UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING BOARD MEETING
April 22, 1993

Holiday Inn
Reno, Nevada

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman
Nuclear Waste Technical Review Board
Dr. D. Warner North, Chairman, Morning Session
Dr. Patrick A. Domenico, Chairman, Afternoon Session
Dr. Clarence R. Allen, Member
Dr. Garry D. Brewer, Member
Dr. Edward J. Cording, Member
Dr. Donald Langmuir, Member
Dr. John J. McKetta, Member
Dr. Dennis L. Price, Member
Dr. Ellis D. Verink, Member

APPEARANCES

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Dennis Condie, Deputy Executive Director, NWTRB
Dr. Sherwood Chu, Senior Professional Staff, NWTRB
Dr. Leon Reiter, Senior Professional Staff, NWTRB
Dr. Carl DiBella, Senior Professional Staff, NWTRB
Dr. Robert Luce, Senior Professional Staff, NWTRB
Mr. Russell McFarland, Senior Professional Staff, NWTRB
Mr. Frank Randall, Assistant, External Affairs
Ms. Karyn Severson, Congressional Liaison
Ms. Helen Einersen, Executive Assistant

Ms. Linda Hiatt, Management Assistant

I N D E X

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INTEGRATION OF DATA AND MODELS

RESOLVING THE ISSUE

I N D E X (Continued)

P R O C E E D I N G S

Time: 8:00 a.m.

DR. PATRICK DOMENICO: This is a continuation of yesterday afternoon's session, but before we get started, Carl Gertz would like to give us a medical update for those that are interested.

MR. GERTZ: In case anyone might be wondering, Dennis Williams, my staff member who got ill yesterday during the presentation, spent the night in the hospital. They're doing some tests. It appears it might be a blood chemistry problem with him; he's had some history of some blood chemistry problems before. So he's doing really pretty good and we think he's going to be all right.

Thanks, Pat.

DR. DOMENICO: Give him our best.

MR. GERTZ: Sure.

DR. DOMENICO: Moving along in the program, our first presentation will be by Linda Lehman, and she will talk to us about the non-unique aspects of modeling.

MS. LEHMAN: Thank you, Dr. Domenico. Thank you very much for the opportunity to present State of Nevada Funded Research to the Board.

DR. DOMENICO: Thank you.

MS. LEHMAN: The State of Nevada has been participating in the INTRAVAL Project, which I believe Alan Flint mentioned briefly yesterday. The purpose of the INTRAVAL Project is to validate models that are used in assessing repository performance or those that represent physical processes.

Now, based on our experience within INTRAVAL, we believe that validation is actually an interative process of confidence building. And to my mind, it's very similar to the process that NRC uses in establishing their findings of reasonable assurance.

Today, one of the parameters under discussion is infiltration. This has proved to be quite an illusive parameter and one which we have found to be highly model dependent, the mathematical model or the conceptual model.

We have examined this parameter using a variety of conceptual models, a variety of mathematical models, and have come up with some confirmatory criteria against which you can test or gain confidence in your model.

The state has been participating in the unsaturated zone test case of INTRAVAL, and basically this test case has two parts. First is to calibrate your models against the water content data provided in shallow boreholes. That's UZN-53, 54 and 55. Then after you have done your calibration, then you perform a blind prediction on the water content profile through the Topopah Springs member of UZ-16, actually the entire column. And this was to be done blind by the modelers, in other words, we were not provided that water content data. In fact, we still haven't. So the predictions you will see here are totally blind. The location of the boreholes that were used was UZ-16, and 53, 54 and 55 are located in a wash called WT-2 wash.

Now, we undertook a number of modeling studies in order to examine the infiltration and the water content, and we used data that were primarily provided by the INTRAVAL working group. But we were also allowed to use any other data that were published on Yucca Mountain that were relative to our interests. And we were not constrained in any way with this modeling exercise. We were allowed to do anything we wanted in terms of initial conditions, boundary conditions, layering schemes, infiltration signals, so we had no constraints placed on us.

Obviously, we were interested to know what infiltration, what flux would best match the water content profiles. So before I go into the results, I want to show you the data that were available. I'm not going to dwell on this because Alan Flint presented most of this yesterday, number of both measured parameters and calculated parameters from the 53, 54, 55 boreholes, but also from three transects which Alan mentioned yesterday along Yucca Mountain and north of Yucca Mountain.

As I said, we looked at a variety of models, conceptual models, one dimensional and two dimensional matrix flow, fracture flow. We also examined different layering schemes, fracture/matrix interactions, evaporation, focused infiltration, which is the term I've used as opposed to concentrated infiltration that was used yesterday, it means the same, and also transient infiltration signals representing pluvial climates.

This is supposed to be easier when you have paper on them, but it doesn't appear to be. This is a schematic of our one dimensional matrix model. It has four layers. Unit 1 is the Tiva Canyon, Unit 2, the Pah Canyon, most porous unit here, 3 is the Topopah and 4 is the Calico Hills.

Now, before I present these results, I'd like to explain what these bounds, these error bars are. These are the actual water content data from Boreholes 53, 54 and 55. What we did was we took all of the measurements within each five meter section and constructed basically a piece-wise graph here which shows--we took the average and the standard deviation on those measurements, lumped them all together from all the boreholes for each five meter unit. And that's what these error bars are. They represent the range of the actual data from the shallow boreholes, and that is what we compared our modeling results against, our calibration results.

These runs are the one dimensional runs, and the problem that we had with one dimensional matrix flow was in this second unit. We were not able to get the water contents up to the midrange without overshooting our water contents in the unit above and below. So we had a great deal of difficulty, no matter what we did, and we tried a number of things to try to move that water content up, but we could not get a good match. We feel this is not a reasonable result. These are much too low. And I'm only presenting a few results, what I feel are our best results from the 1 and 2-D simulations and fracture flow results.

This is a schematic of the two dimensional matrix flow model. We decided we would try whatever we could to bring that second unit up in terms of water content. So we thought we would try allowing infiltration to come in from the side as well as from the top, so we had set our infiltration on the top boundary to be .1 millimeters per year, and then we tried two different scenarios for side infiltration. One is .1 millimeters per year, and the second run uses 1 millimeter per year.

Again we have the same problem, even worse here. The Topopah was wetting up too quickly and so was the upper units in the Tiva, but we still could not bring the water contents up in that wet unit. So we decided we would try a fracture flow model. We used basically the same layering as our one dimensional model, except we put a fracture along the side.

Now, this fracture is not a through-going fracture, and our basis for that was the work of Mike Chornack. He had done a lot of mapping of fractures and believed that this Pah Canyon member was not as fractured as the units above and below. In fact, there were very few fractures. So we decided we would just not put the fracture there, so the water now must move from the top fracture, through the porous unit and then back into the fracture beneath.

The fracture aperture was varied. Most of this is from Chornack's work, the spacing, the average of 200 microns and the fracture path width, an average of all the units to be .15 meters. Later on, we employed an evaporation term in the upper lawyers which seemed to help lower that water content in the upper units.

Then we have the problem of how much infiltration do we dump into the fracture, and so we decided we would look at how much infiltration was possible, how much could be focused into fractures. So first we looked at Solitario Canyon area and then we looked directly at the WT-2 wash.

We used a model called Depression Focused Recharge, which looks at this concentrating mechanism of recharge. What it does, it takes a catchment area and calculates how much runoff would go into an area of focusing, which was this lower region. Then it calculates, based on permeability and a number of other parameters, what the deep recharge would be, in other words, what goes through the zone via evaporation, and what is available for deep percolation.

To get the rainfall signal, we used a climate generator, which is linked to this Depression Focused Recharge Model, and the climate generator takes actual data and weaves twenty years worth of data at Tonopah, which was the closest station that was actually built into the model. The model has tables of most of the National Weather Service meteorological stations. So twenty years worth of data from Tonopah, and the average annual rainfall there is 130 millimeters per year as opposed to the 150 to 160 that are thought to exist at Yucca Mountain.

What the simulator does is it takes these statistics, the actual statistics, and includes things like storm intensity, duration, runoff producing storms, and it generates a daily climate situation which is consistent with the average statistics that were generated.

So we applied this to the Solitario Canyon fault using a number of different parameters, varying a number of them, and then we applied it also to the WT-2 wash.

These are some of the parameters that it utilizes. But basically what I wanted to show here is the infiltration that was possible, the recharge. And for Solitario Canyon, we came up with a range from 12 centimeters per year to 30 centimeters per year, and for WT-wash, we came up with 8 millimeters to 16 centimeters per year. And, of course, we were quite shocked by these numbers because they were orders of magnitude higher than what anyone had previously modeled for this area.

So we thought, well, we'll just dump 10 centimeters down this fracture and see what happens. The time period was totally arbitrary, and we got much wetter conditions and we thought this is not a bad fit for our first run. However, what I want to point out here is a major limitation in all these modeling exercises is the time frame. If you don't run these things to equilibrium conditions, which we need to have some kind of guidance in this area as far as INTRAVAL goes, I believe, for this particular test case, you can--for example, if I ran this run for 500 years, I might be able to dump 20 centimeters down here and get a fairly good signal like this.

I want to also mention here that we did not use that evapotranspiration term here in this particular run. But if I was to run this for, say, 15,000 years, I may only be able to put 10 millimeters per year in it. So I think we need some kind of time constraint or we have to run these models to steady state in order to establish the actual infiltration signal.

One other problem that we see in these models is the problem of the equilibrium flow between the fracture and the matrix. This modeling work was done on VTOUGH, which is the vectorized form of TOUGH which was developed by Karsten Pruess. But it does have this equilibrium term. It's controlled by the pressure between the fracture and the matrix. And for Yucca Mountain, we feel this may not be appropriate, and yesterday they discussed this Weeps model, which is just the opposite, it has no interaction between the fracture and the matrix.

But I think that there has to be some, but we have to have some way of limiting it. Otherwise, what happens over time is all of this matrix will eventually wet up. We tried messing around with the area available to have this interaction, and that does help, but it only helps in the short term. In the long term, the matrix will just soak up all of this water, so we may be missing a large part of the infiltration pulse because that water is being pulled out of the fracture too quickly.

Well, once we saw those results, we decided that we would get serious about this model and do sensitivity analysis. This is not a full-blown sensitivity analysis by any means, but a parametric analysis looking at sensitivity to infiltration history, fracture to matrix contact area, fracture aperture and the water retention curves or characteristic curves, as we sometimes call them.

Now, with the infiltration, we varied it from .5

millimeters per year, all the way up to the 10 centimeter run that you saw, although that's not included in this chart. We also used a pluvial climate simulator which we ran for 45,000 years, and this one started out at .01 millimeters per year and wrapped up in just a ramp function up to about 4 millimeters per year with a maximum at about 18,000 years in the past, and then it ramped down to present infiltration again to .01 millimeters per year. We actually didn't find much change in this signal due to this pluvial ramping.

What was significant and what improved our results more than anything was changing the characteristic curves and the aperture in the fracture. Now, before, I showed you there was an upper part of the fracture and a lower part of the fracture. In the upper part here, we show a 400 micron aperture, and in the lower part, 300. And we thought this could be justified by the fact that maybe there's lithostatic loading, maybe there's fracture fillings or coatings at depth, something that would close this aperture with depth.

Also, we thought because of fillings, we may have a different characteristic curve in the lower part than in the upper part. So this was just intuition, just guesswork. We had really no information about it.

I want to show the curves that we used. I'm going to have to look at it this way. I'm used to looking at it on this angle. This lower curve is the upper curve which was used for the upper part of the fracture, and it came from Wang and Narasimhan's work, directly from the literature. This dark line is the matrix characteristic curve. So what we did was simply moved this curve up the pressure term here, changing the pressure from 600 Pascals up to approximately 30,000 Pascals. We preserved the steepness of the curve, as you can see here, so it's not quite the same as a matrix curve, but it's about halfway in between.

And when we did that, we were able to pull down, and this dark solid line represents that run, we were able to pull these water contents down to the average range here for this second unit, and also to get them into the range now for the Topopah Springs unit.

The other runs represent just changes in permeability and changes in the amount of infiltration. One is 4 millimeters per year; the other is 10. This final run was done with 5 millimeters per year.

So now based on what we learned, we feel that we can now go back in here and match this signal using a variety of infiltration rates, and just vary the curves or apertures to come up with the matching profile. And that's why we determined that these solutions are not unique.

So based on that calibration, we went ahead and predicted the deep borehole, UZ-16. This is a different scale. This is our model results, our model prediction. And just for comparison, we decided we would plot this against the data that were available in the Topopah Springs, and there are several boreholes that have data available, UZ-1, H-1, J-13, and I think UZ-13. Here, we're only shown the results of two of those.

There are a number of water content measurements at UZ-1, so what we did here is we averaged those measurements, and there were about I think it's 18 per interval, something like that is the average, every 15 meters, because otherwise there is tremendous scatter in this data and we were looking more at an average situation here. So we feel that we did a pretty good job on UZ-1.

Now, H-1 is considerably wetter. It's much higher. And that may be because that hole was wet drilled versus dry drilled. One thing we did find out about this rock is it has a hysteretic behavior. Once it's wet, it tends to stay wet; it doesn't dry out as fast as it wets up So it retains this moisture signal for quite a while.

So the conclusions that we arrived at was, of course, that it was a non-unique solution and that it could be varied over many orders of magnitude and probably have a reasonable match with the data. So we need some confirmatory parameters, other parameters to check these results against.

For example, if we had something that would tie down the time history of the fluid tritium or chlorine-36 or some other isotope, that would be very useful in limiting that range of flux because in some of these one dimensional models, for example, it would take maybe 100,000 years to saturate or bring that water content up in that second unit. But, for example, with the fracture flow and maybe 10 centimeters of infiltration, it could wet up in, say, 20 years. So if we had something to tie that down, that would be quite useful.

Also, another parameter that we think would be very useful is the time series of temperature in the unsaturated zone. And I want to reference here the work of John Sass. This should be 1987, USGS. I believe it's an open file publication. He did some measurements, not only in the unsaturated zone, but in the saturated zone, and claims that he saw a pulse move 150 meters quite rapidly through the unsaturated zone. And if we can in fact watch these pulses move by having this time series of temperature, then I think we should be looking at that. We need anything that we can get to tie down this flux.

Other data needs are fracture information. We really have nothing. We have that one characteristic curve by Wang and Narasimhan. We know very little about apertures except the work of Mike Chornack. So those areas of characteristic curves, all of that needs to be developed somehow for fractures.

Also, I'd like to see the data taken in what I call these focusing areas, and I was really pleased to hear Joe Rousseau's talk yesterday that they are looking in these structurally controlled areas, because I really believe we have to look in the fault zones and areas not only of the fault zones, but other areas that may be focusing this infiltration signal.

I also think we need to look at what other data are available that can help us sort out the range of flux or the conceptual model, and I have classified these types of information into two categories; confirmatory data, which I talked about earlier, and also data against which you can check the consistency of your conceptual model. And there are some studies and some work that are available that I think could be potentially useful to us in this regard, and that's the water table elevation, water table frequencies and water table temperature distributions.

I'm going to talk about these briefly and show you some of the data that are available, and then my last two slides I have sort of synthesized this data and put together some conceptual models which I believe need to be looked at in more detail by the project.

This is the water table contours. We're all familiar with this steep gradient that exists to the north of the site. But in a way, this has almost been misleading because this gradient is so steep we have sort of ignored this one. Now, this one is actually quite steep also. As you can see here, the difference between WT-7 and H-3 is 42 meters. That's over 120 feet over a one kilometer distance. So we have quite a steep gradient here, which coincides almost exactly with the Solitario Canyon fault.

Now, this is copied from Scott and Bonk. They traced the fault to move this way, but they also show a splay coming off just south of H-5. H-5 seems to have the same water levels as the western side of Solitario. But soon on the other side, it drops off to this average of 130 meters elevation.

I believe this channel in here, this fault structure that runs through here, is going to be important as far as transmitting fluids, because you'll see here this potential has dropped about 20-some feet from here to here. So this may be another block independent part of the system. It's probably a highly segmented system. It may not be correct to be modeling this as one regional system.

Other information that supports this idea of segmented systems comes from some work I did several years ago where we looked at the water table oscillations. We were looking at long-term oscillations as opposed to the short-term oscillations that the USGS was studying.

What we found was that WT-7 and WT-10 were responding at the same frequency. WT-16, 1 and 11 were responding at a different frequency. These averaged 1,000 days, approximately, and these frequencies were about 880. So we have different signals that we're getting here, so again evidence of maybe separate systems. It's interesting that these are linear. They all lined up in a linear fashion I thought.

Perhaps the most interesting piece of information we have is the temperature at the water table. And this map was provided by Bill Dudley at the water trials, the water hearings for the Yucca Mountain site. And I know this is really hard to see, but what I want to point out is this 30.5, 30.5, 30.6 centigrade area. And then over here to the southwest of the mountain, we have 38.8. Now, this area, as you know, is close to the area of the volcanos.

Over here coming down Fortymile Wash, again we have cold water, 28 degrees centigrade, 29 degrees up here on the other side of the steep barrier.

So as I said, I've synthesized these and put this on a map which shows the location of the Yucca Mountain and the repository. And this cold infiltration signal falls exactly along the Ghost Dance Fault, exactly. So what I'm hypothesizing basically is that maybe we're getting cold recharge coming in from the other side of the steep gradient area, coming and moving down through the Ghost Dance Fault.

Also cold recharge coming in through the Fortymile Canyon area, and then some heat source possibly upflowing water on this west side of the Solitario.

Using our focused recharge concept, we plotted this against these temperature gradients, and it could be that some of this is coming in coinciding with the cooler temperatures here. Again, the coldest areas are coincident with the Ghost Dance Fault.

Now, I don't mean to imply that the infiltration from the unsaturated zone could cool this water by itself, but it's possibly some combination of the two from the saturated zone and infiltration. What I just wanted to point out is that I feel that this is consistent with the data that we have, and it's something that should be looked into.

And that's all I have. If you have any questions, I'll be happy to answer them, or try to.

DR. DOMENICO: Any questions from any of the board members? Don?

DR. LANGMUIR: Yesterday, we learned that the saturated zone was apparently under artesian conditions, which would suggest to me at least that cold recharge wouldn't be likely to make it to the saturated zone in the vicinity of Yucca Mountain. It would suggest that the heat effects you're talking about are coming from a greater distance away. Would you comment?

MS. LEHMAN: Well, the UZ-16 hole is the one that had the upward gradient. I don't know about other wells in the area, but it certainly could be coming from other areas as well. I don't know. This is something that we have to sort out. But, to me, it seems like we're dealing with different basins, different ground water systems, and the sources could be varied.

DR. DOMENICO: Any comments from staff, questions? Ed?

DR. CORDING: Just a brief comment. Ed Cording. I was wondering about that equilibrium term. Is that changing according to the degree of saturation; is that the way it works?

MS. LEHMAN: Yes, it does work that way in the unsaturated equation. Basically, yes, we need to control that. I feel we have to have some kind of control on limiting how much water can come out or from what distance. Tom Buschek feels that this radius for non-equilibrium flow is controlled somehow by the imbibition term that we get. So if we could somehow limit the radius of influence around the fracture, I think that's what we would be looking to do.

DR. DOMENICO: Anything further?

Okay, thank you very much, Linda. I think we'd better get on. We're running a little bit late.

Our next speaker is Jean Younker from M&O, Integration of Data and Models.

DR. YOUNKER: Okay, my purpose in talking with you very briefly today is to kind of set the stage for the rest of the presentations you're going to hear from the DOE speakers to give you a little bit of kind of a synthesis of what I think you've heard before, and then to hopefully give you a little bit of insight into what the M&O's role is in carrying on from what Tom Statton described yesterday at the site, data acquisition, first level synthesis and development of conceptual models, then how we hope that M&O is going to play an important role in taking that on across to and up to, if you think of Jerry Boak's pyramid, to the total system modeling approaches that we're going to use.

I think Jerry explained to you that one of the major roles that performance assessment plays in the program has to do with how we get from the new understanding that you heard presented by people like Alan Flint and Joe Rousseau yesterday about what we think the actual saturation conditions are in the unsaturated zone at Yucca Mountain as an example of one type of new data that we're getting through some alternative conceptual models and numerical models, to some extent, that you heard talked about as we try to figure out what is this telling us about the way processes and conditions are really existing today at Yucca Mountain, such that we can then understand them well enough to get into our predictive modeling that the performance assessment people are responsible for, such as the descriptions you heard yesterday of a couple of the models used in the most recent total system performance assessment, the Weeps model and the composite model, as a way of capturing this and making the link over to what does all this mean in terms of performance of the site.

It's quite a far step from asking someone who's looking and analyzing site data of this sort to tell us what that means in terms of ground water travel time or flux contacting the waste package over some interval of time. So performance assessment attempts to look back at this site data acquisition ongoing and the site model development and select from that the information that allows us in the most credible way to then function within this pyramid of models that I'll talk about a little bit further.

A couple of other comments on this. One of the things I think I heard yesterday was kind of a discussion about how soon should you be over here in these abstract models doing your stochastic simulations and asking your what if questions and attempting to understand what makes sense. And I think I would take the position, of course, having been a simulator back in my college days, that you can learn a lot, even in fairly early phases of understanding of your system, by working with the more abstract models and then continuing of course to fine tune them as we gather information about the site.

I think that the early predictive modeling is important from the standpoint of feeding back to the site program the priorities that the performance assessment regulatory end of the program needs in order to get a better handle on the actual performance of the site. So I think this kind of modeling, stochastic modeling, is very important early in the program.

I think you heard some opinions possibly that you can't necessarily learn that much, or that you shouldn't go forward to that level of abstraction until you really understand your system. And I view this as kind of a fairly traditional argument between the empiricist who says, oh, don't abstract until I can tell you all the details, a full understanding of the system, and the person who wants to step back and abstract and perform the simulations and ask the what if questions at all phases of understanding of the system. So I think we kind of heard that discussion yesterday.

Okay, nothing new really to tell you here except just to remind you that one of the very important phases of this program that we went through that we haven't really talked about or gone back to in a while is the development of the alternative conceptual models.

Back during 1987 when we were putting together the site characterization plan, those of you who were around at that time will recall that when we gave the Nuclear Regulatory Commission our draft, consultation draft of SCP, they came back to us and said, you know, we don't think the Department of Energy has really scanned the horizon of alternative conceptual models that fit the current data base. We believe you need to go back into that data base and do a very systematic review to determine what range of alternative conceptual models could be supported by the current understanding of the site and the current information.

So one of the steps we went through in the site characterization plan was to go through in every one of the site discipline areas that we're going to be studying and establish the full range of alternative models that really could fit the data at that time. And I think that was a process that was very important and very useful and allows us then to go back and see where we fall within that range of alternative models, how the data that we've collected has helped us test the validity, and perhaps in some cases, select some preferred models and eliminate those that are no longer supportive.

This whole process that I have written up here going through the numerical model development and then into your sensitivity and uncertainty analysis should be viewed as something that you would expect to see the site modelers and the site people that you heard talking, like I think a good example is both Alan and Joe yesterday, as they try to establish and understand what behavior site processes are currently today. And then also this same kind of a process goes on over in performance assessment. And I have another, the next view graph lets me explain this a little further.

There's some very important interfaces and feedbacks that go on as these processes continue in parallel. Over here are site data acquisition functions that you heard talked about in terms of the management of them, the test planning phases and the field acquisition of data. The site modeling activities that you heard described yesterday where, in order to understand what the new site data means on a process level, you have to go through some numerical modeling and code development and look at your results and make some predictions of what you're going to find in the next borehole, as Alan Flint was talking about, and a very important part of the way we understand and move forward and understanding the conditions at the site.

The interface then over to performance assessment that I'd like you to think about with me is that those whole range of conceptual models, some of which have been numerically represented, are kind of the menu from which performance assessment must operate, selected the preferred models for the most part, probably not doing an awful lot of new model development, but certainly selected the preferred ones and interfacing with the site people to understand which of those ways of representing the information makes the most sense, to then go on into the performance assessment process and attempt to figure out what this information means in terms of regulatory compliance and predicting against the kinds of numerical requirements that the site would have to meet in a regulatory arena.

I think there was a question asked yesterday that illustrates this interface quite well, where I think it was John Stuckless was asked what does your information about climate mean in terms of site suitability of regulatory compliance. I view where John is operating is over in this side of the overall scheme of the world. The people can answer the questions about what it means in terms of suitability and compliance with the regulations, I believe, have to have been working over on this site where they can make that connection as to what it means if you say climate change in the last 20,000 years was by a factor of ten. Well, what did that mean in terms of the actual performance of the site? It isn't just an obvious answer, an obvious connection for many reasons.

Okay, let me make kind of a little change and move into the M&O's role as we are seeing it as DOE is helping us to understand our role in this overall process that I'm describing. And the role that we're playing is kind of stated in this objective down at the bottom, and that is to make sure over the long term that we have a credible hierarchy of models from the process models, the ones that you hear talked about that are really at the site modeling level, up through the subsystem models where we talk about the engineered system and parts of the natural system, to the total system models.

And I think it's going to be just vital for the Department of Energy as they go forward, if they go forward in the licensing arena, to be able to show some level of credibility that they have the right kinds of connections, the right kinds of models at each level to allow you to make the abstractions that you need to make in order to get up to a total system performance model that you can run thousands of realizations and represent performance over tens of thousands of years.

So I think this overall hierarchy of models and codes and its integrity and its completeness is one of the principal roles in the performance end of M&O's role we see.

Some of the things that we've done so far, just to give you kind of a quick snapshot in time, we've taken a good hard look at all of the available total system models and codes and have made a recommendation I'll mention on the next view graph as to which ones we think have the highest potential in terms of going forward.

We reviewed the subsystem models, particularly package models and codes that are available, and we've selected one that we believe has the best potential and should be focused on for further development and further refinement. We're in the process of reviewing the flow and transport codes to make sure that we can begin to focus in on the ones down in this part of the pyramid that should receive further attention and funding, take them into the process to get them ready to be used in licensing, and we are working, of course, very closely, as you've heard talked about in this meeting, with the people who are doing the mechanistic and process modeling to make sure that all the pieces are there.

In terms of specific actions, just to give you a feeling for how we're attempting to help DOE sort this out, the RIP code, the Repository Integrated Performance code that Golder developed in the past couple of years, looks to us as a real good candidate for a total system code to be focused on in the future. And of course let me mention the reason this focusing and selection is a difficult process, because I think someone mentioned yesterday that there was so much concern about the M&O displacing people and work in the program. Well, you know, part of our job is to help DOE become efficient and focus and make sure that the work that's being done is the right work and that it's cost effective.

So one of the things we believe you have to do is to review the complete suite of codes and models that are out there and begin to determine those that should be taken forward through the very costly verification, validation, software configuration and management that will be necessary in order to take those codes into a licensing arena.

So I recognize that it's painful, but I also think it's something that will have to happen through time, because clearly not all of the codes and models that are available should be or even could be perhaps taken through that process. So I think we have to focus in on those that are the most useful, most likely to be defensible, not too soon, I'm not saying do it today, but I'm saying that through time, that's one of the important roles that we think we have in working with the Department. So we've recommended that this RIP code is a good candidate as a total system model, not necessarily at this point to the exclusion of the any others.

What we did with it was to conduct sensitivity studies on the TSPA results that you've heard presented before. And what we've discovered is it's a very usable code. It's also one that when we get different results, the assumptions are clear, we can sort them out and explain why we get different results from what were obtained in the previous TSPA.

Another step that we've taken is to begin working with the ARREST, which is the PNL subsystem waste package model, working with that to improve some of the parts of it that are fairly abstract and need to be improved for the near-field processes, allow alternative geometries of the waste package and alternative designs, such that, for example, the multi-purpose canister concepts can be accommodated in that subsystem model. And that's going to be an important step as we get a subsystem waste package model that is a little bit more mature, a little bit more useful to us in helping feed some information into some of the design decisions that need to be made about the engineered barriers.

