UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON POSTCLOSURE PERFORMANCE

March 14, 2007

Doubletree Hotel and Executive Meeting Center
200 Marina Boulevard
Berkeley, California 94710

NWTRB BOARD MEMBERS PRESENT

Thure Cerling
Dr. B. John Garrick, Chair, NWTRB
Dr. George Hornberger, Co-Chair
Dr. William Murphy

SENIOR PROFESSIONAL STAFF

Dr. David Diodato
Dr. Bruce Kirstein

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P R O C E E D I N G S

8:00 a.m.

GARRICK: Good morning. I'm John Garrick, Chairman of the Nuclear Waste Technical Review Board. And, on behalf of the Board, we want to welcome you here today for attending this special meeting of the Board's newly organized Panel on the Postclosure Performance meeting on infiltration of water into Yucca Mountain.

I think as most of you know, this meeting arises from questions regarding quality assurance associated with U.S. Geological Survey infiltration estimates that were first identified by DOE, a DOE contractor in December of 2004, and made public by the Department in March one year ago.

The meeting will further a commitment that the Board made to Congress in 2005. And, at that time, I told Congress, and I will quote it, "It would be inappropriate for the Board to draw any conclusions about the impact on the DOE's technical work at Yucca Mountain from the group of redacted e-mails that were posted on the Subcommittee's web site. As disturbing," and I'm still quoting, "As disturbing as it is to see such loosely framed discussions among scientists, the answers to important questions that might be raised by or about the e-mails or related documents should await the completion of comprehensive investigations already underway at the Departments of Energy and Interior.

Continuing with the quote, “The Board will follow the progress of these investigations, and when they are concluded, the Board will evaluate the significance of the results to the DOE’s technical and scientific work. We will then report our findings to Congress and the Secretary of Energy.”

This meeting is part of the process that the Board will use for conducting that evaluation. That process has also included other things: reviewing the findings of the Inspectors General from DOE and Interior; reviewing the technical findings of the Office of Civilian Radioactive Waste Management; and a series of investigatory field interviews with scientists and software engineers at Sandia National Laboratory, the Idaho National Engineering Laboratory, and the U.S. Geological Survey.

As you know, our meetings begin with introductions. This is not a full Board meeting. This is a Panel Meeting. Let me first introduce myself. I am a consultant. I am primarily involved in the application of the risk sciences to a variety of industries, and my background and areas of interest are risk assessment and nuclear science and engineering. And, among my Board assignments is to have the technical lead on radiation Dose Assessment.

As I introduce the rest of the Board members, I ask that they raise their hands, when their name is called. And, let me start with Thure Cerling. Thure is a Distinguished Professor of Geology and Geophysics and is a Distinguished Professor of Biology at the University of Utah. He is a geochemist, with particular expertise in applying geochemistry to a wide range of geological, climatological, and anthropological studies. Working with Panel Co-Chairman George Hornberger, who will be running this meeting, Thure is our technical lead on the Natural System.

George is the Ernest H. Ern Professor of Environmental Sciences at the University of Virginia. And, his research interests include catchment hydrology, hydrochemistry, and transportation of colloids in geological media. George Co-Chairs the Board's Panel on Postclosure Repository Performance, that is sponsoring this particular meeting, and he will be Chairing the meeting. And, George, I want to extend hearty congratulations from the Board to you for being named one of Virginia's Outstanding Scientists and Industrialists of 2007 by Governor Tim Kaine. I really need to find out more about that.

William Murphy. Bill is an Associate Professor in the Department of Geological and Environmental Sciences at California State University at Chico. His areas of expertise are geology, hydrogeology, and geochemistry. Bill is the Board's technical lead on Source Term issues.

You will notice that--where is Dave Diodato? I thought, Dave, you were going to be sitting up here.

DIODATO: So did I.

GARRICK: Okay. Well, Dave is the Jeanie behind the arrangements, the technical arrangements for the meeting, and has been providing the staff support on this particular issue. His expertise is in unsaturated zone and fractured rock hydrogeology.

Before I turn the meeting over to our Chair for the day, George, there are a couple of things that we routinely do. One is to make sure that everybody is clear about the distinction between member opinions and official Board positions. We like to keep the Board meetings as extemporaneously as possible, and unhibitidly as possible. We as Board members express ourselves freely, and we want to be able to continue to do that. So, when Board members speak that way, it is important to realize that we are speaking on our own behalf, and not necessarily on behalf of the Board. And, when we are speaking on behalf of the Board, we will try to make that clear.

The second thing I should mention is that, as usual, following the presentations, we have scheduled time for public comment--as aspect of our meetings that is extremely important to the Board. And, if you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance of the room. And, of course, written copies of any extended remarks can be submitted and are welcomed and will be made part of the meeting record. Some of you have asked about questioning during the course of the presentations. Our preference is for you to write down your questions, and submit them to Davonya Barnes or Linda Coultry, and they are in the back of the room at the sign-in table. We will cover as many questions as we can, time permitting. And, finally, I would like to ask of all of you, including myself--I'd better make sure I did it--to put your cell phones on the silent mode.

So, let's proceed, and, George, will you take over this meeting?

HORNBERGER: Thank you, John.

Because of the nature of this meeting, by the way, I should clarify one thing to start, I am George M. Hornberger. I work for the University of Virginia. Not to be confused with George Z. Hornberger, who works for the National Research Program of the United States Geological Survey. And, neither that George, nor this George, has done any work on the Yucca Mountain Project.

As John said, we are here to examine the scientific and technical aspects of infiltration estimates for Yucca Mountain. During today's meeting, we will hear a series of presentations that range from the general to the specific with regard to Yucca Mountain infiltration.

The first presenter is Dr. Scott Tyler from the University of Nevada-Reno. Professor Tyler has expertise in arid region hydrology, and he will present a talk on groundwater recharge in the Mojave Desert.

That presentation will be followed by a talk by Dr. Alan Flint on the history and technical basis of the 1999 USGS estimates of infiltration at Yucca Mountain, Nevada. A research hydrologist with the U.S. Geological Survey, Dr. Flint was Project Chief for Regional Meteorology Infiltration and Matrix Hydrologic Properties Studies for the Yucca Mountain Project. And, his expertise is in developing and applying methods to characterize arid land hydrology.

After a short break, we will return with an introductory presentation by Dr. William Alley from the USGS, describing how the USGS has responded to the e-mails from USGS hydrologists working on infiltration.

Following that introduction, USGS hydrologist Dave Pollock will describe the preliminary results of the investigations that the USGS has conducted.

After that, we will break for lunch. After the lunch break, the afternoon will feature a series of presentations by DOE and DOE contractors. First, Gene Runkle from DOE will give a high-level overview of all the actions that DOE has taken in response to the USGS e-mails.

Then, Dr. Daniel Levitt, Los Alamos National

Laboratory--Sandia National Lab? Los Alamos National Lab, hydrologist will describe the results of the Idaho National Engineering Laboratory technical review of INFIL, the model used to estimate infiltration for the Yucca Mountain site recommendation.

After a short break, Dr. Josh Stein from Sandia National Laboratory will give two presentations describing the major components and results of the new infiltration modeling method known as MASSIF. Geologists in the audience, it’s not MASSIF, it’s MASSIF, I’m told. Dr. Stein has been leading the team developing this new net infiltration model for the Yucca Mountain Project.

Finally, Lawrence Berkeley Lab hydrologist, Dr. Jim Houseworth, will present a numerical evaluation of the technical impacts of the new model results on unsaturated zone model tests. Dr. Houseworth has been providing technical and management support for performance assessment and site characterization at Yucca Mountain for 14 years.

And, finally, as John said, we have set aside time at the end of the day for public comments. If for some reason you are not able to remain until the end of the day, then please let Davonya Barnes or Linda Coultry know as soon as possible, and we will try to find a few minutes at the end of the morning for your remarks, if time permits. Even if time does not permit, we encourage you to submit your remarks in writing for the record. So, again, please, as John said, please set your cell phones or cell devices to the silent mode, and let’s begin.

And, it’s a pleasure to introduce my friend, Scott Tyler from the University of Nevada at Reno.

TYLER: Thank you, George. Do we have a pointer? Okay, thank you.

Okay, first off, I’d like to thank the Panel and Dave Diodato and his group, the Staff, for inviting me to come and speak to you today. It’s an honor and a privilege.

The title of my talk is, as you can see, quite an overview talk of recharge processes. I’m just going to try to hit on a few of the highlights and discussion points, and raise some questions regarding recharge.

Okay, so in the overview, what we’ll talk about is, again, some of the dominant processes that I think are important with respect to recharge in arid regions; a little bit, a very brief summary of some of the previous work that has been done and published, and some of the methods that we are currently using, or propose to use for recharge; some of the issues regarding uncertainties in either those methods or the data that is collected from those; a discussion of important questions that still remain. And, I think we would all agree that there still remains significant questions. And, then, finally, I want to touch briefly, as this is a postclosure working group, is to talk about the implications and impacts of our knowledge base, our uncertainty in recharge, as to how that relates to potential uncertainties and knowledge base about monitoring the performance of the repository in the long-term. Many of the same processes that are involved with recharge in the shallow soils and fractured rock at Yucca Mountain and other areas in the Mojave apply to the issues and facts that we'll have to deal with in monitoring in fractured rock at the repository horizon. So, I'd like to make that as a little bit of a concluding remark.

Okay, what are the dominant processes controlling recharge in arid climates? Well, there are the usual suspects, I'll call them, the standard things one would think about if one is going to do a water budget in an arid region or any region: precipitation, rainfall, snowfall, how much is there, its timing, when does it occur, is it intense storms, are they low frequency, or low intensity storms, those kinds of things. Obviously, temperature, fluctuations in temperature, seasonal fluctuations, mean annual temperature, whatever you want to put in there, obviously controls the operation. The soil type, whether it's fractured rock, whether it's coarse textured material, fine textured, that controls where the water goes once it hits the land surface. Solar radiation, that's the prime driver for evaporation and transpiration, it's the source of energy. We can also add to that wind, if you want, and if you add wind to it, then you get Penman-Monteith evaporation, or Priestly-Taylor evaporation, whatever, are standard estimates for potential evapotranspiration.

In arid regions, however, there are a few other things that I think we need to consider, and have been considered, and still need consideration, I'd say a little bit more, and, these are things one does not normally think about in humid regions, or even in semi-arid regions. The vegetation and its adaptation to the climate. Climate in arid regions is, as I'll show you in a moment, and you probably all know, is much more variable with respect to the drivers that plants like to see, water, temperature, and those plants have adapted to live in an environment that is much harsher than vegetation that would be living, say, in Virginia, where it's always happy and nice in Virginia. No? Sometimes it rains there.

The other factor, one other factor that we have to deal with in arid regions, which we typically don't think about much in more humid regions, is bare soil evaporation. Bare soil evaporation can be a significant component of the water budget, or the water loss, in an arid environment because we have lots of bare soil. The landscape is not covered by vegetation. Bare soil evaporation is also important in more humid regions, particularly in the early seasons, during say, in agriculture when plants are beginning to grow. But, once the plants get fairly well established or leaf out, then bare soil evaporation is not particularly important.

The variability of the climate, as I said before, desert regions have perhaps, at least in the Mojave, much wider swings in climate than we might expect, and those swings are not just diurnal or seasonal, they are often on much longer time periods than seasonality. And, that separates or distinguishes this environment significantly from more humid regions.

Depth to bedrock. This is an important component, and I think we will hear more about this by my colleagues later on, speaking about infiltration at Yucca Mountain. But, if you have thin soils and fractured rock underneath them, then the amount of storage that you can keep the rainfall in in the soil is much depleted, is much less. And, so, therefore, we have a reservoir issue of where can we store water, and primarily, we store that water in the soil. So, the depth of that soil horizon is important.

There are a lot of other things I just threw up here. Fire is an issue. Certainly, in desert climates, fire is not typically thought of as a major problem, but in the northern parts of the Great Basin Deserts these days, because of fire and the invasive species, we’ve dramatically changed the ecosystems, and the rooting depth, and the water use efficiency of vegetation. And, so, fire is an important component in the long-term of looking at water balances.

Next slide. Let’s talk briefly about vegetation adaptation. Again, arid species, they have adapted to living in a harsh and highly variable world. In the Mojave Desert, as I come from Nevada, so we get to talk about gambling here just a little bit, winter precipitation is a safer bet for vegetation. We typically do get precipitation in the winter season. It’s cooler, it’s at lower intensity, and it’s much more uniform in its distribution. That is, when it rains, it rains pretty much everywhere, as opposed to summer convective storms. So, the species have adapted there to live with that winter rainfall, use it, and perhaps at least in many cases now we’re finding out that the deeper rooted species pay little attention to changes in water content, and changes in nutrient levels if we add those in the summer. Simply energy-wise, not advantageous for that vegetation to try to use the water then, because it is (a) infrequent, unreliable, and also the nutrients are not available at that time as well.

So, Las Vegas and Yucca Mountain is moderately close to that transition of monsoon precipitation that one sees in the Senoran Desert. So, a change to that kind of environment, that kind of precipitation regime, would result in a significant change in the water budget for some time until the vegetation adapts, or the vegetation ecosystems change to deal with that summer dominated precipitation.

Also, a very interesting thing that has come up fairly recently we've begun to look at is this summer senescence of the vegetation, that is, the vegetation not doing much in the summer, leads to exclusion of nutrients and concentration of nutrients in soils that we would never ever see in more humid climates unless we're adding nutrients and fertilizer at tremendous loads.

Take a look at the next one. These are some data that we collected quite a while ago. We're beginning to come back and look at them again. This is nitrogen concentration in a desert profile at Yucca Flat, which is just north of--I'm sorry--east of Yucca Mountain, and we see nitrogen. This is a borehole about 50 meters deep, and we see this enormous concentration of nitrogen. Also underneath here is the chloride concentration in the soil water, basically tracking the nitrogen. We see nitrogen concentrations of 3000 milligrams per liter below the active rooting zone, and then very low concentrations below. This is an enormous concentration of nitrogen, which we would never see in a more humid region. In fact, this level of nitrogen is quite toxic to vegetation, and the source of that nitrogen is atmospheric deposition, fixation at the land surface, and then accumulation below the rooting zone, or through the rooting zone, where it simply is not flushed out of the system. And, the mechanisms then for accumulating that are actually still somewhat questioned.

If I try to simulate this profile with any of our existing numerical simulators, including if I take into account vapor transport, non-isothermal fluid flow, root uptake, the only way I can reproduce these kinds of profiles is to do some odd things to the vegetation, about turning the vegetation on and off as far as its exclusion of nutrients, and also do a, I would say cheat--I don’t want to say cheat, but let’s say put in unrealistic root uptake functions in order to produce this kind of a very high concentration, the way we see it here. So, we still have some things to learn about roots and vegetation in these arid regions that are critical with respect to recharge as well.

Bare soil evaporation, another component that again we need to deal with in arid regions. Because of the sparse vegetation, we do have large bare soil, or large exposed areas in profiles at the surface. However, what we do see is most of the deep rooted species, the shrubs, do typically have large concentrations of roots in the intershrub areas, that is, the bare areas in between, so while they may look bare from the surface, there is root uptake, and then directed to the plants laterally. But, again, those root systems may or may not be active during summer periods. I would suggest to you that our state of knowledge of desert species such as we see in the Mojave is still moderately in its infancy. There's a great deal of work being done as we speak looking at vegetation, but it is just beginning.

And, here's just a typical Mojave leaf area index, if you will. This is from the desert FACE, a carbon experiment, a CO2 experiment on the Nevada Test Site. And, you can see the lack of vegetation, or the sparseness anyway, and the large bare soil components. But, again, there's a component of bare soil evaporation, but there's also root water uptake that would move moisture potentially from the center area, let's say, over to one of these shrubs.

Okay, next slide. Climate variability, a huge driver I think in water balances, and, again, vegetation response in arid regions. This is just annual precipitation from Beatty, which is just west of Yucca Mountain. I took from the Western Regional Climate Center database, rainfall from 1973 on to today. And, on the left axis is rainfall in inches. And, I don't know about you, maybe you've seen this before, but I'm continuously amazed at the level of variability in precipitation in an environment like this, where we go from essentially nothing, let's say in 2002, several years earlier, we had 12 inches of precipitation at the same site.

There is not--maybe you could draw some periodicity to this if you wanted to, and there are some underlying periodic components I’m sure in this, and so derive components. But, if you’re a water manager and your job is to manage a water utility system, and this is your input, this is what you have to work with, there are only two things you can do to manage a water utility if this is your input. Number one, is have a very flexible public, i.e. demand, okay, where they can survive with little water for a whole year, or have a large reservoir, or you can buy water from somewhere else, I suppose. But, if you think of it in terms of water management, this is what the plants have to live with. The plants are like your customer in a water utility environment, and they indeed have learned to adapt and manage in a system like this, and maximize their below ground biomass, their above ground biomass, and their survivability, and their continuation, producing seed, and things like that. And, how they do it is through a variety of complex and at times not well understood ecological responses, essentially shutting down during these times, and going like crazy, changing the ecological community during these wet periods. And, I think the next slide shows that.

Again, this is from the desert FACE site in Frenchman Flat. This is a winter shot in one of the FACE rings. My colleagues are releasing carbon dioxide into these sites to look at the effects of CO_2 on vegetation. And, here’s a winter shot with, you know, what looks like fairly quiet vegetation, sleeping vegetation. And, this is what that same site looked like, or one of the other rings very close-by, in the late spring after one of those 12 inch winters, or 12 inch annual precipitation years. And, you see this blossoming of shallow rooted vegetation that takes advantage of the soil moisture during that time, and this is a once every four or five year occurrence. This doesn’t happen every year. So, seasonality is much longer than we might expect.

And, if one is to do water balance modeling at the land surface and calculate recharge, you have to be able to take into account the fact that your ecosystem, your vegetation will change through the season. There’s a strong coupling between precipitation and even when it comes, and the type of vegetation that turns on and turns off. This grass was here sitting there as seed, it was a wet year, it goes like crazy, produces lots of above ground biomass, uses a lot of nitrogen, by the way, while these other plants are not using much nitrogen. And, then, senesce, set their seed, and wait until the next wet year.

Now, we talked a little bit about depth to bedrock. I just want to give you a very brief example of the importance of depth to bedrock on recharge that we’ve seen.

This is what I'll call the tale of two sites, or cities, at the Nevada Test Site, close-by one another. Frenchman Flat is a site that I worked on years ago. This is the site of a low-level radioactive waste site on the Nevada Test Site. It's just east of Yucca Mountain.

Another site, Cane Spring, which is between Yucca Mountain and Frenchman Flat, the two sites are moderately similar. I just put down some rough estimates of annual rainfall, annual potential evapotranspiration from the two sites. Cane Spring is slightly, the catchment is slightly higher than the Frenchman Flat catchment, but still quite arid, 150 millimeters of precipitation, and PET greatly exceeding rainfall, as one would expect in an arid climate. So, quite similar.

Next slide. This is just an aerial photo of Frenchman Flat. This is one of their low-level radioactive waste sites. Again, you can see the dense vegetation that grows out here, you know, one plant per few square meters, primarily Larria Tridentata (phonetic), as well as if this was after a big spring rain, you'd see a lot of grasses here.

Next slide. This is a photo from near Cane Spring. I couldn't get a good photo of Cane Spring. This is an old low-level radioactive waste disposal site. It's an old cabin. But, the vegetation is actually quite similar, the vegetation species here are similar to what we saw in the other photo, a little bit different, but pretty much the same, and moderately the same density. So, similar sites.

Next slide. But, the similarities do end there, and significant differences after that. Cane Spring is a flowing spring, as the name would imply. And, it is supplied by modern recharge. There is tritium in the spring water. There is model stable isotopes, fairly warm weather stable isotope concentrations. It's an active recharge zone.

Portions of Frenchman Flat near that radioactive waste disposal site have not seen recharge to the water table for as long as in some cases 120,000 years. Roughly the same precipitation. Roughly the same potential ET. Very similar, or very close to one another, and yet dramatically different behavior. The main difference is that the Cane Spring's catchment, the catchment area for that spring is typically thin soils overlying fractured bedrock, while Frenchman Flat is very thick alluvium, 200 to 300 meters of alluvium to the water table. Huge differences in the reservoir to store rainfall. So, again, Cane Spring has little or no storage, or the Cane Spring catchment has little or no storage for evapotranspiration to take hold.

And, I would just postulate here from what I have seen, and I cannot say that I'm an expert in Yucca Mountain literature, I have not followed it as closely as perhaps I should, but I have not seen in my literature searches a significant amount of study of these what I'll call Yucca Mountain analog sites that we could use to test some of our models of recharge. They're simple, they're easy, they're out there, and they provide great datasets for validating, calibrating, verifying, whatever words you'd like to use, models of recharge. There has been some work done, but there still remains I think a significant amount that could be done.

Next. Results from Yucca Mountain, so we'll hone in a little bit on Yucca Mountain. I'll just summarize some of the results that, again, I think most of you may be familiar with. Bridget Scanlon and others in a 2006 study of the world of recharge, or recharge of the world, I guess is the paper, looked at recharge studies in arid regions around the world, came up with on the order of about 100 studies that she and her co-authors summarized. Of those, only about eight that I could count, and I'll say that is approximate, maybe add a few to that, dealt with arid regions where we had thin soils and/or fractured bedrock, or fractured clay horizons that the fluid was passing through. So, by far, the dominant place that people have studied is in soil profiles, soil horizons.

There's a reason for that. It's much easier. I know, I've done it. What I think is important to take home from that work is that of the eight studies that she reported, they report a wide range of fluid fluxes or recharge fluxes, much wider than the range that one would see in, say, soil or alluvial profiles under natural conditions.

Where people use thermal modeling, that is, balance a heat conductive modeling, or numerical studies, numerical fluid flow studies, those typically produce the relatively small range in recharge. However, if there was a study that used tracers as a measure of recharge, there was a much larger range of variability of the predicted or measured, or what have you, fluid flux.

The high variability of tracer fluxes to me has a fairly serious implication for issues at Yucca Mountain. I think we can expect to see a highly variable fluid velocities and highly variable recharge rates in a fractured environment like Yucca Mountain.

Next one. I've started to work a little bit at Yucca Mountain for the State of Nevada back in the early Eighties, and, so, I've actually been able to progress and watch the rate of recharge at Yucca Mountain increase over the years, even though the climate has not changed dramatically. In the early Eighties, it was viewed that recharge at Yucca Mountain was negative, in fact, in some cases. There was upward flow, and, in fact, there probably is upward flow in some areas. But, over time, I think through significant data collection at the site, the recharge rates have increased quasi linearly, if you will, to on the orders of 10 to 20 to 30 millimeters a year, as not an uncommon amount of recharge in certain environments at Yucca Mountain.

And, again, I'd say that causality is not a result of changes in climate, but of quality data collection, and people thinking about recharge in arid regions more carefully. The same kind of a progress of recharge, by the way, if I looked at the Hanford reservation in Eastern Washington, I think I would probably find the same kind of a progression of recharge. As people studied it, we see that there's more recharge.

What's been done at Yucca Mountain is a very brief and annotated summary of some of the work that I'm familiar with. The group working on it has used Darcy's law, using measured water potentials in the non-welded units, the porous media units of Yucca Mountain. The only place one can really do that is in the non-welded units, and unsaturated hydraulic conductivities to calculate fluid fluxes through those zones, I think with moderate success.

There's been numerical simulations used to match the measured water potentials in some of the environments. There's been, and I've done this as well, we've matched temperature profiles that we observe in the deep boreholes to thermal modeling. So, we allow the heat to conduct. We also allow the heat to be advected with the fluid, with the recharging fluid, and from that, you can back out an estimate of fluid fluxes.

However, I would toss out there anyway that the properties of the fractures, which are the dominant flow processes I think, at least in the repository horizon, from what I've read, and in the welded units, are inferred. They are not measured. So, the dominant mechanism of fluid flow in fractures, typically those properties are inferred.

What are some of the questions that remain out there? These are only a few, and there are many more, and there are probably answers to some of these questions. Chloride Mass Balance, we've used this extensively to estimate rates of recharge and rates of fluid flow. I won't go into the details of Chloride Mass Balance. I'm sure others will, or the group has seen those.

However, I would argue that while it's a robust method, it works reasonably well. We don't get the root zone right in our modeling of chloride or nitrogen. Chloride is easier, our nitrogen transport through the root zone. The models we use assume unrealistic root uptake, and as a result, we can make them fit the data, but they are not really following mother nature. So, we have some work to do there, and that's in the top two meters. I won't talk about the next couple hundred meters below that.

Matching of thermal profiles. I think this is a fairly promising approach to look at recharge as long as the rates of recharge are moderately high. I would ask the question, and I don’t know the answer to this, but there are issues potentially with thermal equilibrium, where we have rapidly flowing fluid down a fracture that’s episodic and it needs to transfer its heat into the matrix that you’re actually measuring the temperature of, and I think the transients may be much faster than the equilibrium time for the temperature into the matrix block. So, are we really predicting recharge from our observed temperatures? I’ll leave that as a question.

The fracture flow behavior and the hydraulic properties of fractures I remain quite concerned about at Yucca Mountain. I think this is, even as recently as 2006, these are not direct quotes, but fractures dominate the flow regime, but the fracture density and the fracture apertures are not well characterized. Bulk rock permeability data remain scarce. And, the only fractures that have been hydraulically characterized are those that are filled.

I am not particularly interested in fractures that are filled. While they may be an important fluid flow path, it’s the open fractures that may be the more significant fluid flow paths. The reason we characterized the filled fractures is because, again, they are easier to characterize.

We can use our traditional hydraulic properties, and Richard’s equation type solvers to characterize those filled fractures.

Next slide. So, now, I’d briefly like to just talk a little bit about how issues of recharge may impact monitoring. And, I’m a strong proponent of designing a monitoring program that is well designed before anything is built. I’ve worked on several sites in which monitoring programs were an after thought, and if they’re an after thought, then typically, they don’t work very well, and they don’t do what they’re supposed to do.

So, if you think about it, the possible range of recharge at Yucca Mountain is only about that much, 150 millimeters. That’s all it ranks. So, the recharge is between 150 millimeters and zero. Or, it could be negative, I suppose.

Current studies, we’ve narrowed it down, which is excellent, by a factor of 5 to 10 to maybe 20, maybe a little bit more. So, we’ve closed that gap, and we’re down to something maybe on the order of that distance.

However, my concern is that we still remain, and we still have a significant difficulty measuring the spatial or temporal variability in recharge at the land surface. And, if that’s the case, how will we do it at the repository horizon where it’s much deeper, much more challenging environment to work in? Again, we see this large variability in fluid fluxes. We should use that information when we're designing our monitoring program. That's what we would expect to see then, is large variability in fluxes. So, our repository monitoring program has to be designed around this existing data. What do we know now, and use that information to design our system.

Next slide. Key elements of a monitoring plan, any monitoring plan. You have to be able to detect a leak at low concentrations, in this case, for radioactivity. You have to be able to detect a leak in time to stop it from moving very far, whatever far is, you define that, and, you have to be able to identify its source with your monitoring program, so you can do something about it. Your monitoring program must have in place well defined actions. What do you propose to do if you find, or perhaps when you find behavior. And, the monitoring program must tell you which of your pre-determined actions or courses of action you should follow, is the most logical.

Next slide. So, questions for the monitoring plan at Yucca Mountain. What are the mechanisms of fluid flow in the fractures? What have we learned about that in the near surface environment for recharge? Is it episodic? I believe the answer is yes. Is it chaotic? I don't know. Creeping flow, is it viscous, is it inviscate flow? These are critical questions one has to answer before you design sensors.

If the recharge flux is primarily in the fractures in the repository horizon, how will monitoring equipment detect it there? What kind of monitoring tools are available to measure activity in fractures? And, what monitoring tools are available to detect contamination in, I put up here .01 percent of the rock mass, 1 percent, but the fractures constitute a tiny amount of the rock mass, and yet that's where we need to be able to detect a leak. At typical fracture velocities, there's a recent paper coming out that says fluid moves in fractures at the order of on average 13 meters a day. This is from a whole variety of case studies that were done recently, John Nimo has just published, that's a mean value, geometric mean. But, even at typical values, a leak or a contaminant can move, let's say, a meter per day in a fracture. What techniques do we have that can measure at that frequency in order to detect a leak?

Next slide. Failure rate, monitoring equipment needs to be moderately failproof, and I think colleagues at the USGS have installed quite a few sensors at Yucca Mountain and I think the reliability of--they've certainly learned a lot about the reliability of sensors in harsh environments.

And, finally, what are the courses of action to be followed if a leak is determined? Again, a monitoring plan needs to have those identified a priori. And, I just would ask the audience here and the Panel to consider how you might change your answers if you were designing a repository in a fractured environment versus one in a porous media environment. Which are easier to answer?

So, I'll just summarize, running a little bit low on time, I do believe we have progressed significantly in our ability to measure recharge in arid climates. It's become an area of focus. Twenty years ago, there were very few studies. Now, there are quite a few. We've learned a lot in that time period. Recharge estimates at Yucca Mountain have, I would say, matured considerably and benefited significantly from field data collection and laboratory measures.

The studies of recharge in which fractures or macropores are present typically have shown higher rates of recharge and/or higher rates of fluid velocity, much more rapid migration of water and contaminants than we would see in a porous medium environment. I would postulate that our technology for monitoring fractured medium, whether it's radioactive waste, hazardous waste, or anything, remains significantly untested to this day, and is hampered by our continued lack of experience working in fractured rock. We're learning, but we still have a long ways to go.

One more. I want to close with some remarks from Tom Eakin of Maxey and Eakin, although he pronounces his name

Eakin. Tom recently received a lifetime achievement award from the Nevada Water Resource Association. Tom worked with George Maxey in the Fifties to develop what many of us have used over the years as a very simple empirical, but well used measure of recharge as a function of rainfall or climate. And, Tom is in his nineties, healthy, very lucid, got up and spoke, and I paraphrased some of his words. These days, there’s a lot more focus on recharge, not just at Yucca Mountain, but also in other parts of Nevada and Utah and Arizona for increased groundwater withdrawal, so balancing, understanding recharge is critical for estimating water resource availability.

And, Tom’s parting quote to all of us in the audience was, and again I paraphrase, “We can easily mistake our understanding of our models--which Tom developed the model we used for years--for a deeper understanding of the real workings of nature.” He did leave us with a positive note. “Always keep that in mind and never stop trying new ideas and new experiments.”

Thank you.

HORNBERGER: Thanks, Scott. Questions from Panel members?

TYLER: Do I stay here or do I go there?

HORNBERGER: Stay there.

TYLER: Stay there, okay.

HORNBERGER: I'll start off. I'm intrigued by your comparison with Cane Springs, suggesting that it's a coarse, but shallow soils. But, if you're going to employ a spring, you also have to have some way to drive the flow laterally, and you didn't mention that.

TYLER: Okay, yeah, the geology of Cane Springs, we didn't talk about that, but there is, the geology is such that there is a lower permeability unit, a perching unit well above the regional water table, which provides, which then daylights in the rough topography. And, so, that's where the water daylights out. But, if that perching unit wasn't there, that water would continue on downward to the water table, to the deep regional water table.

HORNBERGER: So, it's in that sense, that you were suggesting that these studies could be used to verify in some sense the models?

TYLER: You have an input, you can measure what the rainfall distribution is, you know what the discharge is within reason. You can actually physically measure that easily by putting a bucket there, not have to estimate it from vadose zone properties.

HORNBERGER: Other questions? John?

GARRICK: You spoke of the dynamics of the ecosystem and the problems of modeling those dynamics using the precipitation history as an example. Is it so difficult to model those kinds of dynamics, and why is it a big issue?

TYLER: No, it’s actually very easy to model those dynamics. It’s very difficult to populate the model with the parameters for the vegetation, how the vegetation would respond to those. So, no, I shouldn’t say it’s easy to model. It can be simulated, numerically, and the codes that are available can indeed handle root water uptake very well, not isothermal vapor transport. However, in all cases, you have to populate that with parameters that relate to the hydraulic properties, and also now the vegetation. When do the roots turn on, when do the roots turn off, and that I would suggest, and I’m not a plant ecophysiologist, but I work with some of them and I’ve read the literature, and we’re a bit far behind on populating those parameters into our models.

GARRICK: Is transpiration a new field?

TYLER: No. But transpiration in desert environments moderately is because it--

GARRICK: Because it seems to be one of the big reasons why there was a difference in the results with the INFIL code versus the MASSIF code, was it all had to do with the evapotranspiration, or at least a lot of the differences, and, so, it obviously raises the question is this such a big deal that it can’t be modeled reasonably accurately based on the supporting data. And, why isn’t it done?

TYLER: Again, I would just say that the supporting data in desert ecosystems, desert vegetation, what is the wilting point of the entire--each vegetation type that's in the, say in the Mojave.

While there are some people have studied bits and pieces of that, it is nowhere near the level of study of, say, alfalfa or corn or cotton that are agricultural crops. No one particularly cared. There was not the economic incentive to really study these. It can be done, and there are people, there are many, many more now, desert ecophysiologists working on these various species. We're working on one site in the Mojave where we're interested in this nitrogen uptake issue, however, we have data only on one of the four major species that are growing in the plots we're looking at, detailed plant physiology data.

GARRICK: But, it would seem that with infiltration being the driver for performance, and that all the comments we're hearing about the lack of information on fractures and the properties in the unsaturated zone, and that we have spent $12 billion and haven't answered these questions yet, that it's something that should be important.

TYLER: Yes.

GARRICK: Okay.

HORNBERGER: Bill?

MURPHY: This is Bill Murphy of the Board. I thought that was a very enlightening talk. I enjoyed it, and I wonder if you would take just a moment and refer back to this slide where you did have the nitrate and chloride as a function of depth?

TYLER: Okay.

MURPHY: And, tell me in rather general qualitative terms what this means about infiltration at this site. Does this mean that there's no infiltration in fact, and that all the chloride gets hung up below the root zone? And, if that's the case, where does the water come from that's below that?

TYLER: Okay. Okay, it's about six or eight slides in. The slide shows a very high concentration of chloride and nitrate down at about, I can't remember, it's about 2 or 3 meters down below.

MURPHY: It's Slide 5.

TYLER: Okay. And, so, what we have postulated, as well as others, is that that is an accumulation point of both nitrogen, which comes from dry deposition and some fixation at the surface, and chloride, which is coming in in wet and dry deposition, and there essentially is no moderate recharge going on at the site. There is no net downward movement of water below about several meters in this graph. Why is there low concentration of, let's say, chloride or nitrate deeper in the profile from 10 meters down to, in this case, the water table is at 300 meters, I believe? We postulate that that very low concentration is an indication that in times past, this vadose zone was flushed. There was recharge of a few centimeters per year, which was sufficient to keep the concentrations of conservative species, such as chloride, very low. So, there are other mechanisms going on. There is perhaps some thermally driven vapor transport in the deep profiles, but essentially, it's just the water comes in and all of it is evapotranspired, leaving behind the solenity.

