UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

PROBABILISTIC SEISMIC AND VOLCANIC HAZARD ESTIMATION

March 9, 1994
San Francisco, California

BOARD MEMBERS PRESENT

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Dr. Keiiti Aki, Consultant, University of Southern California
Dr. Robert Budnitz, Consultant, Future Resources Associates
Dr. C. Allin Cornell, Consultant, Stanford University
Dr. Michael Sheridan, Consultant, State University of New YorkDr. William Melson, Consultant, Smithsonian Institute

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P R O C E E D I N G S

8:30 a.m.

DR. ALLEN: May we get under way, please?

This morning we are going to turn to the volcanic, Probabilistic Volcanic Hazard Assessment. Let me just remind you once again, one of the critical questions that the Board has asked is with respect to Probabilistic Volcanic Hazard Assessment, how valid is the conclusion that estimates of volcanic hazard at Yucca Mountain won't change much in the future? What kinds of discoveries would or could cause them to change? What is the likelihood of these discoveries, and the ability of site characterization to reveal them, and what are the criteria for determining when enough is enough?

With that, let me turn to the first speaker this morning, which is Bruce Crowe of Los Alamos, who has been the leading DOE figure in the volcanic analyses, and as part of his one-hour presentation, at the end, he will turn over the microphone for a few minutes to Kevin Coppersmith to explain his role in this developing program.

So, Bruce, you're on.

DR. CROWE: Thank you.

Let me just start by saying I enjoyed the presentations yesterday, and completely agree with the speakers that made the point that there is considerable overlap and similarity in the approaches of seismic hazard assessment and volcanic hazard assessment, but let me point out that there are two fundamental differences that I think are important.

One of those is, is that we do not have the experience to draw on of siting nuclear facilities and established hazard work that I think is important in establishing precedence of probabilistic hazard assessment; and the second is that volcanism is not an issue that can be mitigated through design. Dave Dobson and I used to joke that perhaps we should draw on submarine technology, and design magma-proof doors on the repository, but, basically, volcanism is a go/no-go issue and, in fact, it's been an important part of our studies from site suitability, as well as looking at the acceptability or non-acceptability of the total system.

Now, what I'm going to talk about is basically the strategy that we're using to assess volcanism, and starting in the early parts of our program, we committed to doing a probabilistic study, and in our study plan, we describe the strategy for that study. We've also pointed out that we plan to do--in fact, this is what I'll be talking about mostly today--simulation modeling to look at the sensitivity of the probabilistic approach, and try to define the uncertainty, and then we also made a commitment early in this program to proceeding into expert judgment. In fact, that's what Kevin

Coppersmith will be talking about. So, we feel like we're committed to many of the things we've been talking about for the last day or so.

Let me start with this view graph, because this is where Frank Perry left off, and let me just remind you that this is a part of the distribution of Pliocene and Quaternary basaltic centers in the Yucca Mountain region, and when I describe the Yucca Mountain region, I'm talking about the area that encompasses all of the distribution area of these particular events, and I think Frank gave you a nice background and update on the status of the studies, and I'll proceed from that foundation to move on into probabilistic risk assessment.

And let me point out that I thought that Chris Fridrich gave a very important talk that brought together a lot of information from a variety of fields, talking about this northwest trending zone and how volcanism appears to be structurally-associated with this half graben system that maybe strikes up bounded in Crater Flat.

Okay. The basic model that we've used for volcanism is we've looked at it as a conditional probability, where we're looking at three parameters, what we call E1, E2, and E3. E1 refers to the recurrence rate of volcanic events, and we describe it in a variety of ways. In fact, this is fairly important, and I'll describe this more, but we looked at volcanic centers, clusters, intrusions, polycyclic episodes, and cluster episodes, and it becomes important in how you define E1 to be careful to note what event you're actually defining in the probabilistic assessment.

E2 is that, given an event, what's the probability that it will intersect a specified area, and here, we're looking at the repository, the controlled area, and also, the waste isolation system, which we just call the Yucca Mountain region.

And then, third, E3 is the work that Frank mentioned that Greg Valentine is doing, and it's becoming a focus of our work, particularly moving on into fiscal year 1995, where we're looking at the effects of volcanism, looking at both what we call eruptive releases, or what some people call direct releases, and then coupled releases from the effects of intrusions either through the repository or adjacent to the repository where they disturb the waste isolation system.

My talk is going to focus entirely on E1 and E2. I should mention that I put in some extra material in your handouts, so you're going to have to kind of scurry to follow me in some places. I figured that would be a good way to keep your attention.

What I want to go through first is the logic of how we're doing this work, because I think it's important to understanding how we're applying this probabilistic hazard assessment.

The first question we have--we asked a series of questions. The first one is: Is there a risk of future volcanism? And I think there is probably one of the few areas that we agree on, that we're here because there is a risk for the Yucca Mountain site, and we've established that through the EA, SCP, and ESSE.

So, given that there is the presence of Quaternary igneous activity in the region, we then progressed down through a series of questions. The first question we asked is: What is the range of possible future events? And, as I mentioned, there are really two categories of them.

The one that we are emphasizing, and is what I'm talking about today, is what we call the formation of a new volcanic center, and the reason that's important is there is spatial uncertainty in the location of where that event might be. We can only approximately bound where it might occur. We cannot predict where it would occur, and that leaves a finite possibility that the event could occur through the repository, and potentially disrupt the repository.

The other events that we'll be doing in future work is looking at polycyclic events, both in terms of an event at an existing center, and in a cluster expanding off of an existing polycyclic center.

Okay, so, given that we've defined the types of events, the next question we asked is: What's the nature of future volcanic activity? And I'm not going to go into a lot of detail here, other than to point out that there are four basic types of eruptions that we see, and perhaps this last box captures what's important. We see mixed dikes. There is a bit of a time trend that the oldest eruptions, the Pliocene eruptions tend to be predominantly Hawaiian, despite Strombolian, and we've seen a bit more of a Strombolian, slightly more gas charge eruptions in the younger sequence of events, the late Quaternary, and that hydrovolcanic activity has occurred, but I want to point out that, first, we think there's a fairly low, percentage-wise, probability of a pure hydrovolcanic event because of the deep groundwater table over 2,000 feet at the Yucca Mountain site.

Now, associated with any one of those events, they have to be accompanied by an intrusive event, and this is an area where we've had some longstanding, what I call communication issues with the NRC. And, basically, what we recognize is that, given any volcanic event, it has to be fed by an intrusive system, the dike feeder systems, and we break out three scenarios for that.

One is that you can have just a simple eruption, accompanied by just linear dikes, and that seems to be the predominant case, we think, when we look at the geologic record.

A second is that, associated with eruption, you can develop some more complex intrusion forms; sills, primarily, and we've pointed out in our papers some examples, like in the Payute Ridge area, where this sort of thing has occurred. It becomes important because it changes the amount of potential interaction with a repository.

And the third one that we've had, where we have some disagreements is, is it possible to have an intrusion, which would be an intrusion without eruptions? And we look at both models here, and what I want to carefully point out is what we've been saying is not that intrusions won't occur, but for every case where we see a subsurface intrusion, we also see evidence that it erupted at the surface, and so that really directly deals with this issue of undetectable intrusions. As long as you have a surface eruption, we think that these are pretty easy to recognize in the volcanic record. We have not been able to recognize any site yet where there was an intrusion without an accompanying eruption, but we caveat that with it. We're still conducting site characterization studies.

Now, given those two questions, how do we set up our probabilities? And, again, I've already described this a little bit. I just want to point out that in the status report that's coming out, we hope, in the next month or so, we focused entirely on these first three probabilities where lambda is describing the different types of events, and kind of the preliminary conclusions we have here, that fall from simple logic, is that the probability of an intrusion has to be greater than or equal to a volcanic event, since for every volcanic event, there has to be an intrusion; and then, second, the probability of a cluster is less than the probability of events, since there are more volcanic centers than there are clusters.

One of our preliminary conclusions that we've presented is that the probability of intrusion appears to be about the same as the probability of a volcanic center forming, and I just mentioned why we established that. We also have argued that we use the 10^{-8} number, recognizing that that number may not hold up with the reexamination of the EPA criteria, but we use it as a basic logic step in our work, where we test whether or not the events are greater or less than 10^{-8} is a way of kind of checking where we are.

And what we argue is, all these recurrence rates are greater than 10^{-8} events per year, and here, we've had complete agreement on this with all workers.

Okay. Now, given that an event occurs, that you have an event, where does it occur? And here is just some simple background information. We describe what we call the Yucca Mountain region, or what Gene Smith has called the Area of Most Recent Volcanism, and we argue that that area is the likely area that a future event's going to occur. Basically, all events that we're looking at have occurred in, by definition, in this area.

And then, second, we recognize a couple of structural zones that are possible here. Chris Fridrich talked about the evidence for the Crater Flat Volcanic Zone, and when we sum the 20 events, basically, greater than 90 per cent of them have occurred within this zone. Only one has occurred out of them, and within this zone, what you see is that there's a tendency for most of the events to occur within alluvial basins, and a lower frequency of occurrence within range fronts and range interiors.

The Northeast Trending Zone, that I'm sure Gene Smith will be talking about some, does account for about 75 per cent of the events, and then for each of these recurrence and spatial models, we've been looking at both homogeneous and non-homogeneous Poissonian models, where we look at these three areas; the Yucca Mountain region, the controlled area, and the repository. So, this fits into our variable definition of E2.

Okay. And then, finally, as I mentioned, we use the criteria of 10^{-8}, and so we set up a conditional probability that, given--E2, given E1, for the specified area, we then test whether or not it's greater or equal to

10^{-8}. If it's significantly less than 10^{-8}, and what's significantly less is something that's not clarified, we would cease our studies and pass on the occurrence probability information to performance assessment. If it's less than this, we initiate detailed studies of effects, and our current assessments right now for the Yucca Mountain region, the controlled area, since they're larger areas, the probability is greater than 10^{-8}. The repository sits kind of--straddles the 10^{-8} boundary, and so, we say, probably no, it's a possible maybe, but at this point we're saying we're going to have to carry through those studies to effects as well.

Okay. What I want to say right up front, that we recognized in the various early applications for probabilistic assessment is we have a data paradox here; that is, we don't have a lot of events, and, because of that, we make a fundamental assumption that the volcanic record is too limited to do any kind of statistically robust calculations.

You can't do tests for statistical significance--or, you can, but it's hard to argue that there is any significance to them, and you can't do goodness of fit modeling to try to look at the record. And so, we just fundamentally assume that you have to look at it through application of risk assessment or probabilistic hazard assessment, where we use the volcanic record to do four projections of probability estimates, and use that record to try to establish the mid-point estimates of our probability distributions.

We then use a variety of assumptions to put bounds on both the upper and lower. The upper bounds we gather from the regulatory guidance, and the--I'm sorry, the lower bounds. The upper bounds are used from looking at how volcanic, what's kind of the maximum rates of volcanic activity you get in large fields that have quite a bit more events than the Yucca Mountain region.

And then, the essence of what we're trying to do here is use multiple alternative models in a probabilistic sense to look at how it affects the probability distributions, or the CDFs, and the argument that we use is that because we have such a limited amount of data, there's going to be a spectrum of models that can be proposed, and we'll never have enough data to either prove conclusively, or disprove that any model is correct, and so, instead of assessing one model versus another and trying to evolve one model, what we argue is, the important thing is not which model is correct, but what is the effect of the different models in the probability distribution, and that's what I'll be showing you for the latter part of my talk.

Then, as I mentioned, the one thing that's really new that I haven't described to the Board before is we've done some work now on risk simulation, where we basically use a Monte Carlo-type simulation to try to generate the probability distributions, and then look at the effects of different assumptions on those probability distributions, and, again, what we do is we look at all alternative models, focusing on the occurrence probability.

We plan, when Greg Valentine's work gets more evolved, to add in effects to look at risk and what we try to do is make sure we set up our models so that we don't underestimate risk, but, within that trying not to underestimate, we also ask that the alternative models must be plausible, physically; that is, if you look at the tectonic record and the physical processes of how volcanism operates, you shouldn't use a model that just doesn't make sense.

And this is where we bring in judgment, that is one of the reasons why I think expert knowledge is so important to bring into this. There's going to be a spectrum of judgments on what is physically plausible or non-plausible.

Okay. We're bringing a slightly new perspective into this volcanism status report that you should have gotten a copy of Chapter 7 on, and the difference is that what we did in all of our previous studies--and I think what most other workers have done--is tried to identify the bounds on volcanism, particularly the maximum bounds to see whether or not it was a disqualifying issue, and we feel we've gone enough beyond that now that what we're trying to do instead is not emphasize the worst cases, but try to take a scientific perspective, and somehow block out all of the value judgments about what it means, and just try to define our distributions as rigorously as possible, concentrating on the mid-point estimates and the maximum mean.

So, in other words, we're trying to do, from our perspective as scientists, do as unbiased--if there's such a thing as an unbiased probability distribution, but present those so that the NRC can then look at--not the NRC, the DOE will then make decisions on how to apply those distributions to the assessments.

And the reason I want to emphasize this is, what I have seen that's happened, both in my work, and I think in some of the other people's work that we can argue about, is that if you don't try to do unbiased distributions, you have tendency to take conservative assumptions as you go through each step of your models, and you fold in a non-systematic bias that's very difficult to deal with; and, in fact, when you look at published data and do simple statistics, you find that the data are strongly skewed toward maximum values, and I think it's because of this bias.

So, what we've tried to do is emphasize the central tendencies of datas, and then draw the distribution about that, and it's really different than anything we've done before.

Now, what I want to proceed through fairly quickly is just show you some examples of how we're doing this, and I don't want to belabor the points on all this, but for E1, the recurrence rate, what we're trying to do is look at a multiple range of defining E1 and bounding it, so the first thing we start with is try to do some simple time-series analysis, which has been applied to a lot of historic eruptions, and the big thing we run into is, we just don't have enough events to do anything statistically significant.

But what I've just shown here is a simple plot of, for clusters, where I've combined the clusters, showing the ages and the error bars, or recurrent assignments of the ages, of what you see, just summing the events versus time, and what you can--the slope of each line segment is roughly the event rate per year.

And, what you see is, there is a tendency toward a slightly steeper slope in the Pliocene, and a somewhat shallower slope in the Quaternary. If we add in clustered events, I think it really doesn't give you much information, but it emphasizes that, in a way, we're somewhat analogous to clustered seismicity, that when we get bursts of volcanic activity, we see multiple events. And, from all of our studies, we have not been able to discriminate, except for the Lathrop Wells, the youngest end, any differences in age by using our method. So, within the uncertainty of our dating methods, these events seem to be synchronous.

We think, based on Frank's chemical data, that there may be subtle differences, but we're probably going to never be able to, except for the very youngest end, ever see those differences.

The only thing I've been able to pull out of a time-series analysis is just to look at repose rates at repose intervals, and what I've plotted is age versus intervals, and you see, you get pretty disbursed data, and all I've done is two fits, just to show you that there's an infinite range of fits that can be done.

The first is a simple linear regression, which has a negative--has a spoke to it, which you would argue could suggest there's been a slight decrease in the repose period, and we think that that may be real. I also fitted it with just a distance weight of these squares, which flexes for each data points, and you end with intersecting the Y axis at almost zero, which suggests that, today, we have a zero repose interval, which is probably pretty difficult.

The only thing we're deriving out of this is you take the mean repose period, it's about a million years, plus or minus 600,000 years. It doesn't tell you much. What we have done is only looked at the shortest interval, so in the last 4.8 million years, there's never been a repose period shorter than 200,000 years, so we just take that as one end point.

But, let me also point out, within the limitations of our data, that shortest repose period also happens in the repose period between the two youngest events, and so, again, we kind of fall up against these difficulties with the completeness of the data.

What I've also done--and I don't want to go through all the details here and bore you with the tables, but they're in your handouts--is we've looked at homogeneous, basically, event counts, and non-homogeneous event counts, and what I mostly just want to show you is we've put together a matrix of most likely, maximum, minimum events, and probably the most important thing to emphasize out of here is we used four different models for doing these calculations.

Two really come out of the regulations, where the NRC specifies looking at the Quaternary events, which they define as two million years. We also used 1.6 million years, and then we do two other models, which I call our preferred models, because we tie these to the geologic record, where we recognize the cycle in volcanic activity, and then do our calculations for those, and this basically represents the last 4.8 million years, and then this is the last million years, where we recognize a period where there might be a slight increase in rates, or the frequency of events goes up slightly while the volume is declining, and we do these calculations.

Now, an important thing in the simple homogeneous models, what we see is, when we use all the four models, we tend to get slightly lower values, and the reason is that somewhere you cross intervals of time where there were no volcanic events. If we just used the Quaternary, from two million to one million, there were no events, and then all the events are in the last million. So, we think it underestimates, a bit, the probability.

So, what I do is also show the statistics for the preferred models, which are just these two models here, and your numbers range from about 2.3 to 5, or the worst case, actually, is around somewhere--if I can find it around here--somewhere around 8, here it is, and then the low is 8 x 10^{-6} versus 1.5 x 10^{-6}, again, trying to emphasize the ranges.

We also did non-homogeneous models, concentrating on the work that Dr. Ho has done for the State of Nevada, where he's proposed a Weibull model, and one of the key issues with the Weibull model is you have two fitting parameters. One of them is the beta factor, and what becomes important is to test whether or not your beta factor is greater than or equal to one. If it's equal to one, you're basically equivalent to an exponential or a Poissonian assumption. If it's greater than one, you can argue that you have waxing volcanism.

And here, a second point emerges that's really important. If you do the Weibull calculations for specified intervals, where it sums across periods of no activities, you get beta factors consistently greater than one. If you do it for where you tie the interval of observation to the geologic record, the preferred models, all of our beta factors are less than or equal to one, and so, I would argue that preferred models are consistent with a waning system, and are probably more appropriate.

And what you see is, when you sum the statistics of them, you see that same--the opposite reversal for the non-homogeneous, or for the homogeneous, that, basically, all models give you slightly higher values, and then because the beta factors for the preferred models are less than one, you get lower values. And, again, what's important here is just the ranges of the calculations.

And then, finally, we've also play a bit with using simple regression models, treating volume as the dependent variable, and age as the independent variable, and we established in some earlier calculations that we had a linear fit between the models, but what's been new is we've added some new points along here, and so, we did some new calculations to kind of look at what's the effect of those, and it basically kind of blows up your regression models to where they don't give you very satisfactory fits. In fact, the residuals have a lot of structure to them, which suggests that your regression assumptions are not valid.

What I've shown here is just a simple regression fit, and then I did a distance weight of these squares, which suggests that you might have a curval linear relationship, and to test that--

DR. REITER: Bruce, could you just explain what that plot shows?

DR. CROWE: Oh, okay; sorry.

What I'm plotting on here is the volume of events, so each point is a volcanic event and its associated volume, and what you see is, there's a dramatic decline in volume through time, versus the age of the events, and then fitting it with different fitting models.

I also took that same data and log transformed it to see whether or not we could use a model of intrinsic linearity, and, actually, it does really nicely until we come down to this data point right here, and what I ended up in some of the status report is, I came up with seven regression models, and only two of them gave you residuals that would be generally accepted, and I used those two to try to generate the slope, which is the magma output rate per year.

And using that, I then try to do some calculations, and I don't want to go through all this table. It's in your handouts, but what I just looked at is, how can you then use that output rate, assuming that there's a representative volume of an event, to constrain the recurrence rate, and because there's been this 30-fold decrease in the volume, if you sum these through any kind of averaging, you end up with very, very long recurrence times, in excess of a million years for events extending for both the two preferred models.

And so, what I've done for the only calculations, is assumed a median value for the smallest events that we recognize, and here we get rates that are not too different, actually, from the homogeneous and non-homogeneous. You tend to get values in the 3 to 5 x 10^{-6} rate.

Well, what does all this mean? And this is what I've been working on probably the most in the last year, is, again, we are not really concerned with any one value, but what all of this suite of values tell us, and so what I plot here is basically what I call the distribution of E1 and probability space, and what I've summed on here is all of the different calculations that I just got done showing you, as well as including work by the State of Nevada, and work that Connor & Hill published recently, that shows you the distribution.

And, actually, what I'm impressed by is our distributions are not that dissimilar, and I'll show you in a second how I used simulation modeling to show you them, and then we explore the boundaries of the distribution to data. On the one end, we have the regulatory perspective that basically says that volcanism would not be an issue if there are no events in the Quaternary, so I turn that into a probability of 5×10^{-7} for this upper bound, and at the lower bound--I'm sorry. That would be the lower bound, and then the upper bound of rates, I turn to volcanic fields, and calculate using simple homogeneous models, what those events would be, and then argue that somewhere in this distribution space, between here and here, our values have to apply.

And just looking at this data, I think it kind of cries out for a simple triangular-type fit that kind of tells you about the data. You know roughly where your bounds are and you have some mid-point estimates, and I use a special type of triangular model that's called a Trigen model, where with most triangular models you have to use zero points for your boundaries, and the Trigen allows you to assign different percentiles to your upper and lower bounds, and then what I did to explore the sensitivity of this is I just set up a simulation matrix, and, again, I hate to present complicated tables like this, but let me just point out a couple of things here.

This is the matrix that I put together for the simulations, and the across the row variation represents different assumptions of models, both a normal model, where I just used one sigma, just to look at one sigma, and then these are the variations of the triangular, the Trigen model that I used. So, the variations that you see across here represent simulation assumptions, and then each vertical column represents differences in the types of models that you can use to do the calculations.

If you then run simulations to generate your distribution curves, here's what they end up looking like, and I think one of the things I've been impressed from, particularly talking to Kevin Coppersmith and looking at some of the seismic issues is, for E1, we end up with a real tight clustering of events. I mean, I've always been impressed when I show seismologists, because we've been arguing about the significance of these for about the last five years, and yet, seismologists go, "Wow, that is tight data."

And, what I've shown on here is the min and maximum, and then I've also showed Dr. Ho's calculation. We have a little bit of disagreement with Dr. Ho, but primarily because the shape of his curve is very similar, but I feel that what he did is applied his calculations to a worst case, and then set up confidence intervals about that worst case, but our approaches are very similar, and I don't want to say that we're different, we're just different in where you would center your distribution.

I feel that his tends to be a little bit more skewed than we would argue, but I also would point out that we're not going to get that upset. If you look at the range of expected values in here, we think that we could just look at this whole range without much sensitivity.

And in order to look at this, what I've actually done is used a method of looking at uncertainty bands about your distribution, so on the Y axis here is the E1 values. The blue line represents the median of values, the 50 percentile, the upper red is the 90 percentile, and the lower red is the 10 percentile, and what I was looking at in this particular calculation is just if the variability of uncertainty depended on the different distribution models that I used.

And what you see is, depending on how tight or broad I make those distributions, the uncertainty expands, and this last one over here is, because I only used one sigma, you have a smaller uncertainty about the normal distribution model.

And then, this is for the homogeneous. I'll just quickly show you, I also did it for the non-homogeneous, and we end up with very, very similar results. The only difference is, because the beta factors for the preferred models are less than one, you tighten up your distribution down here on the normal distribution.

I'm kind of scurrying through this. Let me just show this one quickly. How am I doing on time?

DR. ALLEN: You've been talking for a little less than half an hour.

DR. CROWE: Okay, great. That's about right.

What I've done for this one now is, this is showing the uncertainty in the vertical column variation, so you're looking at the uncertainty created by differing modeling assumptions, and what I'm impressed by is just the bands are about the same in this modeling, and the only difference we have is this is Dr. Ho's model, which has about the same uncertainty, but it's skewed a little bit toward higher values, so you see a slight change in the median value here. But, basically, the uncertainty is not that different with differing modeling assumptions.

Okay, and then, quickly, I wanted to say a little bit about E2 now. What I've done with E2 is used the same approach; again, not try to emphasize what anyone model is, but what are the ranges of results that you can get from multiple models, and, here again, we have some areas of controversy.

