's history where we're moving from an elliptical orbit into a circular orbit.

If we simply compare the time frame in the next 100,000 years, 0 to 100,000, the bottom axis, to the time in history when it was most similar, part of that 400,000 year cycle, we see that the insolation curves are relatively similar, but they are not identical.

Well, does any of this matter? Does insolation have a record in the Great Basin? And Ike has already discussed this, in part, and, indeed, what the Devils Hole record does, by having precise chronology, by having a superb chronology, is it tell us, as, I think, Ike just said, that these kinds of cycles do exist in the Great Basin.

Ike has noted in great detail in his article in Science that there's a difference in timing between his record and that of SPECMAP, SPECMAP being the oceanographic record shown here on the bottom curve. Indeed, there are differences, and when you consider the climate forcing functions are operating in all time scales, are largely terrestrial, and even that the orbital cycles are not perfectly linear or symmetrical, the differences between SPECMAP, between insolation-driven assumptions, purely insolation-driven assumptions, and that of Devils Hole, are not all that great.

In fact, we can plot the Devils Hole. We can plot the Devils Hole interglacials on the eccentricity curve, the maximum temperature implications from the interglacials shown in gray, and the transition to glacial periods shown in light gray. In most of those instances, as Ike has said, the lengths of the interglacial, plus transitions, run from 20 to 30,000 years in duration, and each of those interglacials then fall on a particular part of the eccentricity curve, and can then be related to insolation and the orbital parameters, in general.

As Ike has also said, the Devils Hole record, although it provides us with an excellent chronology, and, quite likely, a temperature record, does not provide us with a moisture record. What we have to do to get effective moisture is go to something in the area at the earth's surface.

So, in summary of where we are now, the Devils Hole record shows that southern Nevada responds primarily, but not entirely, to orbital insolation forcing functions on a millenial time scale. Orbital forcing functions may be calculated for the next climate cycle, and offer a general guide to future climate change.

The regional effect of future climate changes may be evaluated by a study of the long-term past climate and hydrologic records, which is what I'll talk about next.

Future 400,000 year insolation climate cycles should be similar to the past cycles, just as the 800 to 400 cycle was similar to the last, and long-term climate forcing functions, tectonisms and continental drift, will not change significantly in the next 400,000 years.

Further, as Ike indicated in his talk, proxies are not perfect. By no means are they perfect. In fact, that's why we always try to look at a multitude of proxies, because even in paleoecologic proxies, the most commonly used for climate, there are great differences between each different kind of organism.

In the case of Owens Lake, which is the primary record that I'll talk about here--but we also have records from Death Valley, from Walker Lake, from Kowich Playa, from Desert Dry Playa, and from the Great Salt Lake, but in terms of Owens Lake, we have to understand how the system operates with respect to climate, and how our proxies might measure that process.

At Owens Lake, the primary source of water for the lake is derived from snow and rain at high elevation in the surrounding mountains. Those waters are very dilute.

The primary source of salts, and an important secondary source of water is derived from spring discharge at low elevation.

During the very wet, cold climates, the dilute river water completely dominates the lake, resulting in a large, deep freshwater lake. During the very dry climates, spring discharge dominates the lake, resulting in a shallow, warm or cool saline lake. Intermediate climates result in lakes with intermediate depths, thermal, and chemical characteristics.

The volume, thermal, and chemical characteristics of past lakes may be interpreted from the fossil diatoms and ostracodes, and we also have pollen data from this site, transforming fossil data into chemical data. Climate data derived from those fossils may then be compared with other records in the region, like that of Devils Hole, or insolation criteria.

The first record that I'll show you--and this is a tremendous oversimplification, and, by the way, you should move Stage 11 on your handouts up to where it is shown here.

It slipped down into Stage 13 during copying.

The diatom record here is greatly oversimplified. There are 50 or 60 or more species of diatoms, all of which contribute to the climate signal. What we've done here is simply show that we have freshwater diatoms representing relatively large lakes in the planktic category, various kinds of lakes with the benthic category, and the saline diatoms representing, presumably, the dry climate periods.

Careful comparison of this record with the insolation diagrams, or insolation forcing functions show that the system is not responding in a perfectly linear way in this area.

The major glacials are shown in circles, numbered 2, 4, 6, 8 and 10, representing the last 400,000 years, and the interglacials are shown with triangles and numbered 5, 7, 9 and 11.

I could create a similar kind of diagram for the ostracodes, but I decided to do it a little bit differently. The ostracode diagram then would show a stratigraphic profile of fresh and saline taxa.

What I did for the ostracodes was to take--and I should back up and say that in this diagram, from about 200,000 years up, the blank spaces represent non-occurrence of ostracodes. In those instances, we have to rely entirely on the diatoms for a climate signal. Along about 200,000 years, most of the blank space represents portions of the record that are currently being collected.

What I simply did was to take the dominant ostracode in the assemblages that we're looking at, and both in terms of the diatom and the ostracode record, we're looking at in the order of a thousand or more samples from this particular site for the last 450,000 years.

The modern world, the modern dry kind of climate, a perfectly spring-dominated kind of world is shown in red for limnocythre sappaensis. The very opposite end of the spectra is cytherissa lacustris, which is purple. In that case, cytherissa lacustris is a borial taxon and it represents cold, dilute, very stable, unchanging kinds of lakes. So, between red and purple, we're going from modern style climates to the extremes of the glacial periods.

Now, one of the things we should notice about this particular diagram--and I think it's important to the program at large--and that is, if we believe that insolation is at least a crudely predictive kind of criteria, and that 400,000 years ago represents something to do with where we are today, then we go back to 400,000 on this diagram and simply read it forward like a bar code reader, and what we find is what Ike has just said. It's an important point, and this particular record does differ greatly from Devils Hole.

Devils Hole shows its greatest magnitude of change, what might be the coldest or the wettest, being in that Stage 10 period, that Stage 10 period that we, perhaps, are headed into. What this record, and what the diatoms suggest as well is that the next 200,000 years, on this diagram, is filled with largely reds, yellows, greens. It's intermediate-style climates. It is not a severe climate.

But, as we get up to 160,000 years, and, indeed, as we come into Stage 6, again, consistent with what Ike was saying, we go through a lot of red, which means modern like. Then we come into the purples and blues. We move to the extreme climates. The last two glacial cycles in this record, which is within 100 miles of Yucca Mountain, shows that Stages 6 and 2 are much colder and much wetter. We don't know how wet yet. We're still trying to put numbers on this relative scheme, but suggest that it's much wetter and colder than what we saw in the earlier two cycles, the earlier 200,000 year cycles, which look to be, in terms of the future, drier, wet, wetter than today, certainly, but not ice-filled worlds.

Now I would like to move from here to a multitude of proxy records that exist in the last 40,000 years, and, again, let me emphasize, it may be that the last 40,000 years, for insolation reasons, may not be a good analog for what we're moving into in the future, but what the last 40,000 years does tell us, because we have many kinds of proxy records, what it does tell us is a lot about how the hydrologic system in this area changes in response to climate.

Now, one of the first things we have to realize is that in the Pleistocene, in this area, atmospheric circulation is very different from today. A Pleistocene world is not, in any way, shape, or form, like a Holocene world, so when we talk about deviations in precipitation and temperature, we're not talking about operations about a Holocene mean. We're talking about an entirely different mean.

In terms of circulation style, the kinds of--and, in that consequence, the positions, in this case, of the polar front, which today have an average position on the Canadian border, in the Pleistocene, had an average position in this area. Indeed, a good deal of Stage 2, they probably were south of this area. The consequence of that is cold, extreme cold, and then a moisture level that is representative of the interaction of that circulation style with the local topography.

This is some of the packrat midden data that is generated, in part, by the Desert Research Institute, but, indeed, the pattern that is here is also replicated in other data sets; most notably, Jeff Spaulding's, and what it represents, in this case--and, again, as with the other data sets, it's a gross oversimplification of what total data we have, but what it represents is an expression of, A, that colder, wetter condition associated with polar fronts being in the region. White fir and limber pine, which, today, live in the area at high elevation, have moved down to lower elevation. They're moving down to around 5,000 feet.

We see the end of that period represented dramatically right here, and the appearance of pinyon pine. That represents the retreat of the polar front out of this region and to the north, as we go into a Holocene world.

Limber pine today is a plant that can live in relatively dry kinds of conditions. Common average for limber pine precipitation is something in the order of 16 inches. More importantly, limber pine is a plant that really likes it cold. A common average for limber pine, mean annual air temperature is 4 or 5°C, and, most importantly, limber pine does not like warm summers. Limber pine does not like summers above about 15°C on an average.

What limber pine coming down to lower elevation suggests is that, at a minimum, temperatures in this area, mean annual air temperatures are getting much colder. I would suggest they're getting 10°C colder, but we're in the process, a number of us are in the process of debating that order of magnitude. Precipitation is probably increasing at a minimum to bring those trees down to the elevation that they move to, at a minimum, is increasing by 40 or 50 per cent.

If we go to the center of the distribution of the species itself, rather than worrying about what it takes to bring us down from the mountain tops, because, again, circulation has changed. We're not simply looking at a modern world. We're looking at something more on the order of doubling precipitation, and making it very, very cold.

White fir, by contrast, represents somewhat warmer conditions. I should back up and say limber pine only intervals, or quite likely intervals when the polar front is largely south of the test site throughout the year. Today, the polar front rarely gets to the test site.

White fir, by contrast, is wetter, and is warmer. Warm is a relative term, and you'd be cool relative to today; perhaps four or five degrees mean annual air temperature cooler or colder, and an increase in precipitation--again, a minimum number to bring the limber pine down, and this is just Spaulding's number--he, indeed, has agreed that things are wetter than he thought they were in the early eighties--would be a 75 per cent increase in precipitation.

I would argue that we could easily be in the, again, double or perhaps as high as triple, although I'm beginning--as Jeff is beginning to think it's wetter, I'm beginning to think it's drier, so there is some consensus.

What white fir then represents is a period when the polar fronts are oscillating through the region, and, quite likely, are bringing moisture in from the southwest.

They're bringing it in in a quasi monsoon style, and, again, a sense of what that represents, yesterday, Alan mentioned something about the importance of El Ninos--I said monsoon, I meant El Ninos. Alan said something about the importance of El Ninos. What white fir represents are mega El Ninos, in a sense, only on a thousand-year time scale, rather than every six years. So, it represents a huge amount of moisture coming into the region. Huge might be double, triple, something in that particular order.

We are also working on determining whether or not the modern day precipitation gradient that Alan described yesterday exists in the Pleistocene at the same magnitude that it exists in the Holocene. If it exists in the same magnitude, then the white fir intervals, in terms of the top of Yucca Mountain, represent something on the order of 14 to 16 inches of precipitation.

We don't know the sigma one about that mean. Modern day sigma one for climate stations in the area usually suggest a variation of about 50 per cent of the mean, so taking a 50 per cent of the mean, about a doubling gives you a sigma one high value of triple, and a sigma one low value of something comparable to the modern world.

In addition, the region and the valley bottoms are filled with marsh deposits. Jay Quade, in particular, has spent a lot of time studying those particular marsh deposits. They simply represent an illustration that climate was wet enough to support surface water in the region.

In this particular case, the Corn Creek Flat area, we have several hundred samples through here. In the high-level waste papers in 1994, I reported a climate interpretation of 400 to 600 mm, 16 to 24 inches for what I thought at the time was valley bottom climate. I'm now convinced that there is sufficient moisture in the system, and I'm convinced that there's sufficient flow through the system, that, although I still believe the 16 to 24 inches, I believe that has to represent where the precipitation fell, integrating the mountain top.

Further, when we first started studying these deposits, we thought they represented relatively continuous deposition. As we began to date the deposits, we discovered that, in fact, they represent very episodic deposition, and, indeed, if we crudely plot the white fir intervals on against these ages, we find that the major sediment packages are representative of the white fir periods; again, supporting the fact that white fir is probably a wetter period. Either the limber pine periods have not had much deposition and were drier and colder, or the limber pine period deposition has been largely eroded.

And the last section I'd like to mention is the one of the Lathrop Wells Diatomite. This is an outcrop that was thought at one time to be early quaternary or Pliocene, more than a million years in age. We have subsequently dated it using radiocarbon techniques, uranium thorium techniques, and thermoluminescent techniques, and have discovered that, in fact, it represents at least deposition in the last cycle, and, depending on the differences in thermoluminescent versus uranium, may also represent deposition in the penultimate cycle.

The kinds of fossils that we see in this deposit, in particular, the diatoms, suggest that we're looking at discharge water that is dominated by a sodium bicarbonate composition, and is high in silica, suggestive of water from a volcanic aquifer. Some of the ostracode assemblages also suggest that we're looking at regional aquifer deposition by virtue of high flow and other chemical characteristics. Other ostracode assemblages, which are not in the same places as the diatoms, suggest that we could be looking at water from a perched system as well.

So, the fossil evidence suggests that we're seeing both regional aquifer discharge and, potentially, perched aquifer discharge.

If we then look at the change in elevation of the water table, as described by Brian Marshal and Zell Peterman and John Stuckless in '93, Jay Quade and a number of others in '95, and if, in fact, we are seeing the regional aquifer at this site, then we're looking at evidence for the modern water table coming up about 115 meters in order to create the discharge at the Lathrop Wells Diatomite.

As Zell said a moment ago, there's evidence within the repository block of a similar rise, but the age of that particular rise is unknown, and, as Ike said, aquifers are very complex. Whether or not we're looking at a single aquifer that also rises in response to a climate change within the mountain, as it apparently did for the Lathrop Wells Diatomite is not known at this time.

In conclusion, climate change cycles between dry and wet modes. In southern Nevada, the wet modes have existed about 70 per cent of the time. Those wet modes, in some cases, are extremely wet, but, in most cases, are simply something wetter than today.

A full climate cycle is about 400,000 years long, a full climate insolation cycle, I should say, and contains roughly 100,000 year subcycles, each having glacial and interglacial conditions. Devils Hole shows the subcycles are less than 100,000 early and are greater than 100,000 years later in the 400,000 year cycles.

Climate cycles correlate with orbital parameters, which govern insolation; the key multi-millennial climate forcing function.

Past climate-driven hydrologic change serves to estimate change in the future according to the known progression of insolation cycles.

Present day dry climate may begin to change towards wet climates in about 1,000 years based on past records. Now, past record is the Owens Lake record in terms of figuring out in insolation terms, where we are today, and when the transition started to occur according to the Devils Hole record; sooner if global warming persists according to some model interpretations.

Notably, one model interpretation argues that the formation of north Atlantic deep water could be shut down if we have continued melting of ice and snow at the polar ice caps; and, secondly, a model by Steve Hostetler that suggests that double CO_2 increase results in both higher evaporation and higher precipitation in the Great Basin.

Current interpretation of the Owens Lake data suggests the next wet period may not be as wet as the last and penultimate glacial cycles. In terms of simply the magnitude of the Devils Hole record, this is the opposite kind of sigma that you might conclude from Devils Hole.

Finally, during the last glacial period, high effective moisture produced a 100 m rise in the regional water tables near Yucca Mountain and supported wetlands and streams throughout the region. Drainage from the Amargosa River, which was apparently a permanent stream through much of the last glacial, helped to support a large permanent lake, about 90 meters in depth, in Death Valley.

During the last glacial cycle, within 100 miles of Yucca Mountain, mean annual precipitation--and when we use the term, "mean annual," in terms of the proxy records, we really should be talking about mean century precipitation--likely varied from about 15 to more than 20 inches at some localities between five and six thousand feet, with as yet unknown standard deviation and regional variability.

Thank you.

DR. DOMENICO: Thank you very much, Rick.

Do we have any questions from Board members?

DR. COHON: It seems that one of the most important observations and conclusions you made, looking at your page nine, is that Devils Hole record shows southern Nevada climate responds primarily to orbital forcing functions. I would really like to understand the basis for that, and page seven just went by me too fast to catch that.

Could you spend a little more time on that? It seemed to me that you were citing the figure on page seven as the primary basis for that conclusion.

MR. FORESTER: The SPECMAP originally got started as much to understand climate as it got started to understand the ages of date marine sediments, so SPECMAP, when they found the isotopic variation; that is, the SPECMAP curve shown on the bottom, they made the assumption that is was perfectly driven, and still argue, in some cases, that it was perfectly driven, entirely by insolation criteria.

Ike and company came along and actually dated the changes. We have a similar curves that suggests that climate change in the ocean and at Devils Hole are strongly correlated. Ike came along and noted that, in particular, the transition periods assumed by SPECMAP in dash lines are at a different time than that clearly dated from Devils Hole.

However, when you look at the basic structure of the Devils Hole record, and if we have perfectly driven--SPECMAP is perfectly driven, Devils Hole suggests that it is imperfectly driven by insolation--the differences are then represented by the differences in those dashed versus solid lines, the differences are not all that great.

In other words, although insolation clearly does not drive the entire record, because we have other forcing functions that are involved in climate change, the principal variability of Devils Hole can be correlated, at a minimum, with that of the insolation criteria, because the insolation criteria would argue for perfect driver of SPECMAP, and the two records are relatively close. They are not perfectly close.

Is that better?

DR. COHON: Yeah. Would you accept the qualification that the orbital driving driver is the primary influence of the long-term, low frequency part of the variability question?

MR. FORESTER: Right.

DR. COHON: But not necessarily the shorter term?

MR. FORESTER: Absolutely not. On the short term, forcing functions are a multitude of things, from oceans to variability to quite a variety of other things, and on the long term, continental drift, mountain building, and so on, are also important, and may well explain why Milankovich-style cycles are operating, as Ike said, in the Triassic without making ice sheets. The long term forcing functions on earth are different. The continents have a different configuration.

DR. COHON: So, just extending this, then, but not trying to put too many words in your mouth, you go on to say on page nine that future 400,000 year insolation-climate cycles should be similar to past cycles, and the point you're making there is, again, looking at the long term fluctuations of this sort you just talked about?

MR. FORESTER: That's right.

DR. COHON: You should expect to see.

MR. FORESTER: Similar, and similar is the key word there, not identical; similar.

DR. COHON: Right, and it doesn't mean that the next 10,000 years should look just like the last 10,000 years.

MR. FORESTER: Absolutely.

DR. COHON: Okay. Thanks. I just wanted to clarify that.

DR. DOMENICO: John Cantlon, Board.

DR. CANTLON: For the interest of the repository, this paleoclimate needs to be thought of in terms of what the behavior of the specific site will be, and you talked about increasing moisture and increasing cold, but when one looks at infiltration in a particular site over the repository, the depth of snow, the distribution of snow as opposed to rainfall is one of the important variables.

Do you have any feeling of what the snow picture would be like?

MR. FORESTER: Yes, and I meant to mention that when I had the midden tree diagram up.

Both of those tree types, and a number of other plants in those records, suggest that an important form of precipitation during that time frame was the snow, and, indeed, that, quite likely--this is Jeff's words, not mine--snow may exist at lower elevations much farther into the season than it does today. Snow pack would be substantially higher than it is today, would be at lower elevation, and might persist all the way into the late spring, early summer season, so the potential for snow and snow infiltration is large during portions of the glacials.

DR. CANTLON: And the important variable there is that snow is not uniformly distributed over a topographically variable surface. You have deep drifts that persist well into the summer, putting infiltration into specific sites, so that as one begins to think about the hydrology of the repository, the snow depth may be the most important variable in the out years.

MR. FORESTER: Yeah, and I think that's consistent with what Alan said yesterday. It's important to note the kind of precipitation, the timing of it, and the associated temperature in terms of the infiltration, and, yes, I think, again, when ice volume is maximal, which is not a lot of the time, there is a good potential for high snow pack, persistent well into the late spring, summer seasons in this area.

The kinds of precip that are likely to occur in the next 100--again, using insolation as a guide--may be far more seasonal, representing summer rain, as well as winter rain, perhaps, rather than snow.

DR. DOMENICO: Any questions further from the Board? From the staff? Ike, go ahead.

MR. WINOGRAD: On page nine, again, the same item that you had, Rick, first bullet, Devils Hole responds primarily to orbital forcing, I think one other thing needs to be said.

You recall, in John Embry and 19 others Magnum Opus, December, 1993, Paleoceanography, they went through a series of models, and these curves that you have seen, the SPECMAP curve, the Devils Hole curve, the Vostok curve, in that wonderful summation of Embry's retirement project, a wonderful paper that summarizes all the different models that had been proposed to explain these curves, and they can be explained giving a major role to insolation, as one end number. They can be explained, as Barry Salzman did years ago, without insolation at all, or something in between, so you have a choice of models.

So, to the degree that SPECMAP can be explained with these various models, so can Devils Hole be explained with the various models, and the role of insolation may be major, may be minor, or may not be present.

MR. FORESTER: And if insolation is not a major goal, then our capacity to try and predict or forecast future conditions is greatly hampered.

DR. DOMENICO: Any further comments?

DR. PARIZEK: In talking about the El Nino, talking about maybe a 1,000 year mega cycle, because according to Alan Flint yesterday, that was a critical part of his whole recharge data requirements. Can you say a little bit more about the mega El Ninos that the white fir suggests?

MR. FORESTER: I used the El Nino to draw attention to what white fir represents in terms of a circulation mode. Quite likely, the circulation in that time frame is directly a result of polar front storm tracks coming in from the southwest. It is not an El Nino, but it would behave, in terms of moisture delivery, in an analogous way to what the El Nino appears to do in today's world for infiltration.

Quite likely, if, indeed, the central Pacific Ocean is cooler in the Pleistocenes, there probably aren't El Ninos, or they're very, very mild compared to a warmer world.

DR. PARIZEK: But still, more precipitation to have the fir?

MR. FORESTER: Yes, yes, but it's more related to a precipitation style, not amount. It is associated with the arboreal forests today.

DR. PARIZEK: The other question about the Corn Creek Flat marsh deposits, did I understand you to say that precipitation is implied to be 400 to 600 millimeters?

MR. FORESTER: In a paper in '94, that was the interpretation I came to, and I thought at the time, because I thought the water was basically still standing on the valley bottom, that that number applied to the valley bottoms.

I now realize, in further analysis of that data and more data, that, in fact, the water is flowing continuously. The consequence of that, for the mode by which I arrived at precipitation numbers, would demand that four to six hundred millimeters be up on the mountain face, not on the valley bottom, so the valley bottom would be less than 400. Four hundred for the valley bottom in Corn Creek is about four times modern, so mean annual precip, or mean century precip at Corn Creek and the valley bottom would be less than four times in that time frame; likely double.

DR. PARIZEK: If you have a shallow water table, you could also have swamp deposits independent of the rainfall, but you need rainfall to get a swamp deposit to come from runoff from the mountains?

MR. FORESTER: No. The way I interpret the climate is not dependent on the relative rise of the table, but, rather, the chemical and thermal characteristics of that water.

DR. DOMENICO: Are there any further questions from staff members?

The schedule calls for a break at this time, and let's take a thirteen-minute break and be back at ten minutes after ten.

(Whereupon, a brief recess was taken.)

DR. DOMENICO: It's time to continue these climatic discussions. Can we reconvene, please?

Our next presentation will be from Stan Davis, giving us some perspective on paleohydrology.

Stanley?

MR. DAVIS: I'll give about half a minute yet for people to sit down.

For those of you that have picked up the duplication of the overheads that I prepared, I want to shift the blame for the postage stamp-size reproductions onto the new Denver International Airport. I was faced with the problem of trying to hand-carry my heavy luggage, and to avoid the highly-publicized baggage shredder, so, taking a clue from corporate America, I resorted to down-sizing, so that's what you have.

In preparation for my talk, I was given only three small reports; one by Paces, and one that came from the USGS, and I don't know who the authors were. These two were on uranium and its use, and then the third was one by June Fabryka-Martin and others concerning the use of primarily chlorine-36, so my remarks that are related to the Yucca Mountain site are related to these three reports, and I will make, also, some very general remarks.

A common trap that we fall into in considering the use of radionuclides in hydrogeologic studies is to delude ourselves into thinking that we can date water. With the exception of the use of tritium, this is impossible. We are studying radionuclides, not the water, and we can only say something about the history of the water if we know the relationship between the history of the radionuclides and the history of the water.

Fortunately, the authors of the reports that I looked at, didn't fall into this trap, but it's one that's very prevalent.

First, I want to touch briefly on uranium and thorium, and I'll move over to the overhead. This is an outline, briefly, of what I'm going to talk about. I'll just barely touch on uranium, because we're going to have some presentations, I'm sure, that will cover in great detail some of the aspects of uranium. I'll spend most of my time on chlorine-36.

However, I wanted to put up a very simplistic diagram concerning the use of uranium as a dating technique, and I'll point out one thing. Again, the authors, Paces and the other authors of papers I read did not do what I'm going to talk about. They did not make certain assumptions, but I want to make sure that these assumptions are identified.

In a very simple-minded manner, this is what we're dealing with in uranium dating. One would be to look at the disequilibrium between 234 and 238 in the active zone soil and below in the oxidizing region. We have a tendency to selectively expel uranium-234 from the minerals, and so you initially have some ratio of activity. These are activity ratios and not mass ratios.

So, the activity ratios, to begin with, are at some high value due to alpha recoil and a number of other reasons, and this initial value may vary quite a bit. It's quite a problem to determine some reasonable initial value, but, then, once the mineral is precipitated, you consider it as a closed system, and, eventually, after a few hundred thousand to maybe two or three million years, you approach very closely an activity ratio of one.

On the other hand, if you start out and precipitate mineral, the thorium has such a low solubility, that you assume that thorium is virtually absent, and you are simply precipitating uranium, and you start out with a very low activity ratio of thorium and uranium, both being--of the thorium being the second moderately long-lived decay product, then, after awhile, it'll approach one, so we can use this curve, or the deviation from this ratio as a variable related to age, or the date at which the mineral was deposited, or opaline, in the case of opaline deposits.

Now, the thing that I wanted to mention here was the fact that we don't really know anything about the distance of travel of water that may be associated with this deposition, nor do we know anything about the travel time from looking at the mineral itself.

Now, the studies that have been made--and I would add that they're most impressive studies, scientifically, as well as in a practical vein--that these studies that were made included a lot more than just analyzing the uranium and thorium, so what we can say is far more than what I've just said. You have the age or the date at which the mineral has been formed, but from the mineral itself, you don't know where the water came from, you don't know how fast it came. You can't say much about the hydrologic system from that alone.

Of course, from other studies, from the shiny surfaces on the minerals, and so forth, very valid and reasonable conclusions can be drawn concerning some of these more important things.

I wanted to go on and spend most of my time on chlorine-36, and just very briefly touch on the origin of chlorine-36.

We have four major mechanisms by which the chlorine-36 is produced in the atmosphere. It's primarily spallation of argon-40 by primary and secondary cosmic particles. In the land surface, right at the surface, within a few centimeters of the surface, we have several processes that go on; activation of chlorine-35, a negative neuron on calcium-40, and a neutron on potassium-39.