MURPHY: I recall in the past, Yucca Mountain, people interested in Yucca Mountain have collected similar sets of data for the alluvium filled areas, like Frenchman Flat and for the ridges. In general, are there data such as this to draw generalizations for Yucca Mountain?

TYLER: I'm not familiar with how much chloride data has been collected at Yucca Mountain in recent years. I know that in the past, there was some, and perhaps some of the other, the people working on the site could elucidate more on that. I think there are some data that would be very similar to this, certainly from the deeper soil horizons.

MURPHY: I have one other line of questions. I was interested in the distinction you made between the fractures with fracture fillings and those that were unfilled, and a very clear impression one gets underground at Yucca Mountain is that indeed, some fractures are loaded with calcite, and others are completely bare of any mineral infilling, and what’s your sense of the relative significance of those two types of fractures in terms of fracture flow?

TYLER: My first reaction would be that the ones that are filled with calcite are going to behave much like a porous media, low rates of fluid velocity, low permeability. I haven’t looked at those data, so don’t quote me on that. But, the fractures that are open, would behave in a completely different manner. They will behave in a way which is not necessarily predicted by a Darcy’s law lamina or viscous type flow. They can behave in a more chaotic manner, gravity dominated flows. It’s very similar to what we see in soils, in that the flow mechanisms in these open fractures, if they are indeed open significantly, are completely different than what we would model in our porous media. They are simply much faster, like water running down your car windshield. It doesn’t behave like water running into soil, unstable flow, rapid, some areas of rapid flow, some areas are very slow flow.

MURPHY: Thank you.

HORNBERGER: Doesn’t that beg the question as to why if there is water flowing in the fractures, they don’t deposit calcite?

TYLER: Yes. And, I’m not a geochemist. There clearly is a relationship--I didn’t mean to joke on that--there clearly is a relationship between fluid flow and chemical transport, mass transport. So, if you see filled fractures, that filling material came from somewhere, whether it came from matrix blocks or whether it came from, probably from the land surface, I would imagine.

CERLING: Yeah, just following up. Cerling, Board. Just following on this slide with the chloride and the nitrate profile, is there significantly more to be learned about recharge as opposed to ecology, by studying nitrate profiles along with the chloride profiles, or is this simply an ecology question?

TYLER: No, actually I think it’s--the relationship to hydrology comes in, number one, why aren’t the plants using this nitrogen, and there’s a reason, they’re probably not pumping out much in the way of leaf matter. They don’t need much nitrogen, the deeper rooted species. The shallow rooted species do. So, perhaps the nitrogen distributions can be used as an indication of which plants have been dominant for long periods of time on the landscape. Perhaps one could back out something about climate, frequencies of wet years and dry years. I’m speculating. But, yeah, I think there’s something to be learned here about particularly root water uptake, and that’s important with respect to the hydrology and balancing the evapotranspiration.

CERLING: You also made a probably a rather extensive plea for a monitoring program, and, so, if you could do this sort of thing, how would you use it in such a way that you could better evaluate the system before you do anything?

TYLER: Okay.

CERLING: Sort of a vision as a realistic use of a monitoring program, also provide information before you go ahead with whatever project it is in the mountain or otherwise.

TYLER: Well, my sense is if you're going to build a monitoring program to monitor the, say, waste that you're disposing of, you need to understand what the main mechanisms of transport are before you design that monitoring program. So, you have to have a good sense ahead of time through other experiments that you've been running to know how is the fluid flow going to behave, what are the characteristics, times and length scales. It may have been a somewhat expensive plea for a--or a plea for an expensive monitoring program, but personally, I mean, I'm a Navadan and if this site is to be built, I want it to work. I want it to, as someone from the public, I don't want to come back and have, as I've seen in other sites, ten years down the road at some site where oh, now we've found something, what do we do now. It's a priori knowing what to expect and what to do about it, is to me just a responsible monitoring program.

To get back, though, to your question about what other things can be learned, clearly, understanding, measuring fractures in the near surface to look at episodic infiltration events, that's where you can test the sensors that you're going to put around a repository that would be in a fractured environment. And, I know there's been some work in that, but I think we have to make sure those systems can function for long periods of time.

HORNBERGER: That raises another question. You mentioned episodic. In the near surface, of course, that makes total sense to me. I'm trying to picture how you can go through non-welded units and be a couple hundred meters down and still have this high variability, especially temporally. Doesn't that get filtered out?

TYLER: Well, I don't think we really know. My understanding of the observations of Chlorine-36 deep in the repository horizon, if those are correct, then that was somewhere less than a 50 year travel time. That's still fairly rapid, you know, that's perhaps not a meter a day, but several tens of centimeters per day if we average that out over the entire time period. And, I would doubt that it was a nice uniform fluid flow down through the fractures. So, I guess my answer to you is I don't know the answer to that. I know in tunnels and mine shafts and in other places where I have been underground, you do see significant variations in the fluid flow as a function of the seasons. So, there is fairly rapid transport. And, certainly in Karst environments, this is not a Karst environment, but you do see almost instantaneous response to precipitation events. So, I think there is some indication that it certainly is a possibility.

GARRICK: Is there a precedent for a monitoring system that has some of these characteristics, particularly with respect to the infiltration rates? Can you effectively monitor such small quantities of water in low time media?

TYLER: Off the top of my head, I can't think of an analogy for water. I could think of an analogy for contaminants, and that might be pesticide migration in macro porous soil or fractured soils, where we do see very rapid, in some cases, very rapid transport of a material which has shown very--which should have interacted with the solid matrix, and should have been absorbed and thereby degraded, not, so, therefore, moving quickly through porous media without much interaction with the solid phase. So, there would be my example of a contaminant. Water? No, it's very difficult. We're talking tiny amounts of water, I agree.

GARRICK: Yes, I think the problem here is that the contaminant of greatest interest is not going to happen for a long, long, long time, and you'd like to have some sort of precursor event that you're monitoring that you can correlate well with what you're really worried about. And, of course, monitor is a possibility.

TYLER: Just off the top of my head, then if you were going to build a monitoring program, perhaps you would put a tracer around your waste that would be easily transported if there was water moving around, and something that could be detected in very low concentrations.

GARRICK: Well, your comments about monitoring are very good and very welcomed.

TYLER: Thank you.

HORNBERGER: David? Anyone else? Thank you very much, Scott.

TYLER: Thank you.

ELZEFTAWY: Scott, by the way, this is Atef Elzeftawy and I'm speaking now on my behalf. When I came to--in 1980, I saw that graph and one of the master pieces was done by a female, I don't remember her name, and it struck me, and it was in Yucca Flat, and it struck me at the time why did we have that nitrate and that high concentration in that place. Now, when I put my spectrometer 50, 60 feet deep, 60 feet deep in the Sugar Bunker area, under the Sugar Bunker area, the spectrometer measured almost saturation point, water was about 15 percent. And, the question was, to me, where is the water coming from, given all that dry climate, 50 feet down the road, or under the surface, and why the nitrate is sitting over there. I'm not going to tell you what my opinion is, but that nitrate and the chloride sitting there is a very, very complicated process. It needs four to five parts of an equation to be solved simultaneously, and here it is, we are $12 billion in the hole, and we don’t have even understanding, a clear understanding of what’s going on with the unsaturated hydrogeology of that specific site.

HORNBERGER: You know, we do have to move on.

ELZEFTAWY: But, anyway, so I just wanted to show you that that’s not new.

HORNBERGER: Alan?

FLINT: Although I have quite a few slides, I’m not going to talk in detail about all of them, but I wanted to put them in for completeness so that we have something for the record if it needs to be discussed at a later time. So, if I talk too much, just every 30 seconds, we’ll just keep going and we’ll probably get through this.

I’m going to talk a little bit about the--this is the outline for the talk. We’re going to talk about the history and the timeline for developing the conceptual and numerical models. I’m going to talk about the development of the processes, observations, spatial distribution, the conceptual model itself, the numerical testing of processes and our submodels, our sort of bucket approach, and the distributed 1996 milestone report results, what happened between ’96 and ’99 to get us to the final product, the results and future climate, and some supporting data.

Scott mention soil thickness as an important issue, and dealing with infiltration, that’s how sort of I got working on this. If you recall, when Ike Winograd in 1981 said that unsaturated zones are a good place for nuclear waste because these thick unsaturated zones have no infiltration, I think Scott showed that very well, that you get no infiltration.

In ’83 when they’re talking about Yucca Mountain, Gene Roseboom said, well, 30 to 60 feet of soil over fractured rock is basically the same thing, however, because there was very little soil at Yucca Mountain, he postulated that the non-welded tuff would solve the problem.

So, we come along in ’86, or at least I did, and started on the project. We had our natural infiltration program underway, that was of the neutron boreholes, the artificial infiltration program and the matrix properties program. One thing missing here, there was no intent at that time, and it was not in the project for a numerical model of infiltration. That was not part of the process.

In 1987, I added the regional meteorology program as a study itself, because we were looking at climate, because climate was a very important program, and we wanted to look at how Yucca Mountain looked in relation to a larger scale.

In 1991, we started neutron borehole drilling again. We added new neutron holes. And one of the things that we started doing in this particular time was we started looking in different topographic positions. As I'll show later on in the talk, a lot of the thinking in the Eighties was about channels. That's where all the infiltration occurred, in channels, that's where everybody wanted to put all the instrumentation, in channels, that's where our deep boreholes were, and we started looking at some of this neutron data, realizing that channels were not the only place recharge was occurring.

In 1992, this was when we started integrating the infiltration work with the 3D site scale model, working with Bo and his group. We started putting this together realizing that we had to tie our results, which were all point measurements, into a larger numerical process. So, it started us thinking about numerical models, even though it wasn't on our chart at the time to do one for infiltration.

And, also in 1992, we had our first geostatistical estimate of precipitation. The estimates at the time were about 150 millimeters a year. We did a Co-Krig analysis on all the available data, and we have our first maps of precipitation, so we can see the variability on the mountain itself.

By 1993, we had made our first estimates of unsaturated flow in some of these deep boreholes, in the washes, and in side slopes, from thermal, from Darcy approaches, to the PTn, through tracer techniques, we were getting some numbers now at points.

And, by 1994, we had changed the program, the artificial infiltration wasn’t something we were looking at much anymore. We were going into surficial materials. This was when we started to characterize the soils in a lot more detail, and we added numerical modeling. We knew we had to have a numerical model. So, this was the point in time which we added that.

In 1994, we had a distributed flux map based on matrix properties only. This was surface exposed bedrock and hydraulic properties of the bedrock itself.

By ’95, we had our first distributed flux map of infiltration. This was on INFIL, version 1.0. By ’96, we had the milestone report that documented the infiltration, and this was anybody working on the project at this time, I think this was kind of a very exciting time for us because we were getting out I think they called it a map a week of infiltration, because we were trying new things.

And, by 1999, we had the analysis and modeling report which documented INFIL 2.0, which I’ll describe the difference between 1 and 2.

When we look at a water balance approach, Scott sort of brought this up, precipitation minus the ET, minus drainage, plus or minus the change in storage to zero. A lot of people don't like using this in the desert because if you look at it over time, ET is just a huge component, and precipitation is very small. But, if you get all of your rain in March in 1996 for a six year period, the ET is really low for March for that period, and you have 300 millimeters of rain infiltrating, you can actually make these calculations. So, they are applicable for short periods of time. So, we like looking at this approach to a certain degree.

Next? In this conventional wisdom was in the Eighties that the channels were the important place. We knew that these big alluvial terraces didn't matter, and channels were what was left. And, the neutron holes were concentrated there, deep boreholes concentrated there.

Next? So, as we're developing our conceptual model, we're starting to make some observations that get us to thinking a little differently about this. We're looking at water content profiles. This is in the soil and bedrock. We were looking at climate trends, a very important component. There's some subsurface flow of water in the bedrock interface. Differences between these geomorphic positions, the soil depth, the spatial distribution of material, bedrock.

One of the things that we had the advantage of is our offices were right there on the test site, and it didn't rain much, but when it rained, we were all in our pickup trucks and we were out to the field, and we were looking to see where water was moving, where could we see it. Could we hear it dripping in a borehole? And, we could in many cases. But, we looked in detail, and we would see runoff, we would run up the hill trying to find the source of it, what was causing it, why was it in this channel and not in that channel. The observations we made on the ground were a very important part of our understanding of this process.

Next? So, this is what a typical neutron borehole might look like. And, notice that we have a rain gauge on this one. We had a rain gauge on every single neutron hole so we could try to look at how much rain and how the neutron hole related to that. This one, just as an example of March 7^{th} versus March 13^{th}, and we see a change in water content in the borehole. This is in alluvial terrace, so we can see that we're getting infiltration down to about 6, 7 meters in this particular example, this is in a runoff event--or, this isn't a terrace, this is actually in a channel itself. So, this is an infiltration event. We can actually make some calculations from this information.

Next? And, what we're looking at here is the way we started to process the information. You see on the left is a graph of water content and time in a channel. We can see the infiltration pulse. We can see it moving down with time. You can find these kind of examples in a soil physics book. You look at a sideslope with fractured rock, this is the middle graph, the first maybe four meters is in fractured, low permeability bedrock, so we don't see much imbibition of the water. But, as we get down into the units below where we have more porous material, we start to pick up some of that and capture it in there, or a ridgetop, we can also look at graphs and we can see water moving down. So, we're starting to see water infiltration in sideslopes and ridgetops, realizing this is an important component.

Next? We looked at water content. That was a little hard for us to look at at the time, so we went to standard deviation of water content to see change. We were just looking for relative change. So, now we can easily pick out at about 3 meters, we don't see any change in water content, looking at maybe five or six years of data.

We looked at this using the standard deviation technique. We looked at channels and terraces with runoff, without, north facing, south facing slopes, ridgetops, and looking at neutron hole after neutron hole, trying to get an understanding of how this system was working.

Next? So, here's an example of a welded tuff, 10 percent porosity, or 20 percent porosity, or 30 percent, non-welded, and you can see the difference in penetration. The high porosity non-welded, you only get down about 5 meters. The moderately welded, you can get down maybe 6 or 7 meters, so we're starting to see the bedrock makes a difference, the bedrock properties make a difference. The soil thickness makes a difference. And, this is developing our understanding.

Next? Now, here's an example, this is one of the first kind of new pieces of information we gained. We started pairing up boreholes when we did the drilling. This is across the wash. The south facing slope, N-53. And this is the porosity saturation profile, the material type, in a channel in a north facing slope, and the north facing slope is a little bit drier, and that may be one of these areas where the matrix is drying out from the long climate change, but water is still going through the fractures and they're not in equilibrium.

But, N-55 is faulted. There are two faults cross-cutting in this particular area. And, when we looked at the, in this case, if we look at the Chloride-36, in N-55, you can see that Chloride-36 makes it all the way through the PTn into the top of the Topopah Spring. In the channel, as you would expect, the Chlorine makes it down a couple meters, and it stops. It doesn't get below that thick soil, not much happening.

N-53, shallow soil, it makes it down into the PTn, but stops because it's not faulted. There's not a way to get through there. So, this helps us develop our conceptual model, how things are working, and giving us an understanding of the infiltration process.

Next? Scott talked about this and had some good pictures. This is the drought in, I think, 1989 or 1990. And, if you look, there's a borehole right here.

Next slide? That's that same borehole, and this is the 1992 El Nino event. It really picks up the vegetation a lot, and we had to try to consider all of this, too. As Scott said, bare soil was an important part of our ET modeling and our measurements dealt with a lot of that, and this change in climate and change in vegetation was something we were concerned about, but weren't able to deal with as much as we wanted to.

Next? Now, this is our first really good picture of looking at Neutron hole data. This tells a tremendous amount of information. This is a time series of depth versus time, from 1984 to 1995, of all of the neutron data from this one particular borehole. And, if you look at it very carefully, you're going to see several things. And, I'll try to go up and actually point them. Hopefully, you can hear me on this. But, this is where the borehole is installed, and then each one of these is a winter period of a particular year, you can see the year, you can see the drought period. But, what you're looking at, which is real interesting, is you see the water content change, so it infiltrated, but as it comes down, it goes at an angle. What it's doing is it's moving down with time. So, this is the infiltration with time in the neutron hole.

Now, what's the most noticing and striking thing here is that none of these events go down very deep. The water is used up by the plants. It evapotranspires. And, this is how we were understanding how Yucca Mountain behaved. It was a desert out there, and this is how we saw everything. We didn't really mess around with this, because we didn't understand it. Until 1995, we had an infiltration event, we had two of them, and we hit this channel real hard with a lot of water, and it infiltrated, and it went down into the non-welded tuffs, very saturated conditions there. What we realize, in going back in our records, this borehole was installed a week after the 1984 major runoff event. This process happened in 1984, and again it happened in 1996.

So, what we're seeing is the dryout of this borehole over time as water eventually moves downward, or is removed by plants. This view was our first time. Now, I had 99 of these, and I spent a long, long time looking at every single one of these records in detail. This is how we came to understand the process.

Next? Another example over shallow soil. A welded tuff, fractured, but low matrix imbibition rates, low permeability, there's two infiltration events, it moved down into a more non-welded type tuff, more permeable, and we can pick up the water. We can see the water. We can see it move with time, and we can see it disappear.

Two more events, the same process, and you can see what's happening here. Water is moving down. It's below the root zone. It's going to become net infiltration. So, this, we can actually calculate a net infiltration rate with this number. We can do this, and we will, in the next slide.

So, these are those events, looking at changes in water content, and these are the fluxes that we get. 300 millimeters in one event. That's a lot of water in one event, but we had a lot of water. Now, if we looked at the drainage between events, just took the total water content in the profile, we see this change, and this is down I think 2 meters below bedrock to the bottom. We see this gradual decline, which is about 23 millimeters a year of water moving through the system. So, that's a calculation of the flux. That's one of the calculations we made.

Next? We did that for all the boreholes, and we can see the calculated flux versus the soil thickness, and we see this very nice trend. And, we do have some thick soils that have infiltration, but those are in channels. It's very clear here in looking at thickness versus--the mean with the soil thickness of flux. As the soil gets thicker, the water gets less and less. Soil thickness became an important part. So, we can start to characterize that.

Next? Now, Scott may not care about filled fractures, but if you're studying infiltration at Yucca Mountain, you have to care about filled fractures, because they're filled at the near surface. And, so, we did a lot of studies on the filled fractures. We also did a lot of studies on the unfilled fractures, and actually, I have, and I showed this at the 2004 NWTRB meeting, actually hydraulic conductivity, unsaturated conductivity of a fracture in the underground that we did, over a couple months of work.

But, the thing I wanted to point out here was we're looking at a soil on top of fractured bedrock, with fracture filling just about everywhere, and after a major rain event, we could see water, and all the soil was wet along the rim at the surface, it was wet, and underneath this mound, water was coming out, but the intermediate layer was dry, because it was channeled from the material around it, and not allowed to go underground. We got June Fabrika Martin out there, we did Chloride-36 bomb pulse measurements, and the further we went down the hillside, the more bomb pulse we found looking at this concentration, telling us about lateral subsurface flow. We instrumented some of the fractures. We could see water moving into the fractures, and we took them back to the lab and made measurements, so we have a better understanding of what was happening from this observation.

Next? One of the other sets of measurements we had were some of our heat dissipation probes. These are water potential sensors buried in the soil in the absence of a neutron hole, away from the neutron holes. And, we were lucky enough to put these instruments in two weeks before the first major giant rain storm, we had them installed. And, what we're looking at with different depths is water potential versus time, and what I want to point out is that as we get down toward the bottom of this profile at 36 centimeters or 73 centimeters, we're basically very, very dry--or very, very wet. The system is saturated here.

So, we put enough water in, the soil that Scott talked about can't hold it, it's at the bedrock interface. Now, it's going to infiltrate into the bedrock at the permeability of the bedrock. So, we picked certain points in time, looked at the total soil profile, water content, and at each of those points of time on the red line, that's the selected data, we calculated a change, and that's the purple spot. So, that's the 24 hour flux data.

The first one was 10 millimeters a day, and 8 millimeters a day, 6 millimeters a day, that's what was going into this fractured bedrock. The ET rates were about a millimeter a day, so, doing a mass balance calculation is not a particular problem in this case. But, we can see a way to calculate the flux, and then we get down here, these are typical ET rates for this time of year. Now, this event says that when we get wet at the interface, we get infiltration. How often does that happen?

Next slide. This is that record carried on from 1994 to about 2000. This is that event we were just looking at. If you don’t have a saturated condition at the fractured bedrock to get into the fractures, do you have infiltration? Well, we had one event here, maybe one here, but the rest of the time, those deeper soils are fairly dry. We get up to a balance and bars, air dryout in the desert is probably about a 1,000 to 2,000 bars, so these soils were air dry for a lot of the time during that period of time. And, if you make water potential measurements, I think you should be impressed with the range of this instrument to do this. And, we also put these underground in lots of places. So, we can look at the time. It only happens every now and then, but when it happens, it happens over a short period of time, a lot of water.

Next? We started looking at the distribution of geology and thinking about how this imbibition and infiltration would occur under these shallow soils, and we’re starting to think now about how to take this information onto the next step to tie it back to the model. So, we have our rock properties, we're going to make calculations and see what's in the rocks.

Next slide? This is the 3D site scale model that was originally developed in '92, and a lot of these cells were based on faults which were tied to the channels, where we were trying to look at infiltration rates, and some selected boreholes have them for the model calibration. This was our early version.

Next slide? This is our first map to fit that kind of model. This is a calculation of infiltration rates at Yucca Mountain, looking just at the bedrock itself. This does not account for fractures. It just says what's getting into the bedrock below where it's going to ET. And, now, we're starting to see our first numbers. Remember, Scott said it was .5 millimeters a year, no fracture flow. Now, we're looking at flow in the Paintbrush group of over 13 millimeters a year, just using Darcy law calculations. But, we're starting to get somewhere now, we're starting to get distribution of properties. And, we'll come back to later stuff.

Next one? This was a statistical analysis of all the neutron holes. We looked at soil thickness, rainfall, bedrock permeability, fractures, whatever, and came up with a statistical model, looked at each good cell, and applied it to that statistic, and we came up with our first spatially distributed map using statistics only, not a numerical model. And, it gives us sort of a perspective that the higher rainfall, thin soils, high permeability bedrock, has an influence on infiltration. So, now we're starting to see a picture develop here.

Let's go to the next. So, in terms of what controlled infiltration, and how we're getting towards our numerical model, and Scott put all this up, is precipitation, soil thickness, the porosity, and drainage characteristics. Bedrock permeability, once it hits the bedrock, what does it do? And, as Scott was saying, if it's real deep alluvium, it goes down to the root zone and pretty much stays there. It's done. And, then, evapotranspiration, that's what removes the water. Without that, we'd just keep going all the time.

Next slide? So, this is our conceptual model now that we're going to try to work with. We have all the processes we have talked about, our ridges, our sideslopes. But, one of the things to notice here is this green line. Water that gets below that green line is net infiltration to us. It's deeper and thicker soils, because plant roots can get down quite a bit in these. It is closer to the surface in shallow soils. But, you notice it is below the bedrock/soil interface. The bedrock at Yucca Mountain holds water, plants extract that water through microphysal associations, and it’s an important component to the water balance in the shallow fractured rocks.

Next slide? So, there’s some observations and refinements that we wanted to make. We’re starting to look at the spatial variability of storms. Snow melt becomes an issue, we were looking at that, we were getting more and more snow. North versus south slopes? Interesting thing about Yucca Mountain is that the south facing slopes are Mojave Desert plants. The north facing slopes are Great Basin Desert plants. This is a transition zone. Transitions have a big impact on this area, and it can go either way. But, that became an important part. And, then, deterministic rock properties, this is where we get the Sandia, where we try to look at--if you know where you are in a volcanic tuff, you know what the properties might be in the middle or the edges. And, we use that to distribute some of the properties with depth.

The rooting depths were exposed after flooding. In Thirty Mile Wash, we had a whole hill bank go away, and it cut into the alluvial fan. We went out there right after the event, and saw creosote plants down 6 meters, because water was getting down 6 meters. This is how we got our 6 meter number, because that’s the extent that we could find any plant material. Water potentials in fractured rock? Interesting thing. They had done a study where they had the soil taken away, and soil and plants and rock and plants, and they did water potential measurements on them after rainfall events, and the soil was saturated, and neither plant responded. They were rooted in the rocks, and they weren't going to take any water out of the soils. But, as the water got down to the rock system, both plants responded, water coming out of the rocks, so that was an observation we made.

Next? Our conceptual understanding? Arid conditions, net infiltration is an infrequent occurrence. Wet winters, we get enough water to get at the bedrock interface. Deep soils, it holds water and there is nowhere for it to go, except back up through transpiration. But, runoff does accumulate and infiltrate and overcome storage capacity in the root zone in channels.

Next? So, to get to numerical modeling, we wanted to apply the physics of the water-balance approach to these arid climates. We had to define the physical setting for each area. We had slope, aspect, elevation, soil properties, rock properties, vegetation. We went out and we did surface seismic. We did resistivity. We did emissivity. We did all sorts of measurements, trying to look at the spatial distribution of what was out there.

Next? So, our numerical modeling was to convert this conceptual model to a numerical or mathematical model. But, to do this, we had to do sub-models. We had a sub-model for precipitation, infiltration, ET, percolation, run-on and runoff, and we’re going to go through in just general terms about those, but I have a lot more information in the handout.

Next? Our precipitation model, we can use surrogate rainfall because we didn’t have a lot of long-term records at Yucca Mountain. We had 4JA, low elevation in Jackass Flats, about 10, 15 miles east of the site. That was our lower bound modern climate. Area 12 upon Rainier Mesa was our upper bound modern climate. We had those two data sets for long-term records. We could use those records directly, or we could use a stochastic simulator for longer term modeling, which we did. We used a third order, two-state Markov chain to determine the occurrence of daily precipitation, and that went to several orders, because if it rained on a given day, what was the likelihood it would rain the next day? It was higher, it was always higher if it rained one day. And, then, the third day, it was equal to maybe a little bit more, and then it went down.

We used the modified exponential cumulative-probability function for the magnitude of daily precipitation where we had four seasonal distributions. So, we conditions on the local rainfall data, and we scaled it to elevation for the site. So, this is our rainfall model, and again, like I say, we had co-located data at each neutron hole.

Next? The ET model, lots of details in here. It's the modified Priestly-Taylor, and we calibrated it to Yucca Mountain. We have detailed net radiation and ways to calculate the net radiation come from our solar radiation, the model important component. Ground heat was just calibrated with heat flux plates and measurements. Solar radiation was modeled using the SOLRAD model that is a very, very detailed, it accounts for all the atmospheric components. It accounts for slopes, aspects, blocking ridges, circumsolar radiation, that's the radiation diffuse that follows the sun around. If you get mountainous terrain, the sun gets a little behind the hill, you lose a lot of diffuse, not just direct beam, a very, very detailed model.

We also had a bone ratio station, and we had eddy correlation stations out there to measure ET, and those data were used, go ahead to the next slide, to calibrate to the neutron hole data.

So, now we're looking at rainfall measured, neutron hole water contents, ET is the function in these alluvial soils, so, we're doing a reasonably good job. We start the model off anywhere it wants to start, and eventually it catches up to the neutron hole data. And, I've been out measuring this. We just published a paper, DOE put it on its web site for measurements we've been making on Rainier Mesa for the last couple of years, looking at infiltration over the tunnels there. So, we've been still doing some of that. But, this is a reasonably good technique for matching the water content profiles.

Next? So, infiltration. Now, how did we calculate infiltration? Well, basically, we said all precipitation is modeled as infiltration. When the storage capacity is exceeded, that is, we can't hold anymore water, we're saturated, we generate runoff. That's the '96 version. So, we just simply said it infiltrated. It was mostly winter precipitation that was low enough below the saturated conductivity of the soil, so it was not a problem.

The precipitation run-on and snow melt infiltrated unless they exceeded the hydraulic conductivity or porosity. Then, we generated runoff. That's in the '99 version. So, we added a hortonian overland flow. That's where if you exceed the capacity of the soil to take it on, versus saturated overland flow, which is you exceed the storage capacity. We used two hour summer events. We knew how much it rained in a day, but we just said in the summer, it happened over two hours. Winter, it happened over twelve.

Runoff was counted and removed in the '96 version. And, then, it was routed downstream in the '99 version. We used kinematic wave, and we reinfiltrated downstream if there was a space for the water. And, we counted and removed, we ended up dealing with that later on, but this is how we did infiltration and runoff.

Next? Percolation, in this case, once the soil was in there, we let it drain to what we called field capacity, which was about .1 bars. Excess water was allowed to drain, go to the bedrock, and infiltrated the bedrock permeability. The rest was put into this bucket and it stayed there, and it would infiltrate, and that was our ’96 model. So, we basically redistributed the water in the profile.

We used a forward cascade in the ’99 model, where we filled the first layer up to field capacity. Then, it drained to the second layer, and on, on, down through the system. Then, we used a backward reverse cascade, which was just a mathematical technique to bring the water back up, if we put too much in there, until we get back to the top, and then we have runoff generation, if we put too much in the system. But, it was a way just to do the mathematics.

And, we did have a bedrock root zone, it was about 2 meters thick, to capture some infiltration. We measured it with the neutron hole data, and these plant observations gave us more information on that. So, that’s how we’re trying to deal with the percolation. We used a modified Jerry Gardner equation for the forward cascade to calculate it, sort of a Richard’s equation.

Next? So, we wanted to take our numerical models, we calibrated by matching observations and data, not just the data alone, but observations. We wanted saturations, and things, to happen. We want to run the model for range of geomorphic and topographic positions, soils, and climates to see how the system responds in areas we have no data and under climates we haven’t observed. That’s what we did. So cast the simulations. If we see something in the model that’s unique, then we’re going to go look in the field and see if we find things there. And, we wanted, most importantly test the model against independent data sets, something we never used, and a lot of that was easy to do because a lot of data came after we had done this first model.

Next? So, this is our calibration data, the 100 year simulations, the Maxey-Eakin model that Scott talked about. This is that curve in this location. Neutron hole data in these diamonds. And, then, our simulated data for all the different neutron holes, whether it be for the Area 12 high-end modern climate, or the Yucca Mountain simulated climate. Again, we get some pretty high values, and in general, pretty good agreement with the neutron hole data. So, there’s our calibration data set, and that’s what we made our maps on. Then, once we had our maps, now the job was to look at other data sets.

So, next? Oh, I should show you the map because it’s really pretty. This was our infiltration map, and the repository horizon--or, the repository outline on there, so we can see, and there were questions about, you know, what Scott talked about, about deep alluvium being in places, so if you had this repository over here where it was zero infiltration, then it wouldn't be an issue. But, that's one of the things we see when we look at these maps.

Next? Just a close-up of this, and some of the boreholes that we used for our calibration. And, we did some work underground in looking at whether we could see these infiltration rates underground. So, we did do some observations like that, and there are some things that very well correlated with this, which I showed at an earlier meeting.

Next? So, some of the corroborating data sets, Darcy flux calculations in the PTn, tritium, Carbon-14, thermal profiles, Chloride mass balance, other chemical techniques, now, we can start to apply these.

Next? This is our net infiltration model with some of the thermal fluxes that were done from some boreholes on the site. Some pretty good agreement, in general, not much difference in numbers. I mean, we're down here to the zero to 15 range. Remember, that infiltration at Yucca Mountain is temporally and spatially variable. There are going to be different techniques that are going to see different things.

Next? This is some of the chloride mass balance techniques that were used. We have a range of values from the model flux because when you're looking at a borehole, we don't want to just use the borehole, we want to use an area that might be 30 meters or 60 meters or 90 meters around the borehole, and look at what's happening in that, because we think as you get deep underground, that's going to have an influence. And, so, that's our range, and, we do a reasonably good job in many cases, a few cases were off. We had a little higher flux than the chloride mass balance does. But, this is sort of a corroborative data set that we used to test our model and see how we're doing. A lot of this was done after the model was done.

Next? This is an example from another paper that we did on percolation flux. This is the neutron hole flux calculations, the data we observed. This is the watershed model and these are the range of values, because some places, there's no infiltration. And, we can start looking at other techniques, whether we use Maxey-Eakin or whether we use chloride in the perched water body, we start to see some narrow range, depending on where you are, and again, larger time scale and spatial scale averages. I mean, chlorine data--chloride mass balance data is not from the last 30 years, it's from the last 1,300 years or more. So, it's a large time scale. So, my model here, or measurements from neutron holes, may not match it exactly, because we're looking at different time scales and space scales. But, this just shows some of the range of values of different techniques.

Next? Refinements in ’96 to ’99, we added the surface routing. We did multiple layers. We started using streamflow calibration data, and we added climate scenarios. You can see the water was moving down to about here, and then you could just stand there and watch the water running down to your feet, and never get past you. Very interesting.

Next? So, this is what Joe started working on when he put in these different layers. The bedrock layer is thick, under shallow soils. The bedrock layer gets thinner as the soils get thicker. And, finally, with very deep soils, we have no bedrock holding water, but it just shows some of our layering that we worked on, and how this model was set up to deal with some of the runoff in some of the rock layers.

Next? So, this is our modern day climate precipitation estimate for the site, and there are three areas here to look at. One is the extent of the modeling domain, the extent of the 3D site scale model, and the extent of the repository, because I’m going to talk about the results of the three of these in the final slides.

Next? This is the estimated infiltration. You can see infiltration now occurring in the channels, but a lot of

Yucca Mountain infiltration is still on the ridgetops and sideslopes.

Next? So, in the infiltration modeling domain, that's that whole large area, we have values of 188 millimeters, 3.6 net infiltration, because a lot of the area is deep alluvium. In the UZ flow and transport modeling domain, the LBL model, the infiltration rate is about 4.6, and more rainfall because it's got concentrated high elevation. In the 1999 potential repository, the design in '99, a smaller area, but about 4.7 millimeters a year, and a little bit more precipitation for this modern climate. And, that's higher than the average at Yucca Mountain, because, again, we're dealing with the highest part. So, that's what the results were, so, we're dealing with around 5 millimeters a year.

Next? So, this is the long-term future climate, more of the glacial types of climate, much higher precipitation rates along this location. Even the alluvial valleys are up to 280 or 300 millimeters.

Next? This is the infiltration, a lot more in the channels, still out in the thick alluvial valleys, still that's just not enough water because of the storage and the plants that are out there. But, we can see a little bit higher value over the repository.