What I've plotted here is just the distribution of volcanic events in Mercador-projected coordinates, and we've had some arguments about whether or not this is a homogeneous or a non-homogeneous distribution. I argue that if you look at this, and visually, in two dimensions, you can see patterns very nicely, I don't think anybody--it doesn't take a statistical wizard to say that this is a non-homogeneous distribution, and we would agree with that.

Now, what we've done in our models, in our spatial and structural models, what we have done is, perhaps incorrectly, we called them random models, but what we did is we used different combinations of the spatial distributions of events, and so, we actually had structural control, we think, in the way we did these distributions, but within the area defined by the distributions, we assumed that events were random enough so that we could do calculations to come up with the results.

So, the only caveat I add is that we never have said that this is a homogeneous distribution, and I would not want to try to defend that, just looking at this distribution. And just to show you, we've used--what we did is, we've looked at all the published, we've looked at spatial models and structurally-controlled models, and I just want to show you one example of how we project these into disruption ratios.

What we do is, here I've just done some visual clustering. Since we only have two dimensions, I didn't bother going to formal multi-varied cluster analysis. I just visually clustered these based on all the geologic information that we had, and I went through iterations of, here's the first simple cluster, the second joins adjacent clusters, and the third joins--it goes to a third iteration. You end up with a Crater Flat Volcanic Zone, the Northeast Trending Structural Zone, and one that nobody's yet named here, and, obviously, it's not related to the repository, which is just an East/West Trending Zone.

And then, what I do with those models is just the same sort of thing that I did with E1. I look at a range of the different calculations in this table, and I try to look at all the variation. Now, what we end up with this data set is it's strongly skewed. You tend to always get values skewed as greater than one, approaching two or three in some cases, and the reason is that there are some models that have high disruption ratios.

And, again, I don't want to make you go through all this stuff, but let me just point out that the model that we've emphasized is what I call the intersection model. For Model 2 and 3, we weighted them for the percentages of occurrence of volcanic events and ranges versus alluvial basins, and whether or not DOE chooses to use that is really their option.

Let me quickly point out that one of the things where I think volcanism differs a little bit from seismicity, is we really have some spatial problems in the predictability, and you have to guard yourself against assuming some predictability that's not in the data set, and I want to show this by just showing you how events have jumped around.

Again, this is a lat-longitude plot of the events, and these bars represent the maximum cluster lengths that we've observed, and I've drawn them parallel to what I think are the controlling vent alignments that we see at each center.

And what you see if you step through time is where there is a pattern of concentration in this northwest trending zone, the position of sequential events jumps quite unpredictably, both in their locations and their jump directions. So, the first event starts up at 4.7 here, the next event jumps way down to here. The next one is in Crater Flat here, next one jumps out to here, then to here, then back out up to Sleeping Butte, and then back down to Lathrop Wells.

And so, one of the cautions that I have used in the models, and I urge other people to use, is that you have to be careful that you don't do some simple distribution models, assuming that there is more predictability in the path distribution than is really represented in the record.

And, I'm just going to quickly show just this matrix. All I've done is taken structural models, and in your handout, I went through a table of 13 structural models that I used to generate these, and I have 17 cases of application of them. I don't have the time to go through all of those, but, again, what I tried to emphasize is structural model approach is to look at different tectonic framework models, and then to look at how that would affect the distribution, and then I expand those models so they don't include intersection, to be consistent with how I did the spatial models.

Now, the only thing I want to point out here is we do have one set of models which are all down at the bottom here, which does include the repository. All the other models, when you just use their structural areas, do not include the repository. You have to expand them, using some assumptions for expansion, in order to get the probabilities, and when you just take the subset of these models--this is what I call the northeast trending models, they tend to give you lower values than the average of all the others, and what I point out is I think that the northeast trending models are really not sensitive to the probability distribution.

Then one last thing that I do is I make analog comparisons to other volcanic fields, and here, what I'm looking at is kind of testing the model of we only have--this is the Lunar Crater field, with each little star being an event that we noted for Quaternary--these are all the

Quaternary events in the Lunar Crater volcanic field. There's roughly 82 events, and, actually, the details of event distribution isn't important, but what I did is some simple spatial fitting to look at the dimensions.

And the analog here is that, say we had a busy volcanic field with a lot of events, what kind of dispersion, what are its length/width ratios, and how would that compare if we superimpose that on Yucca Mountain? So what I did is calculated the centroid of the distribution, and then I calculated using a bivaried Gaussian ellipsoid, the 90 percentile and the 95 percentile, and I just used these to measure the half-lengths across the field, and the length dimensions, and then I've done this for a variety of fields, and then superimposed it on the Yucca Mountain region, and, again, this will be controversial because I assumed that the northwest trending model was the most proper way to put this, and we can have lots of discussion about that if we'd like.

But, the point is, that if you put this--I located it in the center of the centroid of the distribution of Pliocene events, dropping out Buckboard Mesa, which I don't think belongs in here, and what you see is that the dispersion in Lunar Crater is not that dissimilar from the dispersion we see in the Yucca Mountain region. The half-width is only slightly longer than the maximum cluster length here, and if you put--if you plot Lunar Crater down into

Yucca Mountain, you would not, naturally, intersect the repository.

Now, that doesn't mean that all volcanic centers are that way. What I've used is the two most--closest analogs to Yucca Mountain. If you go to like the San Francisco Peaks field or some of the platform-type fields, you get a lot more dispersion, and I've superimposed those, and those would represent intersection. And so, again, there's judgment in deciding what analogs are appropriate.

Okay. What I end up here is the same thing that I did with E1, where I just look in probability space at the distribution of events, and it's a very busy diagram. I don't want to belabor you with all the points, but most of the values do tend to cluster in the 3 to 5 x 10^{-3} range, but you do get some numbers that sit way out here, and what I've shown is, this would be the ratio if you just randomly located a repository within Lunar or Cima Volcanic Field. This would be the ratio you'd get.

I just did statistical analysis using exploratory data techniques to look at what would be identified as outliers, and this vertical green line identifies models that--this would be a standard outlier division, so anything across this way. I use this primarily because it gives you kind of a non-biased way to look at these models.

What I would argue is, anything, certainly, exceeding the Lunar and Cima, or approaching these, are probably skewed models that may not be physically plausible. Again, and then what I used was simulation modeling of E2 to look at the sensitivity of these, and what we get is a bit more disbursed curves with E2.

This represents the sum of all the spatial models. The red line is all the published calculations. The blue line is the summation of all the structural models. This is the northeast trending models that I pointed out, and then if you edit outliers out of the spatial and the published, they plot within this more vertical; again, much less variance by taking out outliers. The issue is whether or not that's a valid way to look at the data.

And then, again, I also look at that in terms of the uncertainty variation about E2, and what you see is if you just take the raw data sets, you have pretty large uncertainty bands. If you edit them or make judgments about the suitability, you narrow those bands, but, again, this requires judgment. It's one of the reasons why I think we're going to have to go to expert opinion to try to get as unbiased view of that as possible.

Okay. Now, quickly, what does all this mean? What I then next did is, using the probability model, looked at the conditional probability of both E1 and E2 occurring, and ran simulation modeling, and what I did is ran three sets.

What I first did is set up a matrix of E1 values, and then I fixed E2, and then ran simulations to look at the curves, and I was actually a little bit surprised by this. The median values, the 50 percentile, come out pretty tightly around 2 to about 3 x 10^{-8}, and there just is not much variation. I plotted the min-max. All the rest of those curves would sit right in the middle here, and so what I would argue is there just is not a lot of sensitivity of the recurrence rate models for our calculations.

I repeated the same process, but, instead, fixed E1 at a median value of about 5 x 10^{-6}, and then ran results for variable E2s, and what you see is for a lot of the models, particularly if I edit the outliers out, they sit in about the same position. Again, the values are not that different. They're about 2 to 3 x 10^{-8}, but you do get some high values with an expected value as high as 7.3 x 10^{-8} when you look at all the potential cluster models.

So, what I then did is used this distribution, began looking at what combinations generated these high values, and when I did that, what I found was two things: One is that a number of these models may not be structurally correct. I mean, you're really pushing them to get intersection; and second is that it made me recognize that there is a bit of interdependency of E1 and E2 that you have to look at, and developed one last matrix.

And what I've done here is, if you take different assumptions about a spatial model, they may or may not include all the volcanic events, and so, if you just look at E1 and E2 independently, you can come up with combinations that are not physically real, and so, what I did is, I went back to my matrices and I said, "Okay, for each model of E1 that we propose, let's take a look at what its effects--", I'm sorry, "--each model of E2, the distribution models, what are the effects on E1?"

And, you end up dropping out a number of models that, because they exclude a lot of events, have a low E1, and I was able--and I ended up only being able to identify two models, which are variations of the spatial cluster models, that give you values that are greater than the typical values of the kind of the median points, and you get numbers of 7.5 and 4 x 10^{-8} for those.

And, what I've done is just flagged these as sensitive models that I think the logical thing is, perhaps, to focus our site characterization studies to look at the feasibility, and begin to test those models as being the most sensitive parts of your distribution.

Okay. I have two last view graphs to kind of sum up what we've done. I think what we've learned from the probability estimates, and particularly, the simulation modeling, is that we have surprisingly well-constrained recurrence models, and they just are not that sensitive, particularly to variations in your mid-point estimates, and what's more important is the shape of your boundaries really affect how it pulls that distribution in the simulation modeling much more than the mid-point.

And then, second, I'd just like to make a point here that Jeanne Nesbit will expand upon, that we have been looking at this 10^{-8} criteria for kind of sensitivity, and the 10^{-8} criteria was originally written, was that if you can show that the occurrence probability is less than 10^{-8}, you may not have to proceed to PA, or performance assessment, to investigate this.

What we've never looked at is what is our upward sensitivity? And so, what I just showed here is some of the things that could change would be, let's say we have undetected intrusions or centers. If we just say, how much would it take to move from our median values to, say, 10^{-7}, and we don't even know if 10^{-7} is a sensitive area, you'd need like a factor of two or three, and that would have to mean that, for the record, that we have as many as 14 to 21 undetected intrusions and, you know, I'm kind of anxious to see what the NRC would say to that.

My argument would be that that would be very unlikely, but we may debate that for a long time, and that's another reason why I want to go to expert opinion.

And then, additionally, on the structural models, we were able to identify a couple of small number of structural models that are sensitive to the distributions, and what we've planned to do now is particularly use geophysics and field studies to focus on those models, just to test how important they are, how real they are, et cetera.

And I do want to point out that, like in the dike propagation models that we use, if some of the models that have been used with very long dike lengths are true, we probably should be having dikes already in the exploration block, and it's going to be interesting to see, with the tunnel-boring system going, whether we'll actually see any of those. It'll be, actually, one test of kind of the extreme end of the models.

And then, finally, the northeast trending models are not sensitive. They just do not drive your distributions in either direction. They tend to be centered right around the median values.

What we do need is that we're going to have to use judgment in assessing these high probability disruption ratios, these disruption ratios that give you the higher probabilities, and then, the next step is that, clearly, from where we've gone, the effects become very important, and this is going to be the emphasis of our future program.

The fourth, or the five things, actually, again, that I've kind of touched on through my whole presentation is, as Frank Perry pointed out yesterday, our next step is to begin exploring the sensitivity of probability calculations for the polycyclic model, and here, the polycyclic model uses a spatial predictability that we didn't think we had. What it says is that the most likely event is likely to be another event at the existing volcanos, which are 20 and 47 kilometers away, and we would argue that these may have fairly high E1s, but they're going to have very low E2s, and probably extremely low E3s, and this is something that Greg Valentine is looking at right now.

He's trying to define what we call a standoff distance, how far away do we have to be from the repository before the effects virtually go to zero. And then, additionally, we're going to be focusing on geophysics studies. George Thompson from Stanford University is helping us kind of do an overview of the presence of magma bodies, undetected features, and this could be the one area that we might see some variations in our calculations.

And then, third, an important thing is that from Frank's geochemistry perspective, we want to look at different models of how these volcanic fields evolve, and, again, look at a spectrum of models to see how they would affect your probability estimates.

And then, finally, what we plan to do from here is we've established a probability framework through simulation, and what I think we can do nicely now is begin to get new data and new different models. We can just begin to plug those into these matrices, and look at our sensitivity, where we define the sensitivity as the effect on the distributions.

And the last one underscored is kind of a lead-in for Kevin. Where are you, Kevin? Oh, out there. We basically feel that this is an important area to go into and, in fact, I feel so importantly that I'm giving up part of my presentation time for Kevin, and I also gave part of my volcanism budget this year to PA, so that they could give Kevin some seed money, so that's the depth of my commitment.

DR. ALLEN: I think we'll go on with Kevin before we take questions on this, since it's all part of the same presentation.

DR. COPPERSMITH: That's pretty impressive. You wore out an Energizer.

(Laughter.)

DR. COPPERSMITH: I'm out of breath, and I haven't started yet.

What I'm going to talk about, very briefly, is a project that is just beginning to look at the Probabilistic Volcanic Hazard Assessment. Obviously, Bruce and his colleagues have made a lot of progress after the last several years in field data collection, the quantification of their information, their dates, their spatial distributions, basically, where the different units are, and now they're getting into the types of work that Bruce has been showing, and have been, actually, for several years, looking at many of the elements of probabilistic modeling of volcanic hazard assessment.

In that process, they've identified particular areas where there is a diversity of judgment about particular interpretations that could be made, whether or not it's the structural models, information on E1 related to rates, and so on, and what we're doing now, as a follow-on to that, is to develop a program to actually formally incorporate that diversity of expert judgment about these particular areas.

I do want to point out that in terms of the sponsorship of the study, a real champion in the system is Jean Younker, who's not here, I don't think, TRW, who is really very much committed to carrying out this type of study, and to the application of similar types of studies in other areas for performance assessment.

In terms of an overview of the project, the status is, it's just beginning. I hear that the contract's in the mail. The first task, obviously, will be the development of a program plan and a peer review plan, which are very important to the project overall, and we talked a little bit yesterday, and there may be some more discussion in the round table today of this concept of the use of peer review, and I think, to me, this is something that's vitally important, is an understanding of how expert judgment can be used in the context of the peer review program and procedures that exist within the program, and I think they can.

The twofold purpose of the study is, obviously, to quantify the probability of occurrence, as well as the disruption probabilities. This is E1 and E2 that Bruce talked about, and to quantify the uncertainties associated with these assessments, including the diversity of interpretations that might exist among multiple experts.

The exact procedure for doing that, particularly Item 2, is to be determined. We're just beginning. I think that the role of a technical facilitator or integrator will be defined. This is very important in terms of how assessments are actually done. We'll go through a process that does involve selection of experts, the elicitation and aggregation and documentation of procedures as well, and I'll talk a bit more about those in just a second.

But, what do we draw on? Where are the precedents? We talked about some of these studies yesterday, and they're well-known to many of the people in the room. From my standpoint, these types of studies--and there are others, as well--are the sort of experience database that we would begin with to develop an appropriate methodology for this type of assessment, and I want to point out a couple of these; some that I think are particularly pertinent.

One is the top one, is a recent study that was done by EPRI on the earthquakes and tectonics. You'll notice that these, almost in every case, deal primarily with seismic issues. Earthquake hazard assessment has been a ground breaker in many ways in dealing with some of these things, no pun intended. It's non-coseismic displacement.

This particular project was one that was done as a demonstration of ways of quantifying uncertainty about the earthquake hazard, fault displacement hazard at Yucca Mountain. It involved a formal elicitation of seven experts. Workshops were held, and so on; again, a demonstration to show that not only this type of diversity of judgment could be incorporated, but, also, that a wide variety of experts could be brought together and could interact, and judgments could be developed in a very cooperative manner, and it was very successful from that point of view.

Also, the EPRI performance assessment project, which this is actually the master project--this is a sub-component of that--performance assessment methodology was developed, earthquake being one element of that; volcanic hazard, Mike Sheridan was involved in that component as well.

A recent study that has been done by the Center, I think, is very appropriate in terms of the use of expert judgment. I think in some ways is, again, a demonstration project for how expert judgments can be incorporated, in this case, into an assessment of the likelihood of future climate change.

And actually, this project, which has been known now and formerly as the SSHAC Project, S-S-H-A-C, the Senior Seismic Hazard Analysis Committee, which is a jointly-funded program to basically develop a seismic hazard methodology that these particular agencies can feel comfortable with over the next decade or so.

These issues of expert judgment elicitation are being dealt with head-on. For example, I just wrote a white paper that the committee will discuss in a couple of weeks about the relative value of the use of expert judgment formal elicitation versus a process of a single team developing a particular assessment that's subject to intensive peer review or participatory peer review. What is the difference in those? When would you use one, and when would you use the other? What's the relative value? So, this project is focused very much on these issues. Bob Budnitz is the chair of that project.

Other studies that we were aware of that have been done in the eastern United States, formal elicitation of experts in a variety of ways, either in writing or through interviews, are all appropriate and pertinent to this particular study; same thing in terms of the EPRI studies. Also, recent studies that are being done for WIPP by Sandia are important experience databases to draw on.

I want to jump up onto a soap box, since Bruce gave me a little bit of time, and talk about--

DR. ALLEN: You've got three minutes left.

DR. COPPERSMITH: Great--about some of the subjects that people ask questions about. These are actually commonly-asked questions about expert judgment, period, but in the context of the Probabilistic Volcanic Hazard Assessment, they are again being asked, and I would offer, for example, this definition of what expert judgment is.

It's the analysis of data by knowledgeable individuals to make interpretations about the future. This means, then, that there's a couple of key components that has to do with the analysis of data. It's not data itself. It also means that knowledgeable individuals do this. That's why they're called experts, and they are used to make these future assessments of what we might expect to happen in the future.

Why do you use expert judgment, then? Primarily, because we do not get a unique--geologic data or earth sciences data by themselves do not provide a unique assessment that is needed for this assessment or prediction about the likelihood of future events. No matter how good our data are, models are needed to put those data together, to analyze them, and to make those future assessments.

So, when do we use expert judgment? Obviously, in a strict sense, it's used everywhere. It's used throughout this process. Even if it's one person doing a hazard analysis, it's used everywhere, but I think from the standpoint of what we're really getting at here is some key issues, like when do we need to use multiple experts? Do we use expert judgment to avoid data collection?

This particular one, I think, is, to me, is always very troubling, that that misconception is made, that experts are being brought in in lieu of a data collection program. You saw Paul Pomeroy's slide yesterday that shows a sliding scale that goes from something that's purely expert judgment, without any data, or not much, over to a case where it's very data-driven, and I would say even in that case--and Paul would probably agree--on a very data-intensive effort, experts are still needed to make an assessment of that.

There is no attempt, throughout the volcanic hazard assessment, certainly, and seismic as well, to use expert judgment as a way of getting around data collection, but from the standpoint of even a very data-intensive program like Yucca Mountain, experts are still needed to make the analysis.

Why do you use multiple experts? Basically, I think it's the fact that you can have different interpretations of the same sets of data, different models, different conclusions.

DR. ALLEN: Could you draw it to a close, Kevin?

DR. COPPERSMITH: Yes. Let me just finish here a couple of other key areas. I think they're important to this.

The issue of the use of other experts from outside of the project, I think it is likely that other interpretations that we know right now those interpretations exist. They exist within the peer reviewed literature. It's important that those other judgments be used and looked at, and actually incorporated into the analysis.

Precedents for this type of approach come largely from earth sciences, from the seismic hazard field, and we can draw on those, and in terms of what's the best way, I think that is still being decided. There are a number of alternative mechanisms.

And, finally, on the last view graph, the procedures that we'll follow are the standard procedures for developing any formal expert judgment elicitation, and step through the selection of the experts on through the different components and elicitations and feedbacks.

Okay, thanks.

DR. ALLEN: Okay. Thank you, Kevin; thank you, Bruce.

We have time now for questions from the Board or the consultants to either or both of these speakers.

Yeah, Mike Sheridan.

DR. SHERIDAN: I'd just like to make a comment that I've been attending these sorts of hearings for some period of time now, four or five years, and that it appears to me that this is a major step forward that we've just seen presented that will be very helpful in the resolution of this problem, and I hope that there will be a publication coming forth to document this data so that it is open to the general scientific community.

DR. CROWE: I just want to say, I hope so, too.

DR. ALLEN: That's Bruce Crowe speaking.

Warner North.

DR. NORTH: First of all, I'd like to comment that the contrast between these presentations and yesterday's, as far as I'm concerned, is night and day. It does look like a major step forward, and it's very encouraging. You're clearly very well-organized in terms of having put together the data that exists and thinking about how you were going to do the expert elicitation part to further clarify interpretation.

I would note there are other areas outside of earth sciences where this kind of thing either has been done, or is being done at the current time. I would commend to you the efforts of Carnegie-Mellon on global climate change, and the efforts for EPA done out of Argonne Laboratory, in part, on health effects of ambient lead and ambient ozone.

I think one of the messages that came out of those exercises is the importance of having a protocol both for how you are going to do the expert elicitation, and for how you are going to deal with the documentation questions that you develop at the beginning of the process, rather than at the end of the process.

What EPA found useful was to do, essentially, a pilot version, and de-bug the methodology for the expert elicitation on a small group of people before going out to a large community of experts and trying to do it, shall we say, in an operational phase. So, those ideas may have some merit in this situation as well.

DR. ALLEN: Staff?

DR. CORNELL: I just had a comment, and it sort of supports what Warner was saying. I have not seen the volcano work before, and I would also like to point out that it is exactly the kind of thing we were talking about yesterday. It's really a good exercise in working backwards from the probabilistic models as to what the implications are as to the physics and the structural characteristics, what additional studies you could do to perhaps reject hypotheses that are leading to probabilities at one end or the other, to help focus the research. I like it very much. Good job.

DR. REITER: Leon Reiter.

It's a question for Bruce, and I guess for Allin, and that is a question about it seems to me that you're relying upon the mid-point as the most robust indicator of your estimate, and in the past, we've had lots of concerns about mid-point and what kind of mid-point should be used, the median or the mean of distribution, and I wonder if somebody, you or Allin, could address that, or maybe both of you?

DR. CORNELL: Is this the old mean versus median controversy?

DR. REITER: Right, sort of. In other words, I guess I was looking at what Bruce presented, and he was very happy with the robustness of the central estimate, and then at some places, the distribution was more skewed, and I was wondering if that's sort of a problem lying in wait that has to be addressed somehow, and does he have any sort of recommendations or--

DR. CORNELL: Yes. I think in those cases where your distributions are skewed, the mean probability will be different from the median probability, as you've already seen, and will be greater in those cases.

There was a concern that we went through in the seismic area over this. There tended to be a certain population that tended to focus around the applicants that thought the median was a better number, and another end that preferred the mean.

I think, basically, the support today is we're using the mean, and the reasons for doing it are quite numerous, but include, ultimately, in my mind, the fact that if you're going to use probabilities, ultimately, for assessments and prioritization of resources, you want to use mean probabilities in any kind of risk benefit analysis, and that's ultimately a primary use.

The quantitative safety goals of NRC came down on the mean basically because they said, if there is more uncertainty in the probability, the ratio of the mean to median will be higher, and this is one way to sort of reflect the uncertainty. That's kind of ad hoc reasoning, but it comes to the same conclusion.

DR. ALLEN: Bob Budnitz.

DR. BUDNITZ: I know Allin would agree with the comment that if you've got to use one number, okay, but it doesn't make sense to use one number. The fact is that, what I saw from your distributions, Dr. Crowe, represented in the distribution our state of knowledge, and anyone making a decision that wants the details--and you shouldn't make a decision without the details--needs to know the whole state of knowledge, and sometimes the state of knowledge is adequately captured by the mean for the purposes of what you're doing, but often, it isn't.

MR. COPPERSMITH: And, of course, the application here for performance assessment, we'll use the entire distribution.

DR. ALLEN: A final question by Bill Melson.

DR. MELSON: Bruce, you remember your, of course, your earliest work on this project, and it seems to me your probabilities came out around 10^{-8}; is that right? Do you want to comment on, is this--are we seeing a housecleaning and an improving methodology, getting back to what was your original estimate?