Then, in the deep subsurface, if we have an ore deposit where there's a concentration of neutrons due to alpha n reactions, then the primary reaction is activation of chlorine-35 by thermal neutrons. It's very efficient. The cross-section here is quite large, and so, we have this as the primary production mechanism in the deep subsurface.

Now, one question is, what is the, or what would be the expected background on chlorine-36 if there were no anthropogenic sources? It's a very complex question. It's one that I'm involved in right now. We're making measurements and trying to work out some of the variables involved.

This is a very rough preliminary map, and the points here are two types; one, the open circles would be shallow, or fairly shallow groundwater where the chloride content is extremely low, and where there is no large amount of tritium, indicating recent recharge, but there is still a fair amount of carbon-14, indicating that we are in, say, a period less than 10,000 years old as far as the recharge of the water's concerned.

Using those criteria, you eliminate most of the chlorine-36 determinations that have been made throughout the country, and you're left with just a scattering of data points here.

The thing that's of most interest here, we wanted to check June Fabryka-Martin's assumption of about 500 as being the ratio between chlorine-36, the numbers of atoms of chlorine-36, the total number of atoms of chlorine, and she used, as I understand, 500 x 10^{-15} as being the ratio here.

Well, the data that I have--these are soil column data, these are groundwater data--they seem to fit quite well with this assumption, so, say, three or four hundred years ago, we would expect, from atmospheric precipitation, we would expect this ratio of around 500 x 10^{-15}.

One reason why we want to look at only the very low chloride content waters is that very commonly, we have sources other than precipitation, where we are dealing with a mixture of some kind. We're not dealing with only precipitation. This is why I bring out the very important fact that we're looking at the history of chlorine-36. We're not looking at the history of water, and we have to relate the two eventually, or we should, in order to draw useful conclusions, but we have this basic problem.

These are unpublished data given to me by the USGS, and these are data from northeastern Arizona, roughly at the same latitude as the Nevada test site.

You can see the data points from groundwater here follow almost a perfect mixing line, with a few scattered points out here. What we're plotting is the amount of chlorine-36 related to the total stable chlorine x 10^{-15} on the horizontal scale, and on the vertical scale, we have plotted the total chloride. So, to get some idea of what the rainfall would be, or the snowfall in the area, you have to look at these values down here, not these other values that have chlorine from other sources; namely, here, it would probably be Mesozoic salt beds that affect the regional amount of chloride in the groundwater.

Now, to sort this out, we can use bromide. Bromide, geochemically, is almost, not quite, identical to chloride in its chemical behavior, and it so happens that precipitation, in general, has a fairly low chloride to bromide ratio. This is a mass ratio now, and we have values that generally range between about 50 and 150, in that range for normal precipitation, and if we go into an area where there are other sources of chloride, very commonly we get values that are considerably higher, and up in the range of several thousand as the ratio between these two elements and the water.

So, this dotted line here that I have is a hypothetical mixing curve. The red lines or red crosses are the values, median values of several score--in each case, each point represents about 15 separate analyses grouped together, so we have here a fairly good fit, suggesting that, indeed, in this place in Kansas, we have this mixture problem, and if we're going to look at chlorine-36, which they haven't done as yet, in that area, we have to go down here in a very low value of chloride, which is the horizontal scale.

Unfortunately, in some areas, we have sources that are mixed in that are almost at the same level as precipitation, and here, our Milk River values are a case in point, so this method of sorting things out is not always as useful as we've indicated on these two curves. The one for Tucson here indicates a concentration by evaporation first, and then a mixing effect later, and, here, the cutoff is about 40 mg/L. Here, the cutoff for the precipitation source is somewhere below 10 mg/L.

I want to now turn to what might be expected in various anomaly sources. Oh, I would add that in the Yucca Mountain, as I understand it, bromide was added in copious amounts so that the ratio was vastly altered, so that water introduced into the operations then could be identified and separated from the water existing in the rocks.

These are just some samples, and these are values that are from soil column tests, and all I'm saying here is that we have a sufficient input from bomb fallout, which started in the very late 1950s, actually, late part of 1952, and extended on through about 1960.

This fallout residing in the soil gave rise to these pretty high ratios. Now, I was just talking to June Fabryka-Martin before the talk here, and she says that some of the values here are far above the soil values, some of the values in the rocks along the fractures.

One interesting thing that we'll come back to is that in the stratosphere, we have, in some cases, relatively high values for chlorine-36, and, of course, it gets into the troposphere seasonally, in various amounts, and so, we do have a source of chlorine-36 that's variable with precipitation.

I've stuck in a couple of values from--this is some of June Fabryka-Martin's work in Australia, and this is simply the ratios found in the solid ore itself, indicating the importance of ore deposits, in some cases, as a source of chlorine-36.

Lastly, I show just two values for the activation of potassium, calcium, and chlorine in surficial rock materials, indicating that you can get up to fairly high values if these objects are at the land surface and receive a continuing bombardment of secondary cosmic ray particles, and you can then generate moderately high values.

Now, I want to next go to the data that were presented from the studies in the tunnel at Yucca Mountain, just to show that the largest number, of course, as stated in the reports, are fairly low values, most probably representing some background value.

However, there are some values that are very high in comparison, and do suggest very strongly that we have bomb pulse materials that have reached the level of excavation along fracture and a permeable zone. These colors are almost meaningless. This indicates that there may be some question here in these two, but I don't think there's any question that we have a very strong anomaly here, and these are more or less normal values that one might expect.

Now, I want to go into a little bit of philosophy, and it's something that is sorely lacking in a lot of the work that's done in relation to evaluating potential sites for waste deposits. This is the famous Ockham's razor. The idea is that you just don't introduce a lot of complexity unless you have to. That's one way to say it.

A doctor once told me, in diagnosing something, that if you're in the west and you hear hoofbeats outside, and you can't see what's out there, you don't assume that it's a zebra, you assume that it's a horse, and the idea is that we're looking at things that are probable, and we don't reach out and look for little green men from Venus or something to do our job for us.

With that, then, we're going to violate what I've just said, and we'll go to some fantasies. The question is, of course, where do these anomalies arise? Are they really from bomb fallout or what? And I've pondered these questions. They're related to the kinds of research that I'm doing, and I don't think that I've reached any really successful conclusion in my own mind.

However, in terms of probabilities and importance, I think the testing of atomic explosives is really the cause of most of the large anomalies that are being measured.

We do have past fluctuations of cosmic ray production. These are documented in a number of ways. They're documented in relation to Carbon-14, and the picture is emerging that perhaps there was the pre-anthropogenic chlorine-36 in precipitation back more than 10,000 years ago may have been more than double what it is today, so we have this as a real possibility.

In situ natural production, I showed the values for ore deposits. We don't know, as far as I know, there are no ore deposits in the vicinity of Yucca Mountain. It might pay to scan very carefully some of the gamma logs just to double check, particularly if we have zones that are more than a meter thick. We might possibly find some zones that have sufficient uranium and thorium to produce fairly large anomalies, but, then, I think we're getting off into Fantasy World already.

Dissolution of surface rocks. I'll come back to this. There is sufficient chlorine-36 being produced in the upper sort of skin of the earth to account for some of the anomalies, but one has to go through a very imaginative set of assumptions in order to reach any values of interest.

Variation in total chloride deposition. This can be very important, and I showed you the data from northeastern Arizona, indicating that, in that case, the amount of dead chloride mixing in greatly altered the ratios that we had.

This I will come back to, No. 6, variations of chlorine-36 in the troposphere, as measured mainly by samples of rainfall.

Atomic reactor sources, I don't think this is important. It has been measured near the Idaho facilities, also, Savannah River, anomalously high amounts of chlorine-36 being present. However, if we get this as an origin for the anomalies that are measured in the subsurface, this is practically the same in terms of travel time as we would have with bomb fallout.

Contamination of the sample. In our early work, we had some samples from Australia, from the Lucas Laboratory, and we thought we had found the world's greatest ore deposit for uranium. It was actually some contamination, but this is handled through normal procedures and analytical problems, and so forth.

Gas transport. Now we're getting off into real fantasy, but gas transport might be possible. There are volatile types of chemicals, such as carbon tetrachloride, that contain chlorine, and if they're activated, then you could have chlorine-36 in the gaseous form. Where this comes from, I'll leave it to your imagination, but, anyway, gas transport, physically possible, unlikely.

And the last item, we have, in the literature, a suggestion by a Japanese scientist that we could find the anomalies related to prehistoric super nova when we add very large impact of cosmic ray particles activating chlorine, and this has not been followed up. I don't think it's taken seriously by anybody, including myself.

So, here we have a list of possible origins, ideas. The anomalies that we find may be related to some of these factors, but I think the one on the top is the important one.

I want to just put on just a wild estimate that I made with some assumptions. These assumptions, all of them would tend to produce more chlorine-36, I believe, than actually would take place. We assume that the rock has 100 mg/kg chloride. That's the content. The chloride in the rock has been activated. It's right at the surface, and the ratio is 10^{-11}, which is higher than any of the published ratios that I could find, but not a great deal higher.

The available water is very large, so that we have to assume some sort of surface runoff, and the water going down may be in a low depression. The inwash cancels erosion. That's a tricky one. That's simply to mean that we are not reducing the land level. We're not taking the surface off. We're inwashing stuff as we take it out by dissolution, and then we assume that the dissolution produces water with 100 mg/L total dissolved solids from the initial value of rainfall, which would be close to zero.

And then, lastly, we assume that the precipitation has .5 mg/L of chloride, and that the ratio is the value that June Fabryka-Martin has assumed.

The results. We get a fair amount of chlorine-36 going down, and perhaps about a third of what would come down from the rainfall itself. These values are off some way. I couldn't adjust them. I couldn't juggle them enough to come into a closer agreement with a possible fallout of 10^5 atoms of chlorine-36 per centimeter per year. I don't know whether this value is any good or not. Somebody can correct me afterwards on that.

But, the sum total of this is that if you really strain yourself and go after things that aren't very probable, then you might possibly get a slight anomaly. I don't think that dissolution of surface rocks would answer our question. There may be people that are more nimble with figures than I.

Okay. This is simply a diagram indicating what may happen. We have data on precipitation. The data on precipitation are ambiguous. There's a lot of instrumental scatter because of the low concentration of chloride, and there is a suggestion, however, that the chlorine-36 has a seasonal pattern, and we do know that there's about a tenfold difference in between the amount of chlorine-36 in the rainfall, according to various times of the year and times of storm, and so forth.

And, if one looks at the tritium values, where we get a definite cyclical effect due to the exchange between the stratosphere and the troposphere, and assume that something is happening with chlorine-36, then we might say this is a seasonal variation, with maybe a peak in the late spring here, or something like that.

Now, what does this possibly mean? It means that we may get an infiltration into the subsurface that is not related directly to the average composition of precipitation, and that may vary from place to place. This would be due to --the simplest would be to think in terms of a more colder climate where we have a frozen ground, and the initial runoff from melting snow would simply take that water out from the region.

It would not become recharged, so that we can selectively recharge at certain times of the year, depending upon storm intensity and a number of other variables, so that we're not putting in the average here. We're putting in maybe the peak value or the trough value, and so there is this microhydrology in the surface that might affect the chlorine-36 in the subsurface.

Now, some surfaces, such as a nice talus slope, might capture everything, so here we would get the average; here, we would not. So, that's a variable that needs to be assessed, at least kept in mind.

Well, what I've tried to do is to give you a glimpse of the variables involved in using chlorine-36. I certainly don't think that I've really answered too many questions, but at least, I hope, it's food for thought.

Thank you.

DR. DOMENICO: Thank you, Stanley.

Any questions from Board members? Don Langmuir.

DR. LANGMUIR: Stan, I was thinking about Al Yang's observation that you have higher chlorides in the unsat zone than you find in the sat zone, and wondering whether that--maybe June has to speak to this, maybe this waits until June's presentation, but the age dates in the unsat zone that I've seen have been tens of thousands of years, and I didn't recall anything that you could call bomb pulse in the unsat zone collected from surface borings, from the surface sampling.

Is there any connection there?

MR. DAVIS: Don, I have to dodge the question because I don't have information specifically for the Yucca Mountain area. I'll answer it in a very general way, however.

The profiles that I've helped do, and that my students have done in other areas will show an accumulation of chloride in soil; now, not in rock settings, but in soils, so you have an accumulation of chloride that indicates that there's very little through infiltration in the arid, say, the alluvial fan materials, except in the channels, of course.

But, by taking some reasonable amount of chloride per year and doing some calculation, you can pretty much show that these chloride accumulations represent thousands of years, and the bomb pulse doesn't go down very far, maybe one or two meters. You can pick the bomb pulse out and then, below that, in some cases, at least, you have a fairly constant value.

DR. LANGMUIR: I have another unrelated question. You raised the possibility of carbon tetrachloride, and I was just chatting with my colleague about it being used to clean old TBMs, perhaps, machinery at depth. Is it possible that you could get contamination from carbon tetrachloride, specifically, in the ESF, I wonder from--this may be not for you, but for those in the program that are familiar with the construction industry, and Garry's pointing at Ed; whether Ed Cording would have a thought on whether there's carbon tet as a possible contaminant in the tunnel.

DR. CORDING: I don't know of it being down in the tunnel. Rick's not here. I'm not sure they'd be cleaning it with that, but I really don't know what they have there. I wouldn't think they'd have it, but...

MR. DAVIS: All I'm suggesting is that there might possibly be a volatile compound that would have chlorine-36 in it that would arise from activities at the Nevada test site.

Now, I've been associated one way or another with the Nevada test site for, since the Year One, almost, and I have not run across anything that would give substance to this suggestion. It's just one of those wild things that I hope that I dismissed in my comments.

DR. LANGMUIR: And chlorofluorocarbons, CFCs would also, perhaps, be another way to do this, wouldn't they?

MR. DAVIS: If they're present in a high neutron flux area and get into the atmosphere, yes, but I don't know of any--I don't know enough about some of the operations that go on to say whether this is even a remote possibility. It might just be the zebra in Wyoming. I don't know.

DR. DOMENICO: I think the time constraints on us tell us that we should move on. Thanks very much, Stan. We'll get some more questions back at the open table.

We're now going to hear on the paleohydrology, especially, dating the calcite and opal deposits in the ESF by Zell Peterman and James Paces, USGS.

MR. PETERMAN: Before they put the Alpine miner underground, it was covered with a lot of muck from its previous usage, and they cleaned it with wire brushes and compressed air. As far as I know, there were no solvents. I think they're pretty careful about what they put in the ESF, at least that's my impression.

The key words here--well, let me say, first, Jim Paces, my colleague, and I flipped a coin and he lost, so I get to make the presentation and he gets to answer the questions.

The key words in the title, of course, are paleohydrology, age control, and ESF, and, as Dennis Williams mentioned yesterday, a large part of this study would not have been possible using samples from boreholes, because the fragile accumulations of calcite and opal in the ground mass were destroyed by the--when they were encountered by drilling. They just didn't survive.

We did invest some time on dating occurrences from drill core, and those results are probably always biased towards the old side, because the younger, delicate materials were, in fact, removed.

We've heard repeatedly at the meeting the last day and a half that one of the remaining key technical issues at Yucca Mountain is to try to understand how much water moves through the repository block, and, basically, the work we're doing is directed towards that. We're trying to provide a time framework, and isotopic understanding of the deposition of these minerals that occur in fractures and cavities in the repository block.

Pat, I think that geologic axiom you're search for is the present is the key to the past, but I like yours better, the past is the key to the future, and I think that's more appropriate for the type of work we're doing. The only, really, chance we have, we can understand the present day system, and it's absolutely essential that we do, but it's only a snapshot in time. We're never going to be able to observe the future, and all we can do is look at the past records and try to understand how those may relate to the future.

This is just a definition of paleohydrology I liked, and, of course, I agree with Chapman and McEwen's statement at the bottom there, that you have to understand the paleohydrology and the paleoclimate--the two are inextricably linked--in order to come up with any credible performance assessment.

In the rock mass at Yucca Mountain, there are paleohydrological records, and elsewhere, too, not only in the rock mass. The ones we're worrying about are the number one, it says here, low temperature mineral deposits, mainly calcite and opal, in fractures and cavities. Other types of paleohydrological records, you've heard about ancient spring deposits from Mike and Rick this morning, and, of course, the lake and playas, alteration zones in the rock mass that may relate to past alteration.

That last one is just a conceptual thing that I stuck in there, and I'm not aware of any perched water that's in the repository block that relates to past high water stands, but, of course, it's a possibility.

The dating program that I'll describe had been going on at a fairly modest level, and in November, this last November, Yucca Mountain Project decided to accelerate the dating, and it was a two-pronged effort. Los Alamos was instructed to increase its chlorine-36 dating in the ESF, and we were instructed to increase our uranium series, Carbon-14 and isotopic studies in the ESF.

It wasn't just something that, you know, just randomly happened. The idea was that we knew we had the physical records of deposition, the calcite and fracture filling; whereas, the chlorine-36 people could extract the pore salts, which represented the pore waters in the rock mass, and so, a part of this dual approach was if there were zones where there was fluid flow but no physical deposition, then that would be picked up, or possibly picked up by the chlorine-36 work, so these were mutually-supportive approaches.

The objectives of both studies, I think, one major objective, of course, is to come up with some sort of estimate or some sort of bound, independent bound on the amount of water passing through the block.

Now, yesterday, we heard--I sort of kept track of the estimates. If we take Ed Kwicklis's minimum estimate and Alan Flint's maximum, you know, we had six orders of magnitude range in percolation and infiltration. I am 100 per cent confident that we can hit that target. I think we can do better.

Our primary data gathering objective is to produce a credible time framework of deposition, and in terms of the calcite and opal, to relate their ages and isotopic attributes to what may be happening at the surface of the repository block.

The materials that we have to work with, as I said, are calcite and opal. Calcite's simply calcium carbonate, calcium and carbon both being major constituents, major ions, bicarbonate, major anions and cations, and in any of the waters, and then opal, which is a hydrated silicon dioxide, more or less, amorphous mineral.

These minerals are common at Yucca Mountain. They were deposited from water in the unsaturated zone. The important thing is that they contain information about their times of deposition. They contain uranium; therefore, we can do uranium series dating. They may contain Carbon-14. We can do Carbon-14 dating, and they contain isotope systematics, which give us information about the waters from which they were deposited.

And then, finally, the mass of calcite and opal per unit volume of host rock is in some way related to the past water flux. It's just going to be somewhat difficult to determine that connection. The depositional rate is equal to the flux rate times some complex function. We don't fully know what that complex function is yet.

I'm going to give the conclusions right up front, and then if I run out of time, I can shut down and still we will have talked about these. I guess these should really be preliminary conclusions, because the study is only partially through.

We have been meeting and collecting with the Los Alamos group ever since the work started, and I think, collectively, we've come up with this concept; the chlorine-36, U-series, and Carbon-14 actually, in a very large scale, indicate a dual permeability system at a large scale, and that basically is the paths. There fast pathways where we see bomb pulse chlorine-36. These appear to be, at least in some areas, related in maybe a complex way with fault zones, and then there's sort of everything else, and that's where our information comes to bear, and in this everything else, we see a very slow deposition, and we would say very low percolation.

We get uranium ages, the youngest is 37,000 to greater than the range of the method. Carbon-14 ages, 16,000 to greater than the limit of the method, and in these zones, June sees her background chlorine-36/chloride ratios. So those are really two very critical conclusions.

There are some lesser order conclusions which we think are important in terms of understanding the style or mode of percolation. One of these is that we never see any evidence that the fractures that were carrying the water were ever filled with water. The deposits are always on the downhill side. Gravity did, indeed, work in the subsurface, and if these fractures and cavities that would have been filled, we would expect deposits, say, around lithophysal cavities and on both sides of fractures, and we don't see that.

Information that we have from the dating work says that depositional rates were exceedingly slow, micrometers per thousands of years, indicating to us a low flux.

Mineral textures suggest to us that these are low-volume water films migrating down fracture surfaces and into cavities, and, finally, tracer isotopes indicate some modification of the infiltrating waters, but still give us a connection with surface conditions.

Now, we've had a number of collecting trips to Yucca Mountain, and, of course, when we collect, we also observe these features, and we think there are some very important physical observations that we've made, and some of these were just embodied in the previous slide on conclusions.

You generally see in deposits in fractures or cavities with significant openings. You don't see very many thin, very thin fractures that are full of calcite. You see a lot of thin fractures. A lot of them are high temperature fractures, and they date back to the cooling of the rock, and they have little thin, white veins, but those are high temperature minerals.

Oftentimes, you can trace a single fracture across the wall of the ESF, and it will vary in terms of openings. The narrow intervals will have little mineralization, whereas, the wider zones are mineralized, and they occur on the lower sides of the openings. Deposits tend to be thicker and more complex on low angle features, and we never seem to see any high water marks that would indicate standing water in these features.

So, we figure that these observations tell us something about what was happening. As I have mentioned before, and as Dennis mentioned yesterday, the deposits are very complicated, very delicate, often very delicate.

Now, this particular specimen, this photograph was taken under ultraviolet light, so the opals, which may have 50 to 300 ppm uranium, fluoresce green, and the calcites are not as flamboyant. They fluoresce kind of a soft blue, but this is a very interesting specimen. This specimen was donated to us by Clark County, but it does have a USGS QA pedigree, so that's sort of a private joke, but Englebrecht understands it.

These are thin stocks of calcite, with what our people have been calling suckerhead calcite, or these enlargements at the surface, and then there's these little droplets of opal on top of the calcite, and Jim and his group--and if I didn't say so already, this is very much a team effort, and there are about eight people that are heavily involved in the dating work and the isotope tracer work, and I think most of them are here today, and I just don't have time to mention names, but they're all sitting at various places out here.

Anyway, Jim tells me that they've got about five ages now on this particular specimen, and there seems to be about 200,000 years of depositional history embodied in these different parts of these very delicate features, delicate mineral accumulations.

As you look at these things in even greater magnification, they only become more complex, and these are SEN photographs of one of those little calcite stalks. This is the scepter head, and then it's sort of lying flat now. It's been broken off. This is calcite. These are SEN photographs. That's a millimeter, 100 micrometers, and so forth calcite. There is a layer of opal here, and then, on top of that opal, there is new calcite growing in these little euhedral crystals.

This is the contact between some of this calcite and the opal substrate, and then this is even a greater magnification here, where you see little spheroids of opal residing in these little holes in the calcite, so, exceedingly complicated. We'll never be able to sample at this scale, but, nonetheless, very interesting, and certainly fascinating.

Sampling is a problem. We have to microsample. People sample under the microscope using little dental picks, or dental-type drills.

As you saw in that previous slide, there's a submicron growth layering, or a very, very complex growth layering, so our age resolution is limited by the sample size we need.

We can get away with 10-20 mg for Carbon-14 of calcite. Calcite is very low in uranium, so it takes quite a bit more calcite to get a uranium series age. The opals are much higher in uranium, as I said, 50-300 ppm uranium, and they've pushed the technology down to as small as a tenth of a milligram, so, the smaller sampling equates with greater and better age resolution.

We have come to the conclusion, over the months now, looking at a lot of these, dating a lot of these, that even the smallest samples probably integrate over finite intervals of growth history, and, therefore, yield some sort of composite age. We're not dating discrete depositional ages, but we're getting composite ages. Nonetheless, the depositional histories can be established by these composite ages.

Here's another nice specimen. Maybe it's part of the same one. I'm not sure. This is a different occurrence. These are little blades of calcite, up to a centimeter long. One of the few types of occurrence which apparently shows some growth banding, which is hard to see in here, but there are these very discrete bands, but, apparently, they represent very long intervals of depositional time.

The ages that have been gotten here are the outermost part, and in order to get enough calcite for uranium series, these had to be composited, so a number of blades were broken off, and then these pieces were combined into single samples; 75,000 years on the outer part, the older part of calcite, 254,000. There are opal bubbles in here, or occurrences in here that get 96,000, so, here, this gives you an idea of the growth rates, and that's why we say very slow growth rates. Everything that we've done to try to pull these apart, we have to conclude that the growth rates, the depositional rates are exceedingly slow.

These are histograms of the databases that exist right now. The upper one is Carbon-14, the lower one are uranium series ages. There's no difference in age distribution, whether we sample in the lithophysal cavities of the fractures. Of course, the ranges of these methods are dramatically different. This is about the limit of the range of the thorium-230, uranium-234 method, and, of course, Carbon-14 has a much smaller range.

Unfortunately, this is one blank that was submitted. It comes out with a finite of 37,000, so, right now, we have to say anything older than 35,000 is probably not significant in the Carbon-14, based upon those blanks.

We see no systematic distribution of numbers so far in the ESF. These are the Carbon-14 ages, so, anything greater than 35,000, we'll just say that's dead carbon. ESF station in meters. These stipple zones are the zones where June has found the elevated chlorine-36. We don't see any spatial variation, a systematic spatial variation or correlation.

The same with the uranium series ages. These back here were mainly earlier determinations that were made before the start of the accelerated program. They were mostly from the Tiva Canyon. Our sampling at that time was strongly influenced by what we had found in the drill core. We don't think this trend is real.

With the accelerated program, we started somewhere right in here, and so these are basically the new data here. Again, we don't see any correlation with the zones of elevated chlorine-36.

I should mention that when you're looking at the fault zone specifically, the actual ruptures, we don't see calcite and silica deposited in those. They tend to be very tight features, so they fit in with our observations that you only find these in open fractures, but we can't date material exactly from the fault planes themselves.

One interesting outgrowth that's come out of the uranium series dating, here's calculated initial ratio of 234/238 against station, and there is a very good correlation, there is a correlation here, anyway, in the shallow levels of the Tiva Canyon.

The initial ratios are very much like the ratios in the surficial calcite, calcretes, the pedogenic calcites; whereas, you go down section, you're starting to get very large initial ratios, and, basically, these are starting-- well, the other place we see initial ratios is in saturated zone groundwater, which just means the waters are picking up these elevated ratios from the rocks themselves, so this could be an indication of progressive water/rock interaction as we're moving down stratigraphically in the section.

We think the stable isotope work is really just starting on some of these, or the radiogenic isotope work, but the tracer isotopes appear to link the subsurface deposits with surface conditions. We also indicate some sort of past temporal variability in these conditions.

The next slide I'm going to show you now, I show you age, but it's only relative. These samples have not been--the particular samples used for the isotope tracers, there is not really good absolute age control yet, but we will try to get that eventually, and what I was sort of mumbling about through that slide is this variation here, where we see, you know, some sort of coherent signal between the strontium isotopic composition and the carbon isotopic composition.