Next? So, this is a summary for the future climate. Now, we're looking at the whole modeling domain, about 13 millimeters a year, getting up towards 18 for the UZ flow model, and almost 20 for the repository. So, this is our 20 millimeter, long-term climate estimate for the repository design.

Next? So, the results and what are presented here is in two milestone reports. One is the USGS milestone report. Even though it was only a milestone report, it's been cited in the literature a lot of times. It contains everything up to the '96 set. And, then, the 1999 report, which was approved in June of 2000 is the analysis and modeling report, and between here and here, we showed some of the things that happened in terms of how we made some changes. We started doing a lot more work underground. But, these are where most of the information lies that I've described.

Next? So, in summary, the field observations and measurements through wet and dry periods were really necessary. To be out there and see it when it's really dry, develop the conceptual thinking and then see it get really wet and go out there and watch that, really is important in understanding. If you had one group of scientists looking when it's dry, another group looking when it's wet, you're not going to get the same answer as one group looking at both times.

The conceptual model was converted to a numerical model and calibrated to the borehole and stream data, and the results are in general agreement we think we the thermal, the chloride mass balance, and other isotopic approaches. And, the single infiltration events may be 100 to 200 millimeters in a month, two weeks period of time, and there were, I think, six major events that occurred between those 15 years.

The primary controls on net infiltration were soil, water storage, bedrock permeability. Scott brought that up. And, it’s a grid-based deterministic model we used, it’s a good method to spatially distribute and calculate these kind of infiltration rates.

Next slide is I think the last, and that is.

HORNBERGER: Thank you, Alan. Let me ask the first one, okay?

I seem to recall that there was a difference between the 1996 and 1999 in how the runoff data were used to calibrate the model. Am I mistaken there?

FLINT: No. No, we didn’t use much runoff data in the ’96. Well, when I wrote the original model, what I did for runoff was look at the occurrence of runoff. All I did was try to see if I got runoff or not. I didn’t do anything with it. I just looked to see if I exceeded the storage capacity. I compared it to what data existed. I found two events that I said I had runoff that didn’t occur, and I went and asked our water people, the surface people, and Chuck Savard in particular, and he said no, there were two events that occurred on those days. They just never made it to the stream gauge. So, we felt real good about that, so we developed a model to look more at the neutron hole data, and we just didn't--I didn't have the runoff routing at the time, and it wasn't until Joe took over the model in '96 that he started developing the runoff capabilities and doing the 2D surface routing. And, then, once the routing was over, he'd go back to the 1D column, sort of the old '96 approach. But, he did have the routing, and that was one of the major differences, is he started using and calibrating two, not only the neutron hole data, but the stream gauging.

HORNBERGER: But, the recalibration in 1999, you still matched the neutron data, I mean, the neutron data were still front and center? It wasn't just the watershed?

FLINT: It was both. It was both, because we had to deal with, to match the neutron hole data, we had to change some things about the fracture properties and the fracture permeabilities, and we had to have certain bedrock permeabilities to get the runoff generated, and, so, we had to work and coordinate between those two.

HORNBERGER: Thure?

CERLING: Cerling, Board.

You've made a very good case for the climate in

Nevada being different than Virginia, and the importance of the long-term operation, which is sort of 15 years. And, what I was wondering is if you go to your future climate, which is presumably glacial, how that variability translates, or rather, how you account for the variability and what confidence you have in the variability of your recharge in the glacial, for which you really have not the same amount of data, presumably you have some analog data, but--

FLINT: Basically, what we did is we used analog data, and our climatology group and people within the USGS looked at records around the United States, and looked at lake levels and histories in and around the Yucca Mountain area and said, well, in the past glacial periods, the climate here looks like this other city, you know, Minnesota or Montana or some other place. The monsoonal system looked like Nogales, Mexico. And, they came up with what they would say were analog sites.

Then, we took those analog sites, temperature, rainfall records, and did our 100 year stochastic simulations to say how variable the climate was based on the analog. So, we did our same stochastic model by using other climates that the scientists and the USGS said these are the most likely things that--most likely way that Yucca Mountain would have looked 21,000 years ago. And, that’s how we did the variability.

CERLING: But, then also presumably, with the different vegetation?

FLINT: Different vegetation.

CERLING: Did you account for different vegetation, which would affect the ET kind of--

FLINT: Not so much different vegetation, because we don't know really how the vegetation would work at that time. So, I don't think we dealt as much with changing vegetation, but, the ET rates change because the air temperatures change. It gets a lot colder. The ET rates go down. So, those were accounted for in the ET model. But, the vegetation wasn't changed, and we didn't grow vegetation and kill off vegetation. That's something we thought of doing and that's something we were going to add to the model at one point, as we really start to change the climate and let the vegetation be a component that changed with time. But, that's not something that we got done.

MURPHY: Bill Murphy, Board.

Thank you very much. That was a fascinating talk, and I'm curious, you made a very strong case for the strong spatial and temporal heterogeneity of the infiltration process, and from data that have been collected deep in the mountain from the exploratory studies facility, or for deep boreholes, can you comment on the heterogeneity in those processes from, for instance, the water flow in the south ramp last winter, or the rehydration of the cross-drift, or are data--do the data from that also reflect this heterogeneity or to what extent is it modern?

FLINT: I think that's a real good question, and one of the reasons why I may go on about the spatial and temporal variability of recharge is this. A lot of colleague work on problems, and they would do a borehole analysis and they'd say, well, what do you get for Yucca Mountain? About 5 millimeters a year? Well, we get nothing over here. Well, that's because your borehole is over in my model where it says nothing. And, someone else says, well, it looks like it's like 20 millimeters where I am. I say, well, my model says yeah. And, so, we started to look at the spatial variability because we were trying to match borehole data.

As we got deeper and deeper underground, we realized two things. One, we have this integration of the signal over the site, and the other thing is that we're looking now at a time, a different time. If you were to look at Yucca Mountain 21,000 years ago, and look at where the infiltration was occurring, for example, here's infiltration rate at Yucca Mountain and over the repository, and here's what the channels are doing, look at time. As we get back in time, the channels become a much more significant part of flow.

So, if you're looking at things like calcite deposits, you might find them underneath the major channels because for most of historic time when the climate was much wetter than today. Those were the major source of recharge at Yucca Mountain. So, that’s where you would look to see historical evidence of flow. But, under current conditions, we don’t see that today.

So, when we start looking at things underground, and we did, if you look at my presentation from 2004, I show a graph of the infiltration model, and the water potential data that we collected in the cross-drift, and where the infiltration rates were really low, the water potentials were a lot drier. Where the infiltration rate was high, the water potentials were a lot wetter. So, we saw that correlation.

So, there are some things that we’re starting to make those observations underground. Now, if we, you know, seal up this, or close it off for a couple years and do a lot of measurements in there, we might start to see some of this stuff happen. We closed off a couple of areas. One, we thought was low infiltration, it stayed pretty dry when it was closed. One, we thought was a lot, it got real wet. So, we made those. But, there are other people who have been doing work on the geochemistry that can sort of address that, and are trying to address that now in looking at the spatial distribution.

But, it’s real important to remember that is exactly the case. It’s spatially distributed, so wherever you are, you might see something different, and what you are seeing may have occurred thousands of years ago, or as Scott said, you know, 50 years ago in some cases. In his case, he was saying, you know, some parts were that way, but I think that for the most part, most of the infiltration is dampened. Where you have faults, that’s where you’re going to see this pass through.

MURPHY: Thank you.

HORNBERGER: Dave?

DIODATO: Yes, Diodato, Staff.

Thank you for a really interesting presentation. I just for the record want to be clear about the difference between INFIL 2.0 and 1.0, and I think what I heard you saying is that 2.0 is when Joe Hevesi came on and did the hortonian overland routing algorithm, and then added in the description of the soil profile. Is that correct?

FLINT: That’s correct. Basically, when we finished INFIL 1, we started working underground, and I started putting my efforts into going underground and doing underground experiments and collecting data, trying to verify what we had done with the infiltration program, and doing more measurements. Joe took over, started added the routing. We had had a couple of good wet years, and, so, we had some runoff data, so he was adding that to the system. And, so, he more or less took over that, and added those component parts starting in ’96.

DIODATO: Thanks. I’m glad you brought up the underground work. You described, in ’99, neutron logging holes that were on site, but didn’t mention the work with the ring infiltrometers and the other experiments that have been done out there that you have been a part of. And, we look back at 1986, is your starting point, and then over a decade of effort, and I know that Lorrie Flint also did a considerable amount of field work, and then a lot of laboratory work also. What would be your estimate in terms of person years of effort that have gone into the understanding, just from your project here for the Yucca Mountain?

FLINT: Personal years, like--

DIODATO: Total person years of effort of the team?

FLINT: Of my team?

DIODATO: Yes.

FLINT: Well, in the ten years that I worked on this, I put in 20 years of personal effort. I think Lorrie put in 19 and a half, and--

DIODATO: Is this a 30 year effort or 40 year effort?

FLINT: Well, you know, at the peak of our effort in probably ’96, I had 25 people that worked for me at the time. Neutron loggers and the rain gauge monitoring people, we went out there, we could get all the rain gauges, 100 rain gauges done in six to eight hours, because we had to do that. And, so we did that, and we said one month, a neutron hole would get logged once a month. If it rained, they got logged sometimes two or three times a day, and every couple of days for a long period of time.

So, there were, you know, hundreds of man years probably in that effort, and a lot of it was, you know, technicians just sitting out there pushing the buttons on the neutron hole.

DIODATO: Thank you.

HORNBERGER: Alan, could I follow up on just a little more on that, you know, Dave mentioned ring infiltrometers. You hinted early on that you had actually done some, I guess, permeability measurements at open fractures, and filled fractures. Can you tell us a little bit about the actual data base, the measurements for hydraulic conductivities of the surficial materials?

FLINT: We did some ring infiltrometer, we did a lot of ring infiltrometer measurements. We went out with the double ring infiltrometers, and we would set up, for instance, a site where we would do a ten by ten matrix, so we would have 100 points, and we would do 100 measurements, so we could look at the spatial variability of infiltration with that technique. We'd go out around neutron holes, and we'd put in the four meter ring infiltrometers, and we had auto loggers set up so we could monitor all the boreholes, measure the infiltration rates. We had camps out there, so we stayed at night and did all these measurements for two or three weeks at a time, looking at the infiltration processes. We set up some measurements.

We brought a lot of fracture filling back into our lab and did work in the lab on our centrifuge work. We set up instruments on the ground, measured infiltration rates on a sloping surface until water started to flow in the subsurface, and then we'd turn the infiltration rate down until it quit flowing. This was our Alcove 1 experiment. So, we got our first measure of infiltration capacity of the bedrock.

We set up these monitoring states up higher on the mountain where we measured infiltration rates and water contents along the bedrock itself, and looked at the drainage characteristics. We did some of these measurements when we went out to the Ghost Dance Fault above Alcove 7, and we did a whole set of paired instruments on both sides of the fault under this rain event period that had been predicted for some time in '96, where we could actually see that the faulted side, the down-drop side, the fractured side had a much greater permeability than the other side of the rock in comparison by watching the draining.

So, this is how we started getting some of these higher permeability numbers for the fractured bedrock, rather than .3 millimeters, it became 3 or 30 millimeters. But, there’s a series of these kind of measurements and observations that we made, doing a lot of these infiltration measurements over some of the fractured bedrock. But, yeah, we did do quite a bit of things like that.

HORNBERGER: And, were all of these part of your, at least, the thought process in calibrating the--

FLINT: Yes. Yes. Now, one of the things that went on, as the bedrock permeability came up, people started saying, well, we’re going to have to have more infiltration. We have higher bedrock permeabilities. And, we said, well, we put bedrock permeabilities in and we did other things in the model based on uncertainties, but we still calibrated to neutron holes. So, if you want to make bedrock permeability higher, then you can do that, but you still have to match your field data. So, you’re going to have to change something else to match the field data if you’re trying to make the observations.

Again, we only tried to match runoff and neutron hole data, and get those in balance, even if we had more permeable bedrock. But, in the end, we ended up still hopefully matching the thermal and chloride data. But, yeah, we started making these observations, and this is the point where we were headed at the time we sort of ended all this field work, is trying to get out there and start making more measurements over some of these more important bedrock types and get some of the permeabilities.

HORNBERGER: Okay, we're going to take a break then, and I think we can come back, let's get a head start, and come back in 15 minutes.

(Whereupon, a brief recess was taken.)

HORNBERGER: I'd like to reconvene. We have just a very small change in the agenda, and that is that Bill Alley, who is going to go next, basically comments of an introductory nature, so rather than have discussion immediately following Bill's presentation, we're going to go directly to Dave Pollock's presentation, and we'll hold the questions until after that.

So, Bill, will you introduce us?

ALLEY: Thanks, George.

I'm going to begin with a few statements about USGS support to DOE, and our own model documentation activities to resolve some of the issues surrounding the INFIL model. Dave Pollock, who is in my office, has been the technical lead on this, so he's going to really provide the more detailed look.

But, let me begin. I want to reiterate a few comments that I made at the NWTRB meeting in May of 2006, I think, back in wonderful Virginia. And, first of all, I lived in Virginia for 23 years, so it’s really quite a nice place.

First of all, I want to say that, you know, reiterate that the discovery of the e-mails written by USGS scientists suggesting circumvention or misrepresentation of QA has been a traumatic experience for the USGS, and a very tumultuous time for us, and we’ve taken the matter seriously. And, we continue, and I described this at the May meeting, to make sure that we learn from the episode, and that we make sure that the technical products produced by USGS meet all the quality assurance requirements for nuclear regulatory needs.

The other thing I want to say is that being--the Yucca Mountain project branch within the USGS reports up through Ken Skipper, the branch chief, who is here today, to me. And, so, I have a very good knowledge of all the people that work on the project, and one of the unfortunate aspects of the whole affair is it’s really cast appall across the whole branch for a while. I think we’re getting past that.

I can tell you that there is always a natural tension between scientists and QA requirements. That will never go away. It exists. To say that it does not exist would be to tell a falsehood, really. But, I find that at the end of the day, and I always find this in discussions, actually, the individual discussions with people on the project, they say, well, at the end of the day, I will follow, as long as you made the QA requirements to me well known, I will comply with them, and that attitude is the primary attitude across the project today, and I think Gene Runkle will verify that later on with the extent of condition reviews that DOE has done.

The other thing is I always want to make sure that we characterize this very carefully, because it has been mischaracterized and continues to be, actually, I notice in certain venues that there was never any--data falsification was loosely thrown around, and there's never been any evidence, and certainly we've looked under every rock on this particular project, and there's been no evidence found of any kind of data falsification. But, there clearly was an attitude about QA that was clearly portrayed in the e-mails that everybody has seen.

So, with that as a preliminary remark, let me say our support of DOE to try to resolve these issues has consisted really of two elements. One is that we have, where there's been difficulties in either trying to reproduce things and have gone back and tried to essentially check everything to make sure that everything can be traced and verified, and so forth, and questions have arisen, there have been a number of questions that have arisen over the course of that, and so we have worked, either Dave Pollock has been assigned to a couple of those items, and then within our project branch, we have tried to resolve, and I think we have managed to resolve most, if not all, of those issues at this point. I’m not aware of any outstanding ones.

The other element that we’ve taken forth, as many of you know who are familiar with our modeling activities, know that we have a, to us, model documentation means a certain thing, and we think of it in terms of the kind of documentation that we do for our MODFLOW model series, CWAT, SUTRA, the models that probably a lot of you see, development of models and adopted in their documentation is the major part of our groundwater activities actually.

And, so, when we actually looked at this project very early on, aside from the quality assurance issues, there was another issue that we felt here was a model that was being used not only in the Yucca Mountain project, but also is being used in several locations, primarily in California, I believe, and that really it should be documented in the way we think of documentation, and our sense of documentation is more a scientific document, and one that provides in a concise manner, but thorough manner, a user’s manual for the model, so that somebody else can pick it up and use it. And, that did not really, did not exist. And, so, that’s been a lot of our focus.

And, of course, part of that effort involves, in this case, cleaning up the code, testing the code, and so forth so that we make sure we can stand behind it, that will equate, what the model says it does, is actually in fact what it does. And, so, that's going to be the primary focus of what Dave is going to talk about this morning.

POLLOCK: Thank you, Bill.

I think Bill gave a very good introduction into what I am going to talk about today, which is the involvement that Paul Barlow and I have had. Paul and I are both on the staff, Bill's staff in the Office of Groundwater, although I'm in Restin (phonetic), and Paul is headquartered in Massachusetts, although he spent much of last year in the office next to me in Restin. So, has recently gone back there.

Our involvement in this began a little over a year ago, but has really developed seriously in the last several months. And, it's been much more a mechanical involvement with the documentation of the INFIL model and the production of a package, as Bill said, sort of meets the standards that we think of in terms of model documentation in the USGS. And, one way to look at that is to think in terms of where you get software from the USGS, like MODFLOW. We basically distribute everything we have on a software web page, and what we would like to do with the INFIL model is basically put together a package that can be distributed on the web in the same way we would distribute MODFLOW or CWAT or SUTRA. And, that package would have not only the code, which hopefully would be cleaned up and tested, would also have a complete set of documentation, which would also be not only documentation of the theory and methods, but also a user's guide, and we tend to wrap those things up into a single package, if we can.

And, then, thirdly, it would have a complete set of sample problems that, in some cases, double as test problems. And, our objective is really, you can think of it is we like to have our models packaged so that when people obtain them, they essentially get a starter kit. You know, they get the model, they get something that is out there in a specific format, and they get enough information to jump-start them and get it going.

And, so, if you look at the tasks on the next slide, our objective is basically what I just stated, to produce that sort of package, and it really involves the four tasks that are listed there. The first one, restructuring of the FORTRAN code, is one just essentially code clean-up, is what's intended there.

Checking the computational algorithms, writing a model documentation and user's guide, and developing a set of sample problems. And, the names I've listed there, Paul's name and my name, sort of indicate where we've split the lead emphasis on those tasks, although each one of us is involved to some degree or another in each of those four tasks. So, what I'd like to do in the next few minutes is just talk about where we are in terms of these tasks, and there will really be, I'll lump the algorithm checking with the model, preparing and developing the model documentation. So, I'm really going to talk about three things. So, if we could go on to the next slide?

Restructuring of the FORTRAN code. I guess before I start there, what I need to do is say a little bit about where we're starting. I think Alan's talk was a very good introduction to what the history has been with INFIL, going back to 1995. So, if you go back to the earliest roots of INFIL, we're really talking about a model that's gone through several generations of development over a twelve year period. So, if you look at the various versions of INFIL, its core has remained relatively constant, with minor changes. But, a lot of the aspects of the INFIL model have sort of evolved over the years in these many versions. There have been a lot of additions, changes around this core.

And, if you look at the version of INFIL that's being used now in our projects in California, it's not INFIL 1, it's not INFIL 2, it's what Joe Hevesi has labeled INFIL 3, and actually, one of our tasks was, when we started this, was we were looking at this sort of product, and we had to sort of say okay, where are we going to start. Which version are we going to pick to sort of freeze that version, clean it up, and put it out there, recognizing that it's still continuing to change.

And, what we selected, with Joe's help after talking to him about what was involved in the different versions, we settled on a version of INFIL 3 that's labeled Version 5p, which won't mean anything to you, but it's a specific version of INFIL that dates from about November of 2005, and it's actually--the INFIL version that some people are using has actually changed since then. But, this version has enough of the sort of modern features in it that we all sort of agree that if we were going to pick one thing to document and work on, that the result of producing a package for this version would be a model that people would consider to use. It had enough things in it that it wasn't going to be considered outdated. So, we're starting with INFIL Version P.

The other thing I want to say, which isn't listed on this slide, is to emphasize that in order to make our work manageable, we're really focusing on the FORTRAN code that is INFIL. But, when you look at the work that's involved doing a "INFIL" simulation, for instance, at Yucca Mountain, or elsewhere, you realize that INFIL is really a package of many, many pre-processing codes, the INFIL model, some post-processing codes, maybe eight or nine codes between them, of which the INFIL model is one that all go into producing an INFIL simulation. And, those pre-processing codes have undergone significant changes over the years. They've moved from primarily FORTRAN codes to pre-process the DEM and other types of data, to much more GIS based codes, like ARCHYDRA now that are being used. So, the pre-processing has changed a lot.

What we decided to focus on was to do like we do with most of our other models, for instance, MODFLOW. We were going to focus on INFIL and simply document the INFIL model, very clearly document the input and output requirements, and say, you know, you need to have this data. How you get it is up to you. That way, we sort of detach ourselves from pre-processing steps, which tend to change much more rapidly than the core codes themselves.

So, with that said, the focus that we have made on the INFIL model in terms of cleaning it up is largely revolved around improving the modularity of the code with respect to its data input. The core routine, and sub-routines that actually do the computations, the hydrologic computations, the potential evaporation routine, the cascading bucket routines, the stream flow, re-routing and run-on routines, those were already fairly modular, and we have tried not to touch those, except to, you know, clean up things that didn’t need to be there anymore. We don’t want to change INFIL. We don’t want it to become our model. We simply want it to be cleaned up to the point where it’s easier to follow.

So, most of our “restructuring” has been involved on the read and prepare the input data side of the INFIL, where if you look at the INFIL 2 or 3 that exists now out there, you have essentially one big main program that might, I think it’s almost 2,000 lines, and probably 1,200 or 1,300 of those lines are continuous sequence of input data preparation before you ever get to the calculation. There’s no modularity to it at all. It’s just sort of one right after the other, and it’s all sort of in one big main program, very difficult to follow.

So, what we’ve put a lot of emphasis on earlier initially in our work was to take that, using the MODFLOW model to try to recognize modular components of the input data, input and preparation, that we could break it up into, and take chunks as intact as possible, but to move them into sub-routines so that when we had the main program, could be cast in a way that was much more readable and much more modular.

The other thing we did that was significant in terms of code restructuring has been to take a lot of the data that was passed between these computational routines, in some cases, as many as 150, 160 arguments in a sub-routine, it was almost impossible for me, when I first looked at it, to sort it out. We moved that to a FORTRAN, essentially a common FORTRAN module for those common data, and then reused that data. So, that was a more mechanical thing that didn't affect the computational elements of the code, but it made it much easier to look at, and it also made it much--is going to make it much easier to manage in the future, because, for instance, if you need to re-dimension things now, to go from size to another, now you do it in one place instead of a half a dozen or more places. So, very mechanical but very necessary.

We did a lot of work removing dead-end code and unused variables. I would categorize INFIL as a very, very classic research code, not a production code like MODFLOW. It's a code that was written by a professional hydrologist to answer a problem that they had, not written by professional software engineers. You know how this goes. If you develop codes of your own, you do what you need to do to get the job done. And, so, if you look at a code like that that's evolved over ten or twelve years, after twelve years, what you have are things that are, it's like all your clothes have not been picked up, things are nice, but you've got a lot of things that used to be necessary, they're not necessary anymore, but instead of cleanly taking them out, you loop around them and you do different things.

And, so, there were a lot of these things in there that we had to struggle with, and it didn't affect the computational aspect of the code, but it affected--and, it would affect anyone who wanted to go into the code and try to figure it out, because when you first go in, you don't know what things are doing, and you have to fight your way through it. So, one of the things we've done, and by cleaning a lot of that up, removing the dead-end code, it's amazing how much easier it is to look at and follow what's going on.

In addition, and I'll skip down to the last one there, we simplified and standardized a lot of the input and output file formats. What we found when we went in and looked at INFIL was that there were a half a dozen file formats to read in a certain type of data, you know, and that's fine, but then we discovered well, there's only one of them that's really used now. And, then, it turns out, as you might expect, well, we added this one, you know, five years ago because in one application, we got data from this source, and it had these extra columns in it, for whatever reason. To us, that's a job for a pre-processing step. If somebody has data in another form, we want to tell them this is the way we want to see it. You do whatever work you have to do to put it in that format.

So, we carefully went through and have highly simplified the file input and output structure, and that's also helped.

So, that's something that I basically have been taking the lead in, with Paul helping out on that, and we're pretty much where we want to be on that.

The second major task is the model documentation and algorithm checking. I've combined those because we made a decision early on when we first got involved in this that neither Paul or I were experienced with INFIL, or really even with a lot of the unsaturated zone aspects that it was built on, and it was clear very early that we couldn't just jump into the testing right away. We needed to basically educate ourselves on what was going on with this model before we could develop a level of competency to really start testing it.

And, so, we decided to do that by working through each of these sub-models, as Alan described, working and used the documentation development, right the documentation as a product of our essentially study. And, what we--and, we've essentially completed that for the theory section. We've produced it's about a 40 page document, and it pulls together a lot of the theory that's published elsewhere on INFIL, but there were a lot of holes in that theory from the point of view of a user's guide to INFIL. It would be hard to go from any of the things we saw published to someone sitting down trying to figure out how to prepare the control file for the INFIL model. A lot of the connections were not made.

So, what we've tried to do is to fill in those holes, and bring the theory into the model documentation, but very tightly tie it to the variables in the code, saying this is what's being done here. These variables in the control file relate to these aspects of the ET theory, or the precipitation sub-model, or whatever.

And, so, we've finished that. We sent a copy of that to Joe Hevesi last week, essentially asking him did we get this right. We had some questions highlighted. We're not sure we completely understand what's going on here. Let us know. So, he's working through that. I think we're about 75 percent done overall. We still have a lot of work to do on the sort of mechanical input and output sides of the program. We're continuing that.

So, the final thing to mention is our testing, and I alluded to the fact that we're really just now starting the testing. You might sort of expect that if we're working on this for almost a year, or whatever, why we're not further into the testing, and the answer is what I just indicated before. We really felt like we needed to go through our education process before we knew how to do a good job with the testing. And, so, we're at that point now, so we're just now starting in the early stages of testing. Our testing is going to involve a couple different types of tests for the--you might be familiar with how the testing was done for INFIL 2. There were approximately somewhere between 30 and 40 test runs that were done during the evaluation of the INFIL 2 code, and those were pretty idealized, very, very highly simplistic tests to just test the sub-elements of the code.

Some of those I think are worth repeating. I've looked at the 40. I'm not convinced that the best use of our time would be to reproduce the exact 40 runs that were done for INFIL 2. I think there's probably 10 to 15 that we'll pick out, maybe modify somewhat to test the basic elements. But, most of our testing, we would like to center on a realistic model, or at least a test problem that we're developing from a real model in the Big Bear Lake watershed study that's going on now, or finishing up now, in the California Water Science Center.

We're taking one basin from that, which has two feeder basins, and it's a very nice sized problem that we can manage as a sample problem, but it's also got a lot of--it is a real problem, so it's got a lot of nice variability in it. So, it will work well for a sample problem, but it will also work well as a test bed for us. What we plan to do, if you look at the INFIL model, it can be very daunting when you look at the sort of maps that Alan was showing that are produced where you've got 100,000 or more grid cells that are producing, all doing these calculations, it generates a huge amount of output. How you sort through all of that, because it's--each of these sub-models is extremely complex, and how do you sort all of that out?

Well, one of the advantages of the INFIL model over something like MODFLOW or sort of the classic flow model or transport model is that even though it's spatially large in extent and complex, there's really, each aerial cell is pretty much independent in INFIL. The only thing that really links them is the flow routing, which can be turned on or off for testing. If you take the flow routing out, basically you've got 100,000 aerial calculations that are all sort of going on independently. So, what that allows you to do is pick out sort of selected cells to spy on, and if you just look at those cells, you can really begin to look at those and break them down into detail, look at what's going on with the ET, with all of the sub-model calculations, look at them in detail for that cell at a level you'd never be able to do if you had to somehow consider the aggregate.

And, by doing a model like the Big Bear, where we've got a huge range of--if we go to different parts of the Big Bear watershed, we can get very different environments in terms of snow fall, precipitation, different aspects. So, we can pick different parts of the Big Bear model that will exercise different parts of the INFIL model. So, that's the strategy we're going to use for our testing, and we're still in the design of that.

Neither Paul nor I want to spend the rest of our careers on the INFIL model, so we're all, each of us is looking forward to finishing this up, and we're beginning to see the daylight at the end of the tunnel. We expect to have the report, model documentation report, internal USGS review, probably within a month, say by the middle of April. The testing will go on simultaneously with that review, and, of course, you never can predict what you're going to run into with the testing. So, we are committed to basically taking as long as it takes to put out something that we're comfortable with. And, if everything goes right with the testing, we're hoping that a couple months will be enough time to do that. But, it could be longer. We just won't know. We're just really going to start hitting that hard in the next week or two.

So, that's I think a summary of where we are and what my involvement and what Paul's involvement is. And, so, we'd be happy to take any questions.

GARRICK: Dave, you mentioned something to the effect that this was a research code, or a code for research as opposed to a production code. Could you comment on what that means in terms of using it on the project in question?

POLLOCK: I didn't have that written down here, but I did use that word. That’s the way I think of it, and I can tell you what my definition of that would be. And, that is, and probably the best way is to use an example of something that’s a production code, the classic production code for the USGS, which is MODFLOW. And, we would define production code as being one that is cleaned up and processed and documented in such a way that a new user could come in and pick that up and learn enough about how it works and how to use it to start applying it in their own work.

Whereas, to me, a research code is one that is produced by a few individuals basically for their own self-use, and, so, they don’t tend to be documented as well for input, instructions, and the other, because they’re really not looking ahead to other people using it. They are the ones that use it. They know the ins and outs, and, so, it’s the quality of the code is the same, except maybe you don’t clean your house as well by deleting unused stuff. You comment it out instead of taking it out, and you don’t document in a written documentation the input instructions, for example, as clearly as you would in production code. That’s what I meant.

To me, production versus research code doesn’t have any connotations in terms of the quality of the computations that are being done. It’s more a presentation issue.

GARRICK: Just a follow up question on that. In terms of what you're doing to make INTEL 3, or 4, or whatever it is--

POLLOCK: INFIL. We're not modifying INTEL.

GARRICK: Yes, INFIL. Have any of the activities that have been going on with respect to the Yucca Mountain analysis influenced what you're doing? And, if so, how?

POLLOCK: Not really.

GARRICK: Are the reviews that have come about subsequently, such as the work on MASSIF and the reviews that have been made at Idaho, et cetera, in the past?

POLLOCK: Well, I haven't focused personally on those too much yet, although we need to start doing that. For one thing, we're working with a different version of INFIL. It's basically the same in the core routines, but there's some significant differences. So, it's a little bit hard for me to compare INFIL 2 and INFIL 3. I'm not as familiar with INFIL 2. That was the code that was used at Yucca Mountain. We've gone straight to INFIL 3 because we're looking ahead to documenting something that was currently in use, and probably would be more the thing that would be likely to be used in the future.

So, I think your question is good, and I do intend to try to fold those things into our work. And, in one sense, the timing is good for us because we're just now starting our testing. Other people have looked at it already. So, we can take things that are in findings from those reports, and use those to help us focus the things that we look at in our testing and make sure we don't miss something.

HORNBERGER: So, you described how you view a documented production code. Do you have any insights to offer on what the differences would be in terms of qualifying on code under a regulatory, NRC regulatory issues, such as DOE has?

POLLOCK: No. I guess I've never had to operate in that environment, and I'm not really familiar with all of the requirements there, so I think I could only offer a really uneducated opinion on that. So, I guess I really don't know. I just haven't had any experience.

ALLEY: One of our intents here was to find a couple of divisions who knew nothing about Yucca Mountain to take a look at the code, so they were completely independent, actually, of all those activities.

POLLOCK: I don't know if that's a compliment or not.

ALLEY: But, had knowledge about model documentation and the general types of things that it was simulating. So, Dave and Paul were starting off fairly--very cold, actually, here. But, the trade-off was do we have somebody that's not cold that's actually attached to the project, or do we go with an independent view. And, so, our purpose was to go through an independent review and to approach it that way. And, actually, there was a benefit to that because they had to go line by line and figure out exactly what was going on, and try to translate that back to the documentation. So, Dave would have no idea of what's involved in the QA.

HORNBERGER: I actually anticipated that, but earlier, he said he was going to defer all the tough questions to you. I assumed that it would be you answering it, though.

I guess what I'm trying to understand a little bit is Dave described it as a research code, I mean, some people say a spaghetti code because we all know how that goes, how does one qualify code in that situation? So, up through 1999, the QA procedures were being followed. How does that go for a code like that?

ALLEY: Of course, I've never done this, so I'm not too familiar, but my understanding is is that essentially along each step of the way, you must approach this in a step-wise manner, and each step, you describe very carefully what it is you did, and you describe the problems you're using to test, and then--that's my understanding of the process. The QA process is actually largely very much a traceability process, so that you can understand exactly how this code is developed. That process does not take place typically in the kind of code documentation that we have.

The process there is more you might go along, you might develop a code for a fair bid, or you might do a lot of activities, and then at the end, you basically rely on testing against sample problems. Ideally, there's an analytical solution. Unfortunately, in this case, it's not so easy to compare these kinds of models to an analytical solution. So, there's actually a different approach to the problem.

Now, as I understand it, this model is not going to be used in a regulatory environment. But, obviously, if it was to be used as part of the Yucca Mountain project, it would have to go through that other type of process, in addition to whatever we were doing here.

POLLOCK: And, I think one of the advantages that would occur in that case of what we're going to produce, is at least when our package is provided and our software archived, at least you'd have a starting point that you could at least be sure what your starting point was if you had to do that.

FLINT: Both INFIL 1 and INFIL 2 have been fully documented through the DOE QA system. I did INFIL 1 myself, and it goes through a lot like what Bill said, is we document how we did the code, we document test cases, we do it against known solutions, and we can do analytical solutions, but both INFIL 1 and INFIL 2 were fully QA'd under the DOE system. And, it seems a very similar process, but more, as Bill said, it's traceability. But, both of those were done, and the two documents I have on that last slight are the codes themselves, but there are QA manuals that have test cases, how to set the infiltration model up, how to set your input deck up, what the output files are like, and those are QA documents.

HORNBERGER: You're just confirming in my mind then one is more traceability and one Dave has described to us is more transparency, so that people can actually read the code.

FLINT: Right. His is I think more with the actual in-working of the code, the technical parts of the code, how it works, what's in there, where the equations come from, whereas, our DOE side was the traceability, where do your input files come from, where did that data come from, how does the code calculate it, does it do it correctly. There's a list of things that you answer from a QA world.

POLLOCK: Yes, we did spend a lot of time in preparing the report that we're talking about here in trying to talk to not only Alan, but Joe, about what are the original sources for some of these things. We tried to pull as much of that in as we could to fill some of these holes. There weren't holes in their work, but it's just by virtue of doing the documentation, we felt that previous writings, almost everything was said somewhere, but not always in one place. So, that's one of the things we tried to do, was to write the report that we would want to read if we were coming at it cold, and had to, you know, start using this model in the next week.