And then the next total system performance assessment is underway and you've already heard some points about that from Jerry Boak yesterday. One of the things that we're focusing on this one is some level of representation of non-isothermal conditions. You probably recall the last one was isothermal. Also, once again, getting in alternate waste package designs and emplacement modes, and then some of the improved hydrogeologic understanding that our site people have helped us gain in the past few years since we set up the models and codes for the last TSPA.

Okay, let me give you a perspective on what I think you've heard and what I think you're going to hear in the rest of the session. You've heard a number of talks where the information that feeds into, the way I view it, the bottom of the pyramid, meaning the detailed site information that helps us understand conditions and processes at the site as they exist now. You're going to hear about some modeling of flow in the unsaturated zone this morning from Ed Kwicklis and from Bo Bodvarsson from LBL and USGS work going on that takes some of this information and begins to apply it to the way you model flow in the unsaturated zone. You heard some of that also yesterday.

You'll hear some information about the climate modeling that's going on that really, to me, it's a fairly broad type of modeling. You'll hear about the broader global and also the regional climate modeling that ties into our site modeling. You won't hear anything in this presentation, the way the meeting has been set up, you won't hear anything further really about things that happened near the top of the pyramid. We've really had most of our exercise this couple of days down toward the bottom of the pyramid of models, from a PA perspective, of course.

You're also going to hear some interesting discussions about how software will have to be qualified, given the current quality assurance program that we operate under. And from Susan Jones later, you'll hear about how we take existing data, using an RC approach that has been documented for us, how we can take existing data and qualify it, if it wasn't collected under a quality assurance program, and use it in very specific and focused ways.

You'll also hear from Chin-Fu Tsang talking about overall kind of an approach and philosophy for building confidence in the models really at all levels of the pyramid. But he'll give you a perspective, I think, that has some agreement from the international community as well as some good thoughts about how we really build the confidence we need, which I view, as I said, as one of the major roles that the M&O has, is to make sure that all those pieces are hooked together. All this sits, of course, upon the regulatory basis for understanding climate and infiltration that Jerry Boak told you about yesterday.

And between the regulations and all of this work going on, the DOE management process you heard Max Blanchard talk about, you'll hear Russ Dyer wrap up as he talks about how we attempt to close this all out as we determine or ask the difficult question how much is enough. So I think this gives you a perspective on what we tried to do for you in this two day presentation.

Questions?

DR. DOMENICO: Thank you, Jean. Any questions from board members? Warner?

DR. NORTH: I'm very encouraged by this, but on the other hand, I'm a little troubled by the lack of detail. My sense is a tremendous amount has been learned, if we go back to your interfaces and feedback slide, in looking at the interface with arrows going back and forth between site scale modeling, which then connects with site data acquisition, and on the right side, performance assessment modeling. But my sense is that at this point, we're still at the level of making major changes, very large increments of learning as we go through the generation with those two arrows.

You've given us some plans with regard to how you propose to do the next total system performance assessment. I think we would be very interested in seeing those plans made much more specific in understanding why you were making the choices you were making and what you expect to accomplish in the next generation.

So I think one of the decisions for my colleagues on the board here is to what extent other board members would like to hear this in our July meeting, or to what extent this should be a separate meeting of the Risk and Performance Analysis Panel, where we will go into those issues in some detail. But that definitely is something that we want to have on the agenda. We've talked about it privately, but I'd like to put it on the record for my colleagues at this time.

I don't think I want to draw you out at this point and try to have you explain those details but, rather, say I think they're extremely important in looking at how, essentially, we are going to get better communication across the gap between the details of the science and the needs for the regulatory aspects of the program.

DR. YOUNKER: And we're spending a lot of time on that. I think the PA Management team, with the leads from Sandia, from the M&O and from Livermore, spent a lot of time in the last couple of months, you know, trying to make sure that we have all the pieces tied together and the plans in place for that next TSPA, with exactly the point that you make, to make sure that we have, in there, built in the way that we learn from the previous work and from the ongoing site studies.

DR. NORTH: Well, just to pick up one aspect, it seems to me bringing in the thermal loading issue when you say let's go non-isothermal, that's a major challenge, and I'm not quite sure exactly how it is you propose to do it to have a simplified version of the modeling work that Tom Buschek and his colleagues have been doing, such that it can be used in a total system performance analysis.

DR. YOUNKER: Yeah, we are not going to be able to do it in a fully integrated way, as I'm sure you're aware. I think our approach is probably going to be to be able to run the codes that can be run at, say, three different temperatures. We're not going to be able to do something that's really an integrated approach to non-isothermal modeling. But that is one of the areas that we're working on trying to figure out exactly how we can encompass that.

DR. NORTH: Well, it seems to me that carrying out some sensitivity analysis and having some workshops where people on both sides of this interface talk to each other would be extremely valuable.

DR. YOUNKER: That's exactly right.

DR. NORTH: I think we've seen the value of that in the meetings we've had over the last couple of days. We've had some alternative presentations from people who have been working with Nevada on some of the issues of the hydrogeology. I must say I haven't got it all sorted out, but I have the sense that this kind of interaction and discussion between the people goes a long way towards sorting it out.

Professor Tsang may tell us a little bit more about the general process with respect to model validation, and I look forward to those comments. But what I'm urging is that you think about how you can communicate with us more rather than less.

As one specific example, I have not seen anything on the use of the RIP model and the sensitivity cases that you describe. I'm very eager to see that documentation, and I would urge you to find ways of packaging that kind of material so that it can be broadly shared.

DR. YOUNKER: Right.

DR. NORTH: You're learning from it. Let the rest of us learn too.

DR. YOUNKER: That's right. There will be a document available. In fact, it's in review with the DOE right now. So within a month probably you can have copies of that.

DR. DOMENICO: Anything from the staff? Questions?

Well, now that we know where we're going, I think maybe we should get there. Our next presentation is modeling flow in the unsaturated zone fractured rocks by Ed Kwicklis from the USGS.

MR. KWICKLIS: I guess Jean has already told you where I fit in in the overall scheme of things. The modeling I'm going to talk about this morning fits in at the very base of the modeling pyramid that you've seen Jean and several other speakers present here today. And it's both I guess flattering to be supporting all the modeling that comes afterward, and at the same time, somewhat dismaying to see yourself at the very bottom of that pyramid.

The way that my work relates to the issue that we're discussing today is that some of the hydrologic process models that we're developing at the USGS would help support this site modeling work that's being conducted jointly by LBL and the USGS, and I hope that in the next 35 minutes or so, that that relationship becomes a little clearer.

The outline of my talk; first I want to talk about some of the study objectives. And actually, I'm involved in two different studies; one is fluid flow in unsaturated fractured rock and the other is the site modeling and synthesis study. And the fluid flow in unsaturated fractured rock study has as its objective to develop numerical models of the processes that we believe are operating at the site so that this understanding can be incorporated in the site models.

Next I want to review some of the dominant site features, including the hydrologic and geologic features of the site that may impact our assessment of the flux at the Yucca Mountain, and talk a little bit about what implications of these processes may be for our ability to estimate the flux through Yucca Mountain, using methods that have been traditionally applied in unsaturated zone studies.

Then I'd like to go ahead and present some examples of recent work, and I'm going to present two examples which kind of represent opposing ends of the spectrum of work I've been involved in. The first is a very purely hypothetical study of how flow occurs in unsaturated fractured systems, and the next study would be a very applied study in which we looked at data collected from a wide variety of sources and tried to estimate the liquid flux through four unsaturated zone boreholes. All I'm going to have time this morning to show you are flux estimates through one of those boreholes, and then finally some general conclusions.

The study objectives are simple enough, which were to construct conceptual and numerical representations of the physical processes that govern fluid flow in nonreactive tracer transport through unsaturated fractured rock, and then evaluate these through comparison with the results of controlled experiments, some of which I'll be describing to you briefly.

As one aspect of the G-29 study, we'd also use data collected from the site, as you've seen several presenters here do already, to develop models of how fluid flow occurs through Yucca Mountain.

Chin-Fu Tsang will speak just a little later on where these process models fit into the site, and I'll touch on these only briefly. The models that we're developing in our study hope to identify processes important to the hydrologic behavior of the unsaturated zone, and it's important to remember that prior to the Yucca Mountain project, flow in unsaturated volcanic rocks was of little concern, and so when this project started, very little was known about the actual flow behavior within these types of media. And, therefore, there's some fundamental work that needs to be done to help us understand even the basic processes important to describing and modeling flow in this kind of environment.

For the same reasons, we would want to use these models to design and analyze experiments and interpret field data. The testing methodology itself to characterize these kinds of environment aren't as well developed as further media and, therefore, these models will help to do scoping calculations to analyze various experimental designs prior to their execution.

The models would also be used to integrate data collected from a variety of scales. The properties of fractured rocks are notoriously scale dependent, and we're going to be collecting data over a wide variety of scales, ranging from information collected on single fractures sampled with coring to cross-hole tests that cover many tens of meters, and we want to be able to integrate data collected from a variety of scales. And then because the site modeling scale is so much larger than the scale at which we can conduct experiments, we need tools to help us estimate what the parameters are at the modeling scale based on our measurements made at a much smaller scale.

And Linda very well described this morning the need to have constraints on our estimates of model parameters, and so while we can do sensitivity studies and hypothesis testing with the site model to help constrain the parameters, there's also an additional need to further constrain to the best that we can our choice of parameters in the site model.

Finally, we want to assess the conceptual model representation of the physical system in large-scale models of the site. And this again will go back to the confidence building process that Chin-Fu will describe, and inevitably there is going to be simplifications of both geometry and process made in the site models and we want to assess what the significance of those simplifications are.

Yesterday, you heard several presenters from the USGS describe the activities that are now taking place either at the surface or from surface based boreholes, and I'd like briefly to describe to you an additional component of the site characterization program that would be conducted from the underground ESF facility.

This figure shows the various test and modeling scales at which some of our process oriented modeling and experimentation would be conducted. At a scale of perhaps several tens of meters, we would be doing cross-hole air injection and tracer tests in order to characterize the fracture continuity and connectivity at and in the vicinity of the repository horizon. And from these same holes, do single hole packer tests that would help us calibrate an aperture distribution for our fracture network models.

At a scale of perhaps a couple of meters, we would be conducting percolation tests on blocks of rock that have been excavated from the repository horizon and subject it to boundary conditions intended to induce significant fracture flow within the fracture networks contained in those blocks. And because we are controlling the applied flux, we can make independent estimates of that flux through the block based on our fracture mapping of the block or monitoring of the water potentials of the block, matrix properties and essentially evaluate our ability to estimate what percolation flux is and compare it with the known percolation flux that we applied to the block.

Also, there will be single fracture tests done on fractures collected with large diameter core that would look at the effects of fracture transmissivity as a function of normal stress, and look at the interference of the liquid and gas phases within the fracture in terms of relative permeability curves. And we could also look at and do some experiments to look at the importance of channelling within a fracture plane or matrix diffusion.

Also from the underground workings, we would be able to do air and water injection tests into fracture into the major faults encountered in the underground workings and be able to determine the capillary and permeability properties of any fill material within the fractures based on the cores that we obtain from there.

So collectively, the underground working provides an additional opportunity to both characterize flux in the vicinity of the repository and also will hopefully shed some light on our ability to characterize the flux.

Next, I'd like to review some of the site processes that may factor into the way we think about characterizing flux. And anybody who's done any preliminary reading about the hydrology of Yucca Mountain has undoubtedly seen this figure which was taken from a conceptual model in a 1984 USGS report.

And just to show you that, even as early as 1984, we recognized that several of the processes that were being discussed here in the last couple of days. One is the fact that the infiltration into the mountain is not only temporally but also spatially variable. A second is that beneath the surface of Yucca Mountain, the contrasting properties of the various layers set up the possibility that there's a lot of lateral flow beneath the surface of Yucca Mountain, both at the contacts between the welded and non-welded Paintbrush Tuff, and also at the contact between the densely welded Topopah Springs unit and the underlying Calico Hills. The Calico Hills, because it has such low intrinsic permeability and very little secondary permeability, is potentially a location for perched water, and the contact between these two formations is potentially a location for lateral flow.

The large scale features within the mountain may intercept that lateral flow and redirect it downward, or depending upon the capillary and permeability properties, water may perch on the up dip side of the fault, in which case, focused recharge may be occurring adjacent to the fault.

We also recognize that because the fractures in the densely welded units are probably air filled under present day recharge conditions, that air circulation within these units may be important in redistributing moisture.

So the next slide merely summarizes the processes that any analysis of percolation flux would need to consider. And these are listed on the slide.

Next, I just want to briefly review the possible methods for estimating of percolation flux and they've traditionally been applied to unsaturated zone studies. The first is direct calculation from Darcy's law. This is one of the simpler techniques, and it involves merely determining the coefficient of hydraulic conductivity in Darcy's law at the existing water saturations based on measurements made in cores, and determining the hydraulic gradient over that same interval, either by making water potential measurements on cores or through in situ monitoring and by determining all the variables in Darcy's law, simply calculating the flux.

The principal uncertainty associated with this approach results from uncertainty in the hydraulic conductivity versus saturation or water potential curves.

But in our case, also because of the fact that there may be non-equilibrium fracture flow occurring within the fracture that isn't reflected in either the water potentials or water saturations of the adjacent matrix. And as I'll describe to you later, this is almost certainly true in the near surface of densely welded tuffs of Yucca Mountain, and our hope is that less non-equilibrium fracture flow is occurring at depth.

A second possible method is direct observation. For example, we can measure inflow in ramps or drifts. And the perched water study is intended to collect any freely flowing water that enters the boreholes and drifts. And presumably from the isotopic content and the manner of discharge from those features, we can determine if it's coming from a relatively isolated fracture or whether it's connected more intimately to the near surface to surface processes.

Another technique that's traditionally applied is the use of environmental tracers, for example, Carbon-14, tritium, Chlorine-36. Most of these applications of using these tracers have been made for conditions in which it's reasonable to assume one dimensional porous media type flow. And in structured systems like fractured systems that exist at Yucca Mountain, it's questionable whether we have a system that can be approximated as one dimensional porous media type flow.

And so it seems that in our case, the principal use of environmental tracers will be to reveal what flow mechanisms, such as advective flow versus diffusive flow, or distinguished between matrix flow versus fracture flow, or if the data is sufficiently distributed in space may indicate flow paths. And so the principal use of environmental tracers, you need to compute travel times and thereby constrain numerical models that actually do calculate the flux.

Finally, numerical modeling. Numerical modeling is subject to the same constraints that the direct application of Darcy's law is, namely that all the numerical models require that the hydraulic conductivity versus saturation relationship be developed, and so we're subject to that uncertainty. And also there's additional uncertainty in that we assume dimensionality of the model, we assume whether it's a steady state or transient behavior, and these may influence how we interpret our results.

However, we're also able to honor more soft data as constraints in the numerical model, such as the presence or absence of perched water bodies, the existence or non-existence of seeps inflowing into the underground drifts and that type of thing.

So we concluded that the flux estimates from each method are subject to some uncertainty and we need to use complementary methods that yield an internally consistent picture of liquid flux and its spatial distribution.

We also say that the accuracy and certainty required in our estimates of percolation flux depend, in part, on performance assessment analysis and the characteristics of the engineered barriers. And conversely, we believe that the characteristics of the engineered barriers may depend upon our estimates of flux, and that if we can demonstrate that flux is low today and likely to remain low, then maybe less reliance needs to be placed on various types of engineered barriers.

Next, I'd like to give some examples of recent work. As I said, the first is a very hypothetical study intended to look at how water movement occurs in variably saturated fracture networks. The second example is estimation of unsaturated zone liquid water flux at four UZ boreholes, 4, 5, 7 and 13.

The first study, numerical investigation of steady liquid water flow in a variably saturated fracture network, had various objectives. The first was to gain insight into the formation of preferential pathways in variably saturated fracture systems.

Numerous observations have been made in other nation's repository programs that water inflow into underground drifts located beneath the water table that flow into those drifts occurred across a very small fraction of the surface area of those drifts.

And then closer to home in Ranier Mesa, similar observations have been made about water inflow into some of the tunnels that have been drilled into Ranier Mesa. So one of the objectives was to look at the conditions leading to the formation of preferential pathways through fracture networks in variably saturated fracture networks.

A second objective was to assess the limitations and implications of point measurements of water potential in the field. And, specially, we were concerned that in some of the block experiments that I alluded to earlier in which we induce significant fracture flow, we questioned to what degree we could expect variability in water potential measurements within that block and how many points we would need to actually characterize an average water potential.

A third objective was to evaluate the equivalent porous media representation of variably saturated fracture systems. One of the principal ways in which we were going to evaluate flux is through modeling and, therefore, we wanted to know if a porous media representation of a fracture system that would potentially be used to evaluate flux was the correct representation of the system we're trying to estimate the flux in. And so we're also going to look at whether our fracture network approximates an equivalent porous media.

The fracture network that I assumed in simulations is shown here, a very simple network. I want to emphasize that this is the first step and not the last word in the fracture modeling we're going to be doing in the study. The fracture network, instead of nine fractures, the five sub-vertical fractures were 125 micron average apertures, and they were intersected and connected to the outflow boundary via four sub-horizontal 25 micron fractures.

The sides of this five by five meter flow domain were no-flow boundaries. The upper and lower boundaries were prescribed water potential boundaries. The network doesn't really reflect any particular geographical environment, but was chosen solely to help us develop conceptual models about how flow occurs in this and similar systems.

In addition to the assumptions that I have already mentioned, we assumed impermeable matrix drops, and so there was no cross-block flow or any other fracture matrix interactions. We assumed that the flow was steady state and, therefore, our results are probably more relevant to deep unsaturated conditions and we have ignored hysteretic effects and, in fact, have taken care to avoid hysteretic effects. But in doing the modeling, it was clear that hysteretic effects in fracture systems would be important in newly wetted or drained fractured rocks.

Linda made a plea a little earlier for the need for more information on the unsaturated properties of fractures. We at the USGS developed a numerical model that takes account of small scale aperture variation to predict how the transmissivities of fractures change as a function of the water potential. And this shows the transmissivities as a function of water potential for the 125 micron fracture as well as the 25 micron fracture, and you can see at a large water potential, that is to say less negative water potentials, the 125 micron fracture is much more transmissive than the 25 micron aperture fracture. But because the average openings of the 25 micron fracture are smaller, it's able to retain water at greater tensions and, therefore, at tensions below about .11 meters, it remains much more transmissive to water than does the 125 micron fracture.

So those were the properties.

I'd also like to point out that both fractures, even the 25 micron fracture, are largely drained at minus one meter water potential, and that almost every rock type at Yucca Mountain will be completely saturated at that water potential. And so if you want to assume fracture matrix potential equilibrium, fracture flow doesn't really become significant until the matrix is fully saturated.

Over the next four slides, I merely want to show how the location of the principal pathways, that is, the principal pathways for the flux through the network, as well as the distribution of relatively wet versus relatively dry areas within the fracture network change as a function of boundary potential that we apply to that system. And we change the boundary potential from zero meters to minus .25 meters in increments of .025, and I'll run through these really quickly.

I just want to show you that for this particular boundary condition, the large aperture fractures that intersect the inflow boundary carry most of the water, and so we see that most of the water flows through the pathways that are shaded in your overheads, that roughly 70 per cent of the total flow existing in the system exists along here. Because of its small aperture, very little flow occurs along the 25 micron fracture, and so very little water exists in the fracture network through those fractures.

There are significant positive heads developed in that same area because the connecting small aperture fractures can it transmit the amount of water carried by this fracture, even under saturated conditions. So we developed positive heads in this region of the fracture network as great as 2.5 meters. Conversely, fractures that are connected to the inflow boundary only through connecting small aperture fractures remain at negative water potentials for both this as well as all of the boundary conditions that we considered.

As you go from zero to minus .25 meters, we see that the dominant flow path is now along the left side of the flow boundary, and actually over 97 per cent of the flow through the fracture network now occurs through this system because at this water potential, the 25 micron fractures are orders of magnitude more transmissive to water than the 125 micron fractures. And for this case, most of the fracture system was there did not conduct any of the flow.

So we see in this case a tendency for flow to become concentrated along very specific pathways, again for the same reason, because the 125 micron fractures are relatively non-transmissive. This is now the driest zone in terms of water potential, whereas, this feature is now the wettest. And so what we saw was complete reversal over a very small range in water potentials where most of the flow was occurring as well as where the driest and wettest zones within the fracture network were in terms of the water potential.

So we next wanted to look at how good a representation of an equivalent porous media is our fracture network, and the criteria we chose to evaluate that is we asked how would a porous media behave had a porous media filled that five by five meter flow domain. And the answer is there would have been no variability inherent within that flow region, and also the flux would have been very uniformly distributed through that five by five meter flow region.

And so in the next two slides, we want to look at how well that ideal is approximated at the various boundary potentials, water potentials, and so we looked at the variability in the flux through the network as a function of the boundary potential and we see that there are zones in which that idea was more closely approximated than others. This is expressed in the variability and fracture flux as a function of the water potential, and we see near the potential at which the transmissivities of the true fractures are equal, we best approximate an equivalent porous media.

Similarly, when we look at the variability in pressure head or water potential expressed as the standard deviation in water potential as a function of boundary water potential, again we see a minimum there. So this suggests that the fracture network better approximates equivalent porous media at some conditions more so than other water potentials.

Again, I want to emphasize that we only had nine fractures in our network, and speculate that if we assumed a log normal distribution of fractures, as has been reported for fracture apertures in non-Yucca Mountain studies, we'd have a great preponderance of small aperture fractures relative to a few large aperture fractures. And, therefore, in our case, we saw that the variability decreased but then increased again at very small water potentials.

We speculate that if we had assumed a log normal distribution of fractures with many fractures in the network, we would have seen this initial decline, and because there were so many more small fractures than large fractures, this variability would have remained low. And, therefore, we concluded that equivalent porous media representation may prove to be a much more accurate representation of a fracture network for conditions of partial saturation relative to fully saturated conditions.

This figure shows the equivalent porous media properties that if substituted for the fracture network would result in the same flux through our network as did the original fracture.

So the conclusions were that water potential and flux are spatially variable even for steady flow. The tendency for flow to become concentrated varied as a function of water potential, and that variability in water potential within the flow domain is a function of the boundary water potential.

The implications are that measurements of water potential in our block experiments may reflect only local environment for certain experimental conditions, and also that some poorly connected fractures may be saturated or drained when intersected by boreholes or drifts. And based on our analysis, we want to point out that the fact that they're fully saturated reflects the relatively poor connection to underlying fractures and, therefore, the presence of these perched water bodies may not necessarily represent a threat of potential pathways for radionuclide migration.

I also want to point out that based on this simple analysis, dominant flow pathways through the fracture networks, if they occur, may change in response to changing climatic conditions. And, therefore, even if we locate emplacement drifts today to to avoid the predominant fracture flow paths that were identified today, we also need to look for pathways that could potentially become active under future climatic conditions.

To go from the very abstract to the more concrete, hopefully, I'm going to describe to you a study that we did to estimate unsaturated zone liquid flux at four unsaturated zone boreholes, and these were located in diverse topographic and geographic environments and, therefore, the stratigraphy changed as we went from location to location. I'm only going to have time this morning to present to you the results that we obtained from UZ-5.

The objectives were to estimate liquid water fluxes through the nonwelded and bedded units. We realized over the years that flow may be highly intermittent and spatially localized in the near surface unit as a result of fracture flow, and that analysis of the nonwelded and bedded units, therefore, provided the best opportunity to utilize many of the flux estimation techniques I listed for you earlier.

We want also to better understand recharge mechanisms and the role of the nonwelded units in redistributing infiltration. Site characterization and, in particular, our modeling of the site could be greatly simplified if we could be reasonably assured that the temporal and spatial variations in recharge were moderated in some way by these bedded units, such that flux at the base of these units was relatively uniform in time and space. So we wanted to look at how infiltration into these bedded units was redistributed.

We also wanted to look at the internal consistency of the hydrologic data collected to date in order to help determine whether numerical modeling could or should honor that data.

We also wanted to derive statistical correlations between different types of data in order to fill in gaps in the existing data set. What we haven't yet done is to develop models consistent with the available data. Work that we've done to date has focused on simply analyzing the data and trying to see what implications this data has for future numerical modeling.

The methods that we utilized were diverse. This slide outlines the steps that we've taken in estimating the flux at these four boreholes. First, we did a regression analysis using porosity as a predictor variable for hydraulic conductivity and the van Genuchten parameters alpha and beta. And alpha and beta are fitting parameters in an hydraulic function that characterizes the relationship between water saturation and water potential.

These same parameters then go into a related function that estimates the effect of hydraulic conductivity as a function of either saturation or water potential.

We used porosity as a predictor variable because it's relatively easy to measure and, therefore, enables us to estimate these other variables where we haven't been able to measure them.

We then calculated saturation profiles for the unsaturated zone boreholes from porosity, bulk-density and gravimetric water content information. This was information that existed in several previously published U. S. Geological Survey reports.

We estimated more or less continuous profiles of unsaturated hydraulic conductivity versus depth in the unsaturated zone boreholes based on the regression analysis between hydraulic conductivity, alpha, beta and porosity that we did in the first step and measured saturation.

Therefore, in each of the four boreholes at which we had both a saturation and water potential measurement, we were able to estimate what the unsaturated hydraulic conductivity at that depth was. We were also able to, on the basis of that information, to make predictions of what we would have expected the water potential to be at that depth.

And then we make a simple application of Darcy's law using the measured and predicted water potentials, along with the estimates of unsaturated hydraulic conductivity to estimate the flux versus depth in those holes.

So this is the porosity versus depth profile at UZ-5. As I said, I'm going to restrict the discussion to UZ-5. The horizontal lines that you see on this figure are the depth intervals over which the same lithologic description was supplied by the geologists who examined the core.

To the right, you see the major stratigraphic units. You see emphasis on the YX porosity is on the X axis. And I just want to point out some of the features of this profile, some parts of which have already been alluded to by Alan and other speakers. And that is at the base of the Tiva Canyon, there's a very sharp increase in porosity as one goes from densely welded through moderately welded to partially welded. It's not as completely captured in this profile as for neighboring borehole UZ-4, but on the basis of UZ-4, which is only 38 meters away, we can, with some confidence, say that there's a very sharp increase in porosity between these two points.

Also, there are some processes related to the deposition and alteration of the tuff that can partially explain this profile. One is that at the centers of the major ash flow units, the Yucca Mountain member and the Pah Canyon member, the centers of these units were cooled more slowly and were able to compact and weld to a greater degree than the more quickly cooled margins, and so you see these decreases in porosity towards the center of the units.

Within the air fall units, you see an overall increase in porosity from the upper part to the lower part of the unit, which reflects the settling of the coarser ash material from the ash cloud prior to--I'm sorry--the finer material settled subsequent to the coarser material, so you see an overall decrease in porosity with depth. Also, in each of the four boreholes that we examined, you see the low porosity cap rock that Alan and other speakers have alluded to.

The saturation profile for this hole begins to make sense in light of porosity variations shown in the previous figure. We see that there's a sharp decrease in saturation at the base of the Tiva Canyon that correlates inversely with the sharp increase in porosity, that there's increases in saturation within the centers of the ash flow units that again correlates inversely with the decrease in porosity that we observed there.

Also what I wanted to point out about this figure was that for most of the interval of the nonwelded to bedded tuffs from minus 30 to minus 110 meters, the saturations within these units are very low and, therefore, substantial buffering capacity exists within these units to absorb any water that could infiltrate through near surface fractures or nearby faults.

And so this significant buffering capacity suggests that liquid water flow at the base of these units may be much more uniform in both time and space than it was when it entered the mountain at the ground surface.