DR. CROWE: Yeah, that's an interesting comment. I've talked with DOE managers about how we've spent ten years and I don't know how many million dollars, and we haven't changed any. That, perhaps, is an argument that we may be getting near a consensus on doing these, but, yes, I mean, the distributions are pretty close.

I mean, as I emphasized, what I did earlier was tend to emphasize the bounds, and I really put quite a bit more effort in trying to produce true distributions that you can then extract to respond to the regulatories. So, there's a little bit of difference there, but the numbers are not significantly different.

DR. ALLEN: I think we ought to be moving on. Thank you Bruce; thank you, Kevin.

The next speaker is Jeanne Nesbit, on the use of Probabilistic Volcanic Hazard Assessment in the Yucca Mountain Program.

DR. NESBIT: Good morning.

As Clarence said, I am Jeanne Nesbit. I work for the Department of Energy, and Leon asked me to speak today for a few minutes about how the Department of Energy is going to take the wealth of information that you just heard Bruce present, and also that Frank summarized yesterday, and try to make some decisions about what's important to do in future years, and whether we've answered the question of when enough is enough, at least for part of the volcanism issue.

So, I'd like to touch on today, in the next ten or fifteen minutes, a few of the things that Leon specifically asked us to address. I think Bruce and Frank, and also, Kevin, have covered some of this. I'm going to try to cover it from a DOE management perspective, basically, from my perspective of managing the volcanism program, and having to try to take this wealth of information and make some decision on what should be done next and where our resources should be allocated in the future.

So, I'm going to touch on the objectives of the PVHA studies, which we've already talked about to some extent; the use of this in our programmatic and statutory decisions; use of expert judgment; determining when enough is enough; and also, I'll summarize by pointing out some of the critical studies that I think we still need to do.

The objectives of Probabilistic Volcanic Hazard Assessment at Yucca Mountain are to assess the probability of magmatic disruption of the potential repository and/or the waste isolation system, and also to constrain the effects of magmatic events on the potential repository, and that's really the next phase which both Bruce and Frank have alluded to in their presentations.

I'd like to point out that our primary focus to date has been to try to determine whether the probability of magmatic disruption of the potential repository is large enough to disqualify the Yucca Mountain site. So, that was really our first key focus, was answering that question. We felt that was most important to determine first, was whether Yucca Mountain site would possibly be disqualified due to, primarily, a volcanic eruption.

What I've shown on the other view graph here--and, I'm sorry, I think in your hard copy, the shading didn't come out for some reason, but what I've highlighted here is what we call our post-closure tectonics program, which, actually, volcanism is not a separate program unto itself, although sometimes it seems to have a life of its own. It is part of our tectonics program, specifically aimed at post-closure issues, and what I've highlighted here, really, is the studies in that program, the volcanism are highlighted.

This is the probability study which Bruce just talked about, and I just show the arrows, the information flow in this program, with data collection and probability calculations being fed into what we've been referring to as Greg Valentine's effect study, which is basically here, and it's really the link between the data analysis and probability calculations and total system performance assessment.

What I also wanted to point out here is that there is this link which we have focused on specifically between the probability study and answering the site suitability question, and that is, would the Yucca Mountain site possibly be disqualified due to the volcanism issue. I'll leave that up there for reference.

I'll also point out that these other studies are not necessarily part of the volcanism program, but that the pre-closure tectonics information that Tim talked about yesterday feeds in here, which is the other study that links the data collection, and looks at tectonic effects on waste isolation, basically, and it's kind of the link between the data collection and analysis, and total system PA.

So, in order to talk about how we're going to use, or how we have used our probabilistic volcanic hazard information in our programmatic and statutory decisions, I need to remind everybody of what our regulatory requirements are, and what we're actually trying to look at, and that's primarily those contained in DOE siting guidelines, which is 10 CFR 960, and the NRC guidelines, which is 10 CFR 60, and they're both focused, really, on complying with total system performance requirements, and also, for 10 CFR 60, engineered barrier system containment and release rate requirements.

DR. BUDNITZ: But the first one doesn't exist.

DR. NESBIT: Yeah, I realize that it's--but that's what's in 10 CFR 960 currently. I realize that this regulation doesn't exist, so, we're basically operating to something that doesn't exist.

Yesterday, however, I think you pointed out that even though it doesn't exist, we still need to be doing the work that's necessary, so...

(Laughter.)

DR. NESBIT: The point is that the focus is on post-closure issues, waste isolation issues, total system performance. Also, that is also what's required in 10 CFR 60, and then, specifically, our guidelines indicate that we need to meet the post-closure tectonics qualifying condition, which is what I alluded to here with this question of potential disqualification of the site.

So, I'd like to give you a couple examples of how we've used a couple of tools and how we've come to some programmatic decisions in the past. First, the Early Site Suitability Evaluation, which Tim summarized yesterday, there was a lower level finding on the tectonics qualifying condition, but their recommendation here was just to continue volcanism studies as planned, and they had a few other recommendations, which I won't go into in detail. That was their basic recommendation for us.

Total System Performance Assessment, the first iteration, looked primarily at eruptive effects of dike intrusion into the proposed repository, and their conclusions were that the consequences did not exceed the regulatory release limits, and I'll point out that those regulatory release limits do not exist not, but this was also based--there's a caveat here that it's based on really limited, what we call "effects" data, basically, the work that Greg Valentine is now really getting started on.

So, their recommendations for us were that they needed to know the estimate of the probability of occurrence of subsurface events. This is what Bruce has alluded to when he's talking about the probability of an intrusion versus an eruption. And, also, they need to know the quantity of debris that could be ejected from repository depths during a volcanic eruption, and as Frank summarized yesterday, this has been Greg Valentine's first focus, and he has quite a bit of that field data now completed, and will be able to provide this information to the TSPA people, hopefully, for inclusion in their next iteration.

So, these were their two recommendations, and, based on these recommendations, we've tried to focus on, over the last year or so, moving the volcanism program from the characterization data collection, analyzing Lathrop Wells studies, and the probability calculations, we've tried to wrap as much of this work up as possible and focus our resources on the effects studies.

I'll touch briefly on determining when enough is enough, just to remind everyone that determining the answer to that question really depends on your perspective, and I've highlighted a couple here.

For the principal investigators, they basically ask whether they've completed the plan that they laid out, and this, in our case, it's something we call study plans, but they spend a lot of time and energy scoping their studies out, writing down what they need to do, and when they have completed this, when they have adequate confidence in their results, then their answer is, "Yes, we have enough."

But, from a DOE perspective, from my perspective, and the information I need to provide to my managers, we need to look at the value of obtaining additional site data versus the cost, the cost benefit of additional performance assessment, and, really, how strong is our case of compliance with the regulations that I highlighted earlier.

And, I'll point out that, in answering some of these questions, there are several tools that we use; site suitability evaluations, issue resolution, which is primarily contained in topical reports, total system PA, which I have just talked about a little bit, and then the formal peer review expert judgment, and also, feedback from oversight groups and our regulator.

Let met summarize for you, then, what we're trying to--some of the tools that I've just alluded to, and how we're going to try to use some of those in the next couple of years. These are primarily major milestones for the tectonics program. The shaded ones are things that are specific to volcanism, and in fiscal year '94, as we have said, Bruce's report, volcanism status report will be completed, and we're trying to finish up the work at the Lathrop Wells Volcanic Center.

In fiscal year '95, we will decide whether it's appropriate to write a topical report to try to start resolving some of this with the NRC, so that would be the tool of issue resolution, if you want to look at the tools that we use. Also, there is possibly a suitability evaluation that will be done in fiscal year '95.

Some of the key things that we will be doing in '94 are, as Kevin has summarized for you, the expert judgment work, which is, hopefully, going to help us take a look at the abundance of data and information that Bruce has just summarized for you, and provide to us some expert opinion on how we can narrow down some of that data, and what data is really important to be looking at, and where we should try to focus future studies.

So, this, combined with the external geophysics review that Bruce also talked about that George Thompson is doing, where he is going to look at whether we have enough geophysics information already to determine the question of buried magma bodies in Crater Flat, or whether there are additional things we should do, this information will all be looked at over the next year or so, and we'll feed that into our decisions about whether it's appropriate to write a topical report. These will be fed into suitability evaluations.

Also in '95, which I don't have on here, is another total system PA evaluation, where this information and Greg Valentine's effects work, which, although officially is starting here in fiscal year '94, some of his preliminary studies began before that, and so he is pretty far along on at least the question of the amount of material that could be entrained and erupted at the surface.

These things will all be factored in to some of the tools that we use in '95, and then these are some of the larger milestones that we plan to complete in '96. Basically, these are final report-type things, using information that we have already heard summarized, and these all feed into the tectonic and geologic models.

And then, finally, the effects milestone that is listed there takes the information not only from Greg Valentine's volcanic effects study, but also, the overall tectonic effects study, and provide that into the final geologic model.

I'll touch briefly on the use of expert judgment in the Yucca Mountain Program. We do use experts to determine the adequacy of our data set and the adequacy of analysis, and this is, as I referred to, the PIs answering the question of whether they have adequate confidence in their results.

We also use independent technical review as an accepted part of the program, and we're considering alternative mechanisms to ensure the diversity of interpretations and completeness, and this is peer review, elicitations, and, for example, in the case of volcanism, we will be using expert judgment to refine the volcanism probabilities, and this is something that Kevin Coppersmith has just summarized for you, and we hope that we have, as Bruce said, he has contributed part of his budget to this effort this year, but it is part of the PA program at Yucca Mountain, and the plan is to include these types of expert judgment and expert elicitation for other issues, and volcanism just happens to be the first one that it's appropriate to do this for.

So, to summarize, I'd like to point out some of the critical studies that we still need to do, and I would say the first one is subsurface effects studies, as I've summarized, and Bruce and Frank have also alluded to, that Greg Valentine is starting to work on.

Sensitivity studies, I think both Bruce and Greg do some of those on their own, but, also, we need some information from Total System Performance Assessment. We need to compile a comprehensive eruptive effects data set from natural analogs. This is what Greg is working on, and for which I think he has most of the field work completed.

Subsurface information, which, primarily, is geophysics, trying to get at the question of whether there are buried magma bodies in Crater Flat.

Bruce needs to include the probability of polycyclic volcanism, which he also alluded to in his talk, and we need to look at a magmatic evolution model for the Crater Flat volcanic zone.

To summarize, finally, now, the last page of your handout is just additional information that kind of summarizes the key points of Greg Valentine's effects work for fiscal year '94 and '95.

I wanted to end with this, really, what is a cartoon of some of the information that Bruce presented, and point out one area where we really need to assess the sensitivity of the information, and here are Bruce's calculations, some of them over here for the different models.

This is the probability of magmatic disruption of the repository. The shaded areas are some of the other models that other researchers have proposed, and then here are some of the, what I would call the physical reality, or some of the other Quaternary fields, and I would argue that, as Bruce did, when we get over into this area, we have reached a point where we're no longer in what's geologically reasonable, because we know we do not have the Lunar Crater volcanic field sitting in Crater Flat.

The question is, how sensitive are some of the numbers, like 10^{-7} here, and this is where we really need to work with Total System PA. I believe Greg Valentine is doing most of this work, where he provides information on the effects and consequences that he has available, and we need for PA to tell us, based on that information, how sensitive some of these numbers are, and where we really need to be worried, because, from my perspective, if the difference between 10^{-8} or 10^{-9} does not make much difference in the final analysis, then we should not spend additional resources trying to define whether 10^{-8} or 10^{-9} is more significant.

And that's all I have to say. Thank you.

DR. ALLEN: Thank you, Jeanne.

Are there questions from the Board or consultants? Staff?

DR. REITER: Jeanne, you said something in the beginning, and I was trying to get a little--you said that one of the concerns is determining whether volcanism was a disqualifying feature of the site, and I never did see a conclusion. Have you reached a conclusion on that, and if you have, what's the basis for it?

DR. NESBIT: Well, I stated this at the Technical Program Review, also. The conclusion is that we feel that volcanism is not a disqualifying condition for the site by itself, and we base that, mostly on what Bruce presented, the work that he presented, and he talked a little bit about that 10^{-8} criteria, but we feel that the probabilities are low enough that it is not a disqualifying feature.

DR. REITER: Did you have any figure in mind when you talk about low enough?

DR. NESBIT: No. We don't have any specific criteria. For the most part, we've used that 10^{-8} value kind of as a ball park, because that was the only criteria that we had available to us, even though that regulation doesn't exist any longer.

DR. ALLEN: Jeanne, you mentioned that effort was still underway in geophysical studies to determine whether there might be a magma chamber beneath Crater Flat. Of course, the results of those studies will always be debatable, but it's not clear to me how it would affect the results.

DR. NESBIT: I think the issue is really we need to make sure that we've used enough geophysics information from Crater Flat to make sure that we aren't overlooking something, and I don't know whether Bruce wants to expand on that or not, but we don't expect to find lots of buried magma bodies in Crater Flat, nor do I believe that geophysics is going to answer all the questions, necessarily, or have the resolution to find things that are this dike width, and I think that might be, in the future, another issue that we'll have to make some tough decisions on, because I don't think it's reasonable to go out and do millions of dollars of geophysical surveys in Crater Flat, looking for things that may or may not have much consequence.

DR. CROWE: Bruce Crowe, if I could just make a couple comments to that.

I think the issue that we've identified, the sensitivity of that question is, assuming that there are chambers there, it's really how long have they been there. If they've been there, and the imprint that we see in the geologic record is what it is, it just doesn't look like it's going to be a major issue. If they're developing now, and might invalidate the past record to where, say, the next 10,000 years will be very different from the future, then it would basically undermine the fundamental assumptions we've made to do the probabilistic assessment, and so, that's probably the key issue.

George Thompson and I have discussed this with John Evans, who has presented teleseismic data, and his view is that this is a very long-lived feature that's probably been there since well into the Miocene, and it would indicate that even if it is there, that it's nothing, you know, there's no process ongoing, either tectonic or volcanic process that would change future rates.

DR. ALLEN: Okay. Aki?

DR. AKI: Yesterday, we heard about the vitreous kind of earthquake triggered by the rare earth earthquake, and as you know, that the same earthquake triggered many swells in many volcanos and geothermal areas in the western United States. Does that imply that this is volcanic-related? There are no comments on that, even on this subject, but I was wondering, what's your interpretation of this vitreous kind of earthquake on volcanic hazard?

DR. ALLEN: And answer yes or no.

(Laughter.)

DR. CROWE: Maybe.

That's a very good question. I mean, I would argue that if there's a relationship, it's a trigger relationship that you have to have the pre-existing conditions that would lead to events; that the earthquake sequencing doesn't cause it, you know, it's not the causative factor.

I mean, what we think with these particular types of events, that they are sent from the mantle, from, say, depths of 30 to 40 kilometers, and so it's not like we have a chamber that's sitting there in a ready state that something like this would trigger. So, I mean, my gut answer--and this is what Mike might say to this--is that we shouldn't see that kind of a causative relationship, but I also recognize that seismologists ten years ago would have made the same comment about earthquake triggering.

DR. ALLEN: Okay. Thank you, Jeanne, and Bruce, and we'll now go on to the final speaker before the break, Keith McConnell, who will again have comments from the Nuclear Regulatory Commission.

MR. McCONNELL: Thank you, Clarence. Again, while I'm making the presentation this morning, the technical lead for the staff is John Trapp, and he's here today to respond to any of the hard questions that come up.

Our presentation today is similar to the one we gave yesterday on seismic hazards and fault displacement hazards. Basically, we'll provide the regulatory basis for our acceptance criteria with respect to volcanism and igneous activity. We'll then give you some of the acceptance criteria. Again, they're general, high-level acceptance criteria for determining when enough is enough, and then, using that as a template, we'll give you the NRC's review of DOE's progress to date, in that the benchmark for us is the volcanism status report that we got last year, and reviewed, and completed our review, and wrote DOE a letter on that topic in August of last year.

So, a large part of what Bruce presented today, I don't think that we're that familiar with, and so, I would say that some of our comments may have been resolved or addressed by what's been going on over the last year and a half or so.

And then, finally, we'll identify again some of the critical investigations that we feel are needed for the hazard assessment.

Now, one thing I would point out on this view graph is our approach to this presentation, and also our review of the status report and other reports DOE would send us, is systematic. Basically, we have our regulatory basis from which we derive our acceptance criteria. We then use that as the template for determining whether they demonstrated compliance with the regulatory requirements.

The regulatory basis, basically, is twofold and parallel. Basically, there is the regulatory requirements for the probabilistic analysis of igneous activity as it relates to determining overall system performance, but this is not the sole or only set of requirements that need to be addressed by DOE when they provide a license application. There are associated, and, again, parallel criteria that also must be addressed, and they relate to siting criteria.

Siting criteria that are applied to igneous activity are largely encompassed by 60.122, which relates to potentially adverse conditions. DOE must provide information to determine whether and to what degree igneous activity is present at the site. They have to provide information to determine to what degree igneous activity is present, but undetected. They must also provide and assure that lateral and vertical extent of data collection is sufficient to determine the presence of igneous activity, and, finally, they have to evaluate information with assumptions and analysis methods that adequately describe igneous activity.

And this refers back to both the bottoms up, and the top down approach that Part 60 takes to determining when enough is enough. Not only do you have to do the performance assessment calculations, you have to build confidence in those assessments by collecting the data and adequately investigating the site conditions; two-pronged parallel approach.

Now, using the regulatory basis, we then go to the general acceptance criteria that were identified yesterday. I won't go back through them, but, again, the first four basically provides the confidence in the modeling and assessment capabilities that you will ultimately do in the assessment of repository performance.

Now, if I could move on, we'll just give a brief overview of our reviews--and this includes reviews of DOE's study plans, as well as Los Alamos National Lab's volcanism status report, and most of this information was communicated to DOE in our August, 1993 letter, but prior to doing that, I guess we would like to commend Bruce and his colleagues for kind of putting a bull's eye on their back, and letting us take a shot at it and give them our critique of that report.

And I think I would also say that I am encouraged from what I heard earlier this morning, that maybe we're not as far apart as I originally thought, and maybe we are moving towards consensus, not closure, because I think you will see similarities in what we've presented here, and what Jeanne and Bruce just talked about.

The staff believes that DOE has made progress towards an acceptable PVHA, but as Steve Wesnousky said yesterday, however, there are some qualifications on that.

First of all, we believe that the approach identified in the volcanism status report did not consider all significant processes and events, and an example of that, we feel, is the Tripartite probability, as it was defined in the status report. It appeared to only address direct intrusion. It did not address indirect effects that have to be considered in the assessment of a repository performance.

Second, the data presented to date--

DR. NORTH: Excuse me. Could you clarify the Tripartite probability? This is the E1, E2, E3?

MR. McCONNELL: That's correct.

DR. NORTH: My interpretation of what you're saying here is that E3 used to be too narrow.

MR. McCONNELL: That's correct.

DR. NORTH: Could you comment on the presentations you just heard? Do you think this is being addressed adequately?

MR. McCONNELL: It appears to be going in that direction. It appears to be including indirect effects as well as direct effects. So, again, there appears to be a coming together.

The data presented to date to support probabilistic analyses are not sufficient to meet Part 60 requirements, and an example of this, I think, is our feeling that geophysical testing to date hasn't established the extent to which the condition may be present, but undetected, and this relates to the issue of, are the detection limits of the geophysical techniques being used sufficient to identify perhaps small features that may be out there. Again, the condition may be present, but undetected. What we would expect would be an analysis of those detection limits, and some sort of analysis of what might be there and not be present that could affect the probability calculation.

Third, DOE's approach appears to emphasize tests and analyses to confirm a preferred model to the detriment of testing alternative models and approaches. Again, what we heard this morning may, in part, resolve some of these concerns.

I give as an example, I think the Center--the acronym there is the Center for Nuclear Waste Regulatory Analysis, which is our consultant in San Antonio--the Center, in fact, when they reviewed the volcanism status report for us, identified that homogeneous Poissonian models are not suitable at Yucca Mountain, and they took it on themselves to develop other non-homogeneous Poissonian models to test some of what DOE was doing, and I think Chuck Connor will speak to that in the next presentation.

To continue, the probabilistic models used to date are not transparent and do not address the uncertainty in the analysis. Again, I think we saw some movement, and I think perhaps it was a function of the speed with which the volcanism status report got out. You know, I think there was a lot of emphasis on getting it out quickly and getting it out so that NRC could comment, so some of these criticisms may be more a function of the mechanism than of the report itself.

But, again, as an example, the Center has demonstrated that there is uncertainty in the ages of basaltic events out at Yucca Mountain, and that this does cause variation in probabilities. What we would expect would be an explicit analysis of these, of this type of uncertainty.

And then, finally, the probabilistic models to date are largely based on statistical models, and do not adequately incorporate the geologic processes and the understanding of these processes. The issue of structural control, I think, is significant; and, also, the issue of whether there is a low velocity zone down there, and what its significance is, is also an important aspect. It needs to be incorporated in the probability models.

Again, we saw in Chris Fridrich's presentation yesterday, and also Bruce's this morning, that there does appear to be some attempt to do this sort of analysis.

And to end it, we've tried to identify some of the critical investigations that we feel are necessary to address the issue of volcanic hazard, and I think the list, in many respects, parallels what Bruce and Jean presented this morning:

An analysis of the geophysical testing techniques to, again, determine the detection limits. What's present out at Yucca Mountain that's not detected?

We would suggest that an appropriate range of models that address structural control needs to be developed, and perhaps they are.

A more robust--in a similar vein, a more robust incorporation of geological data into the statistical analysis would need to be done.

Site-specific subsurface information on the low-velocity zone. The petrologic, mineralogic, and geochemical analyses, I think, relate more to the consequence analysis, and the explosivity of a volcanic event at Yucca Mountain. What are the consequences if a volcanic event does intrude the repository?

A transparent analysis, we would--I think our impression from the draft status report was that there wasn't much transparency in what they're doing. I think with the data that Bruce provided today, we're getting some of that transparency.

And then, finally, an analysis that includes both direct and indirect effects of igneous activity, and that's it for us.

DR. ALLEN: Thank you, Keith.

Questions or comments from the Board? Warner

North?

DR. NORTH: I'd like to encourage you to be a little more specific on the degree to which there isn't a perfect match between the DOE presentations and what you have on your last slide.

Are there any obvious targets for further investigation, particularly field studies, that you think are needed that DOE doesn't have on its agenda? For example, surface-based drilling to look for dikes or sills, evidence of eruption in Crater Flat, or shall we say, evidence of a volcanic event that did not lead to an eruption.

MR. McCONNELL: Yes. I think the issue's been raised in our reviews that there is this concern about the intrusive/extrusive ratio, whether the one-to-one relationship is an actual or a conservative assumption. I think there are other fields where that assumption probably is not valid, so there could be a much higher ratio of intrusive to extrusive event.

DR. NORTH: Have you defined what kind of experimental data you think is necessary to resolve that issue?

MR. McCONNELL: Let me ask my technical lead to respond to that; John Trapp.

MR. TRAPP: There's two basic areas that have been suggested for field investigation, primarily in the area of geophysics.

One is, really, completion of the geophysical seismic lines across Crater Flat, et cetera, across Yucca Mountain, which, I believe, is scheduled for this year, I'm not sure exactly when. Another one, specifically, would be more incorporation and more explicit incorporation of the teleseismic data into understanding the subsurface characteristics.

In addition to this, there is quite a bit of information in the petrology/geochemistry area, where you start getting mainly into consequence, but because you're talking spatial--well, statistics which are driven by spatial understanding of the volcanic process, and the larger processes, et cetera, end up with a different probability than the smaller processes, there is a need to incorporate these into the E2 estimate, to better understand exactly where things fit.

MR. McCONNELL: Did we answer your question?

DR. NORTH: It goes in the right direction, but I'd like to see if we could be specific. I think, in my lack of earth sciences background, I can see that if we go with the tunnel and we find a 10,000-year-old dike of intrusive material in the proposed repository zone, that's likely to be a clear disqualifying situation.