I think the last page of your handout, in a series of supplemental slides, it also shows oxygen, and we were somewhat surprised, but very encouraged that we would get some sort of relationship like this. It tells us that there is a signal there, and all we have to do now is to be clever enough to figure out what this means with regard to what was happening at the surface when these waters that deposit these minerals acquired these parameters.

And, lastly, of course, one of our objectives is to try to constrain, as I said before, the water flux, and, really, what we plan on doing, we're going to buy many copies of Don Langmuir's new textbook, and he's going to tell us how to do it. Now, remember that "many copies" when you start to ask me questions.

It's not going to be easy, but I think we have to make simplifying assumptions, and we have to have a shot at it, and we've just started to try to estimate the abundance of calcite and opal in the ESF. We know for certain that it's spatially inhomogeneously distributed. That would certainly fit in with other observations that the flux is also spatially inhomogeneous.

We have to look at the compositions of water that have been determined by Al Yang and his group, and others. We have to think about, you know, what sort of evaporative concentration does it take to cause deposition, and we have to deconvolute the ages and come up with an age distribution model, and then, eventually, down here, come up with estimates.

Brian Marshal, just yesterday, did some speciation of existing water compositions. These are all unsaturated zone water, and, in terms of calcite, they're very close to saturation. Some appear to be oversaturated, and some appear to be undersaturated with regard to calcite. Most of them seem to be close to saturation, but some are undersaturated with regard to silica, and we have to pursue this approach, but that's pretty much what we've done to date.

I think that's it.

DR. DOMENICO: Thank you very much, Zell.

Any questions from the Board? Don?

MR. PETERMAN: Just wouldn't leave it alone, would you?

DR. LANGMUIR: Just a thought. We've been going back and forth on the possibility of evaporation from Al Yang's work, separate conversations that the matrix is full of water, so the water, if it goes down a fracture zone, doesn't exchange with the matrix.

This suggests to me that evaporation, if it's happening, is very subtle, and is within a per cent or two of saturation, moisture content. It's close to saturation if it's going on. It's a very subtle effect, which made me think about another way to do this.

Calcite and silica tend to be mutually exclusive in terms of the pH effect of solubility, as you know, and you get one or you get the other, often. If the pH is increasing, you're going to dissolve the silica, but you're going to precipitate the calcite. You can get this effect by subtle changes in CO_2 pressure at depth. We would shove you back and forth across the calcite silica lines, giving you, maybe, this intimate crystal growth that you've got, and this could happen best where you have the possibility of some breathing, doesn't have to be much, just a complication other than simply evaporation.

A change in CO_2 pressure will do the very same thing, and might be more likely in a breathing system that's near saturation.

MR. PETERMAN: Well, this is the kind of thinking and information we need to work into this, and our feeling that we need evaporation stems from the observation that we never see--never is, you should never use never in geology. We rarely see filled fractures. They seem to require some head space to get the nice deposits.

DR. LANGMUIR: Well, maybe you need to have a void that contains some fluid from which to make the precipitate. If you don't have enough fluid in one spot, you won't make anything by the breathing. If you've got a little pool--

MR. PETERMAN: I think you have to continually recharge these by this very thin film.

DR. LANGMUIR: That, too, overflow them and pool them out.

MR. PETERMAN: Right.

DR. LANGMUIR: The other thing, other question, unrelated, are you finding your uranium isotope dates are consistent with your Carbon-14, because I noticed you had a factor of two in the apparent ages due to uranium isotopes at least in one example.

MR. PETERMAN: No, that's a good question, and that's--

DR. LANGMUIR: At some point, you've got to bring this to resolution at some point.

MR. PETERMAN: Right. That's partly addressed in those supplementary slides. I think what we're thinking is because of this fine scale layering, we can't sample on a layer-by-layer scale, so we're integrating over a growth history. So, by that integration, we'll bias our ages a bit.

Like if you had two layers, and one was 100,000 years and there was one on top that was zero years, and you sampled them for Carbon-14, but you could only sample them together, the mean would be 50 per cent modern carbon, giving you a 4,000 year age for the collective sample, but the true mean age would be 50,000 years, so there is a bias there.

This is something we want to work on. If we could bring in systems that have different half-lives, that discordancy is predictable from what we call a continuous deposition model, where you have to sample finite thicknesses. We can bring in different methods, like protactinium and radium, with different half-lives. Theoretically, we would expect very systematic discordances, and that's something we want to try to do next year, but that's a problem. We're never going to be able to sample on that monomolecular scale, or submicron scale.

DR. DOMENICO: Zell, I have a question. Can you put on Slide 19, because you went through that one quite rapidly.

MR. PETERMAN: Could you tell me which one that is?

DR. DOMENICO: Estimating water flux, I think it is. I'd like to take a minute with these, because, like I said, that was too quick for me.

Determine abundance of calcite and opal. That's an observation, and I presume that's going forward now?

MR. PETERMAN: We started last week or the week before doing some just line surveys, and measuring the intercepts of fracture fillings and cavities. That's the way we'll do it, just like you would point count a slide.

DR. DOMENICO: Let me get to the next point: Determine level of mineral saturation in possible input water. You say that you're undersaturated with respect to silica, and you're slightly saturated with respect to--

MR. PETERMAN: The available data that--Brian Marshal ran this through PHREEQ-C, to speciate the chemistry, and these are the results. He just did it yesterday. It was only two or three or four samples that appear to be oversaturated in silica. I mean, they're all very close. Everything's close to saturation.

DR. DOMENICO: The third bullet, I guess the bullet says that in order to bring it up to saturation, you require a certain amount of evaporation. Why couldn't you be scavenging or dissolving those minerals from the rock in the early pathways and bringing it to saturation that way?

MR. PETERMAN: This is, I think, a real possibility. We have some indication. Larry Benson reported in a paper in '86 or '89, or somewhere in there that he had measured actually surface runoff, and even it was saturated in calcite. You know, it's such a calcium-rich environment out there at Yucca Mountain at the surface, I think that first drop of water that hits the ground, it practically becomes saturated at that point. Silica's another matter.

DR. DOMENICO: But you still have to get some silica to bring that up to saturation.

MR. PETERMAN: Yes. The problem with, you know, dissolution and redeposition, I think probably happens. In these growth surfaces, these outer surfaces, you saw many of them are very pristine. You don't see evidence of corrosion.

In some of the lithophysal cavities at the bottom of the deposits, there is some indication that there may have been dissolution, and I think you sort of have to conceptualize the thing from the bottom to the top. I think it would be unreasonable to think we have a drop of water that comes in at the top, and we've got a calcite fracture down here, or a fracture down here, and that drop goes all the way down here before it precipitates. That's not a reasonable model. It has to be a dynamic system. There has to be some solution and redeposition.

I would hope that, on the average, if we could cut planes through the rock mass and sample effectively those different planes, that that would somehow, I think that some sort of a dynamic equilibrium would tend to average out. We're still getting good age records of what's moving through the rock mass. That's my feeling. It's a conceptual feeling more than anything else at the moment.

DR. DOMENICO: That fourth bullet, the word "model" confuses me. I imagine, by observations, you can establish the age distribution of calcite and opal in the deposits. That's an observation and a measurement. What model are we talking about here? Establish age distribution model.

MR. PETERMAN: I think we have to understand a little bit better this built-in bias that these very fine scale samples pose in terms of the isotopic ages. As I say, you know, we're hoping to address that by bringing in some other chronometers. That's one thing that we have to sort out a little better than we do now.

I think we have to spend more time--right now, all of our sampling, most of our sampling has been on the youngest materials present, because we started out feeling that those were the most important. We have not spent a lot of time going deeper into the deposits to try to understand the whole growth history, and we think probably some of the old opals, we may even be able to go in and do conventional uranium lead dating. I think that's what we meant.

DR. DOMENICO: Based on that, correct me if I'm wrong, but I think it would be possible for you to calculate some pseudo reaction rate coefficient like the volume reacted per unit volume per unit time, when you combine that with your age dating. I don't know what that would mean, but it might mean something in terms of transport modeling if you're looking for, like I say, I would call it pseudo, but it would be different for both the calcite and the opal, but I would think that, at that point, you should be able to get to that.

Now, to calculate the flux required to deposit minerals, how do you do that?

MR. PETERMAN: Well, in a very simple-minded way. We'll assume that the water is saturated. We'll determine the growth rate of the deposit. I mean, we've done some of these back-of-the-envelope calculations. We can do it for a fracture, say.

You've got so much material. You know the rate of accumulation. How much water does it take, reasonable water, like the waters that have been analyzed, how much water would it take to deposit that amount of material? I think that's what we're looking at.

There are all sorts of complications, like Don has alluded to, and other ways we can look at this, but that's where we're headed. As I say, we're going to have to make a lot of simplifying assumptions, but, you know, that's no sin in geochemistry and hydrology. It's done all the time, but that's where we want to go.

Now, the numbers that we've looked at, they're certainly in that six orders of magnitude range that we saw yesterday, so we're not totally out of the ball park. How we might--I think that you bring up another important question.

What does a distributed flux mean to the repository block? It may not mean much of anything. If these two, two of these gross dual permeability domains are really true, you really have to try to map out these things in two dimensions, three dimensions, and also with regard to time, and, of course, the modelers are going to produce the maps. They're going to cut slices through the block, and they're going to show us where they think the maximum and minimum flux may be, but we've got to provide some hard data to keep them honest; otherwise, they start to believe their models. That's very dangerous.

DR. DOMENICO: Would you accept this as a possibility, that the reason you don't find calcite and opal in those structural features where you do find the chlorine-36 is because the water was moving too fast?

MR. PETERMAN: Too fast for deposition? I don't think we could dismiss that possibility. The other thing is we don't see those open fractures right there in the sheers themselves. They tend to be pretty tight, pretty close features, and so, if our idea that you need a little head space for evaporative concentration is true, you don't have that head space in these fracture zones, yeah, that's true.

Of course, that's why we did this dual approach; date the physical record and do chlorine-36 for exactly that reason.

DR. LANGMUIR: Pat, could I add a complication to the whole thing, just quickly?

DR. DOMENICO: No; just no.

DR. LANGMUIR: It just occurred to me, Zell--I'm sure it has occurred to you both before--what you may well be looking at, as we've just said, you're coming down through here and, almost certainly, you're precipitating silica and carbonate, and redissolving it and reprecipitating it.

You're not looking--you're looking at a bounding oldest age. It may well be a heck of a lot younger, for the water itself going down through the system may be far younger. You're looking at the time it took for those isotopes in whatever mineral they were in to get down there, and it's probably been a series of stops and starts and stops and starts, so this isn't really an infiltration rate you're looking at, it's a migration rate for these isotopes in the minerals.

MR. PETERMAN: No, you're right. It doesn't tell us anything about the travel time.

DR. LANGMUIR: And the chlorine-36 is a better indication, where you find it, of the age of that water, because it's better conserved in the fluids; whereas, reacting elements don't tell you that, because they don't stay in solution all the way down.

MR. PETERMAN: That's right. We're looking solely at the depositional records.

DR. LANGMUIR: You've got to find some other tracers.

DR. DOMENICO: I'm going to have to move us, because these are good points that we should remember for the round-table this afternoon, to bring this up again, and I think our last presentation of the morning is the hydrologic flow paths and rates inferred from the distribution of chlorine-36. That's June Fabryka-Martin and Andrew Wolfsberg, I believe.

MS. FABRYKA-MARTIN: I'm June Fabryka-Martin. I'm the principal investigator of the water movement test, which also is more commonly known as the chlorine-36 study, and, also, my co-presenter today is Andy Wolfsberg, also from Los Alamos, a hydrologist who works with me very closely, particularly on the ESF work we've been doing.

What I'm going to be talking to you about for the next few minutes is our work in the ESF, which, as Zell described, started mid-November, and I'll be talking about what the objectives of that study are, our approach in collecting the samples or selecting sampling sites, present the data and our preliminary interpretation of those data, talk about the implications for our understanding of the unsaturated zone's hydrologic system.

Andy then will compare our interpretation with transport calculations to show how consistent that interpretation is with what we know about hydrologic parameters, and, finally, we'll end with some conclusions.

In November, the objectives of the study in the ESF that we came up with was, first of all, to evaluate the extent to which the nonwelded unit, Paintbrush nonwelded unit, is an effective barrier to vertical flow; secondly, to provide bounding estimates for the travel time of water in the matrix of the Topopah Spring welded unit at the repository horizon; and, finally, to evaluate the frequency and distribution of any preferential flow paths that we might find.

We're not completed with achieving these objectives yet. This is just a status report I'll give you today. I would estimate we're about half through, at least for this phase.

Stan talked a little bit about sources of chlorine-36 in the hydrologic cycle, and I'll just reiterate some of these points. The two signals that we're most interested in are the anthropogenic sources of chlorine-36 from the global fallout resulting from nuclear weapons testing in the late fifties and early sixties. Also, there may be a component from local NTS activities, although I haven't seen any strong sign of that for chlorine-36.

That will give us signals. The peak for a global fallout may have been up as high as 200,000 x 10^{-15}. Compare that against the background, present day background of 500, so it's a really massive signal when it's present, and this will be dominant, of course, in young waters.

The other major type of chlorine-36 we're interested in is just that which is produced naturally in the atmosphere, just like Carbon-14, just like tritium, like your interactions with cosmic rays with atmospheric gases, and even though the present day ratio might be 500 x 10^{-15}, we do have evidence from packrat middens that it may have been as high as 1500 over the past half-million years, and this will be the dominant source in pre-bomb waters.

But, in interpreting the results, we have to also be aware of other sources of chlorine-36 in the hydrologic cycle. Specifically, as Stan talked about, there is production from cosmogenic reactions on rocks and minerals near the surface, and one that many of you have already heard about and been discussing among yourselves, I'm sure, is the production on calcite, and whether or not that calcite then releases the chlorine-36 to be carried down to the water.

This is going to be a variable input function. It's going to depend on the exposure age of the mineral, how deep it is, what the elemental composition is, how much chloride is present that can dilute the signal, and my feeling--although some may argue with me--is it's probably negligible relative to those atmospheric sources.

And, secondly, in deep rocks, there is a continuous production of chlorine-36 just because there's a neutron flux everywhere due to the presence of uranium and thorium and their decay products. At Yucca Mountain, the calculated ratio will be on the order of 20 to 30, far below either of the atmospheric sources, and this is generally negligible in the Yucca Mountain system.

Now, our approach for the ESF study is we use three sampling criteria. First of all, systematic sampling every 200 meters, boom, we would collect a sample, and we've got about 24 so far, of which we've analyzed 13.

The second category, most of our samples were what we call feature-based sampling, and these were ones that were generally selected in close coordination with the USGS, and we were looking at things like fractures, things that looked like they might potentially be fast paths, so fractures and faults. We also, on purpose, were trying to find places where we expected to see old chlorine-36, and we weren't successful at that, but we looked, and that's why it also includes lithophysal cavities, for example.

And then the third category of samples within the PTn we sampled at subunit contacts to see if those contacts, or changes in porosity, for example, or changes in hydraulic permeability could be contributing to, say, lateral diversion of flow in the PTn.

And you can see how we've collected about, oh, 153 samples so far, and five or six field trips--about every month, we go out--of which we've analyzed about a third, and another third of those are waiting at Purdue, waiting to be analyzed now. We expect to have results by the end of the month, or early August.

Now, the results. What I've plotted here is the chlorine-36 to chloride ratio x 10^{-15} as a function of distance from the north portal, station zero, so the stations are every 100 meters, so 10 would be 1,000 meters into the ESF, and then, also, I've used two different plotting symbols here.

The systematic samples, the one that are every 200 meters are plotted with the solid black squares. The feature-based samples are plotted with the shaded squares, and I want you to pull out two observations from these data.

First of all, one thing that probably jumps out at everybody right away is that there's two distinct populations here. We have these fairly sizable spikes going, the ratio is almost as high as 4,000 x 10^{-15}, and these we're interpreting as bomb pulse chlorine-36.

Then the second thing to pull out from it is how few of the samples fall below the present day meteoric ratio of 500. The second population is a band where most of them fall between 500 to 1,000, or certainly 500 to 1500 encompasses all of them. Those I'm not going to talk about in much detail today, except to say that this is consistent with a variable input function that we've been able to reconstruct to some extent by looking at packrat middens for the past 30,000 years.

It's the bomb pulse signals that sparks the most interest and debate and discussion, and so those are what I'll focus on for the rest of the talk.

What I have here is, using Warren Day's preliminary map of surface faults, the dashed lines. Also, on here I have the bedrock alluvial contacts, also taken from his map, and overlaid on top of that, the ESF, with the stations marked every five stations, and the red circles represent places where we saw the elevated chlorine-36 and the chloride that we're interpreting as being bomb pulse.

What you should notice from here is that two of the locations, the Bow Ridge Fault and Drillhole Wash Fault, the high signals seem to be clearly related, or, at least, this is very suggestive that they're related to the--the high signals are related to the fault themselves, but the other three, the relationship is not at all that clear.

But, a more relevant way of looking at the data is --and I'll skip over to here so you can see both at once-- is to look at those same fault features mapped at the depth of the ESF itself, and this is taken from a preliminary map provided by Steve Beason, and, again, plotted the data, well, from the north portal at station zero, going through our current location of sample results, up to station 40, and showing the places where we see the bomb pulse chlorine-36 with the red squares.

And, here again, it just reiterates the point I made with the previous slide, that there seems to be a clear relationship between the fault structure and the bomb pulse signals for the Bow Ridge Fault and the Drillhole Wash Fault, but the relationship of these features to pathways is not quite as clear, and so, our conclusion, or our tentative working hypothesis right now is that if these are related to fault features, it's not a very direct relationship.

We think what's happening is that probably the faults may be important in getting the signal, the bomb pulse signal down through the PTn, but after that, it just takes the closest pathway it can find, because most of these are not in faults at all; actually, in the ESF, but, rather, in cooling joints and features like that.

Now, the bomb pulse interpretation is clearly significant, enough that it's important to get independent lines of evidence for that interpretation because of the implications, so these are the approaches we're taking to corroborate that signal.

First of all, we're evaluating sources of contamination. A lot of our samples do have construction water present in them. You can see it easily because of that bromide tracer that's added. However, the effect of that construction water is not to increase the chlorine-36 to chloride ratio, but, rather, actually, to decrease it, so when I correct for the presence of that construction water, that correction actually kicks the ratio up, because it's J-13 water. It has a ratio of 500 x 10^{-13}.

And, in addition, to check for lab contamination, of course, we have QA/QC measures. We have a blank that goes along with every batch of samples. We've never had any trouble with the blanks for the samples I've reported.

Secondly, we're evaluating surface calcite as an additional source. We're taking two tacks there; first of all, just doing theoretical calculations to try to bound the contribution of chlorine-36 from this source; and then, secondly, our GS colleagues helped us select sampling sites for calcites from a variety of locations, and we're in the process of analyzing those so that we can see what that ratio really is.

Thirdly, we're trying to reconstruct the past chlorine-36 to chloride signal in the atmosphere to see how high it could have been in the past, and based on the packrat midden data that we have so far, our highest ratio is 1300. I don't think we're going to get anywhere near 4,000, so I think we'll be able to rule this out as an alternative hypothesis for those high signals.

We're looking hard at field relations of the samples, and the mineralogic features of the sampling locations, particularly those with the elevated chlorine-36 to chloride ratios to see if there's other evidence for water flow and movement, and it's early days to draw any conclusions from this as yet.

We're working closely with Alan Flint to see what correlation there is between the net infiltration estimates he's come up with, and where the high signals occur.

And, finally, we're also looking for other bomb pulse nuclides, and I'll show you next some results for tritium, cesium, plutonium, technetium-99 and iodine-129.

First, the tritium results. Both these sets of data for the bomb pulse nuclide results, you shouldn't view it as the final word. It was just really just scoping studies to see whether it was feasible to continue. Now, with the tritium results, these are all GS results and just in the interest of saving time, we agreed that I should present them.

The first group of samples are from the ESF main tunnel, collected by Alan Flint and Joe Hevesi, and what you'll notice right away from here is that the tritium is all below detection. These are all picked from locations that had the high chlorine-36 to chloride ratios. However, Gary Patterson has radial boreholes in Alcove 3 that are in the Tiva Canyon welded unit, and you can see that four of those five samples had measurable tritium levels.

And so, there's many different hypotheses you can come up with to explain this, but the one that we're thinking is most likely is that these collected from the tunnel walls have been diluted by J-13 water, the construction water, to such an extent it's just diluted out any bomb pulse tritium that may have been present, and so, what we're doing for the next round is drilling into the walls five to ten feet, to be collecting samples, then, further back in away from the construction water influence.

Then for the other bomb pulse nuclides, I selected a suite of samples on purpose to try to maximize the possibility of seeing technetium, cesium, and plutonium if it were all present, so this is just sort of like a proof of principle. It's not supposed to be an unambiguous indication of whether or not the chlorine-36 is bomb pulse or not.

And so, I picked the Bow Ridge Fault at Station 2, and there is a sample from Borehole N55 that had extremely high chlorine-36 to chloride ratio. We saw technetium-99 in both of those samples, which I think is pretty strong evidence that there is, indeed, bomb pulse chlorine-36 at those locations as well.

We did not see cesium-137 or plutonium in those deeper samples, but we did see it in the surface soils, and these distributions, of course, are consistent with how we expect the geochemical behavior of these isotopes to be, meaning that we expect cesium and plutonium to be hung up in the upper surface and not be mobile, and technetium-99, on the other hand, as an anion, we would expect it to be mobile, just like chlorine-36. So, we're going to be continuing processing of additional samples now from the ESF for technetium-99.

The other isotope we're looking at, iodine-129, we've sent samples off to Purdue University for analysis, and I hope to have results back on that in the next couple of months.

Finally, the implications of these elevated chlorine-36 results for our understanding of the UZ hydrology, first of all, that bimodal distribution of the ratios demonstrates that there's isolated fast paths from the surface to the ESF. I think it's pretty conclusive.

Secondly, the penetration of recent water into the Topopah Spring welded unit is indicated by the bomb pulse chlorine-36 in the fractures. However, it's important to realize that these bomb pulse signals by themselves say nothing about the flux. In fact, the flux is likely to be small or negligible, but you can't quantify it based on this result alone.

Thirdly, the fast paths that carry water into the TSw may be associated in some way with major fault zones that can cut through the PTn.

And, finally, transport calculations that Andy will be talking about following me, indicate that the arrival of the bomb pulse chlorine-36 at the ESF is consistent with the increased fracture permeability in the Paintbrush nonwelded unit that one would expect to be associated with faults.

And, with that, I'll turn it over to Andy to show that.

DR. DOMENICO: Let's hold the questions until the completion of the presentation.

MS. FABRYKA-MARTIN: Right.

MR. WOLFSBERG: When I heard that Ed had 30 view graphs yesterday and Alan had 40, I thought I'd shoot for about 50 view graphs, but June and Jill asked me to keep it to five, so I'll keep the number down so there'll be lots of questions for June.

What I'm going to be talking about is the chlorine-36 transport simulations in support of the interpretations of the data that June's just presented. The objective of the study is to develop a quantitative conceptual model of the movement of chlorine-36 from the surface down to the ESF. That'll help us go through our evaluation and analysis of what do these signals mean, and what are the flow paths and mixing and mechanisms associated with the measurements that she collects.

The methodology that we're using involves one-dimensional and three-dimensional transport simulations. The 1-D simulations are used to really focus on what the mechanistic processes are, what the fracture matrix interactions are, the difference from location to location, and I'll be talking in a few minutes about the impact of the thickness of the PTn and the spatially variable infiltration effects, both spatially, variable, and transient, as Alan was talking about yesterday.

Then we have three-dimensional transport simulations, which I won't be able to get into today, but they extend what we do with the 1-D simulations to examine the lateral flow effects at material boundaries, the full effect of the spatially variable infiltration, and the effect of the full fault system.

Now, June mentioned that there's indication of a variable chlorine-36 production rate through time, and we're using this as our input signal to the model. It's based on some theoretical development associating the production of chlorine-36 with the geomagnetic flux variations through history. It's somewhat substantiated through packrat midden samples and some work that Scott Tyler has done, but this is, as Stan mentioned, it's emerging research. We're using, effectively, one realization of a calculation of what the chlorine-36 production signal through over the last, well, I could go out to two million years.

But, what I want to point out, that you'll be seeing in the simulations, is that here we are at present with the present ratio of 500 x 10^{-15}, and when we go back 50,000 years, we may be dealing with a higher chlorine-36 production rate, as high as close to 1500.

To start the study off, what we did is we did a three-dimensional calculation where we used that input function, and we looked at where the bomb pulse deposited itself. This is a three-dimensional simulation. We don't have faults in this model, so there's no fault zone properties, and this reddish-orange color that you see is at the Tiva Canyon/PTn interface, so this sort of confirmed our initial hunch that the stuff moved quickly through the Tiva

Canyon, and moved into the matrix of the PTn, and deposited itself there.

Throughout a huge portion of time, we end up with a signal between 500 and 1500, which is consistent with the other non-bomb pulse signals that June has been measuring in the tunnel.

Now, the approach that we then went to is, okay, what does it take to get this stuff through the PTn, down to where June was measuring for bomb pulse in the tunnel, and that's when we moved to the one-dimensional column studies.

From our three-dimensional hydrostratigraphic model, we numerically took a bunch of boreholes. We're going to be focusing on this one right here. It's a location where June found bomb pulse chlorine-36, and we're going to be looking at the migration just vertically through a 1-D column there.

This is at Station 35. We're dealing with 21-some odd units of the system, all with hydrologic properties that are being developed by the various participants in the project, and we're going to be looking at the effect of property variations and the infiltration rate effects on what does it take to get bomb pulse down to the ESF.

What I've plotted here is a typical solution that we would calculate on a column like this. One of the things that we're very interested in is the pressure difference between the matrix and the fractures, because, depending on which is greater, we're either driving fluid from the fractures into the matrix, a process called imbibition, or we may be bringing fluid back out of the matrix into the fractures. That would be more consistent in yielding flowing fractures.

We're very interested in what happens here in the PTn. What does it take to get fluid to move through the PTn quickly? That's the bounding ladder, and that's the rate limiting determinant on whether we get bomb pulse to the ESF.

What we've plotted here is a solution for the chloride concentration in both the matrix and the fracture in one of these simulations, and, as with that 3-D simulation I just showed, we see the bomb pulse basically depositing itself in the initial matrix of the PTn as it encounters that.

As you move down the system in this particular simulation, the fractures of the PTn could not sustain flow through the entire thickness, and, therefore, the fractures effectively dry out and we don't have a continuous concentration profile through the PTn in the fractures.

Once we get into the TSw, the matrix acts as a source to the fractures, and we basically get input function from the matrix into the fractures, leading to a signal in the fractures there.