These are the measured water potentials for this hole. The water potentials are shown in megapascals versus depth. Also shown is a fifth order polynomial regression that's been fit to the measured water potentials in the nonwelded and bedded units. .1 megapascal equals one bar, so we see that throughout most of the nonwelded and partially welded tuffs, the water potentials are in a range of about .1 to 6 bars.

We also believe that the water potentials that were measured for the moderately to densely welded tuffs were subjected to a large experimental error and, therefore, we didn't consider them any further in any analysis.

That was what we measured for this hole. Now I'm going to show you what we predict for this hole. On the basis of the regression analysis that we did in the first step that related alpha beta to porosity, and on the basis of measured porosity profile, we measured saturation profile. This is the water potential profile that we predicted, and we were encouraged by the fact that the overall trends in the measured water potential profile, as indicated by the polynomial fit, were pretty well described with our model.

In particular, the sharp decrease in water potential with depth at the base of the Tiva Canyon, the more or less constant water potential with depth, minus 40 to minus 80 meters, the gradual increase in water potentials at the base of the Pah Canyon and the local water potential maximum, all captured fairly well with our model, which encouraged us, although we are sobered by the fact that the water potentials we predict are too negative by a factor of two.

We chose to use this hole as an opportunity to further calibrate our regression relations, and because water potential correlates inversely with alpha, the alpha parameter, by simply multiplying by a factor of two the alpha parameter predicted by our regression, we were able to produce the matched with the measured profile that you see here.

In two of the three remaining holes, the fit between our predicted and the measured water potentials was also much better when we adjusted our alpha parameter and this value, although at UZ-7, the agreement with the measured data wasn't good when we used the adjusted alpha parameter.

So we've predicted the water potentials reasonably well with our regression relations that we developed in our first step of our process.

We then used the same van Genuchten equation to estimate what the effective hydraulic conductivity versus depth would be at UZ-5. I'd just point out the overall trends. The hydraulic conductivity at the base of the Tiva Canyon is very high relative to the Yucca Mountain member. We see that it remains fairly low. The effective hydraulic conductivity is the hydraulic conductivity at the existing water saturation and it's listed here in meters per year.

At the base of this bedded unit at about minus 55 meters, we see a sharp increase in effective hydraulic conductivity and a decline, and then a gradual increase in effective hydraulic conductivity that reflects the increasing saturation. We also see that at the top of the Topopah Springs, there's also a large zone down here that has high effective hydraulic conductivity.

We then used that measured hydraulic conductivity that I've just showed you in combination with the predicted water potentials to estimate what the flux through this borehole was. Two things I'd like to show you in this; one is that the estimated liquid flux in the moderately to densely welded portion of the Tiva Canyon is orders of magnitude lower than it is in the non to partially welded base of the Tiva Canyon. And because the fluxes that I list here reflect only the fluxes within the matrix, we interpret this as meaning that a lot of non-equilibrium fracture flow has occurred in the near surface fractures that is not reflected in the water saturations here, and that at the base of the Tiva Canyon where the fractures die out when the degree of welding decreases, we see a transition from fracture flow to matrix flow and that the difference between the two values of flux that we see in this figure, therefore, is indicative that a lot of non-equilibrium fracture flow has occurred in the near surface densely welded and fractured rocks.

On this figure, the negative water fluxes are downward fluxes, and the upward fluxes are indicated by positive values. We see that there's a lot of small scale reversals in this profile that are undoubtedly a consequence of experimental error and uncertainty in our estimates of the alpha and beta parameters. However, we believe that some of the major reversals are indicative that lateral flow is occurring. For strictly one dimensional flow, you would expect that flux profile would be reasonably continuous, however, we see not only these jumps within the bedded units, but actually also reversals at certain horizons.

We also did the same analysis with the smooth water profile, the fifth order polynomial fit. And, again, because this profile was somewhat more smooth, we don't see the reversals in flow direction, but we still see that we obtain fluxes on the order of several meters per year downward at the base of the Tiva Canyon, and that there's another sharp spike in downward flux at the base of this middle bedded unit, and also that there seems to be a lot of lateral flow occurring either within or just above this bedded unit at about 100 meters depth.

We said earlier that what we desire is that complementary methods of estimating the flux yield consistent interpretations. In this case, this was so. Al Yang had published some tritium data a few years ago that showed relatively high tritium concentrations in UZ-5 at the nonwelded base of the Tiva Canyon that are consistent with our interpretation based on hydraulic evidence that a lot of non-equilibrium fracture flow has occurred in the near surface rocks.

What the tritium distribution at UZ-5 doesn't show but the tritium profile at nearby UZ-4 does show is that there is also a tritium peak within the bedded unit at about 55 meters depth. Unfortunately, at UZ-5, no measurements were made at that depth. However, the measurement at nearby UZ-4 is consistent with our interpretation that a lot of lateral flow has occurred within these units. So this cartoon represents a conceptual model for what's going on there.

Water flowing down the channel following major rain storms enters a fault that's yet to be identified but is surmised to exist on the basis of some offset in strata across the wash, that it's absorbed preferentially by certain units which, because of their accommodation of their capillary and the permeability properties, have a larger tendency to absorb water. And locally at least this water moves laterally along these beds and supplies water to both the underlying and overlying units.

So the conclusions that we got from this study are that we established important statistical correlations that allow for augmentation of existing hydrologic data sets and constrain parameter space in numerical models.

At present, we have to admit that our estimates, because our estimates of hydraulic conductivity versus saturation are imprecise, the estimates of flux that we've made are subject to large uncertainty and potential error, although they are useful as qualitative indicators at this point of large flux versus small flux.

We also say that the calculated flux profiles for the boreholes within and adjacent to the alluvial channels indicate that past recharge has been high, possibly on the order of several meters per year, relative to previous estimates of the average flux over the site, which have generally been 1 millimeter per year.

We don't see that this, based on conversations with Alan, don't see that this is necessarily inconsistent with the average flux estimates of 1 millimeter per year, because the alluvial channels represent a very small fraction of the surface area of Yucca Mountain. And so although flux is locally high, when weighted by its surface area, it still may not cause the average flux over the site to be much more than 1 millimeter per year.

The calculated flux profiles display systematic trends, including large-scale reversals in flow direction near the air-fall units, that suggest the occurrence of lateral flow.

The implications are that, as Alan pointed out, we do need to understand the microstratigraphy to understand the observed saturation profiles, and that flux within the upper part of the unsaturated zone is neither one dimensional nor steady state, and that numerical models that don't account for multi-dimensional flow and transient behavior won't be able to reproduce the observed profiles.

Conclusions are that fracture network models are going to be important in helping conceptualize flow through unsaturated fractured rock and that we need a very multi-disciplinary approach and complementary methods to constrain our estimates of the percolation flux.

Thank you.

DR. DOMENICO: Thank you, Ed. I have a question.

UZ-5 that goes up to the Topopah, but you did look at four of them, did any of them penetrate the Topopah member?

MR. KWICKLIS: Any of the four boreholes?

DR. DOMENICO: Yes.

MR. KWICKLIS: They got several meters into the Topopah and were terminated in the nonwelded part, I think.

DR. DOMENICO: Were you able to reproduce what you measure in the Topopah by similar methods?

MR. KWICKLIS: The water potentials that we estimated in the top of the Topopah were always too negative, and in some cases, these rocks had very little porosity. And where the porosity is low, the relative error in estimating what the saturation was is potentially much larger than for very porous units where a small error results in a small change, and gravimetric water content results in small change in saturation. So in the tighter rocks, the more low porosity rocks, our estimates of what the water potentials were weren't as good as for the more porous rocks.

DR. DOMENICO: I see.

MR. KWICKLIS: So our agreement there fit less well than for the overlying unit.

DR. DOMENICO: My last question is how do you factor in Ed Weeks' idea of an upward flux in that base when everything we've seen there is going down? Is that a net flux we're looking at?

MR. KWICKLIS: Well, certainly modeling that's been done within the project has shown that the potential for air circulation in the upper part of Yucca Mountain exists, and that air circulation has a potential to remove water.

DR. DOMENICO: But that is not part of--

MR. KWICKLIS: That was not a part of our analysis. We confined our analysis solely to the liquid phase.

DR. DOMENICO: Very good. Any board questions? Ed?

DR. CORDING: I had a question regarding your comments on the testing of the ESF. Do you have a summary, or are there summaries available for the work done, unsaturated result of testing within the ESF?

MR. KWICKLIS: I know that that study plan has been developed in phases and that certain portions of that study plan are complete. And certain portions, such as the testing at the main test level, are still being developed. Although we have a good idea of exactly how the tests are going to be conducted, we haven't yet received any request to formally submit that information to complete that study plan.

DR. CORDING: I know some of these plans need to develop as experimenters get ready for them and all, but the underground facility obviously is going to change from what the SCP envisioned, but I was interested in what sort of updates there are on how that's being utilized. You described the way you're going to be using some of those.

MR. KWICKLIS: There is something called the test planning package that we submitted information towards a couple of summers ago, and as far as I know, that was the last bit of information that we submitted on our test design and what the tests were intended to accomplish. And maybe someone else in the room would be better prepared to tell you what the status of that plan and document is.

DR. CORDING: So at present, you have ideas that aren't official plans yet, but go, I assume, well beyond what you had maybe two years ago?

MR. KWICKLIS: Well, actually, the ESF funding for prototype testing within ESF has been a fairly low priority in the last couple of years, and not all of the tests have been funded for development. We anticipate that that's going to change somewhat in the next fiscal year. We have continued to do planning, though, on some of those tests, although not a lot of funding has been available for equipment development.

DR. CORDING: I'd be interested in any sort of summaries or documentation that is available and some guidance as to where that sort of information is being developed.

DR. DOMENICO: Any other questions? Does staff have any questions?

Thank you very much, Ed. We can get on now on three dimensional site-scale model of the unsaturated zone by Bodvarsson from LBL.

MR. DYER: Dr. Cording, I believe the Board already has test planning package 91-5, which lays out all of the test plans for the ESF. That's being updated now. We have, starting in '93, put considerably more emphasis in putting together the ESF tests, so there is more resources being allocated to getting those tests in place, and we're bringing those on line as necessary. The original emphasis, as you know, as we reported at the November meeting, the tests that we had to get in place were the ones that we had to have in hand to follow the TBM-3. So emphasis has been put on developing those. The other study plans will be brought on line as necessary, and as soon as we get something beyond the 91-5 planning basis, that will be forwarded to the Board.

DR. CORDING: That 91-5, you summarized much of that in the November meeting, as I recall.

MR. DYER: That's right, test planning package 91-5 was the outline of all the tests that were for the ESF. It was originally put together a couple of years ago. It's been modified over time. We've been through several revisions of that test planning package. That shows how all the tests hang together and what the entire universe of underground tests are, and there's more details in some than in others at the present time.

DR. CORDING: I think some of those details will be of interest to us as they're developed.

MR. DYER: Yes.

MR. BODVARSSON: Good morning, Ladies and Gentlemen and distinguished Board. It's a pleasure to be here. I'm here at the request of Larry Hayes of USGS, which I work very closely with. I'm Bodvarsson from Lawrence Berkeley Lab, and I'm also here at the request of DOE, the Department of Energy.

I'm going to be talking about three-dimensional site-scale model of UZ flow at Yucca Mountain, which is a joint USGS and LBL cooperative effort, and I appreciate the help of the slides that Lynn Hoffman provided and Jeanne Cooper. They helped me with the management slides. They didn't censor them at all; they just changed them.

No, actually when you come from a different country, you don't appreciate them correcting your English.

It's okay if they rewrite; that's fine. I'm just joking. They didn't change anything.

I also appreciate the help of Alan Flint who took out all the technical graphs. He had them yesterday, they were his, but they were actually mine.

Before I start, I would like to use this opportunity to make a couple of points for two minutes. As you know, when you give a talk in front of the distinguished Board and wear these kind of clothes, I mean there are always some people that go one step too far, like at the Oscars, you remember the streaker that ran across, or at the last Grammys they were talking about the Haitians that had AIDS and shouldn't go into the country. So I just wanted to make a couple of comments close to my heart for two minutes.

One is that in the last couple of years, these kind of meetings are getting more and more interesting because we are seeing so much interesting data all of a sudden coming from the project. And just looking at the results yesterday on the water level in UZ-16, it's really exciting to me, and Alan Flint's statement and all of the data. I mean, this is a total change over the last few years when we used to stand up here and talk about normal results without any data.

And also what Carl Gertz talked about--in the next couple of years, it's going to be extremely exciting. So this is very good. But I also share a little bit the viewpoint that Don raised yesterday about you have to be careful in this project not to lose the scientists that have worked on this project over the last fifteen years, ten or fifteen years, and just looking at home at LBL, there are some very good people like Joe Wang, Karsten Pruess, really developed a lot of understanding on these processes that are not fully funded in this area, and I'm sure there are many other places too, like you mentioned. So this is a little concern to me.

Another little concern to me, too, being a code developer myself, I've developed numerical codes, is that you have to be really careful when other people use your codes and tell you how you should use your own code, because in many cases, they don't know how to use your own codes. You know how to use your own code. And just one example--I mean, you talked about that the TOUGH code couldn't handle the non-equilibrium fracture matrix flow, or that's too strong absorption for the water to go into the matrix. I would like to convince you that the TOUGH code as it is now without any modification can give you any degree of coupling between the fractures and the matrix. So afterwards, I will be glad to tell you exactly how to do that.

So just a couple of comments that we feel that we were involved in this site characterization modeling, Alan, myself and many other people that developed these codes, we feel that we're fully capable of selecting those codes we think are most appropriate for the job I'm going to talk about now.

So site-scale model of Yucca Mountain coworkers. Most of the people that really did the work, we are very fortunate, this is a cooperative project between USGS and LBL, Sandia. We also interact with Los Alamos and other people on this cooperative work.

Let me give you a overview of what I'm going to talk about. I followed the letter and request by the Board that said that we don't need any details, technical details, we want to know your approach, your general approach and how you handled things, and that's what I'm going to be talking about. I'm going to be putting a lot of emphasis on how we integrate the unsaturated zone studies at the USGS and LBL, how we put together these models, and then I'll briefly talk about some of the results and why this work is credible and what we're trying to do in the future.

If you have any questions during the talk, please don't hesitate to raise them.

Why a model? Why are we doing this three-dimensional model of Yucca Mountain? The reasons are very simple. We have to have a model to integrate all the available data and information at Yucca Mountain. We have to have this model to provide estimates of moisture, heat and gas within the mountain, and also to help us in the site characterization effort, because this is very important, when do we have enough data or when do we need more data.

Also, with respect to some of the important issues raised today that concern infiltration and climate changes, we hope to use this model to study the effects of spatial and temporal infiltration at the mountain, and also to predict the effects of future changes to see how this affects moisture flow, heat flow and gas flow within the unsaturated zone.

The handouts that you have, unfortunately I went through them last night, they are not quite in order in some cases, so you might have to skip a couple of slides or find them a little later.

The general approach is the next topic. And here, I want to introduce the site. You all know the site. But I want to show you where our model area is. This is, of course, Nevada and here's Yucca Mountain, and now we are going to zero in on the model area.

This is the model area that we considered for the three dimensional unsaturated zone study. Here, you have the repository in yellow. It's bound to the east by the Bow Ridge Fault and to the west by the Solitario Canyon fault. It's bound to the north by Yucca Wash. Actually, there is no direct evidence for a fault there, but this is the boundary that we chose. And then we went far enough to the south so that the results would not be affected with what is happening in the repository zone.

So basically, we feel for unsaturated fluid flow, that this model is large enough so that the results we get from the central part where we're most interested are going to be accurate.

How are we going to do this? First, I'll tell you a little bit about the general approach, and then I'll talk about some of the important issues that we have to consider when we do this kind of modeling, and finally end up with some of the modeling steps and how we develop these things.

This is similar to what Dennis Williams talked about yesterday and Tom Statton. This kind of shows how the site-scale model here is supported by all these activities that we are talking about in unsaturated zone research. What we have here is a data collection and analysis, and these are Joe Rousseau's and Alan Flint's and John Stuckless' and all those people that generate data.

We have to have conceptual models, and that's what Jean Younker told us about and Linda, and so we have to integrate this data into some kind of conceptual model. It's a very important step.

Second, and also a very important step that Jean also talked about, is that we have to have the proper numerical codes, not only have to handle three dimensions, they have to have the right processes and the right kind of physics in them.

All of this feeds into the site-scale model. This is the input paper, this is the mathematics. Along with that, also are the sub-models that Ed Kwicklis talked about. These are the ones that we have to use to address specific issues. For example, what if we have concentrated flow or focused flow, as Linda talked about. How are we going to incorporate that in this complex three dimensional model, how is it best to do that, and we preferred to use sub-models to do that because it's too expensive and time consuming to do it in a very complex three dimensional model.

Also very important is the uncertainty analysis or stochastic modeling to make sure that we know how reliable is our model or how incorrect is our model.

Finally, this is also extremely valuable, and this is why we are here today, one reason, is peer review, is to tell us and make sure that what we are doing here is correct, it feeds back to the data. If we need more data, we have to tell the data collectors we need more data, and tell them that the model really relies on more data in certain situations.

Now, what are some of the issues? When we start to model the mountain with a complex model, we have to be aware of all these issues. Linda talked about uncertainties in flux determination, and Ed Kwicklis and Alan, this is very important. They talk about how many fractures we have. There are millions of fractures and matrix blocks. We talked about the faults. We don't know the characteristics of the faults. The USGS has several strata plans that address this, and this is very, very important for our model, as I'll show you a little bit later.

Matrix versus fracture flow. We have to consider gas flow, thermal effects, lateral flow and capillary barriers. All these complex phenomena have to be considered in a model. Now, how are we going to do this model? We do it of course, as Alan Flint says, right. Isn't that what you say, Alan?

But besides that, we do it in steps because we want to do it as easy as we can. We want to start with the moisture flow model, and this we have developed already. What does that mean? That means we only solve for the water flow. We don't worry about the temperature effects or gas effects in the beginning. We want to make sure that we can handle water flow in three dimensions of the entire mountain. Very important.

Then we incorporate the geothermal gradient and the gas flow components to address concerns like Ed Weeks has about gas flow shallow, how that effects vapor flow condensation, heat flow and all of these coupling effects. Very important.

We anticipate, and the way we have laid this out, is that we will calibrate this model periodically. More importantly, as has been pointed out several times, we will use it to predict the state variables, that means capillary pressure, saturation, temperatures, gas pressures, for all of the new wells that will be drilled at Yucca Mountain in three dimensions, just like Linda was doing with her one dimensional exercise. We want to do this, and hopefully when the last well is drilled, we will have predicted very accurately what we will get in that well.

And, of course, periodic use of the model for sensitivity studies to tell the performance assessment people how sensitive the model is and when do we need to collect more data. And then use of said models for hypothesis testing.

Next part; data needs, contribution from other studies. What data are essential? I'm going to talk a little bit about the contribution from other studies. I'll tell you a little bit about hydrogeologic maps of the different units, and then some of the important hydrological parameters.

Just to put this up here to give credit to Scott and Bonk, they did a marvelous job in mapping the geology, but it's very complex because you have heavily faulted areas in the unsaturated zone with welded and unwelded units.

All these studies that DOE, USGS and all the major participants have put together will contribute to this study. The emphasis today is on infiltration, climate changes and infiltration. As Dwight Hoxie told us yesterday, climate changes that John Stuckless talked about and Thompson is going to talk about later, we represent--so we are going to have a three dimensional model and we're going to have a three dimensional spatial distribution and infiltration as well as temporal distribution.

All the other studies are also very important. You take the geological framework by Rick Spengler and by Mike Chornack, fracture properties, matrix properties. This, Joe Rousseau's work is extremely important in calibrating the model because we want to predict how the pressure, capillary pressures, air pressures, temperatures, are going to be at the next well we drill.

Hydrogeological thicknesses of units. We spend a lot of time pouring through all the literature and all the maps from the walls to get the height of the hydrogeological thicknesses of the units, not hydrogeological thicknesses, but the height of the geological thicknesses, which is the thicknesses of the units which pretty much have the same hydrological properties.

This just happens to be the Topopah Spring member. And you will note this tremendous three dimensional effect just in the thickness of the repository unit. It's very thick here close to where the repository is, and then thins out very rapidly in all directions. This will have major effects on the moisture flow, gas flow and heat flow.

Next part. How are we going to do the numerical modeling? That in itself is not a trivial situation at all, because we are talking about very highly non-linear problems with thousands of gridblocks and connections.

First of all, how are we possibly going to grid the mountain. The mountain is large. How are we going to do that? What factors control the horizontal gridding? And if you know numerical modeling, I'm not going to do into details with this, that means whatever you're interested in, you divide it into little volume elements. But you have to divide it into the right amount of volume elements, because where you have large changes in properties, that's where you have small boxes, so to speak.

So think of it as this room here. You can divide it in as many or as few boxes as you will. But the accuracy depends on how you do it. So I'm going to talk about some of these. You have to include the faults and fractures. We have to be flexible in a grid modification. This is extremely important because DOE funded us to work on this grid for a whole year, and if then all of a sudden we change the geology or we change the locations, I'm not going to ask

DOE to fund me another year. I want it to be computer driven so it's flexible enough that changes can be readily made. That's very important.

So horizontal grid. Example, this is what we came up with at this time. How did we come up with this? It looks kind of--all this like a spider web or whatever you want to call it. There are purposes of this. What are the purposes?

First of all, we want to have grid flux located where existing wells are so that we can calibrate the model against all the data that we have already. We also want to have the grid flux located near a proposed well site because we want to be able to predict what the conditions are at those wells. Very important.

We want to be sure to have the grid aligned along major faults, Ghost Dance Fault. Why is that? If this fault becomes important, we want to make a sub-grid along this fault here so we can represent it in as much or as little details as is necessary.

We also want to make sure that the grid is consistent with infiltration. And here is a map that Alan Flint and Lorrie Flint did. They helped me and us at LBL develop this grid. And you see the grid flux align along some of the alluvium--that some of these zones are alluvium zones, the ridgetops. So the grid aligns along different infiltration focus areas, so to speak. Now, that's all the details I want to tell you about the horizontal grid.

Vertical grid; also very important. Why is that? Because of the faults. Faults may play a key role in the Yucca Mountain, and we have to understand them. What do we want to do about the fault? This happens to be a cross-section from northwest to southeast. We have to take care of these offsets. These offsets may be very important. This is the Paintbrush unit. You may have discontinuous flow along here because it can't go through the fault, or you might have rapid flow down the fault. You might have perched water here. You have to take into account the faults, and we have done that with all the faults in the model.

This distribution here just shows the major units, the Topopah and the Paintbrush and the Tiva. But then if you have those evaluations like Alan Flint has and some of the others, this shows kind of the divisions, so to speak. And I'm not sure you have this view graph. Don't look too far if you don't have it because I made some errors when I sent it to Lynn. It's not her fault; it's my fault. So here you have contrast and permeability; it's very important.

Flexibility of the grid. This was our number one priority when we developed the grid. We wanted to be near completely computer-generated in case that we have to modify a grid. It's very, very important.

I'm going to skip some of the development and new simulation results because they go just to some of the results to date, to save time since we are behind schedule.

What have we learned so far? I'm just going to talk briefly to you about the two-dimensional simulations and the effects of major faults and other important issues, just illustrating the use of this model through cross-sections.

This you don't have in your packets I know. This just shows the grid. It's computer-generated, as I said, so you can specify any cross-sections you want to look at in the mountain. And we happened to look at these two. You see B, B prime here, it goes through Ghost Dance Fault, and this one, D, D prime here goes through two of these, Abandoned Wash Fault and some of the other faults, so you have two faults there. And I'm not going to go into details with these simulations because I know it's boring to most of you, although it's extremely exciting to myself.

The only thing I want to show you here is that when we develop this complex grid like this, you have to make sure the grid is correct. If it is not correct, then you can have all kinds of errors. And the way you do this with saturated flow, you run it without any fluxes, and if you don't get hydrostatic pressure profiles, you don't have a good grid. Your flow should be--the same thing we do in the unsaturated zone. We put a water table at the bottom and if you don't have capillary pressure following Pgh, negative Pgh, the distance from the water table, your grid is horrible. But if the grid is good, you see an equal distance on the water table, capillary pressures are uniform, meaning that even though it's fairly coarse, it's representative of all the actual three dimension situations. And we did this for the entire grid. So this is just to show you that we actually check all things. You may not believe it.

Linda told us a lot about the capillary pressures of liquid saturations, and the only thing I wanted to tell you is that these one dimensional simulations that you did there with the matrix flow, you don't have to run the simulator. You can calculate them by hand. They all depend on this curve, so you can just go into the curve and read them off.

You have to take into account fracture flow, and I don't want to bore you with details because Ed already did. What this shows is just simply that when you have large capillary pressures, the matrix likes to keep all the water there. But when you have low capillary pressures, you go into what is called a fracture flow and much higher permeabilities. And Linda talked a lot about that, too.

So we select these curves. These curves come from Alan Flint's measurements of some of the transect samples.

Example 1. A major problem today now is that you don't know the flow characteristics of the faults, and this is going to be a critical issue in the site characterization. And I'm sure we're going to address that. Various tests are planned to address that situation.

What we do in this simulation is we prescribe a flux at the surface, uniform reliable flux. In this case, it's uniform flux just to illustrate an example. What we like to see then is how this flux here changes the capillary pressure distribution in the mountain, how it affects the saturation distribution in the mountain, and very importantly, these two things, one, how much water is going through our repository unit at Topopah, two, how much water is concentrated in the faults. So what we like to map is the spatial variation of the flux at the bottom, even though you have a uniform one at the top.

What this shows here is that when you have a fault which is very permeable and you obey capillary pressure loss, you will find that the capillary pressure curves of the faults are such that the water doesn't want to go into the fault. It's so light it wants to stick in the matrix blocks. That's why you see here low capillary pressures, that means the water doesn't want to go there. It wants to stay in the fine grained rock material and the flow wants to go vertically down. This is for a very low infiltration rate.

Saturation. We see saturations similar to what have been measured by Alan Flint and others. We see low saturations in the Paintbrush units because this is a very permeable unit and it's a porous medium unit, so it doesn't have to be very saturated to conduct a lot of flow. We have fairly high saturations in the welded units, Topopah and Tiva. And then we have fairly high saturations also in the Calico Hills, almost saturated, and this is what Alan was telling us yesterday and Joe Rousseau. And within the fault for this assumption for a highly permeable fault like a parallel plate, the saturations are extremely, extremely low. the water doesn't want to go in there.

If we increase the flux, let's increase the flux .1 millimeter per year. Now we are getting close to anybody's guess. Does anybody here know what the flux is? Are we getting close; do people think it's maybe .1 millimeter or something like that? The situation gets clearer. You're now getting higher saturations in the Tiva Canyon, close to 80 or 90 per cent saturations. You have fairly high--no, these are capillary pressures, I'm sorry. Capillary pressures, since you put more flux, the capillary pressures are much lower, except for here.

Here, we have a very dry out zone that would be nice to measure in the field. Why is that? It's because the water comes here and it flows laterally in the bedded units and then concentrates here because it wants to flow there because of the bedding. So if we see this in the field here, you have much higher capillary pressure, 25 here rather than maybe 10. We should be able to see that in the field data very easily. And this should give us a very strong indication of lateral flow.

In this case, saturation shows the same trend; high saturations in Tiva, high saturations in the Topopah, especially in this low permeability unit here in the middle. Similar things happen here; lower saturation, high capillary pressures.

One step higher; .5 millimeters per year. Now we are starting to induce fracture flow in some of the units, like in the Tiva Canyon and in the Topopah units. The lateral flow becomes more and more concentrated flow here, but still you have this zone here of very high capillary pressures. Flow vectors show this very well, too.

You might prefer this horizontally maybe. Here, you see here in the flow vectors again significant lateral flow here in the Paintbrush unit, less flow than through the Topopah, concentrated flow here.