However, the issue becomes, can we, by indirect measurements, the geophysics, assure that there aren't dikes near Crater Flat that would provide evidence for such things. Do we have to drill more holes down there to be as sure as we are going to need to be? And, if it would appear that we have to do some expensive investigations that are not currently in the study plan, would it not be well to try to identify this now so that we can budget for them and plan for them, rather than to determine three or four years from now that the information is not adequate to resolve this issue.

MR. McCONNELL: Let me try to respond. I think that we've attempted, in our reviews of study plans, to identify any weaknesses in the characterization program. I don't know that we've identified any specific drilling program or anything else that needs to be done.

What we feel is necessary is analysis of the detection when it's of the geophysical techniques, so we know what size of feature, or what we may not be seeing, and then we can make a judgment of whether we need to do additional testing, or spend that money to do a testing. Maybe we can bound, or maybe DOE can bound what could or could not be there.

That would be my response. Maybe John--

DR. ALLEN: John, do you have any further comments on that?

MR. TRAPP: No. I think Keith has hit it fairly well, because one of the key parts of Part 60 that we're looking at is the "basis" that you make sure that there are not underestimations.

If you take a look, for instance, at a lot of the geophysical data, there has been, at least in the past, the basic contention that everything's been seen. The aeromagnetic is such that you can pick up all these features, et cetera.

As I think Jeanne pointed out, there is not, with the present level of geophysics we've got out there, the real potential of detecting a lot of dikes, et cetera, this type of thing. They just cannot be seen with this thing.

Now, what is the real effect of these additional dikes that may be there on any type of calculation? Do these type of analysis, and then maybe you can find out how sensitive these things are.

DR. ALLEN: Thank you.

Other comments or questions from the Board or consultants? Bob Budnitz?

DR. BUDNITZ: Put on that last slide again. I want to ask a question.

The penultimate bullet used the word "transparent," but it was nowhere else in this slide. Is there something special about why that one has to be transparent, or some gripe that it wasn't before or something that made you put that word there?

MR. McCONNELL: I think it is in response to our review of the status report, where a lot of the uncertainty, we didn't believe, was included in the analysis. And, therefore, the numbers, the probability ranges that came up were fine, perhaps, but we couldn't see the basis for developing those numbers. The basis and the path towards developing those ranges and the specific most likely number that was given was not clear to us.

DR. ALLEN: Allin Cornell?

DR. CORNELL: I have a question to the Board. Keith's presentation this morning of a review by NRC of work submitted a year ago and done the year and a half before that, and the possibility that it's been changed by work that Dr. Crowe has done in the subsequent year and a half reminds me of something I learned in one of my previous rare non-disruptive intrusions into Yucca Mountain, where I learned that the NRC is prohibited from talking to DOE on a technical level, and finding out what they're doing, what their models are, how things are going on except in very, very controlled circumstances that don't seem to happen very often.

This, to me, as a taxpayer, seems to be sort of wasteful, and, technically, seems to be at least a delay, if not a constraint in getting to a good solution. As a Board, you must have encountered this in many other circumstances, and I wonder their view and opinion is about the reasons and causes and benefits and dis-benefits of this being some kind of constraint in the process.

DR. ALLEN: Well, let me just say, there have been an increasing number of workshops involving NRC, ACNW, this Board, DOE, and my impression is the extent of communication, in a constructive way, has been increasing and much better.

Would you agree with that, Keith?

MR. McCONNELL: It's been increasing. I think we would still like to--and we're struggling to find it--find a mechanism where the technical people can sit down in an unstructured forum and go through these issues, where there isn't the aura of a recording device.

DR. ALLEN: It's not just an aura, it's there.

MR. McCONNELL: And other factors that tend to formalize it, and to, I think, make it more difficult for people just to sit down and discuss the issues, and I think there is a mechanism, it's just trying to do it appropriately.

You're correct, though, there is--I mean, the size of this meeting is consistent with the size of most meetings. They're technical exchanges that are held on tectonic issues, and so, it makes it very difficult to have five or six people sit down and talk to each other, and there always has to be the cognizance that there are other parties involved in this process; the state and the affected governments.

DR. ALLEN: Other comments and questions? Staff?

(No audible response.)

DR. ALLEN: If not, thank you, Keith; thank you, John Trapp.

We'll now take a fifteen-minute break until ten-thirty.

(Whereupon, a brief recess was taken.)

DR. ALLEN: We will proceed with the next presentation on the models of volcanic hazard at Yucca Mountain by Charles Connor of the Center for Nuclear Waste Regulatory Analyses.

So, Chuck, it's all yours.

DR. CONNOR: Thanks.

Today I'd like to talk about some of the status of CNWRA volcanism probability models, the way I'm specifically asked today, to spend some time comparing some of the non-homogeneous Poisson models with other models that we've been developing, and looking into the range in probability distribution functions that result, some of the significance of these differences from my view.

Here's an outline of the presentation, then. I'd like to give you an overview of some of the probability model development at CNWRA that's been going on, effectively, in volcanism for a little more than a year now. Spatial and temporal patterns in vent distribution are an important aspect, I think, of the study, and I'd just like to very briefly talk about some of the spatial and temporal patterns in volcanism that we've looked at in a statistical way.

I'd like to talk specifically about two models we're developing. One is a near-neighbor non-homogeneous Poisson model; another is a spatio-temporal homogeneous Markov model, and then I'm going to discuss the limitations of these models as they exist today, and how I think some of the numbers are going to change as a result of improvements in these models through time.

Really, right now, we see three groups of models, effectively, in our volcanism program. The near-neighbor non-homogeneous Poisson model really, you know, starts with the supposition that there are spatial and temporal changes in the recurrence rate of volcanism in the area, and these need to be taken into account. They are through that model.

The Markov model assumes that volcanism can be treated as a spatio-temporal Markov variable. I'll talk a little bit more about that in the middle, and then there's the Cox cluster process as well, which I'm not going to talk too much about today, but in systems that show clustering, and that goes from epidemiology through galaxies, in general, that's a model that's widely used and probably should be investigated in some detail in this process.

So, how are these models different? Well, they're based on spatial and temporal patterns in volcanism; that is, statistically significant spatio-temporal clustering, and I'll get into that briefly in a minute.

One difference is you don't really need to define discrete areas in order to estimate probability functions. Another difference is you can map probability surfaces, and that really provides a sense of spatial variability in the system.

That leads to the next bullet, which is: It's possible to capture some geologic detail in this model a little bit easier, and I think it's easier to integrate into iterative performance assessment, and work toward a geologic hazard map.

Just as an example, if you've got a probability surface, and you think that that probability varies over some region, you've got spatial geologic data, like faults or something like that that you want to integrate into that model, I think the probability map, or, say, recurrence rate map is a pretty reasonable approach to looking at the impact of that sort of geologic detail in the long run.

Just real briefly, volcanos in the Yucca Mountain region form spatial clusters. I think everybody agrees with this. In a sense, it doesn't matter if people agree or not, because you can prove it statistically, so, with 99 per cent confidence, you can say that volcanos form spatial clusters, and then there are also differences, small differences in the relative ages of nearby cinder cones. In other words, young cinder cones in the region tend to be close together, old cinder cones in the region tend to be close together, and it's comparatively rare that you find young and old cinder cones sitting right next to each other. So there's a spatial clustering and there's a temporal clustering in the area.

That leads to two bullets here that the recurrence rate must vary in the Yucca Mountain region. It's got to vary temporally, it's got to vary spatially. It'd be nice if probability models can be developed with some confidence to capture that kind of variation.

Homogeneous models are not going to adequately capture that kind of variation, and my basic supposition here, then, is that these homogeneous Poisson models are going to overestimate the probability of volcanism in some parts of the region. For example, far from wherever volcanos have occurred in the past, a homogeneous model is going to predict that the volcanism, the likelihood of volcanism in that spot is the same as it is in the middle of Crater Flat Valley. I don't think that's a reasonable approach.

So, I'd say that when we look at these cumulative probability distributions for probability models, for example, we have to look very carefully what models are incorporated into those kinds of probability distributions.

I would say that the homogeneous models, it's tough to incorporate those, given the fact that you've got this variation in the recurrence rate, which is statistically significant. So it is valuable to look at these data in some detail.

Well, if you decide that there is spatio-temporal clustering of events with 99 per cent confidence in the Yucca Mountain region, then your next problem is how to estimate a recurrence rate which varies through the region.

There are various ways to do this, and one way to do it is to use a near-neighbor model, where you're estimating the recurrence rate at a point. It's a function of the number of near neighbors you choose in your model--I'll talk about the significance of them in a minute--and a time/distance term, which is defined in more detail down here, and I'll let you go through that.

The basic idea, then, is that you can estimate recurrence rate at a point by moving it through this formula, and that recurrence rate is going to change as a function of things. It will change as a function of distance from a nearby volcano and the age of that volcano, and it will change as a function of the number of nearby volcanos you use in that model.

Well, how do we get a handle on the right number for m, or what range of values for m, the number of near neighbors you ought to use. Well, what we've done so far is just integrate this value across the entire region and compare that with the regional recurrence rate. There's not a lot of agreement on what the regional recurrence rate is. That's okay. You can do the calculation using a whole range of recurrence rate, and look at the sensitivity of the model through those changes in recurrence rate.

So, we develop these models, use a series of Ns, numbers of near neighbors, and integrate that over the entire region to look at the recurrence rate. It's then possible to plug that through the familiar formula, this model which say that the probability of the spot is going to depend on, if you just look down here, the recurrence rate at that point, the area covered there, a, and the time you're considering, which is Poisson's formula.

In this case, you can change the probability by changing the area considered. For instance, if you decide to use an area for the repository of six square kilometers, you expand that to eight kilometers, cut that down to five, that's going to impact the model and, of course, t will impact the model as well. If you push that out to 100,000 years, the probability goes up.

The net result of this type of model is a map that looks like this. This is essentially a map of probability, and what I've done here is I've contoured it to simplify the model a little bit. Basically, it's a log contour map. This is explained in some detail in your handout, but -4 here, for example, is a log probability. That's a probability of one in 10,000 in 10,000 years.

So, basically, what I'm saying there is that inside this line, the probability of a given eight square kilometer chunk of land will experience volcanism in the next 10,000 years is on the order of one in ten--is greater than one in 10,000.

A couple things about this map. It's put together using a six near-neighbor model, and that corresponds to a regional recurrence rate of about seven volcanos per million years. The open triangles here are the Pliocene cinder cones, closed triangles are Quaternary age cinder cones, and so on. So, here, we see that the repository, in this case, would actually sit on a probable gradient. The probability is much higher than the center of Crater Flat, where we'd be predicting values using this model of greater than one in a thousand in 10,000 years for the likelihood of a new volcano forming, and that probability drops off pretty rapidly with distance from that spot, primarily because volcanism has persisted in this region for a pretty long period of time, in a relatively concentrated way.

Well, what we do with this model? How can we test this model, and is it really useful in predicting where volcanism is compared to, say, the homogeneous model?

We've done a number of tests in that regard. I'll show you the results of some of them. We can't go into the future. I wish we could. We know, you know, we wouldn't be having this discussion if we could, so we don't know how well any of these models are going to work in the future. We certainly can't wait around for the next volcano, the way we can for the next earthquake, to test and prove our models. It's got a very low recurrence interval.

But, we can go back in time, if we put a little faith in geochronology. The geochronology data set out here is outstanding, and has been the result of a lot of work on the part of DOE and its contractors, so let's use that data to help test some of these probability models.

So, these different graphs represent slices in time. This graph represents the area five million years ago. The white triangles, or the open triangles represent volcanos that have--events that have already formed at that time. The black triangles represent volcanos or centers that have not yet formed.

Now, if we put together a probability surface for five million years ago, we can see that at that time, most volcanism actually, in the region, is concentrated east even of this map area, and a little bit to the north. Very little volcanism in the last ten million years has occurred in

Crater Flat Valley. All those triangles are black, and the model does a pretty poor job of predicting a big shift in that direction. It's based on the past distribution of the events, so it doesn't quite capture that shift, so the probability over here is low.

Over the next million years, between, say, four and a half or three and a half million years ago or so, some volcanism occurred in this area, which just doesn't get predicted well by the model.

If we go over to 1.6 million years ago, there's a lot more white triangles on the plot. Volcanism has occurred during that interval of time, and a lot of that volcanism has occurred in Crater Flat Valley, and there are aeromagnetic anomalies down here, some of which have been drilled, and that's assumed to be volcanic centers down in the Amargosa Valley.

Okay, so we've got a change, a shift in the probability map. 1.6 million years ago, sometime just prior to the formation of red cone, black cone, and other cones in the Crater Flat alignment, the probability density map has formed in this region. There's an anomaly, or a mode over here in this region simply because of these older volcanos that formed four and a half to three and a half million years ago. That's the area most likely to experience volcanism in the future, based on the model, 1.6 million years ago. The recurrence rate is relatively low at this period of time because it's been awhile since volcanism has occurred, and I should point out, this is a 6 Near-Neighbor model, just using that throughout.

Well, the model predicts the location of the next volcanic events pretty well. Those are these five cones in the Crater Flat alignment. It does about twice to three times as good as the homogeneous Poisson model, using that recurrence rate.

After that event, these probability contours expand because there is an increase in the recurrence rate. We've got more volcanos right here. There's an increase in the recurrence rate. These probability contours expand, so we can look at the behavior of the model through time.

The next, I'll say episode of volcanism in the region was near Sleeping Buttes, something like .3, ±.2 million years ago. The model doesn't do such a good job of predicting the locations of Sleeping Buttes. That's primarily because these events take place far from where volcanism has been center over time and, in fact, if we chose, somehow, an area and went through a homogeneous Poisson calculation, a homogeneous model would likely do better for predicting the location of Sleeping Buttes than this non-homogeneous model.

And then, if we choose a slice of time, then, just before the formation of Lathrop Wells, which I'm just going to say is 100,000 years ago. I know there's some controversy about that, but that's okay. We'll just say just before 100,000 years ago, how well does the model do for predicting the location of Lathrop Wells? Pretty good, about two to three times better than a homogeneous Poisson model would.

So, here is what's happened, just in summary. Through time, you can look at the probability of volcanism occurring in these eight square kilometer areas across the entire region, and compare that to where the volcanos actually formed. Through since the beginning of the Quaternary, and actually going back to about four million years ago, the model predicts that volcanism is going to be concentrated in this region around Crater Flat, and that turns out to be the case. It's persisted through time.

So, the Quaternary volcanos that have formed, cinder cones that have formed, you've got eight of them, six of them in the Crater Flat region. The model does a pretty good job of predicting the location of those guys compared to Crater Flat, so that's one conclusion, is it seems to do an okay job of predicting where the volcanos are going to be.

The second conclusion you could draw from this is that, well, at least on the basis of one probability model, volcanism has persisted over a pretty long period of time in the Crater Flat area itself, and the Yucca Mountain block, the repository, proposed repository is relatively close to that cluster. So, this is the kind of utility that a non-homogeneous, a spatial and temporal model can do.

It's not the only model that we can use to explore spatial and temporal patterns in volcanism. Another example of a model that's been used in volcanology in the past, but not really in a spatial sense, is a Markov model; and that is, we're going to treat volcanos as Markov variables for a minute, and in words, effectively, what that means is the location of the most recent eruption most influences the position of future eruptions. So, in this kind of model, Lathrop Wells is going to play a real important role, because that's the most recent eruption.

Through time, if volcanism doesn't occur, if there's a long repose period, if you will, the position of future volcanism tends toward a homogeneous model. So, if volcanism doesn't occur for--homogeneous Poisson model, so if volcanism doesn't occur for a long, long period of time, you're going to tend toward a homogeneous model. That tendency is described by the diffusion equation; actually, by the Fokker-Planck equation, if you want that diffusion and how it takes place over time.

Now, the parameters have to be estimated in that diffusion equation, and those parameters are estimated from the positions of past eruptions in the Yucca Mountain region.

There's a little math here that I don't really want to go through in detail. Basically, you've got a Fokker-Planck equation describes this probability, conditional probability density distribution. The probability is going to be a function of time, and it's going to depend on two variables which need to be estimated somehow.

One is this variable eta. It's a time derivative of mean volcano position, and sigma squared is a time derivative of the variance in mean volcano distribution. These parameters depend on the moments of the probability function, where, for instance, eta is a time derivative of a, and sigma squared is a time derivative of b, such as depends on the first and second models of the probability density.

In this case, we estimated these parameters with a sledge hammer, just said that we're going to use the root mean square fit of eta and sigma squared in this distribution to estimate those parameters; that is, based on the positions and timing of volcanism over time. If you expand this into two dimensions, you have to deal with a sigma squared x, a sigma squared y, and eta sub x, eta sub y. So, you've got a probability density function. We estimate those parameters from the distribution and timing of past events.

This is the kind of model you come up with when you use this kind of probability density function, this Markov approach. I want to go through the way I've cast this a little bit carefully, because the--you can cast the solution to this in different ways.

Here, basically, what I've said is I'm going to look at this as an area term. If volcanism were to occur today, where would it be most likely to occur in the region? And, again, this is a log probability plot, so -4 indicates that if volcanism were going to occur today, the probability that it would fall within this region, within an eight square kilometer area, would be about 1 x 10^{-4}. Within this, it would be 1 x 10^{-3}.

And, of course, this model indicates that volcanism is most likely to occur at Lathrop Wells. Why is that? Because that time derivative of eta, estimated from the distribution of volcanos as a whole, moves pretty slowly on the time scales we're interested in. Lathrop Wells is a real young event, so, as this model would say, volcanism is most likely to occur near Lathrop Wells, since that change in mean volcano position is relatively slow.

On the other hand, this thing's got some kind of variance to it, and that variance is estimated from past distributions, from the distribution time and the past volcanism as well, so out here at the repository site, then, using these kinds of--using the parameters I've used in this model, you wind up with a probability that volcanism would occur there on the order of 1 x 10^{-3} to about 3 or 4 x 10^{-3}.

That would be the probability would occur there if volcanism were to occur now.

There are various ways to cast this, and so, what we've done here is contour if volcanism were to occur now, where would that volcanism most likely occur. That's not per 10,000 years. So, we've got a slice in time. Where is the most likely position volcanism would occur if it were to occur now?

One thing you could do if you wanted to take this approach is multiply that by some recurrence rate or whatever, and get a probability density function for the disruption of volcanism in 10,000 years. So it's two contour plots that contour different things.

So, I just want to summarize some of the results using these parameters. If you assume a regional recurrence rate of between four and ten volcanos per million years, and using this non-homogeneous Poisson model, you wind up with a range of probabilities of disruptions on this order, from about 8×10^{-5} to about 3.5×10^{-4}, and most of the values fall within here. It would be great to re-cast these as uncertainties, and we plan to do that.

Based on the preliminary results from the homogeneous Markov model, and using a age of Lathrop Wells between .05 and .15 million years. The uncertainty of the age of Lathrop Wells is important in this model. You wind up with a probability that a new volcano will form within the repository bounds, should volcanism occur, of between about 1.5 and 3 x 10^{-3}. This is just an area term. Should volcanism occur, that's where it would be.

DR. CORNELL: And you might consider hitting that with a number like 10^{-8} if you wanted to ask what is the probability one will occur there next year?

DR. ALLEN: That's Allin Cornell speaking, for the record.

DR. CONNOR: Well, except nobody would use that low a number, I think. I think everybody's estimate of the regional recurrence rate is higher than that.

So, I want to point out, these numbers are likely to change, and I think it's important to keep this in mind. Some of these are applicable only to our probability models. I think some of these are applicable to everybody's probability model.

Right now, in our models, we treat volcanos as points, and we're just looking at where the volcanos have been, at what time they've been there. There's enough discussion on how to treat that data set. We're not really considering things like probability density functions for dike geometries in these models. We're not really accounting for satellite vents in these models and the probability of disruption. I think that once we include those kinds of things, the probability of disruption is likely to increase.

Probability models generally don't incorporate geologic and geophysical information to a convincing degree. I think every presentation so far on volcanism has basically stated that in one form or another.

One thing that's important is we need to look at the indirect effects of volcanism. What's the change in the hydrologic setting? What are the changes in the geochemical transport rates as a result of volcanic degassing in the subsurface, for example? Bruce mentioned that, but the DOE is looking into that. I think it's an important thing to constrain, because that will greatly increase some of these area terms.

These are sort of equivalent to what Allin Cornell referred to as the loading terms in his discussion yesterday. I think they're important. One is, what's the scale of structural control on magma ascent? Can structures capture these things or not? How likely is that to occur? What effect does change in the magma supply rate in the system have? It would be nice to somehow try to incorporate those factors into probability models in a very systematic and pragmatic way.

There's a lot of uncertainty in these calculations. We just finished talking a bit about the shallow intrusion/extrusion ratio. That could be a very important factor; for instance, in the coastal volcanic field, Charlie Bacon, based on heat flow measurements, said that the intrusion and extrusion ratio might be about 200 to 1, so there are ways, using some geophysical methods in some circumstances to address that kind of issue, and help constrain it.

Geochronology, of course, has an impact on uncertainty as well, which several models have attempted to address. It needs to be propagated through all these calculations.

Is there a range for explosivity of small volume basaltic eruptions? Is there a PDF for explosivity? What about ash and waste dispersion models? It's unlikely that, given a normalized release standard, for example, an ash or waste dispersion model would be very important, but on a dose-based model, the way ash is distributed as a result of one of these eruptions is probably very significant.

I'd just like to point out that if you just take the literature as it exists, probability calculations for one or more events disrupting the repository during a 10,000 year period of time vary from about 5×10^{-5}--that's essentially the number that was in the LANL status report draft of about a year ago--up to about 3×10^{-3}, based on some of the UNLV work where N is the number of small volume basaltic volcanos and, you know, as you guys are aware, these are based on widely varying assumptions and solution strategies.

Well, I think this is clear. As people have stated previously, all these models indicate that volcanism is a performance assessment concern. I don't think there's much question about that. From that, it follows naturally that a probability model that does not incorporate the geologic detail; that is, these load terms I was talking about, doesn't fully address the volcanism issue.

Since no probability model right now incorporates the geologic detail to a sufficient degree, I'd say there is no question about closure on a probability issue. That needs to be addressed, and the range in current models, even if you want to pare that down a bit, has a pretty important impact on performance assessment. It makes a big difference whether we're down here in the high 10^{-5} to the mid-10^{-4} range in terms of performance assessment, and I think that's also important to keep in mind.

So, I'd just like to summarize this in three ways. First, some specific results of our probability analyses at the Center:

Vents cluster in time and space in the Yucca Mountain region. It's a fact. You don't need to worry about it any more. Probability of eruptions is highest near Crater Flat, and it's been that way since about the beginning of the Quaternary, and, actually, we could send that back all the way to about four million years. To me, that would indicate that there's a problem applying temporally or spatially, particularly spatially homogeneous Poisson models to this kind of system. Since there are so many other well-developed classes of probability models designed specifically to deal with that sort of issue, it's auspicious to incorporate those in an analysis.

The probability of a new volcano forming within the candidate repository site, based on this non-homogeneous model, is on the order of 1×10^{-4} to 3×10^{-4} in 10,000 years. Markov models, of course, support the idea that volcanism is most likely to occur in the Crater Flat region, simply because that's where Lathrop Wells is, and that time-dependent mean volcano position doesn't move very fast. The time derivative isn't large.

At least our models aren't going to be complete until we can do a bit more work. Well, for example, incorporating indirect effects, explosivity, structural and tectonic control into these probability models.

And I'd just like to end with kind of an overview statement that it's worth exploring a full range of these models, and I'd like to make a couple points about that. First, the effort that goes into probability model development is really small compared to the effort that has gone into getting this data. There's a lot of blood on the floor over geochronological issues, and it seems like it's finally coming to some kind of resolution. Lots of people have spent an awful lot of time mapping out there. Compared to that, the probability model development is not a big deal, and not a big sport.

There's a whole range of probability models--three of which I've shown you, I've described here, that we're pursuing--that can be applied, and, finally, it's auspicious to test models using other volcanic fields, and I think that'll reveal some of the strengths and weaknesses inherent in this approach, and, as Bruce showed, he's moving in that direction. I think it's an important thing to do. Before there is any kind of closure on this probability issue, I would say that--or consensus, I guess I'm supposed to say, on this probability issue, it's got to get to that point in order to get some confidence in these kinds of models.