HORNBERGER: One other question that I have for you, since you’ve been dealing with these technical issues for months and months. Scott told us at the beginning here how tough it is for arid regions, and how Nevada really is different from Virginia. I hadn’t noticed before. But, I’m curious, is it your view that INFIL is just generally appropriate, whether it be for Bear Lake, or the Mojave Desert, or Virginia, or do you think that it has some special appeal, in particular, for arid zone hydrology?

POLLOCK: You know, I would love to answer that question, but I just--it’s so far from my hydrologic area of expertise that I’m not sure. My sense has always been that it’s fairly widely applicable, but probably leaning more towards the regions, the drier regions, where it’s been applied sort of, but, you know, I don’t know. If I sound like I’m sort of dancing around, it’s because I really don’t feel very qualified to offer an opinion up here.

FLINT: I could address that question. The original INFIL 1 was written because none of the arid land--or the humid land codes worked, because there was no runoff to calibrate to. So, INFIL 1 was written for that. As we got a couple of good El Nino years, and we saw the runoff becoming a more significant portion, then INFIL 2, which added the stream routing, became a more universal code. And, it was more applicable outside of the arid southwest. But, it was mostly for arid, and semi-arid areas. INFIL 3, and where the code is today, where we're looking at Big Bear Lake, which is up in the San Bernardino Mountains, is really that more general code that works with snow and rainfall and runoff and continuous stream flow, and things like that, still a little more toward the semi-arid dry sub-humid types, not as much the humid. It wouldn't work as well in Virginia, but it would still work. But, it's more designed for the Western United States kind of climatology. But, it's developed and it's progressed through time toward the wetter climates.

HORNBERGER: Dave?

DIODATO: Diodato, Staff.

Thanks for the talks. I just wanted to be clear about the difference between INFIL 2 and INFIL 3. You said that there were significant differences in the core routines. Can you name one significant difference?

POLLOCK: Actually, I don't think I said there were significant differences in the core routine. I think a lot of the core routines are the same, and Alan, you might want to help me on this, but I actually asked Joe that question directly because we were working with INFIL 3, so I wanted to know, you know, I haven't worked as much with INFIL 2, what are the differences? As I understand it, probably the major difference going to INFIL 3 was the ability to--essentially changes that helped it work better at larger regional scales, with the interpolation of the climate data, and that sort of thing. But, I think there also have been changes to the using a continuous drainage curve now, as opposed to the type of mechanism that you described.

Do you want to say something about that?

FLINT: No, that's pretty much it. I think the core processes are pretty much the same.

POLLOCK: Yes, if I said that they weren't, I misspoke. The core processes are basically the same, except for these two things that I just mentioned. But, those are significant.

FLINT: I mean, there were some simple little things in how the drainage function might change a little bit. But, it's still reproducing the old system.

POLLOCK: And, it still uses a cascading bucket approach, and that sort of thing.

DIODATO: Great, thanks. In your examination of the code, have you identified any errors that significantly change the output, or would affect materially the result of the calculation?

POLLOCK: We found a few minor things, but the things that we found really haven't, to the extent that we've gone back and actually, when we fixed them, sort of checked, they don't seem to have been things that have really had major effects, which is probably why they were still in there. You know, the way things work is if you have a model and it's ten years old, and you find a problem in the eleventh year, it's usually because it's there in a way that hasn't affected anything significantly that's been done so far. And, that's where we are.

But, having said that, I've done this sort of thing too many times to say that there isn't anything in there, I mean, and I think this next phase of checking, the testing that we're going to do, we're prepared to run into--find some things, you know, if we test something that hasn't been exercised before, we may find something, and we'll deal with it. But, so far, we've been fairly pleased that when we finally got to the point where we could work through all of the, you know, sources and explanations of the algorithms, and work through the code, we were basically able to follow it, especially after we cleaned up some of the things.

DIODATO: Thanks. I guess the one follow on question would be for Bill Alley, and that's the question of the person years of effort that have gone into this response on the code side, would you have an estimate on this effort that Dave Pollock is describing?

ALLEY: They're all doing other things, too.

DIODATO: So, total effort is maybe one person year?

ALLEY: That would be a fair estimate.

DIODATO: All right, thanks.

ALLEY: The other thing I would say, to answer your previous question, and correct me if I'm wrong here, Dave, but I guess--and I think Dave said this, one would never describe this as a pretty code. But, it's also, when I look at the time frame in which it was developed, you know, you would not get a pretty code, when I look at that time frame, you would not have--and I think that's really part of what's meant here. So, you're not prettying it up to make it so that anybody who's done computer programming knows what it takes to go, if you get something to work, and for your specific problem, but I think to make it so that it's actually very clean.

POLLOCK: I might add, and I think I tried to say this before, but I might not have been that clear, and I probably shouldn't have used that term "code restructuring." I tried to emphasize, when I talked about it, that was mostly on the input side. We tried, as much as possible, not to tinker with the core routines, except to clean up stuff in there that we clearly recognize that we could clean up. We didn't want to start messing with them too much. We didn't want to turn INFIL into something else. And, so, what we're going to end up with, people will look at and say, well, that still looks sort of, you know, why did you do that. Well, it's because our project is going to be sort of an intermediate compromise that’s cleaned up, but still recognizable by the original developers.

MURPHY: Given that there’s been such a long history and an immense amount of work on Yucca Mountain using the predecessors, have you considered, or have you excluded using Yucca Mountain as a test case?

POLLOCK: As a test case, I think we have, primarily because the Big Bear test case that was already developed in INFIL 3, so we had a nice complex data set that we could sort get jump started with there, as opposed to going back and redoing Yucca Mountain. But, we have talked about at the very end of this when we’re done, when we’ve essentially produced our thing, we do feel like we have an obligation to go back to Yucca Mountain for at least the test base, and sort of do a proof of concept that if you wanted to apply this modified or this new documented version of INFIL to Yucca Mountain, here is how the data sets would have to be changed in order to put them in our format.

So, that’s what we’ve talked about, but not using it as a test problem. I mean, we made a conscious effort in our part of it here to really sort of be Yucca Mountain neutral, you know. I mean, we didn’t want to have our test problems focused on Yucca Mountain, because really our work on INFIL, this part of it, really didn’t involve Yucca Mountain. So, we wanted our test problems to be separate, too. But, we have talked about going back at the very end and making that--closing that loop.

HORNBERGER: One last thing, and it’s probably not a very interesting question. But, you described INFIL inappropriately as being a, by and large, a whole host of independent individual vertical columns, if you like, which would be a natural candidate for parallel processing.

POLLOCK: Right.

HORNBERGER: And, computers are probably so fast you don’t care about that.

POLLOCK: Well, actually, when Dave visited us a few months ago, he brought up the same point. It is glaringly obvious that it would be a good candidate. But, the answer I gave him, and I’ll give you, is that that would require a structural change to the code that would sort of break out rule of--I mean, it needs to be done, maybe, in a future version, but it was beyond the scope of what we wanted to address.

FLINT: I do want to address that, because INFIL 1 has been parallelized now because of that very problem. And, it’s running actually right now on my home computer.

POLLOCK: And, INFIL 1 is probably more suited to that because it’s totally--

FLINT: Yes, you are absolutely right, and I think in INFIL 4, we’re probably going to move to parallelization of it, because we have built a small Bay Wolf (phonetic) cluster in our building that we're using to speed it up, because when you get larger and larger areas, it takes a long time, and this has really enhanced the capability of the code.

POLLOCK: Thanks, I didn't know that you had that in the works.

FLINT: I just gave a talk on it yesterday. That's how I remember.

POLLOCK: Well, I wasn't there.

HORNBERGER: We have one quick question?

RUNKLE: Gene Runkle with the Department of Energy. I'd just like to clarify and make clear the relationship of INFIL 3 to the rest of the processes that we're talking about here. About a year ago, in discussions with Bill Alley and Ken Skipper, we talked about the fact that we could not reproduce all the climate maps, the nine infiltration climate maps that had been done as part of the initial work. And, we were having trouble particularly with one map, and, so, we asked Bill about bringing this back to USGS and seeing if they could help us and to clarify that particular event.

Bill took the initiative and this work that is currently described here has been at the cost of USGS, not funded by the Department of Energy. They went back and indicated that they would go back and look at the whole infiltration model, look at the documentation associated with it, and bring it up to full traceability and transparency that you've heard described here today. And, so, that work is being done by the USGS. The way we see this from the Department's perspective is it would come back, sit on our shelf the same as it would sit on their shelf, and be available to the public for whatever purposes. There is no relationship to this work and the license application.

HORNBERGER: Dave?

DIODATO: Gene, I appreciate your remarks. I just wanted to follow up. This is not a question for you, but just to emphasize Bill Alley's earlier. You're looking at the code here, but the question of the data, the underlying input data that go into the processing of the code, has there ever been any reason to think that there would be anything wrong with the data that have been collected thus far in terms of the USGS standing behind it or not standing behind it?

ALLEY: Gene will talk about that. I mean, they've undergone extensive validation.

DIODATO: All right. So, we'll get to that this afternoon.

HORNBERGER: Okay. Thank you very much, Dave. And, thank you, Bill, for your introduction. We have some time for public comment, and in particular, I know Atef Elzeftawy wanted to make a comment because he won't be able to stay this afternoon. Atef?

ALZEFTAWI: If John had introduced me here, he would practice my name very. I’m just kidding you. I lived in Virginia, in Sterling, and for three years, and I worked more than 60, 70 hours a day for--I mean, every week, working for the NRC, and everybody wanted to get the job done yesterday to go down to tell the President about whatever Yucca Mountain is. And, so, it was a good time. But, I didn’t like the Virginia salute. You know what I’m talking about. So, I came back to Las Vegas.

On more formal notes, I’m here, I just want to say a couple things on behalf of the Las Vegas Paiute Tribe, not too many, but I think when Scott was ten years old, and Alan Flint, when they were ten years old, I think, I learned that science and politics do mix. And, then, Yucca Mountain program, or the nuclear waste issue came along. We think, in NRC, we think Hanford because they wanted to have it 3,000 feet below the water table, a repository. I said do you have a submarine that can go down there and stay there? The answer is no, 3,000 feet. So, that was a simple question, and the answer was swift, as a model, got rid of that site.

And, then, we came to Yucca Mountain, and the question was, like John said, the Chairman, is infiltration, the toss back, and the performance assessment, and so on. Well, whatever you make it as a scientist comes down to one single point, the infiltration and how much water is going through. Everything else is going to depend on that. The DOE, as you said, back then in 1993, they said 1 millimeter. I said 1 millimeter? That makes the result look pretty good. I said you need to get it. And, then, as you see, from 1 to 5 to 6 to 10, and so on.

The problem of the fracture flow, or the porous fracture flow, Sandia published, I read the report last night, 1983, a report about the fracture flow versus the matrix flow, and they did, and they just added the two equations together, and it looks like you add one plus 100, and you get the average. You can’t do that. There’s a problem here, and I think that’s the problem we have with the modeling.

And, to answer John’s question, I think the models are fine. I was a modeler by myself, I still can do a lot of modeling. Computers are great. You can do a model of things. I can model George, but I may not be able to model all his DNA, the 30 billion nucleotides. If I go 1 nucleotide haywire, he might have a sickle cell anemia, or whatever it is, one.

So, what I’m saying here to the Board, not to the public, not to the DOE, you guys are going to have to really be a good support to your leader. I feel for John, because he’s going to be sitting there. In five minutes, he’s going to try to tell something to the Congress, and they can assure him for every second, they're going to give him maybe ten minutes or fifteen minutes to make a presentation, some nice wording, and he'll think about it, but after that comes the question. The question needs to be addressed to the Congress people to say okay, you are fine, and also to the NRC. Now, the situation of Yucca Mountain, which is needed for the country, it needs to be looked at in terms of a very, very, very important project to the nation. We spent $12 billion, and what did we get? We're still doing research. Nothing wrong with research, don't take me wrong. But, we need to do some focus.

The tribe opinion is where is the focus. Are we focusing on the major issues, one, two, three, four, to resolve that, from the Board members and the Chairman, or are we still into the jelly fish mode of the DOE and the NRC. And, I think John has done a great job since he came to be the Chairman. I remember when he was working for the NRC, and he said at one point, he said the thermal of the process is still out. The jury is still out on the thermal process, is still out today, and that program.

Well, the infiltration that I came a couple hundred miles to say, the jury is still out. A lot of good research, a lot of good picture research. But, as I read the details, I want to see something that it gives me a good feeling that this ship is going to cross, even if it hits the iceberg. It wouldn't go down, like the Titanic goes down, the Titanic was our problem. So, all the engineers would agree on things, and we need to stay away from the best design, but we need to test that design. Models are great, but research versus applicability, that's a different story.

So, QA/QC, I think the tribe is very concerned about the QA/QC and how the Board is going to resolve that, because that's another perception problem. And, when you get into perception problems, and try to fix what happens, sometimes you have to put a lot of documentation, and you may not even be able to obtain it.

Just to leave you with one last comment. Last night, I got this thing--the 1990, and I was reading it, and it said a Nobel prize winner's a collection of the articles, Albert Einstein and some of the physicists, and all that, talked about physics and RNA and oncology and oncogenes, and so on. What impresses me about these people, that they went to the heart of the matter, and they asked the question. What is it you're going to do? I'm proud to say that even though I don't have a whole lot of money, I told Chester Seats (phonetic) when I was back in Illinois, you can't put the Alaska pipeline under the ground, because the perma-frost is going to push it up little by little, and it's going to be up. Put it above the ground. And, you know the rest of the story.

The reason was I knew a little bit about soils, I knew a little bit about perma-frost, I knew a little bit about modeling. But, that idea made the rest of the story. So, I think, I'm not giving myself credit, but the Board members, you guys need to dig and you need to dig deep and help this guy. He needs all the help he can get.

So, thank you for your time, and have a good trip home. I'm glad you're not a president.

GARRICK: You're right about needing all the help I can get.

HORNBERGER: Okay, well, we are going to break for lunch now, and we will reconvene promptly at 12:30.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

HORNBERGER: Okay, we're going to come to order. We're back in session.

So, I already went over this morning the order of presentation, so, we're just going to continue. Our first speaker this afternoon is Gene Runkle to tell us about the DOE responses.

RUNKLE: Thank you. What I'm going to be describing is what Dr. Garrick indicated in remarks to Congress, that there was extensive investigation, evaluation, re-evaluation, that would be ongoing in response to this. And, I hope to be able to clarify some of the processes there, and some of the outcomes.

The e-mails that we are referring to were reported to the Department in March of 2005. Those e-mails suggested some of the technical outputs, software, information related to the infiltration analysis and models did not meet all of the quality assurance requirements that were in place at the time.

It's important to know that these e-mails were found by the M&O contractor in November of 2004 during a review of the legacy e-mails from people that were no longer associated with our program for potential inclusion of those that were relevant into the licensing support network that is required with our license application to the NRC.

So, it has been two years since those e-mails were reported by the Department in March of 2005. So, the effort that I'm going to talk about is over that time period.

Since that time, there have been investigations by both the DOE IG, as well as the Department of Interior's IG, and the Department of Justice was involved in this review and investigation process.

They concluded in April 24th of 2006 that there were some problems, but no charges were filed.

We also looked at the technical aspects of the infiltration modeling results that had been reported by USGS prior in the 2000 and 2004 time frame. These had been used as part of the 2001 site recommendation, and it was important that we look at those as far as corroborating data, and looking at the relativity and relative aspects of those infiltration results to other data from the Southwestern United States.

That report was issued on February 17th of 2006. There have been copies provided outside here to the audience today, and all those are gone, is my understanding. If you really still need one, let me know and I will try to get you a hard copy of that report.

We have also been doing validation and rework on the infiltration products. The validation that I'm speaking of here is to look at the input data that was going into the infiltration model, look at its pedigree and reverifying and revalidating that process, ensuring that all of the quality assurance requirements have been fully met.

In addition, OCRWM directed Sandia National Laboratories to develop new infiltration rate estimates and maps, incorporate the net infiltration uncertainty, and develop a new infiltration AMR.

There has been an ongoing QA oversight process in there. There have been surveillances. There will be an audit performed in the next few months of that process at Sandia. And, after the final product is ready for final acceptance by OCRWM, there will be an independent review by experts through the Oak Ridge Institute of Science and Energy, which will perform that independent review and provide management with a perspective of the acceptability of that product.

In addition to these activities, we have also been doing a root cause analysis and extent of condition, as known within our program as CR 5223. It has been ongoing since July of 2005.

The root cause analysis and overview. The team looked carefully at the USGS e-mail situation. They determined the root cause and the contributing causes of that. There were several other questions that the team looked at: whether the 2000 AMR that was prepared by the

USGS, whether the 2004 M&O contractor infiltration AMR that used the work from USGS, whether those met the applicable quality assurance and other requirements in place at the time. Whether the attitudes and behaviors exhibited by the USGS employees who wrote the e-mails were seen in other parts of the project. We really needed to establish the credibility of the program, as to whether this was an isolated situation, or whether it was pervasive across the overall program. Whether there were opportunities missed that could have identified and acted upon conditions adverse to quality associated with the infiltration AMR.

The root cause analysis report has been completed. The action plan is being developed by Ward Sproat and the senior managers from the M&O contractor, the lead lab, and USGS. The root cause analysis report and action plan are being fully integrated. We are really making a lot of effort to address not only the root causes, but any other activities that we need to look at in both quality and nuclear culture, and rolling them into the overall action plan.

The root cause analysis report and the action plan will be discussed at the NRC and DOE quarterly management meeting on March 27th of 2008. That is open to the public and there will be copies of the reports available at that time.

The root cause team, as I worked with them, wanted to make a difference in what they were doing, and make a difference with our program. I truly believe that the fact that Ward Sproat and the senior managers from the three main organizations associated with our program are taking the lead in this root cause analysis report. It is being taken very seriously, and chartering the path forward is absolutely the right management approach. It is owned by Ward Sproat, and he has clearly taken a very proactive role in addressing the issues identified in the CR, as well as the overall quality and nuclear culture within our program.

The USGS e-mail situation. These e-mails were written over a six year period. They ran between 1998 and 2004. They were essentially exchanged by three USGS employees. But, they were also provided to other managers and personnel, both within USGS and within the project.

The e-mails expressed a negative attitude, suggested non-compliance with requirements. They talked about back dating of scientific notebooks, back dating of reports, making up dates of task completion, and basically misrepresenting data. These were also, as I said, received by other managers and personnel within the project. There is no evidence that even though these other people were aware of it, that there were any other condition reports developed prior to the initiation of CR 5223 in July of 2005.

An examination by the root cause team of the modeling software, the model reports, the scientific notebooks associated with the USGS work found no evidence that the information was falsified or modified, as suggested in the e-mails. The e-mails may have suggested that you back dated to a given date, but the date on the scientific notebook may have been that date, but we had no correlation back to say that that had been back dated. We couldn't establish that process there. So, we know that there were indications, but we had no clear evidence that it had been done.

In looking at the extent of the condition across our program, we did extensive searches in various data bases. We did key word searches of over 900,000 e-mails. We did physical review of more than 60,000 e-mails that were both relevant and non-relevant, meaning that relevant, it would go into the licensing support network and would be provided for public review. Non-relevant, meaning that that was not appropriate. It might be a birthday party or some other discussion in the e-mail. Our data base consists of about 14 million e-mails. So, we have a very large, extensive data base out there.

We also looked at 7,000 documents from the Corrective Action Program. These describe various issues, and corrective actions that were put in place. We reviewed all of the employee concerns files that we had available to see if there was any correlation there.

The extent of condition found that the--or we found about 75 additional USGS e-mails that were written by the same individuals that continued to express the same attitudes and behaviors. We had five other isolated instances suggesting similar attitudes and behaviors. That is out of the entire search processes that we did, we had five. One of those was associated with one of the employees from USGS that had been exchanging the other e-mails. It involved one of the infiltration software packages, and we opened a CR to address that issue.

We also had two other e-mails from USGS employees other than the three that I've been describing here, and those suggested back dating. Again, we were not able to validate that that back dating process had actually occurred.

We had one e-mail that had disparaging remarks about quality assurance. That particular individual was deceased. We went back and searched through other colleagues--or talked with colleagues of his to try to figure out the extenuating circumstances around those remarks. We were not able to establish that, and our path basically ended because we had nothing more to go on in that regard.

And, the fifth e-mail addressed a situation where there was improper signature on a Q document. This was not a critical quality document, but it was--the individual indicated that that was not his signature. They had a handwriting expert look at that. We were not able to establish. It came back that there was no conclusive evidence that it was an improper signature or a forgery. We turned it over to the DOE IG, and they did not pursue it any further after they did their initial review of the process. Those five are the five that were of similar attitudes.

There were no instances comparable in significance or duration to those associated with the USGS e-mails. And, the conclusion of this whole thing is we did not have a widespread and pervasive pattern across OCRWM of a negative attitude toward quality assurance.

It's important to note that we dispositioned all of the e-mails that were looked at by our review teams. If there was any question, we put them into a further review process. We had, looking at things that were already in a condition report, we allowed that process to continue. We had expert reviews, looking at the e-mails to see if they really believed that it was a condition adverse or quality or other issues. So, it was dispositioned. Many of those came back that we didn't need to take any further action.

In many cases, the way we handled the disposition was to go back and talk to the author of that e-mail if they were available in the project. And, most times, I personally did many of those calls, and the individual would say well, I was just having a bad day, I was just expressing frustration, no, it had nothing to do with it. We didn't take any further action at that point with that particular e-mail, except to say to the individual there are processes that you can use to disposition concerns that you may have of a quality perspective, and here's the method. You can also go anonymously to our employee concerns program, and express a concern to them, and they will investigate it. So, we gave them the avenues for dispositioning their concerns. But, we, again, in our data base, we indicate that we had talked with the individual, and that we had taken care of it in that manner.

Last fall, the GAO started to look at the project costs in response to the USGS matter, in response to a request from Congressman Porter. They reviewed the progress that we had made on a root cause processes to date. This is a viewgraph that they had used in the presentation with Congressman Porter. It's on their web site. It talks about the overall costs of the review processes and the rework processes that we have put forth.

It indicates here that there were seven new issues. That has been further refined, and there are only five, as I previously described, but that was at the time that this report was prepared. We found that two of those issues had already been incorporated in another CR. We didn't have an additional new item that needed to be brought forward.

What you can see here is the process of how we went through, looking at both relevant e-mails, and we looked at 900 of them, and then the disposition. Here was the initial review. Upon the expert review, we ended up with these, and here is the final disposition that we had with each of those.

Likewise, with the non-relevant e-mails, we felt that there was a lot of non-relevant e-mails in the 14 million data base, some 13 million of my round numbers out there. And, so, we were trying to come up with a way to look at this that would have a credible outcome. And, so, what we did was talk to key managers within our program, by identifying 237 key staff. Those were either previous directors or people sitting in very key scientific review processes, and so on, in developing products that were important to the license application, and that should probably be generating e-mails that would then come forward from a relevancy perspective into the LSN.

We went through that process. We originally did 32, looking at 695 of their e-mails from a sampling perspective. We then went back and looked at the full 237. This was their entire composite of e-mails. We pulled a statistical sample out of here, 4,500, physically looked at all those, and dispositioned them.

Likewise, in the 14 million data base, we pulled a statistical sample right there of 25,000 and physically reviewed every one of those. That included both relevant and non-relevant e-mails. There could have been duplication in this process. We didn’t look at it from that perspective because we were making sure that our statistics held credibility there.

Dr. Christopher Morrell was the statistician associated with this. He is the head of the mathematics group there, and he provided a review of all the statistics that had already been accomplished prior to my taking over the project in October of 2005, and then he helped us develop the sampling processes that were used to look at the full composite.

And, that’s documented in our report.

The infiltration AMRs prepared by the USGS and by BSC, that was the USGS in 2000 and the BSC in 2004, were not fully compliant with the traceability and traceability requirements of the QARD. In other words, we didn’t have a product that you could reproduce the results without some additional interactions. And, what you heard in the INFIL 3 discussion earlier was that traceability and transparency.

In discussing with Dave off-line, one of the things that we were not able to do was to reproduce one of the climate maps. After he went back and looked at the data, all the data were there, but they just needed to be reformatted in a different configuration in order to make it run, and we were able to do that. Again, it was just making sure that that description of how to run the model and how to put all the data together was very clear, and could be used by anyone, rather than the scientists that were associated with the process.

The quality assurance processes were not always effective. After these infiltration products that I described in the AMRs from 2000 and 2004 were reviewed and accepted by our program, 35 CRs were written. One of those CRs had 100 items, or issues, associated with it. So, there were many things found from a technical perspective after the product had been accepted.

Some of the data files were not available and the infiltration rate estimates could not be reproduced without further support from USGS. Again, we were able to do eight of the nine maps, but we were not able to get to the ninth one until after we involved the USGS about one year ago. And, they had an exchange of information.

You also asked a question of whether they had used the infiltration to work that was being done with Idaho and Sandia, and those interactions, lessons learned out of that from the modeling. We have provided the USGS with all of that information, and they have our results that we had prior, and they certainly have access to everything that we’re doing right now that may help them in any pieces of work that they want to continue in that effort.

As I indicated before, the infiltration work is being done by Sandia, and we’re going to make sure that all of the quality assurance requirements are met, and that everything is traceable and transparent. You will hear more about the progress that we’re making in those areas with the other presentations from my colleagues.

From a programmatic perspective, reporting of the USGS e-mails as a condition adverse to quality was not timely. It was discovered by the M&O contractor in November of 2004, and was not reported to the Department of Energy until March of 2005. In a good nuclear culture, the CRs should have been written and generated immediately in November of 2004, and then followed up accordingly. That is one of the things that we will be addressing in our action plan associated with this particular process.

Issues with the infiltration products were identified multiple times. Corrective actions were taken, but they were not effective. We didn’t look at it from an effectiveness perspective of what we were correcting, and making sure that it was not reoccurring.

The trending, we did not identify all these reoccurrences as a reoccurring issue, and it should have been looked at in a much broader and higher management perspective. The infiltration work products are being reworked to ensure the accuracy, transparency, and traceability.

As I indicated, the USGS net infiltration rates that were reported and have been used in the site recommendation are supported by corroborating data from the Southwestern United States. We have documented that in our technical report that has been looked at by the NRC. We have exchanged information in that area, and, again, it supports the site recommendation.

The negative attitude toward quality assurance and willful non-compliance with quality assurance requirement displayed by some USGS employees was not pervasive. It was isolated to a few employees within the USGS.

Sandia National Laboratories is developing the new infiltration rate estimates and maps, incorporating the infiltration uncertainty, and redoing the infiltration AMR to ensure full traceability and transparency.

As I indicated before, the root cause report and associated action plan will be discussed at the March 17th NRC and DOE quarterly meeting in Rockville, Maryland, and we will be discussing our path forward in improving both the quality and nuclear culture within our program.

I will take any questions at this time.

HORNBERGER: John?

GARRICK: Gene, you've given us a very good account of what is being done to the technical record, so to speak, as a result of this event. Are you in a position to comment any about what is being done administratively to avoid this from happening in the future?

RUNKLE: Administratively, we're looking right now, John, at the overall quality assurance implementation within our program. We're looking at making improvements, or continuous improvements in that process. We're also looking at enhancing our nuclear culture to make sure that when something is even thought that it is not correct, that we fully identify it, put it into the CR system, and that becomes the way we do business. And, so, those are things that we're already moving forward on.

There have been improvements in the corrective action program over the last few months under Ward's direction. We are making sure that we are classifying our condition reports properly, and that there are effectiveness reviews done to look at the outcomes, and that they are effective and that we're not getting adverse trends. So, there are things that are already started, and those, again, will be part of the action plan to move forward that Ward will be discussing at the NRC meeting. Ward will be doing that presentation, again, to clearly show that top management is engaged, that this is truly his initiative, and that we're moving forward.

GARRICK: Any specific training on how to handle e-mails?

RUNKLE: Yes, there have been--some of the changes that we made were that as part of the review for the relevant e-mails, every employee was required to go through their own e-mails and identify things that were relevant and not relevant. Likewise, we had teams of knowledgeable people go through the legacy e-mails. These people had been technical folks involved with various aspects of the license application development. This was several years ago. And, that particular piece of work was completed. They were brought over to look at these e-mails.

Each of those people took one person’s e-mails and reviewed them, so that you got an idea of what this person, you know, was writing about, and so on, so it wasn’t just a haphazard review. And, that’s where the USGS e-mails were discovered.

Okay, as part of that whole process, we never identified one condition adverse to quality. Okay? Because it wasn’t an emphasis area. So, one of the things that we have done is on our template as we classify each of our e-mails, we now have put in a template that says, you know, is this a relevant, from a relevance perspective of LSN, and is it a potential--or is it a condition adverse to quality.

And, that question has to be mandatorily answered on every e-mail.

We also had absolute, mandatory training of every employee on the program, that here is what you should be doing as far as classifying e-mails, here’s the type of thing you should be looking for, and should you have any question, you know, make sure you check the condition adverse to quality. We’ve gone back through and checked that process. It’s working well.

And, the one individual that did not complete the training on time is no longer allowed to work on our program until that training has been completed. And, that was an M&O contractor employee, and I’m not sure the disposition today. But, we were very serious about making sure that that training was completed, and that people understood their responsibility and accountability.

GARRICK: Thank you.

HORNBERGER: That procedure must cut down on the birthday party e-mails a lot.

RUNKLE: I think this whole experience has brought a different level to our program, because it really has cut out much of the exchange that we have going back and forth, and people, you may have a disagreement, but you don’t need to write it exactly that way. That is so inflammatory and, so on, but, you know, there are some e-mails out there and I don't think we're unique. I think that most industry and other government agencies had this same type of process. The difference and uniqueness with our program versus others is that our e-mails, every one of them, are captured because a potential inclusion into the licensing support network. And, so, we can go back and look at history and pull all this information up. So, we're somewhat unique in that regard, I believe.

HORNBERGER: Looking at your table, the review of e-mails, that really does summarize the Herculean task you undertook. But, I'm curious, over on the far right-hand column, you have several things, new issues or condition reports identified.

RUNKLE: Right.

HORNBERGER: These were not related to quality or, you know, what kind of things fall into new issue or CR?

RUNKLE: The five that I talked about are in there. I think that's the ones that I'm referring to. In other words, there were things there that, or we found e-mails associated with other activities that were part of an existing CR, condition report. And, so, we looked at that and said, well, this is already being addressed through that process. Or, we dispositioned some by saying the experts looked at this and said well, that really doesn't say what you think it said. I am familiar with that particular issue, and it's okay. You know, that would be a disposition. The ones that were new issues, I think you'll find seven of them there, and what I described as five in the final disposition report.

HORNBERGER: Okay.

RUNKLE: Does that answer your question?

HORNBERGER: No, I was just curious, yes.

RUNKLE: Yes, that's how it was done.

HORNBERGER: And, I assume the referred to litigation, is that--

RUNKLE: Yes, there were e-mails associated with some of the silicosis processes within our program, and there is a class action litigation activity there. And, so, they associated with that, and we referred them back to the legal team that was handling that. That's what that means.

HORNBERGER: Okay, thanks. Bill, Thure? Dave? Anyone?

(No response.)

HORNBERGER: Thanks very much, Gene. It was a good update. Okay, Dan Levitt from Los Alamos is going to tell us about the INL technical review.

LEVITT: Good afternoon. I'm summarizing the review that was led by Idaho National Laboratory of the INFIL 2.0 code.

Next slide? This is a brief outline. I'll give an overview of the review, talk about what are the QA objectives, what are the, or just a summary of what some of the QA issues were that we found with the code. Talk about the flow chart of all the codes, of all the pre and post-processors and how they fit together. Then, talk about some specific examples of QA issues that we found, and then I'm going to talk about a simple test case. We had a series of test cases. One of the test cases was to create, recreate INFIL in an Excel spreadsheet, completely independently of INFIL, and prepare the results to INFIL. Talk about the new graphical user interface that's in INFIL 2.2, and then give a summary. So, I want to just explain this number. 2.2, now, we've got the USGS versions are Version 1, Version 2, Version 3, and I heard Version 4 mentioned this morning, and those are USGS. Version 2.2 had nothing to do with USGS. It was developed as part of the Yucca Mountain Project in the last couple years. Just cleaning up, requalifying INFIL 2.0 code.

Next slide? Why are we doing the review? Well, I think we know why, because in light of the e-mails, the decision was made to conduct a QA software review. That's the primary focus of this review, was from a QA software point of view. A technical review was also done, but the focus was QA software.

Idaho was the lead on this. They had software engineers that reviewed the INFIL code, the 11 pre-processors, two post-processors per the latest, most current Yucca Mountain Project QA procedures. Other YMP staff, that includes me and some Sandia staff, conducted the technical review of the INFIL code. And, this took place from about October ’05, for about ten months. And, we were directed to not have contact with the INFIL originators to maintain some independence.

Next slide? So, Idaho conducted the review of all the pre and post-processors, as well as INFIL 2.0, using 68 test cases that we came up with. They updated the code to current FORTRAN standards, with explicit initialization of variables and dimension statements. I’m not a software QA guy, but apparently, the old code, you know, if you took that code, 2.0, and you tried to run it on Windows XP, it might work, it might compile for you, but it probably wouldn’t. And, that’s what Idaho set this up, so that it would work perfectly on Windows XP, as well as on Windows 2000. And, they implemented a graphical user interface, or GUI, to simplify things.

Next slide? The technical review, the primary focus of the technical review was to reproduce the nine infiltration maps that are in what’s called the Technical Data Management System, the TDMS--that’s the data library for Yucca Mountain--and, to reproduce the maps using all the pre-processors and all the post-processors.

What I’m going to describe is just a little bit different than what Gene described, because he was talking about how we could reproduce eight of nine maps. That was actually using--not using the pre-processors, and that was a little bit older in history. And, so, I’ll give the more updated story of what we found.

In the technical review, we also developed and helped run the 68 test cases, and one of them I already mentioned, was reproducing the calculation in Excel.

Next slide? These are the basic QA objectives. I guess you could call them the cornerstone of the Yucca Mountain Project. There’s transparency, traceability, reproducibility. The top two are definitions that I got out of the program. The bottom one I just wrote that you’ve got to have the record of files. The record of files has to be complete for reproducibility. What this means is that I have to be able to come along years later, get Alan’s files, get his input files, get his documentation, get his user manual, get his code, and reproduce the maps. That’s the cornerstone.