Now, the important thing about these things, like I said before, is what happens close to the water table. Where is the flow going? How important is the lateral flow? This is at the water table. Imagine you're sitting at the water table and you're seeing the water particles come by. This is the best set of them all. This is the east side of the model. And you see, because of the lateral flow, this is just normalized by the flow going in on top. So here, you see much less flow is going there because the lateral flow, most of it is flowing close to the fault. Same in this unit here; more increased flow there.

I'm going to just really just show you you can do this for any cross-section you want. You can count several flows, like the Abandoned Wash Fault and Dune Wash Fault. This happened to cross-section D, D-prime. But also in this case, it depends strongly on what you assume for the fault properties.

So I'm getting close to finishing here. Again, I want to emphasize that what you were after in this meeting is how we are going to deal with the infiltration and climate changes. We have written the study plan, Ed Kwicklis and myself, that we hope to do sensitivity studies on the effect of moisture flow, lateral flow and different infiltration rates, look at past climate changes as well as future climate changes, and how that's going to affect the moisture flow and the gas flow and the heat flow.

Current work. What are we doing now? We are writing up a large report describing the model for everybody to use and look at. We are looking to make sure that the fineness of the grid is sufficient to give us reasonable results. We are doing the 3-D simulations with just the moisture flow.

Where are we going? Future work. After we have satisfied ourselves that we can run this in three dimensions and this is going to be a valuable tool for the Yucca Mountain, we incorporate the geothermal gradient, the gas flow and hopefully development of a predictive capability. Because if we don't have predictive capability, this exercise doesn't mean anything. And then look at the model sensitivity.

Credibility of the study. I can tell you, I mean I can say this work is right, like Alan does, but you may not believe me. So I thought I would tell you that DOE and USGS and LBL, who I work for, put together various things to at least try to make sure there is some credibility to this work. Ed Kwicklis and Mike Chornack and at USGS, we have our quarterly modeling meetings where we review all the models and all the data acquisition every quarter and make sure that those data are fit into the model and that the model results that I then get make sense, given the data received.

Every year, we publish in the International High Level Radioactive Waste meeting and in refereed journals. We feel that this is very important to get the peer review on our work. Also, we participate with great eagerness in the periodic peer review. We have our internal LBL peer review, we have the peer review with USGS, we have the DOE peer review and panels, NRC, Nuclear Waste Technical Review Board. And all of these are very constructive. Then the documentation; all this work goes through USGS quality assurance program.

So final slide, and you must be happy as heck. The three dimensional model is under development, and you hope probably it will be developed soon so you don't have to hear about it any more. The major purpose of this model is to integrate all the available data and guide in the site-characterization process.

We will certainly use the model to address the important issue that the Board has raised today and yesterday, the issue of what is going to happen if we have climate changes and the infiltration rate is going to be much different from what we have today.

So if you have any questions, I'll be glad to attempt to answer them.

DR. DOMENICO: Thank you very much, Bo.

Are there any questions from the Board members?

DR. ALLEN: Clarence Allen speaking. It seems that the more detailed geologic mapping we do at the site, the more faults we discover. Now, admittedly, the ones with the large displacement, such as the Ghost Dance Fault, presumably are identified. But isn't it possible some of the minor faults might still be something of great hydrologic significance, and that in a sense, your model may be grossly over simplified?

MR. BODVARSSON: Yes. I can answer the question yes. Your point is very well taken. If we don't have a very important flow path in the model, one that we haven't characterized, there's no way I can say the model is correct, no. This is a very good point. But on the other hand, if we find, and we may very well find this, that faults are not important to moisture flow at Yucca Mountain because the water wants to flow in the matrix, it doesn't want to go into these faults, then maybe the model will be fine.

Another way to answer it also is that in all our studies there are uncertainties and we have to bracket those uncertainties and we have to get confidence in the model by seeing what if we lose a pathway here, what's going to happen. So by doing these what if exercises that Carl talked about in his presentation, should help us say that if there's a very active flow pattern in this region of the mountain, we have to know about it. But if it is in this region of the mountain, it is not so critical. But your point is very well taken.

DR. NORTH: I'd like to ask a follow up to Dr. Allen's question. Supposing you found another fault of strong significance, how difficult is it to put it in your system?

MR. BODVARSSON: It's not very difficult. It's a very good question, too. You remember, I emphasized that we spent a lot of time making this computer generated, and that's not a trivial task, because it's very complex horizontally, but even more vertically. It's really complex vertically. So to answer your question, we have achieved that, so to put in another fault and change the grid there probably would take one week of work, and I don't think that's too much. Whereas, generating this grid as a whole and computer software that goes with it took about a year or more. I have to admit it took me a long time.

DR. NORTH: Just as a calibration for those of us outside the field, has anything like this ever been done at this scale to model the unsaturated zone in three dimensions with this kind of detail. Are there other similar models of this type that have been applied in other areas, or is what you were doing virtually unique?

MR. BODVARSSON: I would say that what we are doing now has never been done before. I would say that. And if somebody wants to say differently, don't. Do you want to say something?

MR. PAHWA: Suresh Pahwa. I think in the oil industry, models with these many grid blocks and fracture matrix maybe have been applied.

MR. BODVARSSON; I have to disagree with that. I worked in both the geothermal and oil industries, and the reason that this is so difficult is because the severity of the non-linear terms, and these are non-linear flow with a strong capillary pressure gradients. I worked on multiplex flow in the oil industry, as well as geothermal, and those problems are not as highly non-linear as this one is.

MS. PAHWA: Now you're getting into the second level of detail of the particular data function and so forth. But three to four phase modeling has been done for very large scale for thousands of grid blocks.

MR. BODVARSSON: I know that I have been in part of those, but this is much more difficult, in my view.

DR. DOMENICO: Any further questions from the Board members?

DR. CORDING: Just a comment. When you get into two dimension flow models, you could get into more details, you can put more nodes in in the two dimensional scale, and there are certain things that one can do in a model like that that you may not be able to do in a 3-D model. This was, to some extent, a question, but it seems to me that you have opportunities from the other modeling efforts to utilize that information within your 3-D model, which may not be able to model all of the features that are being learned or observed in some of the current, say two dimensional type models. And so I'm just interested in how you saw those other model efforts fitting into this and even using portions of the things that you find from that as a calibration for portions of your model.

MR. BODVARSSON: Your point is very well taken. What I mentioned, and I was trying to find that view graph, is that you have the site scale model here, which is kind of cumbersome, three dimensional thing that takes a long time to run, but then we have what we call the sub-models, which are the hypothesis models that Ed talked about.

Now, we will use those models as much as we possibly can. For example, to look at what happens if we have focused infiltration or preferential pathways in two dimensions, we want to investigate that as thoroughly as we can in two dimensions before we put it into the three dimensional model and make sure that we have to put it in before we do. So your point is very well taken. We want to use the two dimensional or one dimensional model as much as possible, yes.

DR. DOMENICO: Have you made any plans or attempts for any validations with regard to Flint's data? And how are you going to handle that in the sense that you don't know the initial conditions, the problem with time that Linda had; do you expect to have similar problems?

MR. BODVARSSON: Yes. My feeling on the problem of time is that there's only one possible solution to that, and this is what we do very routinely in geothermal applications, for example, is that you assume the system is in a steady state. That way the time factor for the initial condition goes away. We have found this to be very satisfactory in geothermal modeling applications, even though some of these systems might only be 1,000 years old, much younger than what we have here.

You had a two part question. I forgot; there was another part.

DR. DOMENICO: I asked if you had made these attempts yet in terms of validation.

MR. BODVARSSON: The validations, now about UZ-16, that is exactly what we plan to do, is we hope to predict the profiles like Linda was doing with the wells before they are drilled. We hope to get better and better results with our predictions as we go along, and that, we feel, is the best way to validate the model or to see how good the predictive capabilities of the models are.

DR. DOMENICO: Has the model been able to reproduce what you see there already?

MR. BODVARSSON: In UZ-16?

DR. DOMENICO: Yes.

MR. BODVARSSON: In some cases, yes, for some, where we have the high saturation in Topopah and Tiva Canyon. We haven't explicitly started to do that exercise for UZ-16 because we are just trying to put the model together now. But hopefully the UZ-14 is going to be drilled to predict that.

DR. DOMENICO: How long do you think it will take before you couple it with the geothermal gradient and the gas flow elements?

MR. BODVARSSON: Six months, I hope.

DR. DOMENICO: You think in six months you'll have a complete--

MR. BODVARSSON: Not complete. That's where we hope to start really the incorporation of the geothermal gradient. See, what we are doing also, like this gentleman here pointed out, is that before we put, for example, the complexities of the gas in there, we want to make sure to look at two dimensional or cross-section to see how important it is in different levels. We know it's important in Tiva Canyon because that's measured, Ed Weeps had measured a very important gas flow. Maybe it's not going to be very important in Topopah Springs and some of the lower members, so we want to, instead of just jump up and put all these processes into a very complex model, we want to try to proceed more slowly like we do with the grid to make sure that we are doing it the right way.

DR. DOMENICO: Any questions from staff? Leon?

MR. REITER: Leon Reiter, staff. I wasn't quite sure about several features. Solitario Canyon, I assume, is a boundary condition?

MR. BODVARSSON: Yes.

DR. REITER: Well, what about the imbricate fault system? That appears to be a feature that I didn't see listed there, and we know that exists.

MR. BODVARSSON; Oh, the smaller faults?

DR. REITER: Right, yes, that are on the eastern edge of the repository.

MR. BODVARSSON: Yes. That's a similar question to what this gentleman brought up. At this time in the model, we have explicitly represented only the major faults with the major offsets. This is the Ghost Dance Fault and the Abandoned Wash Fault. And what's the name of the other? Dune Wash Fault. We have aligned the grid in the northern part to take into account the faults there which don't have big offsets. We also have aligned the grid to take into account the faults on the eastern part where we have major faults. So to answer your questions, we can incorporate all of these faults into the models really readily when we know more.

DR. REITER: Do you eventually plan to incorporate the repository itself in the model and also the effects of the thermal load at the repository?

MR. BODVARSSON: It's a very good question again. We believe that this model of course should not quit--certainly somebody, be it me or somebody else, should use it for thermal loading calculations later on. I don't know who it's going to be or whatever it's going to be. We gave Holly at Sandia that Jerry works with, we gave her a computerized version or hydrological maps of all the units because she was interested in using that for the total system performance. So we are already cooperating as much as possible with the performance assessment studies to give them all the information asked and collected. Now I have no plans of using the performance assessment. My job right now is to help develop this for USGS for the site concentration effort. I'm sure later on it will be used for performance assessment by whoever is going to use it.

Does that answer your question?

DR. ALLEN: To a geologist, a big fault is one with rock displacement.

MR. BODVARSSON: Yes.

DR. ALLEN: But to a hydrologist, this may not be a correct definition of what is a significant fault.

MR. BODVARSSON: That's true.

DR. ALLEN: What is the rationalization for saying the, say the Ghost Dance Fault, is necessarily more important hydrologically than some minor feature that may have hydrologic characteristics that could be much more significant.

MR. BODVARSSON: That's true. The rationale that we are using now is that given the fact that we don't know the hydrologic characteristics of any of the faults now, we incorporate those that have the major offsets, assuming that they may be more important. However, when we get measurements that show, for example, that you have water flow close or near a fault, this is a minor fault, but not such a flow with the Ghost Dance Fault, we will incorporate those faults in.

DR. ALLEN: All 200 of them?

MR. BODVARSSON: It depends on how many are really necessary; we can incorporate them in. Also, look at this point I want to make now, too. I may talk very fancily about that we have incorporated the Ghost Dance Fault in there and the other faults in there, but when you look at the grid blocks, they are still very, very large. All we have done is kind of aligned the grid so we can take them into account. So they are not taking into account much more than the smaller faults at this point because we didn't want to waste a lot of time now putting a small feature, you know, when we don't know the hydrological characteristics. So I think we have to wait, use the wait and see attitude and match the data as they come in.

DR. DOMENICO: We're running over and we do have a discussion period this afternoon. So I think we'd better break. Let's take a 15 minute break and try to gather back here a few minutes before 11:00.

(Whereupon, a brief recess was taken.)

DR. DOMENICO: Our next presentation will be by Starley Thompson from the National Center for Atmospheric Research on long-term climate modeling. And let's try to get back on schedule.

DR. THOMPSON: Thank you. For those of you who don't know what the National Center for Atmospheric Research is, it's a research lab located in Boulder that's largely sponsored by the National Science Foundation. It's been there about 30 years, have about 500 employees. And one of the divisions in the center is dedicated to global climate modeling, and that's where I come from.

Topic is the long-term climate modeling. Parts of this overall project, we're in charge of doing the numerical, global and regional climate modeling that feeds into a lot of the modeling you've already heard about yesterday and this morning, is the chart which you've already seen before, and this is where we are up here, the global and the regional modeling.

We have input from the paleoclimate studies that are being performed. We have output to the hydrologic modeling through, for example, the watershed models.

There's the outline of the talk. First, I'll go through our purpose and objectives, then briefly on the value and limitations of predictive modeling, and particularly the kind of modeling we do in the atmospheric sciences, what our current model basis is, and again, this will not have any great technical detail at all. I'll emphasize what our study approach is, what we're actually trying to do and how we tend to design our analyses. And, lastly, I'll touch on where we are in terms of our current status.

The purpose is quite simple to state, although probably very difficult to execute. We want to provide estimates of future climate conditions in the Yucca Mountain region for use in estimating the effects of future climate changes on the hydrologic status. That is, we feed information to the hydrologic modelers.

There are several objectives that have to be met in order to do this. First, we need to establish that our numerical climate models are valid for this particular problem. We're not in the business of validating numerical climate models for general use. That's a major field, discipline, in its own right. We are particularly interested in seeing whether the models, as they already exist, can be modified and used for this particular application.

Then we want to be able to identify future climate scenarios that may impact the repository performance through their effect on the hydrologic conditions. What this boils down to is can we identify potential future climate states that make it a lot wetter in the Yucca Mountain region that it is now.

And, lastly, we want to be able to use our numerical models once we've identified these potential climate scenarios to provide quantitative estimates of what the climate conditions would be like during the next 100,000 years, with an emphasis on the next 10,000. In practice, this distinction between 100,000 and 10,000 is blurred in the work that we do. We don't actually run our models continuously for 100,000 year time series. We actually sample that time space looking for particular states, biased towards those that would be particularly wet. So since we don't actually run the models continuously for that time, looking at 100,000 year span in principal is as easy for us as looking at 10,000 years, and will likely also encompass some much larger climate changes, for example, ice age conditions, than the 10,000 year span would.

There's another picture showing how we fit into the SCP. Here we are right here, the so-called future climate, which is largely, as I said, a numerical modeling exercise. On this side, we have a lot of paleoclimatic activities feeding to us, and then we feed out to the hydrological modeling.

In practice, although this look like a nice flow drive neatly flowing through here, there are lots of other cross-cutting paths through here. For example, there are undoubtedly paths that can bypass the numerical modeling and go to the future hydrology directly if one is willing to assume that the past is the key to the future.

We're on the outline now, and we'll look at the value and limitations of our predictive models. First, we do modeling in order to increase our confidence in the anticipated performance. We like to build models because they sort of serve as an embodiment of all of our knowledge in a convenient form that we can do hypothesis testing with, which leads us into the next one. This kind of modeling to test hypotheses is a very recognized part of the scientific method, certainly in the atmospheric sciences and in the climate dynamics field, which is my discipline. It's a very large part of the activity of the science.

And lastly on this slide, this is part of the key to why we're even doing this at all as part of the project, the climate modeling that is. Namely by doing modeling, it allows us to identify unanticipated effects, unanticipated phenomenological behavior of the system. There's undoubtedly very good paleoclimatic data gathering and assimilation going on for Yucca Mountain, not only for a few thousand years in the past, but going back hundreds or even millions of years, that encompasses a very wide range of potential climate variation. A lot of things have happened over that time.

So one could make an approximation and say that anything that might happen in the future has already happened in the past. Let's just look at the past and use that as our climate scenario, and if the performance is such that no matter what has happened in the past, things look good, then that's a desirable outcome.

However, just looking at the past is no guarantee that something different won't happen in the future. That's where our modeling comes in. With our modeling, we're trying deliberately to identify scenarios that may not have happened in the past, or amplify things that have happened in the past in a reasonably consistent way such that we're really pushing the enveloped of possible future conditions. Plus, there are a lot of limitations to our predictive modeling. This is not an engineering project. We're really at the cutting edge of the science and climate modeling in our analyses, and so there's no guaranteed answers.

Since we are at the cutting edge and our modeling is highly computer intensive, we have limited simulation periods. We also have model uncertainties either due to the coarse spatial resolution we have to adopt in order to solve the problem in a reasonable amount of time, inaccurate representations of physical processes, an important one being precipitation, for example, which occurs on a sub-grid scale in our models and, therefore, has to be represented by approximations or what we call paramaterizations.

Now, let's talk a bit about what our current model basis is. Let me spend just a minute defining climate modeling. Climate modeling is as outgrowth of numerical weather prediction activities that initially got started at the dawn of the computer age in the early 1950's, and for a long time, activities involved trying to simulate the circulation of the atmosphere, actually was a driving force behind computer development is one of the things that drove the development of super computers.

So our models, when we talk about global climate models, actually simulate weather. We have a world that's discretized with a grid. We solve these coupled non-linear partial differential equations which describe the fluid flow on this rotating sphere, then we incorporate all the physical processes that drive the system, the solar radiation, infrared radiation, cloud formation, hydrologic cycle, and that then constitutes what is in fact a weather model. We have an initial state, you initialize the model and then you march forward in small time steps, forward in time for as long as you want or have the computer time to do it.

For example, in our global climate models, we use a time step of about half an hour. So there's a lot of computation that goes on in these models. In our regional model, which has a smaller domain, the time step is down to a few minutes.

These models typically have very fine temporal resolution as a consequence of the numerical solution procedures that have to be used. On the other hand, they usually have very coarse spatial resolution. It's just the way it works out. It's the way we have to solve the equations. So these models have been developed and worked on at various centers; the National Center for Atmospheric Research just happens to be one.

We are approaching the problem with a two-fold modeling basis. First we want to run global climate models and then use their output to focus in on regional climate models. The global model that we're using is developed by NCAR over the last few years, it's called GENESIS, or Global Environmental and Ecological Simulation of Interactive Systems. It's actually been developed as a component of a full-blown comprehensive earth systems model, but we won't be using the full capabilities of the model, only the atmospheric circulation model and component of it.

As I said, these kinds of models have very coarse grids. This has about a 500 kilometer grid spacing. And I'll show you a picture of that in just a minute. We run this model for different climate scenarios that we think globally are conducive to wetter conditions in the southwestern U.S. For example, we know that it was wetter during ice age times, presumably because of the large ice sheet over the North American continent distorting the jet stream patterns.

We run these models, and then these models can be used to feed boundary conditions to a more regional model, a model that has a smaller spatial domain than just the whole earth. We provide atmospheric winds, temperature, air pressure, moisture conditions as literal boundary conditions in a regional model domain.

Here's what the global model domain, the whole earth, and grid mesh looks like. You can run these with different grid meshes, but this is a very standard mesh for running multi simulations. You can see one grid cell is about the size of the whole state of Nevada. This, however, this darker line in here shows the domain or approximate domain of a regional climate model. So the global model calculates these variables and we throw away almost all of them, but right at the boundary, we save the data to feed to the regional model. But we have to run the global model because when you do things like change Pacific Sea surface temperatures or put in an ice sheet, you need the entire global domain to get a global climate picture.

The regional model, or RCM regional climate model, was also developed at NCAR in conjunction with Penn State University, called the Mesoscale Model 4. It's very similar to a traditional numerical weather prediction model. In fact, before he spent the last three years modifying it for climate purposes, that's exactly what it was. It was never designed to be run for more than a few days because it had fixed boundary conditions on the domain.

We have modified it such that it can continuously input boundary conditions from some other source, for example, a global climate model. So now we can run it in climate mode.

We've adopted so far for our experiments a 60 kilometer grid spacing. That's not arbitrary. Through experiments, we determined that that was the largest grid spacing that we could use and still correctly simulate the effects of the Sierra-Nevada Mountains, which was the major contributor to large scale climate spatial variability in the Western United States.

So we can resolve those important topographic features much better than the global model. Of course in the global model with that 500 kilometer resolution, major mountain ranges are just smooth bulges, more or less correct in terms of the large scale atmospheric flow at the global level, but pathetic when it comes to looking at, for example, why Nevada is dryer than Western California.

Lastly, we do include in this model, as well as in the global model, quite a bit of surface detail in terms of computing vegetation canopy effects of transpiration, multiple layers in the upper couple of meters above the soil. We're not intending actually to make use of that I think in terms of the infiltration calculations, but it would be an interesting cross-check. Basically, we're intending to feed the basic meteorological data, temperature, rainfall, to for example Alan Flint, watershed calculations. But the model also does its own calculations, but albeit on a very large scale, a smooth scale.

Here's what the regional model domain looks like, a mesh. It's much finer mesh of course. And the domain is fairly large. This model turns out to be about--oh, about an order of magnitude more computationally demanding than the global model for an equal length of computation time. The mesh is, of course, probably 100 cells covering Nevada.

I should emphasize this is still a regional climate model; it is not a site specific climate model. There will need to be some interpretation between that grid scale down to the specific site level.

When the regional model runs, we normally run it for, say, three to five years of simulation, which is not a very long time, but enough to accumulate representative climate statistics for a particular climate scenario.

These are the output variables, for example, that would be relevant. There's a lot of other things that the model produces, but for example winds at 10 kilometers altitude are probably not relevant to running a watershed model. Surface variables like temperature, precipitation, wind, solar, infrared radiation, and you can even get the model's predicted soil moisture, runoff and infiltration in the top few meters, sort of as a cross-check.

Basically, that detail surface that the model has in it is to maintain self-consistency between the surface interface and the atmosphere.

The output format; basically it's gridded data, 60 kilometer grid. You can get data at everything down to the model time step resolution, probably hourly data would be acceptable, maybe even daily. It depends on what the watershed or hydrology models need.

Now I'll talk about the approach we're going to take to do this. Basically what we envision and what we've been operating under is a phased approach that allows interative evaluation of results as we perform the study and to incorporate developments in climate modeling. Climate modeling is definitely a moving target. It's advancing quite rapidly now. People are very interested in global change, the greenhouse effect, and climate modeling is a big thing at maybe a dozen different national laboratories worldwide.

In terms of planning and controls, we're still operating under a draft study plan. We've never officially issued a study plan yet, although that will be taken care of in the next couple months, certainly by the end of the fiscal year, implement revised quality assurance controls. By the way, I work under contract to Sandia and I use Sandia's quality assurance program.

We're thinking of initiating an advisory board. Since there's not a lot of climate modeling expertise in the Yucca Mountain project, we thought that maybe having a small external board or panel or a group of people looking over our shoulder would be a good idea, especially in terms of, say, selecting the particular climate scenarios.

In terms of modeling, we have largely finished testing the one-way GCM to RCM, the global model to the regional model interface. When we connect the models together, it's only a one-way interface. They do not feed back to each other. So we can run the global model, collect this data, store it away on tapes and read it back in and run the regional model. The primary reason we don't do the two-way interface is that we don't think that there would be a large feedback effect. The regional model domain, although quite large in terms of the Yucca Mountain site, is still relatively small in terms of the global scale and we didn't think there would be that much feedback to justify going into that much greater level of complexity of having the two codes in the computer simultaneously.

We've done a lot over the last couple of years in terms of validating both the regional model and the regional model coupled with the global model for current climates. In other words, we've put in the best available boundary conditions to the regional model and see if it reproduces the current climate. Then we run the global model, compare it to observations, feed the global model into the regional model, see how the coupled system behaves. So a lot of that has been done in an experimental mode.

The next step, large step for us, is to try to reproduce some paleoclimate conditions, for example, put in boundary conditions, insulation conditions, ice sheets, such that we're actually attempting to model what the climate was like 18,000 years ago, and see if indeed the Western United States was a lot wetter in the model. We need to do that in order to confirm that the models are capable of producing reasonable climate changes.

And lastly, which is the meat of the problem, getting into the future climate analyses. There's an important issue that I should try to make clear. As I said earlier, we don't run the model for 10,000 years continuously or 100,000 years. What we do, or intend to do, is to look at all the potential climate drivers that could make conditions wetter at Yucca Mountain. Actually, we want to look at all the major potential climate drivers with a bias towards things that would make it wetter.

So this is the issue of scenario selection. What I mean by scenarios, I mean things like, okay, let's assume an ice age or let's assume that there's four times as much carbon dioxide, let's assume that a super El-Nino occurs that's three times as big as any El-Ninos that have occurred in the last hundred years. Those are climate scenarios. They essentially act as boundary conditions when we run the global model.

So we're assuming that the future climate can be represented as a finite set of states. We don't have to move continuously between them. If we just sample those states, we'll have a good enough look at the future climate, and those selections from that set are called our future climate scenarios.

So how do we make these selections? Well, make them based on things that have happened in the past, paleoclimate, things that are going on now like El-Ninos, current climate, things that we've seen in our modeling before and, you know, general theoretical insights into how the climate system should behave. Our selection, as I said, is biased towards those anticipated to yield greater precipitation in the Western United States, and I've already mentioned some of these things.

And lastly, we need to come up with some reasonable range. I think that should be reasonable number of scenarios, subject to limitations of computer resources. We can't do a hundred different scenarios. We might be able to do ten, in fact, ten is pretty much our list right now, and hope that we span and expand the full envelope of possible future climate states.

Lastly, what is our current status. Over the last couple of years, the regional climate modeling and the global climate modeling have been separated, PNL responsible for the global and Sandia, down to NCAR, are responsible for the regional climate modeling. That's being transitioned now into a single unified climate modeling program run through SNL. We're doing that right now. We need to complete the study plan and get it into place. We're working to implement some improved contract and quality control procedures, especially involving software quality assurance issues with these largely experimental research codes. And we've essentially completed the preliminary evaluation of the regional model with current regional observations. We've got maybe half a dozen publications in the peer review literature over the last three years in the Atmospheric Sciences literature on the regional climate model, and using it coupled to global climate models.

That's all I have to say.

DR. DOMENICO: Dr. Thompson, you may not realize it, but this is your lucky day. Dr. North has informed me that he has several questions.

DR. NORTH: Let me start off by saying, Dr. Thompson, that I'm delighted that you were able to adapt your schedule to be here and make this presentation.

I think it is at least a reasonable hypothesis that your piece in this huge complex puzzle may be one of the most important. We have talked a lot in the course of the last day and a half about the geosciences models, and I think we've seen some very ambitious state of the art efforts to try to gather together the data and understanding of Yucca Mountain and be able to make predictions.

We have talked about driving those models to failure. What is it that would make Yucca Mountain unacceptable? And as far as I can tell, a crude answer to that question is enough precipitation. Now, I don't yet understand, maybe some of you do, how much is enough. I think that's a critical question. But the critical question from the other side is how wet might it get, and that's really the focus of your work.

DR. THOMPSON: That's right.

DR. NORTH: So far, what I see in place is a plan. I'd like you to comment on the scale of your effort as follows. How much funding do you get from Yucca Mountain and the project and how does that compare to the total funding for your group? The third question in the series is how many centers are there within the United States of scholarship in the climate modeling area that are set up to be able to do what you can do in terms of having a formal basis for predicting how precipitation in a region in the United States may change as we look far into the future and with an altered atmosphere.

DR. THOMPSON: Okay, first part of the question is funding. We've been working on this for about three years. Funding levels have averaged about 350K per year, but it's been a monotonic downward trend. I think we started about 400, 375, 280. This year, which is our transition year, I think we're running about 80, and then we expect to go back up next year. No guarantees.