What I'm going to show now is this numerical experiment that we performed in looking at what does it take to get the fluid to flow rapidly through the PTn. This is by no stretch a Latin hypercube, but I think this is revealing, and indicates that the measurements are consistent with our conceptual thinking and what's occurring.

Away from a fault zone, we have a set of base case hydrologic properties. That's thinking of matrix and fracture properties throughout the system, and what we do is we're looking at infiltration rate versus property modification, so with the base case properties, we ran a variety of infiltration rates to see if we could actually penetrate the PTn and get the bomb pulse in less than 50 years down to the ESF. At .0 to 15 mm/yr, it didn't happen.

So, then, what we did is we started modifying just the PTn properties as may be consistent with the faulted zone, fracture density and fracture aperture, both of which lead to an increased fracture permeability, something which may be consistent with the pneumatic testing that's being performed now. I believe there's indications that in fault zones, the effect of permeability, and, therefore, the effect of air permeability and, therefore, the fracture permeability of the PTn may be substantially higher.

So, what we did is, we looked at increasing either the density or the fracture aperture, and what the effect of fracture permeability relative to the base case conditions were, and whether that led to bomb pulse arrivals at the ESF or not, and I'm just going to run through this real quickly.

What we see is that it's not uniformly consistent. When you increase the density, you have fractures that are closer together and, therefore, that increases the potential to bring fluid out of the fractures back into the matrix, but when you increase the permeability through an aperture increase, we start to see, under reasonable infiltration rates along the lines of what we've been hearing from Ed and Alan, a potential to bring the bomb pulse all the way down to the ESF.

So, this table basically goes through a set of examinations of what if there were some alteration to the PTn, could we get the bomb pulse to the ESF?

So, as June said, the implications of the chlorine-36 transport simulations indicate that the arrival of bomb pulse chlorine-36 at the ESF is consistent with the increased fracture permeability in the PTn, as may be associated with faults, and, with that, I think I'll stop. There's a variety of other things I could talk about, but I think it would be appropriate to have June come back up for questions.

DR. DOMENICO: Thank you very much, Andrew.

We can open up to the Board for questions either for June or Andy. Don?

DR. LANGMUIR: We've talked about this before, but I--both of you, I guess, but the back-of-the-envelope approach to this thing, I would have thought that what we would try to do, perhaps, for chlorine-36 to back out infiltration, at least where you find it, is to maybe do an in-depth cross-section profile of chlorine-36 across a zone.

And then, assuming Al Yang's average unsat zone water, get the volumes that you'd associate with those concentrations of chlorine-36 ratioed to the chloride, and that would then give you a volume of water that would make the trip.

Does that sound like a way to go back? Is that something that you might do? This doesn't address the issue of how to get it there, but, rather, how much water might be making the trip.

MR. WOLFSBERG: Well, yeah, and then we need to know, basically, the effect of the surface area with one of these zones.

DR. LANGMUIR: Well, you presumably have a certain chloride content in the given volume of rock that you sample from one of those zones, and then you've got the chloride ratio, the chlorine-36 ratio to that.

MR. WOLFSBERG: Well, in terms of the actual chloride ratio--

DR. LANGMUIR: Trying to back out a mass balance of quantities.

MR. WOLFSBERG: You have to leach that out of the rock; right? It has to do with the actual volume, the volume associated with the high chlorine-36/chloride ratio, but, see, to actually get that, they have to actually leach the chlorine-36 off the rock.

DR. LANGMUIR: Isn't that basically how you get your sample?

MS. FABRYKA-MARTIN: We don't measure pore water concentration, so we don't know what the chloride concentration is of the sample, of the pore water chloride concentration in the samples. All we do is take rock--it can be perfectly dry--and leach it.

DR. LANGMUIR: Don't you have, from Al Yang, for example, the fraction of moisture content in a volume of rock average for the different horizons?

MS. FABRYKA-MARTIN: Sure. There are moisture profiles.

DR. LANGMUIR: There's wide uncertainty, obviously, in all of this, but...

MS. FABRYKA-MARTIN: Right. There are moisture profiles.

DR. LANGMUIR: But as a way to go backwards to infiltration rate.

MS. FABRYKA-MARTIN: By chloride mass balance?

DR. LANGMUIR: Yeah, chloride mass balance, and to the inferred volume of water that contributed your chloride for the dating.

MS. FABRYKA-MARTIN: It's worth pursuing, sure.

DR. LANGMUIR: And then if you've got a fairly in-depth look at the chlorine-36, the width of the zone in which you get those bomb pulse ages.

MS. FABRYKA-MARTIN: Right. The way I view what we've done so far is like a phase one, where we're essentially doing a screening of the entire tunnel. Then, once the flurry of the fiscal year ends, we'll sit down with our GS colleagues and others and come up with some working hypotheses, and go back in the tunnel and do more intensive study at selected zones and actually test hypotheses, and one of those will be what is the width of the zone that's affected by those bomb pulses? What is the nature of those pathways?

MR. WOLFSBERG: See, one of the problems is they collect a sample in one of these feature-based samples, but it's not necessarily clear what the surface represented, or the surface was. I mean, you go through the Tiva Canyon, you may have a wider or a narrower zone through the PTn that you have move this, and so that's what we're working towards, is basically bounding what the column volume is that this flux is occurring through.

DR. CORDING: Cording; Board.

It seems that putting some dry drillholes out and doing some of that type of sampling across features, and then tying this in with the ambient degree of saturation and that moisture content, and, then, ultimately, maybe in the same locations, doing some more passive monitoring of flow conditions, so some of the things that Alan Flint's been talking about, and perhaps Lawrence Berkeley has been talking about, those sorts of things, putting it all together would seem to be a real benefit to getting towards this sort of thing, getting some of that other type of information can support getting at, perhaps, some of the flux rates, even tying it back to the chlorine-36.

MS. FABRYKA-MARTIN: I agree with you. I think both the GS and I are very excited about increasing our level of working together, because that's helped us make great strides forward in our understanding of the data, and we do have plans to do that for next fiscal year.

DR. LANGMUIR: And putting the plastic sheets on the walls, right, Pat, just for a second, to finish this. Alan's talking about putting plastic sheets up to collect the moisture, and then you really have, perhaps, the real moisture contents that have the chlorine-36 data in them. You don't have to infer much.

MS. FABRYKA-MARTIN: Right. You also have a better--an opportunity to get more valid tritium results, I think, as well.

DR. DOMENICO: June, I'm looking at this distribution versus the station, and like, for example, at Station 35, there's six hits for high bomb pulse. What does that mean? It's on the same station, but is that all from the same structural feature? These are duplicates, or it's a display, or what is it?

MS. FABRYKA-MARTIN: Oh, no. No.

DR. DOMENICO: Because they're all at the same station. I have no spatial concept of what's going on.

MS. FABRYKA-MARTIN: Well, actually, the--

DR. DOMENICO: And the same thing with Station 20 or so. You can see five or six hits. How about that sampling, are they far apart, or what?

MS. FABRYKA-MARTIN: Yes, they are. Around Station 35, each one of those tick marks on the bottom axis represents 100 meters, and so, the width of the signals that we saw around Station 35 is about 100 meters wide.

DR. DOMENICO: So these may be different structurally?

MS. FABRYKA-MARTIN: These are all individual features.

DR. DOMENICO: Structural features.

MS. FABRYKA-MARTIN: Yes, and I think in this case, they're mostly cooling joints. They're not faults.

DR. DOMENICO: And another thing you mentioned that you didn't want to talk about was a so-called variable input function. Can you explain what that means? And then you can answer me, you've ruled those out as bomb pulses; is that fair, or not?

MS. FABRYKA-MARTIN: Yes. The chlorine-36 to chloride ratio in the input signal, the atmospheric signal, has varied over time, and it's varied as a result of two different types of processes: One, the production rate of chlorine-36 in the atmosphere itself has varied in response to changes in the earth's geomagnetic field, and that's represented by the black line down here. This is a reconstruction of what the ratio would be just in response to the geomagnetic field strength alone, and that's what Andy used as the input for his modeling.

But then, also, independently, the ratio has varied because of changes in the chloride deposition rate. That one's harder to get a handle on because it must be a fairly complicated function of different climate factors, storm tracks, contributions from recycling of salts from dry lake beds, and so forth, and I've tried to show that by the dashed lines.

If the chloride deposition were, say, 60 per cent of what it is today, that's what that upper dashed line represents, and in confirmation of this hypothesis, the black squares are packrat midden results that we've obtained over the past few months.

DR. DOMENICO: I thank you.

My last question is have you used what we heard by Zell, where the opal does not occur where you find the chlorine-36? Has that sort of guided you a little bit, or could that guide you a little bit in some sampling procedures, instead of going every 100 meters like you have been doing?

MS. FABRYKA-MARTIN: You mean purposely go for fractures that don't have signs of filling, or a secondary mineralization?

DR. DOMENICO: Yeah.

MS. FABRYKA-MARTIN: That makes sense to me, sure.

DR. DOMENICO: Because it seems like with a random--it's not random. You do it every 100 meters, I guess, for the background--

MS. FABRYKA-MARTIN: 200, every 200 meters.

DR. DOMENICO: --but none of those seem to be showing up with anything.

MS. FABRYKA-MARTIN: Right.

DR. DOMENICO: So, seeing as you've more or less decided these are probably individual fracture pathways, it would seem like--

MS. FABRYKA-MARTIN: Right. There is a difference in the way those two types of samples are collected. I was just thinking as you were talking. With the systematic samples, we have the Test Coordination Office mine us out a niche, so if there's a fracture cutting through there, we may not see the bomb pulse anyway, because we've mined out a one cubic foot. So, maybe we should revise the systematic sampling and not only do a bulk sample, but, actually, the nearest fracture to that particular station.

DR. DOMENICO: That would seem a lot more reasonable.

MS. FABRYKA-MARTIN: That makes sense.

DR. DOMENICO: Are there any other questions? Don?

DR. LANGMUIR: One related to the variable input function data that you've got there, which, I presume, includes all of the data that's been obtained so far from surface-based testing down from the surface.

MS. FABRYKA-MARTIN: Right.

DR. LANGMUIR: Do you still believe the dates? In other words, are the corrections so significant, perhaps, that we can't have great confidence in the dates from that surface-based testing?

MS. FABRYKA-MARTIN: Which dates?

DR. LANGMUIR: We're talking about dates that ranged in the chlorine-36 scheme from maybe 30-40,000 up. How confident are you in the--what's the uncertainty in those?

MS. FABRYKA-MARTIN: Oh, I see. Okay, the samples I said I didn't want to talk about?

DR. LANGMUIR: Yeah, yeah, the ones you didn't talk about, which got us all excited because they were related, but not the same as the Carbon-14. We invented wonderful concepts of gases moving to make the 14 younger and the uranium, or the chlorine-36 being in the fluid. How are we on those dates now, given where you are in terms of understanding this?

MS. FABRYKA-MARTIN: I say we can't do better than bounding it, say, within an order of magnitude, at best. The original intent, remember, was the assumption that the input ratio was more or less constant at 500, and then we went into the ESF in full confidence that we'd find a whole slew of signals below that and just simply estimate ages based on radioactive decay.

And, as you can see, most of those ratios were well above 500, and that's completely consistent with what we now know is a variable input signal. So, the approach that we can take to try to draw as much information as we can from those pre-bomb ratios is these three steps I have here. It basically is just setting bounds.

We can establish upper limits for the travel times, and that's what I did in the March ESF report that I think most of you have seen, just by using the radioactive decay equation from the maximum possible input ratio, which I just assumed was 1500, and so that's going to give us unrealistically high travel times for most of the samples, but it is a bound.

Secondly, we can calculate travel times by transport simulations, and also, in that March ESF report, you've seen the results of Andy's tackling that problem using the reconstructed chlorine-36 to chloride signal, and that generally--he came up with ages between tens of thousand to, well, basically, tens of thousand up to almost 100,000, I think, for those various scenarios he showed. The more realistic ones are going to be when he has the 3-D simulations completed.

And then, finally, we can also get a lower bound on the travel time, constraining it by matching peaks in the reconstructed signal. For example, if we see a ratio that's about 1,000 in a sample, we can say, well, when was the last time in our reconstructed signal that there was a ratio as high as 1,000, and by that way, set a lower bound, as long as we can rule out bomb pulse, a bomb pulse component.

Does that answer your question?

DR. LANGMUIR: I guess what I'm getting out of this is the uncertainties in the chlorine-36 data from surface-based testing above the ESF are very large. The uncertainties are large. We're talking thousands of years, but that's maybe all we can say. Are you better off than that, or is that pretty much where we are?

MS. FABRYKA-MARTIN: Yeah. You're saying that the borehole data--

DR. LANGMUIR: When you say an order of magnitude, that's a pretty big effect on those numbers; 40,000 versus 4,000.

MS. FABRYKA-MARTIN: Right. That may be as best we can do, unless we can do a better job reconstructing the input signals and have more confidence in that. The variations in input signal, it's such recent results that I haven't quite worked out how to deal with the problems that you've identified as yet. We're well aware of them, but we're not quite sure how successfully we are going to be able to resolve them, other than the three things I've talked of here.

DR. DOMENICO: Let me spread this around a little bit. Was there a question?

DR. PALCIAUSKAS: Could you put up the map that follows, previous to the bomb pulse one, showing the ESF locations? The one previous to that.

MS. FABRYKA-MARTIN: Oh, the surface one? Okay.

DR. PALCIAUSKAS: Yes, the surface one. I have just two brief questions.

There were two signal point locations, and I presume you went back to try to verify those?

MS. FABRYKA-MARTIN: Right. We did go back and collect samples. We don't have the results yet, analyses back yet for those samples. They're at Purdue right now.

DR. PALCIAUSKAS: The other question is, could you, just briefly, describe how those five locations correspond to the surface infiltration areas?

MS. FABRYKA-MARTIN: Well, actually, it's interesting you should ask that. What I did, if I can find it, I took the surface map and made an overlay for Alan Flint's infiltration map, if I can line this up. Yeah, it's pretty close.

This is basically what you're asking, right, what the correlation is?

DR. PALCIAUSKAS: Yes.

MS. FABRYKA-MARTIN: And you can just barely make out the circles where the bomb pulse locations are, but here's the north portal entrance, and the thing to note from here is that all of our elevated signals are in zones of low infiltration fluxes, and this just drives home the point once again that a fast path does not equate to a high flux. All it means is that it's a fast path, that water gets in there fast, and, of course, we want to expand on pursuing this hypothesis in greater detail over the next few months or a year.

DR. DOMENICO: There's another question by Dick Parizek.

DR. PARIZEK: I was going to ask whether or not any of the stratigraphic units were crossed where you had Station 19 and 35, which was a chlorine-36 high value, but according to that cross-section, these were in big units, but you were talking about cooling joints being encountered at several of these stations?

MS. FABRYKA-MARTIN: Most of the bomb pulse signals were in cooling joints.

DR. PARIZEK: Now, that's inconsistent with what I thought I heard maybe several meetings ago, that cooling joints don't go anywhere. They kind of go up and die. They don't cross units. So, now you've got a problem. You've got to get water into cooling joints by some other path, since the conclusions is cooling joints don't connect with the surface.

MS. FABRYKA-MARTIN: Right. You're right. The cooling joints do generally seem to be constrained to the particular formation that they're in, the unit that they're in. However, what we think the pathway is, the role of the fault is just getting the bomb pulse signal through the PTn, and once it gets through the PTn, then it just moves laterally and takes the nearest pathway, which is going to be a cooling joint. That's our working hypothesis.

DR. DOMENICO: Question from Ike Winograd.

MR. WINOGRAD: I'd like to suggest twelve more samples, but I have no funding for such work.

Looking at that cross-section, the most likely place for rapid infiltration is in the Tiva in the broken zone. You have three samples there, two of which are pre-bound; two of three are pre-bound. Could you go back and take a dozen samples in there?

MS. FABRYKA-MARTIN: We've done it. We just don't have the results back yet. There are a whole slew of samples we have through that broken zone that we've processed, but--

MR. WINOGRAD: It should be the most liable to.

MS. FABRYKA-MARTIN: Right. Well, the other thing is, as some of you may well remember, we have a number of borehole profiles that we've measured, and pretty much every single borehole, the Tiva Canyon, where it had a thin alluvial cover, you had massive bomb pulse throughout the Tiva Canyon welded, and often, even going down into the PTn, so we have borehole evidence to support the penetration through there in many, many locations, but none of the other boreholes had previously shown anything reaching the TSw unit, and that's the major thing that was new about the ESF study.

DR. DOMENICO: We're ten minutes late, so I think we should adjourn for lunch and meet at ten minutes after one. How's that?

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. DOMENICO: Can we get started, please?

We're about to get into our afternoon session, and our first speaker is Tom Wigley, who I introduced yesterday, giving us the perspective on climate modeling uses and limitations.

MR. WIGLEY: Let me briefly summarize the topics that I'm going to discuss. Firstly, I'll say a little bit about what are climate models. Then I want to address the issue of how good are current climate models, how do we test the models to determine how good they are. How consistent are models when one compares different models produced in different institutions? How do we apply the models to the problem of climate prediction? And then I'll summarize some of the implications.

I'm not necessarily going to use all of the transparencies that are in my handout, and I may use a couple of others, because as this session has progressed, I've realized a little bit more where the emphasis ought to lie.

Firstly, let me just tell you what a climate model is, and there's a whole suite of climate models available of different complexities, but they all have one common characteristic, and that is that they are mathematical representations of a very complicated system involving interactions between the atmosphere, the ocean, ice masses and the land surface.

Nearly all of these models have to use computers in order to run them, and the most complicated such models are three dimensional General Circulation Models. Initially, these models were of the atmosphere only, but the most recent models include three dimensional structure of the ocean, as well as all sorts of interactions with the boundary that the atmosphere and ocean has around it.

General Circulation Model is then three dimensional mathematical representations of physical principles that control the behavior of the atmosphere and the ocean. There are two different types of model that are used currently.

One type of model that's been used for many, many years, a few decades at least, is Atmospheric General Circulation Model that simulates the motion of the atmosphere and the physical and dynamic processes. And that type of model in the simplest form is coupled to a very simple representation of the ocean that is referred to as a mixed layer ocean. It's just a simple layer of water that has no contact or communication with the deeper layers of the ocean. So one only gets the interaction with the upper layer of the ocean, and it's only possible to carry out so-called equilibrium experiments with these types of model.

In other words, we can change the boundary conditions, change the forcing, and allow the model to reach a new steady state, and then see what the overall change is. We don't have any time dimension in that type of calculation.

The other type of model is one where we couple the three dimensional atmospheric circulation to a three dimensional ocean, circulating ocean, and that allows us to look at time-dependent simulations that are very, very important in the context of anthropogenic climate change, but maybe not so important in the context of thousand year time change, as is relevant to the Yucca Mountain problem.

These types of model are the only credible tool to examine future climate change and make estimates of what those changes might be, simply because the interactions are so complicated that you can't just brain storm it and get a reasonable answer. You actually have to use some sort of mathematical model to cover all of these processes. And, of course, the models that exist have to simplify some of the processes, and many processes that we judge to be less important are not even included, even in the most complicated models.

The primary limitation of these types of model is their spatial resolution. There are problems with temporal output that I'll mention shortly. But the spatial resolution of most models, of models that cover the whole globe, is of order of hundreds of kilometers, and most applications of these models require intimation on shorter spatial scales. In particular, the area of the Yucca Mountain site is very much less than hundreds of kilometers, and so we have to employ some method to downscale from the coarse resolution of a global model to the fine resolution required for an analysis such as this one. And there are different methods for downscaling, and one method, the method that's been used by Starley Thompson, is to embed a high resolution model of limited area within the global scale model.

The other standard technique is to use statistical procedures to relate the larger scales, both the larger temporal and the larger spatial scales, to smaller scale processes, using observational data and then assume that those relationships hold in a changed climate.

This diagram is one that appears in a report of Starley's, slightly modified. This just shows the spatial resolution of the GENESIS model, and I've shown the area where the high resolution model is inserted, and that model is driven by the boundary results from the coarse resolution model.

Well, how good are General Circulation Models?

They are both good and bad. They can do some things very well, and they can do other things very poorly. There are some a priori limitations to these models, and the primary one, as I mentioned already, is the coarse spatial resolution that is really a constraint imposed by our computational abilities, by the power of computers that exist today.

Because of the coarse resolution, that means a lot of important details that affect precipitation on small spatial scales, for example, orography, vegetation details and so on, have to be simplified. In addition, processes that occur on scales from meters up to tens of kilometers cannot be represented individually, and they have to be represented in some approximate area or average way, and that process is called parameterization. And the most important aspects of the climate system that have to be parameterized in this way are those involving clouds, which clearly are much smaller than the hundred kilometer resolution of the model, and land surface processes, which can be very heterogeneous over the resolution of these models.

For future climate projections, another problem, and one that is particularly relevant in this case, is deciding exactly what the forcing of the model should be in the future. We can, of course, just consider natural variations and then change the characteristics and the seasonal and spatial distribution of incoming solar radiation and then force the model at different times in the future, but unfortunately we are already perturbing the climate system fairly drastically by burning fossil fuels and other anthropogenic influences, and those influences have to be concatenated with the future natural processes that might occur.

Just to give you one example, this is some work that was done a number of years ago using a complicated model, but not a full three dimensional model, but a model that allowed one to look at time variations on a thousand to ten thousand to hundred thousand year time scale, and this particular model was run in two modes.

In one mode, it was assumed that the boundary conditions for the large ice sheets in Greenland and so on would stay the same in the future. And then in another mode, the model was run by taking the Greenland ice sheet away. And it's quite likely, I think, that if we continue burning fossil fuels at the rate we're doing now, or at an increasing rate, then global warming will, on a time scale of about ten thousand years, cause the Greenland ice sheet to disappear.

And that radically changes the boundary condition for the atmosphere and changes the whole atmospheric general circulation of the northern hemisphere, and what this diagram shows is two different projections; one where Greenland stays in existence, and the other where Greenland disappears. And the variable here is a proxy indicator of global mean temperature.

So the time scale here is from zero to 80,000 years into the future. The full line represents essentially the global mean temperature fluctuations that might occur if Greenland stayed there, and you can see there's this steady cooling down to a minor period of about 20,000 years into the future, and then a major one about 60,000 years into the future. But if Greenland disappears, or is taken away instantly at the start of the simulation, then there is no cooling for 20,000 years, and it takes 60,000, 70,000, 80,000 years before the system catches up with what would otherwise have occurred.

So boundary conditions are extremely important and we don't really know how those boundary conditions are going to change, although we can make informed guesses about them.

I'll skip the next transparency and go onto the issue of how do we test climate models. How do we know how good they are? There are a number of different procedures that have been applied, and some of these have been used by Starley Thompson.

One standard method is just to see how well one of these models can simulate present day climate. A second method is to see whether the recent changes in climate that have occurred agree with what we think the anthropogenic influences on climate have been over the last hundred years.

So in one case, we're looking at the status quo, and the other we're looking at the changes over the last century or so.

And then the final thing we can do is to look at much longer time scales and try and simulate the paleoclimatic past, and then use paleoclimatic data to see whether that model simulation is reasonable.

In order to validate models against present observations, and this is really the most important way to test whether a model is credible or not, we need to look at the mean state of the atmosphere, how variable it is from year to year. And that's an issue that came up yesterday with Alan Flint's presentation, and it relates to interannual variability of climate, and a large fraction of interannual variability of climate, particularly in this region, is controlled by the El Nino sudden oscillation mechanism. And if a model is unable to simulate that type of variability, then one would be rather suspicious about how well it could simulate variability, year to year variability of precipitation in the future.

The other thing we need to do is test whether or not the patterns of simulated climate agree with patterns of observed climate. There are many different ways of making those sorts of comparison, and I will show some good results and bad results, and let's start with some results that look reasonably bad.

This is a rather complicated diagram, but it essentially shows how well the current crop of climate models is able to simulate the variability of mean sea level pressure over the globe, and the actual values of mean sea level pressure.

If a model were perfect, then with these two statistical measures, the values should be down in this bottom left-hand corner here. And these black dots represent results for different models, and what they show is that all of these models are, and some of them spectacularly so, when you try to simulate observed mean sea level pressure, it does not agree with the observations, that there are significant differences between the way the model simulates the behavior of the atmosphere and the way the real world actually is.

There are some models down here that are reasonably good, and the latest version of the GENESIS model I think comes down in this area, although the version that's been used in this comparison is somewhere up here. So that's not a very good result.

Another not very good result is this attempt to simulate the zonal or average on the longitude band, the zonal mean total cloudiness. And the white curve there shows the average of 30-odd models, and the black curve shows the observation of data, and you can see that even the average of the models doesn't agree terribly well with the observations. So models are not very good at simulating observed cloudiness distributions.

However, some models are very good at simulating the spatial variations in precipitation. And what this diagram shows, and I just want you to look at the top panel, and the variable here is the pattern correlation or the spatial correlation between the simulated precipitation and observed precipitation over the whole globe. And the correlation coefficients are given on a monthly time scale for each month of the year, and every line through here corresponds to a different climate model.

So there are some climate models where the pattern correlation is really quite small on a global basis, where there's only about 30 per cent of common variance or correlation coefficient of around .5.

There are some other models up here where the correlations are consistently around .8, and that's a very,

very good result, because if one actually compares different observational data sets, the correlations are only between .8 and .9 between different observational data sets. So an examination like this shows that some models, the best models are actually able to produce precipitation patterns that are just about as good as the reliability of our observational data. So that's quite a promising result.

Another way of testing these models is to see whether they can simulate recent changes in global mean temperature, and this is an example where people at the Goddard Institute for Space Studies tried to simulate the cooling that occurred globally after the eruption of Mount Pinatubo in 1991, and then the recovery after the eruption of Pinatubo. And you can see that there's really quite a good simulation there, so that's also a positive point as far as models are concerned.

The second most important way of testing models is to see whether they can simulate past variations. And this is the issue called detection. Can we actually detect a model generated signal of anthropogenic climate change in the past record. I won't go through that. That's a complicated issue, but the answer is yes, we can provided we force the models with the right type of forcing.

The interesting point here is that if we assume that only carbon dioxide or greenhouse gases were the forcing agent for climate change over the last hundred years, then we do not get a good result. But when we account for the effect of sulfur dioxide emissions and tropospheric aerosols, then the agreement is quite good.

So on balance, although models have known and sometimes quite serious deficiencies, they do a reasonably good job at simulating past changes, the present state of the atmosphere, and on fairly coarse spatial scales.

Now, another important issue with regard to estimating future change is whether or not climate models actually agree with each other. And I'll show a different result than is given in the handout here, and this is an examination of the agreement between different models for temperature and precipitation projections at the Yucca Mountain site, or a region around the Yucca Mountain site. And the simulations here that are compared are where the amount of CO2 in the atmosphere is doubled. So it's a standard type of experiment, and there are I think eleven models that are involved in this comparison, and of course they all give different results.