Next slide? These are the four basic issues that we found, and they’re related to lack of transparency and traceability. One is a possible mistake in the code. We don’t actually know if it’s a mistake, because we didn’t have communication with the originators. We found that documentation was not always sufficient to reproduce the original calculations. And, we found missing files. We also found a software version control problem. This is just singular right here.

Next slide? Okay, this shows how all the codes fit together. You start with nine geospatial input files, and you run them through eight pre-processors, which ultimately give you ten watershed files. Now, the USGS of course ran these pre-processors, and they put these ten watershed files into TDMS. So, we had those.

Way over on the other side, there are data inputs for present day simulations of infiltration. Those are in TDMS. We had the analog site weather data from other stations. Those were in TDMS. They are run through pre-processors, and we had those data files in TDMS. We did not have these files. These are 100 control files in TDMS. That story actually hit the press. This was before Idaho’s review, so it’s not part of the Idaho review.

But, soon after that, we were able to locate those files with the help of USGS, and actually a contract employee who had been running the code and had the files. And, now, all these files are in TDMS. We know they’re the right files because if we run the--and, we did this for all nine infiltration maps, if we run these files with the watershed files that were already in TDMS, and the post-processed precipitation files that are in TDMS, we reproduced all nine infiltration maps exactly to, you know, eight decimal places.

But, that’s if we do not run any of the pre-processors. And, I’m going to talk about what happens if we do run the pre-processors.

Next slide? This was an instance of a lack of transparency. We called it an apparent error in the code. We don’t really know if it was an error or not, but what we found was that in the second and third soil layers, that the calculation was not multiplied by the percent vegetation cover. There were several. As we went round and round this, we thought that it should be, because otherwise, what it means is that plant roots are evenly distributed in the subsurface, regardless of how dense they are at the surface, and we didn’t think that was correct. But, maybe it was correct. We just heard Scott Tyler this morning talking about how roots move into bare spots and exploit that water. So, we didn’t talk to them. We don’t know if it was their intention or not.

It turns out that this error is insignificant, we believe it’s insignificant, because these transpiration terms that are calculated for the layers are multiplied by root zone weighting factors. The root zone weighting factors were adjusted during model calibration. So, if this was an error, the error was built into the model calibration, became insignificant. The INFIL 2.2 has a switch where you can run it either way now.

Okay, another lack of transparency in documentation involves the pre-processors. There is an INFIL user's manual, and it explicitly states that you should use the 1996 version of a file called 30msite.inp. What this means--can you go back two slides? This very first pre-processor creates that file. So, if you're using a file time stamped 1996, that means you're skipping the first pre-processor. It's not explained why. It just says you start with this file. Back two slides?

So, what we did is we said okay, well, we'll use this file, and what we found is a couple of extremely minor differences in generation of the watershed files. I'll show you how minor they were, but this gives you an example. If you go through and calculate net infiltration for mean present day for one of the watersheds, the differences are in the thousandths of a millimeter per year.

Now, if we went ahead and ran Block R7, the first pre-processor, with the original geospatial input files, we had many, many differences in the watershed files in these blocking ridge numbers, and those are used to calculate potential ET. Now, there are many differences. It turned out that that effect, once you use those watershed files to calculate infiltration, it turns out that effect is fairly small, in the 3 to 4 percent range. But, this was an instance of lack of transparency, a documentation of first of all, why should we use this file, and second of all, if we don't use this file, why do we get these differences.

Next slide? In terms of traceability, we found some missing files. I already mentioned the control files. The Idaho found a missing file that's required with the pre-processor called Geomap7. We were able to recreate that file using geology data that's in the TDMS. And, we were missing shape files that are used to calculate infiltration just for the repository area in the unsaturated zone model footprint. We reproduced this shape file because it was one of our test cases. We did not reproduce this file because it was not one of our test cases. So, we found missing files, but we reproduced what we needed to satisfy our test cases.

I want to actually mention one thing, one more issue of a lack of transparency that I actually heard this morning is Alan mentioned that INFIL 2 was calibrated using streamflow data and neutron logging data, and this is actually the first I've ever heard that, and there's absolutely no documentation that INFIL 2 was calibrated using both. The only documentation I've ever seen is that it's calibrated using streamflow data only. So, I don't know if it's a lack of documentation, or if it's just a--if he doesn't remember it correctly.

Next slide? We also found an issue with version control, in that if you take the pre-processor Markov that's in the records, and you run it and compare its output to its supposed output that’s in TDMS, they have different numbers of digits reported, meaning that the two different codes have different write statements, different formatted write statements. So, if you take this issue and combine it with this issue, which is that there are different IMSL libraries between Windows XP and Windows NT, and what that means is when you’re generating random numbers, you will get different results. So, if you combine these two different IMSL libraries with this difference in number of digits, you end up with--we could not exactly reproduce the precipitation record from Markov. And, you could see the effect is very small. When you plug it in and run INFIL, you end up with differences in infiltration of 1 or 2 percent for these two climates and this small watershed. So, the effect is small, but because of these two issues, we couldn’t reproduce the precipitation records.

Next slide? This just shows--I already mentioned this, this got sort of out of order somehow, but this just shows that if we use the ’96 version of 30msite, we end up with one single rock type that’s different out of 47,000 for Yucca Wash One. And, in this Solitario Canyon One watershed, we end up with differences of 83, 64, and 141 for soil depth class, soil depth, and rock type respectively, out of 14,000. I mean, these are very small differences. They are differences nonetheless.

And, we can skip this slide. We already talked about that. Next slide? Okay, this shows that what we did is we took the conceptual model that's described in the AMR as accurately as we could, and coded it into an Excel spreadsheet for a very simple case, the simple case being one year, this was water year 1995, it's Solitario Canyon One watershed, which is a fairly small watershed--well, it's a medium sized watershed, but we set all our soil depths constant at 10 centimeter, and set all our soil properties the same, and our bedrock properties the same, and then we did the same thing in an INFIL control file. We set everything the same, and we ran the two, and this is what we got. They're identical.

So, what this tells us is that the conceptual model that's described in AMR is consistent with the INFIL code, at least for this simple test case.

Next slide? This gives a picture of what the new GUI looks like. If you double click on INFIL 2.2, this will pop up. You know, it looks like a Windows program. It's got a Help button. Actually, the Help button is pretty good. It connects to files that were taken from the user's manual, so there's a lot of information in there. Prep is the pre-processors. Models, INFIL 2.2, and Analysis is the post-processors. It really does help for keeping track of your files better than the old DOS way.

Next slide? So, what we found in this QA review was QA problems that were in the form of instances of lack of transparency and traceability. We could not reproduce the nine infiltration maps, exactly reproduce them, if we ran the pre-processors, because of problems that are described with Markov and in reproducing the watershed files. If we skipped the pre-processors and used the files that were in TDMS, we could exactly reproduce the nine infiltration maps.

The INFIL code was found to be consistent with the conceptual model described in the AMR, and any errors that we found were not considered to be significant to calculations of infiltration.

Last slide? Yucca Mountain Project follows the nuclear culture, which demands strict attention to detail. There were problems that were identified with INFIL 2 and the infiltration AMR, also with the data sets that were used. I haven't even talked about that. That wasn't part of the review. But, Josh will get into that a little bit. And, this exemplified areas where improvements are needed, and those lessons learned have been learned for the new infiltration model, which has extremely good transparency, traceability, reproducibility.

I do want to mention one more thing as sort of an introduction to the new model, by saying that you're going to hear that infiltration numbers are different now with the new model. In the old model, they were, you know, 4 millimeters a year for present day over the whole model area, and now they are more like 14.

Now, one thing to consider is that we did a lot of simulations comparing the two models, and if you take INFIL and you do a couple of things, you change its soil and rock properties so that they're the same as used in MASSIF, so you change the soil and rock properties, and you turn off transpiration from rock in INFIL, if you do those two things, you get virtually the same results. Something to remember for during Josh's presentation, that the models have some differences, but a couple of changes, and you get the same result.

Any questions?

GARRICK: I guess the short answer is that these events resulted in no major compromise of the science, but revealed poor documentation.

LEVITT: Exactly. Instances of lack of transparency, traceability, and inability to exactly reproduce the maps if we use the pre-processors. But, the differences are very small. Any other questions?

HORNBERGER: When you--you created Version 2.2, but did you go through and do some of the kind of things that we heard this morning from Dave, and clean up the codes?

LEVITT: I didn’t personally, but that’s what Idaho did a lot of.

HORNBERGER: Idaho did, yes.

LEVITT: And, they chopped out--apparently, there are loops in there that aren’t ever executed. They had some sort of way of checking what actually gets executed. There’s a lot of lines of comments that were cleaned up or cut out.

HORNBERGER: Anything else? Dave?

DIODATO: I appreciate the talk. I’m asking this question of all speakers, so don’t feel picked upon, but for the Idaho National Engineering Laboratory study, and your effort as well, how many person years of effort is this review?

LEVITT: For me personally, it was probably about a half a year, and for Idaho, I’ll bet it was several man years. I’m taking a wild guess at this, but just based on my participation with them, something like that.

DIODATO: So, about five or six people for a half a year each, or something like that?

LEVITT: I’m sorry?

DIODATO: Five or six people for a half a year each, or something like that?

RUNKLE: Dave, I just wanted to comment that probably the best source of the expenditure of resource that we have made, and that includes Sandia as well as Idaho and all of that work is captured in the GAO report. We worked very, very closely with them in providing the most sound numbers that we could come up with off of our expenditure system that is out there. So, those are some of the best numbers.

DIODATO: All right, thanks, Gene. So, in the GAO report, it’s 2.2 million for this review.

RUNKLE: Yes.

DIODATO: And, that’s a number you’re comfortable with?

RUNKLE: Yes. We are extremely comfortable with the numbers that are in the GAO report, because we worked hand in hand with them in providing all the data. That doesn’t say that we influenced what they did, it was more that we provided the raw data, and then they took and developed--the example that I used in my presentation, they took our report and that’s what they came up with, and so I couldn’t come up with something better than what they had already pulled from our report.

DIODATO: Well, I’m glad you brought up data. I asked about it this morning, and we heard we are going to hear about it later, and Dan said that Josh is maybe going to talk about it. But, I looked through his overheads briefly and I didn’t see any explicit mention of the data. So, in the morning, we heard stories about--well, discussions about a lot of, or many years of effort in terms of, like, for the neutron logging holes, 99 holes that have been logged, quite frequently over the years and tight spatial resolution. So, was there a review of that data, as well?

LEVITT: There sure was, and that data went through a scrubbing, all the way back to its calibration records, and it all got combined into a new data tracking number, a new DTN.

DIODATO: And, so, were you able to identify any significant errors in your analysis of that data?

LEVITT: Significant errors? There were errors that were documented in a condition report from the original data set, where there were things like duplicate records, or multiple records with the same day. And, in fact, we heard Alan saying that some boreholes were logged multiple times in one day, but they didn't have a time stamp on them, so you end up with three neutron logs for one day, and no way to differentiate them.

DIODATO: So, that caused confusion for you because you weren't able to communicate with the investigators, according to the parameters that were set up for you?

LEVITT: Sure. Sure.

DIODATO: So, the only other follow-on to that is, you know, you've done this for INFIL 2.0, or 2.2, do you envision this process for other codes and how that might turn out? The multi-scale model comes to mind is one you might look at, and then TOUGH react and the calculations for the thermal hydrochemistry, and how that might turn out, because these are FORTRAN codes also that have a long historical development. What's your estimate of how that might go? Could you do that in the same six months?

LEVITT: I don't know if I'm qualified to answer that question, because I don't know much about those models and what their issues might be.

DIODATO: Okay.

NEWBURY: Claudia Newbury, DOE. Certainly if the conditions warrant it, we would go back and look at other codes as well. But, I don't think we can say at this time what ones we would look at, or if we would look at them, or under what circumstances, or what it would cost.

DIODATO: Thank you, Claudia. No further questions.

HORNBERGER: Thanks very much.

LEVITT: Sure.

HORNBERGER: Okay. I suggest that we have one more presentation before we take a break. So, Josh, we may give you a slight rest between your two presentations.

STEIN: Okay, I'm giving two presentations this afternoon, and I kind of see them as sort of part of the same presentation, but we were asked to talk about precipitation estimates first, and then infiltration estimates afterwards. So, I'll stick to that.

Next slide? I'm going to go through the motivation of this work a little bit, and go through essentially how do you characterize climate and variability at Yucca Mountain for present and future climate, some of the sources of information that we use. An approach that we took to simulating that, and I'll discuss how that's formulated and implemented in the new model, and discuss some preliminary results. And, we're labeling these as preliminary because the report that all this is documented in is still within the review and checking stage of the procedure, and we are anticipating finishing that stage within the next month or so. But, so far, we've pretty much addressed all the checking comments, and there are no--I don't foresee any major changes necessary. So, I mean, I'm pretty comfortable presenting this.

Next slide, please? There are a number of contributors to this effort, and I just wanted to acknowledge them. This piece of the work has a sort of a smaller group of contributors, mainly, I was the technical lead and PI on the project. Dan Levitt was sort of--he worked very closely because he had insights into the previous work. Bob Walsh and Cedrick Sallaberry are mathematicians who helped with the new stochastic model. And, Saxon Sharpe was a consultant that we used mainly to bounce ideas off of. She was the author of record on the last future climate analysis.

Next slide, please? So, we've discussed most of this already. This was one of our areas that we looked at pretty carefully because in reviewing the old work, in reviewing the INFIL model, it was unclear from the available documentation, the justification for how future weather was actually--how it was incorporated into the model uncertainty. Basically, the model was run using climate, or precipitation inputs from different bounding stations, and then the infiltration at a given cell was averaged from the results of each simulation. And, it was unclear, first of all, it’s unclear that precipitation is linearly related to infiltration. I think there are a lot of, you could think about it, and there’s a lot of non-linear effects that may take place.

So, we decided that it was also important to really assess the uncertainty in future precipitation and acknowledge that there--investigation a little bit about some of the sources of that uncertainty.

So, as our inputs, we used the results of the 2004 future climate analysis, and specifically, this AMR identifies three climate states, and probably most of you are familiar with this, that are expected at Yucca Mountain in the next 10,000 years. It estimates the timing of those climate states, and it identifies upper and lower bound proxy records for the future climates to represent those.

Next slide, please? In considering uncertainty, we look to the NRC recommendations or guidance provided in the Yucca Mountain Review Plan, and I just wanted to highlight that there are some specific guidelines provided by NRC. The first one is related to time-varying boundary conditions, and precipitation is a good example of that. That uncertainty should be--or these conditions should be considered such that net infiltration is not under estimated. I think NRC has a--there is an understanding that net infiltration is a contributor to dose, and they want assurance that there's not an under estimate of that.

The second one relates to making sure that uncertainties in parameters are adequately evaluated. And, we focused a lot on trying to characterize and define the uncertainties. And, that the treatment of the conceptual model uncertainty, your choice of model introduces uncertainty in a problem, and it's important to acknowledge that and try to minimize it.

Next slide? The goal here, or the motivation is to produce long-term estimates of steady state infiltration fluxes. That's the way it's applied in the TSPA. Even though we know that these are episodic, so we need to upscale these to a steady state effective rate that you could apply over very long periods of time, on the order of thousands of years. Specifically, the new model requires daily values from a representative set of years of precipitation, minimum and maximum temperature, and daily wind speed. And, I just mention that that's sort of the, in the development of the model, that constitutes the weather that's applied as a boundary condition.

Some of the concerns in using historical records, and Alan Flint mentioned this, is that climate variability occurs over time-scales that are shorter than climate durations expected at Yucca Mountain. We're trying to model climates that can range as long as 8,000 years. The observed record is a very short representation of that. In order to adequately represent those long periods of time, I think you need to incorporate uncertainty in those estimates, recognizing that you may be experiencing an especially wet or dry period of the record. And, there's support to that if you look to the tree ring records from the--it's hard to tell now, you know, where we are in this long-term variability.

Next slide? Another challenge, and there's many ways of doing this, one of the challenges is you need to not only simulate precipitation, but you need to distribute it over a diverse topographic environment. And, we have in the model domain that we're using, which is very similar to the one that was used previously, it varies by 1,000 meters in difference. The way we are going to handle this is we actually simulate it for a reference elevation, which we treat as the top of Yucca Mountain, and this is provided to us in the Future Climate Report, as the reference point to apply the proxy climate records for the future stations. So, you will hear a little bit about the reference elevation.

Next slide? As a summary of the future climate AMR, I identified three climates, the present day, monsoon, and glacial transition periods that cover from the present to the next 10,000 years. For the present day, the guidance is use the regional observations around Yucca Mountain. It's shown there as a circle. Essentially, that's how they identify the uncertainty in Yucca Mountain. It says, "Use the regional available data." They don't specify any given stations to use or how to use them.

For the monsoon climate, and I'm showing the durations, there's some uncertainty as to the timing, for the monsoon climate, it identifies an upper and a lower bound. The lower bound is defined as the present day climate. And, the upper bound is defined as weather observed in Hobbs, New Mexico and Nogales, Arizona. And, this was actually a challenge. I'll discuss this a little bit. This was a challenging climate to simulate because it's defined as being kind of switching between present day conditions and a more monsoonal period. And, furthermore, Hobbs and Nogales actually behave slightly differently. They have slightly different weather patterns temporally, so it was actually a challenge.

The glacial transition climate has lower bound stations from Beowawe, I believe is the way you pronounce it, Nevada, and Delta, Utah. And, upper bound stations up in Washington state, Spokane, Rosalia, and St. John.

Next slide, please? Estimates available for present day precipitation, mean annual precipitation, indicate that there's quite a bit of uncertainty in the published estimates that we were able to find. One thing that's very clear is that precipitation in the vicinity of Yucca Mountain, and in most areas, is dominated by elevation changes. And, I'll show you an example of this.

The published estimates vary significantly. There was a study by Spaulding based on data from the Nevada Test Site estimated precipitation at Yucca Mountain of 189 millimeters a year. It was based on records from '63 to '72, local to the site.

Thompson, in a paper in 1999, came up with a much lower estimate, but that was based on climate division normals. And, if your climate division normal is essentially, it's a region of the country where they take a set of weather stations, not picked to be either aerially distributed, or distributed by elevation, it's just an arithmetic mean of the records.

2002, Chris Daly, who runs the prism model, you're probably familiar with that, it's a model for distributing precipitation over complex terrains, he actually had an interesting just conference paper where he looked at elevation biases of climate normals. And, he identified actually the Nevada 3 and 4 as being under represented because they preferentially have lower elevation stations. So, they are probably under estimated.

I don’t really want to assess--I didn’t go into the details of this. I’m just saying that there’s reason to believe that the estimates near or around 200 millimeters a year seem to be supported by the local data when you include elevation effects.

So, an analysis of the meteorological data up to about 2004, is where we had our stop period, we used ten regional stations, we used stations on Yucca Mountain, and we used stations Area 12, 4JA, King Springs, we used a bunch of different stations with longer records. We come up with basically a mean annual precipitation range of 200 to 220.

Next slide? This just demonstrates the importance of elevation in considering precipitation. This is basically the mean annual precipitation from those ten stations plotted against the station elevation, and you can see the correlation is very significant.

Next slide? Actually, back up one slide, please. One of the goals in trying to estimate the--you can see there is scatter along that line. So, one way of estimating an uncertainty at a particular location is to estimate the uncertainty around that regression.

Next slide? Yes?

GARRICK: I have a question about uncertainty. I take it that the uppers and lower bounds are based more on station observations than they are on the propagation of uncertainties in parameters through the model.

STEIN: This was actually--it's not clear in the future climate report how you take upper and lower bounds, or actually, I should, let me back up. It is clear for the future climate results. For the present day climate, they don't identify upper and lower bounds. That's the bottom line.

GARRICK: So, there are, in the infiltration calculations, in the information we got, there's upper and lower bounds in those, indicating an attempt to account for uncertainty, but there's some anomalies there that are not very well understood.

STEIN: In terms of the MASSIF results?

GARRICK: Yes.

STEIN: Yes, I can--we can discuss those.

GARRICK: Okay.

STEIN: I mean, I think what you're referring to is the fact that if you look at mean annual precipitation from the stochastic simulations that are used as input, they aren't directly--the highest precipitation doesn’t necessarily correspond to the highest infiltration.

GARRICK: Yes, and not only that, but there’s greater uncertainty in the far out climates than there is in the near-term climates.

STEIN: Yes, and I’ll get a little bit into that. I think I would support that, just because the establishment of, you know, when you’re trying to predict 8,000 years into the future rather than--I would imagine that there may be more uncertainty.

Next slide? These give kind of a bounds and description of the various climate states. Monsoon, like I said, is really kind of a--it’s a transition period between present day and the monsoonal period. So, the climate report describes periods of time when you really are much more like a present day climate with lots of rain in the winter, or predominant rain in the winter, not very much rain in the summer, and then you move onto a more monsoonal cycle, where you’ve got wetter summer, more intense rains.

The upper bound monsoon, mean annual precipitation, these are based on those analog stations, range from 405 to 420 millimeters a year.

And, then, the glacial transition is a cooler period. The precipitation, that’s focused in the winter season, and you usually have dry summers--usually wet winter season with warm but not too hot, and cool summers, usually dry relative to present day summers.

The lower bound range is 207 to 241 millimeters, based on the analog station, and the upper bound, 419 to 455. And, it's stated in the future climate analysis that these analog stations should be applied to the top of Yucca Mountain.

Next slide? Because of the challenge of simulating net infiltration rates for on the order of hundreds to thousands of years, we chose a similar approach to the Markov approach, except that we simulated 1,000 year sets.

We chose a fairly well established and simple approach based on Woolhiser and Pegram, published in 1978. It's a fairly simple model. It's a Markov chain. It's a first order Markov chain model. It's a model precipitation frequency, and on days that it rains, we examined various probability distributions, and chose a log normal distribution, because it seemed to fit the data the best.

And, then, we used actual observed meteorological data to parameterize the model. And, I'll just explain quickly the model parameters.

Next slide? There are four basic parameters to the model, and Woolhiser and Pegram extended that by allowing seasonal variability in those parameters. And, so, the primary variables are the probabilities of rain, characterized by P00 and P10, and then there are two parameters for the log normal, describing the log normal distribution. Each of those four parameters is described by the Fourier series, the first order Fourier series, shown below, which is described by parameters A, which is the average annual value, B, which is the variation, annual variation, and a theta term, which tells you the shift.

Next slide? Okay, to implement this model, for each climate state, we have these four primary parameters, each described by three fitting parameters, which gives you twelve stochastic parameters, we used a least-squares approach to fit those twelve parameters to the available observations from each of the meteorological stations.

The parameter distributions were defined for each of those stochastic parameters. So, for instance, for present day, we have ten meteorological sites, we have ten values for each of the stochastic parameters that are best fit.

We defined probability distributions for those parameters, and then we screened them into an uncertainty analysis, and the screening was defined as if the relative uncertainty in that parameter was greater than 15 percent, then it was included in our sampling. And, we used a Latin hypercube sampling approach, you're probably familiar with it, it's a structured way of doing a Monte Carlo sample.

And, so, we have, for each climate, different precipitation parameters were screened into the analysis, and then for the other parameters that weren’t screened in, we used the nominal values, either the mean or the median, whatever was appropriate. And, that’s justified in the report.

So, for each LHS--yes?

HORNBERGER: I’m not quite clear on this now.

STEIN: Yes.

HORNBERGER: You screened the stochastic parameters, you said, depending upon some uncertainty threshold of 15 percent. Uncertainty--

STEIN: Standard uncertainty.

HORNBERGER: So, you’re talking about your estimation error of the parameter?

STEIN: Yes. You basically have ten samples of that parameter, based on your meteorological stations. If they all agree very well, you’re going to have a very small uncertainty. And, therefore, you just pick a mean value. If there’s a lot of variability, you define a distribution, you calculate a standard uncertainty. 15 percent was an arbitrary value. We had to choose something because we wanted the problem to be tractable.

For each LHS realization then, so we have one sampling of these twelve parameters, we created a very large set of random numbers, and then using those random numbers, stochastically simulated 1,000 daily values of precipitation. So, we have 1,000 now randomly, stochastically simulated years.

Next slide? So, of each of those sets, we wanted to include--and, when you're trying to understand the implications of long-term processes, and we know that there may be non-linear effects, we wanted to include the effects of some of the low probability events that might really drive net infiltration. We know, like what Scott was talking about, years go by where there's no net infiltration, and then all of a sudden, you have a dumping event. We felt that it was important to include some of these events that we hadn't experienced in these simulations. And, we'll weight them accordingly.

So, each of those 1,000 year sets, we sort by annual precipitation, from highest to lowest, and then we selected from within predefined bins, we randomly selected years, such that we got ten years. And, I'm just giving you an example from one of the replicates. We have representative years, years one through ten. The first year happens to be the wettest year in 1,000 years. And, if you look at the whole replicate, it has an annual precipitation average of 708 millimeters per year. That's a lot of rain. It's more than observed at Yucca Mountain.

However, it's weighted one in a thousand. So, if the effect is, you know, the intent here is to try to understand, and basically include these non-linear effects. And, so, each of these ten years then would be run through the infiltration model, and then the results of the model for each year are weighted according to the weight attributed to the year. So, you get an infiltration from year one, it would be multiplied by a weight of .001, you get infiltration from year two, it would be multiplied by .002, and the sum of all those products gives you your long-term estimate of your net infiltration.

Next slide? Just some comparisons of our stochastic simulations based--I'm comparing back to the actual site meteorological records. These are box plots. The line is the mean. The dashed line is the median. The box represents from your 25^{th} to your 75^{th} percentile. I believe it's 10 to the 90^{th}, are the bars, and then anything outside of that are shown by dots.

You can see if you take the present day sites, and don't correct for elevation, you get a very large variability. But, once you've corrected them for elevation, here is the distribution for the present day observations, and here are two replicates of 20 realizations showing that the realizations compare well to the replicates. And, I've given some statistics comparing some of the observation means to the stochastically simulated means.

Next slide? For monsoon, we have a lower bound, which is present day, so this is the elevation corrected present day. The scale has changed. That’s why it looks a little bit funny. The two observations from the upper bound monsoon site, and the two replicates. You can note that there are a few realizations that are higher than anything observed, or are higher than the mean annual. We feel this is justified based on some of the descriptions about the monsoon climate. There’s some language in the future climate report that suggests that--they weren’t able to find any analog stations that actually matched the criteria that they laid out for that climate. So, these were the best available, and there’s reason to believe, based on the OSTRACOD records that they were using to characterize the monsoon climate, that higher precipitation values might be justified. So, we have included some higher precipitation values.

Next slide? In the glacial transition, we have two lower bound sites, two upper bound sites. And, this is kind of an interesting one, in that the two replicates look quite different, and this is a stochastic result. And, so, this actually, we didn’t go and resample to try to make them look better. So, we went forward ahead with those.

Next slide? So, summary and preliminary

conclusions. We’re using purely stochastically generated weather, and we’re including very low probability events. The stochastic model is based on analog stations, and then because we’re generating 1,000 years, we end up producing some weather that’s outside of our band of representation, as you’d expect. We represent the seasonality in the probabilities and the amount of rain, using a first order Fourier series, which implies that you have a single wet and a single dry season.

We looked actually at a second order Fourier series. You can match the data, obviously, if you increase the order, you can match the data better. The question really is is how do you combine stations. In the first order, the parameter is actually a physical meaning. So, it’s easier to understand. If you go to a second order, how do you actually combine data from different stations when you fit. So, we actually have some comparisons using a second order, but we actually used the first order.

We used ten representative years out of 1,000, and our simulated precipitation matches the observations when you compare the observations to the simulations.

That’s all I have for this.

HORNBERGER: Questions? Thure?

CERLING: Cerling, Board.

So, when you’re making your rainfall simulations, are you taking each day of the year, in any order, and then calculating a probability of rainfall? Or are you taking--

STEIN: Yes. We actually, yeah, to calculate the probabilities, we actually--

CERLING: My follow-on question that's sort of related to this, does this produce El Nino like years, or would you get a different result if you modeled sort of years, and then distributed the rainfall within a year?

STEIN: I does not, because we generate 1,000 random years, and then we selectively pick representative ones, and we run them individually, starting at a fixed initial condition, you know, each one, there's no--we don't incorporate any period in the climate record.

Now, what it does do is it includes years that are much wetter than we've ever experienced, but they are weighted lower because they are presumed to be low probability events.

CERLING: But, those might be because of very high individual events, as opposed to an El Nino year where you might get a lot of smaller events that results in a large infiltration for one year?

STEIN: Yes, you can get both of those. You can get, you know, in 853 years from today, you know, the hurricane that comes up--I'm being facetious--and sits over Yucca Mountain, it captures that type of event, but it also will have events where you will have a very wet winter. The probabilities actually, especially when you get into the glacial transition, your probabilities of having lots of wet days followed by more wet days, you know, increases.

There are other ways of doing this. I mean, you can look at--there are methods that are published looking at spell lengths, trying to do stochastic simulations of the wet spell lengths. You can go to multiple--you can look, you know, Markov, third order of Markov, which then requires six parameters, so it's a trade-off. The more details you add, the more parameters, the more data you really need. And, I think the uncertainty in this case sort of outweighs--you don't know whether what you see today is necessarily going to hold, you know, next millennium.

GARRICK: You said that there's many ways of doing this. Some characteristics of this problem make me think that a Bayesian type analysis might be a corroborating way of going about it. Has anybody looked at it from the standpoint of looking at the past as a basis for a prior, and then looking at the present, updating it with the present, and seeing what kind of posteriors you get and whether or not you can correlate that time span in any effective manner with future conditions? Because in the past, we've had glacial conditions and we've had monsoon conditions, and the evidence is just as strong for those, or stronger than the future.

So, it would seem to me that there is kind of a fundamental foundation here for a pretty effective Bayesian type analysis. I didn’t hear you mention any--

STEIN: We haven’t looked into that. I mean, it’s a--

GARRICK: Sandia is not Bayesian.

STEIN: Right. It’s a difficult, and who’s to tell if we got it right or wrong.

GARRICK: Yeah. It just would seem to me it would be a much simpler and more transparent approach, and I was just curious as to why--

STEIN: Well, one problem with doing that is, I mean, you can go back and look, the approaches of looking back in the past actually have quite a bit of uncertainty, because typically, they are based on pack rat mittens and things where you’re looking at present species, and where you find the seeds from those various species, what conditions, do they exist today. There’s temperature issues that may have been different. There’s a lot of--and I discussed this with Saxon Sharpe, you know, that’s her area, this is not my area, and my take-home message from her was that be careful about narrowing your uncertainty too far, because when you really look at what these records are based on, they’re based on proxies of--lots of assumptions go into, you know, where did that pack rat go for food, you know, what types of--is the pack rat an actual, a good gathering, that it’s gathering a random sample. I mean, there's a lot of issues. I'm not an expert.

HORNBERGER: Bill?

MURPHY: Bill Murphy. You mentioned several times conducting long-term predictions of climate, and you refer to climate states expected in the next 10,000 years on one slide, and for a monsoonal or a glacial transition climate lasting over 8,000 years. Have you really considered long-term precipitation rates over the hundreds of thousands of years period?

STEIN: That's I guess a question that I would throw back at the future climate analysis. I mean, that was there. They were charged, in that analysis, they're looking back hundreds of thousands of years, and trying to get an answer to that question, and their response was use these bounds to characterize the uncertainty, and, you know, these existing records. So, I guess I would defer that to a paleoclimatologist.

HORNBERGER: Josh, as you pointed out with your glacial transition climate stage, it looks like there is a question of the stability of your estimates of the distribution. And, my question is it's cheap to run that stochastic model. Why not run it for 10,000 years and still do your statistics for a thousand year sample?

STEIN: Yes, that will be rev, the next rev. I mean, we had to make a call, and we felt that 1,000 years was, you know, an order of magnitude greater than what was done before, and that--I mean, yeah, you could argue that it could get, you know, for 8,000 years, that the wettest event in 8,000 years is--

HORNBERGER: Yes. That wasn’t what I was suggesting. I was suggesting to simulate 10,000, but then just do the statistics for the thousand years, because in your thousand year simulation, when you pick that wettest year, you have one representative for the thousand year event, which isn’t a very good estimator.

STEIN: Yeah, but we have that for 20 realizations. So, for each climate, we have 40 years that are the wettest years in 1,000. And, for each--that’s for each climate.

HORNBERGER: Okay. So, you’re doing it then.

STEIN: Yes.

HORNBERGER: I mean, you make a point that the mean annual precipitation might be 200 millimeters, or so, at the Yucca crest. My recollection is that for INFIL, it was like 193 millimeters. Am I wrong there?

STEIN: I guess I would ask Dan to help me out on that one if he has information.

LEVITT: I can’t remember exactly.

HORNBERGER: I was just curious whether there was a difference there. You are making a point of it, and I didn’t know whether you were making a point that it was now different.

STEIN: Well, the thing to remember about the INFIL, how they represented, they ran, and Dan could probably help me out on how they actually represented the present day, they ran a 4JA simulation. They ran an Area 12 simulation, which were pretty different, and you could see that in some of Alan's plots, where the Area 12 plot is a lot wetter. And, I guess I found--I guess I was just approaching the problem a little differently, where we have an answer, you know, there's an objective, an estimated value, and we're trying to essential capture what that is from the available records around Yucca Mountain, rather than making assumptions that 4JA and Area 12 are the bounds, and they're symmetrical, or whatever. Because when you make an average between two numbers, you're sort of assuming that your objective is halfway between.

HORNBERGER: I guess what I'm trying to get my arms around is how different are your mean precipitation maps from previous mean precipitation?

STEIN: If you look at precipitation distributed over the whole domain, we typically have slightly lower precipitation values.

HORNBERGER: That's what I thought.

STEIN: And, part of that is due to the--we use a linear lapse rate, precipitation lapse rate, and INFIL used an exponential form. So, the functional forms were different.

HORNBERGER: Dave?

DIODATO: Diodato, Staff.

Thank you for the presentation. I'm going to try to get my hands around your statistics, so if you can bear with me? You had, on Slide 9, you talked about maybe 70 years of data, or so, and you analyzed those and came up with a mean annual precipitation for Yucca Crest, like George has referred to, of about 200 millimeters per year. So, what is mean annual precipitation? Is that the expected value of precipitation? How does that translate?

STEIN: Yes.

DIODATO: What's the mean? Does that include, I guess I'd say straight out, does that include all of the years, consideration of all the years of data?

STEIN: Yes.

DIODATO: All right. So, even years when there is low annual precipitation?

STEIN: Yes.

DIODATO: And, years when there was high?

STEIN: Yes.