DR. NORTH: How does that compare to the total funding for your operation?

DR. THOMPSON: The Climate Global Dynamics Division of NCAR operates on about a $4 million a year budget.

DR. NORTH: So in round numbers, 10 per cent and going down. Does that include Warren Washington's activities and some of the other models?

DR. THOMPSON: Yes. NCAR as a whole is about 40 million. This, as it stands now, is a fairly small part of my overall program. The majority of my work right now is working on earth systems model development for the EPA, and that's a million dollar a year program that comes into my section.

DR. NORTH: The third part of the question, what other groups are there that are set up to do this coupled global model regional model kind of an approach, and I'll add in the vegetation component, looking at surface conditions?

DR. THOMPSON: Although there are probably a dozen or so climate modeling labs, they all emphasize just the global. We're unique.

DR. NORTH: So you're unique. From the point of view of the project office, you may be the only game in town in terms of coming up with a I'll call it formal approach based on modeling as opposed to direct assessment of expert judgment on how wet might it get. Is that a reasonable summary?

DR. THOMPSON: That's true. It would probably take several years for another group to come up with the capability.

DR. NORTH: Are there any groups abroad that are approaching the problem in this way that might have some more capability?

DR. THOMPSON: No. In fact, the developer of the regional model, Felipo Giorgi who developed this coupling approach, is Italian and he's transporting the modeling capability abroad.

DR. NORTH: So he works with you?

DR. THOMPSON: People come to us to do Australia or do Italy or--Switzerland wants to have the capability.

DR. NORTH: Now, you noted in one of your early slides the importance of the phenomenological effects as a very important output of your work. And let me see if I can characterize that a little bit and get you to expand on essentially what you have learned to date about what's important, and I will confess to having some coaching on this from one of your colleagues. The issue is the rain shadow from the Sierra-Nevada, and what that implies with respect to what might happen with an altered atmosphere, and I'll call it altered boundary conditions for the regional weather pattern, and it seems that one of the most crucial issues is going to be how often does the weather arrive across the mountains so that rain shadow has an effect, and how often do we get weather conditions where the weather blows in from the other direction, in particular storms that might come out of the Gulf of Mexico or otherwise cross a different set of mountains in getting to Yucca Mountain.

Now, I don't know enough about climate modeling to have a good understanding of how well your existing system may be able to address that question, basically where is the weather coming from. But it would seem like a critical question. Would you agree?

DR. THOMPSON: Oh, definitely. It may be the critical question. If you change the frequency of where the weather comes from, then you change the mean state. One of the things these models are good at doing is resolving these sorts of weather systems and their generation. As I said, they were outgrowths of numerical weather prediction models, so they had to be or people would have given up on it a long time ago.

One of the key problems, or key issues, in determining how good the regional model output is is how good the boundary conditions are that are driving the regional model. The regional model is actually pretty darned good for the regional work if you feed it good boundary conditions. And one of the problems that we're facing is, when we run these coarse local models, are we getting some of the basic boundary conditions correct? We know that the local models have biases and we can correct for those biases, but we don't know how the biases might change under altered climate conditions. So we don't have a dynamic correction procedure, for example.

It's always been the assumption, and still is, that as we get more powerful computers and develop better parameterizations, we go to higher and higher resolutions, so that local models of ten years from now will have the same mesh as our regional models do now, and the regional models will be down to 10 kilometers. And, in general, things have shown a steady improvement. If we had much more computing time, we could do a better job right now, for example.

DR. NORTH: Well, one of the things I like about your approach is that you've gone to essentially a variable grid size with an embedded regional model, so you can look and find detail where you want to see it without getting into that level of detail everywhere on the globe. But I wonder if it might make sense to go to perhaps a three stage system that's focused on Yucca Mountain specifically and looks at the issue of where does the weather come in from, and, for example, gives quite a lot of detail that would support trying to understand conditions under which the wind would blow in from the east and bring in the precipitation from that direction. Perhaps this involves modeling in some detail what happens in the Gulf Coast area, or it may involve hurricanes that come across. I'm not sure what the issues are. You would know that.

But I'm reminded of the discussion we had with the previous presentation on a three dimensional model for Yucca Mountain, and the questions that were asked about the importance of faults and the answer we got about how relatively easy it was, a week's work, to bring in new detail where it's important. And it would seem to me this would be extremely valuable with respect to your capability to be able to take advantage of the phenomenological insights that you have achieved from the early runs to be able to target important phenomena and then bring those into the modeling system with additional detail in order to address the question how wet might it get at Yucca Mountain.

And what I think concerns me a little bit about your system as I've seen it is it seems to be down at the bottom of the pyramid in the various slides that Jean Younker and others were showing, very detailed model of the processes and not really have a good coupling up to a more abstract model that deals with what do we really need to know for the major decisions on Yucca Mountain, whether it is in fact a suitable site and how we might design repositories if we conclude it's an acceptable site, et cetera.

I think if the output from the model is it can get as wet as on the backside of the mountain on the Island of Kauai, 600 inches a year, which I translate into millimeters is a very large number, clearly this site's not going to be acceptable. And the rain shadow issue and where does the weather come from would seem to be really critical determinants of that.

I'm, frankly, a little bit concerned about extrapolating strictly from the paleoclimate data because as you and your colleagues well know, the atmosphere is being altered in a fashion that has not occurred on the planet for millions of years. So the past may be a particularly lousy predictor of future climates, and as various people around the world try to understand the greenhouse issue, they are driven to groups such as yours to try to understand that through some kind of a modeling approach because the system is so complicated and we clearly have such difficulty in relying on past history, given the data for Mauna Loa and the insights we have about just how much we're altering the atmosphere.

So I think this is all by way of saying we're depending on you. If we can't essentially address this problem through riding the system to failure and finding out that if the infiltration is greater than 3 millimeters a year, or some number like that, basically this thing won't work, we're going to bring the water table up over the repository horizon, and I suspect that that kind of projection would say this is an unacceptable site, an unacceptable program.

The question for you then becomes what are the chances basically that it's going to go over 3 millimeters per year, given what you can tell us about future climates? Now, if the failure turns out to be 30 as opposed to 3, that may be a relatively simple conclusion that you and your colleagues might make, but it's very unlikely that you're going to turn Yucca Mountain into a rain forest. But if it's down there in the region shall we say reasonably close to what may have happened in the paleoclimate periods, the question becomes a lot harder and we may conclude that in your area, we need to invest a lot more money and a lot more effort to try to nail down how wet might it get.

So all that's a fairly long comment and my question for you is what does it really take to make this better, and on what scale might you be able to do it.

DR. ALLEN: Do you want more money?

DR. NORTH: I'm not sure the answer is more money, because if we conclude that there's a bounding calculation from where's the failure level, maybe the answer is we don't need to invest a lot of money in your kind of an effort. But at this point, I stand here probably knowing a good deal more about this issue, having chaired a review for EPA on global climate, than many of my colleagues, and I'm concerned that I don't know the answer of how much importance to ascribe to your work.

I think what I can conclude, it's very important that for the program, we think this through and think it through carefully, which is why I'm so glad you're here. At least this puts in place for the group of us who have oversight responsibility some discussion on this issue, which in the Board's history, we haven't had before.

DR. THOMPSON: In terms of what it would take to do a better job or what is a better job, are we doing the best job we can right now, to a large extent, what would need to be done would require time probably more importantly than money. As I said, we're on a steep ramp of developing better climate models, largely as a result of this global change issue. Although climate modeling itself has been around for 25 years or so, it was a much smaller scientific industry than it has been in the last few years.

We're, you know, rushing headlong to develop these comprehensive earth systems models, which include oceanic circulation models, as well as more comprehensive atmospheric chemistry, all these other things that go into the full earth system. And it will be probably a decade before those kinds of models come on line and are used operationally in applications or assessments.

DR. NORTH: A decade's about the length of time before we're likely to get into the decisions on the licensing of Yucca Mountain, so that doesn't concern me so much.

DR. THOMPSON: Well, then that's convenient for Yucca Mountain, because Yucca Mountain is certainly not driving climate modeling. It's other issues.

DR. NORTH: I think it may be that climate modeling could be driving the Yucca Mountain decision. That's what concerns me.

DR. THOMPSON: In terms of, you know, more immediate things, sure we could run the models with better resolution. Right now, when you look at a model with a 500 kilometer grid, that in fact is considered to be fairly coarse resolution, even in the climate modeling community. People would like to run models with, say, a 2 degree latitude; that's about 200 kilometers. A model like that tends to be about an order of magnitude more expensive to run.

One of our limitations is, you know, we have a couple of Cray super computers at NCAR, we keep them busy around the clock. We need better computers, more access to computing time. We may have to buy the time outright at $300.00 an hour. Things like that get to be considerations and you can drive your budget up pretty quickly when you try to do modeling at better resolution.

DR. DOMENICO: May I assume that Warner has covered all the concerns about climate modeling that the Board would like to hear?

DR. NORTH: I'll make one more comment, and that is I think the idea of an advisory board is extremely important and I would urge that both you and DOE proceed accordingly.

DR. DOMENICO: Thank you very much, Dr. Thompson.

Chin-Fu Tsang from LBL is going to talk to us a little bit now about confidence building for models, and I'm sure he'll get us right back on schedule.

MR. TSANG: Thank you for the opportunity of sharing with you some thoughts I've been considering over a number of years on the confidence building for models and model predictions. With the encouragement of Russ Dyer and others, I've been involved with some of the international efforts in this area and we're trying our best to look at the forest and not to get lost among trees. So we try to have a broader view of this whole situation.

My talk, I will follow this outline. I will say a few words about the motivation for the model and model predictions. I would like to define four types of models. I think this will help us in our considerations when different models are mentioned, it will clarify some of the considerations.

Then there's a whole question of are the models--or not. That has been questioned right from the beginning of the nuclear waste question, and so I will mention the history and some of the recent considerations. And then I will discuss a little bit of the international effort which started with INTRACOIN, HYDROCOIN and then most recently INTRAVAL. That started in 1987 and it's going to conclude this year, 1993. And so I'll mention some of the lessons that were learned from that and some of the conclusions.

We learned a number of things, which is a better understanding of modeling process, three components that were built up for predictions and two types of expert inputs, and then I'll give some concluding remarks.

Of course the job we have in front of us is to try to make predictions into the next 10,000 years, which is a tremendous enterprise, and there's no other way to do it but to do some kind of modeling studies, so we're stuck with that, and we have to build it up from short term experiments. The longest experiment may be ten years time frame, perhaps, plus or minus, and you have to also have a small scale. You can only look for tracer experiments, you can probably look up to 100 meters, but not that much more. But we're predicting into 10,000 years and we're predicting the kilometers, and so that is a major problem.

The second major problem we have is that the problem we're asking ourselves is tracer transport, the travel time, which is very different from the questions we ask in other disciplines, such as water resources. You just ask how much water is supplied or oil and gas reservoirs, how much oil will be there which we can take out of the system, which is a much grosser problem. But when we are talking about tracer transport, a little concentration to reach somebody's house a kilometer away, that's a much more demanding problem. And so that is the reason for all the questions we have.

The next one, I give some references. A number of the thoughts in this talk I have summarized it in the Journal of Ground Water, 1991. There are some additional thoughts in this paper, and then 1992, there's a comment by my friends, Konikow and Bredehoeft, to which I responded. And then in 1992, there are two issues of the Advances in Water Resources which cover the question of validation. There are contributions from Dennis McLaughlin, Mary Anderson and also Bredehoeft and Konikow who said there's no such thing as validation. Then later on, there's a comment against that, and so on and so forth. So you might be interested in reading these.

Then there's a series of progress reports on INTRAVAL which I will mention a little bit more on the international project on model validation, and there are a number of symposiums. The proceedings are available from NEA or SKI. The most recent one or the coming one will be planned for Paris in 1994.

Okay, so the next thing is I will try to define what is a model and the type of model so that we all have the similar language.

Generally, there is some confusion, at least in the early years, about what a model is. They consider a model to be just a code, computer code, and later on, realized when we talk about model, there are two main parts, conceptual model and computer codes.

The conceptual model actually is the most difficult part. It's composed of geometric structures. When you see the picture that Bo showed of the Yucca Mountain with a layering with the fault zones, those are all geometric structures, and then you can get a detail look at the fracture network within that system. And you need to have that in the conceptual model, and if you make a mistake in that, that's big trouble.

Then you also need to consider what are the physical and chemical processes that are present, such as chemical retardation effects, such as you could have heavy thermal loading, how the air flow goes, how does the vapor flow go. You need to consider all that. That usually enters into the model in terms of differential equations with coefficients to be determined. This tends to be entered into the model in terms of grid or mesh design.

Then on top of that, you need to bound the conditions, initial conditions. Unfortunately, most of the natural systems are very hard. They are not in a static state, even if they have long-term variations. Then the question is, if you assume a static state into your calculation, how good are they. And so one has to consider that. And then the scenarios of what happens in the future, such as climatic analysis; what will happen within the next

10,000 years. All these with all the uncertainties there are, go into the conceptual model, and then this actually is a simpler part. Once you know the processes, the geometric structure, you can build up your equations and build up your numerical algorithms, and then you solve the equation. And these could be very simple things or very complicated.

Then it is useful to define four types of models. I call it Type A is the model that you build up to study a process and to try to understand the effect, the significance of a process. You might design a lab experiment to study about it, and this you usually control and prescribe the condition so that it can focus on one separate experiment.

For example, there are some people who are interested to study matrix diffusion, so they design a lab experiment to study that. There's somebody who is interested in studying the flows through single fractures, so you can have a lab experiment on one block to study that. And this you can use to define the parameter determination.

The second type is field experiments where you cannot really select the process because it is there, whatever comes in, comes in. And so you have to distinguish between competing processes and features. And also this system in our standards, in our definition, is of too short duration and small scale, even though this is much larger than that. And then the parameter determination now will include maybe some processes you did not account for within the model, so we use the word calibration, which means that you may have to calibrate some parameters so that some of the processes might be hidden.

Then there are models that you can use to make short-term predictions. By short-term predictions, I mean even ten years or thirty years prediction. With that kind of prediction, you can build a model and basically you can use like an extrapolation scheme. You might not need to be so careful; your ten year experiment previously might be extrapolated into the next ten years, plus or minus. And a lot of this you can have a chance to revise it.

For example, in the geothermal reservoir development, you might make a prediction, say, for thirty years we'll predict this reservoir can produce energy for the next thirty years. So five years later, you can revise that and that's commonly done also.

Now, we are here. We want to make long-term predictions which we have to make sure we have the proper physics and chemistry, we have the proper scenarios and boundary conditions with the uncertainties, and we have to worry about slow processes. There are processes which are slow if you do an experiment in your lab for one month, it would not show very much. But if you are looking at a prediction into the next 1,000 years, it shows up. So you have to worry about that.

Then this is very important; we need to know what is the quantity we want to predict. For example, if you want to predict the temperature in Reno 9,000 years from now on April the 22nd, it's probably impossible. However, if you want to say what is the average precipitation for the whole state of Nevada 9,000 years from now, maybe you've got a better chance. So this becomes very important, especially for the NRC rule making, what is the quantity. We do consider that.

So our concern unfortunately is the confidence building for model Type D, so we're facing the most difficult part of the whole thing.

Now I would like to turn to the question of model validation. Because it is so difficult so people right from the beginning were asking can we show, can we be confident that a model we use is valid.

The first time I heard about the question of model validation was in 1977 in the so-called GAIN Symposium. There, it was interesting enough, we used two words, verification and validation. The meaning at that time was exactly reversed. Validate means that a computer code works well, and verify means the computer is applicable to real system. But today, the definition is opposite, so we stick with the recent definition, and the first question is verification. That means the computer code operates as you hope it would within numerical accuracy, you know its error, et cetera, et cetera. That is relatively simple in one sense, and that you can check with some degree of confidence and make sure it works.

Then there's a question of validation, is whether it is valid for the particular application. So there was such a hope, these two, a number of international cooperation since 1981.

I would just jump ahead of myself and put in some conclusions here. After all the international cooperation, all the thought process to many smart people all around the world, the basic thing is there's no absolute validation. A theory can only be invalidated. For example, Newton's Law is not valid because Einstein showed it up. So this I call absolute validation, which is very much discussed by a number of philosophers of science. But this does not mean that in our ordinary life calculations, predictions, we don't use Newton's Law any more. That would be disastrous for most of us. So this is used, so I shall define there could be a practical or conditional validation, that a model could be valid for a particular site, for a particular observation, like the example I mentioned earlier, over some range of parameters, with a certain range of uncertainties.

So under these conditions, you can say the model is valid.

This turns out to be a useful concept. This goes along quite well with the definition of validation of the International Atomic Energy Agency in 1982, and also goes along quite well with the definition that is commonly used by the practitioners of system engineering and operations research.

Now I'm going to turn and talk about INTRAVAL. INTRAVAL is an international project that started in 1987, '88, and it has two phases of three years each, so about six years, and it will finalize by September this year, and the last meeting will be in Stockholm.

The purpose of INTRAVAL is to study indeed validation of geosphere transport model for performance assessment of nuclear waste disposal. Twelve countries are involved. A number of observers from EPA, Atomic Energy Agency in Vienna and the State of Nevada, Linda Lehman is involved in this meeting. And then NEA is helping with all the organization and the publication of reports.

The Phase I report is available. It has come out with about 10 or 15--that summarize the first three years of work on that.

The approach of INTRAVAL is so interesting, and actually turns out to be very helpful afterwards, we find. They select the best set of lab and field experiments that's available in Europe, in the U. S. and also in Japan. All these experiments are performed by the agency that is concerned with the nuclear waste disposal. Some of them are not published, some of them ongoing, and the experimenters are very cooperative to be in meetings. If the experiment is ongoing, they can change them because of these discussions.

The experiment ranges from the core size sample to large scale experiments of 100 meters of that order. That includes some of the pretty well known STRIPA (Sweden) experiments also.

Then each one of these test cases were defined and data compiled in districts, and the reports are available and they are studied by a number of teams from different countries with their own models and with their own approach. Then we have coordinating group meetings and workshops of one week duration every eight to nine months. The most recent one was in November in San Antonio; the one before that was in February in Sydney. And so every few years, we get together.

In this way, there was much in-depth interaction among the modeling teams, and all the modeling teams are encouraged by their national nuclear waste concerns, so they have definite commitment to try to study these problems. And then they all come from different backgrounds and different codes, so there's a pretty broad selection of experiences. And so many groups study the same test case, and in these meetings, we try to have a thorough discussion and we try to understand any differences in the results. And out of this, we suggest new modeling work to be done for the next meeting and we also suggest new measurements to be made.

And then there's a committee, international committee which looks over the whole thing and gives advice to the organizer, Jesus Carrera who is mainly in the modeling of alternative conceptual models and scenarios, Neil Chapman is a chemist and he is mainly concerned with SALT system, and Dave Hodgkinson, it used to be Harwell and now is Intera in UK, which is different from Intera Technology in Texas, and Neretnieks who did a series of experiments in STRIPA and Tom Nicholson at NRC, Shlomo Neuman with modeling and so on, and then myself.

So some outcomes of the modeling of the INTRAVAL. What we learned is that validation, the biggest problem with validation is semantics. What is the definition? Of course this is not the first time in science we find our big problem is semantics. And so different people mean different things, and there could be a huge argument. It actually happened in the meeting in San Antonio when John Bredehoeft was there and Dennis McLaughlin, we had a good discussion and we try to understand each others language meaning, I think.

Now, the second thing is we cannot prove the absolute validity of long-term predictions. I think this is very much the feeling of many others too. And the next thing is that validation is a necessary process. It is a process. You can build up confidence, build up the validity, but you never finish. I'll have more to say about that.

So it's very good to include that as a part of performance assessment. It should not be a separate activity. It should be part to see if you want to make prediction for certain things and for that certain thing, for that prediction, you have to do some validation studies.

Then it should be based on experiments. You cannot just model in thin air. And you also have to have proper science studies. And then we find the benefits of the multiple group approach. We find out even the best group in the world who tries to study a test case, they could be off. But when you have many groups discussing together and argue about the differences, you really can improve tremendously about that.

And then we also find it's very beneficial to have in-depth review and comments. When you have several groups, each spend a few months and study the problem, try their best to give results of the problem, they really know every detail. By the time they come together and discuss, they really help at a detail level. Actually, some countries came in relatively inexperienced and through this, they really progressed very fast.

I want to mention, just on the side, a number of scientific outcome highlights. Through this INTRAVAL process, it's pretty much established that matrix diffusion to be a major retardation mechanism in the European saturated system. I'm not sure in the Yucca Mountain. And at the beginning of the INTRAVAL project, there were huge arguments between the Swedish and the Swiss. One said that is important; the other said that's not important. But through this process of studying and interactions, pretty much established that it's a very important retardation mechanism for the long term. For a short-term experiment, you cannot see that much of an effect. For long-term, it might be important.

The second one is the question of channeling of flow through the system. Through these INTRAVAL exercises, the concept of channeling is very much accepted by the different countries. This is not only the channeling because of fracture flow, but when you have a heterogeneous system, say within the fractures, the aperture changes. And then the flow tends to seek out the best path, and so the things that come out, that emerge from this heterogeneous system, could be that the flow only comes out in spots. It's kind of a fingering effect. So that is very much developed and there are a number of ways now that one can try to handle that.

Then the concept of stochastic modeling is also very much established and applied. Stochastic modeling mainly says that if in a system, the parameters you're not certain, there's a certain probability of different probability values and how do you study that system. And, of course, a number of authors have been looking at that in different ways, Gelhas, Shlomo Neuman and Dagan and others, but this, I think, is the first time it was applied to a real system in a bigger scale. Then there are, through this, there was an inference to a number of experiments, Las Cruces and also with WIPP, which is part of the INTRAVAL problems.

Now, I want to then move on. Out of this INTRAVAL experience, which I was very much involved in, I'd like to bring out some of the lessons we learned. First of all, I'll mention something about modeling process, then I want to identify three components for the predictions, long-term, 10,000 years time frame, and two types of expertise input that we need.

When you consider these things, you find out that probably the optimal modeling process is indicated here, and of course a good modeler, consciously have been doing it, but it is good to spell it out. The first process you will do is to review whatever data you have. In this regard, my personal preference is one should get some data and not to tie in too strongly with models. You do the geological mapping, review it, have a few wells, see what is a flow, where are the fractures, and so on. Without tying too close to the model, you review that.

Based on that, you can try to then build up what I call a conceptual model, which is a model which you try to put all the information on the same picture, a gridblock like Bo had been doing. Again, the warning there is not to tie in with the capability of your code. Suppose you have a two dimensional code and you say, oh, I don't want to see any data but two dimensional numbers. That would be a pretty bad situation. So take the full range of information, put it all together in some conceptual model, you will see there are some contradictions, maybe data are not consistent, and so on. You have to make a record of that.

Then you consider scenarios into the future, the climatic and the regional, and the reasonable alternatives of these scenarios and reasonable alternatives of the conceptual models. I'll mention a little bit about that later.

The next thing is that you want to determine what is the performance criteria, actually it should be performance measure, what you're trying to predict, what is the quantity you're trying to predict.

Now, once you have these three things, then you say, okay, I want to make predictions, so I want to build up a calculational model. This model, if it is done correctly, is probably impossible to calculate because you have so much detail, so many numbers, and now you say I want to make a prediction about certain things, say I want to make a prediction about the precipitation over ten years in the State of Nevada. So, okay, then I want to build up a calculated model. Maybe the State of Nevada can be represented by only one grid block.

So based on this, you simplify it and make it into a model that you can do calculations on, and then with that, you have the question of parameters. Once you have a calculational model, then you need the block parameters. That, in itself, is not an easy problem, because if you say, for example, you make probability in local measurements, which means probability of, say, 100 meter blocks, you want to build it up to the lumped parameter that corresponds to a kilometer block. This is commonly called upscale. There's a number of recent papers that have been published on how to do upscaling. Perhaps the lump parameter quantity cannot be well defined, so there's a lot of problems with that, too.

Then you do the model calculations. It's important to have the uncertainty analysis and sensitivity analysis. So I'd very much like to, here, say what would the climate be like 1,000 years from now and what is the uncertainty about that. So maybe it's uncertain by several orders of magnitude, which is not uncommon in hydrology.

Okay, with that information, with the uncertainty, then the result can be evaluated by the management. Is it too large? If it's too large, can we make new measurements? You can make new measurements, get new data, then go back to here and continue again. So it's an iterative process.

Now, if the uncertainty is acceptable, good enough, then you go to the decision making. Now, that could be a possibility when an uncertainty is too large and it's not acceptable and you cannot get data in a reasonable amount of money, reasonable amount of time, then somebody will have to make a hard decision. No more comment about that.

Now, for long-term predictions of 10,000 years, based on all the experiences with INTRAVAL, I think it's important to have these three components. Actually, I might be repeating myself, but it's important to see this. We need the key experiments like what Al Flint has been doing, Joe Rousseau has been doing, and study them and use the local model to understand all the data involved, and put that data into the modeling for long-term predictions.

On the other side is that you need the proper physical and chemical processes. For example, a lot of these short-term experiments might not give you a proper indication about matrix diffusion, or maybe heat pipe effect in the natural system that Karsten Pruess has been studying, that might be a major effect in the repository concept. And also, for example, what you learn from geothermal energy, you need that, to make sure whatever model you use has the proper physical and chemical process. And then you need to have sensitivity analysis. You always have to give an answer with some kind of uncertainty, because in this game, nothing is certain.

Then I find it also very useful to identify two types of expertise. There is a general state of science which increases with time, I hope. And then the question is these two need to be coupled, these two learn from each other. Let me just take a couple examples. For example, if you were concerned with--repository, you might need to know more from the geothermal energy field where the temperature can go to 300 degrees centigrade. So you need to bring that in. For example, if you are worried about the radionuclide transport and reaction with geologic formation, you might need to bring in the information from, for example, Oklo phenomenon, which is in East Africa. And recently in the last few years, the French have started again a new program to study that in great detail. You need to bring in that information.

So this kind of interaction is important and critical and does take time. And all these will have to be built into a judgment and we also have to learn that judgment has limits.

So the expertise, A, they need to be concerned with the site information and those who are very much concerned with the Yucca Mountain itself, it's very good to consider outside of their study and it's very useful to look at other sites and other processes. Another example is, for example, I think the question of--transport. So that we can also try to bring in.

Now, for both A and B, with this judgment, we have to say that limited by state of science and limited by state of information. So just like Starley was saying, there's no guaranteed prediction. There is a limit of science. And so the public has to realize that, and actually the public is not unexperienced to make decisions with plenty of uncertainties. We all drive a car and there's certainly a good probability that we will get into an accident. But people drive cars. So the public is able to make predictions with uncertainties, but we need to spell it out for them.

Well, what is confidence building then? Is that we want to make sure that inputs from A and B are effective and sufficient? We should do the best we can. So that is the key of the whole prediction process. Are we doing the best we can to draw in the scientific state of the art? Are we doing the best we can to integrate the information of the site?

Now, I'm sure you all have your ideas of how to do it, and here is just my comment of how to bring in the current state of science, of course, through broad selection of experts and expertise. Because sometimes, especially I'm sure Warner knows, when you draw experts, you might be biased already on whom to ask. So perhaps some kind of randomization of drawing on experts is important. And this is very important; we need in-depth discussions of the basis for judgments. We find out through INTRAVAL experience five days is barely enough just to discuss difference and understand them. That is the only way we can really build up the confidence. Then it's very good to publish the papers into the general public. Who knows, there might be a student who can poke a hole in the whole thing.