The results here are presented in normalized form or standardized form. In other words, what has been done is to take the global mean warming and then divide the regional warming or the regional precipitation changes by the global mean warming.

Now, different models have different global mean warming, and that's something that one can use as a scaling factor. What we're more concerned with in this region is the regionality of the prediction, the spatial pattern of the prediction, and what I'll concentrate on here is the precipitation changes and the results are shown by season. The middle value here is the average of eleven different General Circulation Models, and the average shows that in the summer, for every degree of global warming, there's a small reduction in precipitation, but an increase in precipitation in the other seasons. But there's a range of values for different models, and if you take the high end values, you can see that all seasons show roughly a 10 per cent increase in precipitation per degree global warming. And if you take the other extremes, then the average is for a decrease in precipitation.

So that on the basis of that intermodel comparison, one would have to consider a range of possibilities that included increasing and decreasing precipitation in just about all seasons as a function of global mean temperature change.

By the way, there's a very interesting result here, and that is that you'll notice that in general, warmer goes with wetter in these simulations, yet the paleoclimatic evidence and the model evidence suggests that at 80 kbp, cooler and wetter went together. So the system clearly doesn't act in a simple way, and the reasons for that are quite complicated, but they're basically associated with the type of forcing that is imposed.

Let me just add one little point here, and that is that if the world were to warm, say, by 5 degree celsius due to increasing carbon dioxide and other greenhouse gases over a period of hundreds of years maybe, then these changes would go up to about 25 per cent in the mean, or maybe up to plus 50 per cent at the extreme.

What about future climate prediction? Well, that was an example of future climate prediction. I will just remind you of what the needs are, and then I can use this as a focus for my summary points that I'm going to get to next.

Firstly, with this particular problem, the variables that we need are on a daily time scale. So there's a critical issue here of whether or not a General Circulation Model can produce credible daily information. And the answer to that is if we want to just take the data straight out of the model, the answer is no. We also would like to have daily temperature and cloudiness information and maybe other variables on a daily basis because it's this day to day variability that determines the infiltration rate.

The spatial scale that we require is very small. It's down to less than a kilometer spatial scale. The spatial scale of the global models is hundreds of kilometers, and even if we embed a high resolution model, we can only get down to a spatial scale of maybe 20 kilometers at best. So there's a mis-match between the needs of the hydrologic community in the Yucca Mountain area and the credible output of General Circulation Models.

Now, let me just summarize the main points here. The first point is that General Circulation Models, although they have weaknesses, they have strengths as well, and their main strength is that they are the only credible tool for estimating future climate. There is no other way to do this. We can't just take time series and extrapolate them to the future, or anything like that. We have to use physically based General Circulation Climate Models.

These models have weaknesses, but they also are quite good, given the complexity of the problem of simulating present day climate and past variations and paleoclimatic conditions as well.

The primary defect of these models or deficiency is the coarse spatial resolution. We can get over that partly by embedding a high resolution model in the coarse resolution model, but that still doesn't get us down to the requirement of resolution of one to ten kilometers for this particular problem area.

My judgment is that these models, although one would not want to place any faith in them quantitatively, that one can at least get qualitatively reliable information. In doing any future projection, we must consider how the natural processes and the human factors combine together. We can't ignore the human factors. Even though we may solve the problem of anthropogenic climate change on a time scale of a few centuries, we're still going to be left with high levels of carbon dioxide in the atmosphere. We still have the possibility of melting the Greenland ice sheet, the possibility of changing vegetation patterns and so on. Those changes might last for thousands of years, so we can't ignore those changes.

Individual models show quite different results sometimes, and to comprehensively understand the range of possibilities in the future, we should consider results not just of one model, but of a suite of models, and it's possible to do that without necessarily performing the required experiments with a lot of models. We can inter-compare models for standard cases where the comparisons or calculations have already been done.

Because of the uncertainties, and this word has been used already, we should consider these model simulations as scenarios, but they are scenarios that span, or can if carefully chosen, span the range of future possibilities.

And my last few points are more directed toward this specific problem area, and firstly, as I said before, on the spatial scales that are essential for this study, I don't think we can believe the precipitation results of General Circulation models. We have to be very careful in interpreting those results. That's not to say that they are useless. In fact, they do give us a lot of useful information, but that information has to be combined with other types of information in order to reduce the coarse resolution information down to relevant spatial scales.

And the two approaches for doing that, and this is beyond the embedding of a limited area model, the two approaches available are to use statistical techniques, or to use stochastic simulation techniques. We've already been given a good example of the use of stochastic simulation techniques. And so my bottom line is that what is required is a careful interlinked study that involves not only General Circulation Models, but also statistical downscaling methods and stochastic simulation methods. These three form the three sides of a equilateral triangle, and I don't think that we can just take one side; the whole thing will fall apart.

Thanks very much.

DR. DOMENICO: Thank you, Tom. Any questions from Board members? Staff?

MR. WIGLEY: Either nobody understood me, or it was a perfect presentation.

DR. REITER: Leon Reiter from Staff. Tom, one thing that we've been wrestling with is how do you use the--and maybe we'll talk about this later--how do you use the paleoclimate data, the stuff that, say, Rick Forester or Ike has been talking about, how do we use that together with the modeling? What role does the modeling play?

MR. WIGLEY: Well, I think the primary role for the paleoclimatic data is--well, there are two roles. One is validating the General Circulation Models, and that's the sort of thing that Starley has already done. He has shown that his model is able to simulate qualitatively the correct changes in precipitation and temperature on that paleoclimatological 10,000 year time scale. So that's a very important aspect of the use of paleoclimatic data.

The other aspect is to use it directly to bracket the range of possibilities in developing a credible set of future climate change scenarios.

DR. PALCIAUSKAS: I'd like to ask a question concerning the logic of using an average of the multitude of models, because certain models could be imperfectly crafted, then what's the purpose of including them in an average?

MR. WIGLEY: I can talk for hours on this particular topic, but it just happens, as one of my diagrams showed, that when a suite of models is averaged together, then the average validates better than any individual model. In other words, the average of a number of models gives better agreement with the observed status quo than any individual model.

And the reason for that is because although the models are basically similar in their large scale equations of motion and other equations, they all consider the small scale details in different ways, and those so-called parameterizations are not necessarily internally consistent.

They've got to be based on sound physical principles, but they are not internally consistent in the sense that when you look at the output of a model and see how, say, temperature and precipitation relate to each other, globally or regionally, then those relationships between different variables for any individual model will not agree with the observed relationships. And that's actually noise, and when you average the models together, you get rid of some of that noise, and with the present state of the modeling art, you actually improve things by averaging it.

Even when you include really manifestly poor models in the averaging process, you can still improve things a little bit. I think we've just gotten to the point now where the very best models with the highest spatial resolution, down to a couple of degrees by a couple of degrees, are as good as, and in some areas, better than the average. So we've just gotten to that point where the best models can do away with the need for averaging things together to get rid of the noise.

DR. CANTLON: You used the Greenland ice sheet as one of the types of lags in picking up the anthropogenic set of effects. But there are other major ones. Could you sort of touch on what some of the other ones are?

MR. WIGLEY: Well, the time scale for significant changes in the Greenland ice sheet is thousands of years.

DR. CANTLON: Ocean impacts; again, a big lag?

MR. WIGLEY: Well, the types of experiment that Starley and other models can perform in this case are not time dependent experiments, as Starley will probably explain. So we only can consider time slices at different points into the future. So ocean lag effects don't really come into this.

DR. DOMENICO: Thanks, Tom.

The next presentation is going to be by Starley Thompson, and it's going to be on future climate modeling.

MR. THOMPSON: Well, we at NCAR have been working on this problem of future climate modeling for the Yucca Mountain project for quite a while now, and it's only been in the last year or so that we've gotten to the point of actually doing so-called future climate analyses. And I'll report on our first one at the end of my presentation.

The objective of the future climate modeling is relatively simply stated, and that is we want to provide estimates of the future climate conditions so that it can be useful in estimating the effects on future hydrologic conditions. So, in effect, we're providing estimates of future boundary conditions either for qualitative models or quantitative models or very detailed quantitative models, hydrological models.

Our strategy for doing that is three-fold, and has been in place for several years now. First, we wanted to establish that we could in fact simulate climates with reasonable fidelity, develop a climate modeling system that can be used for the project.

We also wanted to be able to identify future climate scenarios that might occur in the next 10,000 to 100,000 years, provide boundary conditions for our so-called future climate simulations. This second bullet was actually done several years ago, and has been in a continuous state of refinement ever since.

And, lastly, the meat of the problem, we actually wanted to perform those climate simulations for the future and then provide our results for both hydrological modeling and performance assessment use.

The modeling strategy is worth taking a little bit of time on. Fortunately, Tom went before me and gave you an introduction to climate modeling and general circulation modeling.

As he noted, it's not feasible to perform long continuous climate simulations, and we do what we refer to as snapshots or stead state climates. The computational limitations of our super computers and of our models and of our understanding of what drives climate on 100,000 year time scales means that we can't set up the model and just run it forward in time for 100,000 years. It's just simply not feasible.

Instead, what we do is perform a finite set of short simulations that are designed to represent equilibrium climate states, and those states are an equilibrium with boundary conditions that we specify. For example, if we want to know what a last glacial maximum climate looks like for the Yucca Mountain site, for Nevada in general in the simulation, we set up the model with boundary conditions suitable for last glacial maximum, put in an ice sheet, change the solar orbital variations, that sort of thing.

So as I said, we do it by prescribing boundary conditions that are slowly varying, and then we do the modeling based on that. So it's not a complete model of the full earth system, which is still some decades away, I would think.

Tom showed you a version of this. What we're using is a nested modeling system, because global climate models, to be economical to run, have to run with a fairly coarse grid, and we embed a high resolution general circulation model. It's effectively the same kind of model, only run with a finer resolution.

Effectively, inside the output of the global model, the two models do not run simultaneously, they run in a two stage sequential process. We run the global model first, save-away output, then run the regional model. This actual domain highlighted here is an older domain we were using. Tom actually had a picture showing the current domain. It's about half that size, centered just over the Western United States.

The models that we're using for this project were both developed at NCAR. The global model is an outgrowth of the so-called community climate model operation at NCAR, which is a global general circulation climate modeling activity. The particular version we're using is Version 2 of GENESIS, which is our latest developed version. It has about a 400 kilometer grid spacing globally. It provides boundary conditions to the regional model.

The regional model is a Version 2 of the regional model developed over the last decade by Filippo Giorgi and his crew at NCAR apart from the Yucca Mountain project. It also has a long lineage and we're running it at a 50 kilometer grid spacing. It's still quite coarse compared to topographic belief at the site.

Here is a picture of the actual regional model domain and the topography contoured that the regional model sees. The contour interval, I believe, is about 200 meters. Even with that small a domain and 50 kilometer resolution, the topography of course is highly smooth. A 50 kilometer resolution was chosen experimentally after several tests as being the minimum resolution needed in order to resolve the rain shadow effect, the major rain shadow effect of the Sierra Nevada Mountain Range, which is a single large factor determining the relative validity of that portion of Nevada, the Great Basin area.

We could go to higher resolution. The model is capable, the actual continuous equations are capable of going down to about a 10 kilometer resolution, but it would be relatively unaffordable to run it at that resolution.

So why are we doing all this? Tom has already told you that succinctly climate modeling gives us the potential to identify and quantify unprecedented, non-analog climate behavior. Non-analog is essentially a buzz phrase referring to things that have not necessarily happened in the past, or may have happened in the past, but we just don't have any information about. The obvious one is anthropogenic climate change.

The limitations? Again, very succinctly, the models are imperfect, as Tom showed you, either because of numerical approximations or we've left things out or we put things in incorrectly. In terms of the Yucca Mountain project, since we have to reduce our effort down to a limited number of scenarios of future climate change, effectively that becomes a limited number of boundary condition scenarios, we may neglect an important one. We may miss it. This is largely dependent on expert judgment, paleoclimatic evidence, theoretical evidence.

And, lastly, even if we had a perfect model and knew precisely which scenarios we wanted to run, the models, by virtue of being highly demanding computationally, we have finite runs. There's simple statistical sampling error. Because they're not able to run the models long enough, you may miss some significant climatic event that might only occur once every 50 years or every ten years, depending on how long you've run the model.

The models have gone through fairly extensive testing. As I said, they both have long histories to them, different model versions. The global model goes back in terms of its antecedents for many years. The same thing is true of the regional model.

A lot of our work at NCAR takes place outside of the boundaries of the Yucca Mountain effort, but the expertise that has developed has been brought into play into this effort as well.

The regional model and the global model, for that matter, have been tested specifically for the Yucca Mountain project, however, to see if they work together well in coupled mode, and also in terms of looking at present day climate around the Yucca Mountain site as simulated by the regional model and climate as simulated over 21,000 years ago, or the last glacial maximum around the Yucca Mountain site. Those two efforts, those two analyses were done in the last few years as precursors, sort of the validation phase of our modeling effort.

This figure here just shows an example of enhanced topography. It's augmented. This looks strange here. One of the things we have in this model is the ability to go in and change the topography around. Since we're smoothing the topography anyway because it goes to a 50 kilometer grid, we ask the question, well, what happens if we modify the topography to really make it simulate all the peaks of the mountain ranges instead of the average. That's the sort of thing that you can play around with to see what the sensitivity of the model is. It turns out in that case it was not a good thing to do, so we went back to the regular topography.

We have begun comparing the results, the model results, to observations, temperature, precipitation, meteorological observations around the site. Nothing nearly as extensive as, for example, Alan Flint does, but just to give us an indication of how well the model is doing.

The picture here shows boxes of various sizes that we chose for averaging site data. The little dots are weather stations. That's not all of the rain gauges that are there, obviously, but those are the ones that are available as regular weather stations. And we've averaged up over different size boxes and compared the averages of the model to the averages of the data, and it turns out it doesn't make too much difference which averaging size you average over. Everything I'm going to show you in the line plots to follow are averaged over the larger box to get rid of some statistical noise.

In our early efforts, which have been the efforts over the last two or three years, they were largely just testing efforts, which means that we were running the regional model for sort of the minimum amount of time necessary to get the answers that we were looking for, which turned out to be two year integrations. This is two years of monthly average precipitation from the regional model for two specific years, 1989 and 1992. This is just running the regional model, not the global model.

We used observed boundary conditions for those two years, and we ran the regional model, and the reason we chose those two years is because they had very different observed precipitations. 1992 was an El Nino year, which as you know from yesterday's discussions, makes it a lot wetter at the site. So here's the two years as simulated by the model, and sure enough, the regional model does in fact show 1992 in the wintertime, late wintertime, to be quite a bit wetter. The mean is just the average of those two years.

So how does that compare to reality? Here's 1992, the wet year, compared to the observations averaged within 150 kilometers of the site. So this is an average involving on the order of, say, 36 regional model grid cells and all the stations within that box for that time. As you can see, there's a very good correlation between the regional model's precipitation and the observed average precipitation, and that was very heartening because it meant that the regional model at the very least, if it's given good boundary conditions, will in fact capture those sorts of anomalous wet years in that region.

Then they went on to test the full up modeling system, full up meaning both the combined global model and regional model, and the paleoclimate test case. We chose 21,000 years ago, is roughly the time of the last glacial maximum, ran the global model with prescribed ice sheets, CO2, insolation correct for 21,000 years ago, and prescribed C surface temperatures from the climate C surface temperature data set, ran the global model, took the output from the global model, put it into the regional model, ran the regional model for two years, and here's the results of the regional model for that time. Again, it's just a two year average, but this is undoubtedly statistically significant.

The model averages anywhere from a couple of degrees colder to up to five to six degree colder in the summer, again averaged over that box around the Yucca Mountain site, and this compares favorable, at least qualitatively, to what Rick Forester was saying this morning, namely colder, wetter. Here's the colder. Now I'll show you a wetter.

The same thing for precipitation. Again, only two years, so it's problematic how statistically significant it is, although I think the winter is significant. Since this model doesn't have El Ninos, it's fairly reproducible from one year to the next. Quite a bit wetter in the cool season; not much change in the warm season. This is about a factor of 75 percent increase in precipitation and five to six degrees colder in the summertime. So qualitatively, quite promising that the model is in fact capable of reproducing a known large climate change.

So we concluded from the testing phase the following. They were in fact able to adequately simulate the wet years for the YMP site region. This is given the provision that we're given reasonable boundary conditions. So it's really a test of the global model now, providing the right interannual climate variability. The kinds of global models that we run, since they don't have coupled dynamical ocean models and are not terribly high resolution, do not produce El Ninos. So the fact that we don't produce El Ninos has to be sort of added in to anything that we produce as an overlay of extra variability.

We already know what El Ninos do pretty much. What we're trying to do is look at the big background type climate changes to which you might add El Nino after the fact.

We correctly simulated qualitatively the climate of the YMP site region as being colder and wetter for 21,000 years ago. And we figured we were ready to take on the task of a future climate analyses.

As I mentioned earlier, the selection of future climate scenarios is really one of expert judgment. We have to reduce the future climate scenarios or future climate boundary condition sets to a finite set in order to be manageable. We call those the future climate scenarios. So this was actually first done on the order of six or seven years ago, and we've effectively been following the set that we created along with Tom Crowley when he was still working in the project, since then with some minor modifications.

You look at paleoclimate, what present climate does, for example El Ninos, theoretical arguments about the future, projections for anthropogenic effects, and you come up with a set of scenarios, which I'll show you in the next overhead.

The selections try to anticipate conditions yielding greater effective moisture in the Yucca Mountain region. So we say we're biased. We want to make sure that we get things in there that are likely to produce wetter conditions, and the choice and the schedule, the actual schedule in which we produced the analyses is highly subject to the limitations of our computer resources.

We started the first of the future climate scenarios in this year. For all the future climate scenarios, you need a control case to compare to, so that's one of the cases we've been working on. And the first one we wanted to do was a two time CO2 case, because we preferred to do the non-analog cases, the ones for which there is no paleoclimatic evidence, first because those, in my opinion, are the highest priority cases for the modeling.

The next fiscal year I think we'll be able to do two out of the following: either a very large anthropogenic greenhouse case, and that case might be the kind of case that would eliminate the Greenland ice sheet, for example, go back to the two time CO2, but ask the question what would happen if you entered a permanent El Nino state with that climate, or perhaps go back to 21,000 years ago and do a longer integration and do a more detailed comparison with the results of the paleo people to see to get better validation on the model. We should have capability of doing two out of those three, and that's to be decided.

Other potential scenarios on the list, and these have been effectively on the list, as I've said, for five or six years, an intermediate glacial case. As Rick Forester I believe noted this morning, or was it Ike, anyway one of the two noted this morning that as paleoclimate observational evidence moves along rapidly, it's, for example, not necessarily clear that there was a mass of fully grown ice sheets up until maybe 40,000 years ago. So an intermediate glacial case may be quite relevant to something that might happen, say, in the next 10,000 to 20,000 years.

On the other hand, as another unprecedented case, at least as far as we know, we could develop the ice sheets even larger and ask what would happen in that case. And also with increasing atmospheric CO2 and global warming, there is some evidence from modeling, coupled ocean modeling in the North Atlantic deep water that circulation might collapse, which we can mimic, even though we don't want dynamical ocean models, we can mimic this by modifying the C surface temperatures in our model and asking the what if question on that.

We've just gotten preliminary results from the two time CO2 future climate analysis, so this really represents the first future climate analysis result that we've produced and presented anywhere for the Yucca Mountain project. This shows the change in winter precipitation between the two time CO_2 case and the present day, showing a large--this is a four year average showing a large and coherent pattern of increase over the central and southern West Coast of the United States and, in fact, it results in an increase in the Yucca Mountain site as well.

Let me just show you the temperature curve. This is using the fully coupled GENESIS regional modeling system. The temperature increases on the order of two to three degrees C pretty much uniformly throughout the year. This is, again, averaged over that box around the Yucca Mountain site. And from what Tom showed, I think this is consistent with the average of the eleven models.

The sensitivity of the GENESIS model to a CO_2 doubling is two and a half degree Celsius, which is right in the middle of the swarm of model estimates, and two and a half degree is the generally accepted sort of centroid number, best estimate for the present day, and GENESIS fits right on that.

In terms of precipitation change, again averaged within the region of the site for double CO_2, it's again very consistent with the average of the eleven models that Tom showed, and GENESIS, at least this version of GENESIS, is not one of the eleven models that Tom had in his little table. An increase in the winter months, and a slight decrease in the summer months. Summer may not be statistically significant since this is only a four year average. The winter, I'll have to go in and do the statistics on this for the report that I'm working on this month to see if it is, but as Tom pointed out, we have a model that shows you make it colder, 21,000 years ago, you make it wetter. Increase the CO2 to make it warmer, and you make it wetter. So cold and warm don't necessarily equate because the real world is a complicated dynamical system.

Lastly, where are we going with all this, which is really the key element here? Our output from these future climate analyses, of which this double CO2 one is the first example, will be going to two different areas. We'll be going to Alan Flint, which is sort of through the hydrological modeling area, and we'll be providing him output from the regional model of daily values of temperature, precipitation and cloud cover in hopes that he will be able to use those data to actually drive his infiltration models. He should be able to use, say, four years output from our modeling, run it through his statistical process to generate, say, a 100 year statistically generated time series that are consistent with our model, and then use those for his infiltration calculations.

He'll also provide data to performance assessment directly. For those data, they'll be more time averaged, and statistical extreme values measures the variability will also be provided.

So I think we're moving right along. Even though it's been a long road, I think we've made good steady progress and are now in a position where we can produce something quite beneficial.

DR. DOMENICO: Thank you. I have a question first.

I saw I think two regional models, one of which is Western United States, and the other is part of Nevada and Southern California. Do you use the Western United States to establish the boundary conditions for that smaller region?

MR. THOMPSON: No. We only have one step in nesting. Okay, I'm just trying to get what you were referring to.

DR. DOMENICO: I did see a small region, it was Yucca Mountain, Nevada, and I've seen that. You keep referring to that as your regional model.

MR. THOMPSON: This is the actual regional domain of the model.

DR. DOMENICO: The Western United States?

MR. THOMPSON: That's the actual computational domain. This is just a zoomed-in view. It has nothing to do with the computational domain of the model. This is just a zoomed-in view on Southern Nevada and California illustrating where we chose averaging boxes.

DR. DOMENICO: Okay. So that is also 50 kilometer spacing, the same as the other one, grid spacing?

MR. THOMPSON: Yes. I mean, the 50 kilometer grid spacing is the basic resolution of the model, supposedly fundamental quanta that we get to average over when we average the model results.

DR. DOMENICO: In other words, the detail that we're seeing at Yucca Mountain is coming off of the detail that we see in all of Western United States?

MR. THOMPSON: That's right. Right, we end up simulating all of the Western United States and out into the Pacific just in order to get the detail around Yucca Mountain. For that matter, we end up simulating the whole globe.

DR. DOMENICO: Just to get the detail. Was there another Board question? Don, did you have something? John Cantlon, Board?

DR. CANTLON: Yes, on one of your overheads, you indicated that one of the areas that would be of interest would be the effectiveness of the moisture, moisture effectiveness.

MR. THOMPSON: Right.

DR. CANTLON: You do have temperature data. Have you done anything in combining the two to get some sort of a moisture index effectiveness? For instance, if you could get a better prediction of how much of it came as snow?

MR. THOMPSON: The model actually does separate or make a distinction between snow and rain. And, in fact, as part of the output for this two time CO2 case, we will include information on snow cover, snow fraction, as well. The regional model also has the ability to look at soil moisture, infiltration, runoff, but at a very coarse scale. Even though the actual physical model, the land surface model, is inside the regional model, it's a relatively good model, a state of the art model called BATS.

The coarse spatial scale pretty much makes that useless for anything other than qualitative looking at it. For the 21,000 year ago paleoclimate simulation that we did, we actually did look at runoff and soil moisture that we produced to make certain that those variables which are effectively more relevant to effective moisture were still consistent with the paleoclimate evidence. And, in fact, quantity such as soil moisture actually looks better than the precipitation because with it being colder and the precipitation going up, soil moisture goes up that much more.

DR. DOMENICO: I have one more. Would the 50 kilometer spacing, wouldn't all of Alan's measurement points more or less have to be lumped into one or two points? I'm sure that he has more than one precipitation station. He may have several precipitation stations within a 50 mile radius. How do you handle that?

MR. THOMPSON: Is Alan still here?

DR. DOMENICO: Well, if he did have, how would you do that?

MR. THOMPSON: Well, he has a lot of points close in, and he has far-flung points as well.

DR. DOMENICO: Yes, but you have a lumped system over there with one point.

MR. THOMPSON: That's right. We have what effectively is trying to represent the average over a 50 kilometer grid cell, and I think I'll let him take it from there.

DR. DOMENICO: I would just suspect there's a lot of variation in his data within a few models.

MR. THOMPSON: Oh, absolutely.

DR. DOMENICO: And then you're going to be using basically the average of this to represent that?

MR. THOMPSON: That's right. I mean, what we're trying to do is just get the right average over a 50 year or 100 or 300 kilometer averages on the assumption that the properties of that average can then be matched down to more detailed spatial properties using some kind of downscaling. It can either be very simple or be very elaborate, but there needs to be another downscaling step past the regional modeling step. This is what Tom noted, you know, there's a triangle and we've got effectively two sides of it. There's one more step that needs to be done, and it can be done simply or more complicatedly, but it will have to be done in order to represent precipitation on the Yucca Mountain site.

MR. HANAUER: I'm Steve Hanauer with DOE. If you could please, sir, find the view graph that had the conclusions from the testing phase? You showed us a very small number of simulations, and my question was these conclusions, that it adequately simulates the climate variability and correctly simulates the glacial climate. Could you characterize for us what the basis is? Is it one, ten or a hundred such simulations, or what is behind those conclusions?

MR. THOMPSON: It's not one and it's not ten. It's more like three or four. The number of simulations that we've done for the present-day climate, if you don't count the long history of the model development, has been on the order of three, of which I only showed you the latest one. No regional model that I know of has ever been run for more than eight to ten years because of the tremendous computational requirements.

So given that, and I think in fact the eight year run was done at NCAR, given that, it's hard to say the model does a perfect job, or even an excellent job or a great job at simulating interannual variability because we haven't done a lot of cases. But we have very limited computational resources.

We've had to try to be smart in the choice of our years that we do, so we deliberately picked, for example, an El Nino year and a dry year and compare the two and show that the model predicts wet, you know, extra precipitation in the El Nino year, and less precipitation in the dry year. And we hope that will apply to every time there's an El Nino condition and every time there's a dry condition. But, no, we haven't done any exhaustive testing to see if that was just a fluke of the regional model or not.