DIODATO: So, okay, so that's your--so, then, if I go to Slide 15, for example, and I know that the recurrence interval relates to the probabilities in some way. But, you've also got these weights here. So, I'm trying to kind of mentally put together a cumulative distribution function, but it should be on the recurrence, not on the weights, right? But, if I did it on the weights, if I count up from the bottom, from year ten, this is out of 365,000 Row Excel spreadsheet. I mean, how did you calculate? This comes from your precipitation calculation?

STEIN: Yes, it's a thousand, I believe it's a thousand rows, and--actually, it would have to be the other way, 365 columns.

DIODATO: Yes. So, every day of every year for 1,000 years in one spreadsheet?

STEIN: Yes.

DIODATO: So, you've got that all put together there. And, then, you pull these representative years out.

STEIN: Yes. Are you clear on what the recurrence interval means?

DIODATO: It's related to the probability of occurrence; right?

STEIN: Yeah, it has to do with, I mean, you could think of it as it's the average number of years that would occur before the event exceeds that event. So, I mean, for the wettest year, it would be, on average, it would be 1,000 years before you'd get an event that was equal to or exceeded that.

DIODATO: But, in terms of looking where the numbers might fall for a mean annual precipitation, to relate it back to this Yucca Crest number, can you do a cumulative distribution function going up your weight column, so adding?

STEIN: Well, the weights add up to one.

DIODATO: Adding up to one. Right.

STEIN: So, if you took your--I mean, I suppose I haven't actually--

DIODATO: So, if I go up from year ten to year nine to year eight, then that will be like 54 percent on the CDF in terms of probability, and read across, I would say with a 54 percent probability, precipitation would be as great as 157 millimeters per year, or less.

STEIN: I think--yes, I believe that's right.

DIODATO: Okay. So, that's less than the mean annual precipitation, even with a higher than 50 percent frequency, right, on the CDF. I'm just trying--I don't know what this all means. I'm trying to figure this out in real time.

STEIN: Part of this is the--you notice the years six through year ten have a probability weight of 18 percent, cumulative.

DIODATO: Yes.

STEIN: And, so, I mean, those are chunks. You know, you have to look, it varies probably between 157 and 186.

DIODATO: Okay, yes, I was just trying to figure out how these--

STEIN: Or 227, yes.

DIODATO: I was just trying to figure out how these numbers relate to a--

HORNBERGER: Basically, it’s not a CDF, it’s a histogram. It’s a discrete form of a CDF.

DIODATO: Right, exactly. But, you can interpret it in a similar fashion.

STEIN: Yes.

DIODATO: That’s kind of an important thing.

STEIN: And, this is for one replicate.

DIODATO: And, you have 40 replicates--

STEIN: Two replicates, 20 realizations in each replicate.

DIODATO: Okay. All right, of 1,000 years of precipitation.

STEIN: Yes.

DIODATO: Great, thanks.

HORNBERGER: Actually, that does raise a question then. So, let’s take your 1,000 year recurrence interval. You’re saying 708 is what, the mean of 20 realizations?

STEIN: Yes.

HORNBERGER: It’s the mean?

STEIN: Yes. We actually included, and this gets to your question about maximum, you know, some of these may be due to very large events, which does occur, we actually, in the infiltration model, we've applied a maximum daily precipitation value that we allow. Now, it turns out it doesn't actually--we've looked at this formally through a sensitivity study, but it doesn't actually--the results aren't very sensitive, because when you get a very large event, and we used the largest daily observed precipitation in the U.S., you get lots of runoff, as you can imagine. So, at that point, you're limited by your soil properties and bedrock properties.

HORNBERGER: Other questions?

(No response.)

HORNBERGER: Well, thanks very much, and we will give you a brief rest, so you can have a glass of water before your next talk. We'll take a 20 minute break.

(Whereupon, a brief recess was taken.)

HORNBERGER: Okay, we are reconvened, if everybody can find a seat. So, Josh, we're now into your second presentation about the estimates of infiltration.

STEIN: Okay, are we ready?

HORNBERGER: We are ready.

STEIN: Okay, next slide, please?

There are a whole long list of people to acknowledge, and I don't probably have enough time today, but I want to point out that this was not only a Sandia National

Lab effort, we involved in the development team folks from academia and folks from a whole variety of different backgrounds.

Dan and I were kind of the leads in terms of getting the conceptual model and the implementation and stuff. Al Reed is a Sandian, and really, without his help, I don't think we would have met the quality assurance and also the--we produced a product that in Mathcad that really I would encourage you, once it's released, to look at, because it's Al Reed that takes a lot of this credit, and it's a good example of how you can use technology to really improve traceability and transparency, and I will talk a little bit about that.

Rick Allen, we had him on contract, he's the author of FAO 56, which is the basis for the new ET model that we're using, and David Groeneveld has a business, and he's worked at Yucca Mountain in the field before for the NRC, I believe, as a consultant. But, he helped us look at satellite data to try to characterize local vegetation characteristics, which I will talk a little bit about. John Stormont at the University of New Mexico started out really helping us go through the INFIL documentation, and try to understand whether we could reproduce the actual properties for the bedrock, for soils, and provided a lot of guidance on conceptual model verification. Because we weren't able to communicate with the originators of the INFIL during this process, he provided a lot of basis on sort of making the judgment call of whether we were going to go with the approach as we understood it from the INFIL, or take a different approach.

Next slide? Actually, the person, Kaylie Rasmuson works at BSC. She’s an expert at Yucca Mountain vegetation, and she actually had collected a lot of data at these environmental study plots, which we used to calibrate our satellite measurements of vegetation. It’s important to have ground trees when you look up from so far.

Next slide? I mentioned those. Daniel B. Stephens was not directly involved in producing the model, but he was tasked to do an independent review, and, so, as we were going from step to step, we would basically apprise him and give him an update and Todd Umstodt is representing Daniel B. Stephens. He was the project lead for that project. So, they’re still looking at our model, and they’ve provided comments and they will produce a final report.

Okay, next slide? So, starting in about July, and this is when I got involved in this project, was July 2005, and we were tasked to do two things, and I think we have spent a lot of time talking about the first one, which is the replacement of the INFIL model, but the original INFIL calculations, those nine maps, in order to be used in TSPA, they had to be weighted, basically provide a probability of a weighting function, and that was done in another AMR called the analysis of infiltration uncertainty. And, when we were starting this project, we decided that we would redo that AMR as well, so, we wanted to really incorporate the estimate of uncertainty into the calculations directly rather than trying to do it afterwards.

In addition, when we started the work, BSC was leading a team to produce nine data qualification reports, and it’s the boundaries between BSC and Sandia, this was during the transition period, so the details are a little bit more complicated than that, and some of the people were from Sandia, but those included site maps of properties, bedrock types, soil properties and bedrock properties, vegetation units, and also kind of a review and compilation of the available weather data. That’s what I talked about before.

We produced a new model, it’s called MASSIF, it’s unlike INFIL in the sense that it’s not a FORTRAN code. It’s actually a--I kind of liken it more to a--you see these e-books out there where you can take--it’s written in Mathcad, and I’ll talk a little bit about that next. The report is going to be documented as a revision in name to the AMR, MDL, MDS, they just 00023, it will be Rev 1, it’s an absolute replacement, it’s just a revision in name. It’s more of a procedural issue. And, it’s, like I said before, the work is still in the checking phase, so it's inherently preliminary.

Next slide? The goals for this, we weren't really given free license to go out and create a brand new model. The idea here was to build upon INFIL, the conceptual model, and so we used a mass balance approach, we used the similar grid domain, we're using a field capacity representation of flow in the system. And, as we were developing this, we were evaluating the sub-models, based on the available documentation, to make sure that we could stand behind the justifications, and, in some cases, we didn't have enough information, that we felt uncomfortable. Based on the information we had, if we had taken that data with the knowledge that we did have, we would have made a different decision. In those cases, we went with that different decision. I'll try to point some of those differences out.

We took the quality assurance priorities and objectives very seriously. I mean, I came from the WIPP project before this, and actually, in listening to some of the--and, when I started on Yucca Mountain, I have a slightly different perspective of quality assurance requirements, because the way it was, and this is purely by happenstance perhaps, but the way it was introduced to me when I started on the WIPP project was we had QA people working on the team with us, and it was described as this is the way you should do your work. This is, and I think QA and the scientific method fit together naturally, so I’m very proud of this work in terms of its transparency, its traceability, its accuracy in terms--I want to mention a little bit that the actual algorithms and the routines used in MASSIF have been independently verified by John Case, who has been independently verifying these things separate from us. We’ve set it up so he can run a complete different set of calculations that are linked to our calculations, and verify in real time essentially that everything is working as planned.

Go to the next slide. Just talk about the choice of Mathcad. Mathcad is not a compiled language. It’s a graphical kind of interface to a calculation. It incorporates documentation, so inline, you don’t have to--you aren’t limited to comment fields, you can draw pictures, you can draw diagrams, you can have every step of the calculation is described in great detail. There are hyperlinks. When you open the calculation, it opens up as a table of contents. It’s just like a book. You can go into the introduction, it talks about how the calculation is structured. If you want to go in and look at how ET is calculated, you double click on it, it will bring you to the section of the documentation where ET is described. It will then have a hyperlink to the actual implementation, and the implementation is documented step by step. So, this has been an eye opener for me, because I did come from a programming background, at least with my graduate work. I used FORTRAN codes, and it can be problematic unless it’s very well managed. And, Mathcad really does offer an environment where you can really document a calculation in real time.

And, I really do believe that when you do see it, you will be able to repeat the calculations. I can guarantee it. And, I’m hoping I’ll never get another call from anybody regarding this.

Next slide? I took this actually, and Alan showed this, and actually, there’s one modification. MASSIF, we had a competition for the acronym, and we came up with MASSIF, mass accounting system for soil infiltration and flow. There’s one change here that I actually just noted when Alan was talking about it. This is the diagram that shows the net infiltration boundary, and one change that we do have in the MASSIF model is that our net infiltration boundary is the bedrock soil interface. I just wanted to make that clear. It’s not shown there.

This equation shown on the bottom here shows essentially the mass, or in reality, it’s a volume balance, where you have--this is done on a cell by cell basis. We chose a 30 by 30 meter digital elevation map based on the shuttle radar topography mission, which is a little different than--we didn’t use the USGS DM. And, basically, this equation is solved on a daily time step for each cell in each watershed, and you start, the order of the calculation is you start at the highest elevation cell, such that you can see in the left-hand side, runoff is the output, and that then becomes run-on to the next downstream cell. So, it's the same watershed routing process, the same model for INFIL.

The parameters there are runoff equals precipitation plus run-on, plus any snow melt that's added to the soil, minus any precipitation that falls as snow, snow fall, minus sublimation, plus any change in storage, minus ET, and then minus any net infiltration that leaves the bottom boundary of your domain.

The challenge, I mean, Scott, I think, alluded to this, the challenge of solving this type of equation is that precipitation typically is a dominant input, and ET is a dominant output. So, we're trying to calculate the difference between two large numbers, with a lot of uncertainty. And, that is a difficult problem to get accurately.

Next slide? I'm going to go through each sort of groups of different input parameters. So, for water input into the system, I've talked about daily precipitation being stochastically simulated, and ten representative years, and I've also talked about the precipitation lapse correction. We model temperature as a sinusoidal function, and it's modeled separately on wet and dry days. Because if you actually look at--there is a difference in many of these stations that you end up getting differences, less difference in daily temperature when it rains than you do on dry days. You get higher highs and lower lows on dry days.

So, on days when the average daily temperature, the mean of the maximum and minimum is below zero, we assume that the precipitation falls as snow. That’s similar to the INFIL model. And, then, water enters the soil as snow melt as a function of the daily average temperature. It’s kind of a temperature index method.

Sublimation losses are represented. They are removed, if the temperature is below zero on the day that it snows, we remove a portion of that water rather than trying to model the removal of the snow pack dynamically.

Next slide? The water content and the change in storage in the system, this is very similar to the INFIL 2 methodology, except with some minor changes. We still use a field capacity approach. One of the differences with out approach is that we define field capacity as ranging between the water contents at a suction pressure of negative one-third, and negative one-tenth bar. So, that we introduce some uncertainty in that, based on just textbook definitions.

The model is layered vertically into as many as three layers, and the top layer has actually two sublayers that are defined in terms of how much plant canopy exists at that time. So, this is a--you can think of the cell as being divided between parts of the area of the cell that’s shaded by any plants, and areas that have bare soil. This gets at the choice of the ET model that we use, which is the FAO 56 guidelines, and specifically, we’re using a dual crop formulation of that which includes explicitly modeling the bare soil evaporation. So, those fractions of that surface layer cell, or the evaporation layer, as we like to call it, vary depending on how much vegetation is present and the time of year, and I’ll get into that in a little bit more detail.

Layer two is the remaining portion of the root zone, and we defined a maximum rooting depth in the model. And, that’s based on looking at present species at the site for present day, and then also looking at future climates and looking at predictions of what plant species will be existing there and doing some statistics.

All these parameters that we assign spatially are upscaled to 30 by 30 meter grid cells. It’s just something to keep in mind. So, you have to be a little careful when you take point measurements at various properties. We’re upscaling them.

The bottom boundary is the top of bedrock, and we do make the assumption that roots do not penetrate bedrock. And, I imagine this is one of the key differences between

INFIL and MASSIF, and perhaps it’s a distinction of definition rather than process, because I think--I mean, I believe that there is water that is removed from bedrock. I mean, that’s why there is caliche there. The question I have is how much, and what basis do you have to estimate those rates?

So, in our modeling, we didn’t identify a quantitative way of estimating that process, and based on some of the NRC guidelines that I had outlined in the previous talk about be concerned about under estimating an infiltration, we made the decision to exclude that process.

Now, one of the questions I do have is, you know, there obviously is some amount of water that is removed from bedrock, and I guess the real question is how much.

Next slide? So, what moves from, just like in the previous one, this is the cascading bucket model, water moves from upper to lower layers when the field capacity is exceeded, and low is limited by the soil conductivity. So, there are two ways that you can generate runoff in this model. One is if your intensity of your rain is such that you are applying water fast enough that the soil can’t absorb it. I didn’t talk about rain intensity. There is a parameter in the--we looked at hourly weather data from the different stations for various climates, and we related the number of hourly intervals in which you measure precipitation on a day, to the amount of rain that actually fell on that day. And there is a loose correlation, it's not a strong correlation, there's a lot of variability, but what you can say is that monsoonal stations tend to have much more intense, shorter bursts of precipitation at Hobbs and Nogales, and glacial transition tends to be a lot longer period of low intensity rain, and present day is somewhere in between. And, so, we have used some data to actually do some regression analyses and come up with parameters for that.

Net infiltration occurs once the soil layer contacting bedrock exceeds field capacity. And, it's limited by the bedrock conductivity. And, bedrock conductivities are significantly different than what was used in the previous results. And, that will actually--that's a fairly significant difference. The soil conductivities are different as well, and I probably should, this has come up. Part of the effort in looking at the original data sources, there were lots of measurements that were referred to this morning, and in going back through the records, and I wasn't involved directly in this work, but this is one of the teams that BSC had put together for looking at the various data sources, the records in many cases were insufficient to reproduce the values that we found in the tables in the INFIL report. And, I don't know, I guess I can't speak to the specifics on every case. Ken Rayfeld was a key member of that team, basically managing that team, so he might be able to answer some questions specific to that.

But, in the cases where we weren't able to verify, basically take records in TDMS and arrive at the values that were in the tables, we chose different approaches, different methods. And, I'll talk a little bit about some of those.

Next slide? The model domain, you've seen some pictures, the composite here is the model domain, and actually, we had information sort of from within that square, or the rectangle, and we essentially had shuttle radar topography data, and we used watershed terrain processing toolbox from ARC GIS to essentially route water through the topography.

We filled sinks, which I'd note it's a common practice when you take digital elevation models, occasionally you will get areas which have actual local lows. And, you know, if you were in a karst terrain, that would actually be expected. In this terrain, it may exist locally, however, for the routing of stream flow, we have assumed that it doesn't exist. So, any time that there's a local sink, we fill it.

In most cases, the fillings are on the order of 1 meter or 2 meters. There's occasional noise in the data that causes higher amounts of sinks. I think there's one cell that had to be filled with 16 meters of soil. It's a radar measurement, so there’s probably, you know, there’s going to be various causes for some anomalies.

So, each watershed, the domain is actually divided into, given this data set, we had to divide it into eleven drainages. The previous DEM was divided into ten drainages. Each watershed drains to a single point at the outside. These are like in Forty Mile Wash down here. And, the constraint is is we had to include everything within the UZ modeling boundary, because we needed to make sure we had coverage, because that’s where the results are moved downstream in the TSPA. And, so, eleven watersheds were required to fill this area, and I’m showing the repository boundary here.

Each cell drains to a neighboring cell with the lowest elevation, and it’s commonly referred to as U8 algorithm because each cell is surrounded by eight cells. You’re looking for the lowest one.

Next slide? For the ET model, I said before we used FAO, which is Food and Agricultural Organization of the United Nations. It’s designed for calculating evapotranspiration for crops, agricultural setting. However, there are extensive, if you look in the back of the document, the FAO document, they have a whole host of extra chapters in there on how to apply it to natural vegetation, and, so we identified this as a good candidate for incorporating actual

Yucca Mountain specific vegetation, desert specific vegetation. It has ways of incorporating plants with high resistances, such as you would expect in the desert, and it also has a dual crop formulation which explicitly introduces the bare soil evaporation. And, those are both potentially important in this environment. It’s based rather than on a potential ET, it’s based on a reference ET, which is a predefined, there’s various references, it’s calculated with a Penman Montief formulation, and it’s based on a well--these are the reference grass, it’s clipped to a certain height, it’s well watered, and it reflects essentially how much that particular reference crop under unstressed conditions would evapotranspire.

And, then, to calculate actual ET, you multiply it by a series of coefficients, which act to modify that. So, there’s a water stress coefficient, which basically varies from zero when there’s plenty of water--or, sorry--zero when there’s no water, or very little water, to 1 when there’s plenty of water. And, that’s a function of soil properties and vegetation itself. There’s the basal transpiration coefficient, or the basal crop coefficient, Kcb, and this has the function of the vegetation. I mean, it varies from zero when there’s no vegetation, to 1.35, approximately. That would be for something like a lettuce, you know, something that actually transpires more. So, we’re dealing with much lower Kcb's in this environment. And, then, there's the Ke, or evaporation coefficient, which is a function of soil properties, and also vegetation, because it varies depending on how much canopy there is.

Next slide? A quick discussion on the reference ET. We used a Penman-Monteith equation as recommended by Rick Allen and FAO 56. You know, recognizing that solar radiation is the most important, probably source of energy in the system, we do some--it's, I guess, it's a similar approach to the SOLRAD program, except there's slightly different processes that are being considered.

We estimate, basically, the solar radiation on a horizontal surface based on the Hargreaves equation, which relates the solar radiation on a daily basis to the temperature difference on that day, so the idea is on a clear day, you tend to have a larger swing in temperatures, and that's especially true in the desert. And, in that case, you would get a higher incoming solar radiation on the horizontal surface. And, on a cloudy day, it would be reduced. That's essentially the Hargreaves equation.

We do a slope-azimuth correction, and this is based on time of year, orientation of the sun. This is work that's published by Rick Allen. And, we have actually validated this approach, the Hargreaves and some of the other coefficients that go into this, using solar radiation data collected at the desert rock station nearby.

Minimum and maximum temperature are modeled separately on wet and dry days. I mentioned that before. And, because we don't have an analog for wind speed in future climates, we make the assumption that the wind speed measured during the present day is applicable for all future climates. And, actually, the reason why we decided to go with a daily fit to actual data is because there's a very strong seasonal pattern in the wind speed, where you get a peak in April, and it's repeatable throughout. It appears to be a fairly robust pattern.

Next slide? Okay, this is a little complicated. The real, you know, we went into this--we decided to go with a new version of the ET model because of some of the stuff that Scott I think was talking about, and there's a bunch of papers published around 2004, which really keyed in on the importance of getting the local vegetation correct, and the importance of vegetation in estimating ET in arid environments. And, so, I think I'm going to give you the lessons learned.

We've gone to great effort to actually incorporate a new ET model that does use available site information. It uses satellite measurements of vegetation, and actually tries to model the dynamics of how vegetation comes and grows over the year, senescence times, and at the end, we're actually getting results that are very similar to INFIL. I just wanted to point that out. We didn’t know that going in there, but we actually, INFIL was doing quite a good job, even with just textbook values.

But, the Kcb, we have to estimate that for each grid cell, for each day of the year, depending on the characteristics of the year. And, so, there’s various components to that. First, what we did is we obtained many, many images, basically land images that you could calculate a normalized difference vegetation index, and this is a standard way, it’s basically the difference between the near infrared, minus the red, divided by the sum of those two, the radiances, and the higher that value is, the greener your good cell is, or your pixel. And, so, it’s been used, it’s a classic, it’s one of the sort of most widely used vegetation indices.

What we did is we collected data on an approximately monthly period through three different years, a very wet year, sort of a moderate year, and a very dry year. This map right here is a parameter that we’re calling the potential vegetation response, and in essence what it is is it’s a normalized difference of NDVI from the wettest year, subtracting out the driest year. And, the reason why we needed to do this is because you actually can get NDVI signals from rock varnish, and actually, this is pretty new stuff. We had some problems looking at some of the dry year. We were getting this NDVI signal, but what you realize is if you subtract out from the dry signal, you could actually--we identified it. It was actually coming from bare exposed rock.

So, this is a difference map, essentially. Anywhere that’s dark, the darker colors down here, indicate a potential for greater amounts of greenness, greater amounts of vegetation. And, the places that are red, like up here, are areas where there was no vegetation, essentially, no difference between the wet and the dry year. These areas up here are characterized by pretty harsh terrain, lots of bare bedrock, things like that. So, that gives you kind of a spatial picture.

This picture right here is a plot of day of water year. This is for the wet year that we chose. And, it shows two things. It shows in the bars here are the NDVI, including the uncertainty, for an upland area--actually, this is for--let me back up. We had data collected at the sit over a number of years from these ecological study plots, where they actually did vegetation monitoring within these areas, well controlled areas, they knew where they were. They had weather stations set up. They measured on a monthly basis, I believe, they went out and measured percentage of different species of plants. They went and did a full catalog of the ecology of the site. They did leaf area measurements. So, they had the types of plants, which you could then go and look in a textbook and find, or, you know, in the literature and find, stomatal resistance values.

So, these lines here are the uncertainties, or basically, this is the mean, the upper bound and the lower bound for a Kcb calculated from site specific data. And, we compare that to NDVI measured at that same location. So, we located the pixels for each of those study plots, and they aren't the exact same years, but they are on years that have comparable precipitation. We just weren't able to get the data on this in time. So, this is the relationship here.

And, then, we do a regression analysis where we plot NDVI versus Kcb and the regression, we're using as a--minimizing Ki squared approach, which incorporates individual uncertainties in each of the data points. So, there's been a little bit of confusion about--the values we get, it's a rigorous treatment of the uncertainty. And, this ends up being a significant uncertainty that gets screened into the uncertainty analysis.

So, essentially, we have a location, we have a day of year, and then we have total annual precipitation. And, we come up with a scaling factor that relates--essentially, it's a model that predicts NDVI for any location as a function of precipitation, and also slope and azimuth.

There’s a lot going on in this slide, and it would be almost a whole talk just to talk about it.

But, essentially, we’re trying to get at spatially and temporally, how does vegetation signal respond at Yucca Mountain. And, then, we’re relating that to NDVI and Kcb values at ground location. And, so, therefore, the model really predicts an NDVI, and we then use this regression to assess out a Kcb.

HORNBERGER: --how you got your Kcb’s. You said you did it from site data.

STEIN: Yes.

HORNBERGER: Site data on evapotranspiration?

STEIN: No, site data on actual plant--it’s from these environmental study plots. There’s a, in the back of FAO 56, they have essentially a whole series of methods to use.

HORNBERGER: So, it’s the FAO empirical approach?

STEIN: Yes.

HORNBERGER: What you did is identified how much of which plant, and then looked up in their tables and picked off--

STEIN: Well--

HORNBERGER: And deleted them?

STEIN: Yeah, yeah, that’s essentially--it’s not in their table because they’re mostly focused on crops. So, we had to go to other literature that actually you can measure stomatal resistance by sticking little sensors over the leaves, I guess.

HORNBERGER: Yes, but you didn't do that. You just looked it up--

STEIN: Yes, that's right, we looked it up from basically other people who had done that. We used those values and we used the equations in FAO 56.

HORNBERGER: Okay.

STEIN: Okay, next slide? Okay, some of the other inputs. These are maps of soil depth class, soil group, and bedrock type. And, these were maps that were produced by some of those other efforts that we're looking at, the underlying data. And, one of the problems we had initially with using soil depth, there was a set of equations in INFIL that related to soil depth at any one location to a set of empirical equations that were related to slope of the surface, and what soil depth class it was in.

And, actually, I should just note that the numbers got reversed. If you're looking at an INFIL, what we call shallow soil, which is soil depth class 4, is soil depth class 1 in the INFIL model.

Ken could speak to specifics, but we looked at, we couldn't trace back where those equations came from, and when we compared soil depths observed at various borehole locations, we were unable to justify those fits. And, there's actually not that--this is approximately 125 square kilometers, and there aren't actually that many soil depth measurements made that we could find. And, so, we decided to treat these as larger regions where we would upscale a value.

So, the way this was set up, we actually assigned soil depth based on measurements that were made within each of these regions. And, the key one, as you can see, is blue, soil depth class 4, we used two different approaches to estimate the uncertainty in the upscaled value for that, and it's defined as a uniform distribution between 10 centimeters and 50 centimeters. And, this ends up becoming a very important parameter, as you might imagine, because it controls how much water you can store. It's one of the components in controlling the water.

The soil groupings, we did a similar thing here. I'm going to talk a little bit about soil properties in the upcoming slide, but we looked at the various soil--information we had on the soils from within these soil groupings, and there were originally nine soil groupings, and we used statistical, basically statistical tests to see whether information from one soil, even if it had a different soil classification, its properties, if they were statistically similar, we grouped those together in order to increase the number of data. We didn't want to propagate a soil classification scheme if we didn't have the data to back up the distinction between them.

So, we ended up grouping soils 2 through 6, 3 and 4, and then 5, 7 and 9. And, I think the real basic way of thinking about this is we have kind of two main soil types. We have a 5, 7 and 9, which is reflective of the uplands area, and we have this soils 3 and 4, which is more focused in channels, so that will actually come up later.

Next slide?

HORNBERGER: I have a question. You mentioned your soil depth and you say you're doing upscaling. What do you mean by upscaling? You're taking your point measurements and doing something to them to--

STEIN: Yeah, what we looked at is we had point measurements from various locations, and what you find--basically, I believe, for soil depth class 4, it was--it looked like the data followed a lognormal distribution. And, so, one of the approaches that we used was to essentially estimate the, basically, the, I think it was estimate an effective value for a lognormal distribution, lying somewhere between the mean and the median. So, that gave you essentially a point 1 to point 5 spread.

Alternatively, we actually went--we had somebody independently go out and take photos at the site, and make soil depth measurements, and it's in a scientific notebook. We had a statistician look at the observations from that scientific notebook independent of the point measurements, and come up with a spatial distribution, and estimate an upper and lower bound based on that data. And, they both actually agreed to that level of uncertainty.

Just to give you a perspective, this area which covered, you know, soil depth class 4 covers, I think, 60 or 70 percent of the model domain, we had 35 point measurements in an area that's like 70 square kilometers. There's a lot of variability going on there.

I wish, I mean, I think that with further work, you could come up with a model of soil depth, and relate it to conditions on the ground. I know I've seen--I've heard of some work done by the Center where they have actually done, you know, a more mechanistic model of soil depth. We just didn't have time.

For soil properties, this was another case where there are tables in the INFIL report, they're in Appendix B, and where it lists essentially the essential properties of each of the soil groups. And, there are DTN lists. We had people go back and try to recreate from the underlying data, and because of transparency and traceability issues, we weren't able to do that for all soils.

We had another effort with the data qualification where we did have lots of soil texture data, several hundred measurements across the site of soil texture data, and this group decided to use a pedotransfer function approach, working with a data set in Hanford, Washington at the Hanford site, where they had taken detailed measurements of the soil characteristic curves for those soils, and then matching them in a non-parametric way, kind of a closest match based on percent of the various size fractions, matching them to a Yucca Mountain sample, and then making a correction for rock fragments. And, there's a whole separate report on that approach.

And, from that, we get hydraulic conductivity, field capacity, which we defined as between these two suction pressures, a wilting point defined at minus 60 bar, same as the INFIL model used, and a saturated moisture content, sometimes referred to as porosity, although it's not really porosity, but it's essentially porosity.

Next slide? Okay, this is bedrock conductivity. I mean, early this morning, Scott talked about the use of inferred data, and once again, we are using inferred data, and we estimate conductivity. Previously, the conductivity was actually assumed that all fractures were filled. And, so, we had essentially all fractures filled with a caliche material. And, then, the differences in conductivity would be attributed to differences in the matrix conductivity, the aperture of the filled fracture, the filled fracture conductivity, and the fracture densities. When looking through the data at various observations, it was noted that there are areas where fracture filling is not pervasive. There are areas that the fractures are not filled at all.

I've been out at the site. I've got to say that if you, depending on where you go, you will get a very different impression. When I was talking to some of the Center, we had an OR visit, I heard--there's places where the fractures are filled with soils. I didn't actually see that, but there's a lot of uncertainty as to how much of the fractures are filled, and what they're filled with. And, it obviously varies depending on where you are.

This is the Alcove 1 infiltration test, which infers a conductivity. This is sort of a strange plot. This is bedrock, hydrogeologic unit number, so there's no meaning in terms of increasing value. It's just a categorical variable. And, this is a log scale of Bulk Ksat. You can see this particular case is approximately an order of magnitude above the inferred 100 percent fracture fill.

What we did--or, this was also a group that was the qualification group. They looked at what--there appeared to be evidence that some portion of these fractures at some locations were actually open, and there were some open components. And, so, we defined an uncertainty between an upper and a lower bound, lower bound being 100 percent fractures filled with caliche, and an upper bound with each fracture having a 200 micron open fracture component. And, that’s shown in the red line here. The black line down here shows your full fractures, and this yellow line is based on air permeability tests done in the same rock types, but at much deeper depths, where you don’t have any pedogenic calcite. So, you could think of that as possibly a representative of an upper bound.

So, that’s how we characterized the uncertainty. Another data point that I don’t have shown on here was a study done at Fran Ridge where they took a large block out for testing, and they did a flow experiment there where they ponded water and watched it infiltrate, and they actually went back and excavated with dye so they could see where the fractures were flowing. And, that actually inferred a much higher infiltration rate, upwards of I don’t remember--Dan, do you remember the actual value? Yes, 4 meters a day.

We didn’t include that in this plot because it was collected not at the surface. It was actually collected slightly below the surface, so you might expect it to be higher.

Next slide? Okay, the model has approximately 200 parameters, and we have vegetation parameters, we have soil and rock property parameters for various different rock classes and categories. For each one of those parameters, we used the available data to define an uncertainty distribution, or a probability distribution for that parameter. And, then, we had to go through a screening process because we didn’t have the resources to run, you know, 200 parameters. We would have had to run thousands of realizations.

We chose a screening process that’s based on these two criteria. If it was a geospatial parameter, meaning that it was a parameter related to location, then if that unit covered more than 15 percent of the UZ modeling domain, then it was included. So, that brought in soil depth class and two bedrock properties, bedrock types that were within the UZ model domain.

If it was a non geospatial parameter, then we used the same screening that we did for the precipitation, that standard uncertainty is greater than 15 percent. Now, this is arbitrary, so we actually later on went back and made a validation of this, which I will refer to, to make sure we didn’t miss any parameters. But, we had to do that on a smaller domain.

So, the screened-in parameters, if they’re in between 11 to 15 that were sampled with LHS, or Latin Hypercube Sampling, for each climate, and for each climate, we created two replicates, and these are the same replicates that I showed for the stochastic parameters, and we did two replicates because we wanted to test the stability. We knew these were small, you know, when you’re doing 20 realizations and 11 or 15 parameters, you may have the opportunity for some instability in your results. So, typically, you can evaluate that, and quantify that, the added uncertainty from the small sample size, by running two replicates. And, we did that.

So, then, when we compiled the actual output of the model, we combined both replicates. So, we have 40 realizations representing each.

Next slide? Okay, this is a very quick and just overview of the new results. And, we’re looking at, these are the MASSIF results and these are the 10^{th}, the 50^{th}, and the 90^{th} percentiles of those 40 realizations. And, these are averaged over the whole domain.

Now, you can slice and dice this a variety of different ways, and we’ve reported different numbers. I mean, if you look at just the repository footprint, you get a slightly different number than you would--and, so, we have here compared with the numbers coming out of the 2004 version of the INFIL report, which those numbers are the same as coming out of the 2001 version as well.

So, you can see that we’ve--the means have gone up, depending on where you look, approximately a factor of two to three. I actually think this is a little bit more interesting, and this is basically presenting the mean water flux fractions as a percentage of the total precipitation, that enters in, and you can see the percent of the precipitation, this is kind of, you know, similar to a Maxey Eakin way of categorizing it, we're getting 8, 9 and 10 percent respectively for each climate of the precipitation as infiltration. And, these are the percentages for ET, percentages for runoff, and I want to just point out that runoff is a very small fraction of the water budget. So, you have to be--things that control runoff don't necessarily control net infiltration. And, this is sublimation. You can see it starts to kick in a little bit when you get to the glacial transition where we're actually getting snow.

HORNBERGER: I want to make sure I have this right. Up until now, I haven't heard you say that you've done any calibration whatsoever. You haven't tried to match anything.

STEIN: We thought about doing calibration, and we--I guess we--I mean, this is a very difficult problem to calibrate because what's your goal? Your goal is to try to estimate net infiltration. If you calibrate to stream flow, I guess my feeling is is that stream flow occurs on a different time scale, you know, it's what controls stream flow. It's controlled by the surface, conditions of the soil. It's controlled by antecedent moisture conditions. You look at it like a runoff curve method.

The parameters that are actually going to control runoff, I don’t feel are sensitive parameters in controlling infiltration. And, you know, I guess I feel uncomfortable calibrating something that’s 1 or 2 percent of the water budget, to calibrate net infiltration.

HORNBERGER: I mean, I didn’t mean to get into an argument with you, I just wanted to clarify that you have not calibrated up to this point.

STEIN: We did not, we decided to, instead of calibrating, we decided to try to assess the uncertainty range. It’s kind of--

HORNBERGER: No, that’s fine. But, I mean, when you’re comparing MASSIF with INFIL, INFIL was calibrated.