So, thanks for your attention, and I think I have time for some questions.

DR. ALLEN: Thank you, Chuck.

Comments or questions from members of the Board or consultants?

DR. MELSON: Chuck, first of all, I think I'd like to commend you on having the mathematics fit a bit more of the reality of the situation by using the cluster approach and whatnot. I think Bruce is incorporating that, kind of in following what you're doing.

However, so acknowledging that you're getting closer maybe to what some sort of real physically-constrained model is of volcanism, I notice you don't look into or don't use what seem to be some of the patterns; for example, the elongation to the northwest of the vents. Your equations don't seem to take that part of what seems to be a significant feature; is that correct?

DR. CONNOR: Well, you know, if--for example, I have applied this in kind of a preliminary way to some, you know, for example, to the Lunar Crater volcanic field, where the patterns really elongate, and the non-homogeneous Poisson models capture that kind of change in the volcanic position through time; and, for example, the Markov model, when you look at the time derivative of mean volcano position, that just moves right along up the chain. So, in a sense, I would say that if that is a truly significant aspect of a volcano distribution, then maybe the models would capture that on their own.

Second, I've done some statistical analysis on the significance of the volcano alignments that have been proposed in the Crater Flat region. I've presented this at a technical exchange last year, and I don't find them to be very convincing in a statistical sense. Now, that doesn't mean that we ignore them, but that means that I think that limiting, or that defining of an area called the Crater Flat Volcanic Zone on the basis of the distribution of volcanos alone might not be warranted.

Now, if additional information could be brought to bear on that, such as regional structures or whatever, I think that's great, but, right now, I don't see that as being any reason to force a model in one direction or another based on patterns in vent distribution.

DR. ALLEN: Bill, why don't you follow up if you wish to?

DR. MELSON: I just wanted to follow up a bit on that. What did you make of John Whitney's model for the distribution of tensional systems that might be showing such a trend? This seems to be--

DR. ALLEN: This was Chris Fridrich's model.

DR. CONNOR: Chris Fridrich's model on the--

DR. MELSON: I'm sorry. Chris Fridrich's model; right.

DR. CONNOR: Yeah, on the right or lateral shear zone. I think that's a great model, and I think that it's a real interesting point, that maybe you can, based on geologic information, confine your analyses to particular areas.

I would point out, though, that it's probably not a conservative approach to drop the probability from, say, very high in the Crater Flat volcanic zone, to zero just outside that zone, based on a structural model, and if you go to fields where a lot of volcanism has occurred, you find out that that wouldn't be a good thing to do, that it does tend to diffuse outward through time, the more volcanos you get. A good example would be the Michoacàn Guanajuato volcanic field in Mexico, or even some of the larger fields in the western United States.

DR. ALLEN: Allin Cornell?

DR. CORNELL: I have a related follow-up question, too. I think the question may go to the form of the diffusion equation for the probability you've got here. It doesn't have a term that, in effect, permits rotation of the axes.

DR. CONNOR: That's right.

DR. CORNELL: And if you did, it might try to line up along that northwest trend.

DR. CONNOR: Yeah, we're working on that.

DR. CORNELL: Okay.

DR. CONNOR: The Markov model is preliminary, and I wanted particularly to present it here just to show there's another class of model that needs to be investigated, and, certainly, that point you raised is an important one, that, right now, we're--

DR. CORNELL: Another way to say it is, if you just changed north, your numbers would change, I think. In other words, you might find that these become, instead of these sort of near circles that your results get, if you change the x and y definition, you might find yourself simply narrowing-

DR. CONNOR: That's right, and I actually have done that, and it doesn't really affect it.

DR. ALLEN: Mike Sheridan?

DR. SHERIDAN: I'd like to indicate that I agree pretty strongly with you, that we should incorporate geological and geophysical concepts, and accepted models into any model of volcanism, and I'd like to have you show that last slide, that last transparency that was on the--that you showed up there. It was just up there a minute ago, to illustrate--yeah, right.

For example, in your Markov model, you've taken into account all of the volcanos that occur in this region, including Sleeping Buttes, isn't that right, and Buckboard Mesa?

DR. CONNOR: Yeah.

DR. SHERIDAN: Now, if we take into account a model for magma generation and transport to the surface, and we imagine some reasonable depth of magma generation, for example, like 60 kilometers, or whatever you want to choose, and then you imagine that we're considering one system here, rather than several different systems that are quite independent and not related in time or space, as the Markov model seems to indicate, that it can jump from one place to another.

I think we then get into rather convoluted geological arguments to explain how magma generated from a single source could be shooting around to all these other sources.

DR. CONNOR: Right, and I don't want to defend the geologic basis of the Markov model. I think there's some things that it might be good for addressing some issues it might be good for addressing, and others where it's not as good at addressing it.

One way to approach the kind of problem you're talking about is to go to a non-homogeneous Markov model, which previous events, not just the last one, get more weight on the actual conditional probability density function you wind up with, and that's an approach. I think this needs to be explored in more detail.

DR. SHERIDAN: I think if we take that approach, then the recurrence intervals for each of the different systems would be quite different. I think it's really something that should be explored.

DR. CONNOR: Yeah, and a good class of models, actually, to deal with that kind of question is this cluster process models, in which you have a parent process, which might be magma generation, and you might say that has a homogeneous Poisson distribution if you want to, or whatever, those pockets where magma is generated, and that results in a daughter process, and daughters being the volcanos themselves, might be distributed according to some probability density function about each parent.

Those kinds of models have a, you know, for the magma generation story, anyway, that kind of model has a good geologic basis, and even some of the probability density functions you might apply to the daughter process could have a good geologic basis as well.

DR. ALLEN: We just have to move on here. Do you have something important, Allin?

DR. CORNELL: Just to say that this is exactly after shock removal, et cetera, in the earthquake problem, and I really recommend we look at those.

DR. ALLEN: We have to move on. For example, I feel sure that Bruce would like to say something here, but this afternoon, hopefully, we'll have a chance to explore some of these further.

The next presentation this morning is comments by the State of Nevada, and Dave Tillson will be speaking for Carl Johnson.

MR. TILLSON: This is going to be short, because of four reasons: One, I only have two view graphs--actually, five reasons. I don't have Steve Wesnousky to run them, and a lot of it is repetitious of yesterday. We want to give much more time to Dr. Ho and Dr. Smith, who are the State of Nevada's experts in volcanology, and, finally, Leon Reiter hasn't written a book on this subject, so I don't have to have some view graph relating to what he's said.

The State of Nevada has been involved in this program for finding a site for the disposal of high-level radioactive waste since 1983, at the start of the National Waste Policy Act, when there were nine sites.

Early in the state's involvement, the then project manager for the Nevada siting project, Don Vieth, stated that resolution of the volcanic hazard had the highest priority relative to studies at Yucca Mountain. He indicated that a need for quick resolution was paramount, in DOE's view, because of the obvious perception of hazards posed by volcanos in close proximity to a possible hazardous waste repository site. Now, here we are, meeting over a decade later, and we still do not have resolution on the volcanic hazard issue. Why is that?

The primary reason is that there is no consensus as to what is the natural volcanic system operating in Yucca Mountain. We know that there have been volcanic processes that have directly affected Yucca Mountain in the past. We know that there are active volcanic processes operating within the Yucca Mountain geologic setting today. What we do not know is the potential process which could trigger volcanic activity in the future.

The State has commented on numerous occasions before this Board about our concerns with the DOE's approach to assessing the volcanic issue. I will not repeat those comments here today. I will only repeat that the basic tenor of our argument that first the DOE must understand the natural system at Yucca Mountain, and the volcanic processes operating within it in order to define the hazard, then they can begin to define the engineering system and the ways it can fail when subject to the hazards, in order to establish the potential consequences.

Now, Gene Smith, particularly, will have a lot more to say about the problems we have with understanding the natural system.

Once an engineered design has been decided upon that minimizes the potential consequences, we think then, and only then, a risk assessment can be made.

I will not read all of these, because I assume most of you have read them while I was making that brief statement. I will only state that, until our knowledge is complete about the volcanic processes operating at Yucca Mountain, we do not feel that an acceptable volcanic hazard assessment can be achieved.

And, with that, I'll turn it over to Dr. Smith, unless there are some questions.

DR. ALLEN: Thank you, Dave.

Are there questions for Dave Tillson?

(No audible response.)

DR. ALLEN: Let me just ask: You state that until the understanding of volcanic processes is complete, we will never be in a position to evaluate the hazard. Well, of course, our understanding will never be complete. Does that mean we will never be able to evaluate the hazard?

MR. TILLSON: I don't think I meant it in--or Carl Johnson meant it in quite that absolute term. We all know, and certainly as earth scientists, that there's some level of understanding. Now, we haven't approached that level, is our position. We still feel that there's differences between the State's understanding of the volcanic processes that are operating, and those that the DOE are proposing, and we don't feel that we have arrived at that point yet.

Now, in terms of a methodology, certainly, it's appropriate to talk about a methodology and the development of such, the expert judgment. I, personally, encourage the use of expert judgment, but to say that we're in a position now to take a final standing on what the volcanic hazard in Yucca Mountain is, I think it's premature.

More importantly, we still feel that there is this schism or difference between the hazard and the consequences. You can't do one without the other, but you have to do the hazards first before you can realistically assess what the consequences are going to be.

DR. ALLEN: Other comments or questions by Board members, consultants, staff?

(No audible response.)

DR. ALLEN: Okay, thanks. Thanks, Dave.

Then our next speaker will be Gene Smith with the University of Nevada at Reno, speaking on alternative models of volcanic zones and probability of eruption near Yucca Mountain.

DR. SMITH: For the record, to start off with, to correct Clarence, since there is a major difference between the University of Nevada, Las Vegas, and the University of Nevada, Reno, I just wanted to point out I am from the University of Nevada, Las Vegas, for the record.

DR. ALLEN: Did I say Reno?

DR. SMITH: Yeah.

DR. ALLEN: I apologize.

DR. SMITH: That's a minor problem.

I was persuaded by our secretary to use some high-tech holders for transparencies, and I'm not sure exactly what this is going to result in, probably total catastrophe as I drop them all.

I just wanted to point out that this will be a two-part presentation. I will begin by talking about the geological aspects of our studies, and then Dr. Ho will talk about the probability studies that he's been doing, so that I will talk about 15 or 20 minutes, and then Professor Ho will then take over and finish the presentation.

Now, there's several important questions I would like to try to address today. The first is, which geologic studies are important for hazardous assessment? This is what I was basically requested to do during the time that I have, rather than give a progress report of my activities for the last year and a half.

And then, secondly--and this is something that's very important--is will these studies make a difference in probability calculations? And, hopefully, this is a topic that Professor Ho will address following mine.

Thirdly, can we have confidence in hazard assessment models, without understanding the volcanic process?

Now, the three geologic studies that I'd like to discuss today--these are three of many--three that I feel are very important is, first, the definition of a volcanic event. This factors into most of the probability models; however, I don't really think we have a good understanding of what a volcanic event is. We have to talk about this.

Secondly, what is the structural control of volcanism, and what are the areas that might be affected by future eruptions? This has been a topic of great debate up to now, during this meeting, and I think something that I'd like to add some comments.

And, also, what is the explosivity of eruption? The first two mainly reflect probability calculations, the last one mainly is related to consequence analysis.

Now, in terms of the definition of a volcanic event, to me, the definition is unclear. There's a variety of different ways that you can define volcanic event. I've seen definitions based on chemistry, field relations, geochronology, geographic distribution. I think we have to come to some consensus and develop a usable definition of a volcanic event.

Now, there's several different ways that one can define this term, and I'll show you examples of each of these. You could define a volcanic event as simply a field of volcanos formed at about the same time, or the eruption of a chemically distinct batch of magma, or you could define a volcanic event as eruption separated by significant periods of time, or you can simply count the number of vents that you have, or you can count the number of volcanic complexes. There's a variety of different ways of doing this.

Let's take a look at the first one. Let's look at a field of volcanos formed at about the same time. If we do it this way, I must point out that the handout that I gave you may not have the transparencies, and there might be some transparencies that are in there that I'm not showing, because I made some late last-minute changes. If you're searching for a transparency, it might not be in there.

A field of volcanos formed at about the same time, we would have three major events in the Yucca Mountain area; the Lathrop Wells cone, about 100,000 years old, even though there's a range of ages that have been proposed; 1.1, or about one million years for the volcanos in Crater Flat; then the older 3.7 million-year-old lavas in the southeast part of Crater Flat would be a third event.

So, looking at a map which I borrowed from one of Chuck Connor's presentations, looking at the Yucca Mountain area, the repository site right there, we would have the Lathrop Wells, call that one event; the 3.7-year-old lavas in Crater Flat, call that Event No. 2; and then the one million-year-old Crater Flat volcanos that formed this northeast trending chain as a third event, so this is one possible way of defining the number of volcanic events in the Yucca Mountain area.

If you look at another possible way, eruptions separated by a significant period of time, looking at Crater Flat itself--this is an area I'm most familiar with--Red Cone would be two events and Black Cone would be two events, so the Red Cone/Black Cone pair would represent a total of four events.

Let me just show you a geologic map of Red Cone, to try to demonstrate the type of information that we're using. At Red Cone, which is in the central part of Crater Flat, there are two major ages of lavas. Here's the main cone itself. The black color represents the younger lavas that erupted from a series of small scoria mounds that are shown by the triangles. These overlie with a fairly good unconformity eroded scoria mounds and older lava present mainly through the south of the Red Cone itself, and this contact represents an unconformity that may represent a significant period of time lapse between the eruption of the older flows and the younger flows.

So, on the basis of time difference, there would be two major episodes here at Red Cone, one representing the eruption of these younger black flows, and the other representing the eruption of these older flows in the red and purple colors, and you can use the same type of arguments at Black Cone, and show that there are two major episodes, separated by a fairly significant period of time.

If you look at eruptions of chemically distinct magma batches, then, right now, my feeling is that Red Cone and Black Cone represent a total of two events, rather than four, as represented by the previous way of looking at things. Let me just show you some of this geochemistry.

I've plotted cerium versus strontium here. Black Cone data clusters very nicely in this area right here. This circle here represents the average Black Cone composition. Red Cone, which is only a short distance to the north--short distance to the south of Black Cone, shows a array of chemical data extending from a fairly enriched composition up in this area here, to the Black Cone chemistry. So, Black Cone tends to cluster; Red Cone produces a data array.

Our current thought is that looking at two independently-derived magma batches here, one representing the Black Cone type, the other representing a more-enriched Red Cone type, then we have mixing between the two magma types to produce the data array that we see at Red Cone.

The same difference is also seen in the isotopes. We'll just focus in on this top part of this figure, plotting here initial strontium ratios on the X axis, epsilon the odimnium values on the Y axis. Black Cone, which is shown by the black squares, falls in this area right here. The more-enriched Red Cone composition falls up here, and we see a data array in Red Cone of isotopic values extending from an enriched Red Cone-type, to the more depleted Black Cone-type, so we see the same two magma batches, one here, one here, with possible mixing between the two in the isotopes.

So, what does this mean? Well, we can produce sort of a cartoon model. We feel that there was independent partial melting in the mantle to produce those two batches; one being the Black Cone magma-type, the other showing green, which is the Red Cone magma-type; that the initial eruption at Red Cone, forming the older eruptions at Red Cone, the initial eruptions at Black Cone, or the Black Cone magma-type.

So, we feel that we probably had coeval eruption at Red Cone and Black Cone early in the history of these two volcanos, that we had a linkage of the magma system between Red Cone and Black Cone, and this one event produced eruptions in two places along this northeast trending zone.

This is then followed by a mixing someplace in the crust, probably fairly low in the crust--this is probably a bit too high--between the Red Cone and Black Cone magma-types, producing the data array shown by the orange and the green colors. So, we feel like we have two histories going on here. Black Cone is relatively simple, all Black Cone-type. Red Cone is this mixture of Black Cone and Red Cone-type, but we're looking at two major events here, one Black Cone, one Red Cone magma-type, so this is another way of defining a volcanic event.

Now, if we simply count vents--and I have to go back and get a slide here that I used before--if we simply count vents, then at Red Cone, we might come up with a total of 14 events, if we simply count the number of vents that we see at Red Cone, and just to remind you of what the geology is like, what I'm doing here is counting each one of these individual scoria mounds which represents a separate eruption of magma as a separate vent or a separate event, and there'll be 14 vents, 14 events at Red Cone, a total of 28 in Crater Flat. This is another way of going about it.

And the last way of doing this is to simply count volcanic complexes. In this case, Red Cone would be one, Black Cone would be one. There'd be four events in Crater Flat, so all we're doing here is looking at a geologic map of Red Cone and Black Cone, simply saying that the formation of this entire volcanic complex is one event, the formation of this entire volcanic complex is one event.

So, just looking at Red Cone, we could see that Red Cone may represent, depending on how we define the volcanic event, is anything from a fraction of a volcanic event, to 14. Now, if you were to ask me, well, which one do I like at the present time, which one do I think is the most reasonable, I think that my prejudice would put me someplace in this range right here. I like either the chemical model or the time-dependent model, so I would say there's somewhere between two and four events recorded by Red Cone and Black Cone.

So, what we have to try to do is, we have to try to do some work to try to provide a better definition of a volcanic event. We might be able to say, well, whether it's two or four, it's not going to make that much difference in the probability studies, but if the real answer is 14 and we have 28 events, then there's a big difference between 28 and 2. We have to find out which one of these definitions is the proper one to use.

I think the types of studies we have to do in order to better define a volcanic event, we have to be able to do more detailed geochemical studies of small volume volcanos. We have to have detailed geochronology. We not only have to date the entire complex, we have to have dates of each individual flow, and I think that the Argon dates that Frank Perry reported, I think the errors are--we're looking at errors of plus or minus 10,000 years. We might be able to see differences at Black Cone between individual flows.

We also have to do detailed mapping of these small volume systems, and as has been mentioned by several other people, we have to look at modern analogs.

Now, the second point, we have to try to determine what the area of concern for hazard assessment is. What is the area that might be affected by future eruptions? Bruce has mentioned, and has shown you today the Crater Flat zone. I, for a long period of time, have been pushing this Area of Most Recent Volcanism, which is a larger area, which includes the Buckboard Mesa center.

I think that, in a way, it doesn't really make, really, that much difference which one of these zones that you choose. It's really more important to try to identify specific zones of high volcanic hazards within these larger areas. This is the way that Dr. Ho has been trying to do the probability calculations. Instead of looking at the AMRV of the Crater Flat zone, we have to find specific targets that might be areas of higher volcanic hazard.

This volcanic risk rectangle concept that I proposed almost four years ago is one attempt at doing this. Just to refresh your memory as to what this is, it's really a very simple model where we considered the volcanos in Crater Flat and Lathrop Wells cones to be polycyclic and polygenetic. If this is the case, then the next eruption will be centered at one of the pre-existing centers; for example, there's a pretty good high probability that the next eruption will occur in or around the Lathrop Wells center.

I also considered the possibility that we had linkage of events; that one event might occur in more than one place, and that the control of the formation of a volcanic chain or a volcanic cluster was related to regional structure. So, these rectangles record not only the direction of the structures that are adjacent to these individual cones, but they also record the lengths of possible volcanic chains.

The smaller dimension rectangle is similar in dimensions to the Crater Flat chain; the larger dimension rectangle is sort of a worst case scenario of the largest possible volcanic chain that we've found in analog areas surrounding Yucca Mountain. So, we feel like this is an approach that has to be looked into in more detail, you know, how can we refine models like this by looking at geologic data?

Chuck Connor has provided some insight into this, and I think that we have to try to become a bit more sophisticated in the way that we define or locate these target zones.

So, how can we do this? The definition of hazard zones must take into account the structures that control volcanism. I don't think there's that much agreement as to whether or not volcanic rocks rising to the upper crust following existing structures, or whether they are able to move through rock without having any previous fractures or faults. What about the formation of volcanic chains? We have to take a look into the dimensions of these chains. We have to understand how these chains form, and we have to understand how this can be applied to the Yucca Mountain area.

And, we also have to realize that a single volcanic event may occur at more than one location. We have to take into account this linkage effect.

So, future research, then, we have to know how magma rises through the upper crust. We also have to understand how important are faults. We also have to try to quantify how important topography is. Bruce has mentioned several times, in fact, he mentioned today that volcanos normally occur in basins. This may, in fact, be true, but we have to determine whether or not that occurs, let's say, 60 per cent of the time, 80 per cent of the time, 90 per cent of the time.

We've done work, for example, in the Reveille Range to the north of Crater Flat where we've been able to show that 10 to 15 per cent of the volcanos actually occur within the range, and several actually occur at the summits of the range, so we have to be able to quantify this. We simply can't say that volcanos preferentially form in basins. We have to have some idea of, if this is true, why this occurs. We have to try to quantify this.

We also have to know a bit more about the dimensions of volcanic chains, so there's still a lot we have to know before we can locate these target zones within either the AMRV at the Crater Flat zone.

Now, in terms of consequence of eruption, I think it's important to realize that cinder cone eruptions can be quite explosive. The traditional view is that cinder cones are erupted by either Hawaiian or Strombolian types of eruption. That's the major sorts of eruptions that are responsible. However, it's beginning to become apparent that many cinder cones can erupt with plinian or subplinian eruptive styles.

For example, in 1975 and 1976, Tolbachik volcano in Kamchatka, which is a cinder cone, erupted in a very explosive manner, and I have a couple slides just to show you--if I can figure out how to turn this off--just to very quickly show you what the nature of this eruption was. I'm not going to go into any great detail.

This is the eruption of Tolbachik, and I'm not sure exactly when this particular eruption occurred, but the purpose of this is just to show you the very high explosivity of this eruption. You're looking at a plinian or subplinian cloud, a lot of turbulent convection in the cloud, and this would have very profound effects on the dispersal of waste if this sort of eruption were to occur through the repository.

And the next slide shows another example. Here's the crater rim right here, and, again, you can see the high degree of convective overturn in the column, a very highly explosive eruption. This is very definitely not a Hawaiian or a Strombolian sort of eruption. We have to try to determine whether or not eruptions like this occurred in the Crater Flat area, occurred at Lathrop Wells.

How can we go about evaluating this? We have to try to determine the explosivity of an eruption. The volatile contents, especially water, is a good indication of the explosivity.

One way of looking at this is to study or analyze melt inclusions, which are quenched samples of magma and volatile phases, and represent, basically, the chemistry and the volatile contents of the magma at the time of the eruption. These melt inclusions are trapped within all of the phenocrysts, not only in Crater Flat, also Lathrop Wells, and they're also present in a wide variety of tectonic settings, and I think the analysis of these melt inclusions will provide us some indication as to the water contents of the magmas at the time of their eruption.

Now, a recent paper in EOS by Sobolev and others, where they've looked at water contents and melt inclusions and olivines from a variety of different tectonic environments. The MORB, both enriched and normal MORB show relatively low water contents, and these are normally associated, this type of rock is normally associated with the relatively quiet eruptions.

Melt inclusions from salts, prime magnesium salts in subduction zones, which can be associated with more violent eruptions, normally have higher water content, so one per cent or greater. The Tolbachik eruption is a subduction zone-related eruption, and most probably has water contents in this particular range.

So, we have at least some basis for what water contents are in various environments, and the basic plan is to look at melt inclusions at Crater Flat and Lathrop Wells, compare that data with data from volcanic centers with known eruptive type, and then similar volatile contents would be an indication, but, of course, not proof of a similar eruptive mechanism. You would have to support this with geologic data in order to demonstrate this, but I think we have a way of getting at, or at least of beginning to get at the water contents and the explosivity of the eruptions.

So, in summary, then, there's still, in my mind, anyway, important data required for hazard assessment studies that we haven't gotten yet, we don't really understand. We don't really understand how to define a volcanic event. There's still a lot of debate as to what area represents the target for the next eruption. Is it the risk rectangles? Should we look at the AMRV? Should we look at the Crater Flat zone?