Then the next question is with the site information, how do you bring in the proper interpretation of this site information. Of course, this involves study site-specific data, which is very important and that's being done very effectively by USGS and others, and then I would like to emphasize that it's very useful to have more than one group doing it. You see, with the geological system, there are a lot of subjective feelings. I don't know whether you know Charlie McCombie, who is in charge of the scientific things in NAGRA. He said it's hard to find; he would prefer to look for one handed geologists because they all say on this hand it's this way, on the other hand, it's that way. So it's always two handed, so you all have the subjective interpretation into the system, but it's very good to look at them all. Look at them and understand the difference and understand maybe it is uncertain.

At this point, maybe I'll just bring in this one. This is a picture I took from Scientific American last year in November. Some of you must have read this; the risks of software, with a beautiful picture of the Patriot missile, and where it discussed the uncertainty with software. Of course that is, in a way, a simpler problem than what we have because with a geological system, we don't have all the information. It says that for the software, you want to make prediction, it's very good to have designed--it helps to increase the reliability of software systems, it helps increase, say, the respect for models.

Each program or replica developed independently by different design teams, then you have this fellow here who will try to integrate them. And this is a difficult job. But perhaps that person can put in the uncertainty and then you come to the output of a system which has sufficient confidence.

Concluding remarks; this basically just summarizes some of the main points that I put in, and it is very useful to have multiple groups and wide scientific public scrutiny, and we do need the detailed discussions, which will be very helpful for us to have come to a scientific consensus of what we are doing.

Thank you.

DR. DOMENICO: Thank you very much. As usual, it was very insightful. Do any Board members have any questions? Staff?

DR. LANGMUIR: Obviously, we all know the U. S. program is an unsaturated program, and the rest of the programs involved in this whole exercise are saturated programs. How useful were the insights to our unsaturated program that came out of INTRAVAL, or were they more generic as to their applicability?

MR. TSANG: In the INTRAVAL project, they have a large number of test cases of saturated systems, but they also have two test cases which are unsaturated systems. So that is part of the INTRAVAL project.

On the other hand, even for saturated system study, it will have very useful input for the unsaturated systems. For example, the whole channeling effect that happens in saturated systems, I'm sure will be even stronger in the unsaturated system. And for our understanding and confidence building, if you cannot understand a simpler problem like saturated systems, you have a harder time with unsaturated systems. So you can say that is a phrase we all need to understand and then go to unsaturated systems. I think it's important.

DR. DOMENICO: Staff?

MR. TSANG: I might add one thing. With all this kind of modeling, I tried to emphasize it's important to do it in a multi-disciplinary way. You should have geology, hydrology, chemistry all come in, because I was thinking about the question you were asking earlier about the fractures. We know when you look at a geology, look at all the fractures, 80 per cent of them are not important for the hydrologist, maybe even 90, so you need to look at geology data, but also you need to look at the well test data, hydrology data, and try to identify what is important. And then this problem gets even more complicated when you look at the traces because of the fingering channeling effect. So you have to integrate that data too. So to get from the conceptual model to calculational models, a simpler model, that is a great judgment, as a hydrologist, what do you need to incorporate in that, and so there's no way to make sure you're absolutely right unless there's somebody else who is doing parallel work to argue with me and we argue everything out.

DR. DOMENICO: Thank you very much. The next presenter is Claudia Newbury. She's scheduled for 20 minutes, and in view of the time, we can do that 20 minute presentation and then perhaps, if everybody agrees, break for an hour lunch and start off the first thing this afternoon with Susan Jones.

So I'll hold you to that 20 minutes, Claudia.

MS. NEWBURY: If you don't ask questions, I can probably do it in five.

I'm Claudia Newbury. I work for Jerry Boak on the project, and one of the things that I've been doing is I've been working with the software advisory group to develop a method for making sure that as we develop software, it's done under a quality assurance program.

There was a question of how do we qualify these big codes, and Chin-Fu talked about model validation, and we still have the question of now we have this huge numeric code that was developed outside of a quality assurance program, how do we bring it in and qualify it. Do we do some kind of a line by line verification to make sure they put all the dots in the right place?

Two years ago, we might have said yes, because we had a very difficult Section 19 of the QARD that was kind of cobbled together from a variety of industry requirements, but it was very difficult to implement. And so quality assurance had a quality improvement group meeting and everyone came together and aired all their concerns about how we ever can qualify software, and we put together a small group, and over two years, we met periodically and developed what we now have as Supplement 1 for the QARD, which we felt was much more reasonable. And there's about two paragraphs in there that tells you what to do if you have some software that was acquired, what we say is acquired, developed outside of quality assurance program, and I'm going to tell you a little bit about those.

Now, in two years of doing this, the first thing we discovered was nobody has the same definition of anything. And if you have more than two people in a room, you have more than two definitions of any word. So these are our definitions, and believe it or not, it took a long time to get these definitions, and the next three I'm going to show you were even worse.

Somehow models had shown up in software, model validation, and we struggled with this and finally we said wait a minute, a model is just some representation of a physical process. It is not necessarily embedded in code. Therefore, take all this stuff that says model and give it to someone else because we're not going to deal with it. All we care about is software and software verification and software validation.

Software, we defined as not just the code, well, it's a collection of code, but it's also the documentation that goes with it. And so when I'm talking about software, I'm not just talking about the numeric codes themselves, but the documentation that we generate to make sure that anyone who wants to use this code can.

Software verification and validation took about a week of argument before we figured out which one was which. And I can summarize it real quick by saying software verification happens while you're developing the software. You said you had some requirements; are all your requirements there. You said the design was going to do something; did the design actually do that.

Validation, on the other hand, at the very end of the process when you've got this code and you're going to use it for something, does it work?

And so I'm going to talk about validation because verification for acquired software is not something you'll worry too much about. That's a very simple requirement and it's our only requirement for acquired software. It basically says that when you've got some software that was developed outside the program, show that it works. If it works, you can use it. If it doesn't work, then maybe it's not what you need.

And we have two types. As we were going through this whole system of software, we had a big problem because we have engineers who use software, we have modelers who use software, we have a lot of different types. We have commercial software that is brought into the system, we have stuff that was developed by somebody else and it looks good and maybe you want to use it. And then we have software that was developed under one participant's quality assurance program maybe and they want to transfer it to another participant. That's easy when they're transferring it because it's under an approved QA program. But that stuff from outside is the stuff we have to worry about.

So if it was developed wholly outside the program, this is what you have to do. And if Starley wants to, when he brings the climate program in, the climate models and his code, he has to run some test cases, and if his test case for the current system shows that Pat and Russ can go fly fishing in Fortymile Wash, then maybe it doesn't work.

On the other hand, if it does, then he has a validated code, he's run test cases, he now places it under what we call software configuration management so that any other time the software is changed, we have documentation of how it was changed, why it was changed and what the changes were and what they were supposed to accomplish.

What this does is we've run the code, we've done the test cases, if it's got bugs in it, it's under software configuration management, we find a bug later, we fix it, we know what it was, we fixed it and we can document it. And by doing so, we think we have accomplished anything that needs to be done as far as quality assurance is concerned and validating the code for use. And that's it. It seems to be workable. It's cut down costs, it's cut down time, and the NRC has agreed to it and that's it. Five minutes?

DR. DOMENICO: Is there any fish in Fortymile Wash?

MS. NEWBURY: The Corps of Engineers designates it a navigable waterway. You figure it out.

DR. DOMENICO: John?

DR. CANTLON: In addition to the investigator who runs the model, is there any oversight within DOE?

MS. NEWBURY: Well, because some of the procedures that have to be developed say you have reviews at periodic intervals and the review is by someone who has not been directly involved in developing the code, so you do have a review process and you have the documentation, so any time anyone wants to come back and look at it, they can.

DR. CANTLON: So that would be a step that you didn't articulate in the slide, I guess.

MS. NEWBURY: Well, yeah.

DR. CANTLON: Okay.

DR. DOMENICO: Don?

DR. LANGMUIR: Claudia, I'm delighted to hear all this. It's about time. There's so many codes out there which DOE has not been allowed to use because they weren't "QA'd." Now you can get at them, they can save time, they can save money, they have other capabilities. Frequently they're shorter, they can do specific things for you. I'm thinking, for example, of MINTEQ or WATEC or PHREEQE, so on, a whole host of other codes, and I'm so pleased to hear this, it's just great.

DR. CHU: This is Woody Chu. How broad has the experience been as far as acquired software?

MS. NEWBURY: It depends on the various parts of the program. Our engineering and design people are using a lot of commercial code and they've been able to bring that in and qualify it to use. If they qualify it for use for its intended purpose, and I should have underlined that in big letters, for its intended purpose, you have to state why it is you're using this code, what it is you want it to do, and if you go outside the bounds of what you originally intended for it to do, then you have to go back and revalidate.

DR. CHU: You have to then redo it?

MS. NEWBURY: Yes.

DR. DOMENICO: Anything further? According to my watch, I'm five minutes ahead of schedule. I think it's prudent now for us to break for lunch till 1:35 and I think we'll be all right.

(Whereupon, a lunch recess was taken.)

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

DR. DOMENICO: Our next speaker of this session is Susan Jones, Rocky Mountain Project Office, and the application of existing data.

MS. JONES: Just like Claudia was asked to talk about bringing codes in, software in from the outside, I was asked to address the subject of bringing existing data in and using it on the program. In order to do that, my focus is going to be on DOE's use of existing data and the process of policies in place to support that. But, first, I do want to review the NRC guidance that has been provided to us on this subject. That comes in the form of NUREG-1298. That's the title there and this is our generic technical position that the NRC staff published several years ago. To be sure that I got the staff's position correct, I took the easy way out and quoted it. The important part of this is that the data that we're talking about today is the data that would be used in support of a license application stating that it should be qualified to meet the quality assurance requirements of 10 CFR 60(g). I'm going to talk about again some more of the

NRC's guidance to us and then how we're implementing that guidance.

Another quick definition here is that the qualification process is a formal process and that will show up in the form of a proceduralized and documented process within DOE. It's important to recognize that NUREG-1298 grouped data into three sections here. The first is this generally accepted category and, as I say, this is the type of thing you can pull out of a handbook and use. It's really not the subject of my talk today. So, it is going to ignore that column, but they do recognize this and clearly state you can just use that data. A second type here is this qualified data and that's data that was originally collected under an accepted QA program. I'm going to exclude that from this afternoon's talk also.

What we're really focusing on is the second column, the existing data; data that is either developed within the DOE's program by one of the participants prior to acceptance of their QA program or the data that was developed outside the program, such as information in the journals that you then want to bring in, run through this formal process, and move from this column into this column, qualified per the guidance in the new reg. So, that's what the process is that I'm going to talk about, moving it from this column to the qualified column.

The NRC has recognized four methods by which that can be done. First is peer review. The second is corroborating the data. That means taking existing data, you identify data set, you look at other data and determine whether or not that corroborates the set of interest for its use. You can also perform confirmatory testing; go out and collect some more data under a QA program and use that to confirm that existing data. The fourth one is interesting. You can go out and look at the quality assurance program under which that existing data set was collected. That means you can go out and look at the procedures, determine whether they were similar at the time that the existing data was collected, compare them to the procedures that are in place now, and determine if there would have been any substantive difference if you had done it under today's QA program. The staff has also said that on a case-by-case basis that if you were to propose some other method of qualification, they would let you know whether that was acceptable. And, they clearly state that using a combination of these four methods would be preferable to using any single method.

This is a point of interest so you understand the context of the program. What we're saying is these are the dates on which the key participants' quality assurance programs were qualified. So, everything on this side of that date constitutes qualified, collected originally under a QA program, and probably to that qualified category. Everything to the left would be existing data.

In the SCP, we did state that we would adopt the guidance from the new regs. We repeated that in a response to one of the NRC comments and today if you were to look within our program, you'd see that we had captured the new regs' guidance in two places. First is in the quality assurance requirements document. Explicitly for science, it's captured in the Supplement III, data validation and qualification. And then that, in turn, is implemented, that requirement, via this procedure on qualification of existing data.

At this point, I want to pause and make this point. That you can put any information you want into a license application; however, you increase the confidence in that data that it's valid for its use if you have qualified data. So, the position we're taking is that we're going to be qualifying existing data in the case where you're going to use that data to explicitly defend your licensing position and there is no other qualified data collected under the program to support that position. And, I would contend that this is going to be used very judicially. This is not going to be a particularly large effort, as near as I can tell now, because the whole point of site characterization was to go out and collect that data.

This is the procedure and it is literally a three-step process; in fact, it's a three-step procedure where it says, okay, you have to identify that data set. And, at the moment, what it is, is the principal investigator through a TPO recommends a particular data set, reaches a point in the study that says I believe I'm going to need to bring this information in from the outside or use this earlier data not collected under a QA program, identify that data set, and then we have two options; either a technical assessment which is done under a project procedure or a peer review that's conducted under an OCRWM procedure. We put the team together, they do their review, they come up with their recommendation to either qualify or not qualify the data. If the answer is no, then you have to go back to that participant, to that principal investigator to tell them to either do more confirmatory testing or continue collecting data under their existing study plan, whatever the instruction is. If the answer is yes, you update the data base where we have a flag that tells you whether it's qualified or not qualified and then you use the data.

And, this is the way we have captured the NRC guidance for those four methods. It's through a series of questions; the first one being a screen that says do you even want to go through a qualification process? Then, you would decide which of the four methods or combination of methods you would choose to use, and based on the results of the review, you would then ask whether or not any confirmatory testing was necessary prior to receiving a recommendation from the team.

And, here's the example which goes through an extreme erosion where we identified a data set to support a position on the potentially adverse condition of extreme erosion, an intensity of R-60. We selected the technical assessment method, doing it under the project office procedure, put together a team of five people, two geomorphologists, two geologists, and a QA professional. They then went out and did a two-phase review. First, we looked at an equivalent QA program, compared the technical procedures then and now, and the QA procedure. When the team got together after looking at the procedures, they decided they were equivalent, but a key to this question would be, "Were they followed?" So, we went out and first interviewed the two principal investigators. It was John Whitney from USGS and Chuck Harrington from Los Alamos. But then, also went in and looked at their scientific notebooks and traced their work from sample collection through to their final analysis, put together a records package to support this, and then the team prepared a report and recommended that the data be qualified. DOE, both from the technical, regulatory, and QA sides, reviewed this report, and in September of '92, we did accept that recommendation.

DR. CANTLON: Could you give us kind of a guesstimate of how many person days that process took?

MS. JONES: Well, I'll tell you, it's a little difficult. I can tell you how much it cost. Now, it took six months from start to finish from the time I--I think Russ signed the authorization to start to the time the record package went in. But, the total cost of this was approximately $70,000. So, you can see those five people just worked off and on through a six month time. I should point out, it took quite a while because this was the first time we had used this procedure and we did a lot of brainstorming as we were going through it.

DR. CANTLON: Sure.

MS. JONES: But, it wasn't a particularly large effort in that regard. And, as I said, our policy is to perform the investigations in the SCP in a manner that would insure that we had sufficient data collected originally under the QA program.

And, furthermore, I'd just like to leave you with the thought that the decision to qualify the existing data is actually a licensing decision. It's made in conjunction with the technical people, the managers, the regulatory people, and legal counsel because all of them have to look at that and help provide input to the decision whether you need this existing data set to support your position. In conjunction with that then, it is also made as part of the resolution process. As you start a study, you probably will be using existing data because the purpose is to collect the qualified data. Through time, you would see more and more qualified data being collected, fed back into your study, into your model. And so, it's somewhere towards the end that you would make the decision whether or not a piece of existing data needed to be qualified and that's going to be done pretty much on a case-by-case basis through time as we develop segments of the annotated outline for the license application.

Questions?

DR. CANTLON: Do you have any thoughts about which domains of information are more likely to be derived from existing literature as opposed to generated de novo?

MS. JONES: Actually, I've been giving it a little bit more thought recently to which data collected on the program might be a candidate here.

DR. CANTLON: Pre-QA data.

MS. JONES: Pre-Q--right.

DR. CANTLON: Yeah.

MS. JONES: Because I'm thinking of some of the very long-term monitoring that we've done might be a good one like seismic work.

DR. CANTLON: Exactly.

MS. JONES: I think it was Alan Flint showed some meteorological monitoring data. And, the reason I say that is because more I've been thinking in terms of can we go in and working with QA, the expenses here, the director of QA--he's going to correct me if I'm wrong on this one--but we've been talking about the ability to go in and look at the time periods from when the participants accepted their QA programs to when DOE accepted it and try to qualify the whole program. So, that would push those stars to the left and give us a whole time period in which we would have more qualified data. So, that's more what I've been thinking now.

I would expect the other, in answer to your question, the broader question of what data would be qualified, is probably going to come out of my annotated outline process where we go in and we look at the information being requested by the NRC in a license application. You look at the data you have, you look at the planning to see what's coming out in the future, see what the match is, and then look to see if any of that existing data would be qualified. And, right now, the format and content guidance from the NRC requires us to identify data sets that have been qualified. And so, we would need to do it as part of that process, systematically work through.

DR. CANTLON: Thank you.

DR. DOMENICO: Any further questions from Board members?

(No response.)

DR. DOMENICO: Staff?

DR. CHU: So, the erosion data set was the only one that you have done, so far?

MS. JONES: Yes, it's the only technical issue that we have taken to completion, we believe.

DR. CHU: And, do you envision qualifying journal articles and so on?

MS. JONES: The gate would be whether that was providing the primary data to support some type of a licensing position. I'm expecting most of our work to have been produced under our QA program from our own investigations, but I'm going to be having to work--I, meaning the regulatory group --is going to have to be working very closely with the principal investigators, the modelers, because they are the ones who are going to be able to know best whether they need to bring that outside expertise in.

DR. CHU: Right.

MS. JONES: For example, I was thinking this morning when I was listening to Starley where he's talking about the global climate model, well, clearly, that's going to have to be data brought in from the outside. So, I'd have to work with each investigator to identify that data set.

DR. CHU: Yeah, but now staying with the subject of journal articles, would the peer review process that would be required for publication, that would not be sufficient?

MS. JONES: Within the program, we have certain require- ments in the QA program and I suspect that what we would have to do is take the literature search--that's what you're talking about?

DR. CHU: No, what I'm referring to was the peer review process that took place--

MS. JONES: Yeah, right. Someone does a literature search and brings in information from the refereed journals.

DR. CHU: Right.

MS. JONES: Okay. That would not meet our definition of what constitutes a technical review. Someone from within the project qualified to do so would have to sit down and do a very straight-forward assessment of whether that it suitable for whatever the intended use. It's very similar to what Claudia was talking about in the software. So, you just have to sit down and say, yes, this is a data set that's valid for this particular application. If it's valid for my application, it might not be valid for your application. So, you would also need to qualify that. That's also part of our QA requirements. So, you just have to verify that it's suitable for what you're using it for.

MR. BLANCHARD: Woody, maybe I can help Susan. I think the weakness in the classical, if you will, peer review process in the publications is one of adequate record keeping that can be used sometime in the future. It's not that you're not getting comments between peers on a certain discipline; it's that the record system that we've adopted which has withstood the test of time for nuclear licensing is one where we have records that would show what the peer review asked questions about and what was challenged and then a comment by comment response by the author and then an agreed-to written resolution for each one of those comments. And, that also is part of the record package. That kind of detail is there in concept, but not in specific details as you go through the publication process with a journal. It's just not kept, you know.

DR. DOMENICO: Thank you very much, Susan. And, with that, I'll hand it over to Warner who will chair the final session of the day.

DR. NORTH: I'm delighted to see we're making such progress getting back on our schedule and, rather than make any introductory remarks, let's just keep going with the next presentation which is Russ Dyer deciding when enough is enough.

DR. DYER: Well, let's start with the answer, it depends, and amplify on that through the rest of the talk.

The things that I'm going to cover are who decides and what's the basis for the decision. What are the ques- tions? How are the decisions made? When are decisions made. These are things that I wish to bring out in a little more detail in the talk. Also, we're going to talk about the tools that can aid different decision makers at different parts of the program.

Who decides and on what basis? I'm going to step back quite a bit. For a non-technical talk, we've seen a lot of graphs and slides, quite a few charts, things that look suspiciously like results. Most of what you've seen have been down in this arena, a view from the site data providers, perhaps some from the performance analysts. We've heard very little except for what Jeremy Boak brought up yesterday, the actual regulations that drive the program. Susan alluded a little bit to some of the concerns from the regulatory side. And, this is not a paid political announcement, but we've heard very little from the design side on this particular topic. Design also has needs for information. How much is enough to satisfy design? We talked about climate and infiltration. I would recommend--suggest that certainly one of the things that needs to be considered by a designer is what might be the probable maximum flood for a particular area which would be tied to some of the things that we've been looking at.

DOE sits in the middle here. We're playing off all the various parts of the program to try to make sure that we're getting enough information to satisfy the needs of all the different users of the information. The ultimate objective, of course, is to make a determination of whether or not this is a suitable site. Suitable for what? Suitable for a system that consists of both a natural and a manmade component to it. So, we have to have information not only about the natural system itself, but also some idea of some level of maturity of our understanding of what the engineered system would be like. It's only when we have some kind of a more evolved understanding that we can make a judgment as to whether or not management risks are acceptable for moving forward to either make a decision to recommend or disqualify the site. If a recommendation is in order, then following that, we would submit a license application and the regulator has a series of decisions to make, also, and may have slightly different criteria for determining how much is enough.

In tabular form here, these are the same components that you saw in the first slide, but let's just look at what the interests are for these different groups that are involved in this particular decision. Who decides? Well, different individuals are making--or different groups are making decisions based on what their perception of the problem is or what their assigned role might be. The providers of site data, Alan, Joe Rousseau, are interested in being assured that the data and interpretation are ready for scrutiny by others. The designer wants to be assured that the design is adequate for the purposes intended for whatever design might be. Jean talked about the role of the performance analyst, that the validity of models are adequate, and Claudia and somewhat Starley talked a little bit about code predictions for use in predicting performance. The regulatory analysts, who we haven't heard very much about today, one of the things they're interested in is whether multiple lines of reasoning provide a basis for demonstrating compliance with the regulatory requirements. The DOE decision is whether management risks are acceptable to proceed or if we need to modify our existing program in some way. The regulator is interested in the question, "Has reasonable assurance been achieved?"

Well, those are the players; now, what are the questions? Well, each group that's involved in this has their own set of questions that they are trying to resolve. The providers of site data, what means are available to currently understand site conditions and processes? What tests might I be able to field? What's the probability that those tests can provide us an accurate indication of what the state of nature is or might be? How can potential for changes in site conditions and processes be established? Again, looking at the testing question, how will I insure I have representative data? Do I want--as Joe was talking about his feature-based drilling program, is that going to provide us representative data across all of Yucca Mountain? Well, probably not. So, there's another element of the drilling program and testing program, a systematic program, that provides essentially not quite random, but a geo-statistical basis for data collection.

The designer has a suite of questions he's interested in. Are data adequate to support design? Is design constructable? These are very practical questions. Is design sufficiently optimized? Sufficient design safety margin? And, here, we just have a conceptual layout of the north portal of the ESF and we're certainly--the interchange with design is pretty intense right now.

The performance analyst, as we've heard, has asked the question are the models valid for their intended use? Do alternative models, plausible alternative models, exist? Do these models, even if they exist, do they produce significant results? What really matters in the various models. And, one question we have, are codes verified? Are the results capable of being duplicated by other organizations or individuals? And, we're interested not only in the present state of nature, but also how this system would perform in the future. This is one thing I've added on here that we have alluded to primarily in the question and answer period is a little stippled area around here to recognize that we're going to change this system. There's going to be an effect due to the construction of this manmade part of the system down here, a repository system. How will this whole system perform?

The regulatory analyst has another series of questions driven primarily by requirements out of 10 CFR 60, 10 CFR 960. What regulatory questions need to be answered? Well, the SCP, the site characterization plan, is based on defining issues that derive from the regulations. So, that gave us a starting point for our characterization program. Does a consensus exist about the preferred site model or models? For some things, there seems to be a consensus; for other things, there does not. I guess, one case that Susan has already alluded to where we think a consensus does exist is extreme erosion, that small sub-element. Do alternative models produce significant differences in results? This again is a concern from the regulatory perspective. Do predictions meet regulatory criteria? If they don't, what options do you have? You can go back and--well, I'll get to that in a minute as part of DOE's concerns. Are there multiple lines of evidence supporting compliance? It's much easier to press a case in a regulatory arena if you have multiple lines of evidence that support your case.

From DOE's perspective, what are our questions? The first one up here is, I think, a very important one. Is performance appropriately allocated among system elements? I've talked already about this system being composed of both a natural and manmade components or system sub-elements. How much performance should be allocated in the grand scheme of things for the total system to the natural system as we observe it now and as it might be modified in the future? How much performance should be allocated to the manmade components of the system; the barriers, the waste packages or containers, seals, those things that we can engineer and design into the system? A very real question that we deal with all the time, what's the value of obtaining additional site data versus the cost? What's the cost benefit? The same thing applies for performance assessment. What's the cost benefit of performing additional analyses, evaluation, predictive modeling, if you will. And, finally, how strong is the case for compliance? Is there some either small or large part of the program that we think the management risk is acceptable that we're willing to move forward with and bring it before the regulator, start the dialogue with the regulator, in the hopes of eventually resolving this question.

This is a little flow chart that outlines the general process that we go through; the characterization, evaluation against the DOE siting guidelines, and then a siting decision where one can have several options here. One can either continue, unmodify the testing program. You can modify some elements of the testing program. You can modify your concept of what the total system might be, and from that, derive a modification of the testing program.

The regulator, of course, has a very key role in this whole operation. Eventually, the regulator will make the ultimate decisions. Like Susan, we've just quoted directly out of the regulations here. This comes out of 10 CFR 60. The regulator is charged with determining whether there's reasonable assurance that radioactive materials can be received, possessed, and disposed of in a geologic repository operations area. And, at least, intuitively, we expect that with time, as one proceeds through site characterization, that there's going to be some rise in competence in both our perception of the system performance and also the regulator's perception. Perhaps, this may be too simplistic. I've put a monotonically increasing function here. It's possible that there may be some dips and valleys in this curve. And, what is this line where reasonable assurance is achieved? We've chosen to show it as kind of a fuzzy region here because I'm not sure you can put an exact quantitative number or description on everything that is going to need to be addressed by the regulator.

What tools are available to assist decision making? Well, different people, different organizations, individuals have need and use for different tools. The providers of the site data dominantly are scientists and use the traditional tools of science, the scientific method, expert judgment, peer review. The designer comes from the engineering world, by and large, has design codes, safety factor evaluations, traditional engineering tools to assist him. The performance analyst has sensitivity and uncertainty analysis. Expert judgment are tools that can be brought to bear by the performance analysis. Regulatory analyst has a couple of things; applicable precedent and the weight of multiple lines of evidence. From the DOE perspective, we've talked about many of these tools already during the course of the last several days; formal peer reviews, such as the review we had conducted on the unsaturated zone hydrology chaired by Dr. Freeze; expert judgment which we've used in several things; feedback from oversight groups and the regulator; decision analysis provides us multiple tools that we can use for various levels of decisions that need to be made. The regulator, himself, has a couple of tools at his disposal; weight of evidence, expert judgment, and independent evaluations.

What about timing? When do these various things happen? Obviously, there are day-to-day decisions being made. There are higher level decisions, if you will, checkpoints at which we can make changes in which way the program is headed. And, what I've tried to capture on this--this is from Dennis Williams and Tom Statton's talk yesterday--is a general--what I want to convey here is the general idea that as one goes from the day-to-day data gathering activities going upward, there is a hierarchy of decisions and actions that need to be made that eventually make their way up into the licensing arena in here, going through--well, come up from day-to-day data gathering, modeling, performance assess-ment, incorporate design in here, make evaluations about suitability of the site and the system and then proceed in a piece-wise manner, as Max laid out yesterday, into a licensing process that is built around a series of building blocks through the annotated outline and different mechanisms for transmitting material to the regulators, the Nuclear Regulatory Commission.