In terms of the paleoclimate test case, again, it was only a two year integration. As I said, we try to keep those integration links down to the minimum necessary to get the qualitative conclusion, and the qualitative conclusion is after two years, it became pretty obvious the model was colder and wetter and we didn't need to run it out any further. It's unlikely that it's going to change.

And so that was the minimum set that we needed to do, that we set out to do as part of the original study plan to verify the model, one, can produce interannual variability when driven with the right boundary conditions and has the right quantitative result to that and, two, can reproduce a paleoclimate vastly different than today, which was also wetter at the Yucca Mountain site.

DR. DOMENICO: Thank you very much, Starley. I think we'd best move on here.

The next presentation is by Michael Wilson of Sandia on TSPA insights into the impacts of climate and Chlorine-36.

MR. WILSON: All right, I'm supposed to talk about the things that you've been hearing about over the last day from a TSPA perspective, and in case anyone here doesn't know, TSPA stands for Total System Performance Assessment and it refers to studies that pull together all the components of the disposal system, the waste form, the waste container, the engineered barrier system and all the components of the natural system to make calculations of quantities related to the safety, the waste isolation, things like calculations of doses to individuals and also for comparison with regulations.

To start with, I want to talk a little bit about how performance assessment fits in with the things you've been hearing about, the site characterization program, and this part of the site characterization program in particular. First of all, I think I should emphasize that the studies that you've been hearing about over the last day based on TSPA calculations to date are among the ones that we consider to be key to predicting repository performance.

First of all, in a number of TSPA studies, repository performance has consistently been shown to be very sensitive to the percolation flux at the repository, which is closely tied to infiltration. The distinction is that percolation is the flux at depth; infiltration is the flux right at the surface, at the top. If you have vertical flow, they're the same. If you have non-vertical flow, then it redistributes itself.

Secondly, repository performance is very sensitive to seepage of water into the emplacement drifts, and in particular to contact of waste containers with seeping water. And that is, of course, very closely tied to the percolation flux, first of all, but it's also very closely dependent on the division of the flow, the spatial heterogeneity of the flow, and in particular how it's divided among matrix and fracture flow.

The idea is that matrix flow, there's a good chance because of capillary force, we'll be able to go around the emplacement drifts and not flow into them. Whereas, fracture flow has much less capillary force, and so is not as likely to be deflected by the drifts. And so if you have fracture flow, it can probably flow freely into the drifts.

And then lastly, repository performance is sensitive to climate changes. That's been seen in studies that have been done.

In PA, we have talked a lot to various PIs and site characterization, and we have provided feedback on what parts of the studies we think are of most value to us in what we need for performance assessment.

We're currently in the middle of some studies to evaluate how this wealth of new information from the ESF might affect our past TSPA predictions. Probably most people hearing, though, that the last big TSPA study was called TSPA 1995 because it took place last year, and the next big one is currently being called TSPA-VA, VA for viability assessment, and as was shown yesterday, it is due to be completed in 1998. So there's a big gap between the big studies and of course in the meantime, we need to be evaluating current information to help us in our development work.

Something that's important to say I think is that some of the data that you have been presented are things that we use directly in performance assessment, like infiltration values. The full-blown TSPA model, including all the components of the system, has an input for infiltration which we can take directly from Alan Flint, possibly with some spatial averaging or something like that, and use it directly.

Some of the data, especially the various kinds of isotopic data, are things that don't go directly into a TSPA calculation, but they are background, so to speak. They help us in determining which models are appropriate to use and what appropriate ranges of parameters for some of the models might be, the kind of thing that Andy Wolfsberg talked about this morning.

In some of these cases, there may be multiple steps of modeling and interpretation between the data and our use of it in performance assessment, and it's important to realize that. And the other thing that I think I should mention is that the site characterization organizations have the responsibility for doing detailed process level modeling, and because of the greater scope of our TSPA calculations, including so many different kinds of models, we typically use simpler or abstracted models for the individual components of the system.

And, lastly, I wanted to mention that it's currently planned that starting at the beginning of next fiscal year, we are going to be forming working groups composed of people from both performance assessment and the site characterization groups to define exactly what the models we should use for TSPA-VA are and what the data sets we should use are. So we should be working very closely on that, and I think that will be of a big benefit to the final product of TSPA-VA.

For the rest of the presentation, I have two parts, first on isotope and ESF kinds of things, and second on climate kinds of things. And when I talk about the importance, as it says there, what I mean is importance to repository performance.

Starting out with isotopic studies and ESF and that kind of thing, first of all, as has been noted more than once in the last day, the various kinds of isotopic data that we have been getting from the ESF and from boreholes are indicative of the existence of flow in isolated fast paths. I don't think we have enough information right now to tell the fraction, how much of the flow is in fast paths and how much of it is in slower flow, but we know that there is some of both.

High levels of Chlorine-36, and I could mention as well tritium, have been found in a number of places, and these indicate the places where there was presumably water flowing in fractures sometime in the last 40 years, and that's places where there might have been seepage into the tunnel at the time it was flowing.

At the present, we don't see any such seeps. We see a dry ESF tunnel. We don't know really how much that's being obscured by ventilation at the present. I think it's going to be very interesting to see the kinds of experiments that were talked about yesterday by Alan Flint and Dennis Williams in which they seal off part of the tunnel for a while or put up plastic sheets or something so that we can negate the effects of ventilation for a while. All these various observations are important because they give us constraints on the models that we use in performance assessment.

I'm going to talk a little bit about models now, flow models. First of all, a model that has been used a lot in the past for performance assessments we call the composite-porosity or equivalent-continuum flow model, and the basis of that model is that you have a very good communication in the flow between fractures and matrix, or strong coupling, if you will. And because of that strong coupling, the effect is to slow down flow in fractures basically, and because of that, it's very hard to see the kind of fast movement that's observed. To get a tracer from the surface to the ESF in 40 years in a composite-porosity type model is difficult. I won't say it's impossible to tinker with the model and input parameters to achieve that, but it's not really a natural sort of thing to happen in that sort of model.

The next step in complexity or sophistication in a flow model is what's referred to as a dual-permeability model, in which you have separate flows calculated for the matrix and for the fractures, and you have a coupling term of interaction between them. And that's the kind of model that Andy Wolfsberg was talking about when he gave his results this morning, and as he showed, with that kind of a model, at least for some ranges of the parameters, it's possible to match the kind of Chlorine-36 travel that we see.

The next step beyond that, or ways that you can get even faster travel are to drop the steady state assumption. If you allow large pulses of infiltration, it's possible to move tracers somewhat faster. If you imagine the flow being in rather discrete channels rather than in kind of long sheets, then that reduces the interaction area between the fractures and matrix, and that, once again, tends to increase the travel speed, or the velocity.

And, lastly, it's possible for things like fracture coatings to reduce the communication between the fractures and the matrix and to enhance the flow down fractures. And one thing I want to talk about later in the talk is what we call the weeps model. That is an alternative conceptual model that we've used in some of the past TSPAs to investigate this type of behavior in which the flow is in discrete fracture paths.

First of all, as I alluded to already, I think the rapidity of transport of Chlorine-36 from the surface to the ESF, and even farther down in some boreholes, not Chlorine-36, but tritium, favors a weeps or a dual permeability type model with a weak coupling between the matrix and fractures because of, whatever reason, because of time scale effects or because of coatings or something along this line.

Dryness in the ESF is something that I consider a very favorable indication for repository performance. Either there just isn't much water flow down there at all, or there is water flow, but it's not going into the tunnels. In either case, the water doesn't contact any waste that would happen to be in the tunnel, which is a good thing.

We, as I said, have been looking at variations in the kind of flow model to use for performance assessment, and one thing that is coming out of this is the importance that I've already mentioned of seepage into tunnels, and in particular the number of waste containers that are under seeps is being found to be very important to performance.

One important question, of course, is whether the dryness is going to carry over to future climates.

I want to go on now to talk in a little more detail on this second bullet with a couple of pictures. This shows results of a calculation that we did a couple years ago to get a feel for how our weeps model, as it was parameterized for TSPA 1993, what it would predict for observations in the ESF, which at that time hadn't been made yet. And I think the results look more or less like what are reasonably similar to what's observed.

Number one, it has almost 50 per cent probability in this calculation of seeing no seeps at all into the tunnel. And then there's some probability of higher numbers, and I think it's important to emphasize that the parameterization of the parameter ranges in this are, for the most part, guess work. As of 1993 when we made these parameter ranges, we didn't have much in the way of hard information to define these, and yet I guess I feel like we did a reasonably good job at guessing at it.

The thing to compare this with is it could be that the observation that this should be compared with is no seeps at all. That's what we see. Though, as I said, I don't know how much that's affected by ventilation. But it's also possible that this should be compared to the Chlorine-36 observations, because the weeps model is intended to be an episodic model so that a weep that only flows once every six years during El Nino periods would be counted in this, so it could be that you should count the seven Chlorine-36 observations from the ESF main drift and compare that to this. In any case, that's not that important.

The main thing that this is for is to kind of put this next graph into perspective a little bit. In that calculation, the average infiltration, or the average percolation flux was half a millimeter per year. Now, this shows this quantity that I have claimed is important, the fraction of containers that are contacted by weeps as a function of the percolation flux, as it has been models in some past TSPAs. This is one of the sub-models that you need for TSPA calculation, an estimate of how many containers are contacted by flowing water, and of course how much water is in the flow as well.

This line here shows the results for that weeps model as it was parameterized in 1993, and a half a millimeter per year is right about here. So I think that kind of gives a feel that somewhere around a fraction of containers of ten to the minus two to ten to the minus three getting wet is reasonable, given the fact of a dry ESF.

That's kind of what I want to point out.

Then compared to these other models, which are really the same model but implemented slightly differently in two different studies, this is based on composite-porosity type model, and for that model, you can see that if you want to have a dry ESF, you're going to have to have a flow probably less than a hundredth of a millimeter per year, which is all right, but the catch is that a composite-porosity model with a flux of a hundredth of a millimeter per year probably has a travel time of a million years between the surface and the ESF, which is not a good match with the speed of transport of Chlorine-36.

And the point to this is more or less that with additional observations, we're starting to be able to constrain our models more with real data, and I think that we could improve on the weeps model. We could make a dual permeability model to replace the composite-porosity model that would, by using the isotopic data and ESF observations, would do a much better job.

Next, let me go on to talk about climate. The effects of climate can kind of be parcelled into two pieces; the timing and the amplitude. The first one I want to talk about is timing. The first point is that for a short performance period, like 10,000 years, the probability of a change to wetter climate during that short period is important to performance.

For past performance assessments, we have in all cases assumed a fairly small probability of change to a wet climate during the next 10,000 year period. Some of the newer data discussed by Rick Forester this morning is indicating that it may be fairly likely that there will be a wetter climate within the next couple thousand years. That's something that might make an important change.

Secondly, for a long performance period, like a million years, we know that there's going to be many climate cycles, so that the timing of the cycles isn't going to be particularly important. What's more important in that case is just the division of the fraction of the time that's in wet climate conditions as opposed to dry climate conditions. And Rick Forester in one of his view graphs this morning said that was 70 percent.

Lastly, I wanted to point out in particular that a change to wetter climate during the thermal period, by which I mean, say the first couple thousand years, might be especially important because the extra influx might change your predictions of dryout time, relative humidity and, therefore, container lifetime.

This is a list of different climate-induced effects that can affect repository performance. Changes in the unsaturated zone flux obviously are going to be important. The redistribution of seeps to different locations, that is, if the seeps don't always flow in one place but sometimes change as the climate changes, that will affect performance. The episodicity of flow, whether the weeps flow once a year or once every six years or once every hundred years, that affects performance.

Changes in the water table elevation affect performance. Changes in the saturated zone flow can affect it in two ways; changes in the amount of dilution and it can create outflows of water from the repository at nearer locations than occur now. And, lastly, changes in the biosphere, for example, a wet climate condition might be more conducive to people living around Yucca Mountain.

This is just an example. I want to make two points with it. This shows three different dose curves; dose to an individual as a function of time over a million year period, and I don't want anybody to look at any of the numbers. It's just, like I say, to make two points. One point is that depending on the assumptions you make about your models and about the climate effects, you can go all the way from having very large climate changes, to having almost no change in the doses over time. And the second point is just that the typical picture that I think almost everyone has in his mind that a dose rises, comes to a peak and then falls in time may not be the correct picture, because we don't have a steady state flow system.

In TSPA calculations so far, we have indeed found the increase in unsaturated zone flux to be one of the important parameters to performance. And in TSPA 1995, that was pointed out explicitly. In that study, they didn't examine the effects of changing the timing of the climate change, so they didn't see the sensitivity to that. But in 1993, it was shown that for 10,000 year calculations, the timing was important.

The weep stability, that is whether the weeps or seeps stay in one place or change over time, has been found to be important to performance, the idea being that you may emplace a container in a dry place, but perhaps in the future, it will turn into a wet place, because the flow patterns change.

And then some of the effects that I listed haven't really been evaluated yet. The changes in saturated zone flow aren't expected to be a really big effect. I'm not aware of any studies recently, but there's a ten year old study by John Czarnecki of the USGS in which he estimated the effects of a wetter climate on saturated zone flow, and he found flow increases of a factor of two to four. So that's important, but we get larger factors due to a lot of other things, so it's not a big player. And biosphere, we haven't done anything with, and that kind of depends on what the regulations look like, I think.

A very short point; that for a composite-porosity type model, the change in flux is what's most important. It's almost the only thing that's important. But for a weeps type model or a dual permeability model with enhanced fracture flow, then the parameters that go into the locations and numbers of those flowing fractures are important.

To conclude, in PA, we do not look at a single flow model. We think it's very important for us to be looking at alternative models of all of these different things, constrained as much as possible by observations, and in some cases, constraints are very weak, and so we may need to include fairly disparate models. In other places where they're tightly constrained, maybe we only need to use a single model.

The new observations from the ESF and from recent boreholes as well are giving us a lot more data to constrained models than we had in the past.

The percolation flux and its spatial and temporal variation are very important to performance. And I include in that spatial variation the distribution between matrix and fractures.

And the number of waste containers contacted by flowing water, by seeps, I think is probably more important than the fact that it is fast flow. We focus on the fact of fast paths, but the fastness isn't so important as the number, is what I see from preliminary modeling that we've done.

And then, lastly, climate change is potentially important. I think we have yet to model a lot of the newer ideas that were presented by Rick Forester and others today.

DR. DOMENICO: Thanks, Mike. I have a question from John Cantlon, Board.

DR. CANTLON: Michael, in your list of conclusions that you put up, your last slide, you indicate that the number of waste containers contacted is really very important. What is your assumption about the role that the drift itself will become a pathway, moving water wherever it gets in to change the humidity for the whole drift?

MR. WILSON: We have not modeled that so far. I guess I don't expect--at first it sounded like you were thinking of water flowing down the drift, and I think the drainage at the bottom of the drift will probably be fairly good, but you could indeed, as you say, increase the humidity for a large region around a seep, and that hasn't really been investigated.

DR. CORDING: I was interested in your Page 8 on your presentation on the estimated number of weeps in the main tunnel. And before I get to some of the conclusion type points on it, were those fractures assumed to be something perpendicular to the tunnel that was flowing 100 per cent over that fracture surface when they ran that calculation?

MR. WILSON: There are a multitude of assumptions in this calculation. They are assumed to be rather discrete pathways, so they're almost like point objects, and there is a range of aperture sizes and a range of flow rates, and there is just a geometrical probability calculation to determine whether they contact containers or not.

DR. CORDING: But they could be channelized? I mean, in other words, 100 percent of the surface of the fracture is not necessarily flowing that's in contact with the tunnel?

MR. WILSON: No, that's right. It may be flowing down two centimeters. In our assumptions, the mean width was half a meter. That was just sort of a number representing a typical cooling fracture, I guess.

DR. CORDING: Okay. So a number of weeps is over, say, a half a meter length of fracture, something like that for each weep of flow?

MR. WILSON: Each weep typically is, yeah, like a half a meter wide by 200 microns thick.

DR. CORDING: Okay. One of the things that's of interest is, and I don't recall exactly what the number is on the amount of water to be taken out in ventilation, but it's orders of magnitude greater than that sort of flux. And so if you had a distributed flux, you wouldn't see anything at all, but that doesn't mean that you wouldn't see something if you had flow in concentrated areas. You would start to see things, and the question is at what levels would you start to see it and expect it. And even with this tremendous amount of ventilation we have, it would seem that you could say that we would--you know, at what level should we be seeing flows in the tunnel? What sort of flows would that represent and what types of flows will we not see because evaporation takes it out? And it has to do with the distribution of that flow, not just an average flux. If it's all in one joint in ten feet of the tunnel, you're going to see the water.

MR. WILSON: That's right.

DR. CORDING: And so even now we could have some information on that in a very gross sense as to what the maximum flow is. And certainly as one starts to seal off some drifts and look at some of those other features, you'd be able to see in more detail what happens.

One of the concerns on the drifts in terms of measuring these flows is that the drift itself is not so much a conduit, but it's a barrier to flow, at least to advected type flow, or may be, and so those are some of the things I think would be very interesting to see. And it seems to me one could at least make an initial calculation here very quickly as to what sort of flows are not occurring, because it would otherwise be overwhelming to the ventilation system.

MR. WILSON: I think we could take the current observations and constrain that model quite a bit. I don't have a plot and I don't know the distribution of the flow rates in those predicted weeps, but I suspect that some of them were probably big enough that they would be observable. If you were to take that into account, you may push this whole curve up some more.

DR. CORDING: Sure. Whether or not that's the distribution you have or not is not so much the problem. You know, this approach is interesting. You can go and do some checking. It would seem to me it could be very useful.

MR. WILSON: Right.

DR. DOMENICO: A question from Jared Cohon, Board?

DR. COHON: On your overhead Number 2, you say that performance assessment analysts have provided specific feedback from priorities, et cetera. Could you give us some examples of that?

MR. WILSON: Examples?

DR. COHON: Yes.

MR. WILSON: Well, one example is that in a memo two or three years ago, we pushed fairly strongly for a lot more calcite dating and isotope studies. And something I meant to emphasize more strongly is that short of finding some actual flowing water in Yucca Mountain, those isotope studies are the only way we have of getting information on how flow is distributed between matrix and fractures.

DR. COHON: I'm more interested in process here than I am substance. How do you communicate this feedback, and is it invited? Is it solicited?

MR. WILSON: A lot of times it's informal, just in talking with people. There have been a few instances where we have sent memos to the PIs or even to DOE people when requested to giving our evaluation of priorities.

DR. COHON: All right.

MR. WILSON: And sometimes it's in the form of reviews of reports and that kind of thing as well.

DR. COHON: Another related question. You made reference to the working groups that will be formed for joint performance assessment and site characterization working groups, and you indicate they'll be formed at the beginning of FY-97.

MR. WILSON: Yes.

DR. COHON: Is this going to leave enough time in order to have the influence it should have on TSPA-VA?

MR. WILSON: I think so. I think it gives about a year before the calculations need to be nearly finalized. It doesn't allow time if there were a lot of computer model development required, but I guess I am hoping that we'll get by with a lesser amount of new computer codes being written.

DR. CORDING: Since this is a public meeting with a record, let me go on record as saying I doubt it. It just seems to me that given the complexity of TSPA and the many, many components it has and the amount of scientific results that have to be integrated into it, that not to start the process of coordination between what I'll refer to as the PIs and the modelers until about a year before you really need the results is very risky. And I--well, I've spoken my piece.

MR. WILSON: Well, let me qualify that I don't expect that we will know everything there is to know at the end of this process, but I think we will have something much better than what we have now. That's what I think.

DR. LANGMUIR: Could I pick up on that?

DR. DOMENICO: Langmuir, question or comment?

DR. LANGMUIR: Well, it's a combination of them, but it's picking up on what Jared was just saying.

About three years ago, we were all introduced to the concept of TSPA as a pyramid within this program. The pyramid had at the top the TSPA models which you've been speaking of today. At the bottom was supposed to be folks that now you're not going to talk to till '97. The pyramid showed all the investigators and the detailed models at the base of the pyramid, and we were led to believe that there was communication going on starting back in the early Nineties between the investigators within the TSPA program, and the folks at the top with the larger models. It clearly hasn't been going on systematically. It hasn't been going on in a structured, as far as I can tell, manner.

MR. WILSON: There were discussions with the PIs, by the way, not to name any individuals. But this is just the first time that the site characterization organizations are going to have a large amount of funding to work intimately with us. It's just going to be a lot closer than in the past.

DR. LANGMUIR: This is not a criticism of you.

MR. WILSON: I think Abe wants to say something.

DR. DOMENICO: We'll take the last comment from Abe Van Luik.

DR. LANGMUIR: Before he speaks up, Abe, let me add something to what you're going to have to respond to. Yesterday, we heard from the DOE that the waste isolation strategy was created in concert with the TSPA folks, and that there was an interplay across the way between TSPA and waste isolation. Yes? No? I got a yes, but I was led to kind of question whether there really had been coordination. Can you respond to that as well as my concerns about the pyramid, which is supposed to have been here for years now?

MR. VAN LUIK: This is Abe Van Luik from DOE. Yeah, I stood up basically because I think some of the questioning of Mike Wilson, who is an analyst in the PA program, has been a little unfair because you're asking programmatic questions.

In the first place, if we talk about the waste isolation strategy, if you read the front part, it is based on TSPA-95 results and works forward from there. The PA people, as well as the site people and the engineering people, have all been intimately involved with this latest version. That doesn't mean that every PI in each one of these areas has been personally involved, but we have basically gotten buy-in from a lot of people representing each organization.

I think some of the other things that were mentioned a minute ago, not starting interactions until fiscal year '97, I think the right spin on that one is that we have had interactions for quite some time and they've been quite intense. For example, Bo has been laden with feedback from us, formal feedback, on the first version of his model and is responding to that feedback.

We have given LANL feedback on their transport model. And something that Mike Wilson said a while ago, that we, you know, need to develop a dual permeability capability for transport, LANL has actually already provided that. So we're working in concert with them and we're going to basically formalize that process by what Mike Wilson was talking about in the boundary, and Mike makes a good point that in the past, this interplay between us and site was a little bit ad hoc, and that now we are actually going to factor into the schedule and into the funding the direct co-working on this next TSPA.

Is one year enough? As Mike points out, if we find that there are holes in the modeling, that one year is not enough. But that's one reason why we're already planning beyond the program plan that was just released. And instead of doing additional sensitivity studies for the TSPA-LA, we are planning a full-blown new performance assessment for the TSPA-LA to give us that extra two years to improve on the products that we have going into the TSPA-VA. So we're very well aware of the issue, and I forget what other questions came up.

DR. LANGMUIR: Well, I'm concerned that the Board isn't going to really know. You guys will disappear into the lab for three years. How do we find out where you've gotten and where the key issues are going? Will we hear about this sort of thing? Will you be in a position to tell us as you progress what the important issues are?

MR. VAN LUIK: I think the answer is an unequivocal yes, and I don't see how we are ever able to in the past or in the present keep secrets from the Board.

DR. DOMENICO: With that note, I think we had best move on. Thanks very much, Michael.

Well, it's time for a wrap-up. Sheryl Morris is going to wrap things up.

MS. MORRIS: My name is Sheryl Morris. I'm one of the members of the hydrology team, geochemistry and climate team. Specifically, I'm the climate WBS manager.

What I'm going to try to do is, as alluded to, I'm going to try to wrap up some of the presentations that you heard both yesterday afternoon and today. To make things a little simpler, this presentation probably looks familiar. This is Russ Patterson's presentation. We thought it would be a good foundation to go back and pick up the strategy, pick up the speakers and give you the highlights.

So, again, we are only going to be addressing the speakers that addressed the waste isolation attribute on seepage.

Again, the overall objectives were to determine the variability and the magnitude of the infiltration and percolation flux, to look at those factors that might influence the infiltration and percolation, to obtain the adequate bounds on those factors, and determine some of the likely impacts on saturated flow and transport.

Strategy was to use the geologic structure as a framework, to look at how today's hydrology relates with the geological structure, and how hydrology responds to the climatic conditions of today, of yesterday, and what we might look for in the future.

The first speakers were Warren Day and Steve Beason. They gave you an overall presentation of their results of the faults, both the superficial and at the ESF level. And then Ed Kwicklis got up and gave a quick presentation on how the hydrology responds to the geologic structure. He talked about fracture flow that occurs within and through the PTn. He gave us some of the magnitude flow numbers for fracture and matrix, as well as the deep percolation.

We've looked at the geologic structure, how hydrology relates to that today. We can look at present day conditions of infiltration and meteorology. Alan Flint did that, and of course he emphasized many times that infiltration is temporally and spatially variable, and he talked about how you could take some atmospheric parameters and turn that into infiltration that would feed the other hydrology models.

We've looked at the present day conditions. We went into the past conditions. First, Rick Forester gave us some highlights of where the paleoclimatic studies are, laying the foundation with the cycles, the 400 and the 100,000 year cycles, laying the paleoclimate records as an overlay on that, covering some of the last glacial parameters and talked about the annual precipitation and the temperature being colder and wetter.

So how did the past hydrology react to the past climate? We looked at it from two approaches; that being Zell Peterman's and one of June Fabryka-Martin. Zell came back that evidence slow percolation, slow deposition and low volume water.

June and Andy came back and next talked about some of the results from the Chlorine-36 studies that they're doing, and that it indicates a fast pathway that as itself does not indicate the magnitude, looking at a pretty good foundation as a path. The present and the past are going to lay the foundation for the future.

As Starley pointed out, looking at the future climate modeling, the test case of the past and the present has been run through the model. He's finishing up the CO_2 case and will be going into the next phase later on, but this numerical output will be given to Alan for his use to send down to the hydrology models, and a copy of it is being given to TSPA for their use.

Overall from a climate perspective, we've looked at today's climate and how it affects hydrology. We've looked at the past climate and how it affects hydrology. We're looking at some of the cases that we have not seen in the past and we're using future modeling to try to incorporate what could occur that would affect repository performance.

As a group, these individuals will get together and package what will be given to TSPA later on next year, and TSPA then will go ahead and follow through with the impacts that future climate could have on future hydrology.

And that's it; a real quick rundown.

DR. DOMENICO: Thank you very much. There's probably no need for questions because that was a wrap-up, unless of course there is some comments or questions.

(No response.)

I was right. Thank you very much.

I have an announcement to make. Before we go on break, we're of course getting set up for the round table, which will be led by Garry Brewer. We have selected some of the presenters and some people who have not presented material for that panel group. Those of you that were not selected, that doesn't mean we didn't want you; we just didn't have room. But don't leave, because we anticipate there's going to be some interchange between the people sitting around in the panel and the remaining people here in the audience, if I can call it an audience. So don't run for the airplane yet.