Also, cinder cones may erupt by a more explosive type of mechanism, plinian or subplinian, and this has very important consequences for consequence analysis.

And I guess something that I'd like to leave you with is that, in my mind, at least my opinion is that we really have to understand the process of volcanism in a better way. We have to try to factor the geology into these models in a much more efficient way before we can really have confidence in any of the statistical models. I think that we're fooling ourselves if we simply look at the numbers and don't try to factor what we see in the field into our models, and I'm not going to be confident in these numbers that are coming out until I see more integration of the geology into the modeling that we're doing.

Now, that's all I have to say. I'm not sure, Clarence, do you want to--

DR. ALLEN: Okay, thank you, Gene. Why don't we go on with Dr. Ho, and then we can take questions for both of you later, if that's okay.

DR. SMITH: Okay, sure.

DR. ALLEN: Dr. C.H. Ho, also of the University of Nevada at Las Vegas.

DR. HO: It's about lunch time, and we have been bombed by--almost for two days--by probabilities, probabilities and probabilities. Enough is enough. I believe that if you are doing the same thing for so many years, you probably get tired of that, and then, certainly, this is a legitimate question. And, at this moment--by the way, I forgot to ask you, how are you? What is the probability that you are flying today?

I believe that it's time to take a quiz at this moment, and, especially, I would like to ask Dr. Leon Reiter about what's the probability that you will pass the following test?

So, here is the first question for the quiz. The most important questions of life are, for the most part, really only what?

(Inaudible response.)

DR. HO: You are right, problems of probabilities; right. This is the trademark of a well-known mathematician, Laplace. He was, or maybe is -- or friends.

My second question is: Now, ask not, it's so? Ask, what?

(Inaudible response.)

DR. HO: You are right. What is the probability that it is so? So, this is the trademark of JFK, okay, or we can form it in this way: Ask questions deterministically, not direct, but ask probabilistically. This is my second question.

Now, again, my third question for the quiz is if I randomly selected one of you standing here, and ask all of you to predict what is probability that this person will ask a question in a deterministical way for the remaining of his life, that's the question. That's a problem. That will have answer. But, what if I ask you, this person is Laplace. What's your answer? The probability that he asks the question in this way is about 10^{-8}. Agree? And any of you certainly will be much higher than this guy. So, knowledge of the historic data help you to answer the question in a very rational way. Okay, so that's the test; that's quiz.

Now, I give you more example about the problem of probabilities here. What I'm going to do, and present a case study today, is the following: To quantify the possibility of direct disruption of the repository by the basaltic volcanism. I highlight "direct," because this talk is for direct only, not indirect, and I have a typo there, because I'm thinking about the money you're going to spend on the repository, so I put S there. It's not correct in your handout there.

The related issues are, as highlighted by Dr. Smith, are: Modeling assumptions, eruptive history of basaltic volcanism, and structural controls on basaltic volcanic activity, and then the counts of volcanic events, but these are not the only issues that we have problem with now, but that's a major issue that relate to the talk I'm going to present today.

The approach of basic models I'm going to present here are chopped into two parts; past, and then future. Using simple Poisson model for both past and future events, we call HPP throughout the whole talk. And, similarly, for past, if we use Weibull process and future for the simple Poisson, I denote as WP-HPP, and then Weibull process throughout, I use WP.

Now, let me give you some background about a homogeneous Poisson process. Homogeneous Poisson processes do not reflect and do not consider the time trend provided by the times or the events throughout the observation time, and the Weibull process, which is widely used in reliability modeling, reflects the time trend provided by the process, either increasing or decreasing, and also, this model includes the simple Poisson process, so that's a generalization of a simple Poisson model. It does not exclude a simple Poisson model.

The duty of simple Poisson is just based on the name, it's simple, but, overlooks the time trend, which is the most important thing in modeling the volcanism at Yucca Mountain. So, this shouldn't be ignored.

And then Weibull/Poisson is generalizing simple Poisson, but not a very complicated model, so simplicity is there and predictability is there, so I chose the Weibull process at this moment for the modeling.

One of the important parameters is the probability of repository disruption. What is the probability of site disruption? This is given observed data of volcanic eruption. What is the chance that this eruption will directly hit the repository? That is defined as p. That is prime in modeling the risk, and Bayesian, a table generated by Bruce Crowe and others in 1993, it ranged from 1.1 x 10^{-3} to 8 x 10^{-2}, and the maximum and the minimum. I use these two to do simple analyses.

The approach for the p are, using these two values, maximum and minimum, and calculating the fixed value, and we call it classic approach. That means for every volcano anywhere in any area, you assume that probability of site eruption is a constant value. It's fixed for any one of you, for every one of the volcano. The top one is a minimum one generated by that table, and then the 8 x 10^{-2} is the maximum, and the approach that are proposed here, the Bayesian approach, p is a parameter, is a probability.

We quantify that probability by another probability, probabilistic distribution, which I used here as uniform, 0 to 8/75. I will explain that.

Now, based on this picture here, you have volcanos scattered around among the area called AMRV or some other names here, and now, let's look at here. The current approach produced by Dr. Crowe is that assume that, okay, take the area, either this one, or somewhere here, and the repository's somewhere here, and take a ratio, and using this one as a fixed value, but that fixed value is highly sensitive to the area you pick.

Or, people use simulations to say, okay, this is the target, let's simulate it and see how many out of how many hit the target, then take a ratio. Using that approach, you have to assume a model, too. This is called simulation. If you run a analytic approach, you use this simulation here, but either one use a fixed p, but in my case, I say, okay, p, now, look at here, if this is the future eruption here, the probability that that one will hit the repository is what? Zero. It's zero, so if they use 1.1 x 10^{-2} that's too high, that's too high.

On the other hand, if that one, that future eruption is here, and the chance is what? One, right on the target. There is a volcano underneath the repository. That's a probability of one, so the possible values for p run from zero to one. If you don't have any knowledge about a structural process of volcanos, where you use a non-homogeneous prior, saying that, okay, the probability is one and zero to one, I don't disclaim anything between zero and one, and take any value between zero and one, and use the uniform zero/one prior.

But, if you incorporate the studies, the result of Dr. Smith and others result, concentrate on a small scale of volcano eruptive process, saying that, okay, if this is here, then it's zero, but the maximum, the highest probability, Lathrop Wells is here, so let's try. What's the highest probability that somewhere here will hit the repository?

DR. ALLEN: Dr. Ho, please remember, you only have about four minutes left.

DR. HO: Okay, yes, so I can start any time.

Okay, so based on that idea, we use A as the area for upper half rectangle, and small a is what? The area for repository, and take a ratio based on that scale, and saying this is the upper limit, and then still use the uniform distribution for that, and use another prior for the Bayesian approach, and then embed that distribution into the probabilistic calculation, and then, eventually that will be averaged out. It's different from the approach that you pick one here, pick one there, pick one there, and then later on take a median, or take an average. So this is a diffusion and then average out all math into the model calculations.

So, overall approach is model--three models, two approaches, and then parameters, p, p, and then uniform zero to that. So this is a summary for the models.

How about the data here? We don't have a great information about number of events. Therefore, let's try a list of class of volcanos, different counts within each volcano, so let's try to say, okay, Thirsty Mesa can be one to three events; Crater Flat, four to six events, and so on, and consider the minimum up to maximum, unless the number of counts varies and you generate so many, and then try any one and see whether it matters or not.

And in doing so, you have to also say what you are observing in time. Your data have to be accumulated up to what time? Post-six million years, or just the Quaternary? So I do both. And we have 90 data sets for the first one, and six data sets for second one.

I define risk in this way, to simplify the explanation here: Probability of at least one disruptive event over the next 10,000 years. Ten thousand is t_0, and those are theoretical equations. It's not too difficult to understand.

So, where's the beef here? How models and data affect the calculation of volcanic risk? Is the difference significant? How important is it related to future work?

So, recurrence rates are higher, based on the Quaternary data, because we have longer lengths, and then not proportionally a low number of events. The recurrence rates produced by WP are higher than those HPP, showing that the trend is increasing, which is not the same as produced by other people here, specifically here.

Now, let's see. How do we interpret this result here? The recurrence rate based on different data and different model is summarized into this two columns of data here. Now, the effects, the roles that the data play is reflect by this row here. The effects that model play is reflect by this column here. We see there about 42 per cent of change produced by a different data set, about 32 per cent of change produced by different models that we chose, but those two you see are very similar, and if you want to see the effect of both of them, you see the diagonal, but all of them have the same order of magnitude based on the recurrence rate.

DR. ALLEN: Could you begin to wrap up, please?

DR. HO: Sure.

So, the kind of approach using that one, either the lowest one and highest one for the risk, and the Bayesian approach is in between, so this is the overall picture here, and you interpret exactly same way.

So, I will fly over everything here, and then here, even though you have small data set, you can see the trend, and the model pick up the trend nicely, even based on five data points. Decreasing trend, simple homogeneous Poisson, and then increasing trend, based on only five data set, five data points, so it can be done. Is this change significant?

I'll give one example here, Weibull model, same prior. If I include two data set, this one reflecting that if there is a highly polycyclic volcanism for Lathrop Wells, I include, supposedly, two. Two additional one would change the probability by 43 per cent. Two more additional data could increase 43 per cent.

So, the major results here is overall probability run from here to there, automatically to two. If you increase time to 10,000, you increase the probability from 0.02 per cent to 6.57 per cent for a range of that time.

Then how do you answer this question? When is enough is enough? A logical question to be answered. For example, are those two probabilities both acceptable? Is the difference significant? If someone came to me and said, I don't understand this two probability. What do we mean by .02 and 6.57 per cent? You can compare it to the rate for your income tax, you understand.

And over here, again, I use 0,8 over 75 for the prior, which is now former to prior, but we try and say, okay, we don't take care of the geology. Let's assume that a uniform 0,1. Fine. Use uniform 0,1, and then your probability calculation would change 816 per cent.

People have to answer this question: Which one is the right one? If you don't believe something, which makes sense?

DR. ALLEN: We've simply got to finish up here very quickly.

DR. HO: Two more, please, yeah. See, my mind is like a parachute, that it work best when it's open.

If you had to choose the approach, you also have a lot of problems. You have to dilute the effect, compounded by among those three; power, mystery, and the volcano. We have plenty of examples showing that what we are doing is deadly science. Volcanology is tragically imprecise.

I thank you for your patience.

DR. ALLEN: Okay. Thank you, Dr. Ho.

We're into the lunch period here, but let me just ask if there are any quick comments or questions by our people, particularly our consultants?

DR. SHERIDAN: One question that I might resolve, because it appears that a very important element of this presentation was the identification of the high-risk zone, which has a particular geometry which seems to be highly deterministic.

I would like you to tell me, what is the probability that the high risks that the geometry and location of the high-risk zone is correct?

DR. SMITH: All I can say is that, geologically, it makes considerable sense. I can't give you a number, saying that it's correct to a certain--it's 60 per cent correct or 80 per cent correct, but it makes sense in terms of the structure, makes sense in terms of the links of volcanic chains, not only in the Crater Flat area, but in adjacent areas. I think that we've sort of placed the bounds on the longest possible chains and reasonable chains in the area.

We know that these volcanos produce chains. We know they produce clusters, and I think that the fact that Crater Flat volcanos are oriented in a northeast direction, and that we've shown that there currently is linkage between Red Cone and Black Cone, and we had two events occurring in a northeast orientation, two events occurring at the same time, oriented with respect to one another, oriented in a northeast direction, I think that, geologically, it makes sense. I can't answer your question about probability.

DR. SHERIDAN: Let me just explain why this is important, because if your angle of this trend were changed by just five degrees or ten degrees--I don't know what it would be--it would mean that your high-risk zone would exactly miss the repository, and your calculations would show probabilities that are extremely low.

DR. SMITH: I try to be as conservative as possible in terms of choosing the dimensions of those zones.

DR. SHERIDAN: I mean the angle of the zone, rather than--starting from Lathrop--

DR. SMITH: Yeah. The angle of the zone is based on the orientations of faults that are adjacent to the volcano. I mean, you can quibble about the angle if you wish, but the width of the zone is also critical. I mean, if you change the angle by one degree and make the zone a little bit wider, then it won't, you know, the zones are based on analogs, analog studies, and you can quibble about whether they should be a kilometer wide or kilometer point two wide or what. I try to do my best to come up with a conservative estimate for the size of the zone, and you can disagree, saying, you know, that the angle should be one degree to the east or one degree to the west. I can't argue with that.

DR. SHERIDAN: What I'm really saying is that you should indicate what the sensitivity or range of these values would be, and how they would affect your probabilities. I think it would increase the strength of your argument.

DR. ALLEN: Okay, any other--excuse me.

DR. HO: I have a comment on Dr. Sheridan's question here. This is very good question here. This is a point that, for example, those qualified geologists have to answer, whether we should direct it in this way or that way. So far, all I see is that way. If we have a convincing case, we'll probably go that way. If we don't have anything, we will pick the non-informant prior, saying it's zero to one, and nobody will say this is wrong, because that's a legitimate value for everything, and then the probability will go 800 and something higher per cent; higher.

So, it's indication of more fissure work is required, geologically speaking. The tool of statistics and probability approach are ready, but are volcanology ready for this kind of tool to go incorporate into the probability calculations?

DR. ALLEN: Okay, thank you. I think we just have to call it quits, partly because we do not have the opportunity this afternoon to go beyond four o'clock. We must cut it off at four o'clock because many of our people are leaving, so thank you, Dr. Ho, and thank you, Dr. Smith.

We'll adjourn for lunch, and we'll reconvene in an hour and five minutes, at one-fifteen.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. ALLEN: Can we get going on the afternoon session, please?

Okay. Our first speaker this afternoon is Peter Wallmann, who's a senior geologist with Golder Associates in Redmond, Washington, and he'll be presenting material on sensitivity studies on volcanic hazard at Yucca Mountain.

Peter?

DR. WALLMANN: Thank you. I'd like to thank you for this opportunity to speak with you today, and one of the things that I think I want to try to get across first, is I actually feel, as a fairly independent person here--I work for neither DOE nor Los Alamos nor the NRC nor the Center.

DR. ALLEN: Why are you here?

(Laughter.)

DR. WALLMANN: Because I guess I've developed connections somewhere along the line. Unfortunately for my company, I'm working for no one on this project at the moment, and so, the viewpoints that I've arrived at, I think, have really been independent of the other groups. Now, they may concur with some of those in the other groups, but I think it's really pretty much an independent--they've been arrived at independently.

Now, you've heard a lot about volcanic disruptive events. We have: How frequently does an event occur, which has been given the designation of E1, the event rate, number of events per year. Does an event disrupt the repository? The disruption probability. Given an event, how frequently will it disrupt the repository? The disruptive event can rather, in a very simple-minded way, simply be taken as a product of that. Given this disruptive event rate, then, one must consider the consequences of disruption of the repository.

Now, on the left here is a schematic map taken from the status report, with three different "structural models" displayed on it, as well as the repository. You have a region which will be used as the simulation region for the Crater Flats volcanic zone, simulation region extending off the chart an equal distance to the southwest as it does to the northeast, which I think is an extremely important point of what I have called the Lathrop Wells rectangle proposed by Gene Smith and his co-authors, and the Area of Most Recent Volcanism, also proposed by Gene Smith and his co-authors.

Now, what I'm going to speak about today is simply the disruptive probability. At least, those are the results that I will present, but I will also show how those impact on--how the choice of one of these models has an impact, or should have an impact on your E1 calculations.

Consequently, I'm going to put up my conclusions first, not because I'm going to finish, but just so--these are some of the conclusions that I would draw.

One of them we will see is that the sensitivity of disruptive probability is dependent on the model that is chosen. Now, if we think about this in a very logical manner, this is not very surprising. Two of these models actually contain the repository block in the model region within which future events will occur. One of the regions does not contain the repository block. It seems very logical to assume that this Crater Flat volcanic zone will behave very differently in a parametric sense than the other two regions.

Second, I will show some results of some simple clustering models, and contend that clustering, in and of itself, does not increase disruptive probability.

The final, and perhaps most important conclusion of this portion of the talk is that the event rate--I say here it is essential--I would say must be recalculated based on which model you choose. You can't have your cake and eat it, too. You can't calculate a high event rate based on a large region, and then stuff that event rate into a small rectangle.

What I've done here, I've used a discrete fracture generation program called FracMan, which was developed by Golder Associates for doing discrete feature or fracture analysis and generation. It has been used, and successfully used, in fluid flow, fracture flow modeling at the Stripa and Aspo sites in Sweden, at the Kamaishi Mine test facility in Japan, and it has been used some at Yucca Mountain, as it relates to fractures and fracture networks.

The program contains multiple distributions for fracture radius and orientation--that's very general--and multiple models for spatial distribution of fracture centers.

For the model simulations, here are some of the important points: Because of the way that the model generates fractures, and, also, for a mechanistic reason which I will get to, each point that is selected as a point for generating a fracture, which I call the initiation point, each fracture generated from that point represents two dikes propagating in opposite directions.

When we look at stress fields and fracture fault orientations, there is a good body of knowledge which indicates that dikes will tend to propagate as opening mode fractures along, parallel to the greatest compressive stress, but, the trick is, we don't--well, so while we know the strength in which a dike will propagate, we may not know the trend in which it will propagate. Will it propagate to the northeast, in this direction, or will it propagate to the southwest? So, in my models, for each point, I have a dike propagating, in essence, each direction, so that I remove what I feel is a potential source of bias, in biasing all the features to propagate toward the repository or in a certain direction.

At this point, I have simply used distribution models, which lead to a Poisson distribution of the initiation points. This can be overcome. There are other models I could choose from, and for the cluster models, I did choose one which leads to clustering. But, for this set of models, I did use a Poisson distribution.

Then, for each set of model parameters, ten realizations of 10,000 fractures simulating 200,000 dikes were performed, and we sampled each of these networks to see how many features intersected the repository block.

By the way, I should point out, this is a three-dimensional model, it is not two-dimensional. Each of these model regions, each of these simulation regions extends down three kilometers, so there is a volume within which these dikes can be thought of as initiating and propagating. It is not a mechanistic model. I do not mechanistically model the propagation of the dike.

What I show here are just what I refer to as my base case parameters. For dike orientation, the mean pole, or the normal to the flame is 110 degrees and 0, so that my planes have a strike, if I can do 20 degrees, of about this orient, a mean strike, for the base case, of about this orientation.

There is a standard deviation of trend of 20 degrees, and plunge of 10 degrees. You will some results vary in these parameters, and see how sensitive the different structural models are to varying these parameters.

Dike length, remembering that this is, then, the total length of one of these fractures, or twice the dike length, is a uniform distribution with a mean of 7500 meters, and a maximum deviation of 6500. This would lead to features which would range uniformly from 500 meters to 7 kilometers.

As the dike height--remember, it's how high is my feature--for the base case, the mean is 1500 meters, and the maximum deviation is 500 meters.

DR. ALLEN: Excuse me. Where did this 110 degrees come from?

DR. WALLMANN: It's based more or less on what I could find on the regional--on the stresses at the site, that the maximum compression is about 110. It is varied, and you'll see the--it is a parameter which is varied throughout the study and you can see the variance, so, if you like, a strike of north 10 degrees east, you will see the results for that, also.

I would also, I guess I would say these are, in a sense, have I covered all the bases? No. Could these things be revised? Yes, they could, but I think we'll see, from the sensitivity of certain models to some parameters, that they're not all that sensitive.

Just so people can get a bit of a feeling for the orientation distribution, this is the Bivariate normal distribution for the base case, so here is our mean, and standard deviation in the trend of 20 degrees, and 10 degrees in the plunge, so there can be a fair amount of variance there, but the major locations will be here, and varying the standard deviation for both trend and plunge is also done.

Looking at some results for the Area of Most Recent Volcanism, now, for each of these cases, I have normalized, for display purposes, to the base case for each model. So, the Area of Most Recent Volcanism, the Crater Flat volcanic zone, and the Lathrop Wells rectangle are normalized to their own base case, and this is so that, in displaying the results, we're sort of comparing apples to apples and oranges to oranges.

What we see, one, is there's not a whole lot of variance from about .8, a little above .8, to less than 1.1. Oh, I should have--excuse me--pointed out that this is for varying the strike standard deviation, so here, our base case is this model right here, with a mean strike of north 20 degrees east, and a standard deviation of that strike of 20 degrees. This would be for a constant orientation, and north 10 degrees, 20, 30, and 40.

And what we see from the Area of Most Recent

Volcanism, generally speaking, increasing the mean, or the strike, the orientation, leads to a subtle increase for each of the standard deviations. Increasing the standard deviation in one case leads to a decrease in the disruptive probability; in another case here, leads to an increase.

If we look at the Lathrop Wells rectangle model, we see somewhat different results. One thing that is the same is for the mean for sort of the base case strike of the features, we get a decrease for increasing standard deviation, but in each of these models, as we vary the mean strike, we tend to peak out somewhere right here, around north 20 degrees east for each of the strike standard deviations. And notice, again, too, that we don't have big variations from about .8 to about 1.1 in the normalized disruptive probability.

If we look at the Crater Flat volcanic zone model, here we see some very significant variability in the results, from less than .4, to almost 1.5, depending on the orientation, the mean orientation, and even as we increase the strike--and as we increase the strike standard deviation for the north case, we get a rather large increase in the probability of disruption, a decrease for north 20, and a decrease for north 40.

If we look at the standard deviation, we see that we increase the probability for any of the standard deviations as we move the mean strike more and more to the east.

Now, to display these all together, the different models, you have to kind of look at each line separately. So, here, if we look at the strike variation, where the triangles are the Area of Most Recent Volcanism, the diamonds are the Lathrop Wells rectangle, and the Crater Flat volcanic zone are the squares, really, the largest variation is for the Crater Flat volcanic zone, but would we say this is a really significant parameter?

The variation in result is only from about .95 or so for the Crater Flat volcanic zone on the low end, to about 1.15. The variation in dip is even less significant. Basically, changing the dip leads to very small changes in the normalized disruptive probability.

And, I had to make a bit of a change. As I was looking at this, I discovered that the wrong row got plotted here. The Lathrop Wells rectangle should be this other line drawn in here, and now, when we look at the mean POLE variation, we see this very large change.

I should say--excuse me, I didn't point this out--for each of these plots, the only parameter which is varied is the one which is being plotted, so everything else is the base case; length, height, in this case, standard deviation of the trend and plunge of the POLE, or strike and dip. So, the only parameter which is varied here is the mean POLE, and I think we would go ahead and say in this case that for the Crater Flat volcanic zone, the mean POLE for a dike distribution is a very significant parameter, probably the first one which we've seen so far which is significant for any of the different models.

Height variation, they almost plot right on top of each other, and I suspect that a very good argument would be made that these are all within the statistical significance. This is just one line. Basically, the effect we see here is that of fracture intensity.

As you increase the intensity of features within the region, you will increase the probability of intercepting any feature, and if intensity is considered as the area of dikes or fractures to the volume of the region, as we increase the mean in this uniform distribution for height, as we increase the mean for a set number of fractures, we will increase the intensity, the area of features. So, this is to be very much expected, that height should lead to an increase in the disruptive probability for all of the models.

Length, we see it's still important, but it's, again, more important for the Crater Flat volcanic zone, the one which does not contain the repository within the simulation region, than for the other models.

I'm going to skip over the distribution variation, which simply says that choosing different distributional models for your dike length can lead to different results, and that may be a--that is possibly a significant parameter, although one which would be very difficult to obtain.

These are the results of a number for the Area of Most Recent Volcanism, number of models, one with a Poissonian distribution, and then models where this number here, .25, .5, refers to a coefficient in a clustering model, where, as you increase the coefficient, you increase the degree of clustering, and we see some variation in the means.

I've talked with other people in the office about this apparent cycle here, and we aren't quite sure what to make of that, but what I feel is the important thing is to look at the range which we see here. The range for all of these, basically, the range of results for any given model covers the mean of the other models.