STEIN: Right.

HORNBERGER: And, so, this factor of 3 or 4, whatever, is not surprising at all.

STEIN: Okay.

CERLING: Well, another question that has to do with what exactly the 10^{th} or 90^{th} percentile means, and is that like the 10^{th} percentile or the 90^{th} percentile of 20 realizations or--

STEIN: Of 40 realizations.

CERLING: Of 40 realizations.

STEIN: Yes, we combined the replicates.

CERLING: You take all of those realizations and that’s the 10^{th} and 90^{th} of the realizations, not of all of the things making up each realization?

STEIN: That’s correct. So, that’s one way of--I mean, that was our way of characterizing an upper--I mean, one thing to note is, you know, when you’re trying to estimate uncertainty at the tails of a distribution, you’re in--we didn’t want to go and try to say that the estimate--a maximum or a minimum, because the uncertainty in a maximum or a minimum are much larger than when you get into--closer into the distribution. It’s just more robust.

And, actually, the UZ flow group is using the 10^{th}, the 30^{th}, the 50^{th}, and the 90^{th}, and that will be explained. Their model uses a different set of data. It uses our data coming in as a boundary condition. But, it’s also based on data collected deeper within the UZ. And, so, they have more information in order to help guide weights and stuff.

HORNBERGER: Now, again, the top left table, this is what I think John had alluded to earlier. So, your 90^{th} percentile under monsoon, you have 52 at the 90^{th}--53 at the 90^{th} percent, and 47 at the glacial transition.

STEIN: Yeah, that’s because--

HORNBERGER: In your precipitation model, you have more wet days with less precip under one--

STEIN: There are differences between the glacial transition and the monsoon, both in precipitation, so, it’s one of the reasons, maybe, and I’m going to show you a more detailed plot next.

Maybe we can just go to the next plot. Is this one not in there?

HORNBERGER: There it is.

STEIN: Okay, good. 22, okay. It's different than what I have. Here, I've plotted precipitation versus infiltration on a linear scale, and the pink, there are 40 small pink boxes. Those are the present day results. The triangles are the glacial transition and the green boxes, the small green boxes are the monsoon.

And, one thing that's clear when you look at the monsoon is that it really spans a much larger precipitation range, and that has to do with the fact that when I first talked, talking about the monsoon is really a transition period, and it's unclear, there's a lot of uncertainty in terms of whether it's going to be a classic monsoon or whether it will be reflective of present day, which is the lower bound. And, so, you get a larger range.

I've also just plotted on here for reference some of the INFIL calculations. These are the raw calculations. This is an Area 12 calculation. These are the MOD 3 PBT, 4JA and then a subset of 4JA for the driest years. And, then, these are the upper bound monsoon, and these are the upper bound glacial transition sites, and then lower bound glacial transition.

I've also put in just a reference line. I find this useful, is just putting in a 5 and 10 percent of precipitation, because when you're comparing different plots, it's always good to--those are kind of easy ones to draw in.

So, one thing that's clear here, though, is that precipitation is one aspect of net infiltration. What are the other aspects? Well, I'm going to discuss those. We did a pretty detailed sensitivity study, and we'll discuss those in a future slide.

But, let me just show you some of the spatial distributions. I've just chosen the 10^{th}, 50^{th}, and 90^{th} percentiles. These scales go from zero, which are gray, to 100, it's a truncated scale, so, there are some values that are higher than 100, specifically in some of the channel areas and, you know, very localized areas. But, this seems to be a scale, but it works well through all the different climates.

And, this shows you kind of the variability. Can you go back? So, this is 10^{th}, 50^{th}, and 90^{th}, and there's a lot of variability, both in the magnitude, and also if you look at various locations in the details, and where the infiltration occurs. You can see in this case, you can actually see stream channels pretty clearly, and you can see them showing up within here, and there's other simulations where you don't see this. I'll talk a little bit about that in some of the validation stuff later on.

Let's go to the next slide. This is for the monsoon climate, same kind of variability.

Next slide? The glacial transition. These are representative of, you know, there are 40 different pictures you could look at.

So, the sensitivity analysis, we had, you know, we had run these LHS analyses with these parameters. We actually did three separate types of sensitive analyses. Basically, we used a stepwise regression method, where we related the uncertainty in the inputs to the uncertainty in the outputs. And, we basically recognized that there's two types of uncertainty in this system. There's the uncertainty in the future weather patterns, based on, this is like the uncertainty in the future, it's aleatory uncertainty. This has to do with the particular years that we chose, the particular patterns of wet and dry days, the--you can imagine you could get the same amount of annual precipitation, but if it all occurred on one day, you'd get a different answer than if it occurred for two weeks straight. You might get a lot more infiltration if you had steady rain for two weeks, rather than a very intense storm where you got a lot of runoff.

So, the first analysis was just--used all the raw--it included the aleatory uncertainty. It included, basically, it took the results, using the 40 different precipitation files, the 40 different parameter sets, and it did a stepwise regression, and essentially it identified which parameters were important.

The second analysis, we chose to fix the aleatory uncertainty, and this is typically done if you want to try to focus in on parameters that are epistemic, have epistemic uncertainty, that you have a chance at actually going back later and doing more work, trying to reduce that uncertainty. Soil depth would be a good example. If you came up with a more detailed approach, you might be able to reduce the uncertainty in your soil depth, and, therefore, you would reduce the uncertainty in your predictions.

And, then, the third analysis was an extended analysis. This is that validation. We ran a single watershed. We did 200 realizations, and we decreased those criteria such that we included a lot more parameters, allowed them to vary, and then we ran it.

So, the results here, the first analysis, 70 percent of the variance in mean infiltration is attributed to annual precipitation and shallow soil depth. So, if you fix those, you can really dial in your uncertainty--I mean, dial in your result. It’s controlled by the uncertainty in those.

In the analysis two, if you take out the aleatory uncertainty, so we use the same weather input file for each of the LHS samples, soil depth and the water holding capacity, which is defined as the difference in water content between field capacity and wilting point, that accounts for 90 percent of the remaining variants. So, you know from this that soil depth and holding capacity, of the parameters if you had to go and design a monitoring program, that’s what I would recommend, you know, if you wanted to extend this and try to reduce the uncertainty, if that’s deemed necessary. You would focus in on those parameters, because that’s going to increase your confidence.

The third analysis is just that we ran this extended set, and we found the same top players. So, we didn’t miss--it sort of gives us confidence that we didn’t miss any important parameters.

Next slide? Okay, I’d like to go through quickly now some comparisons in the model, running with actual observed weather data, and trying to match predictions, both at the site, and at other sites, in an effort to--this is sort of a summary of the model validation.

We compared--I already showed you comparisons of precipitation records from our stochastic stuff, so, you have seen that. Let me back up. There’s two kind of methodologies that you need to do for model validation, according to the procedure. One is confidence building during model development. And, we treated those as tests to the specific submodels. As we were developing the submodels, we wanted to have test cases so that we could make sure that they were reasonable.

And, then, there's post-model development validation, which corroborates our predictions at Yucca Mountain in various locations to actual measurements at Yucca Mountain. So, the ET submodel, we compared against a data set collected at the Nevada Test Site. That's weighing lysimeters. And, if you aren't familiar with weighing lysimeters, they're essentially a--you can think of them as a dumpster that sits buried in the ground on top of a load cell, and very accurately measures changes with time with the weight of that, and basically, you use that to track precipitation entering the system, and then tracking ET losses over time.

We have a nine or ten year data set from the Nevada Test Site, Area 5. We also had a lysimeter data set from Reynolds Creek Experimental Watershed up in Idaho, which is probably a more--reflective of conditions more representative, more like a glacial transition climate.

We also compared runoff results. We validated those by simulating some smaller sub-watersheds where there was actual gauging station data, and comparing them. Alan Flint talked a little bit about this, similar data sets to what he had used to calibrate the model, the INFIL model.

And, then, we did extended sensitivity study, which was just validating that we hadn’t missed any parameters.

For the post-model development validation, we looked at the observed seepage event that occurred in the South Ramp in the winter of 2005. And, we actually simulated and worked with Berkeley National Lab in their simulations of that, too, and basically verified that we actually found seepage at that location.

We also looked at some data compiled by Gary Lucain (phonetic), at the USGS, infiltration data from Pagany Wash, at the base of Pagany Wash. And, I’ll discuss that a little bit.

We looked at a literature search of published regional recharge estimates. And, then, we also compared the model to sort of a--the MASSIF model to a more mechanistic or Richard’s equation approach, based on HYDRUS-1D. And, we also compared our results to an expert elicitation, which is another form of making estimates on the project.

So, let’s jump ahead. I’m going to talk about the HYDRUS-1D experiments first, because we actually used HYDRUS 2 for the lysimeter data. The alternative model comparisons, we defined four models that differed only on the soil depth. So, they’re 1D simulations. We ran MASSIF, and we ran HYDRUS-1D, which is based on the Richard’s equation, and although the transient responses, if you look on a day to day basis, as you might imagine, a more mechanistic approach, the transient responses were different. When you integrated them over the year, which is more reflective of kind of what we're trying to get at of these predictions, we have a pretty good comparison.

This just compares the results, net infiltration, and these are all for net infiltration, or actually net infiltration, ET, runoff and change in storage between MASSIF and HYDRUS for the four simulations. And, they are pretty comparable.

Next slide? This is a plot of data from the Area 5 weighing lysimeters from the Nevada Test Site. The red curve here is the actual observations for the bare soil lysimeter. There are two lysimeters. One where they tried to plant native vegetation, and it took a while for it to get hold. Actually, Dan Levitt was involved in actually the set-up and installation of those lysimeters. And, the vegetated lysimeter. So, the red is the data. The blue is the MASSIF simulation. And, the darker, the black line is the HYDRUS simulation. And, there's also a published simulation using UNSAT-H for a smaller subsection of this data. And, all of the approaches do a pretty good job, especially when you compare cumulative differences.

You know, the transients are a little harder to predict. Some of that is uncertainty in terms of, you know, the weather station data. There's uncertainty in how do you actually take weather station data and apply them to the lysimeter, because there are some days when, you know, the change in storage is not possible, given the amount of precipitation you're predicting. So, there are some uncertainties in this, but the comparisons were encouraging.

HORNBERGER: You use site specific soil properties for these tests?

STEIN: Yes.

HORNBERGER: So, actual measurements?

STEIN: Yes. Next slide? Okay, this is an example. There were, I believe, six stream gauges--or five stream gauges and three major and several minor runoff events that were logged at stream gauges. And, this is just an example of Wren Wash. And, one of the real uncertainties in trying to predict, you know, actual response at a given location is that you have limited positions of your weather station. So, we actually took surrounding weather station data and ran each one of those separately. And, so, you can see some of the differences. Those are the four plots here.

The information on the plot, the dotted line just shows the snow level at the top of the watershed. The solid line shows the snow level and equivalent, just height of water at the bottom of the watershed. We show that just because sometimes in the simulations, snow melt actually is contributing in these cases to runoff.

The blue dots are precipitation, shown on the right. The triangles are the actual gauge measurement of runoff in cubic feet per second. And, the bars here are a range of model estimates. And, what we did in order to match the runoff, we had to adjust soil conductivities. We had to dial down soil conductivities, because with the nominal values that we had, we didn’t get runoff in those cases. So, we turned down the soil conductivities. Typically, it’s a modest amount considering that conductivity is usually log normally distributed. We had to reduce it by a factor of between 2 and 3 generally. And, then, we are able to match both the timing and the relative magnitudes pretty well, and I just chose one example. There’s a whole series of examples of those matches.

The mean infiltration, I should just say, for those simulations, when you reduce the soil conductivity, the mean infiltration is relatively insensitive to changes in soil conductivity. So, we didn’t feel that--I mean, this is a case where like, you know, you might say well, why didn’t you calibrate each of these watersheds, soil conductivity? Well, there didn’t seem to be any need to because we can demonstrate that essentially, the infiltration isn’t sensitive to it.

I want to show the next slide, but don’t go there quite yet, we also had one additional estimate of an infiltration event in lower Pagany Wash from Gary Lucain, and we re-examined that data and we were actually able to run that watershed and adjust soil properties in a little bit different fashion. We actually had to increase the soil conductivity in the channel, and decrease the soil conductivities outside the channel, and we were able to match both the runoff and that infiltration event for 1998.

And, those changes were--I don’t remember the specifics, I think we had to increase the permeability an order of magnitude. It was consistent with some of the--there’s two published data points, one of which occurs in Pagany Wash from a double ring infiltrometer test from the USGS, and it’s consistent with the adjustments that we had to make.

We took that just to test the sensitivity of the model to those particular changes, so, you know, we have one infiltration estimate, and a few runoff events, we’ve dialed the dials and we’ve changed just soil conductivity. We decided to use those parameters and rerun the full domain. This is just an example. So, go to the next slide.

So, here is a base case. This is the 30th percentile infiltration map from present day. We took the present day and we varied the conductivity in order to match both runoff and infiltration measurement at the bottom of UZ.

And, what you see is the infiltration rates are relatively similar. I mean, I would caution anybody from--we've arbitrarily taken one, or a tenth of a millimeter as a cutoff, but when you compare replicates, the uncertainty in a mean, if you want to take an uncertainty in a mean as the standard error, we're looking at approximately 2 to 3 millimeters of uncertainty in the mean. And, it would a greater uncertainty in your--so, I consider these to be essentially equivalent.

However, look what's happened, is that the infiltration has moved to the channels in this case. The take-home message here is that there is a lot of uncertainty given the data that we have spatially around Yucca Mountain as to exactly where the infiltration is occurring. And, you'd have to do a lot of very specific site studies, I think, to really get at--characterize that uncertainty.

Next slide? Here's the same data point, the 40 for each climate. This is just a comparison to a literature search of models that are out in the literature. There's the Maxey Eakin relationship here, and these are some other published model estimates. We tend to be, especially at the lower precipitation values, we tend to be on the higher end of estimates, and that could be due to the uncertainty in our soil depth, or other parameters. You know, if you went and studied the site--if you needed to get that much--if you needed to refine that, you may find that your numbers would come down if you actually made detailed measurements at every, you know, all 100,000 grid cells. Is that necessary? Well, that's a question for TSPA. I mean, if it ends up being a critical sensitive parameter, maybe more effort will be placed there. There's probably other features in the system that may play in that would be more important.

Let's go to the next slide. I think you saw this slide. This was included in a talk by Russ Dyer in January. One thing I wanted to mention here, this is basically our predictions from the MASSIF model, compared to regional predictions based in Nevada. And, one thing just to--one thing I think is not necessarily obvious, if you're looking at this, we show that Maxey Eakin model as a line, and then we show a whole bunch of Maxey Eakin model specific points, and they don't fall on the line. The reason for that is each of those point estimates is looking at a basin that has a different precipitation map.

And, so, you're looking at it, it's like a weighted mean infiltration. So, you know, the upper parts of the basin will have more precipitation, and it may, you know, fall over here. The lower parts may fall over here and have zero precipitation. And, when you combine the whole basin together, you get an effective value that ranges in this region. And, these are very consistent with our estimates.

Next slide? So summary and preliminary conclusions? As Dan alluded to, we have, although it's not in the scope of the model report that we're producing, we have run the INFIL code against the MASSIF code, and if you parameterize them the same way, you get very, very similar results. So, one of the things I just want to make sure it's clear, we went into this thinking that the ET may--the ET model and the characterization of site specific data might be an important factor, and it's not as important, apparently, as other factors.

Our estimates, if you look at the uncertainty analysis, and, you know, look at a 50th percentile as a representative value, the infiltration estimates are generally higher, they're always higher. As Jim will explain, there is more data, and there's other sources, ways of basically ranking these estimates based on other information. So, this would be more like a Bayesian approach, where you're kind of like, this is a first prior information, if you add more information from different parts of the mountain, you may find that you would weight some of these differently.

MASSIF accounts for parameter uncertainty, and it's kind of incorporated in the whole model development. The bedrock conductivity values are significantly higher than those used in INFIL, and that has to do with this component of open fractures. And, I think if you wanted to go out and constrain that a little bit, you'd have to go out and do some more ponding experiment. And, those are difficult to do, and sometimes difficult to interpret. But, that's the type of data you'd need to get in order to really get more of that information.

Soil depth, soil properties and precipitation are the most important parameters, and the MASSIF model appears to match the available site data that we've compared to quite well. And, I think that's it.

HORNBERGER: Thank you, Joshua. Questions?

GARRICK: I just want to comment. I very much liked Scott Tyler's comments this morning about monitoring, because that's something we have to face. Based on what you have done, and your sensitivity analysis, and if you were put on the spot to make a recommendation of a monitoring program, what are some of the things you would do?

STEIN: I would try to go out and collect information to create a model of soil depth, is the first thing I would do. I would go and--so, I mean, I would try to, you know, either use satellite data, use some sort of a--

GARRICK: Isn't that characterizing rather than monitoring?

STEIN: Okay, so let's talk about monitoring. Let me think a little bit about that. I don't know, Dan, do you want to--do you have any ideas?

LEVITT: Right now, as far as the numbers, no. As far as stream flow monitoring--

STEIN: Yes.

LEVITT: We have data from events in ’95, 1995, a couple of events in ’95, and one or two in ’98. Alan used just the ’95 data, I believe. They’re very rare, and I don’t think they are being monitored right now. I would be useful to have more stream flow gauges. Again, that was, Josh pointed out that’s a small percentage of the water budget, but it still helps to know how much water is running off the mountain. It would help to be able to either, to have some data to calibrate to or validate to that is better direct measurements of infiltration.

Neutron logging is one way to do that, but neutron logging is really labor intensive. But, there are--

HORNBERGER: So, did you use the neutron log data that were available? Why not use them if you need those to calibrate?

LEVITT: We did, we tried to. Comparisons weren’t, they weren’t that great.

STEIN: One of the issues with the neutron logging data, when we went and actually looked at that data, there were questions concerning the calibrations. A lot of those neutron logging profiles that were shown were shown through soil and bedrock. They were not separate calibrations done as far as I know for those. I mean, the instruments, it’s sort of a raw--it’s a more qualitative measurement than a strictly quantitative one, because it’s uncertain how to calibrate.

GARRICK: How do you measure water holding capacity? Is that an input/output?

STEIN: That would be, you would measure that by actually collecting soil, and doing a--basically, characterizing the moisture characteristic curves for that. And, water holding capacity is the conceptual approach. So you have to make a definition of what it is, so you basically would saturate the sample, and start drawing suction on it, and measure water content at various intervals.

LEVITT: Instead of neutron logging, you know, technology has gotten quite a bit better for things like sensors, you can go out and bury sensors to measure water content, depth of wetting fronts, so that you have direct measurements of infiltration. There’s very little of that data that was collected that’s actually qualified and in TDMS, other than the neutron logging data. That would be quite useful.

HORNBERGER: I’m a fan of technology, but I fail to see why the neutron logging data are so deficient. That’s the first time I’ve heard anyone suggest that those neutron logging data weren’t good to get moisture contents.

LEVITT: Well, I mean, let me clarify, a great effort was taken to go through all the old neutron log data, clean it up, trace it back to original calibrations, and come out with new qualified DTN’s, that was done, but there were, we know of limitations such as one calibration equation is applied for the probe being dropped through alluvium, and through the alluvium/bedrock interface, and into tuff.

So, that’s a limitation of the data. It’s been suggested that when the boreholes are drilled, they disturb the surrounding bedrock, and, so, you know the rest of the story, yes, so maybe you’ve created preferential pathways through the rock by doing that. I mean, we definitely looked very hard at this data set, to compare model results to data results, and we actually give a figure comparing the two in the AMR. But, it’s not a real clean one to one figure.

HORNBERGER: Other questions? Anyone?

Okay, I have several. So, another thing that you said that you found, that the data, the conductivity data, you couldn’t find--the data weren’t sufficient to reproduce the values in the tables?

STEIN: Yes.

HORNBERGER: So, that’s why you had to go to your pedotransfer function?

STEIN: I don’t know. Ken, do you want to--you are more familiar with the details of those issues.

RAYFELD: My name is Ken Rayfeld. I’m from Los Alamos. I was part of the team that was assessing the data sets that were originally used in the INFIL model, and what we did is we took all of those existing DTN’s and we had a team of QA specialists essentially who went through those DTN’s with respect to compliance with the procedures, with respect to traceability and transparency, and that’s documented in I think it’s CR 6334. And, through that process, we identified data sets that we thought we could use and keep as qualified data sets that had some form of deficiency, and it was at that point that the team that we were using could not take that information out of the DTN’s and then go forward with it to reproduce what was in the original report. And, then, we chose to use another approach that we thought we could, that we could take through the whole process, from the beginning until the end, and trace everything that we had done.

HORNBERGER: I mean, this seems to me on the surface that there really then were some significant problems with your whole QA review. I didn’t--Gene’s presentation to me didn’t jive with what I just heard.

RUNKLE: What is being referred to here is part of the validation and verification process of the data sets, and that was handled under another CR than what I was discussing. Okay, that was handled under--

RAYFELD: We started out at 6334, I think.

RUNKLE: Right. And, so, there are many of those types of things that are documented in that CR. The 5223 team that was looking at that root cause looked at the information that was coming out of here as part of their extent of condition, and so on. So, that's where you might be disconnected, but there is a process that was used to look at data sets. Jerry Westerman (phonetic) is the lead for that process.

HORNBERGER: I mean, the concern is obviously that, to over-simplify it unfairly, we're saying the model is found to be good, but the data used to run the model are not.

RUNKLE: Well, I mean, I think it might be better characterized as saying that some of the data sets needed to be, or the quality assurance and the pedigree behind them may not have been where they needed to be, and, so, they were going back and redoing all that to make sure that absolutely everything had a full pedigree and that it could be brought forward into the Sandia work without question of traceability and transparency. And, so, with doing that, there were things that may have been questioned or they couldn't be reproduced or they couldn't come to the same bottom line, and, so, that was the process that was used.

HORNBERGER: But, the anticipation might be that in the future, the neutron log data and the ring infiltrometer data, and all the other data, could be qualified and brought forward to refine?

RUNKLE: We have most of it brought forward at this point in time.

HORNBERGER: You do?

RUNKLE: Yes, I mean, that was needed to do the work with Sandia.

HORNBERGER: So, do your pedotransfer function, hydraulic conductivities, jive with the measurements that were actually done at the site?

STEIN: There are significant differences, but there's a lot of uncertainty. I mean, so, I guess no.

HORNBERGER: Okay.

STEIN: But, in our sensitivity analysis, those moved to a lower priority because they weren't sensitive, the infiltration results were not sensitive to those conductivities.

HORNBERGER: Fair enough. I'm just trying to get a handle on this. Another thing, of course, that you mentioned that does make a difference is you said that you don't have any transpiration from the bedrock. And, it struck me when you were saying this, but I saw sort of an inconsistency in your whole approach, and you just said oh, we know that there's something going on in the bedrock, we just don't know how much. Now, in the rest of your approach, if there's uncertainty to be had, you build it into the model, and then you evaluate the uncertainty. Here, you didn't seem to do that. You just said zero.

STEIN: Here, we didn't identify a basis to quantify it. So, I mean, I think with further work, you might be able to go in and do that. But, we didn't.

HORNBERGER: Okay. I know you didn't. I'm just trying to understand what the philosophy is.

STEIN: A lot of it was a time and resource issue. We had a deadline.

GARRICK: It seems to me, though, that when you have a situation like that, and we have seen that on several presentations, that you somehow ought to account for that as a contributor to uncertainty in the bottom line results. You know, it's sort of like assuming that the solubility of neptunium is a constant, and, therefore, there's no uncertainty. And, assumptions do not take away the uncertainty. So, that is kind of a modeling anomaly that we've seen several times in these presentations.

STEIN: I guess I attribute it to model uncertainty. I mean, you know, with the absence of an actual mechanism, you know, what's the physics behind it, and how do we parameterize it. With the absence of that, it's, you know, it's a model uncertainty, and model uncertainties are difficult to quantify because you need to--how do you quantify them? You create a new model and you create multiple models, that's typically the approach.

HORNBERGER: Yeah, well, I mean, I suppose I could argue that you have a plant removal function in your third soil layer, and why couldn't you have a plant removal function for the top two meters of the bedrock. But, never mind, I understand your answer. One more--so, again, I understand all the issues with calibration and why you don't want to do it, or maybe why you don't want to use some of the data.

On a larger scale, my recollection is that if you look at the chloride concentration in the groundwater, at some level, you have to believe a chloride mass balance, at some level, and I think the suggestion is that the groundwater gives you something on the order of 10 to 15 millimeters per year recharge. And, on the face of it, that might sound good to you, except that it's thought to have recharged in the late Pleistocene, which isn't very good for you. How do you make your estimate of 13 jibe with that?

STEIN: I guess the way I think of it is that we have focused on the surface data available at the surface, and characterized that uncertainty. I don't claim to say that that net infiltration pattern or amounts absolutely have to be recharge. I mean, there's a lot of distance in the vadose zone. There's a lot of potential processes that could occur.

HORNBERGER: Okay, fair enough.

STEIN: Are you familiar with like some of the--there's some data about secondary calcite, you know, that suggests maybe there are water removal mechanisms. So, it’s a complicated system.

HORNBERGER: It sure is. David?

DIODATO: Thanks. Dave Diodato, Staff.

I’m going to follow up on some of the data questions, and then get back to some of the statistics questions. I appreciate your presentation, Josh.

On the bedrock, George already asked about the soil hydraulic conductivity, but on the bedrock hydraulic conductivity, you chose to add these 200 micron apertures because of your--

STEIN: As an upper bound.

DIODATO: As an upper bound, based on a belief that you have that most of the fractures would not be filled.

STEIN: It’s not based on a belief. It’s based on a limitation of the underlying observation that all fractures are filled. I mean, nobody went in there with a microscope and did an actual thorough study. You look at a photograph and you say all the fractures are filled. From, I mean, experiences in other environments, volcanic environments, you know, it’s the exceptions to those that where all the water is flowing. So, I just feel like that there’s enough evidence in the fracture flow literature that it’s the very small fraction of the focused channelized pathways that really will drive that flow. So, I feel like you can't justify not considering the possibility.

DIODATO: Have to add the fractures in on that justification. So, then, is that consistent with the rock properties in the UZ model? I mean, you've got Slide 17 showing all the different--showing the bedrock hydraulic conductivity. So, the UZ model, you've got these--the line kind of would suggest they're connected, but they're not really connected. They are discrete units that don't have any relation lined out. So, are the numbers that you use for these different units with the aperture, with the hydraulic aperture of the added fractures, is that consistent with what's going to be used in the UZ model then?

STEIN: I guess I would--I haven't looked at that explicitly, but because I believe the UZ model doesn't include a significant portion of fracture filling--Jim could address that. I would assume that the UZ permeabilities or conductivities are higher.

HOUSEWORTH: Yeah, the UX permeabilities are not built on any assumption in terms of fracture filling or not. They're built on borehole air permeability measurements and subsequent calibration, and I'll be discussing some of that in the next talk.

DIODATO: Excellent. Okay, I appreciate that. So, we'll hold off on the discussion of the rock permeability.

But, it gets us back to the one data point. In discussion of neutron logging, having done it myself, I mean, typically, I'm very comfortable doing logging through multiple stratographic units with a single calibration at the beginning of the log and at the end of the log, and you know what units, and you do the correction according to that measured standard. So, that's not really something that I would view as a major limitation for doing neutron logging across multiple units. You know the geology, and maybe do some other logging associated with that.

Getting back to the statistics of your infiltration results, I'd like to look at Slide 19 again, and this has the MASSIF net infiltration results. So, let's look at the 90th percentile for present day, 26.8 millimeters per year. And, I apologize for not quite understanding how the precipitation works still. We don't have the slide up now. It's a different presentation. But, recall that the representative years had 40 1,000 year simulations, and you took the average of those, or you had two replicates of 20 each.

And, George has said this is a histogram, really, can be viewed as a histogram of the observations of the magnitude of precipitation in your simulations of precipitation. So, then, if I add up the bins from the representative year ten, all the way up to representative year six, that's like 90 percent right there, or 90 percent on the weight. Is that 90^{th} percentile also on the observations? Do 90 percent of the observations, are they at or less than 227 millimeters per year? Is that how I can read that, or could I not read it as--this is for Slide 15 from the precipitation. And, so, how would that number relate to the 90^{th} percentile?

STEIN: I guess I haven't done the exact analysis that you--I'd have to--I mean, the data is available to do it. I mean, in the post-processing of the model results, we have the precipitation for each year. We have the infiltration for each year. Everything is--we just haven't done that analysis. I guess I'm not sure I--I don't exactly understand what you're getting at, what observation?

HORNBERGER: I think what Dave is asking is perhaps we're misinterpreting the link between your precip talk and your ET, your infiltration talk. So, in your precip talk, if we understood it correctly, you're using this table where you use ten representative years with the weights.

STEIN: Uh-huh.

HORNBERGER: They're the ones that carry forward through your infiltration calculation.

STEIN: Right. So, for a given realization, we run ten separate years, one year simulations, it's based on the water year, so October to the end of September, and we run the first year, we get an infiltration map. We run the second year, we get an infiltration map. We take, at each cell location, we have ten infiltration values, and we calculate a weighted mean infiltration. And, that's what we're reporting, you know, when we show the--

HORNBERGER: So, there is a link between this table of ten representative values, and your infiltration map.

STEIN: Yes.

HORNBERGER: So, then, if we go back to your precipitation table, half a dozen of your things have equal weight of .18, or five of them.

STEIN: Yes.

HORNBERGER: Which takes you all the way up to--

STEIN: 90 percent.

HORNBERGER: 90th percentile, and that corresponds with a precip of 227 or less. So, 90 percent of your precip values fall below 227?

STEIN: You know, can I look at--I have a--

HORNBERGER: Go ahead.

STEIN: I have a CDF of the precipitation for each climate in the report. You have seen a--this is Draft B, it's on Page 207, there's a CDF of mean annual precipitation versus--or probabilities, and so if you go up to 90 percent, it's upwards of--what did you say, 227?

HORNBERGER: 227.

STEIN: That looks right.

DIODATO: Okay. Thank you. That’s very helpful to clear that up. So, we would just put this plot into the record.

STEIN: I mean, one of the things to keep in mind with that table is, you know, although we are calculating for those very high years, it’s not--yeah.

HORNBERGER: Gene?

RUNKLE: George, if I could come back to the comment where you said that my presentation was not consistent with this other one, there’s a subtlety here and I would like to clarify it because in the AMR issues, I said that there were multiple issues found after the AMR had been accepted. That is where I said 35 CRs were created, one of them having 100 issues, if you recall that statement? It’s not in the--that’s what I was referring to, is this stuff over here with the data and the other processes. And, so, that’s where it was said maybe not with the clarity that you’ve heard--but, that’s where it was included.

HORNBERGER: Thanks. David?

DIODATO: And, I have to ask one follow-up question of Gene that was prompted by Atef, and he asked me to ask this question. This was related to the release of INFIL 2.2, but I think you might remember back in May, we talked about the value to the scientific community of the utility of this kind of tool, and when I asked you at that time at the meeting if you would, subsequent to your work on it, just release it to the public, and you seemed to suggest that that would be not a problem. Is that still your intention to do that? I mean, Atef asked about this, and, so, we wanted to kind of follow up on that. Back in May of ’06, you seemed like that wasn’t going to be a problem, but I don’t know if that’s still your position, so I just want to be clear.

RUNKLE: Are you referring to the work that USGS is doing?

DIODATO: No, this would be the Idaho National Engineering Laboratory product with the DOE interface, the cleaned up version, because that’s what we were talking about, whether that would be available for the public or not. That’s what we asked about back in May.

RUNKLE: I don’t recall.

HORNBERGER: David, what you presented, is that publicly available?

LEVITT: Not right now, it’s not.

HORNBERGER: Okay. Are there plans to make it publicly available?

LEVITT: Not that I know of.

DIODATO: Okay, so that’s changed since last year. All right, I just wanted to ask that for Atef.

RUNKLE: We can check on that to see what the position is.

HORNBERGER: Well, thanks very much. I’m sure there are more questions, but we’ll let you off the hook for now.

I’m going to take the Chair of the meeting prerogative and declare a five minute stretch break.

(Whereupon, a brief recess was taken.)

HORNBERGER: All right, we are going to reconvene, and the final scheduled presentation for today is Jim Houseworth, who is going to tell us a little bit about the connection with the UZ model.

HOUSEWORTH: The UZ model is the principal user of the infiltration information, and, so, I will be going over some of the effects of the new infiltration results on the UZ model.

Okay, going over the outline, I’ll begin with an overview, just a couple of slides that discuss the unsaturated zone flow model, describing the geological characteristics and hydrological processes that are in the model.

Then, I’ll give just a one slide, very brief summary of comparison of new infiltration results and the old infiltration model, kind of couched in a format that’s applicable to the UZ model.

Then, I will go through several data sets that have been used to develop the UZ model, and looking in particular at model sensitivities, using the infiltration model, and the old infiltration maps, trying to define which parameters are sensitive to infiltration and which aren't.

Then, we will, with that background, go over some of the preliminary results from the UZ model, using the new infiltration maps for present day climate. And, that will lead us into a discussion of our efforts to integrate the UZ model with the infiltration model.

Next? The figures shown here give the picture of the UZ modeling domain, and the geological characteristics that are contained in that domain. The figure on the right shows the model footprint. It's about a 40 square kilometer footprint, and that's roughly a third the size of the infiltration model. It also shows the incorporation of discrete faults, the major faults in the model, which are labeled on that figure.

The repository domain lies within the center, roughly, of this domain, and it has a footprint of about six square kilometers. The cross-sectional diagram gives a little bit more detail and the geology that's included in the model, and this shows some of the structural features that are incorporated, the faults, the fault offsets, the stratigraphic dip that is represented in the grid. And, the model has, on average, about 59 computational layers to represent the 30 stratigraphic units, and each of those unites carries its own set of hydrologic properties.

Zeolitic alteration is also included. And, in fact, this alteration in some layers is, the variations within the layer are also captured within the grid because of the hydrologic and mineralogic significance of that alteration.

In addition to the flow model, I'm going to be discussing a few other models that support the low model, namely the chloride model, which uses this same grid, also a pneumatic and temperature model that use a similar 3D grid, have a slightly smaller footprint, and somewhat coarser girding, and I'll also be discussing a calcite model, which is a 1D model, has a grid that represents geology along Borehole 1WT24, which is just north of the waste emplacement area.

Next? The UZ model, flow model, computes steady state flow fields over the three dimensional domain, and it uses the spatially variable infiltration rate that's computed from the infiltration model as the boundary condition at the ground surface. The model represents flow in fractured rock using a dual permeability approach, which explicitly accounts for flow in the fractures and flow in the matrix, and inner-flow between those continuum.