They'll need 15 minutes to put that together, I believe, so let's take a 15 minute break.

(Whereupon, a short recess was taken.)

DR. BREWER: Will everyone please reconvene, including our panel. My name is Garry Brewer, and I have been the silent member of the Nuclear Waste Technical Review Board. I'm neither a hydrologist nor climatologist, and one can wonder why I am chairing this panel, and it's either because there is plausible denial, doesn't go well, but more to the point, it's because I got the short straw. So it's my job this afternoon to keep this thing on track.

We have for the past day and a half heard a lot about climate and hydrology, trying to figure out the past and the present and the future. From a performance perspective, we're particularly interested in the present hydrologic regime at Yucca Mountain and how it might change as the climate changes. It's really been the main reason for having this particular theme for the meeting.

We've heard a lot. A lot of the work was nicely summarized right at the end by Sheryl Morris, and we need not go back through that. She's done the job.

The round table really has a couple of fundamental questions, and as a non-hydrologist/climatologist, I kept asking myself versions of these questions for the last day and a half, and I'll take the prerogative of the chair to ask them again in very specific terms as we go along. Is the DOE program going to be successful and what do we mean by success? Success in the sense of reaching an understanding of climate and hydrologic regimes in the past and more particularly in the future, and how these might affect the repository. I think that is basically the question; what's going to happen on this one place in Nevada that we're all so concerned about.

More to the point, and Jerry Cohon at the end got to it in terms of process, are we heading collectively, and more particularly the PIs and DOE, heading in the right direction? To begin to ask and answer that question, are we going to have success in terms of the program, this particular goal? Are we collecting the right data? Do the models make sense. Is the interaction good, the processes good, communication good? Those are all things that a Board like ours has to be concerned about. That's one of the main reasons we invited you all here for the last couple of days.

In addition to the speakers who were formally on the program and people who made presentations, we knew in advance that there were three or four others that we wanted to involve in the round table, and as the two days progressed, there were a couple of other individuals we thought could make meaningful contributions to the round table. I'll talk a bit more about that in a moment, the round table and how we proceed.

Neil Coleman, a hydrologist in the Division of Waste Management with the NRC, is one of these individuals we've invited. Abe Van Luik, who stood up and was beginning to get into the panel before we were ready for the panel, so he's primed, who is in charge of performance assessment at Yucca Mountain. Parvis Montazer, a consulting hydrologist in Nye County, Parvis is at the end of the table, who formally was with the USGS and is to a large extent, or to a significant extent, responsible for some of the unsaturated zone modeling at Yucca Mountain.

Two other individuals who we have invited to join the round table are Marty Mifflin of Mifflin MAI, Mifflin Associates, and Roger Morrison here of Roger Morrison and Associates.

Now, in terms of how the round table works, I'd like to start off by giving those who did not make presentations a moment or two to kind of speak their piece, and then we get into basically a free-for-all, and my job is going to be good cop, disinterested party kind of referee. And we will take conversation from people around the table first, following up on anything that comes to mind, and then as that either dies down or dissolves into something out of control, we will then go to the audience.

We have, as always, left time at the end for anyone from the public who wants to ask questions. Our basic objective is to be finished with the round table about 4:40 or 4:45.

Now, a word to my colleagues on the Board. We will wrap up. The luggage is next door in the safe. We will meet for a quick businesslike meeting in Room 240, and then we will proceed directly to our retreat. There's no need to move the luggage, is the basic point.

Now, having gotten that monumental piece of business out of the way, let me invite Neil Coleman to comment on whatever comes to mind. Neil?

MR. COLEMAN: You said a moment. Is that the three minutes I heard about?

DR. BREWER: Three minutes is a moment.

MR. COLEMAN: We should start this out with a bang then.

As everyone knows, it's a tough job trying to understand the hydrology of Yucca Mountain, even under present day conditions, and the need to consider future climates adds another dimension to the problem. As of now, we don't know what the period of performance will be for a repository. Hopefully, the new EPA standard will be out soon. If it turns out that the performance period will be hundreds of thousands of years, or perhaps even a million years, climate would be even more important.

At NRC, we see a key question here. What's a defensible range of future climates at Yucca Mountain? Climate change is the most important factor in estimating long-term shallow infiltration and deep percolation at the site.

Site characterization gives us information about the present day conditions, and that's an essential starting point. But the climate will certainly change over thousands of years, and precipitation will increase. In the Great

Basin, past climates have been significantly wetter than now, and such conditions have to be considered to provide reasonable assurance that climate change is an integral part of performance assessment.

Now, DOE has proposed an extensive program of global and regional climate modeling. From what I've heard, it's a little less extensive than it used to be, but it's still an important effort.

But attempts to use global climate models to predict climate changes over tens of thousands of years will almost certainly remain very controversial, leading to debate over the competence of one model and data set versus another. Efforts to validate global climate models will likely result in continual attempts at validation and model calibration.

In addition, only highly unreliable speculation is available to predict the manner and degree to which future human activities will affect climate.

Now, I personally advocate an approach to bound the hydrologic consequences of climate change in which one would develop a reference climate scenario that is consistent with known climatic patterns during the Quaternary. As I see it, that would include conditions that would be challenging to repository performance. Using what is known about paleoclimatic trends, reasonable and realistic assumptions about future climate change could be made to support performance assessments.

Projecting the Quaternary cycles into the future has several advantages. We don't need to know the exact causes of glacial cycles. We don't have to praise or persecute Milankovich. The cycles would speak for themselves.

Also, as I mentioned, we can avoid the highly unreliable speculation about future human activity. There are, after all, limits on the availability of the fossil fuels that are being consumed, producing greenhouse gases. And even after a century of industrial revolution, average surface temperatures, air temperatures on the earth are estimated to only be about half a degree centigrade higher than a century ago.

Now, this approach to climate change was presented in our paper at the recent High Level Waste Conference. Our paper was intended to encourage discussion on this topic at about a reference climate scenario, but as I found that participation in this year's conference was much less than in prior years, there wasn't a whole lot of discussion to be had.

Efforts in global climate modeling do not appear to provide a lot of added value to the program through predictions of the likelihood of various climate scenarios. Realistically, there seems to be greater value in the interpretation of paleoclimatic data specifically from the Great Basin region, and these data include information from paleo-discharge sites, pollen studies, paleo-lake levels, the Devil's Hole work, and other sources.

The key work in these areas have been done by Jeffrey Spalding, Ike Winograd, Jake Wade, Marty Mifflin and a host of other folks.

I personally believe that there is a clear path to a scientific consensus on this issue. I don't mean a consensus on what the ranges of precipitation will be in the future, but on the variability and periodisity of what we might expect in future climate.

That concludes my statement.

DR. BREWER: Okay, thank you, Neil.

The next individual will be Abe Van Luik. Abe?

MR. VAN LUIK: I think I may not even take the three minutes I'm allowed, which makes me a hero.

I would like to remind people that we have one repository program, but we have divided it into several divisions to make it workable and manageable. As part of that manageability division, performance assessment was created to look at total system performance and the implications of work being done by others on total system.

I think what you have seen in the last two days is ample evidence that the scientific program is on the case and is looking seriously into the process level understanding of the mountain in terms of flow and transport in its evaluation of new revelations from the site.

I think it's important to recognize that alternative conceptual models that were talked about by PA and by several other people are to be considered and evaluated as part of the process level model development for flow and transport, and they are to be evaluated as part of the site program. However, the PA program is intimately involved in that exercise because the evaluation of these things in terms of what they mean in terms of total system performance is our responsibility.

So we are working arm in arm, sometimes head to head with the sites, the engineers and the joint groups that are looking, for example, at the near field environment, and we will be, as was pointed out by Mike Wilson, starting in early fiscal year 1997, starting a very intense program of cooperative work to make sure that whatever we put into the TSPA-VA will have the understanding and the blessing of the site program and the engineering program.

And I think the likelihood of success goes up as we get the whole program behind, the results that we put forth to bodies such as this and to the public, and I think that's all I'd like to say.

DR. BREWER: Thanks, Abe. Parvis Montazer?

MR. MONTAZER: Over the five years that I was involved in the program, from 1982 to 1987, I basically did nothing but to develop a site characterization program. And a lot of my effort went into the exploratory shaft test plan development which is now the exploratory study facility program.

I came back to the project about two years ago. In 1987, I thought that we had a good solid site characterization program presented to NRC, and it was conceptually solid. There were a lot of things that we didn't know how to do and we were hoping to learn the process. When I came back a couple years ago, I found that a lot of basically the bricks, the foundation on which we based that program are basically taken apart. And in the past two days presentation, I'm getting a feeling that there are a lot of holes left open that are really making this program in a critical state as far as coming with a conclusion in a quick manner.

I would like to see if we can focus on a way of going back and filling those holes and gaps as soon as we can while we have the opportunity in the process.

The main thing that I'd like to point out is the exploratory study facility, as we planned it, is going to create a major boundary condition, induced boundary condition in the mountain. Unfortunately when I came, I saw that there was very minimal effort to characterize that boundary condition, and still really there is not a good effort for the characterization of that boundary condition.

Also, I have noticed that there's been a lot of variation and deviation from what was planned, which is basically blamed on the construction problems and health and safety problems, one of which is the water issue, water use in the tunnel that was supposed to be at a minimum possible amount. And what I'm finding out is that there's really not 100 percent care used in that process.

I think we have very little left in the completion of the ESF tunnel, and I would like to encourage the scientists involved in the program to rethink seriously to see if they can get all they can in that process in that short of a period of time between now and December or April, whatever the completion of the ESF is concerned.

That's about it, and I can go to specifics if there are questions.

DR. BREWER: I think it's appropriate that we come back to it, because one of our major questions is changes in the program that ought to be considered, and your comment about holes in the program is certainly a leading question. We should probably come back to it.

Marty Mifflin next, if you would, sir.

MR. MIFFLIN: I started out on this project I think it was 1980. NRC made a tour around Nevada and other places on possible proposed sites, and in 1981, I recall--I was reminded actually yesterday by Phil Justice of NRC that things had come down to what was discussed in a smoke filled room I think it was in 1981, trying to brainstorm what the issues would be of the Yucca Mountain site. One was climate change, and the position of the site in the vadose zone in a region that was known to have had major changes in both climate and effective moisture, and that particular issue has been worked on subsequently by a number of different folks, both within the Yucca Mountain program and outside because it's a very challenging issue.

To me, it's a potential site, if not killer site modifier, because the quantitative evidence for effective moisture, what I call effective moisture or effective precipitation that comes from the Great Basin pluvial lakes at their maximum stage is about one order of magnitude greater than the current hydrologic budgets in the basins, and the extremes are during the pluvial periods, would appear to be about .5 orders of magnitude greater, to 1.5.

So if Alan Flint's numbers of, say, five or ten millimeters of net infiltration are turned into 50 or 100 millimeters of net infiltration during the pluvial, and then we hear today, and others have already said it, that the last 2.5 million years, the climate has shifted back and forth on some type of semi-cyclic basis, but looking at the various records that Ike presented, you could say 60 to 70 or even 80 percent is some type of a cooler or wetter global climate over that time period.

So, really, based on the inventory life of the high level waste, which for plutonium is ten to the five years, and for the uranium it's forever, for all practical purposes, 60 at the minimum, and say 80 percent at the maximum, or something in those rough ranges, the site will be in some type of a so-called pluvial or glacial climate. So it's a very, very important issue from the perspective of the performance of the site, not so much in licensing criteria, but from the practical perspective of how long will the engineered barriers persist and what happens to the waste after the engineered barriers are gone.

I think I'll stop there.

DR. BREWER: Thanks, Marty. Thank you very much.

Final opening comments from Roger Morrison.

MR. MORRISON: Hello. Ten years ago, I started work on Lake Tecopa, which for several million years was the sump, the terminus for the Amargosa River which heads at Yucca Mountain. And Lake Tecopa has a superb stratigraphic, climatic, hydrologic record, and it's been a fascinating place to study. The record is exposed in deep badlands in three dimensions, much better record than you can get from any boreholes, which are highly site specific.

But Lake Tecopa had a several million year history with long periods of playa conditions, long periods, one was almost a million years long, in which chiefly playa conditions prevailed in the basin. But that ended about a million years ago, and since then, there's been a trend of rising lake cycles. Each lake maximum tended to be higher than the preceding one, with some long periods of desiccation, dry playa conditions, shallow lake playa conditions.

The trend is an upward trend, as I perceive it, and this is parallel with the record of other pluvial lakes in the Great Basin, of which there are more than a hundred. Lake Lanontan and Bonneville are well known. Their last two seen lake cycles, the last two high lake cycles were as high, and in some cases higher than any of the middle lake cycles.

I do not see from this record that there was a trend toward increasing aridity. Rather, the opposite.

Furthermore, I'm glad to see more emphasis upon recognition of climatic cycles, of which there were about 44 during the two and a half million year term of the whole Quaternary period, and these were inter-glacial type cycles. The usual trend in climate was to have many more fluctuations. These have been now recognized, say, in the last inter-glacial, and especially during the transition from the last inter-glacial into the last glacial. Lots of warm times, then within decades, probably within a person's lifetime, sudden cooling to glacial conditions. This is what not only the Greenland ice cores show, but also pollen records in Europe, and it shows to some extent in the last records in Europe and China.

But we see many more high frequency and high amplitude changes during these larger cycles, and we're just beginning to get the chronologic and stratigraphic resolution to detect it. But what is likely is that there will be a growth of ice sheets in Europe and North America within the next several thousand years. This is a prediction of Alan Berger, a Belgian astronomer who is one of the leading specialists in Milankovich studies.

The Milankovich mechanism seems to be a pacemaker for Quaternary climatic cycles, and being astronomical, it can be predicted, these first insolation changes can be forecast, provide a forecast hundreds of thousands of years into the future.

We understand that there are various feedback mechanisms and so forth, but I won't go into that. But the opinion of Berger, I heard him talk in Berlin at the INQUA Congress last summer. He predicted substantial growth of ice sheets in Europe and North America in 3,000 years. He's published I think between three and 5,000 years, which is rather alarming.

I would like to point out in the--for instance, one of the talks had some interesting data that might be followed, the talk by Peterman on paleohydrology age control from U series and C-14 dating of calcite and opal in the veins in the Yucca Mountain area. Many of those dates are rather old, and they go back to about 50,000 years. Being radiocarbon dates, those probably are only minimum dates and not reliable. But that touches upon, as some of the paleoclimatic studies in Europe and some in other parts of the world have suggested that during the growth of anaglacial, the waxing glacial, the ocean's surface water, the surface water in the oceans of the world is relatively warm, particularly of course in the tropics and the middle latitudes, but this, with the cooling, this is a time where the pluvials of the Quaternary apparently occurred. It is a mistaken concept that the full glacials were both cold and wet.

I've heard this mentioned several times at this meeting. The last glacial apparently ended cold and dry. This is what the pollen records in Europe and North America seem to show. And I think some of these vein dates on calcite and opal, these old dates actually probably are indicative that there were important pluvials during the build-up of the last glacial period in the Yucca Mountain area. That ought to be looked into. That's probably enough to say.

DR. BREWER: Okay. Well, thank you very much, Mr. Morrison. I'm going to take the chairman's prerogative, mainly because it's a question that I have in my own mind based on what I've heard the last couple of days, and Neil Coleman mentioned it as well. The trade-off or the conflict or the difficulties in resolving the modeling approach versus the paleoclimate data approach, and I would really like to hear, by way of summary, I'm going to ask Ike Winograd to comment on your views, and also Tom Wigley, because from my point of view as a non-specialist, I mean you really represent two very different ways of getting at the uncertainties of what's likely to happen at that mountain.

NRC's representative, Mr. Coleman, is saying, in effect, that the models don't add that much value. He'd rather see some more data. I think I represented your point reasonably well. If not, say so. So could you, for me, and I would hope maybe even three other people in the audience, try to resolve a bit the conflict.

Is it either/or, is it and, or where does it all stand? Should we be putting more energy and money into modeling of the climate? Should we be working harder? And I take--it's not even by inference, I just listened to Roger and, you know, he doesn't talk about models, he was talking about data, and your views on this. I'm confused. But I'm easily confused, Ike. Ike, and then Tom, if you would. That's by way of just getting the discussion started.

MR. WINOGRAD: If one believes that there is a role for geologic disposal, i.e. underground, I'm going to start a little off the subject, but I'll get back to it within a minute, if this is still a consideration that the nation wants to follow, then no matter what site one chooses, these type of debates will appear and reappear and reappear, for the unsaturated zone in the sand dune, for saturated zones in rocks, which are always fractured, and there will never been a resolution. There will never be a resolution because you will always have these differences of opinion, valid differences of opinion between scientists and engineers about which model to use and so forth.

So one can abandon geologic disposal altogether and say that we will never converge on any site because of all these questions that are being raised, and we can go to a surface storage system above ground, and Gene Roseboom has written a wonderful essay on this subject, and some of the sociologists, not those here necessarily, are saying store it above ground because the uncertainties are much less than below ground, but they never put error bars on their statements. So that's just one opening statement.

If we stick with the attempt for geologic disposal, then I think it would be arrogant to choose one or the other, between the paleoclimate, which is dear to my heart, although I'm not funded by this project, or climate modeling. I don't think we know enough in either of these areas. There are not many well dated proxy records out there; very, very few, and most of them are less than 30,000 years old. So I don't have that much confidence in the proxy records, which was one of the messages I tried to relay this morning.

I want all the help, if I was in this program, all the help I could get from the other disciplines, all other disciplines. So that would be my first answer, and I'll stop at this point, both are needed in our present state of ignorance if we're going with geologic disposal. But you can chuck geologic disposal if you wish to.

DR. BREWER: All right, thank you very much. That was right on target. Tom Wigley?

MR. WIGLEY: Let me begin by saying I agree with the initial premise of Coleman, and that is that what we need is a defensible range of climates. And I agree totally with what Ike said about needing to define that defensible range using all of the available tools, and that means a synthesis of information from the paleo-record and a judicious use of models.

One of the reasons why models must have a role to play is because I think there is a very high probability that the future, or at least the future will be very different from the past. In other words, the assumption that the past is the key to the future in this particular case I think is a very poor assumption and a dangerous assumption to make. And there are a number of reasons for that.

One reason is that if one makes statistically based projections using Milankovich as the fundamental driving mechanism, projections of future global mean temperature, then the future over the next 100,000 years looks nothing like the future of the last 100,000 years. It's just not possible to just pull out, say, the last 400,000 years and then tack that onto the present and use that as an idea of what might happen in the future. You've got to do something a little bit more sophisticated than that.

One of the disadvantages with using that type of approach, which I think Coleman was partly advocating, is that that only gives you an idea of the global change. It doesn't tell you anything about what's happening at Yucca Mountain, and I don't think there is enough information from Yucca Mountain to be able to extrapolate in a defensible statistical way what's going to happen to that particular site, particularly for changes in effective moisture, which is a very complicated function of temperature changes and precipitation changes.

And another real problem with using local paleoclimatic data is that Ike Winograd has shown in reviewing various types of evidence that there's high spatial variability in changes in effective moisture availability, and there is no particular record for Yucca Mountain, and if you've got a lot of records that show great spatial variability, how in the hell do you get Yucca Mountain out of that.

I think that one has to use both types of evidence, because both types of evidence, modeling and paleoclimatic evidence, is rife with uncertainties, and we've just got to make the best use of these imperfect tools that we possibly can.

DR. BREWER: Anyone care to follow on this particular comment, or this line of discussion?

The uncertainties are huge. I take the advice, I mean, you get whatever help you can get to reduce the uncertainties and that's where we are. But how much is enough? And I want it exactly. Marty Mifflin?

MR. MIFFLIN: Tom, I agree that the climate modeling has important purposes, but I don't think that the purpose that you mentioned to try to get at site specific climatic parameters is a valid one. I think it's very useful for the modeling for other purposes. The reason I say I don't agree with you is that within the region, agreed in the Basin, but right within the region, there is not a proxy record of effective moisture. There is a good record that is a direct record of effective moisture by virtue of the ground water discharge deposits, the evidence where the water table was, and just in the basins immediately to the north, the closed hydrographic basins, the size of the pluvial lakes, and that is a direct measure of effective moisture with all of the climatic parameters lumped in to yield whatever moisture escaped, evaporation after rainfall and runoff.

These direct measures, very, very less proxy than all of the climatic parameters, are there in the region. And so from that particular perspective, I'd say that you can create through the modeling effort a whole series of combinations of, say, temperature, precipitation, wind perhaps, evaporation, but you have something to calibrate those climatic models with right locally at the same scale as your grid scale. I mean, you're starting with something that's already there well dated and measured, and that's my comment.

DR. BREWER: Alan Flint was next, and then Abe Van Luik.

MR. FLINT: There are I guess several ways that I think about using both of these approaches, and Marty's comments about water table rise is a regional concern because the water most likely did not come from Yucca Mountain that caused the water table to rise, but from some area of higher elevations.

What we've tried to do in our analysis, I presented several ways in which we modeled infiltration. One was a static way, which is dependent on current climate. The other way is a more dynamic approach which uses the correct physics we think, and the physics include, I didn't go into detail, but it includes solar radiation, ozone, precipitable water in the atmosphere, turbidity, all of those different kind of components, slope aspects, blocking ridges, whether we have snow on the north slope, all of that.

But to make the transition from a climate scenario into a flux, specifically at Yucca Mountain, you need another model for any kind of scenario that we apply. And we have to look at how we're going to make that transition. I've proposed one way, and that's to use an infiltration model that uses daily rainfall values, because as we've pretty much determined, infiltration is dependent upon conditions within weeks, not within hundreds of years. And what Starley can do, which is of tremendous value to us, is he can look at something like an increase in CO2, which is not something we've necessarily seen in the past, or if we have, we don't recognize it as that, and he can covert that information to daily values of precipitation, air temperature, cloudiness, to be consistent with what his best estimates are.

The other thing that he can do, which is what makes the global climate model very important in looking at paleoclimate records, unless there's another way and somebody could suggest that, of converting that paleoclimate record using a model to match, which he's done, and from that match, he can then provide us the kind of input in terms of the rainfall patterns, not just that it's wetter or colder, but whether it's wetter or colder together or at different times, how that's distributed, whether we have wetter conditions because we have a higher frequency of storms, or whether we have wetter conditions because we have a higher intensity of rainfall or just longer duration. Those are very critical items, and the ability to convert a climate scenario from the past paleo-records into values that we can then put into a conversion model to get the flux at Yucca Mountain is very important.

We need two sets of conditions. We need a regional set of conditions so that we can deal with saturated zone flow systems like the rise in water table that Marty is talking about, and where that water might end up going, and then a localized at Yucca Mountain approach. So I think we do need this way to convert climate scenarios into values that we can then do another modeling approach, convert to specifics at Yucca Mountain specific for the site, for a very, very small footprint on the surface of the Southwestern United States.

DR. BREWER: Abe was next, but I'm going to go to Starley. Would you like to respond to this, just amplify, pick up on it, say it's a great idea because that's what you should say?

MR. THOMPSON: Well, I hope Alan is not giving us too much credit, but in principle what he says is true. If in fact we're able to show that we can reproduce the paleoclimate, analog state, with sufficient veracity, then in principle, you could then take the output of the model, which is much more detailed at least temporally that any paleoclimatic reconstruction is, and then use it to run the infiltration models that Alan has. It's actually an interesting use that I hadn't thought of until Alan just brought it up, but in principle, it should be possible.

DR. BREWER: The thing that's really interesting, and I know Jerry Cohon, because of his question and some conversation we've had, is where do all these parts and pieces kind of come together to answer the question is Yucca Mountain a good place? Abe?

MR. VAN LUIK: The Tecopa Basin badlands, I think that's a beautiful place. I visited there many a time to visit the hot springs and other things, and I'd recommend it to anyone. If you come to Las Vegas, take the time to drive the 80 miles to Tecopa Basin.

In TSPA, we have played around with climate change by multiplying flux by two times, by five times, by .5 times, and we find that these things are significant to performance, but whether or not they are meaningful to performance from the perspective of showing compliance with the regulation of course is up in the air at this point.

Where we feel even more vulnerable than what the multiplier is in the pluvial versus a non-pluvial cycle is exactly what Alan was talking about. How was this flux distributed in the mountain, and this is where we look to the flow modelers to tell us what the distribution is between the matrix and the fractures. The work that Michael Wilson has showed us in the weeps shows that actually it's a good thing to have it confined to fracture flow and have it pass out of the system quite rapidly without seeing must waste.

So, to me, the weightier issue, and I hope one that we get to before this round table is over, is not only the input from the climate, but also the modeling of the flux from the surface to the deep surface. And I think the reason that I mention Tecopa Basin is if you have an extreme climate change, you will have much more runoff. I don't know what that means in terms of the internal water in Yucca Mountain, but you will have a very different biosphere than what we're dealing with now.

DR. BREWER: Jerry had a comment. If you'd grab a microphone somewhere?

DR. COHON: I read something else into what Mr. Coleman said, and a lot of people are putting a lot of words in your mouth, Mr. Coleman. You might want to speak up. But what his statement did for me was create the following scenario, which I can easily imagine in 1998 or 2001 or 2003. Abe, you're sitting in a Congressional hearing or in a licensing hearing, and someone says to you you mean to tell me that the flux will not be 100 millimeters per year? I have 18 climatologists willing to testify right now that at some point during the useful lifetime of this repository, it's going to be that or more.

Now, this is not to say we should ignore climatology and studying the situation both in terms of data and modeling. But what seems certain is that you, me, no one can really defend a position that says we will not have such a flow, a flow that will create conditions in the repository that could create real problems.

DR. BREWER: Does anyone care to respond to that?

MR. MONTAZER: It's not in response to that. I just wanted to comment on something that was earlier discussed as far as the modeling as opposed to the paleohydrologic, how we tie these things together, whether to choose one or the other. Do you want to pass that on?

DR. BREWER: Let's hold that. Abe wanted to respond I think directly to Jerry Cohon's questions. We'll come back to that.

MR. VAN LUIK: But I think we're talking about the same response, but I'm probably wrong. I was going to say that in such a hearing, we would go directly to a person like Alan and say with this increased precipitation, what is the break-out? How much flows down the mountain? It has in the past had some surface flow. How much infiltrates with the new climate scenario? How much is evaporated out of the first 60 meters, or six meters, or whatever? And then we would turn to the hydrologists in the site program and say what is physically possible to shove down that mountain, and that's another constraint on the problem.

So just going by climate alone is not the correct answer, and I thought that's what you were going to say.

DR. COHON: This is go germane here. But the question then is how much money should we invest now in predicting future climate? That's really the question. But in terms of using program resources to refine what will unavoidably be extremely uncertain and very difficult to defend, do we want to use the money that way?

DR. BREWER: Let's see, we've got a queue. Rick wanted to say something, Marty and Parvis. So, Rick?