Another set of simulations, with a slightly different set of parameters, some the same, some different, and a smaller number of simulations shows very much the same thing, that we do get--perhaps the mean in these cluster models is elevated here from just over 5 to about 7, but the range of results more than spans the range of all of the means.

I believe this suggests that if you don't have a mechanistic model for clustering near an area, near the repository, if you just allow clusters to occur wherever they may occur in space, that clustering, in and of itself, does not lead to an increase in the disruptive probability.

I think, intuitively, we can see that if we allow clusters to occur up near Sleeping Butte. The best thing for the program would be to prove that volcanos will cluster here. If they cluster up here, we're not going to worry about their effect down here. So, lacking a mechanistic model for the distribution of the clusters, I think clustering, in and of itself, will not lead to an increase in the disruptive probability.

Now, I'm going to show the range of all the results on both a log scale and an arithmetic scale, so that no one can accuse me of wanting to skew my distributions wherever they may. We all know what fun games one can play with log scales.

And, here you see the range, Area of Most Recent Volcanism, Lathrop Wells rectangle, Crater Flat volcanic zone, and these are now the absolutes, probability of disruption; given a volcanic event, the probability of disruption in the repository, and the range of results taken from the table, the appropriate table in the status report, the same on both sides.

Now, we look at this, and we say, "Wow, the Lathrop Wells rectangle leads to a much greater probability of disruption." Again, I propose that that's intuitively obvious. It has the smallest area to be considered. That's what's going to happen.

But, what I'd like to point out is that if we choose this as our structural model, we have left out quite a number of points to consider in the rate calculation. Over the last 3.7 million years, based on one of the tables in the status report, and on a larger version of a map which Bruce sent to me, I find there are one, maybe two events in this rectangle within that time period, as opposed to up to 20 in the entire region.

If you want to choose this for your structural model, I do not believe that you can calculate the event rate based on all of the other events, and what happens if we re-calculate E1, based on the structural model. And, again, I show these both for--

DR. ALLEN: You have two more minutes, please.

DR. WALLMANN: Okay. I show these for both arithmetic scale and log scale. On the bottom, we have the results if you do not re-calculate E1. On the top, in the fields, what happens if you do re-calculate E1, and for these calculations, I just chose a very simple--I took the number of events over the time, just to get a reasonable estimate of the mean rate. I did not want to enter into the homogeneous versus non-homogeneous issue for this, just to display a point.

The point is, that for both the Crater Flat volcanic zone and the Area of Most Recent Volcanism, you don't really --you don't shift the curves too much. They do shift down, but if you look at the Lathrop Wells rectangle, where it had such a large probability of disruption, if we are consistent, and then re-calculate the event rate based on that structural model, we see that there is a very large change in the disruptive probability, and that, in fact, rather than being the most disruptive, this display in the minimum, mean, and maximum ranges of the E2 values, which I calculated, we find that the Area of Most Recent Volcanism becomes the most disruptive.

And, since there has been a lot of talk about the effect of volcanism, I would just like to show people that calculations such as this can and have been done for a total system model for Yucca Mountain. Is the consequence model perfect? No, it's not perfect. It could certainly stand additional scrutiny and more work.

On the left, we see there are no volcanic events in this block, and there are 47 out of the 500 realizations had volcanic events in this block, and if you're looking at them trying to figure out if they're different, I'll help you. They are plotted on the same scale, and one has to do this very precisely; otherwise, you can mislead yourself, and you can see that they are functionally the same.

Is this the end of the question? No. But I think it shows that we have the capability of doing total system performance models, which include, in any way we would like to describe, the volcanism. These also include seismic events. This could be updated to the most appropriate model, indirect effects, raising the water table, increasing the hydrologic gradient. All these things can be factored in, and, to date, with the limited simulations which we have done at Golder, we have not seen either of these seismic or volcanic to be a significant perturbation of the system.

DR. ALLEN: Thank you, Peter.

Do we have comments or questions from the Board or from consultants, or from staff?

DR. REITER: Peter, I think that you made a very wise point about that if you're changing the zone, you have to look at E1, but, similarly, is it appropriate to use the same time period? In other words, if you look only at--as far as I can understand--3.7 million years covers the area of events, the whole area, so by just looking at, let's say, the last million years, or the last 100,000 years, that might cause the probability to go up again.

DR. WALLMANN: It certainly would. I mean, that's--

DR. REITER: So how do you choose which is the proper period to look at?

DR. WALLMANN: No, I can't fall back on that, but it really does. I don't know who it was that said that, but it does, I think, come down to some sort of decision on an expert's part. I mean, that is a classic example of why some sort of convergence needs to be achieved.

If DOE is going to choose 3.7 million years, and NRC says, no, it's one million, and neither budges, then you're at an impasse, unless you can show--and some of the simulations we have done, we had disruptive probabilities of 2.5×10^{-6}. That is an order of magnitude greater than those proposed by DOE, and we, again, saw--we had a range they could vary between about 1×10^{-8} to 2.5×10^{-6}. We saw no effect.

So, I am not convinced that, even if you increase that event rate, that it will impact significantly the total performance of the repository. I think the key here is what are reasonable consequences. That could be a very difficult question to answer, but raising the event, in and of itself, does you very little, remembering that we have this disruptive probability, also. So if you add ten new events in the region, that doesn't mean that you have ten new intersections in the repository. It means that you maybe have to have 100 or 1,000 new events before you get an additional intersection in the repository.

DR. ALLEN: Other questions or comments?

(No audible response.)

DR. ALLEN: Okay. Thank you, Peter.

We'll go on to the final presentation before the round table, by Mike Sheridan, who is Chairman and Professor of Geology at State University of New York at Buffalo. He'll be making general comments on PVHA.

DR. SHERIDAN: I was asked by Leon Reiter to answer some specific questions which I felt were somewhat ponderous at the beginning of this task, but as I see the way we've progressed today, I think a lot of these questions have been either answered, or at least raised to individuals for consideration. So, I had felt that I might have to have taken very accurate notes and make comments on various presenters' presentations, but I'm not going to do that, and that's certainly going to save some time in this presentation.

The questions I would like to address are related, or at least, they're not even going to be questions. I'm going to discuss some aspects of geological perspectives on volcanic hazard assessment, probabilistic or not, and to present some basic elements for what we could call good probabilistic volcanic hazard assessment.

I'd like to give some examples of how volcanic hazard forecasting has been traditionally done in the geological literature, and maybe there's some surprises in that particular slide. We'll see; maybe not.

I was asked to comment on deterministic versus probabilistic methodologies, and I didn't know, really, what that meant when I read it, so I put down methods, and I think I probably won't even say anything about that issue.

And then, relevant to Yucca Mountain. Before coming to this meeting, I had listed a number of items I felt were important relative to Yucca Mountain. I've done some more thinking about that. During the presentation, I'll say something there, and probably, I won't really comment on work presented here, and leave that for the workshop session, in case the general discussion round table, in case there are some questions, or maybe there'll be questions from the floor related to that.

A lot of these items have come up in this afternoon regarding the geological perspectives that are relevant to probabilistic hazard forecasting, and the key questions--and some of these haven't been answered, by the way--what, is the first question. What are we trying to predict? What may happen? And that means the type of event to be expected, and I think there might be still some room for discussion and refinement of our vision of what might happen.

The question of when. Maybe this is this E1, this elusive E1 that's out there. I would again say it's a repose frequency, probably, or the time until the next expected event. That's another type of when. They're slightly different.

Where? A very important question. This is not an exciting volcano. I used my spell checker when I put this together, and it--

DR. ALLEN: That's the one in Kamchatka?

DR. SHERIDAN: That would be exciting, yes.

The size. A question that really hasn't been addressed at this meeting is the size of the expected events, or the probable size distribution of the events. It's something that is, I think, a very important issue and should be addressed, and then the anticipated effects or the vulnerability is a very important aspect to this particular case.

Let's take a look at some more aspects related to this geological perspective, and I think one of the questions--one important question is the mass eruptive rate, which would be equivalent to the energy release rate, since most of the energy of a volcanic eruption is thermal energy, and this depends on the temperature of the magma evolved, and an important aspect of constructing the geological component is to have some controls on the mass eruption rate, and the mass eruption rate may or may not be constant with time. If it's not constant, is it decreasing or increasing, and what do we expect? What is the size of the next probable batch of magma that may make it to the surface?

An important concept, also, is the survivor function, which is the probability that a repose has ended after a specific amount of time. It's a probability function for repose periods, and the age-specific eruptive rate, which would be the probability function for an event occurring, and then the spatial event predictors.

Now, it's possible to consider these functions--all of them are important--as independent functions, and, essentially, stochastic, or we could consider them to be interdependent to various degrees, and I think, as a matter of fact, that they are somewhat dependent; that the size of the next eruption at a volcano--quite characteristically, in many volcanos--depends on the repose period, and for those volcanos, that could be tied to specific models of magma generation, delivery to the surface, repose in a chamber and tectonic events that occur.

This type of conceptual model, I think, is something very important and something that still needs to be developed for the Yucca Mountain site, because we don't have a clear idea of where this magma is being generated, in what size batches, how this is moving towards the surface, how much of the magma freezes as it moves towards the surface, and, ultimately, what fraction of the magma generated comes out at the surface. These would be useful bits of information.

I want to define some of these terms, and I'm going to give you a different definition that was put together by Wickman in 1965. This is one of the first studies, probabilistic studies conducted on volcanos, and, at this time, Wickman used the data from a catalog of active volcanos. He looked at lots of volcanos. In fact, he looked at all of the volcanos for which there was a record, in which he could identify the event. And, actually, if you're concerned about the definition of event, take a look at his first paper in the series, because he does identify event quite specifically.

The survivor function can be identified in terms of event, in terms of these repose periods, and the probability of a repose larger than a certain time period, and I'll show you how he applied these. He actually was very practical and did graphical solutions to these, and the age-specific eruption rate is related to these events.

Now, before I get into using some of Wickman's examples, I wanted to get on to another question that was asked about elements of good PVHA. I think that Kevin Coppersmith actually covered this in great detail, and spelled out many of these items.

I think what is a very important aspect, from going back to the geological characterization, is to actually define the problem very carefully, and then to be able to test if the problem is defined in terms of some conceptual model, so to be able to test those various models, and as I was listening to presentations and these probabilities of 10^{-3}, 10^{-4}, 10^{-6}, very difficult, it's very difficult for the general public to be able to conceptualize these sorts of probabilities, and I think it's useful at some stage in defining this problem to break the probabilistic analysis down into components that can be categorized on a scale of one to ten, like the weather forecast; two chances out of ten that it's going to rain. That's something that people can understand, but if you say the probability of rain is 10^{-4}, it's very difficult for people to understand that.

And, to break the problem into these small elements, whereby even judgment decisions can be made, say, there's a 50/50 chance, I think those sorts of estimates are probably good at this level.

I have down here, set the limits of acceptability, and I think I put that down there because I was very much concerned that we don't really know what is acceptable or unacceptable, but now I think I would draw a line through set the limits of acceptability and say that's not really in the realm of the people who are developing the probabilistic analysis. We better take that one off, but that's in the realm of the policy makers and the decision makers to set these limits, so I think I would take that out.

And this identify the key processes, parameters, and the uncertainties associated with those, and I think, in these models, there must be multiple processes and sources of parameters. The parameters can be deterministic or probabilistic, and for, getting on to the next one, include all of the possibilities in the model, or maybe most of the possibilities. At least the model should be quite inclusive. I would say the basic elements of a good probabilistic model, it should be inclusive of many different ideas.

These processes should be arranged in this probabilistic model according to interdependencies. It would be useful to have feedback mechanisms in the PVHA so that we could take into account multiple events, or changing scenarios that would then have a feedback in the progression of probabilities through this type of system.

Of course, we should be able to perform sensitivity studies on the parameters, and many people have mentioned that today, and I think we've come a long way in this. I think that the analyses we've seen for Yucca Mountain, many of the presenters have shown the sensitivity studies related to their probability functions, which I think is something that's relatively new, and it's an important improvement.

And we should look at the interactive effects of all elements on the model, and what do I mean by all elements? Well, I think by all elements, I mean, also, the tectonic regime, and how do we expect the tectonic regime to change with time. What about the climate? Is there a relationship between climate and volcanism?

Well, I think if we're talking about hydrovolcanism, there may very well be an interrelationship of climate with volcanism, so we should pay attention to these factors. If, indeed, we go into another pluvial period, there will be more water around in the near surface that could interact with the volcano, making more explosive volcanism prominent, if not otherwise.

What about the relationship between seismicity and volcanism, or volcanism and seismicity? Is there some sort of linkage? Well, let's at least look at the interactive nature in these effects, determine the sensitivity, total model sensitivity to these sorts of parameters, and either include them or exclude them, or regroup them according to our needs.

Now, I want to present some examples of probability studies done on various volcanos, and I'm afraid that the volcanos I'm going to show are not small-volume basaltic fields, but they represent mainly central volcanos that have a long record, and one of the recommendations that Wickman used in his--one of the criteria, at least, that he used in his analysis was that there should be at least 60 events to be looked at.

And, in this case, he shows the number of events, and then this repose time on the horizontal axis, and he ranks these, re-sorts them, assumes they're stochastic, so that one event doesn't really depend on the preceding event, and ranks them in order, and then draws this line, which is the probability--it's not a probability function. It's a function showing the relationship of the number of--just essentially a histogram showing the number versus the time, but the slope of that gives this event a specific eruption rate.

And, for this volcano, there apparently are two slopes, a slope here and a slope below, and these show up as these two horizontal lines, indicating that the volcano seems to have two types of activity, and it has four relatively short repose periods. It has a fairly high eruption rate, and--has a higher eruption rate, let's put it that way, and for long repose periods, it has a shorter eruption rate.

Another way that we could look at volcanos is in the cumulative volume erupted, and this tells us something about the energy release at volcanos, and we're looking at a somewhat longer period of time. Perhaps you could see that the period of time Wickman was looking at was a 60-year period. Here, for Oshima Volcano--Oshima's just outside of Tokyo--the period from the year 2000 down to the year 500, it's about a 1500-year period of record at that volcano where volume estimates can be made, and it's a more or less linear trend. That volcano is erupting in essentially a steady-state condition over that period of time.

This is date from Wadge, and he's done a lot of work on probabilistic analysis, including a recent article that has just come out this year, showing spatial distribution and predicting probable eruptions in the future at Mount Etna.

Now, in this case, we're looking at Kaimon-Dake, another Japanese volcano, for which a record goes back some 4,000 years, and, in this case, we're getting closer to our 10,000-year time framework.

For Kaimon-Dake, the overall mass eruption rate for the volcano could be seen as a line, but there seem to be periods of activity, and other periods of relative quiet, and then a spurt of activity, and maybe quiet, or something intermediate, and for this type of volcano, it's possible to construct a Markov type of model, and the eruption rate could then be dependent on the state that the volcano is in, and you could list the condition that maybe it would go to another state.

Another person who has worked on volcanic hazard assessment is Scandone. He is at the Volcanological Observatory in Naples. He looked at a long-term volcanic hazard assessment for some very active volcanos in Mexico, and determined, say, the number of events per year on these volcanos, and, actually, Colima Volcano is the most active volcano in Mexico. It's considered to be the most dangerous volcano in Mexico. It's actually active right now, and it's a volcano that is close to my heart, and also to Chuck Connor's heart, and you can see what sort of rates, event rates we get for those kind of volcanos.

And, I think it's important for the Yucca Mountain study to examine this event rate for very active areas, and moderately active areas, and low active areas to get an idea of what is the global range of eruption rates, and there's data out there. I don't think it would take a lot of time to do that, but it would be worthwhile to incorporate those into considerations.

Scandone also looked at volcanic fields in regions, and he made some estimates of eruption rates in these areas. For example, for the Mexican volcanic belt, trans-Mexican volcanic belt, he made some estimates at rates, and, let's see, Chichinautzin is just above Unam in Mexico City. That erupted during historic times, or at least pre-conquest times, and a number here is 236 divided by--well, it looks like there's an extra zero there. That should be 700,000, and what this is, over a period of 700,000 years, nearly a million years, there were 236 events, and I think these are taken to be scoria cone, so that they might be one event, or they might be many events, but, at any rate, it's sort of an estimate.

He estimated, then, activities based on number of cones over a number of period of time, and we see here that a rate for that particular volcanic field was like 3 x 10^{-4} for events in a pretty active area.

And here's another one, Tlapacaya. In that case, there were only 12 events in 23,000 years, and he made some estimates, so there is some precedent for making these kinds of estimates over large areas.

Now, in terms of methods for Probabilistic Volcanic Hazard Assessment, there are many sorts of methods, and we've seen a few things presented, but I'd like to say that I think that the logic tree has a real advantage in this type of an analysis, and I think the first thing is, it can incorporate--it says here, "applied to a wide range of problems." You can incorporate a large number of models as nodes with different probabilities, from even deterministic calculations, but given probabilities of those on various nodes.

It can analyze sources of uncertainty, and it can accommodate interpretations with uncertainties, and the uncertainties can be passed on through in the analysis. It can also use probabilities from expert judgments, and it can incorporate extreme interpretations by assigning various levels of probability to those, and feedback between nodes is possible. So, I would say this is one method that--and it looks to me like this is a method that will be used in the analysis of Yucca Mountain.

Now, these are issues that I put together before coming to the meeting, and, possibly, they should be amplified to some degree here, or changed.

First of all, the vulnerability problem. There are lots of questions related to vulnerability, and one of them is: What is the minimum sized event that would present an unacceptable safety hazard? Very difficult, because we don't know, for example, what the characteristics of the canister distribution, or orientation, or loading, and so on. Therefore, we can't make a prediction of, say, a volcanic dike of one meter width would have with various orientations.

But, we might be able to get an idea what minimum sized events, because there might be some truncation there, and that would have some feedback, then, on what we would consider as a dangerous type of event, anyway.

And then, what is the probability of events according to size, and what is the probability of events according to space? I think, actually, this probability of space still needs to be determined. What is close enough? The word disruption of the repository has been used in conjunction with what I would call intersection. I would just say that a volcanic conduit intersects the repository. It might not disrupt it. It might just pass through, or it might fill the repository up, or it could explode.

But, what is the probability of events being close enough for effects? And I understand now that Valentine is going to be working on these kind of issues.

Now, other questions on the problem resolution, I didn't put down, "Enough is enough." I changed it to resolution or convergence or something like that, I think, might be a way we could think of this, too. I think one thing is to put this problem into a global framework, and, for example, compare these local forecasts of probability functions with those of larger regions; for example, the Great Basin, or some larger volcanic fields within the Great Basin, and it looks to me like some of these predictions have been made already at this meeting, setting some bounds on the probabilities.

Maybe we have to consider some--I call them qualitative scientific issues, but maybe we could express it in terms of geological models, and I put down here an example being expected mass rate of eruptions. Another concern of mine at this point is to incorporate, or to develop a more or less unified model for volcanism at Yucca Mountain from the generation of the magma and magma transport upward, possible storage in the near surface, and eruption, but to give some weight to these issues that maybe we can't put numbers on them, but we can certainly understand how the processes work, and understand the processes.

And, again, another recommendation I would have would be to incorporate expert judgment to evaluate some of these conceptual issues. For example, in spatial models, I think expert judgment could be useful in that respect, but in all of these other questions, too, it seems like a broader element of the community should be brought into the discussion of these issues.

And, I think I'll end with that.

DR. ALLEN: Okay. Thank you, Mike.

Are there questions or comments from the Board or from consultants, or from staff?

DR. CORNELL: Just a point of clarification. I'm, listening through the day, a little confused about some of the variations in terminology, and so on, that are going on in the models. I think some of what you said maybe clarified it for me.

There seems to be some interest in whether the average rate of what's going on should be measured in terms of number of events, should be raised in terms of mass rate, which, presumably, would translate one-to-one, provided there's sort of an average, the same about a mass per event, and I guess there's a question about that, also.

And there's a question--I guess there's a question of is that rate, one or the other of those, stationary in time, or homogeneous in time? We've heard this word being used, versus this question--I thought I heard Dr. Ho calling this a Weibull model. I'm a little confused about that.

But you also brought up the notion here of the non-Poissonian aspect; that is, what you--it was Wickman and his, it looks like what we would call a renewal model.

DR. SHERIDAN: Yeah, exactly; a renewal model.

DR. CORNELL: And this goes to the non-Poissonian, the non-memory aspect; that is, that where--and depending on the coefficients you get out of that, where, again, we often use a Weibull model to characterize the inter-event time, and then that Weibull model, depending on that beta parameter Weibull model, you either get these things clustered in time, or nearly cyclic in time, those being sort of the two extremes, with an exponential being in between.

Am I confused, or is there confusion in what I'm hearing today, or which of these remain issues in your interpretation?

DR. SHERIDAN: In my interpretation, yeah, you're quite right that there's a great deal of fog surrounding these issues, and the--do we use events, and then somehow take another function to be multiplied by the event, and, say, the expected size of the event, because, certainly, the size of the event that is expected is what is important for the vulnerability of the site to the event.

So, if we take the mass eruption rate, and we can get some distribution function in time and space of this, then that incorporates this other element. Otherwise, this logic tree, we need to introduce another function for the type of event; what will happen.

So, and I have some problem with this event myself, of how are these events in the historic past recorded. Now, what do we say is an event, and when did this event occur? And, apparently, this is kind of an important factor, but if we could think of--more like what would be the effect of volcanic activity, what is the distribution curve of the effect, that's really what we want to identify.

So, the mass eruption rate, we have one curve, and if we look at the event curve, then we have to have another function that identifies the type of event. So, you're quite right, and I think that the mass eruption rate is a much easier item to quantify, and I was going to give a little digression at the beginning and say it's something like predicting the damage done to cars striking a deer crossing the highway, and we're on the ark. Since I'm from the Niagara frontier, this comes to mind.

And in that sort of analogy, it depends on the size of the car you're driving, and it depends on a lot of factors, and we can go into these theoretical aspects and solve for this, or you can count road kill on the highway very accurately, and in the case of volcanos, you can count the road kill pretty accurately, and these other things are much less accurate to determine.

So, if we are to determine this mass eruption rate, then I think we have to determine what these magnetic anomalies represent, and probably drill into them, determine the age of the magnetic anomaly, and more or less the size of the material that's there, but that would certainly tell us whether we have a waxing or waning aspect and we don't have to worry about this beta function, this beta parameter for the Weibull function.

DR. ALLEN: Yeah, Bill Melson.

DR. MELSON: I was wondering in terms of our road kills at volcanos. I think at the break we had a little discussion about how much of the tephra's been lost by erosion here. I think this is a very valid question, and one that may be hard to answer, but we do know it moves very fast and very far erosionally, so it may not be so easy, I think to reconstruct a volume versus time rate, but it would be interesting to hear what Bruce Crowe and some other people here might say about that.

DR. ALLEN: Any other questions or comments?

(No audible response.)

DR. ALLEN: Okay. In five minutes, we're scheduled to start the round-table discussion. What we're going to do, as I understand it, is the tables, during the next few minutes, will be reorganized here, and when we come back, we wish all of the speakers during yesterday and today will sit at this front table, including our consultants. The Board is being relegated back to the audience, and staff, and we'll see how it goes from there.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Let's make sure that all of the speakers have taken their places at the head table, leaving the minority people in the audience.

We have no very formalized structure for the round-table discussion, but, hopefully, things that have been left over from the earlier presentations that some of you may feel you wish to talk about will do so. At least initially, I think we'll probably restrict ourselves mainly to people at this table, but be assured that before we end, we'll be glad to entertain thoughts and comments from anybody out there in the minority of you who are in the audience.

We've had a couple of requests, some dating even before this meeting, to make two presentations, so we will have two short presentations initially. I'd like everybody to try to limit themselves to five minutes, whatever you say.

Unlike McNeil and Lehrer, I don't have the power to turn off the microphone or put the television camera on someone else, so only the power of persuasion.

Carl Stepp, some time ago, requested permission to make a short presentation, and then George Thompson also would like to make a short presentation, so why don't we start off with this two.

Carl?