We propose an iterative process for evaluating site suitability at periodic intervals, at six month intervals. We prepare the progress report on the site characterization plan. At periodic intervals, of course, we need to stop and check where we are to determine what we need to bring on line for next year in the following fiscal year's planning and budget. So, whenever the annual plan is prepared, we need a good understanding of what we've accomplished, what we need to accomplish. These give us opportunities to sit back and look at what our state of understanding is and essentially an evaluation of where our uncertainties are. The ESSE Report, the early site suitability evaluation, served as a checkpoint the first time we had systematically reviewed the entire status of the site characterization program since the days of the environmental assessment back in the mid-80's. And, right now, we're thinking of using a process somewhat similar to that at periodic intervals of maybe 18 months to two years to evaluate where we stand as far as the suitability of the Yucca Mountain system.

What did the ESSE Report tell us about climatology and infiltration? Well, the recommendation that came out was that this was an area that required more study before a suitability decision could be made. What action does DOE take on that? Obviously, more work needs to be done. What work needs to be done? The challenge that we've had is to allocate resources to insure that the high priority surface-based and underground geohydrologic tests are accomplished and data evaluated in a timely manner. We've used a decision aiding tool for this which I talked to you about at the November Board meeting, the integrated test evaluation, to identify those tests that have the highest potential for providing us timely information regarding suitability of the system. That's what we used for prioritizing our allocation of resources in fiscal year '93 in the site characterization program.

So far, the one topical report that we've submitted to the Nuclear Regulatory Commission, that topical report on extreme erosion, was one that was initiated by the principal investigators. The scientists themselves felt that they had enough information to move forward on a particular topic and that happened to coincide with judgments that came out of both the EA and the ESSE Report. There was a determination that management risks for moving forward on this was accept- able. So, we moved forward to put this into a topical report, bring it into the annotated outline process, and initiate the dialogue with the regulator on this particular topic. Probably, the next one that will come on line is the seismic hazard methodology topical report.

The issue resolution process which, I believe, we've talked to you before about the issue of resolution. I think so. If we haven't, I'm sure we will. From a suite of potential issues, the objective is to identify candidates for early resolution through site suitability and through dia- logue with the regulator. And, to reach this state, the site conditions and processes must be understood, the consequences for performance must be documented and acceptable. and we must have a good body of evidence that support compliance, at least adequate to proceed. For each issue that's brought up through the issue resolution process, determine what is the appropriate action by which a dialogue with a regulator can be established; a topical report or perhaps some other action. Rulemaking might be an appropriate action for involvement with the regulator.

This kicks us into yet another area which is preparing the basis for the dialogue with the regulator. I think it came as probably a big surprise to John Whitney, the principal investigator on the extreme erosion studies, the amount of work. Once he thought he was done, how much more work he had to do to get his ideas couched into a framework that is appropriate in a regulatory environment. In fact, it kind of puts me in the mind of the days in grad school when you said, yes, I've finished my PhD except for writing my dissertation.

The issue resolution initiative is an ongoing active endeavor. There are a couple of key elements to it; the annotated outline supplemented by topical reports. We have run an official topical report. We've submitted to the NRC the extreme erosion topical report. We've put in a draft the volcanism technical report, not a topical report, to the NRC this month. This is a summary of Bruce Crowe's understanding of the volcanism issue to date. And, there will be a subsequent topical report on volcanism probability and consequence.

In May of this year, we have a revision of the annotated outline; the concept being that we have a living document, if you will, that we continually submit to the NRC that keeps them appraised of where we think we stand in making progress both on suitability issues and on the development of an eventual license application. The two other things that are related specifically to the topics we've talked about over the last day and a half are eventu- ally a report on ground-water travel time and I suspect that the first thing that we will initiate dialogue with the NRC with is a methodology for ground-water travel time, either through a topical report or a request for rulemaking.

To go back to a diagram that Max used yesterday which has the same information as what we called the fish hook diagram that Dennis Williams and Tom Statton used, translating the information garnered by the scientists, the investigators down at the lower level, incorporating that information into reports, running it through performance assessment and design, getting a concept of how the entire system fits together and performs, taking that up through the suitability assessments and into the licensing arena, is the challenge that we face. And, there are many decisions. It depends on where you are on this particular diagram both chronologically and also in the hierarchy scale here as to the criteria you would use to make the determination of how much is enough. So, in summary, it depends.

Yes, sir, Dr. Cantlon?

DR. CANTLON: In the day-to-day management of a program and particularly as you put together your annual program plan and you make those tough priority decisions, you are in a sense putting on hold various R&D projects, then. You've done that recently in cutting back on the number of drill-holes. Earlier, you scaled back on container R&D and so on. And, I guess, what I'd like to get you to sort of expand a little bit for me would be the distinction between those priority allocations of scarce funds to keep your momentum going towards regulatory work, then the added on role of performance assessment in either working with or helping with those decisions, and to what extent are the PIs, themselves, able to in a sense input into that decision? Could you give us a little feel into that?

DR. DYER: Yes. Once again, though, there's a hierarchy of priorities. There are hierarchies established, I mean, from the Secretary's level, and how much this whole program receives in the way of allocations. Within the program, the director may assign priorities to different parts of the program; to the MRS, to the Yucca Mountain Project, to transportation. Within the project, Carl has demands placed on him by engineering and design organizations, the site characterization effort. With in-site characterization, I am faced with the task of trying to fund about 6x worth of work with

1x worth of dollars. So, which part of this--where do we allocate the resources? Because I have a relatively coherent block of work, we were able to put together a tool of the ITE that uses the input from the investigators to essentially assign a value of information, their judgment of value of information, that we used to help us in the prioritization process within my particular WBS element. And, I took these priorities to Carl and he weighed them off against all the other priorities, competing priorities from other parts of the program, and we established a working number and a working scope of work for this year. And, we created an annual plan that implemented that. Right now, we're in the process for fiscal year '94. We already know that we would like to do much more work than we're going to be able to do. So, we've laid out what the objectives of the particular tests are, what the expectations of those tests are, and we will go through and also prioritize those activities and go as far down the priority list as we can.

DR. CANTLON: So, in a sense, how much--when is enough enough is an evolving target.

DR. DYER: Oh, yes.

DR. CANTLON: And, early-on, you visualize that much is necessary. As you look at budget, you get down to a smaller amount, and eventually, the absolute minimum which is the acceptable administrative risk.

DR. DYER: Yes. And, that's one way to do it if we continue on the program that we have laid out now. We also have to have the flexibility to change the program if need be. If we have--well, let's take the issue of thermal design. If Tom Buschek's thermal loading ideas are credible, can be validated, it may be that a considerable amount of the information that we currently are requiring in the site characterization program, you may not need to have that information to answer the system question. That remains to be resolved. I mean, that question is still hanging out there.

MR. BLANCHARD: Russ?

DR. DYER: Sir?

MR. BLANCHARD: Would you mind if I help?

DR. DYER: Okay.

MR. BLANCHARD: John, I'm not quite sure how to say this, but in a very practical sense, there's cost and schedule that enter into this picture and it enters in at many different levels. The highest level is perhaps at some point maybe the appropriations process simply says we don't want to give you any more money. And so, we have to be aware of that as we try to manage this program and the oversight bodies also have to be aware of that because we can do things with so much painstaking attention to detail that we just never get there because we turn the appropriations process off psychologically.

This means that looking at how to distribute costs is something that is just inherently in the process and what the impact is on schedule and it forces us to look for other solutions, in addition to those where we get advice from performance and from testing. For instance, there's a point where--and, we're not there yet--but there's a point where we'd expect the maturation of the process that Russ has described will allow us to make some tradeoffs where we'd say if we increase the safety margin on this engineering design by this much more than what they ordinarily are planning to do, could we reduce the length of time that some of the work for gathering information is now allocated and would that be a prudent way to do it? In some areas, the way technology advances, it may be a lot cheaper to build in a larger safety factor in the engineering area and you can get it done faster and with equal or better--lower uncertainty by going in that direction than by continuing to work on some of the features of the natural processes. We've not yet faced how to do that, but we know inherently it's downstream.

MR. GERTZ: John, I want to add one other thing to your description of we started with this much data and maybe then this much and this much. Up to now, we still intend to collect this much data. Much of the budgeting decisions have deferred data to other years right now. We are also looking at other processes. Maybe we can reduce the amount of data and not go to the Calico Hills and reduce the boreholes. But, up to now, we've not made any wholesale changes in our overall plan to reduce the amount of science or data at this time. But, there are pressures upon us to do that, to reduce the four billion left to spend on the program. But, when we're talking about our annual appropriations, it's more deferring. That's the kind of decisions we're making. What do we need to do this year and what can we defer in the next year?

DR. DYER: We still have a baseline program right now. The trick is choosing what out of the baseline to implement right now and what we can defer. And, changes to the base-line should not be made without valid justification.

DR. CANTLON: It was essentially that issue that I was wanting to get a look at the interplay between performance assessment, input from the investigators, and administrative decisions. Those are, you know, sort of characterizing three inputs to a tough set of decisions.

DR. DYER: Right. The performance assessment, per se--let me take, for example, the ITE model. Performance assessment, per se, wasn't a direct feed. I mean, however, I think the results that we have got out of the performance assessment program over the last three or four years honed the intuition or the understanding of the experts who made up the panels who were polled for this particular activity.

MR. GERTZ: I'll just expand how that process works in real life at the project. As I think I alluded to before when we started out with--when I knew we were going to get about 240 million, we set our priorities and I told my staff this is all I want you to do. Tell me how much money you need to just do that. Well, I didn't get a total of 244 back. I got a total of $310 million back. And, that was to do only what I wanted to do. So, we had to work our way through that and see which activities or which ones we could defer and that involved both the scientific community, it involved the engineering community, it involved the administrative community. And, first, I went through with my division directors and then we allocated money to the lab, the USGS, and I heard appeals from Larry Hayes and his peers about what they thought they could or couldn't do with the money I allocated them. And then, my staff and I made a final decision and said, now, let's go do the work.

DR. NORTH: Shall we go on? Max, I believe you're last on the formal program or agenda for discussion.

MR. BLANCHARD: My talk in closing will be, I think at least from my view, anti-climactic. What I'm really going to try to do in only a few viewgraphs is simply tell you what we tried to tell you in hopes that we got the message across. If we didn't, I apologize. Maybe, we'll try it again sometime.

Russ just used this. Our siting decision is clearly where we're going, but we always keep in mind that the siting decision is not based on the intrinsic properties of the site. The siting decision is a system decision. It includes everything we can glean from the design of the engineered barriers combined with the properties of the site and the processes that are operating at that site to change the characteristics of the site with time and the data providers are providing us information which allow us to estimate the magnitude and the recurrence interval of those changes, but not directly the impact on waste isolation. That, in itself, is much more complicated and it needs all that data, it needs the design, a finished design, of an engineered barrier or at least a design mature enough so you can make adequate scoping calculations and then the performance assessment people do the best they can among the critics. And so, obviously, for a lot of studies, we're in this loop. We've not yet crossed a barrier that would cause us to think we're here and, of course, we've not gotten mature enough in this understanding the site process to say that we're here.

The management process, we follow a law, we keep in mind all of these applicable regulations. The data we collect and the engineering designs must be adequate for licensing. We don't want to do this in a way where we can't take advantage of the money we spent and the things we did during the time we characterized the site and then have to do it again to get proper documentation for the license application. We are following lessons learned from a management standpoint. There are some nuclear power plants who did not have adequate records and could not verify the as-built condition. We have lots of good expert advice on how to avoid those kinds of problems and we're implementing them.

You've listened to several speakers talk about the planning phase, the test implementation phase, and the data use phase. One thing that perhaps wasn't clear at the beginning, but I'm hoping by the series of discussions today it becomes more and more clear, that we've set up kind of a separate process for those involved with the data providers and those providing the information that relates to waste isolation. We felt there needed to be good communication and that everybody--the design team, the performance team, and the regulatory team--need to use the same data that the data providers or the testers are acquiring, but we also felt that there needed to be a separation. And, the separation starts right here in the planning phase because limiting the controls of adverse impacts on waste isolation during the time you conduct site characterization is where the performance assessment people get into the picture right at the beginning. How much water do we put on the roads and paths? What kind of adverse effects can occur to the long-term waste isolation if you're removing the topsoil from the mountain? Those kind of things, we get the performance assessment people into right in the beginning, as well as this phase over here.

So, we've talked through the step-wise sequence we go through. We recognize that we have some things in planning, but we're mostly through the planning phase. The culmination of the maturation of that is the study plan, but we make changes to study plans and recycle them. We're deeply involved in test implementation from the surface-based program and are about to embark on it for an underground test program for in-situ tests from the exploratory shaft.

We've talked about data use and evaluation and I think most of the speakers have aptly described processes that are applicable there. Yesterday and today, we've talked about using the data use evaluation process, and interestingly enough, this side was used very early-on. Our team of people or the PI that's collecting erosion information on his own said I'm not sure I want to spend any more time collecting this kind of information. Let's see if we can get down this path and come out here with a topical report. But, nevertheless, the management is not waiting for the principal investigator to say I think I'm finished. We have this interim evaluation process whereby with our eyes and ears open, hopefully, we're listening to input from the oversight bodies, we're looking at the issue resolution strategy and the test strategy, and we're using on a day-to-day basis peer review, technical assessment review, and anything else that seems to be prudent from a technical and from a management standpoint in an attempt to get down here in a reasonable way that will be meaningful with respect to showing progress.

To be sure, as you all have just identified, there is a DOE management dilemma and Carl shared a cycle that he goes through every single year when he says here's what I'd like you to do given the amount of money I think I'm going to get. We do this before the beginning of the fiscal year starts and he invariably ends up with a list of things to do that's a lot more than we can afford. We go through a sorting and a prioritization and there has to be a balancing. For instance, without site access, without permits, without taking proper care of Native American cultural resources, without taking care of the desert tortoise, we can't get there. And so, those things, pre-activity environmental surveys, absolutely have to be done; otherwise, we're violating the law. So, there's a lot that goes into moving this process forward and tradeoffs between testing, design activities, the environmental program, and public involvement and outreach have to be made on an annual basis and Carl does that and he explained that.

If there's a challenge, I think, from a management viewpoint that those of us that are at the middle level of management, Russ and myself and Bill Simecka, Dale and Carl, it's trying to create a management environment where we facilitate and enhance the opportunity to get synergism between the performance assessment specialists that we have, those people who are specialists in interpreting the regulations, the testers that are acquiring the information to describe the processes acting at the site, and the design team. And, by creating a working environment that will allow us to get not only communication, but the effect of teamwork so that the whole is better than any one of the independent parts, we feel that's our goal as managers.

With that, what I'd like to do is to close on our view of the issue resolution process that we're trying to implement today from a management standpoint.

DR. NORTH: Comments and questions from members of the Board? Dennis Price?

DR. PRICE: I've kind of been quiet this day. Some may have not noticed. But, I would like to make up for that by getting some equal time here and see if we can't from a systems engineering perspective look at maybe what's gone on and see if it sounds a little different or sounds the same today.

I heard the term "life cycle" used a couple of times, and if you look at the life cycle of this project, it would involve some phases--maybe you'd name them differently like concept phase, design phase, development and construction phase, operational readiness, or test and evaluation phase, operational phase, and then maybe closure phase; something like that for a life cycle view of it. If I were to try to figure out where has most of this stuff been couched that we've been listening to in the last couple of days, in the concept phase.

We're dealing with concept things. If you look at the concept phase, where does it come from? First of all, from the mission and then from the requirements. And, I think the program, in general, has had a tendency to get the shoe ahead of the foot or the foot behind the shoe or some-thing like that. Things kind of get out of phase and we know that the requirements document has been long waited for and some 6,000 some requirements put together and so forth and yet you have to move ahead. But, that's not quite the way it really ought to be.

But then, you look at the functional flow and the functional allocations that come out of that and the concepts that need to be developed. I see four concepts that need to be developed kind of in parallel and maybe you decide which gets what when in some way like critical path, but I know you've had a lot of trouble with critical path because of being jerked around by external forces that set controls elsewhere.

But, the four major things that I see are the concept of the mountain. That's the environs and so forth and I'll talk a little bit more about that. Then, the con-cept of the EBS is another one; concept of interim storage is a third; and the concept of transportation is the fourth one. These concepts go together when they're put together to be the system concept, as I would see it. And, we're dealing mostly in this stuff that we've been talking about in the concept phase.

Within the concept phase, I see the environment or the environs and mountain concept being where tradeoffs are being made to understand or discover, more than anything else. Rather than saying I'll take this concept of a mountain or that concept of a mountain, you've got a mountain and you've got the environments and it's geohydrology and geology, seismicity, geochemistry, geothermal volcanism, climate, and even the futurist human intrusion kinds of concept. The ESF is part of this as part of the environs and the mountain concept. But, that concept, as you're developing it, doesn't stand alone, as I think Russ was pointing out toward the end.

And, you've got the EBS concept and within the EBS that should be running in parallel, maybe with some strength, there's the package concept and the placement concept. In the package concept, you've got thin wall versus robust that you've talked about. In placement, you've got in-drift, vertical holes, horizontal holes, and so forth.

Then, in interim storage, being the third of those concepts that need to be developed under this concept phase, we have location and function at stake. Location being MRS, at reactor, federal facilities for interim storage and functions being is it passed through, is it a consolidation, are you making repairs, what are you doing during interim storage, and what are the functions of an MRS and so forth. And, I see that going on necessarily.

Interacting, when I gave the initial life cycle, you've got all kinds of endowed feedback loops and then interactions among all of these like transportation is the fourth one. And, it interacts with interim storage and it interacts with EBS and it interacts with the mountain concept. And, in transportation, you have the mode with rail, highway, or maybe even a little bit of barge. And, you have the packaging with multi-purpose container, the universal cask, the dual cask, the over-pack concept with the container, the single container. And, all of these things competing kind of together or in the mix together to finally come out to a system concept that you resolve as a system concept that says now I can get into the design side of things. And, there you get into the--no more cartoons, but hard line drawings and procurement, getting ready for procurement, and things like this.

So far, though, the system, you get cask procurement going on way ahead of things, requirements coming out way later than things, and things not intertwining and fitting nor having the dynamic kind of feedback loops where you try to check something out and say we looked at that, it's over with, that I think ought to be in this overall thing.

So, I kept quiet for a long time, but at least I got this chance to say my little piece.

DR. NORTH: Thank you very much, Dr. Price. That was marvelously well-put. I think as much as I heartily support that speech, I will endeavor to defend the presenters from the Department of Energy that we really asked them to focus on resolving difficult issues, infiltration and future climate. Given that was the focus we asked for, I don't feel I can be disappointed that we didn't get more emphasis on, let us say, beyond the concept of the mountain. However, I was intending myself to make some remarks about, gee, we didn't hear much about the interaction with the design decision and I'll come back to that in a few minutes.

DR. PRICE: Yeah, I thought one of the goals was to use this as an example to understand how DOE will fold such issues and things into the entire process. And, in part, I thought that overall view was lacking, but maybe that's my perception of what I thought was going to go on versus what was really the intent of it.

DR. NORTH: I will speak for myself. I would be happy to reiterate my critical remarks from January, but I think it might be impolite to do so. There is a real deficiency here in the overall program as opposed to the project. To get these systems' aspects well-thought out and get the shoe onto the foot or the horse in front of the cart or out of ready, fire, aim and then to ready, aim, fire, there are many ways one might express it. It clearly needs to be done. On the other hand, I think it would be reasonable to commend the DOE in this presentation for within the concept of the mountain having done a reasonably good job of applying systems engineering principles. Would you agree?

DR. PRICE: Yes. Well, I think we did say in the conceptual phase and in the mountain concept and I guess I was just trying to put it into the overall picture.

DR. NORTH: Any other comments, questions, little speeches?

(No response.)

DR. NORTH: Well, in the silence--let's see, how about the staff?

(No response.)

DR. NORTH: Well, let me give my little speech because I think it follows reasonably on yours, Dr. Price. To do that, I'm going to ask Russ to put up two of his viewgraphs and I just warned him in advance that I'm going to do that. Whoops, did they disappear?

DR. DYER: No.

DR. NORTH: I was giving you warning so you could find them.

DR. DYER: I've got them.

DR. NORTH: Basically, the things I'd like to emphasize are communication and expert judgment. I think either if we view it as design phase and within the mountain concept or I hope, as we will view it in subsequent meetings, getting into the other aspects that my colleagues so beautifully articu- lated which very much needs to be done, the crucial issue here I think is getting the various players within a very complex operation.

On the left, we have the players and their deci- sions and decision criteria laid out there. I don't know if it was said sufficiently strongly that there needs to be teamwork here. This can't be a situation where everybody on that left hand side is off doing their job without thinking through how do the other people in the system do their part and make decisions? So, in particular, DOE has to anticipate how the regulator is going to be making decisions. The regulatory analyst, performance analyst, designer, and site data providers all have to be thinking about their inter- action with each other and with DOE management and how the collective result is going to play against the regulator decisions.

That is a very difficult thing to accomplish and I think it becomes even more difficult to accomplish when we consider the tools that assist decision making. I count four out of six having expert judgment in there. With respect to the designer and the regulatory analyst, I have difficulty conceiving how those individuals can do their jobs without strong reliance on expert judgment, as well.

Would you agree, Russ?

DR. DYER: I concur.

DR. NORTH: Okay. Then, we go into the issue of expert judgment and how does one deal with that? That was the subject of a workshop last fall and I just got the summary documents on that, one of which is entitled technical summary. And, I'd like to read into the record the way my closing remarks were summarized. It's about 10 lines. So, it won't take long.

I think I'd like to read the last line from Dr. Bartlett's preceding comments. As Dr. Bartlett posed the questions of "What unusual difficulties the OCRWN program may face in demonstrating compliance and how the use of expert judgment might help resolve those difficulties." I think that's directly on the subject of your left hand slide. Then, "In his closing remarks, Dr. North praised the workshop enthusiastically. He went on to say that he agreed with most of what was said. However, Dr. North stated that the DOE has room for improvement and is still on the steep part of the learning curve regarding the use of expert judgment. He continued that lack of teamwork is the most important area for DOE to address. He also pointed out that it is important for DOE to prepare for NRC licensing. Dr. North added that improving credibility and clarity should be job number one for the DOE. He said that to achieve credibility, the public must believe either the process or the people. Dr. North concluded that there is often a tradeoff between common sense and formalism, but the project needs to have both and can get both through continued practice."

Now, I'd like to commend whoever wrote this summary. In some ways, I think it's a much better statement of what I would have liked to have said than I managed to say at the time. And, I think it very much bears on what we've just heard. I thought it was an excellent summary of my judgment of the last two days that you are indeed on the steep part of the learning curve and within the concept of the mountain and the design phase, I'd like to commend you for a very great deal of progress, I think, on how far this thinking is coming from the time at which I joined the Board and I'd like to commend you very enthusiastically for a job well done. On the other hand, I think there are many areas in which progress can still be continued. It's still the steep part of the learning curve with a lot of steep part to go.

I made my little speech on the climate modeling issue and that's one where getting somebody in from left field to be much more part of the core team would seem to be indicated. I think that issue of how wet can it get is a crucial issue in the program. And, if you don't conceive it as a show-stopper, you really should. I think having a credible story with the combination of all you can get from the modeling exercises and the expert judgment is going to be a central issue to you. And, if the tendency has been let's do the geo-sciences because we understand that, we know how to do that kind of modeling and analysis, please consider that this issue may be the most important driver or one of the most important drivers to all of your hydrological analyses and you really need to have it understood in detail. Now, maybe it can be dealt with by a bounding calculation that indicates that it can't get so wet as it's going to create a problem for site acceptability. But, given that the atmosphere is changing dramatically from the last million years and virtually everybody in the community knows that--that is the community doing climate analysis--I think you really have to look at this one very carefully and determine what is going to be the driver in terms of future climate and place more emphasis on that as you do the top down performance study.

Then, the next point, I think, ties into Dennis Price's speech. You really need to give more effort and emphasis on the design decisions. How does this whole process feed into your decisions on how the repository is to be designed? You might have made more of that. I see the extreme erosion issue is coupling immediately into do you want to have or can you have a flat main drift and a flat repository--that is a slope within a degree or so--so that it would be easy to use rail transport within the repository? And, couple that into the possible use of drift emplacement. I'd love to see that set of issues laid out in detail as a decision that needs to be carefully examined and supported. It's certainly in my judgment deserves, at least, the prominence of some of the things that you described to us in detail as to the decisions into which this leads. That one is relatively immediate because you're going to make a decision on what the angle of the tunnel boring machine is coming up rather quickly. I think others have commented on the need to look at the rail issue and emplacement as crucial decisions in the way ESF is going to be designed, as ESF may lead into a repository.

I'd like to go back a second to the climate issue and just say what about the issue of if it gets wetter, what will that mean in site acceptability and what do you need to do to do the analysis? It strikes me that a crucial issue is how much is that water table going to change? It was on your slides. And, I think where it leads is you've got to understand what happens in the saturated zone and how much that water table changes per unit of recharge. How big is the underground lake may be the simple minded way of thinking of that and I haven't seen a good analysis of that issue. I think I read in the letter on the Stanford stationery from the group in the National Academy that they think that's a really central issue. I would have to agree. I think it's a very important issue and I would hope you would have it high on your agenda to try to understand what you need to know to deal with it. Is the existing program for putting in the deep boreholes and doing the hydrological modeling of the saturated zone, which we didn't really hear about, is that going to be adequate for an understanding of does getting water to some degree mean a potentially unacceptable site?

The other issue that I think is really central where we've heard a little bit about it, but factoring it into this analytical framework, as yet not well described so I understand it, is the issue of thermal loading. That again is a really critical issue for the program management and getting in place the analytical tools to help you do it would seem like an extremely important need.

So, I'll conclude by saying I'm very enthusiastic about what you presented and how much progress you're making going up the steep learning curve and I would commend you to keep it up.

DR. LANGMUIR: To provoke a little more discussion, I think the USGS has some information I'd like to hear on this. I have heard that--and, correct me if I'm wrong--that even if we assume a maximal precipitation that currently is being considered in the models, all you're going to do in 1000 years is add 10% to the moisture content that currently exists in the unsat zone. And, even at that, you're not likely to raise the water table more than--I think the figure was perhaps 80 meters or so. Could somebody correct me or clarify what I'm saying? Is that what the thinking currently is?

MR. LUCKEY: Dr. Langmuir, I think you're essentially correct. The USGS did some modeling simulations a couple of years ago and continues to look at the effect of increased recharge on the saturated zone. They did a "what if" sort of simulation that included 15 times current effective moisture. They saw something less than a 100 meter rise in the water table. Now, the National Academy of Sciences took the survey to task, perhaps incorrectly, saying that was maybe a little way out. And, listening to Marty Mifflin when he was talking about maybe 10 times the increase in effective moisture, perhaps the survey was a little too far out. I wouldn't want to say that that's the final answer. Hopefully, down the line, we're going to have better models, but that model seemed to provide some sort of understanding that the water table is probably not likely to raise 1000 meters beneath Yucca Mountain. That's just not physically possible. We may find 100 meters, plus or minus 10 meters, but we're not going to go off the deep end on it. It's not a closed issue, but at least we have a significant amount of information. Perhaps, in terms of the work that the Survey is doing, what future climate does to flux through the unsaturated zones is a more open issue that we need to look at in much more detail.

DR. NORTH: With 1000 meters, I think the mountain goes under the water. So, it doesn't seem--isn't about 200 meters the magic number? Again, I'll go back to my statement.

MR. GERTZ: We're about 700 to 1200 feet above the water table. Being a civil engineer, I still operate in feet.

DR. NORTH: Yeah, okay. But, that translates into about 200 meters as I count it. I think in feet and inches, too.