And, in the table below the figure, you will see that there's variations in the percent of flux that's carried in the fractures and matrix, and that varies in the different layers. And, that reflects changes in properties of those layers. The model also includes small scale focusing at the sub-grid level through the use of an active fracture model, which limits the population of flowing fractures to a subset of the total fracture population.

Lateral flow is included in the model through incorporation of capillary and permeability barriers. The main capillary barrier are in the Paintbrush, non-welded unit, above the repository horizon. And, the main permeability barriers lie at or near the Topopah Spring/Calico Hills zeolitic interface below the repository. The permeability barriers are the main features responsible for the formation of perched water that has been observed in those regions, and, it also leads to a substantial amount of flow focusing in the faults in the model results. And, again, if you look in the table, you can see below the repository. We have a substantial pickup in the amount of flow that’s moved into the faults.

Actually, I had one other thing to say. I wanted to also mention that the processes, these hydrologic processes are what’s involved in the flow model. When we go through the chloride model, we’ll also be talking about advective and diffusive mass transfer processes that are represented.

In the thermal model, we have phased behavior and advective and diffusive heat transfer processes that are represented. And, the calcite model, the most process rich model, we have--it incorporates all of those, as well as geochemical reactions.

Next? The new infiltration model has resulted in a change in the range and distribution of infiltration and precipitation. And, the figure in the upper right shows the probability distribution for infiltration rate for the old and the new models, averaged over the repository domain. And, I will be talking primarily about repository domain averages in this talk.

And, as you can see, there's a greater infiltration, as we've heard before, in the new infiltration model, and over the repository domain, this was about, on the average, a factor of three higher. And, from this figure, I would also like to define some terminology. The three points there for the old infiltration model, I'll be referring to its present day lower, present day middle, and present day upper. And, then, these four points, which are points out of that distribution of 40 realizations that Josh spoke of, these are the present day 10^{th} percentile, present day 30^{th}, present day 50^{th}, and then 90^{th} percentile.

I'd also like to state at this time, and we'll come back to this later, is that these percentages in the past have been essentially what we have used to weight the UZ flow fields for sampling in TSPA. So, the infiltration distribution also represents a flow field weighting distribution that's sampled in the TSPA. And, that's what we did previously with these points from the old infiltration model.

In the lower figure, we plot the evaporative concentration as a function of infiltration rate. Well, evaporative concentration is kind of the net arrival of water at the ground surface. It's the precipitation, plus run-on, minus runoff, and then divided by the net infiltration. And, as you can see from the figure, there's been a decrease in the evaporative concentration in the new model as compared with the old model, and that reduction is roughly a factor of six on average.

The significance of this is that the evaporative concentration times the surficial water chloride content, gives the chloride content of the infiltrating water. So, in terms of the UZ model, it's primarily important for the chloride model.

Next? So, for the next few slides, I'll be going over the data that has been used to develop the UZ model, and I will be talking about model sensitivities to infiltration for each of these data types.

On this slide, we're talking about water saturation and water potential data. This data has been used to calibrate rock properties in the unsaturated zone flow model. These calibrations have been carried out in a series of two steps. There’s 1D calibrations, which use data from 16 boreholes, and then an automated inversion methodology, which simultaneously optimizes the calibration for the data from those boreholes.

The parameters that have been calibrated in the 1D calibrations are the matrix permeability, fracture and matrix capillary strength, the van Genuchten alpha, and an active fracture parameter.

Then, the 3D calibrations are carried out to account for effects of lateral flow and perched water, which cannot be captured in the 1D calibrations. But, in the figures, what we show are the 3D results after calibration, so we have water saturation against depth and log of water potential against depth from SZ 12. So, on these figures, we have data points as well as model calculated results.

And, let’s look first at the water saturations. What we see is we’ve made calculations for three infiltration cases, and those are listed down here on the table at the bottom. There’s present day middle case has an average infiltration rate of 3.9 millimeters per year, and we go through up to the glacial transition middle case of 17.6 millimeters per year. For that range, we see very little variation in the predicted water saturations.

And, similarly, we ran actually there’s nine different model calculations here, which has an even greater range of infiltration rate, and there’s, again, not much sensitivity to these matrix water saturation, water potential properties.

So, the conclusion from this is that there’s really very low sensitivity to changes in percolation flux in the matrix water saturation and water potential.

Next? We also used pneumatic pressure data, and this data is used as part of the calibration of the fracture permeabilities in the model. Again, we go through a two step process of 1D and 3D calibrations. The calibrations are based on the natural barometric pressure fluctuations at the ground surface, and the propagation of those pressure fluctuations into the unsaturated zone.

The plot on the right shows a 3D calibration calculation, along with data from SD-12, at various depth intervals in the borehole. The calculations were done using the present day middle infiltration map I referred to before.

And, in terms of the sensitivity of this kind of parameter to infiltration, if you look at the lower figure, we have effective hydraulic conductivity against water saturation for a fracture continuum in the TSw35. And, what this shows is that over the range of fracture, of flow rates that are consistent with what we think is going on at Yucca

Mountain, this goes from zero to over 40 millimeters a year, we get a range of water saturations of about 1 percent. That leads to very little, or small changes in the effective gas permeability, and therefore, very low sensitivity of any pressure fluctuations to this kind of process.

Next? Calcite model is used to check the long-term percolation rates in the repository host horizon. And, calcite is a secondary mineral that precipitates from infiltrating water. Its precipitation is primarily driven by the geothermal gradient, and the reduction in calcite solubility in water, with increasing temperature.

The figure here shown a total calcite abundance, as measured in Borehole WT-24 in terms of parts per million by volume, and stuff. And, 1D geochemical simulations are shown along with this data. These simulations incorporate heat transfer and geochemical reaction processes, and the calculations are done over a 10 million year period, representative of the history of Yucca Mountain. So, this is kind of a cumulative build-up of calcite over a very long period of time.

The findings from the model study is that over a range, a fairly wide range of infiltration rates, 2 to 20 millimeters per year, simulated abundances generally fall within the range of the observed values. But, the limited sensitivity here to infiltration rate and fairly poor time resolution relative to the interglacial period means this is not a data set that's particularly suited for looking at present day infiltration rates, at least.

Next slide? The temperature data has been used primarily as a check on infiltration rates, percolation rates really in the unsaturated zone. Temperature is dependent on the boundary conditions. The temperature at the ground surface and at the water table depends upon infiltration rates, and it also depends upon advective and diffusive heat transfer processes within the unsaturated zone.

What we have here on the right are four boreholes with temperature data that's been taken from these boreholes. And, we have model calculations along with those temperature data. The model calculations were conducted using the old present day middle infiltration map, an average rate of 3.9 millimeters per year.

But, I should point out that there are significant spatial variations locally in those infiltration rates, and that would be perhaps important to what's measured at the boreholes.

The sensitivity of temperature to infiltration is shown in this figure here. This is a 1D analytical result for temperature distribution in a homogeneous rock. And, what I've calculated here is the temperature profiles, over a range of flow rates from zero to 34 millimeters a year.

What you find is that at low flow rates, zero up to at least 4 millimeters a year, you get very little deflection of the temperature profile. But, at higher infiltration rates, you start to see more effects of the percolation on the temperature. This is a result of the dominance of the thermal diffusion process over advection in the low flow rates.

So, what we can take away from this is that in areas where the infiltration rates are sufficiently high, we can expect to see sensitivity to infiltration rates in the temperature profiles.

MR. HORNBERGER: How would your simulations for right-hand panels look if you used the new infiltration rates?

HOUSEWORTH: I will show that in two slides.

The UZ model uses chloride data similarly to check percolation rates in the unsaturated zone. Chloride concentrations in the unsaturated zone depend on chloride concentrations in the infiltrating water, infiltration rates and advective and diffusive mass transport processes within the unsaturated zone.

The plots on the right show chloride data taken from the ESF and the ECRB, as well as model calculations for a variety of infiltrate maps. Probably the feature that stands out the most is that for the present day low, you can see that the concentrations, or the predicted concentrations, tend to come up relatively high compared to the data. And, that's true also in the ECRB. These two lines are two alternative present day low models.

And, similarly, if you look at the table here, the infiltration rates that we use in the chloride calculation, as well as the evaporative concentrations that go along with those infiltration maps, you can see that the present day low has substantially higher evaporative concentration. So, that's driving a much higher concentration. What that's telling us is that the average level of chloride concentration in the saturated zone is sensitive to the evaporative concentration, which really sets the average concentration of chloride in the infiltrating water.

HORNBERGER: So, Jim, can you just--I'm not sure I understand the legend.

HOUSEWORTH: Oh, yes, okay. These have some different--this is present day, these three are present day, upper, mean and lower, it's a U, M, and L.

HORNBERGER: Upper, mean and lower.

HOUSEWORTH: And, then, this is the glacial transition mean, is also run for the ESF case.

HORNBERGER: Okay.

HOUSEWORTH: On this legend, we have present day, upper, mean and lower with two alternative cases, A and B, which I didn't really discuss in this talk, but there's an alternative model for the property sets that primarily changes in the PTn properties, in effect, the degree of lateral flow that occurs in the PTn. And, for Model A, you have more lateral flow in the PTn. For Model B, you have less lateral flow.

Next slide? So, with that background now, we'll take a look at some pretty early preliminary results of the UZ models using the new infiltration maps.

So, here we have, again, these same four boreholes that I showed earlier, with the temperature profile, only this time now, we're using the new infiltration maps, the 10th percentile, 30th percentile, 50th and 90th percentile. So, all four cases are plotted on here, along with the borehole measured temperature data.

These infiltration rates are shown in the table in the lower left, range from 4 to 34 millimeters a year. What we're seeing in the upper figures are not a great deal of sensitivity until maybe we get out to the 90th percentile. Certainly, the 10th, 30th and 50th percentile cases tend to lie right on top of each other. This is indicative of a low percolation flux zone. And, in fact, I happen to know NRG-6 has very low local percolation flux. And, when you have those kinds of conditions, you won't see much spreading of the temperature profile with changes in infiltration rate.

The bottom figures show a different story, where you get a much more significant movement of the temperature profile with the change in infiltration rates. And, in these cases, what we see is where we have sensitivity, where we have sufficient percolation flux to get this sensitivity, the predictions of temperature using the 50^{th} percentile filtration map tend to run below the data, tend to be cool, and that the 10^{th} percentile case provides a better match to the data.

Next? So, we're now going back over the same ground here with the new infiltration maps for the chloride, which were one of the data sets we said would be expected to be sensitive to changes in the infiltration result.

Again, we have the ESF data and the ECRB data, now with the model plotted using the new infiltration, same set of infiltration maps as before. And, generally, what we find is that the 50^{th} percentile case, which is the dotted gold line there, and kind of a thin gold line down here in the ECRB, tend to fall below most of the data. In general, it doesn't match up too well with the observed data.

However, the 10^{th} percentile case does a pretty good job in the ECRB, tends to run a little bit high in the ESF. And, this is consistent again with the evaporative concentration, where now we're down at present day to a level of around 42, which is pretty close to the present day middle case from the old infiltration map. It then drops down quite a bit for the 30^{th}, 50^{th} and 90^{th} percentile. That drops your average chloride concentrations pretty much across the board.

The reductions that we find in the chloride concentration appear to be more universal here and in other cases that we’ve run than for temperature, and that’s because we don’t lose sensitivity in the chloride case at low infiltration rates, like we do in the temperature case.

Next? So, recapping, what we have found is that the UZ model predictions for temperature and chloride, using the present day median case from the new infiltration model, tends to deviate somewhat from the temperature and chloride observations in the unsaturated zone.

So, in an effort to try to integrate and align these models, what we want to do and what we are pursuing is using the, again, the probabilities as generated from the new infiltration model, in combination with the residuals between the calculated temperature and chloride values, and the observed values. And, the mechanism for pulling all that together is using this Generalized Likelihood Uncertainty Estimate methodology, and it goes something like this in the way we are trying to do this.

You could say determine the prior weights. We, the prior weights are the weights as determined by the infiltration model. Then, there are likelihood values based on the chloride and temperature data and the UZ model predictions.

So, we have residuals, and we use a likelihood function, which gives a greater magnitude value for lower residuals, tending to weight calculations that produce values closer to the observations. It gives those greater weight. Then, we calculate the final weighting factors using a relationship shown there.

So, now we’re coming back to the same plot I showed at the beginning of the talk, these are the same points and as I mentioned, this is not only an infiltration probability of distribution, but a distribution for sampling of the unsaturated zone flow models that use these infiltration rates in the TSPA.

So, we have, again, the old infiltration distribution, the new infiltration distribution, and then this is a very preliminary output in terms of a weighted adjusted value using this GLUE method that accounts for both infiltration probability, as well as the observations and calculations from the unsaturated zone for chloride and temperature.

Next slide? So, to summarize, first of all, UZ flow model is a 3D mountain-scale process model that is calibrated and validated against a number of data sets, which I have attempted to present here in this talk. The results that we found primarily in terms of sensitivities are that water saturation, water potential, and pneumatic pressure have very low sensitivity to infiltration flux.

Calcite deposition has limited sensitivity to infiltration rate, but really poor present day climate, has poor time resolution. Temperature profiles along boreholes are sensitive to infiltration at locations with sufficiently high infiltration rates, but not sensitive in the low infiltration environment. Average chloride concentrations are sensitive to the infiltration output, and in particular, the average values get shifted in terms of the changes in the evaporative concentration, which is a function of the infiltration and the precipitation.

The preliminary UZ model results for temperature and chloride, using the median new infiltration rates for present day climate, tend to deviate from the observation in terms of temperature and chloride.

The UZ flow and infiltration models are going to be integrated using the prior uncertainty information from the infiltration model, plus the residuals between UZ model predictions and UZ observations for temperature and chloride, using GLUE methodology to develop an adjusted weighting factor distribution for sampling the flow fields in TSPA.

We believe that the integration of the US flow and infiltration models through these weighting factors provides an improved treatment of uncertainty while maintaining consistency with the unsaturated zone observations.

I'll take questions at this time.

HORNBERGER: Thanks, Jim. Questions? Thure?

CERLING: I'd like to go to Slide 11. I'm just really trying to understand what's going on here, because all of these have, you model it with different infiltration rates, but it's only the lower two panels that show the sensitivity. So, is that basically a difference in the thermal conductivity of the upper two versus the lower two?

HOUSEWORTH: No, I don't think there's a great deal of difference in the geology and the thermal conductivity at these boreholes that would be driving this difference. It's basically that when you have a low infiltration rate, like let's say NRG-6, and we say our infiltration rate is may be millimeter a year, or less there. It's just a low coolant environment, has a low rate. The advective heat transport is very weak, and, so, you double that, and you go to 2 millimeters a year, you still don't see anything because it's still dominated by thermal diffusion. You have to really bump it up a long way to start to see any deflection of the temperature profile.

Down at UZ 7a and SD-12, where it's starting, you know, at the 10th percentile, it's several millimeters a year, well above the 4 millimeters a year here, that I showed on the other slide where you started to see some sensitivity, so it’s the local variations in the percolation flux, and they do vary considerably.

CERLING: Well, I guess the way I was interpreting these slides is that all of these, according to the modeling, had different percolation fluxes, but the upper two, the high percolation flux and the low percolation flux don’t seem to make any difference in the temperature profile.

HOUSEWORTH: Okay. Well, here, maybe I can clarify this. This is a 3D model, okay? And, we have spatially variable infiltration and percolation, and these boreholes are at different locations in the domain. And, so, locally, you’re not in the same percolation flux environment. PD10, the 10^{th} percentile, you might be at 5 millimeters a year at SD-12, and .5 millimeters a year at NRG-6.

CERLING: But, all your models, your 10^{th} percentile, 30^{th} percentile, presumably, those refer to these infiltration fluxes, or percolation flux; right?

HOUSEWORTH: They’re the infiltration maps. Josh showed they computed 40 realizations. The 10^{th} percentile, if you lined them up in their rank order and give them a percentile, the 10^{th} percentile is the--10 percent of the way up that distribution. The 90^{th} percentile is close to his--coming up close to his 40^{th}. This is 36 realizations.

CERLING: Yes, but the 4 millimeters per year, for example, is the mean infiltration over the whole footprint.

But, that has to be spatially distributed.

HOUSEWORTH: Right.

CERLING: So each of these is--the 10th percentile is spatially distributed.

HOUSEWORTH: Right.

CERLING: Okay. I get it now.

HORNBERG: Other questions. John?

GARRICK: I just want to--what's your prognosis as to--I know you're not the TSPA guy, but what's your prognosis as to what this is going to do to the performance assessment, with respect to the performance of the UZ?

HOUSEWORTH: Well, I mean, the increase in the infiltration rates is fairly large, and it would probably lead to more seepage, more rapid radionuclide transport, but, I mean, I think with our adjustments of the sampling frequencies of these things, we may not see quite as strong of an effect.

GARRICK: So, one possible characterization is that with the increased infiltration, it's actually less of a barrier, but there's less uncertainty about that? You say one of the outcomes of this model is reduction--is a better treatment of the uncertainty.

HOUSEWORTH: Yes, I think that what I'm really saying is is a broader look at uncertainty as done through the MASSIF model. I think that that model put a lot more emphasis on uncertainty than the previous model, and, therefore, has perhaps a broader range, a little bit better representation of uncertainty than what we've had in the past.

By utilizing that, and carrying forward with the adjusting weighting factors, as I'm suggesting, should help allow us to incorporate that wider range of uncertainty, and not deviate too much from what we believe are the results based on chloride and temperature.

GARRICK: But, without getting into the chemistry, there isn't much we can--we don't see much good news here, as far as the performance.

HOUSEWORTH: Performance?

GARRICK: Of the UZ.

HOUSEWORTH: No, I wouldn't say that this is going to be good news for UZ performance necessarily, no, not this.

HORNBERGER: I can understand why you are proposing to use the GLUE procedure. On the other hand, I would think that a skeptic might see this as in one of the categories of "lies, damned lies, or statistics," and I'm not sure which. We might want to put it in statistics because it's a statistical adjustment. But, it can look a little funny because we go to all of this trouble to develop an infiltration model, and to produce these estimates of net infiltration, and then we come back and say, well, but these don't tie in with our data, so we have to adjust them, and lo and behold, we adjust them back to a model that we calibrated for the site.

HOUSEWORTH: I don’t really find that unusual at all. And, let me remark as to why.

The infiltration model was developed with as much information as they had available to them in this very narrow zone at the ground surface. But, they did not delve and use any of this information that we’re pulling in now. We’re pulling in entire new data sets. That model had no benefit of that information. And, these data sets are maybe some of the best ways we have for estimating for glacial flux in the unsaturated zone, and, thereby, infiltration in an average sense. It doesn’t perhaps help nearly as much in terms of the spatial distribution, but when you pull in this information, I think that it’s to be expected that you may be adjusting some things, because it’s new information, and there’s a lot of uncertainty in the infiltration model. So, that’s where we’re ending up.

GARRICK: Just to be argumentative a little bit, if one were doing this project as a totally integrated project, do you think that this would be the best way to do it, or do you think that one might calibrate on, let’s say, soil depth?

HOUSEWORTH: Well, I guess if you were talking about an idealized world, what I would recommend is an integrated infiltration and unsaturated zone flow model. And, that way, we can integrate these effects in the unsaturated zone, which are key for us bounding our estimates of infiltration directly into the model. I mean, we kind of cut up these things somewhat arbitrarily as a matter of convenience. The system is broken into all these different pieces, and sometimes when you break them up, you lose on some of the integration--that you could gain from if the model was--

HORNBERGER: David? Anyone else?

(No response.)

HORNBERGER: Great, thank you very much, Jim.

We have requests from three people who wish to make comments during our public comments period. First on my list here is Judy Treichel.

GARRICK: Judy, you can come up and use the podium if you--

TREICHEL: Oh, no, no, this is just fine.

Judy Treichel, Nevada Nuclear Waste Task Force. I just made a couple of notes and it’s going to sound probably like the same thing I say all the time. But I was wondering how much of the new climate data that’s come out of the international committee has been looked at and incorporated in? Because much of it goes away from the sort of waves that it showed on old DOE maps when they would show the glacial, inter-glacial, monsoon, those sorts of things, and for, I don’t know, hundreds of thousands of years, you would see these up and down kind of evenly rolling things. And with some of the new data coming out of the new studies, they are showing that if a lot of the climate change is man made, then you're not going to have these normal cycles the way they have been, and the predictions given for the southwest are actually drier, hotter, more fires, and the precipitation would be far more drastic and you might get less rain, but you might get it in maybe two events. So, you'd have huge events, and those might be very different if you had burned off a lot of the vegetation. So, I just wondered if any of that had been considered?

So, it's likely that at least with the new data that's coming in from the international community, that the past doesn't predict the future. If it did before, they find that it does less now.

And, I always get upset about the weighting factor, whether it's in doses or no matter what it's used for, but once you come up with weighting and averages, or weighted mean, which is a weighted average, you have a double mask on something. And, when you're talking about what happens with Yucca Mountain, you're going to have to use some realistic stuff rather than averaging and weighting and GLUEing and whatever else is going on. But, even when you call it net, it's still another average.

So, that's it, thank you.

HORNBERGER: Thank you, Judy. Tom Buqo (phonetic)?

BUQO: My name is Tom Buqo, I'm a hydrogeologist, consultant to Nye County. The comments I'm going to make do not necessarily reflect the views, opinions or stated policies of either the Nye County Board of County Commissioners, nor the Nye County Nuclear Waste Repository Office.

I'd like to touch on five topics. The significance of recharge to Nye County, the e-mails, the water rights hearing that Nye County was involved in last year, an AGU book that we're aware of, and then the topic of corroboration.

We don't look on it as infiltration. Nye County looks at it as recharge, and we are desperate for every drop we get. Every millimeter of water that falls over Amargosa Desert translates to 1,600 plus acre feet a year, because there's over half a million acres there. So, we've been following this work very closely, because the more recharge, the better we like it, because the more water resources we have.

I sat at a meeting, I want to say it was in 1985, when a gentleman from one of the National Labs got up and stated with a certain Markovian certainty that it was going to take 80,000 years for that molecule of water to get from the top of Yucca Mountain to the saturated zone. Well, things have changed, and we now realize that’s probably not the case. So, with great interest, we have watched as the work has continued.

Moving onto the e-mails, I think this document is really a good document. It was good to see it. We had seen most of the e-mails before, and our immediate reaction when they came out was just go, but three of them in particular kind of give me pause. One is the one on Page A-9, where they state, “We need a product or we’re screwed and we’ll take the blame.” It tells me it’s very product oriented. They had to get a product out.

The one on Page A-12, “They’re going to continue their regional model even if it ignores direct orders from YMP management. Get a project out and the Death Valley regional model fits the bill.”

And, then, the last one, which I think is the one that really gives me the most pause. “These guys are trying to put bandaids on a road kill. They don’t get it. The more they start digging, the more dangerous it starts to get. There are many skeletons in the closet.”

One of my roles in working for Nye County and oversight is to dig. So, we’re always looking at the work that’s being done. We’re always digging into it. So, a statement like that really kind of concerns me.

Moving onto the water rights hearing last year in

June, this is where things kind of came to a head, because we were hit with the results, they took that regional flow system model, and they turned around and used it against us in our water rights hearing to try to use it as a transient model to forecast the impacts of groundwater development in Amargosa Desert.

According to the results that they presented in that water rights hearing, the Devil's Hole pupfish ceased to exist in about 1976 because the water table was lowered so far below the breeding shelf, that it would no longer be able to be viable, and the water level never rose again to current levels. In fact, they show a significant drop later.

The point is they're trying to use a model for something that it's not intended to be used for. We really got into digging into that model, because into that model because we had to be able to present an argument when we went to the water rights hearing. So, I'm probably one of the few people that sat down and read every chapter, and put it all on the regional closed system modeling report. We read the stuff about infiltration, the chapter. We're well aware of some problems with that, and we brought those out at the hearing.

But, there's a link between recharge and discharge, and we have consistently maintained that the estimates of discharge being used in that flow model are significantly under-estimating the amount of discharge by a factor of at least two.

Now, it's interesting hearing these folks come out today and say, well, when we look at it this way, our estimates are two to four times higher than the previous estimates. Well, let's apply that to the recharge. Does that apply to the Spring Mountains and the Sheep Mountains which feed our system? Is the recharge there two to four times higher? We certainly hope so. We've always thought it was at least doubled, and that's why we think that if you're going to go out digging and looking for skeletons, the discharge estimates that are used to balance this regional model are something that should be subjected to a high level of forensics.

Corroboration. You know, we try to keep an eye on the literature. I may live on the side of a hill in rural Nevada, but I belong to AGU, I get--so when I saw this book come out, Groundwater Recharge in Desert Environment in the Southwestern United States, I grabbed a copy.

And, in that, there's a paper by Flint, Flint and Hevesi, Fundamental Concepts of Recharge in the Desert Southwest and Regional Modeling Perspective. And, in that, they summarized their results. Let's use Amargosa Desert as an example. Amargosa Desert, the Maxey Eakin recharge predicted, or estimated 1,500 acre feet of recharge to

Amargosa Desert. Hevesi, et al, 2002, estimated over 8,000. Hevesi, et al., 2003, estimated a little over 2,000, a factor of four difference between the two years. Then in 2004, they come back with 2,000 again.

What happened between 2002 and 2003? I can only speculate. But, I speculate it works something like this. The Department of Energy went to a lot of trouble, time and effort, 15 years and $20 million, to develop the most sophisticated groundwater flow model I have ever seen. Then in 2002, someone comes along and says, oh, by the way, your recharge may be off by a factor of four. Well, when that happens, it has an incredible cost and schedule impact, and I think there was a reluctance to go in and take a look at it and say well, what happens if the recharge is significantly higher? We have to redo our regional model, we have to recalibrate it, and we have to take a look at it all from new, and I didn't see any sort of effort that that be done, and I see the next year, the same people come back with totally different results.

And, then, finally, on corroboration, there's a Table H in this report, which I think is kind of interesting. Table H goes in and list recharge estimates for West Texas, New Mexico, and Arizona. That's Table H-3. Table H-1 lists estimates of recharge for Nevada hydrographic areas. When I look at the basins that are of concern to Nye County, Mercury

Valley, Rock Valley, Buckboard Mesa, Forty Mile Wash, Crater Flat, Amargosa Desert, Pahrump Valley, everything that’s listed in Table H-1 comes from Maxey Eakin. Maxey Eakin is a very old estimate, dating back 40 or 50 years. That’s not corroboration.

When I look at the next table, H-3, recharge estimates in West Texas, New Mexico, and Arizona, that’s not corroboration, until I see in there the studies that have been done in the State of Nevada. I know for a fact that there have been chloride balance methods applied in the state of Nevada, yet as I look at this, I don’t see any reference to any of those studies.

And, I appreciate the time to express these comments. Thank you.

HORNBERGER: Thank you, Tom. Finally, I have Charles Fitzpatrick.

FITZPATRICK: It’s a tough act to follow. Charles Fitzpatrick, State of Nevada.

I guess I have one sort of inquiry for Josh Stein, and then a comment. The inquiry has to do with timing, because I think you made the point when you began that the MASSIF model is preliminary at this stage, and so your comments are to be taken that way. And, I think it was also mentioned that when it is complete, it’s going to be reviewed independently by Oak Ridge. I guess that could result in some refinements backed by you all.

It's a two-part question. The first question is when do you think that the model will be in final, final, final form? And, then, the second part of it is we recently heard that DOE is going to begin doing many of the--lots and lots of runs they have to do on the TSPA as soon as the end of March or April here, and do them over succeeding months. And, I'm wondering, depending on your answer to Part A, when will the MASSIF model be final? How can it be successfully incorporated into these many TSPA runs that are going to begin well before the model is finished?

STEIN: I can answer the first question. The issue of the Oak Ridge review--to either lead lab representative or DOE. But, the last question is we actually, this is in the draft report right now, we ran the model and got results back in I want to say October, it may have been earlier than that. I'd have to look. We got preliminary results. Thos preliminary results were fed to TSPA, or fed to the UZ flow model, and then propagated through.

After that point, we found a series of minor errors or inconsistencies in some of the data as a function of the review that was going on. We made those corrections. We submitted a CR on ourselves for basically, you know, to document that, and what we've done is actually, we're carrying forward both sets. The errors were in--essentially had very small effects on the actual predicted infiltrations. There were minor rounding errors, and that's an example of one of the data sets.

So, we've run both of them. We have records of both of them. We're going ahead and qualifying in the report the current results, and then we're going to do a data qualification effort basically by comparing the preliminary results of the run for the UZ model and show that they are essentially within the--for their intended use, the preliminary results are adequate.

FITZPATRICK: Forgetting Oak Ridge for the moment, you can't control it--

STEIN: We are going to be--we're going to try to resolve checking by end of next week. Then, we go, the procedure then goes for interdisciplinary review, and I guess I'd have to defer to management in terms of--I'd have to--I don't know exactly how long that will take.

NEWBURY: This is Claudia Newbury, DOE. Hi. We've done this before. This is a deliverable to DOE, so in terms of when, I'd have to check our schedules for when that particular document will be delivered to us. But, then, we will do a final review on it before it is a final document that's available. So, I can get back to you with a schedule.

FITZPATRICK: And, would it be incorporated in the TSPA sort of in preliminary form?

NEWBURY: The model results could be incorporated into the UZ model, which then feeds into the TSPA, so we can use those preliminary results and make sure that they’re valid when the model is complete.

FITZPATRICK: I’ll now turn to a comment. I’m concerned that what we may see over the next many months is preliminary, not yet final inputs being submitted for TSPA runs, which will then become preliminary TSPA/LA output, not available to the public because the TSPA/LA itself is preliminary, and the inputs are preliminary. And, on the eve of LA, we will suddenly have a dump of all the final inputs and the final TSPA/LA. That’s my concern.

The other comment was sort of a follow-up to Judy Treichel’s observation about she’s concerned about the use of means and averages and I mean, I just have a practical question about it in my own mind. Mr. Stein, Dr. Stein, showed us that by way of example perhaps the most precipitation you might anticipate over a thousand year period would be about 700 millimeters, you know, and all the others would be well under that, most of them under 200. But, once every thousand years, you’re going to get 700 millimeters, or more.

And, we also saw that net infiltration tracks, I mean, the most significant input to net infiltration is precipitation. So, my curiosity, and this is just a comment, not a question because you're not a corrosion guy, but, for instance, when it comes to corrosion, it is said that the most important factor in corrosion is when water gets to the tunnel, and how much water gets to the tunnel, and when.

So, from what you seem to be illustrating, and I understand it's preliminary, is that about once every 1,000 years, you're going to have a humongous precipitation year and a humongous infiltration year, and maybe that would be important to the corrosion--to the issue of corrosion, access of water to the waste containers, and initiation of corrosion. But, it may never be analyzed that way if the DOE corrosion experts use mean precipitation and mean infiltration.

In the real world, the mean may never happen. But, I mean, over a thousand years, I guess you'd say that's just what would happen. But, in a visual year within the thousand years, you may have sufficient precipitation and infiltration to initiate a lot of corrosion. And, I'm concerned if they use only the outputs of mean this and average that, that they won't come up with correct information on when corrosion will take place. And, that's just an observation.

HORNBERGER: Thank you, Charles. Scott?

TYLER: Scott Tyler from the University of Nevada, Reno.

Just two quick comments, if I may, to the Panel. First off, is I too was surprised to hear that there's some questions regarding some of the neutron data. That is, to me, some of the longest running data sets that the project has with respect to infiltration and recharge, and perhaps some of the most valuable data. So, I'm surprised that those data were not used in the analysis.

Secondly, the modeling from Sandia, my sense is that it shows, and Josh showed us, that the depth to bedrock was one of the most key factors. The storage in the soil zone, that is, the water holding capacity was perhaps second. And, then, the question of whether there are roots that are extracting water from the fractures, appears also to be a fairly important factor.

I think probably Alan Flint's INFIL model showed probably pretty much the same kind of sensitivity. I haven't read it, but I would assume so. And, to me, it seems like those are two, at least, of the most simple things that could be measured at the site, depth to bedrock, when the bedrock is between 10 and 30 or 50 centimeters below the land surface. My students and I will happily go out with a hammer and a stick if you pay us at the rate that's being paid, and we will do that work all over the mountain. And, I am serious about that. I am surprised that those data have not been collected at the frequency that they should be.

The effects of roots in the fractured rock, again, soil pits are not that difficult to dig with a backhoe.

These things can be measured, and why they haven't been measured, surprises me.

Thank you.

HORNBERGER: Claudia?

NEWBURY: Claudia Newbury, DOE.

Earlier today, there were a couple comments made about $12 billion being spent on this program. Total amount of money spent on this program to date is only $9 billion. So, there's a small difference there.

HORNBERGER: A billion here, a billion there.

NEWBURY: And, that's on everything, including a second repository, the transportation program, the waste acceptance program. So, it's not just on the repository itself. The amount spent on the repository is, of course, less than that, and that's over more than 20 years.

So, two points on that. One, yes, there's a lot of data that could have been collected and wasn't, and work could have been modeled and wasn't, but it's not because we didn't want to, but we have been restricted, and if Congress would appropriate the amount of money at $12 billion over the same time period, we probably could have done a lot more.

The second point is that, you know, science is science, and are there still questions? Of course there are questions. They are different questions than they were 20 years ago. But, every time you find out something new, there are more questions, and we certainly have a long-term testing program that we intend to have in place, and a performance confirmation program that will do a lot toward answering some of those questions, and no doubt will raise more.

And, then, I have one other thing to say, and that is the MASSIF model was out here in public for the first time today, and it's a new model, it's been developed in a very short time frame, with a limited amount of people, and money. And, I really appreciate their coming out here and talking to you about it. It will be controversial. There's a lot of work that needs to be done on it yet, but we appreciate the opportunity to talk about it.

Thanks.

HORNBERGER: Okay. Well, I want to thank all of the people who came here to speak to us. I thought that the presentations today were excellent. I think we learned a lot and I appreciate your willingness to put up with all of our questions and sometimes seemingly hostile behavior. It's only that we want answers to questions. That's all.

We do thank you, seriously, for coming. And, I think with that, I will turn it back to you, John, or should I just close the meeting?

GARRICK: Close the meeting.

HORNBERGER: The public meeting is hereby closed.

(5:23 p.m. - The meeting was adjourned.)

C E R T I F I C A T E

I certify that the foregoing is a correct transcript of the Panel on Postclosure Performance of the Nuclear Waste Technical Review Board held on March 14, 2007 in Berkeley, Californa taken from the electronic recording of proceedings in the above-entitled matter.

March 26, 2007

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