MR. FORESTER: I just wanted to quickly say that I think models have a great value in terms of exploring all of the ramifications of data, but with all of the weaknesses that go with data when you have 100,000 or 500,000 years of record, that still represents the best actual reality that you can have for a region, and logically when that is taken forward into the future or used as a criterion to discuss the future, it clearly can have problems, but it still represents the best estimation of past boundaries outside of artificial shifts in climate in the future.

DR. BREWER: Okay. The whole boundary issue is something that Marty mentioned in his opening comments. Do you want to follow on this? You've got your hand up.

MR. MIFFLIN: Well, I wanted to make a comment on performance assessment and how it's being handled. If I understand in reading the '93 and the '95 total performance assessment, that the maximum fluxes that have actually been run is something like less than a millimeter. Is that correct? Am I wrong? I saw .3 millimeters per year at the repository level on the '95 total system performance.

MR. WILSON: The fluxes get much higher than that in the simulations.

MR. MIFFLIN: It was? I read something that .3 was the actual simulation.

MR. WILSON: Well, .3 in TSPA-95 was used for some of the hydrothermal calculations. But in the release in flow and transport calculations, they went as high as--I'm not sure I can remember--about ten, I think, millimeters per year.

MR. MIFFLIN: Ten millimeters?

MR. WILSON: In TSPA-93, we went higher than that.

MR. MIFFLIN: Okay. It seems to me that in the performance assessment, based on what we've heard in the last two days where you now have real pretty strong evidence of some of the behavior of the Yucca Mountain site specific conditions where there is localized fracture flow that may be very ephemeral in the present climate and it's very localized, and it looks like you cannot get a distributed flux down through the Topopah Spring host rock, that your performance modeling should be looking at running more water down localized areas.

Now, I understand that we don't know how many of those you should be running it down or what they should be or how long they should be, but that seems like the evidence is there, you don't have to wait, and the evidence was developing quite a while ago, so my feeling is is that the modeling should be redesigned if necessary so you can handle this problem, because it's going to be the problem and it's going to be the issue. And if Alan decides that in a distributed flux over the repository block under climate and in extreme wet period is 11 millimeters per year, and then some of the water is diverted so you can't say it's only the repository area, so you might have to up it, or if he decides it's five millimeters per year and you have to up it then, whatever the scenario comes out, then you have to take that and either you have to get rid of it some place, or you've got to put it down into some zone. And that's what performance modeling should be, I think, and you don't have to have exact numbers.

DR. BREWER: Abe, do you want to pick up on that, and then Purvis?

MR. VAN LUIK: Yes, I'd like to pick up on that, because I'm basically in agreement that the way that we have modeled to this point, we have done some extreme cases using the weeps model, throwing everything into fractures, and show that that has one consequence, and then you were shown those consequences a minute ago, and then we have also used the ECM approach, manipulating the fracture matrix interaction to see what the sensitivity is, and we see there is great sensitivity.

But I think that we are looking to the site program to provide us the model that fits the observations, and then we will work with them to make sure that the next TSPA gives us the best possible, most defensible product that mimics what we see in the mountain.

DR. BREWER: Parvis, you've been patient.

MR. MONTAZER: I just wanted to mention I don't think we have a choice as far as modeling or paleohydrology and paleoclimate. I think they all have to go hand in hand with the site characterization. The signature of paleohydrology is in the site, is in the unsaturated zone and saturated zone. The paleoclimatology is going to provide us with broken records. I don't think there's anybody that's going to tell me that we're going to have a continuous record for the past 10,000 years.

The only way we can tie these things together is through the climatic and infiltration and the site hydrologic models. Adding to that, I think, just following in the performance assessment, the performance assessment eventually has to be based on the hydrologic model for the site. Once the hydrology of the model of the site is verified, to the extent what's the definition of that, to the extent that we can afford in this project, then that hydrologic model should be used with all these different inputs for the performance assessment.

DR. BREWER: Tom Wigley on this point, and then I'll go to Roger. Tom, and then Roger.

MR. WIGLEY: I'd just like to go a little bit beyond what Alan Flint said with regard to the estimation of changes in or the range of possible infiltration rates. First of all, it seems to me that the primary thing that we're trying to do is get a range of possible infiltration rates, and what Alan says, and I agree completely with him, is that in order to do that in a credible way, you need information on a daily time scale, and how one gets that information is a real problem. It's a problem in trying to get it from paleo-data.

I have no idea how you can get daily time scale information from paleo-data unless you go backwards using some sort of model, you know, starting with ground water fluctuations, then going backwards through infiltration and vadose flow model to try and figure out what the variations in infiltration are. And in any inverse calculation, small uncertainties in the input lead to large uncertainties in the output, which is actually what you want as the input. So I don't think that's a good way of approaching the problem.

I don't think water table fluctuations, for example, would be directly related to variations in local infiltration rate. I think it's a very difficult problem to back one out of the other. But I still think paleoclimatic data per se are very useful, but one has to be very careful how they're used.

There are similar problems with using model climate data, and one of the points I made in my presentation is that I don't believe any model data on the resolution that is required for this study. I don't believe model precipitation projections on a 100 kilometer resolution, or even maybe 1,000 kilometer resolution. So, you know, how do you actually use the output of the climate model?

Now, the way that Alan was suggesting is to develop a stochastic simulation model, and the simplest types of such model might have three parameters, two for the amount and one for the model process. And so what you need to do is in some credible way tweak those stochastic simulation parameters, and it is possible to do that with low spatial resolution climate date provided one can show that the spatial variability of those stochastic parameters is not as high as the spatial variability of precipitation, and there is evidence that that is true.

So that it is possible that we can use coarse resolution information in order to tweak the high spatial resolution stochastic simulation model. And it might be possible to do that with paleoclimatic data as well.

I don't think these problems have been carefully thought through at this stage, and maybe we're just at the stage where that issue needs to be addressed more carefully. How do we make the optimum use of the crude data that we have available from two different pathways.

DR. BREWER: Roger Morrison was next on line, and then Ike on this topic.

MR. MORRISON: Models tend to be highly deterministic, and keep in mind that the geologic record, particularly of the Quaternary period, shows frequent crossings of thresholds. The stratigraphic climatic record of the Quaternary shows that there were many, many times of sometimes very sudden changes in types and rates of all sorts of surficial processes, whether it be a stream, various kinds of stream regiment, downcutting, lateral planation, aggredation, soil developing, sand, dust, deposition, all that sort of thing. But I think we need to keep in mind that we need to consider thresholds and changes of maybe one or more orders of magnitude in rates of various kinds of processes.

Models are getting better than they were a few years ago, but we need to perhaps consider chaos there, something of this sort, and open to a larger field of exploration, not just using present modern historic conditions.

DR. BREWER: Thank you. Yes, Ike, did you want to follow up, or even talk about chaos?

MR. WINOGRAD: Chaos? Can I talk on something else?

DR. BREWER: Please do.

MR. WINOGRAD: Okay, thank you. I think we need all these studies, but I get the impression, again, I only pop in on this program once every four or five years and it's exciting and I appreciate the invitation, I truly do, and Ray Wallace tries to keep me up to date, but there's just too much going on. Anyway, the original concept of the site, as written by Gene Roseboom in USGS Circular 903, was that (a) there would be recharge, (b) that the vertical transmisivity of the fractures in the Topopah would transmit the recharge, (c) that other engineering measures could be used, shields, umbrellas, other things, drains, to minimize the contact of water with the waste.

It turns out from what I've heard today and over the phone with Ed Weeks, the fracture permeability is on the order of tens of darcies, much greater than Gene or I ever thought, and I just would like some--well, my question is, and I have a question I guess to Abe, is, now before going on with engineered barriers, I would agree with Marty that you cannot count on engineered barriers if you are tied, if the nation is tied to the Academy's one million year proclamation, then certainly engineered barriers fall, and I think the whole concept of geologic disposal then falls. But if we're not tied to that, then it appears to me this site is just admirably suited to engineered barriers to get around the fact that we cannot answer all these questions about the natural system, and I don't think we ever will, we hopefully will start to converge.

So my question to you, Abe, is is a major effort being given by DOE to engineered ways of keeping the water from the wastes, putting the high fracture permeability to work? Are our studies being conducted to increase the natural flow of air through the mountain to reduce the humidity after the repository is shut? And even when heat is below boiling, is below 100 degrees, what sort of effort is being given to putting the natural permeability to work for you?

DR. BREWER: There's that issue, and if I could add, because it's been a question in my mind for years, I mean the relationship of the uncertainties which are huge, engineered barriers, which is an issue that keeps popping up, but how does this relate to the design of the repository itself, the advanced conceptual design? That always seemed to be kind of the missing piece. Is that kind of on target?

MR. WINOGRAD: Exactly. But it's been there from the beginning. It was there from the initial conceptual writings on Yucca Mountain that this site lends itself superbly to simple engineered barriers if we're not tied into a million years of protection.

DR. BREWER: I wanted to go to Abe because the question was really addressed to him, and then Neil Coleman, in that order. Abe?

MR. VAN LUIK: I'm madly thrashing about looking for an engineer. But from the performance assessment perspective, we have felt all along that what we need in engineered barriers is our engineered barriers that take advantage of the physics that flow in an unsaturated environment, which I think is what you were pointing to.

There is, in fact, a system study which comes due I believe at the end of August, which is looking at various options for enhancing the effectiveness of the engineered barrier system, and I believe they are looking at some of the options that you are in fact hinting at.

As far as air flow enhancements in the mountain, that's a new concept on me personally, but others in this audience may know something of it.

DR. BREWER: Parvis, on this point?

MR. MONTAZER: We have been looking at the potential possibility, actually we've been doing some simulations and we've proposed this on several occasions as to using the natural ventilation if we can keep the repository open without any backfill, without any real engineered barrier. And preliminary results show that we can keep the waste and everything dry if, and without any forced ventilation, just by natural ventilation you can maintain a flow system that will remove the moisture basically for as long as the repository stays open naturally. You know, under certain conditions, the repository can close and collapse, but as long at it stays open, that waste can stay dry.

DR. BREWER: Before I forget about it, Abe, when that study is available, I think we of the Board would like very much to see it if it's possible.

MR. VAN LUIK: Yes, it's been referred to in the past as the backfill study, but it's looking at other options besides backfill. So you may remember it from previous discussions.

DR. BREWER: As the backfill study, yes. I think we've talked about that, haven't we?

Any followup on this particular line? Ed Cording, and then--

DR. CORDING: I know there's been discussion in the program regarding ventilation, and my understanding, I don't know if Dick Snell is still here from the M&O, but my understanding is at present, the plan does not include a ventilation component to it, at least certainly after closure. But I know that several people have been interested in that, and we'd certainly be interested in learning more about what some of these studies are showing.

DR. BREWER: Neil Coleman?

MR. COLEMAN: I wanted to follow up on some of the comments from earlier about paleoclimatology and the global climate modeling, and so on. I want to be sure to differentiate between climate modeling and the hydrologic modeling that is being done at the site, the comments I made earlier referring to global and regional climate modeling per se.

Tom mentioned that you have to be very careful how you use paleoclimate data. Well, I would also add you have to be extremely careful how you use any model and that a model used for any purpose has no more value than the information used to construct it, and that isn't going to change in 10,000 years.

I would submit that projections of future climate that are based on Quaternary cycles would be just as good as any projections that could be made with the assistance of climate models, and especially climate models that pretend to know what people might be doing 10,000 years from now. I can tell you what people will be doing 200 years from now. They'll be scrambling around looking for energy sources. That's for sure.

Climate modeling won't hurt anything in this program. It can't hurt a thing. But I just don't really see the added value, what you have with it that you wouldn't have without it. Proponents of climate modeling often use words like catastrophic to talk about the changes that will come about from anthropogenic activity, especially talking about greenhouse warming. And I mentioned earlier that there is a very finite resource of fossil fuels on this planet. There's no question about that.

But in the debate, you seldom hear about the most important aspect of planet earth as far as life on earth is concerned. The earth is an enormous heat sink. It very much resists major changes in climate, and even to compare the coldest climates of, say, the Wisconsin Glacial Stage with today, they are not actually on a planet wide scale huge changes that would affect the existence of life on earth. These are natural cycles that have been going on for a very long time. And even during the current holocene, there was a period of time with a warmer climate than today.

I mentioned earlier we've had a century of industrial revolution. I believe it was called the hypsothermal time, it was on or about 6000 years ago, also known as the warmest post-glacial time. And one estimate I've seen for it, and some folks here may have more current information, is that the temperature was maybe a degree or two degrees centigrade warmer than today. Now, there was not any major anthropogenic activity going on, and it was during the current holocene, and shows that natural variability even during the current holocene could, I feel, swamps the change in climate that we have seen from anthropogenic activity. And anthropogenic effects are detectable, they are increasing, but I would submit that they have not yet approached the natural climate range of the holocene.

DR. BREWER: Okay, let's have one kind of closing comment on that, and there's one big issue that we haven't had a chance to air in the round table, and that really relates to the presentation by June of the Chlorine-36, and I know that Don Langmuir has been very, very patient and so has Pat Domenico.

MR. MIFFLIN: Could I ask Ike one question, one quick question?

DR. BREWER: Okay, here we go, and then we get to the new topic.

MR. MIFFLIN: Ike, I detected in a polite way you did not really subscribe to the Milankovich correlations, or you kind of are holding back from adopting that as a predictive tool. Is that true?

MR. WINOGRAD: That's correct, yes.

MR. MIFFLIN: And a second question. You did spend quite a bit of time on the predictability of the duration of an interglacial with respect to where we are right now. I would like to ask you, or get some kind of response, if you feel there is not a predictive tool out there that is qualified to project into the future, wouldn't you have to take a conservative analysis and say, okay, we have 10,000 year duration interglacials and we have 20,000 and we cannot say when climate is going to change, so we have to assume in a conservative sense that it will be sooner rather than later, and secondly, we have to assume that it would be a strong one if the Milankovich doesn't--so I think Neil was getting down to how do we bound the decision making.

MR. WINOGRAD: May I answer that?

DR. BREWER: Yes. Ike, with a quick response.

MR. WINOGRAD: A quick response would be I would put on my engineering geologic cap and take a reasonable worst case, not the worst, but something close to it, and then I would go to the engineering geologic community and ask can this site, with this flux, can this site handle it. Is the transmisivity of the unit large enough and other engineered barriers to keep the waste dry most of the time? Because there's no underground environment that I can picture that would keep it any dryer, underground environment. That's what I would do.

I mean, you can take that attitude and dispense with all the academic studies. I would not do that personally, because I believe in a well rounded program. So that's what I would do in response to your question, worst case. And in fact if you read Gene Roseboom's Circular 903, and I plug it, we should all reread it, and by the way, the air circulation concept is in that circular also, you'll see that he did that. He said I'm going to take all the recharge, the annual recharge, and put it down to the repository in a couple hours.

DR. BREWER: Abe?

MR. VAN LUIK: I was going to take the opportunity to kind of answer the question that was asked earlier. I think it's a reasonable thing for the program to continue to invest a modest amount in continuing this, you know, looking at various angles of evidence for bounding what we should be modeling.

On the other hand, you can see by the controversy involved that we don't expect a definitive answer any time soon. But I keep trying to drag this discussion underground because I think, and your answer kind of hit on it, because I think what June has shown is, for example, that Chlorine-36, the good news it's still available there for us to look at, so the repository didn't flush in one day, one month or one year. It took some time for that material to get there.

And then Zell Peterman's stuff shows that in the matrix, we essentially have already endured one or two global climate changes, and still the evidence shows that we have extremely low flux within the matrix, and that's where I think I would focus my major effort, with a minor effort continuing in looking at bounding the problem through global climate work.

DR. BREWER: Don Langmuir?

DR. LANGMUIR: I like Abe's lead-in there. Thank you very much, Abe.

What I was going to try and do was from my very prejudiced personal viewpoint, try to pull together what geochemistry I've heard. This is another piece of our program, obviously, we've been talking about climatology and some hydrology so far, but this is a multifaceted program and my sense is that in the last year or so, geochemistry has contributed a great deal to our understanding of whether this is a suitable mountain or not. And, to me, I've been suggested a number of directions to go because of this.

Let me tell you where I think we are now because of the geochemical information that's come to us, and I will tend to over simplify it because I'm not a hydrologist. I think the issue really is what is the infiltration in the repository block. We're worrying about it up on the surface within 15 meters, but the bottom line is where is the water going in the repository block, and the ESF studies bear directly on that issue.

June Fabryka's studies with Chlorine-36 are telling us we have some fast pathways. I had hoped we would be able to back out of June's work and Zell and Jim's work the volumes of water involved from the chemistry. I'm not sure we can, not easily. But there's a lot of other information available that is highly relevant here.

If it's true, and the sense I'm getting from Zell and Jim's work is that they're seeing very slow precipitation rates of silica and calcium carbonate. If they can show, and I think maybe they're not too far from there now, that these rates have always been very, very slow and they've been fairly uniform, then maybe it doesn't matter what climate is doing.

Maybe the issue is that no matter what climate has done in the past or might do in the future, it will go down those tubes with Chlorine-36, and the block in between, if we can show that the rates of precipitation have been constant and uniform for millennia, are suggesting that it's going off to the side of the block, even though it's coming down all over the place perhaps in the shallow horizons, as Alan Flint would say, the bottom line is what's going on inside the repository block. And if we can find a place in there to put the waste that is as dry as it appears to be from these age dates with uranium and Carbon-14, maybe that's all we need.

So I'm just suggesting that there are a number of things we could pursue to continue with this. I would add Alan had this great idea and started doing it, putting in plastic sheets. We can add the infiltration piece to this, get the volumes by putting those sheets up, and get current volumes of infiltration through the matrix and through the fractures with the plastic sheets. We can put the chemistry into that water and get current information on transport proportions in fractures and matrix.

Anyway, I think these are some pieces that have come to me in the last couple of days, and I don't know whether everybody agrees with me on this or where we are, but I see this as much to the point on suitability as anything we've talked about this week.

DR. BREWER: Let's see, Ed Kwicklis, you haven't said a thing.

MR. KWICKLIS: It hasn't gotten too much attention at this meeting, but I'm always encouraged by the results of the weeps model that Mike Wilson showed very briefly here this afternoon. And he showed a diagram between the number of canisters contacted and flux, and unlike some very early performance assessment analysis, it showed a very linear relationship and a very robust relationship between performance and flux, in that performance didn't deteriorate extremely rapidly at some threshold value and the site fail, and didn't require that we determine between .1 and .2 millimeters per year. It was a very robust performance over a very broad range of percolation fluxes and assumed essentially instantaneous transport from the ground surface to the water table, no retardation.

I don't know what some of the details of their assumptions are, but I think that the project has in some sense overly accounted for in their PA analysis some of the implications of the data that we've heard discussed here in the last two days, and I would ask the Board to keep those analysis and results in mind when considering the implications of these recent data sets.

DR. BREWER: Okay, Ed, thank you very much.

One last comment here, and then we're going to have to move on.

MR. WIGLEY: I just feel as though I have to defend the vast community of climatologists and related disciplinarians who have contributed to estimates of how anthropogenic climate change, or how large anthropogenic climate change may be in the future, and also to add a little bit to what Mr. Coleman said about the paleoclimatic record, which I think was misleading.

Firstly, it is true that proxy indicators show that parts of the land areas of the globe in the summer period of the year were significantly warmer, say, 6000 years ago than today, but there is no evidence to suggest that the global annual mean temperature was any warmer 6000 years ago than it was today.

And in that context, a global mean warming of half a degree celsius that's occurred over the last hundred years is quite a significant event, and we can't say how significant it is because it's quite difficult to reconstruct global mean temperature variations in the past. We can get seasonal variations, site specific variations. We can't get good global mean. So it's an open question as to whether or not the past record over a hundred years is significant relative to natural variability, and that's well admitted in, for example, the IPCC report that has just been published.

But that's not really the issue here. The issue is, and if I go back to another statement that Coleman made by default essentially, he implied that there wasn't enough fossil fuel around to be able to raise global mean temperature very much in the future. Well, that is just wrong.

There's an enormous amount of coal available, in the United States alone an enormous amount, but if you add in

China and other parts of the world, it's enough to raise the CO_2 concentration in the world to something well over 1500 parts per million, and that would have a very large effect on global mean temperature. And the projections, the best projections that can be made that certainly don't have any inkling of using up all the fossil fuel available, the best projections that we have global mean temperature change out to the year 2100 are for a warming of one to four degrees.

At the top end, that's the same amount of warming as occurred over the last 20,000 years from the last glacial maximum to the present, and that's something that one ought to be concerned about, and I think that's a pretty realistic upper bound to the possible change on a 100 year time scale. That's pretty rapid. The change could only be one degree, but even one degree would be, I believe, and I think I can support this with a lot of evidence, that a one degree warming would be well outside the range of variability that's occurred in global mean temperature over the last 10,000 years.

Another unfair statement, I'll just close with this one, that was made was that to the effect that climate modelers say that catastrophic changes are in store. Well, I know of no climate modeler who has made such a statement. There have been plenty such statements in the press, but I don't believe these changes are catastrophic. I think they are very important, but, boy, I'd never use a word like that. I think humanity can adapt to quite large changes and quite rapid changes in climate.

DR. BREWER: Okay, thank you very much, Tom. A quick response from Neil Coleman?

MR. COLEMAN: Sea level was higher 6,000 years ago, so some melting was going on somewhere. I just thought I'd mention that. Also, the coal reserves in China have been cut in half in recent years, the proven reserves.

And I'd also have to mention the fact that it took approximately 3,000 to 5,000 years for the earth to reach its warmest post-glacial time after the melting of the great continental ice sheets. So it shows how long it takes the earth to respond, because of that enormous heat sink I was talking about, how long it takes to respond to these changes in climate. And that's why even 100 years, 200, 300, 500 years of fossil fuels, I question whether that can bring about really, and I think you used the word catastrophic in this meeting at some point, a word like it or a synonym of it--

MR. WIGLEY: I never would use that word. I never would use that, and I think, you know, you're speaking out of the top of your head basically.

DR. BREWER; Time out, please. I think this is a conversation that you two guys should probably carry on on your own, and I expect you will.

What I'd like to do, because he was the leader of the band here for the last day and a half, is to turn it over to Pat Domenico to talk about some implications for the program, as he sees them. And when Pat's finished, we will ask if there's any questioning or comment from the public. So far, no one has signed up. Pat?

DR. DOMENICO: We haven't really addressed that. But, you know, several months ago when I walked into that tunnel and everything was dry, it would give you a nice warm feeling. I'm sure anybody who's been down there gets that nice cozy feeling. And then we start hearing about so-called fast pathways, and presumably there's a lot of them, but they always couple that with but it's a small flux.

Well, now let's bring the climate into it. Is it going to be a small flux under new climatic regime, and that's not a problem that you can engineer. That's a source of water into that repository if they're in that repository that we weren't thinking about, other than the matrix flow in the fracture/matrix interconnections. This is another source, and it may be a large source under different climatic regimes, because those things may be almost ubiquitous down there. You have to investigate every structural feature to see if it's a fast pathway.

But where are we seeing this? We are investigating the east side of the block, and my understanding is the east side of the block is the more structurally disturbed one. The repository is going to lie to the west in less structurally disturbed parts of the rock, and we understand the correlation between fast pathways and structure.

This program needs a western extension of the ESF at the repository level into the rocks that will serve as a repository. Maybe some of the fast pathway problems will disappear in that part of the rock in the sense that we, at least based on the surface mapping, it's less structurally disturbed, and then we're not worried about that any more.

And what might that do to the climate issue? It makes the climate issue much more tenable, much more tractable because if indeed the fast pathways do not exist in the repository region, then the only thing we have to worry about is matrix, coupled matrix and fracture flow as the water getting into the repository as one item, and one I think that is less worrisome is the rise of the water table 200 meters from waters coming in from the north someplace.

So this program is, I think, in desperate need of a western extension of the ESF. And keep in mind we're only getting a small sample of those rocks in that area. And I won't ask June, but I'd be curious to see how many structural features don't produce hits. About half. About half don't produce hits, and there's a hell of a lot of structural features down there.

So I urge the program to think about a westward extension, and maybe some of these problems with disappear, or at least if we believe that correlation holds, maybe some of those problems will disappear. If it's not done that way, we're going to be talking about this forever, and it's never going to be resolved. It's never going to be resolved. So I think let's go to the rocks where the rocks count. That's what I believe.

DR. BREWER: Okay, thanks, Pat.

We do need to make some time for public comment. I've got one name, Dr. Gilles Bussod from Los Alamos.

DR. BUSSOD: Thank you for allowing me to speak.

I simply wanted to bring up an aspect of the program that impacts all of the studies, particularly the predicted ones that go out to a million years, both in climate, hydrology and transport.

We are in a period that's not much addressed this way, but we are in the Dr. Jekyll and Mr. Hyde period of having two regulatory missions, one that is the official present day one of 10,000 years, the other one that is dose based and leads us to do modeling to a million years. And to be honest, I have a very great difficulty understanding what is the scientific or social economic basis for even considering a million years.

A lot of the studies that are coming together in terms of the integration modeling, the integration of all our data over the past decade is showing that if the site will fail, it's on the order of several hundred thousands of years. And sort of the rhetorical question here is is it even reasonable for us to talk about a million year prediction.

Thank you.

DR. BREWER: Thank you very much. And since it was rhetorical, we don't have to find an answer.

MR. MIFFLIN: Can I answer that?

DR. BREWER: Marty Mifflin has an answer.

MR. MIFFLIN: I would like to know who is projecting that the site will fail in 200,000 years in terms of engineered barriers. I think that that's the question; is the site going to fail in terms of engineered barriers in 200,000 years.

DR. BREWER: Dr. Bussod?

DR. BUSSOD: Very quickly, we're dealing with a multibarrier redundant system and what all the studies show is that we have on the level of hundreds of thousands of years. We do have defense in depth. That is, you could have failure of the engineered barrier system and under most scenarios to date, and granted it's only to date, we have the natural barrier system that can limit the dose to the accessible environment within reasonable limits for over several hundred thousands of years. So I would again remind that the barrier system at Yucca Mountain is not an engineered one; it allows for a very good engineered barrier, but it's a redundant barrier system.

DR. BREWER: Okay. If there are no other comments from the public, I would like to thank everyone who participated in the panel. I think we actually covered a great deal of territory. It served as, for me, a very nice summary of discussions that were sometimes, as a non-technician, a bit hard to follow. I think some of the major issues are underlined. They're on the record and that's where they belong.

Thanks to one and all. Thanks to everyone for a very, very productive two day session, a good session.

John Cantlon, our chairman. John, do you have the benediction?

DR. CANTLON: I have nothing further to add. Thanks to everybody. Peace.

(Whereupon, at 4:50 p.m., the meeting was adjourned.)