The public must believe either the process or the people. I think if you can get that calculation so it's really clear and everybody believes it, that is a very, very important accomplishment in issue resolution. And, if you can get from some combination of the paleoclimate, the model-ing, consideration of rain shadows, and where does the weather come from, then it can't be any worse than 10 times today's level. I'm not sure whether that's an infiltration or precipitation complicated relationship clearly, but if you can bound that as essentially here's a set of calculations from model process people that you can believe that we can't put the repository under the level of the water table in 10,000 years, that is an extremely important thing for the program to be able to show.

MR. GERTZ: Dr. North, I'll just add and I think Dick pointed out that when the National Academy looked at water level at Yucca Mountain, how high it would rise, I forget-- they talked, just as Dick said, about the survey model and they thought it was very conservative. But, they said we'd better have some more information.

DR. NORTH: Well, my sense is that if you think that you're right and it's very conservative, if you can get that accepted within the scientific community, not just the eight people that were on that panel, that's an extremely important accomplishment in terms of bounding out a very large and great area of uncertainty.

DR. DOMENICO: Marty Mifflin had some points, I think, that he wanted to bring up and talk to the people who were in the session on regional climate.

MR. MIFFLIN: I had a question that I didn't get a chance to ask Dr. Thompson on the discussion on his modeling with respect to paleohydrology calibration and paleoclimate calibration. If I understood his statement that the grid--the number of elements in the grid for the state of Nevada was about 100, I wanted to ask him a question with respect to how difficult it was to reduce that grid space?

DR. NORTH: Would you care to respond, Dr. Thompson?

MR. MIFFLIN: Let me just elaborate why I asked the question. The paleohydrology data that I tried to present divides up the Great Basin into basins which have a quantitative result from a paleoclimate, hydrologic result. And, that the grid space that you mentioned doesn't give enough resolution whether the very apparent gradient and the differences in climate right about at Yucca Mountain--or paleoclimate or paleohydrology. So, if you would lose the sensitivity for calibration of your--or at least comparing paleohydrology with the climate.

DR. THOMPSON: That's correct. To try to match up the model graded results with its 60 kilometer grid with very site-specific or even less site-specific results. Like the paleoclimatic information being derived specifically in the Yucca Mountain area would be difficult. To do our validation, we're going to have to be somewhat more general than that which is why we're going to probably choose a time when, for example, a larger area of the west is wetter. So, we're probably not going to try to validate against Yucca Mountain data; we'll probably try to validate against, say, general hydrologic conditions for, say, Lake Bonneville time where the scale match would be better suited for what the model can actually resolve. So, the validation won't be particular to Yucca Mountain or even that section of Nevada, but it may be a validation that the model can produce a reasonable climatic change in part of the world, namely the western United States, that at least is relevant to Yucca Mountain. As I said, the model can't get down to the Yucca Mountain scale without some further extrapolation or some other modeling activity that takes it down to that scale and we don't have that activity lined up yet.

DR. DOMENICO: It's my understanding, Marty--correct me if I'm wrong--that a few basins north of Yucca actually housed the lakes during the Pleistocene.

MR. MIFFLIN: That's true.

DR. DOMENICO: Will the resolution be good enough to at least reflect that? Do we have at least one node per basin?

MR. MIFFLIN: That's what I was trying to get at and my concern with your answer is that the sensitivity of the hydrogeology, if you will, or the paleohydrology of the basins is such that if you calibrate it on Lake Lahontan or Lake Bonneville which is about your appropriate grid scale, you really lose the sensitivity based on that paleohydrology information base of very, very important differences between that part of the Great Basin and the Yucca Mountain area. And, we're talking about differences that mean something like, in a kind of a relative sense, basin indices that I played with that are like .4 or .6 in value versus indices that come down to .02 right at Yucca Mountain. So, we're one order of magnitude difference in hydrologic response to the paleoclimate.

DR. NORTH: Bob Williams?

MR. WILLIAMS: Thank you, Warner.

First, let me compliment the project and the TRB both. I think we in the utility industry are particularly pleased with the progress that has been made in the coming new era of experimental data and new experimental interpretations. In some ways, I'd like to offer you tremendous compliments for steadfastness. This has been a tough past 10 years. Now, let me draw a little simile between 1983 and 1993. In 1983 when the Nuclear Waste Policy Act was passed, the Act mandated that there would be an EPA criteria in 1984 or early '85. Now, in 1993, there has just been the Energy Policy Act of 1992 passed and it has mandated that there will be an EPA criteria by 1994/1995 with conformance by the NRC. Now, I thought that would draw a laugh.

But, the first lesson I would like to draw from that is there is a need for redundancy in models. I strongly endorse and the utility industry strongly endorses a focusing of modeling, such as we heard planned in the M&O. But, I come here from a meeting with the NRC at Lawrence Livermore Laboratory where the lack of a model by the NRC kept a model by the applicant, in this case the utility industry, from being approved. And, it's a very simple issue. It's the extent to which a piece of concrete breaks when you drop something heavy on it like a cask. It doesn't have all of the razzmatazz; the vadose zone, hydrology, conductant coef- ficients, and all that razzmatazz. This is something that you can do an experiment on for $15,000 and we're still arguing after two and a half years because we didn't pursue the development of redundant models.

The second lesson. One becomes captive to the construct that the model was originally intended for. As one example, for the past 10 years, we've heard a lot of discus- sion about modeling of undisturbed hydrology because the current regulatory construct makes that an important parameter. And, I'd be the first to agree that understanding the undisturbed hydrology is known for going to the disturbed hydrology. But, you inevitably become captive to the complexities of your original model.

So, the short answer to this preachment is periodically it should almost be an article of faith that you pick a new bright guy and tell him, goddamnit, start over. Or, goll darn it, excuse me. Now, in this context, I think we need to have somebody start over modeling the disturbed mountain with the tunnels in it because if the permeabilities of the tunnels is going to be one number and the permeability of the mountain is a dramatically different number, then the disturbed hydrology and disturbed performance is going to be governed by the tunnels and the permeability of those constructs, not the permeability of the undisturbed mountain. And, when I hear people talking about the microns that are in the crack width and nobody has done an analysis that says how the thermal pulse in the mountain changes the cracks and how the periodic earthquakes are going to make the cracks shake, rattle, and roll, then I wonder really about all of the modeling and the characterization of the undisturbed hydrology, whether it's really very valid.

The point that falls out of that, are our second order of parameters being focused on to the exclusion of first order parameters? The short answer is yes. And so, I think that can be addressed by looking at what really the end use of the analysis is going to be rather than some of the steps along the way that proved compliance. But, let me hasten to add that I couldn't agree more with the thesis that Russ Dyer has taught that it depends.

Finally, I bring this subject up really to help gain support for it in the program, not to cause anybody embarrassment. But, the utility industry and even Robby Robertson of TRW have urged that there be some sort of capability for a quick look. That is that when you are in this new era of getting experimental data, there needs to be the capability to quickly interpret it in the system's context. So that the experimenter out on the mountain isn't asked to infer the impact of his latest result on the big picture, but instead it's being done at the systems level. Now, there are all the human factor kinds of things that enter into that. You want to give Alan Flint, for example, his time in the spotlight, but at the same time, you need to give somebody with a quick look capability and the job of doing that, the role of standing up and saying here's what it means in the larger scheme of things.

So, let me just close again by saying how pleased the utility industry is. The steadfastness is evidenced by the fact that they have indulged keeping me on the payroll for over 10 years coming to meetings like this. I hope we can make it productive now as we start to get real data and real results.

DR. NORTH: Thank you.

Max, did you want to respond?

MR. BLANCHARD: Well, I feel compelled to ask you a question. I certainly have the same feeling for need to link climate and hydrology and then look at the consequences that you do. And, I think a number of us in the program, as well as not only the investigators, but the managers, recognize that. But, at the same time, I'm struggling with respect to a leap which you seem to have made that I can't quite make and that is to link that to a measure of suitability of the site. Not that they're not related, but that if we're deal- ing with the natural processes, then we have a certainty of, one, of a significant climate change, or a pluvial occurring here at Yucca Mountain. That may also be true for every other site in the world that's currently being considered for disposal of radioactive wastes. And, I can't help being reminded that all the other sites already are in the saturated zone. And, so maybe now is the time to make sure that we have a very flexible engineering design team and we take advantage of concepts in the processes that are here in the models, as well as the design approaches so that if we have to deal with that in the future, 10,000 or 100,000 years in the future, we have accommodated the design so that it could accommodate that eventuality. In other words, recog-nize that the design process is going to be a good bit more complicated than we thought. We don't just design something for an oxidizing condition and assume that there are mechan-ical processes operating on it, but that there aren't any-thing as serious as a total climate change and that, all of a sudden, it goes from an oxidizing condition to a reducing condition, but that we straight forwardly recognize that as a possibility. And, in order to get the best engineering design, we have a design approach which is not mutually exclusive to those two environments, but can work reasonably well in both.

DR. NORTH: Well, I think you've answered the question. I don't need to. If, in fact, a credible scenario is that the repository will go into the saturated zone during the period of interest, 10,000 years, the question then becomes does that compromise the safety of the repository? And, there ought to be an answer to it. The fear that I have is, looking at the experience with WIPP where human intrusion was something that was discovered after the repository was largely built, as a potential show-stopper from the performance assessment and I think the lesson there was you ought to really work hard to try to anticipate these things so that you can allocate your resources and base your design decisions on those types of scenarios rather than getting caught at the end of the process and, frankly, not have the resources or the time to be able to do a good job. Because I think the lesson is that will put you into decision gridlock costing a great deal of money and great anguish among all the concerned parties that what everybody had hoped to be a relatively simple process suddenly got far more difficult and complicated.

MR. NIEDZIELSKI: It's always interesting to be an observer and kind of an affected party and come in at the tail end of things and you kind of wonder whether you even want to interrupt the profound thinking going on for something so mundane as the question I'm going to be asking. But, you take advantage of the opportunity to get up here.

I wanted to get some clarification, if I could, on this question of issue closure. I noticed on the slide dealing with issue closure that topical reports come down to NRC reviewing comments. We've noticed as observers that the --is going on right now with NRC and DOE relative to this issue of closure and, of course, we're participating ourselves. We have some concerns about it. But, the topical report concept is of particular concern because we look at it from a--if we look at it from a reactor standpoint, we see topical reports having a fair degree of finality to them within the licensing consideration closure, if you will, in a real sense. I would like to get a sense of how DOE is looking at topical reports, and in your best world situation, what do you want to get from NRC with these topical reports? What type of closure are you looking for?

MS. JONES: Let me ask a question first. Have you had a chance to look at the NRC staff position paper? I think we're on the same lines with them that a topical report is to be used at that time when you have to consider data to address a particular regulation or particular type of--or particular methodology. So, it's something that's used judiciously at the end of a particular phase of a project. Let's say in the case of methodology what we'd be looking for is agreements of the staff that that particular methodology is acceptable. This is something we wanted to talk with them about at the meeting. We're a little unclear about the language. We're going to ask the question if they are also considering topical reports acceptable for use in addressing the compliance of a particular regulation.

MR. NIEDZIELSKI: Susan, let's talk about specific--the erosion topical report that--how do you see that? What are you trying--what were in the best of all worlds your desires --what did you want NRC to do with that topical report?

DR. NORTH: Susan, could you use the microphone?

MS. JONES: Sure. Thank you. We would like to see a safety evaluation report returned that said at this point there were no further questions on the subject of the potentially adverse condition of extreme erosion. Our position was that we could not have extreme erosion during quaternary and, for the time being, the staff agreed.

There's also included in that a description of the approach that we're going to take for looking at potentially adverse conditions. First, we would look at the condition and determine if it existed. If it existed, you would do--if it did not exist, then you would have to do no further evaluation. If the potential adverse condition did exist, then you would have to go through the analyses required by 10 CFR 60. And, we had also asked for a response to that approach to dealing with potentially adverse conditions.

DR. NORTH: Is there anybody present from NRC that would like to comment on this issue?

MS. ABRAMS: It's a little inappropriate actually for me to comment to any extent. We're scheduled to have a meeting on May 3 with the DOE on this subject and we expect to have some input from the state and the affected counties. We do have something out on the street, as Susan mentioned, that gives some perspective as to how we're looking at it. And, I can provide it to Warner if you'd like to see it.

DR. NORTH: Yes.

MS. ABRAMS: Okay.

MS. JONES: I didn't realize that you all hadn't received that.

DR. DOMENICO: I've asked Alan and he gracefully accepted to put one of his slides on the board here and maybe he'll clarify me and all of us. I'd hope that Bo or Ed would chip in where they want or, for that matter, any qualified or unqualified soil physicists. I consider myself an unqual-ified soil physicist in the sense that I'm not a soil physicist, at all.

DR. FLINT: Can I take two minutes at the microphone on my way back just to clarify a point that Warner was talking about?

DR. DOMENICO: Sure. Oh, sure.

DR. FLINT: Just to sort of butt in.

DR. NORTH: By all means, get in that spotlight.

DR. FLINT: I wanted to make a point on something that you had said earlier I thought was important to show you that the project is actually perhaps a little further ahead than what you might have thought. One of the things we needed to talk about there was like the global climate model. We have to be aware that it is not necessarily wetter conditions that make it more recharge to the site. And, if you recall, several years ago, I made a presentation--actually, it was a long time--on regional meteorology program where we showed in the infiltration program how you could get more recharge through the site under drier conditions by simply eliminating the monsoonal rainstorms and increasing winter. But, I wanted to point out that what we're doing now is we're actively looking at the kind of thing you were talking about. What is the direction of the storms from and how much rain do we get from the southeast or southwest across the Sierras? So, we have a very active program classifying all of those storm types and then we want to--when we talk about stressing the system to failure, we want to use a model that includes those storm types so we can define whether we get a lot of them through San Diego which is what we've had this year--the most water getting deep in the system does come through San Diego--and then tie that with the global climate model to see where the jet stream is, what causes the jet streams to move up and down that would cause this storm type sea to come in more frequently and tie it in with what they're doing. But, the project is actively involved in trying to classify the storm types that come in from these different regions. I think we're making actually pretty good progress on that.

DR. DOMENICO: All I want to do is reason with somebody about this to see if I understand it because my old chemistry professor once said say not this is so, but so it seems to me to be the thing I think I see.

But, my observations on this is if we take the upper members above the Topopah, especially the non-welded members, a rather high degree of saturation. They also have a rather high porosity, maybe 20 or 30%. So, there's a bunch of moisture up there, but they're not at saturation. And, if there's any connectivity of those pores and hydraulic conductivity being a function of moisture content, maybe they had a reasonable hydraulic conductivity. I don't know. But, they are certainly in a reasonably high level of saturation, .8 or better. We don't have much of the Topopah Springs there, but what I'm told about the point measurements is that the degree of saturation is also high, maybe 50%, a little bit more than 50%. But, the porosity there in that welded member is about 10%. So, 50% of 10% means we really don't have a lot of moisture in the Topopah Springs matrix certainly compared to the material above. And, in the sense that 10% porosity, low moisture content, perhaps the hydraulic conductivity is not as large in the matrix as it might be for that saturation in the upper units. And, even though we don't see it here, if we go down below the Topopah Springs, we find something similar to the Tiva Canyon, even more so; unwelded, high porosity, extremely high saturation levels, 90%, a lot of moisture. And, if there's any inter-connectivity between the pores, we would expect to have a reasonably high hydraulic conductivity for that moisture content.

Now, let's reason together. That Calico Hills is unsaturated. The question I would ask is the flux coming out--and, let's make another assumption that, you know, at least with respect to the present climatic regime we're at somewhat of a sad state, but my question is how can we main-tain such a high degree of saturation in the Calico Hills, a very high degree, maintain that high degree of saturation in the absence of a reasonable flux across the Topopah Springs member boundary unless it's coming laterally? I would think that thing would be if there was a low flux across that boundary, we would not maintain that high degree of satura-tion. That's one question.

The other question is what is the flux into the Topopah Springs across the top and how does that flux take place? We're going from a high moisture content, maybe reasonably high permeability material to something low moisture content material, and if that flux takes place within the matrix and continues in the matrix, I don't see how we can get a necessary flux out the bottom to maintain the high degree of saturation. So, my question is is it possible that those fractures in the Topopah Springs are taking that flux and moving it through the Paintbrush? Now, we know that's against theory because we're supposed to--things at atmos-phere pressure aren't supposed to take on water from materials in some sort of suction, but Apache Leap demon-strated that that's not true. You can have 95% saturation and the fracture still will take on water. So, if we need a large flux across the bottom to maintain the high degree of saturation--I'm trying to articulate this as best I can--is it possible that there is water flow through the fracture system in the Topopah Springs member, more or less, avoiding the matrix and, if that is so, it would be continuous? We would expect to have a lot of perched water and I don't see anything wrong with perched water, but I mean--so, my ques-tion is how do you maintain that high degree of saturation in the Calico Hills? Is there an answer?

DR. NORTH: We have a candidate who'd like to try to answer it.

DR. DOMENICO: Now, if it's an unreasonable question, tell me, Alan.

DR. FLINT: No, I think we have some information that might address that. What I wanted to put up next to this slide just to show you some confirmation, then we're going to go to one of the other slides.

DR. DOMENICO: You've been ready for me, haven't you?

DR. FLINT: If you look at the saturation, this is very high saturation. We go to a low saturation and then a high saturation. We cross this very, very thin layer which is something that was kind of surprising and then back to a lower saturation. If you look at Ed Kwicklis' data now, what you're going to see is this zone right here, this near high saturation zone, is this same material. Then, we go to a lower saturation, back up--back and forth. What you're missing is the one data point here which is a little higher than one, but it's fully saturated or near fully saturated. This is the cap rock and then it drops back down again. So, if you look at this, you're seeing this same profile again.

DR. DOMENICO: Agreed.

DR. FLINT: So, one of the things I wanted to show you is that as far south, south of the repository, or as far north as we go and based on the drilling we've done on about seven other holes where we went through the unit, this is all the same.

DR. DOMENICO: I don't disagree with the distribution.

I'm just--yeah.

DR. FLINT: Could someone go to the next slide for me? Okay. Now, when we were looking at this slide, this was that model analysis that we tried to do and we tried to look at the system in an equilibrium and that's what this orange line was, an equilibrium with the water table. So, we had the higher saturation here, about what we see in the PTN, this high point and then back low again, but this was not consis- tent here. We had to have a higher flux because we need to have some kind of flux through here to keep this saturation up.

DR. DOMENICO: Where are we there? At the top of the Topopah?

DR. FLINT: No, we're at the top of the Paintbrush non- welded tuff.

DR. DOMENICO: Okay.

DR. FLINT: Now, what I want to do--well, we have these fractures and I may actually get the opportunity to show those slides from yesterday I get to show. Could you go to about six or seven slides forward and I want to show the picture--keep going. These are the porosity numbers. So, you're right, there were some low porosities in a couple of points that are very important. But, go ahead, a few more. Keep going. This one. What we're talking about is the high saturation for one thing, nearly 90%--90% saturation.

DR. DOMENICO: Where are we again?

DR. FLINT: That's in the Calico Hills. That's the Calico Hill unit.

DR. DOMENICO: That's what we said. Correct.

DR. FLINT: You can keep this saturation high because of the capillary forces that can move water and this blue line is in equilibrium with the water table, no flow at all. And so, with no flow, you can see that you have areas of higher saturation, lower saturations, very low. Again, this same point at the top of the Topopah, very high, and in this location. But, under no flow conditions, it's just too dry, but you do see that we have these varying saturations because of the rock properties and because the unsaturated characteristics are very non-linear.

DR. DOMENICO: You mean, you maintain the high saturation because of capillary forces holding it?

DR. FLINT: Right, right.

DR. DOMENICO: But, we're talking about capillary forces in a nice--if it's in the Calico, a nice sort of porous type material with a reasonably high porosity, are we not?

DR. FLINT: Well, no, actually, in part of the Calico, we're seeing what we would expect to be low saturation. The orange line is 1/10 of a millimeter a year steady state flux through the matrix. No fractures yet. We have only come close to perched water in one location. But, again, we do get these high saturations under very low flux condition. We don't need a lot of water to keep these saturations high. But, we get quite a degree of change in the system. This is what we're looking for at UZ-16. Not this data, but we're looking for the data to answer these kinds of questions. We expect to see low saturations. If we see high saturations in this part of the rock--

DR. DOMENICO: Wait, wait, you're losing me again. Wait. Low saturations. We see low saturations in the Topopah. We see high saturations in the Calico.

DR. FLINT: In parts of the Calico, we do. We see high saturations--

DR. DOMENICO: Like almost fully saturated and that's about 30% porosity.

DR. FLINT: We don't have enough data, I don't believe, on this part of the Calico. Part of this is the non-welded Topopah. This is the real Calico that we expect to be at high saturations and we see these high saturations and these high saturations can be maintained by a simple equilibrium or near equilibrium with the water table.

DR. DOMENICO: Which means you don't need a flux, at all, is what you're saying?

DR. FLINT: You don't need a flux, at all.

DR. DOMENICO: You're calling on capillary forces?

DR. FLINT: Right.

DR. DOMENICO: You're saying it's not moving.

DR. FLINT: It doesn't have to move. In fact, you can see the difference between no flow and 1/10 of a millimeter a year. You don't see any change, very little change, because it's so permeable. Any water you put in, goes out. The permeability is very high already because it is too high in saturation.

DR. DOMENICO: That's right. That's why I would think that without a sizeable flux across the boundary, you would find lower degrees of saturation. That was my thinking.

DR. FLINT: You would find what?

DR. DOMENICO: You would find lesser degrees of--smaller saturation. The question is how do you maintain high saturations in a situation where I can't see you have much of a flux?

DR. FLINT: Very small flux. A very small flux can maintain high saturations in--

DR. DOMENICO: Through the Topopah, right?

DR. FLINT: Through the Topopah--this from here to down here is all Topopah. The whole topopah Springs repository horizon is somewhere in this region, but again very high saturations, near 95%, with a very, very small flux.

DR. DOMENICO: Okay. My question--another question was why do you preclude that that flux of the Topopah doesn't take place through the fractures?

DR. FLINT: In this particular case, the saturation, although it is high, it is only 95% saturated which is way--it's too small for the fracture flow to start or to occur. We need higher saturations for fracture flow.

DR. DOMENICO: That's theory. That's--yeah, like I said, at Apache Leap, they found fracture flow occurring at 90 or 95% saturation.

DR. FLINT: No, no, you can get fracture flow occurring like they have at Apache Leap and I agree with that and I have--and, I get to do this now. This is a case looking at the top of the Tiva Canyon and this is a water content profile over time. What we've generally seen is very little change except for this last couple of years--or over this last season. We're seeing a pulse of water in this case down to about five meters. This has to be fracture flow. There's no other way to get that moisture there. This is on a north facing slope. If we start on the--I'm sorry, south facing slope. If we go to an area where we have good alluvial cover in 54, a couple hundred feet away in the channel, we see that the wetting front goes to two meters and stops because there's a tremendous storage capacity. But, if we go to a north facing slope where we have lots of fractured rock, fractured fill material, we see the wetting front moving down to about 12 meters in this case. A tremendous amount of water flow through this system through the fractures--

DR. DOMENICO: These are your pulses?

DR. FLINT: --through the Tiva Canyon. It's a very important mechanism. But, that's up in this location. When it hits this non-welded material which has very few fractures--in some of the bedded units we can find hardly any; they're the Ash Fall units that Ed was talking about--we're going to transition. From the fracture flow that we see over here--which this may stop here, in fact, because some work that June Martin has done has good evidence of Chloride-36 down to about 15 to 20 meters and this kind of information supports her information that it does move down, but it may stop. Once it gets here, transitions into matrix flow, then to start back into fracture flow, we don't have any rainfall down here, it has to reach high saturations to get out of the matrix--again, according to theory, to get out of the matrix and start fracture flow occurring again.

DR. DOMENICO: So, that .1 flux through the matrix into --it takes place through the matrix into Topopah?

DR. FLINT: Because this hole goes through the matrix, right.

DR. DOMENICO: Into Topopah and there is a .1 flux getting into the Calico through the matrix?

DR. FLINT: There's .1 millimeter flux in this whole system to keep these water contents at this particular--

DR. DOMENICO: And, we have a flux across the bottom of sorts?

DR. FLINT: And, there's a flux through here at 1/10 of a millimeter. So, anywhere you look in here, the flux is at 1/10 of a millimeter. If you went back through and looked at the saturation, looked at the water potential, looked at the unsaturated conductivity, with all those combinations using Darcy's Law, you would calculate .1 millimeter flux anywhere in the system.

DR. DOMENICO: My last question is how do you get a flux from a high suction Topopah to a low suction Calico Hills?

DR. FLINT: What you have to do to go from the high saturation here, you have to get the relative permeability and the potential gradient across here about the same. So, you have to reduce the saturation to get it in equilibrium. So, a high saturation has about the same permeability with that unit gradient as this does. They're about the same.

DR. DOMENICO: I thought we had low saturation at the base in Topopah which is high suction.

DR. FLINT: We have low saturation. The fully saturated Topopah has about the same permeability as the unsaturated Calico Hills. The permeabilities are very, very similar. Do you have the next slide?

DR. DOMENICO: Well, I didn't want to take all the time, but--

DR. NORTH: I hate to--

DR. DOMENICO: I'm satisfied if you are.

DR. FLINT: These are what we're looking at, some of the permeabilities. You have to take the gradients across them, but--

DR. NORTH: I think my job here with my chairman to my left reminding me is with this particular spotlight on the vaudeville performer, I'm supposed to have the hook, and I'm going to ask that the song and dance be kept to the assigned time.

DR. FLINT: Thank you.

DR. NORTH: Bob Williams, I think, had some--

MR. WILLIAMS: Well, one more clarifying question. Would we be happier or less happier if the Calico Hills were fractured?

DR. FLINT: I think we should be very happy that it's the way it is.

MR. WILLIAMS: The Calico Hills is acting as a big filter, isn't it?

DR. FLINT: Right. The fact that we have matrix flow is, I think, a very, very good thing. I just wanted to--just so you could see what we've seen, so far, from UZ-16, this is the Calico Hills unit. This is from 1500 feet to about 1400. So, we're looking at the bottom 100 foot of it. It is nearly-saturated. And, we have a very low saturation zone in the Prow Pass and Joe alluded to some of this data where we have what looks like less than saturated conditions even though the water level is high. But, we have this information now that shows in this case almost 40% saturation. So, we have to be able to explain this. We need more properties on it, but the Calico is very saturated, but oddly enough, it's saturated above a less saturated. But, we need water potentials to know why and what's going on.

DR. CANTLON: Well, on behalf of the Board, let me bring this to closure. This exchange here looking at data sets is precisely the reason that in asking for the layout of this particular two day session, we asked that the data be kept minimal and that we look at the process because with a group of interested scientists, it's almost beyond human capacity to withstand the temptation to get into a good scientific argument.

Let me close by thanking this group. I think it's one of the most fruitful of the sessions that the Board has had with DOE. I would just make one very brief comment in terms of Dennis Price's comment about the overall system. The Board, of course, almost from its inception has been desirous of getting a total system look put together, but this is not entirely a criticism of DOE. DOE works in a changing political and a changing financial world in which the total system is still politically at risk, let's say, politically in flux. And so, as one looks at the total U.S. program, we have to recognize that DOE is an agency trying to put together a system in which the system has not yet been agreed to at the level of the funding and at the level of the general layout. So, it's very important to make the distinction about it.

So, on behalf of the Board then, I would like to thank and to commend the individual speakers who presented for us a very lucid examination of the way DOE is using the R&D that's going on out there, prioritizing it, putting it together into decision framework and I would again reinforce Bob Williams' comment that looking at the progress that we see here, it is very gratifying to see this coming together.

So, thank you very much and we look forward to another session on a different topic and maybe we'll let the data hounds get at it. Thank you.

(Whereupon, at 3:45 p.m., the meeting was adjourned.)

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