MR. STEPP: When Leon and I talked about this presentation, the first suggestion was that I discuss the ASCE guideline on seismic and dynamic analysis and design of considerations that is being put out by the ASCE. That guideline is in review right now by ASCE. We discussed this and decided there wasn't enough time, really, to say something that would be very informative to you about that guideline, and so he asked me to, in five minutes, say something provocative.

DR. ALLEN: You've got four minutes left.

(Laughter.)

MR. STEPP: Thank you, sir.

What I decided to do is address this issue of probabilistic versus deterministic approach with a very brief example of what's in the guideline, which I think we can rally around. It's, I think, a good way of getting past this, so that's the topic here, with just an example.

The example I want to show is for a hypothetical situation of a site in Colorado, where it becomes hypothetical because we modified the USGS hazard maps for that region by putting in a fault source here, with a cluster of earthquakes that was actually associated with fluid injection. Our site's located about 20 kilometers from that fault source. It actually has contributions to the hazard from Source 71 and Source 45.

Now, we do a hazard computation for those sources and for the region surrounding the site, and we determine a total hazard curve in the typical fashion of annual probability versus, in this instance, spectral velocity at 10 hertz, and we can do, also, the same computation for each of the contributing sources.

And, as you can see, at 10 hertz, the hazard at this site--and this is the first cut at determining where the hazard's coming from--is dominated by the fault source that we've introduced, even though the seismicity in this fault source is order of magnitude lower than the rate, for example, in Source 45, the magnitude is two orders of magnitude lower, a maximum magnitude estimate for that.

Now, a second cut at it, then, is simply to look at another frequency band, and this picture changes. For the 1 hertz frequency band, the major dominant contributor to the hazard is now Source 45, I believe--no, it's Source 71, which is the source that is within which the site is located. So, it's the major contributor to the hazard at the 1 hertz site. I think I'm wrong on that. That's the more distant source with a higher magnitude earthquake.

Now, at another cut in this, one can also do further de-aggregation, either on total hazard, or on a source-by-source basis to look at the relative contribution to the hazard as a function of magnitude and range, and also as a function of distance range, and as a function of this parameter epsilon, which is really the amount by which the log distribution, or the log of the ground motion varies from the median of the ground motion.

Now, this is very informative in that it immediately shows you at the 10 hertz range that the hazard is coming from a nearby source, and it's coming from very small magnitudes; that is, magnitudes less than about six, certainly, and they're clustered around this nearby source, and it shows you, also, that for those magnitudes, the hazard is being driven by ground motions that are well above the mean.

It's useful, also, to display these--and I'm going to stop with this slide, Clarence--in a three-dimensional fashion, just to get a better perspective of these contributions and, in this instance, we can see that the hazard at 10 hertz is dominated at about 68 per cent by small earthquakes on this nearby source. There is some minor contribution from earthquakes slightly more distant, but, still, in the lower magnitude ranges, and these earthquakes are coming, of course, from the more spatially distributed source within which the site is located.

May I show one more, just quickly?

When I contrast that with the same plot, showing a now distance versus magnitude for the 1 hertz range, you can see that the hazard is much more distributed over the source--the distances and, therefore, the sources, but approximately 70 per cent of the hazard is clustered out here at higher magnitudes at distances beyond 75 kilometers.

So, what this says is that at 1 hertz, we really need to be concerned with looking at these more distant earthquakes, I mean, larger earthquakes, and the more distant sources, and out of this, the reason I wanted to show this is, out of this, I think, is the solution to the probabilistic versus deterministic arguments that we have had some trouble with dealing with here in the regulatory environment.

This method that we propose in the guideline is purely probabilistically-based so far as deriving the hazard is concerned, and establishing the design level ground motions, but there is a follow-on activity that is recommended in the guideline of this de-aggregation of the hazard to develop a data base which can be used, then, to do a straightforward deterministic comparison.

In other words, if you want to look at the sources in a deterministic fashion, and compare the motions from them with the probabilistic results, this can be done in a straightforward way.

There's another two or three levels of depth that one can go into with this, but I'll stop there.

DR. ALLEN: Thanks, Carl.

Any comments or questions?

(No audible response.)

DR. ALLEN: Okay. One other person who requested to say something was George Thompson.

George, you can either do it from there, or--you have a view graph. Okay.

MR. THOMPSON: Leon invited me to come to this meeting probably because of my naturally disruptive personality, and I have sat meekly for a day and a half now, so I'm allowed to say a word.

I've been interested in dikes for a long time as stress indicators, and I think the important thing that I might say in just a couple of minutes here today is that there's a very close complimentary action, some physics between dike injection and normal faulting in extending areas, and the general notion is ultra simple.

In the elastic extension of the seismogenic layer, which you see at the top there, you can get response either by an earthquake showing the extension here taken up by the earthquake, or you can get it by injection of a dike, and, just briefly, there's evidence of this kind of relationship all around the world in a lot of extensional environments, and I think it's expressed at Yucca Mountain by the lack of earthquakes in the Crater Flat area, even micro earthquakes are scarce there, and that's the area where there is volcanic action, and also expressed by the flatness and lack of topography in that area.

Now, what we're seeing here is that as elastic stress builds up, it can be relieved either by injection of a dike, or by a normal fault like this, and it's shown in a Mohr Coulomb diagram, for those of you who are familiar with that.

Dikes, in doing this, are like hydrofracs, which are used either to measure stress, or they're used to increase permeability and oil production, and the dikes frac in perpendicular to the least principal stress, and one can see lots of geologic evidence that it doesn't pay much attention to inhomogeneities in the rock. If there's stress difference, the dike goes across those inhomogeneities quite faithfully.

A dike differs from a hydrofrac in one very important way, and that is that the basalt magma chills, freezes, it doesn't penetrate the pores of the rock, it doesn't leak off. In petroleum production, they're lucky to get a few millimeters of opening when they hydrofrac to increase the permeability. Dike widths are commonly something like a meter, sometimes several meters.

In injecting this way, they operate against the least principal stress, which was over here this way, and they change the stress relations so that some other stress direction of the principal stress has become released, and if another pulse of magmatism comes in, then you get an orthogonal direction of injection, which is what's shown here.

In this case, what was originally the least principal stress this way has now become a maximum principal stress because of the dike injection, and the new sheet comes in perpendicular to the vertical.

I think that's important at Yucca Mountain because there is some interplay, then, of earthquakes and dikes. Looking at the geologic map of the area--you've seen this several times--there's this very nice alignment of the one million-year-old cones, and that tells us--that's underlain by a dike or dike swarms in this direction here. It tells us the direction parallel to that is the maximum principal stress, or, perpendicular to it, the least, and that's the record at a million years.

The record today is given by active hydrofracs in Yucca Mountain, which agrees very nicely with this direction, and the record today, at depth, is also given by the focal mechanisms of earthquakes, most recently, the Little Skull Mountain earthquake, which also gives that same direction.

So, I think that that's all I'll try to say about that topic now, but I do want to say, in regard to some of the questions that were asked about what can geophysics find under Crater Flat, what's it capable of doing, dikes are picked up aeromagnetically routinely all over the Canadian Shield. Some of them go for more than 1,000 kilometers on the Canadian shield, and they're picked up under glacial drift.

They're picked up in the Appalachians, and I just looked at the map of Virginia yesterday. They go for hundreds of kilometers in the Appalachians, so dikes are readily recognizable by magnetic means if they're big enough.

These things around Yucca Mountain are very puny compared to the ones that one sees, which are often tens of meters wide, or at least a swarm is large like that. The largest aeromagnetic map, the most obvious thing of the aeromagnetic of Nevada is a dike swarm which extends for several hundred kilometers, but the composite width of that is a couple of kilometers, and it's bigger than a meter.

Now, you'd quickly lose a one-meter dike at Yucca Mountain as it gets buried in alluvium, or if it's injected into the magnetic noise of the tuffs, which are also somewhat magnetic.

May I show one more view graph?

DR. ALLEN: Sure.

MR. THOMPSON: To indicate that things are picked up very nicely, I brought in this one view graph from an AGU presentation by Victoria Langenheim of the U.S. Geological Survey. This is just south of Crater Flat, and there are magnetic anomalies which have been mapped A, B, C, D, E here. She's modeled one of them in this next view graph--I'll slide one more in on you.

Now, these are buried cones, and one of them has been drilled by a commercial company, and you've heard about this. Here it is. It's a couple hundred meters deep. It's a small cinder cone that's been modeled here and fits the magnetic anomaly quite nicely, so this thing's already detectable.

For geophysics, in general, the high resolution to look inside Crater Flat is going to have to be reflection seismology, and, as you know, some of that is planned in the near future.

Thank you.

DR. ALLEN: George, you've stated that the lack of current seismicity is consistent with your hypothesis here. If dike injection were going on right now, I mean, in this period of five years, wouldn't you expect to see some seismic signal of that?

MR. THOMPSON: The rates of extension that have been quoted for this area would suggest perhaps a hundredth of a millimeter per year. That's a one-meter dike in 100,000 years. Any dike injection will tend to decrease the deviatory stress and make it near zero, so something 100,000 years ago would have relieved the stress for that kind of period.

So, if we've had things 10,000 years ago at Lathrop Wells, they may, indeed, have had an effect on the seismicity in this area.

DR. ALLEN: On this particular question, idea, any particular comments or questions? Either everyone believes you or no one believes you.

Incidentally, it's even more important now that we identify ourselves, because it's very difficult for Scott over there to tell who's speaking.

MR. THOMPSON: I think Frank made a statement that volcanos are good for preventing earthquakes.

DR. ALLEN: Okay. Thanks, George.

Instead of asking a specific question here, I had the feeling during the meeting yesterday and today that some of you have some impelling things you would like to say, and either you didn't get a chance to, or got cut off, or ideas came up later, so let me just ask the people at the front table here:

Do some of you have--are you impelled to say something? We welcome such remarks. Bruce, you looked sort of fidgety this morning a couple of times.

DR. CROWE: Let me try to just synthesize my comments into two, and the first one really is directed at a range of people.

I think I disagree a little bit with the concern that was expressed about the definition of events, and just let me say, I invite people to read the revised version of the status report, because I tried to address that in some detail. But, the reason that's important is, in my presentation, what I talked about is how we describe recurrence rates based on definitions of the formation of a new volcanic center, and then we draw a distinction between recurrence rates for polycyclic activity.

And the reason we do that is, again, the formation of a new volcanic center--and the volcanic center refers to like Lathrop Wells, Red Cone, Black Cone, a spatially discrete volcanic center--has spatial uncertainty where it could form.

A polycyclic event requires the pre-existing event to have started to form a center, and then there is a spatial restriction of the next events to either the same center, or to within clusters, like the model that Frank described for dispersed.

So, what I have seen in a number of the talks that gave higher probability recurrence rates, was they were intermixing volcanic events or center events with polycyclic events, and then, actually, discrete events, and all I can do is plead for some rationality, and I certainly took a shot at trying to describe this in a uniform way. I'm sure it won't be acceptable to everybody, but at least, perhaps, if we could begin to agree on our definitions, and either agree that these should not be mixed, or make arguments of why they can be mixed to do probability calculations, we would get rid of one area of confusion.

Okay, and then the second comment really is for Chuck. You know, I think that your mathematical models are important, and I think anything that brings a new perspective at looking at probability is useful, but one of the points that I made in my talk was there is an inherent non-predictability spatially when you look at the sequential position of events, and all of your models assume that clusters are the locations of other events gives you some information that constrains the position of next events, and what you run into a risk of doing is imposing, by a statistical approach, a predictability that's not there in the record.

And, additionally, one issue that I don't think you addressed was the limitations of your database. You very cleverly--I have to compliment you for this--sneaked in Miocene events with the Pliocene and Quaternary to make your cluster models have better significance, but if you're dealing with just the Pliocene and Quaternary, you're talking about either--let's just say the Quaternary. You're dealing with seven total events, or three clusters, and there, any kind of a multi-variate approach, you really have to question the significance of it with that kind of a database.

DR. ALLEN: Chuck Connor?

DR. CONNOR: My name is Chuck Connor. I'll respond to a couple things there.

One is, I think that, as you recognized in a qualitative way, and we quantified, volcanism is spatially and temporally clustered over time in the Yucca Mountain region. That's information we can use. That's information we can use to help look at probability models.

In my test over time in the probability model, I found that looking at events that had happened, say, before the Quaternary, gave you good indication of where the future Quaternary events were going to be, so I don't know how you can really begin to do that with your homogeneous model, and you probably don't want to, but with some of the calculations I did, trying, in a very honest way, to calculate what the Poisson equivalent, what the homogeneous Poisson equivalent would be, it turned out that the non-homogeneous model was fairly successful, comparatively very successful at predicting the future locations of volcanos, and I think that helps us, puts some faith in a utility of slightly more elaborate probability models, spatially and temporally non-homogeneous probability models.

Second, the main feature of the analysis that I presented is that volcanism tends to cluster through time, close to the Yucca Mountain repository; that is--well, that's a relative term, but in the Crater Flat area, and that cluster doesn't depend on Miocene events at all. I didn't sneak anything in there. All of the events I used in that area were by your dates, which are great, of 4.4 million-years-old or less. They go from 4.4 to 3.7, and those events are what created a anomaly in that region which helped increase the probability that future events would occur there, based on my model. In fact, that's why the Crater Flat volcanos, Red Cone/Black Cone, were predicted well with that model, compared to other models.

Now, again, I would get back to the idea that--and I think other people who work in probability can do this--if you do show that volcanism in the area is spatially clustered and temporally clustered, and we know that's a very common condition in volcanic fields--I don't think anybody's ever found a volcanic field where that didn't occur--is it really reasonable to use a homogeneous model, and should those be included in the kinds of probability compilations that you're doing? I would think not, but that's just my position, I realize.

DR. CROWE: Let me respond. You still didn't answer my questions. There are two questions.

One is: Given that when you plot the sequential position, that you get very non-systematic jump directions and jump lengths in where the next event, how can you justify the confidence you put in spatial predictability; and second is the statistical distribution of the small number of data sets. You didn't answer either of those.

Let me bring a third question that you can try not to answer, and that is that what we have pointed out with the cluster model, that when you have like a 1.0 million year event, Red Cone, Black Cone, those four events, or five, or whatever you want to call them, what we've pointed out is that we have not been able to separate them in age given the uncertainty of our dating methods.

In your cluster model, if you treat each one of those as an independent point in cluster, you're gathering too much weight to what could be just one single event that just was spatially dispersed. So, could you also address that?

MR. CONNOR: Yeah, I'll address that. I think for the Quaternary events, in particular, it's okay to go out there and count volcanos, but other than that, yeah, I treat those as separate events and, geochronologically, they're--right now, with the resolution of methods, apparently not distinguishable. That's fine.

I would point out that it's possible, therefore, to take that date and say that the area affected by volcanism, by a likely volcanic event is much larger than the area I've been considering so far, and I'd be willing to do those calculations. In other words, if we say that the Crater Flat volcanos are one event, then, certainly, one event has to be considered to affect a much larger area than anybody's considered so far. So, I think in the wash, that'll all come out as being a pretty--wind up with a pretty similar solution.

Second, I showed a slide of four maps of the non-homogeneous Poisson probability model, and those maps were created to test the model in what I think is a very conservative way; that is, just before episodes of volcanism, and those maps were only based on volcanos that had happened prior to that time, and, as I pointed out, that analysis indicates that for what's happened so far, particularly in the Quaternary, the spatial locations of the Crater Flat volcanos and Lathrop Wells are much better predicted using a non-homogeneous model than a Poisson model, and the Sleeping Buttes are not, and if you put different parameters into that model, you can make it predict the location of Sleeping Buttes a little bit better, but I just didn't do that. I don't want to push the analysis so hard.

The point is, is that volcanism has clustered through time in Crater Flat Valley. That creates a non-homogeneity in the recurrence rate spatially throughout the region, and when you take that into account, you get higher probabilities of volcanism in that area, and that's important because the Yucca Mountain, the candidate repository, is close to Crater Flat Valley, and so it's important to take into account that kind of detail if it's available in your data.

This is coming out of just spatial and temporal data that have been gathered, so I don't know how to say it other than that.

DR. ALLEN: Let me ask if any of our other volcanostatisticians have anything to say about this.

John Trapp?

MR. TRAPP: Yeah, there's just one point. The one with the Markov model that was shown, Chuck, while I was down at the Center, ran another one, and the difference between the two was really kind of including the Sleeping Buttes area versus not including the Sleeping Buttes area in the whole calculation.

What that seemed to show is that you've got a tremendous difference in the probability calculations. One of the possible things that is being shown by some of the things that Chuck has got is maybe the Sleeping Buttes, as far as a probability calculation, do not fit the same model as the Crater Flat. There may be something structurally and statistically significant about those two zones, and the Crater Flat zone may not be a geologic reality.

DR. HO: I have one comment on the homogeneous and non-homogeneous Poisson models here. I think this shouldn't be the issue here, because the Weibull model that I used is a generalization of the homogeneous Poisson model, so in older days, when motorcycles not available, people ride bicycle. But, now, we have motorcycle available. You can climb the hills, you can go down hills, and you can also ride comfortably on the even space here, so why do you want to compare a motorcycle and then bicycle? Because have the ability of doing the other completely, 100 per cent.

So, this Weibull model, including homogeneous Poisson model, so we should 100 per cent abandon that model. That not the issue, though. It's dead, it's dead, and I'm showing that already. When the beta is one, indicating that is homogeneous Poisson model, reflect by the Weibull model, so why this is an issue?

And Chuck's model is a two-dimensional uniform distribution for the volcanos. I hesitate to use the homogeneous Poisson and non-homogeneous Poisson model. It's a non-uniform, two-dimensional spatial distribution, so the terminologies is different, so I think this should be clarified in a very clear manner in any kind of report.

And, again, my second comment is I have a question for Dr. Bruce Crowe about, I don't quite understand your definition about a Tripartite probability models. Would you please tell me what is your definition about E1? What is the E1?

DR. ALLEN: Bruce?

DR. CROWE: E1 is what I presented to you as defined on three things. One is the recurrence rate of new volcanic centers. That's what we call lambda(v). The second one is the recurrence rate of volcanic intrusions, and the third one is the recurrence rate of clusters of volcanos, assuming that the clusters form one synchronous event.

DR. HO: Okay. Now, here, after receiving your definition here, E1 is a recurrence rate, right, a rate which is not random, which is not a random variable, which is a constant. How can put a probability on a constant?

For example, this E1 can be the true weight of everyone's weight in this room, the true weight, true average weight of everyone's weight in this room. How can you put a probability on this true weight when it's a constant, is a constant, which is a known?

So, this models into a statistician or to a mathematician who has a better training in probability, you say, gee, what's the probability of a parameter? That's nonsense, that's nonsense; is a constant.

DR. BUDNITZ: Absolutely not. If I asked everybody in this room to try to see if they could estimate my stature, my height, which is exactly known, you'd get a distribution which would represent everybody's state of knowledge, leaving me out, because I know it, and that has an absolutely perfect validity for the purposes of analysis like this.

What it is, it is an assessment of people's state of knowledge, and that state of knowledge is represented by distribution, a density distribution. You can call it a probability if you want, but I think that's a better notation. It is a density distribution of people's state of knowledge, and it has a perfectly well-defined meaning and validity, I insist.

DR. ALLEN: Peter Wallmann also had a response to this.

DR. BUDNITZ: It's a completely non-trivial point that you shouldn't be confused about.

DR. ALLEN: Just a moment. Peter Wallmann also raised his hand.

DR. WALLMANN: I think we have to remember what we're going to use this information for. We are not going to use this information for--I mean, to think that we are going to find the recurrence rate is nonsense, but we're going to use it in performance assessment. How is it used in performance assessment? It's used as a distribution. It is not known. You use parameter uncertainty in performance assessment, because you don't have enough knowledge to fix all parameters in your system, so you describe both model uncertainty and processes, and parameter uncertainty that feed the models.

DR. ALLEN: And now back to you, Dr. Ho.

DR. HO: Okay, yes. Now, back to your comment here. So, you use--this is serious business here, yes. You use the distribution to quantify your weight, your weight, but that distribution have a center which is true value. There is a mean there with a true value here, so--which is the norm. So I accept that we can distribution to quantify something, something, but over here, you are talking about probability of E1. I think I will prefer that E1 shall be the event, the event of an eruption, of an eruption; event, and that's random, that's random, and that event is quantified by a Poisson distribution, which has a known true average, E1.

DR. ALLEN: Allin, you started to say something. Do you want to finish?

DR. CORNELL: No, I think I'll stay out of this one. I've said enough.

DR. ALLEN: John Trapp?

DR. TRAPP: I'd actually like to go back to some of the questions that you were asking. I mean, this discussion really kind of brings the whole thing out.

This really ties in, because one of the points that you were trying to bring out in this meeting, are the numbers going to change? And I think what you've got right here is a very good example that the numbers really are not going to change much in the future. We are going to have the same range of numbers, I believe, when we get to licensing.

One of the most compelling things that I have seen brought out in this meeting, one of the things that really does give me a lot of confidence, at least in the volcanological field, was what was presented by DOE as far as their Probabilistic Volcanic Hazard Assessment that they're planning on doing that Kevin presented, and I think that is really the one thing that may get this question off dead center and really get us going.

DR. ALLEN: Any comments on that statement?

DR. BUDNITZ: In my talk, I made an appeal, because I'm an ignorant person in the volcanic area, although I know some about seismic. I made an appeal for somebody to tell me--excuse me--tell the world how much difference matters. If

Crowe is right, and all of those things within a factor of, plus or minus factors of two or three, full range, less than a factor of ten, maybe that's--maybe we know enough, but I don't know that we know enough, because I don't know.

But, somebody needs to know that and tell us, and then we have to scrub up that analysis and see if we agree. The same thing with seismic.

DR. HO: I have a comment on that point.

We have a very common slogan or trademark indicating that since the event is stochastic, therefore, the answer is necessarily probabilistic. So, if the answer is probabilistic, your question probably very hard to answer. Probably the true answer is existing somewhere, but nobody knows at this time. So, somehow, we have to live with that one, and then work on that. Maybe it's a little bit too abstract, but I've done my best.

DR. CROWE: Let me make a comment. First, I'm encouraged by John's comment, and I find that we can have agreement that proceeding into expert knowledge, I think that's a very positive step forward.

Second is just one suggestion I have, that a way, perhaps, we could at least try to understand our differences a little bit better is to try to cast our distributions, our values for the probability, E1/E2, or E1 given E2, is a probability distribution, and when we talk about how here's our numbers that I got, here's the numbers that they've got, talk about where they sit in the distribution, because sometimes I think we're talking about a mid-point or a median or an expected value; other times, I think we're comparing tales, and we're not always being as careful as we should be about where we are in those.

For example, I think some of the ranges that I heard the State and Chuck and the NRC present, I think, are captured in my probability distributions. I just don't know where they would put them on a distribution, and so, perhaps, as a suggestion for a way of getting over a communication barrier here is try to assign a probability distribution and tell us where your numbers sit in that distribution.

DR. ALLEN: Well, let me ask a question here, and change the subject a bit.

I suspect that more people for a longer time have been working on earthquakes hazards at Yucca Mountain than have been working on volcanic hazards; a greater number of people, a greater effort, an awful lot of work has been done. John summarized some of the most recent results.

Why aren't we arguing here about probabilities of earthquakes? Why is all our attention on volcanos? Why haven't we, in our structure here, why haven't we gone into the probabilistic--and I'm not sure John even wants, you know, it's not necessarily your responsibility, but do you have any comments on that?

MR. WHITNEY: Yes. This is John Whitney with USGS.

You know, yesterday, I think I heard a note of irritation from part of the Board about--

DR. ALLEN: Oh?

MR. WHITNEY: --the fact that seismic hazard hasn't progressed very far, but I think you really, in all fairness to the program, you have to look at the history since the site characterization plan was written in '85 and '86, and then was released at the end of '86. The program went on hold in a data collection sense, and, actually, because of the public perception of a new hazard at Yucca Mountain; i.e., volcanism, Lathrop Wells became a very large center of attention. That program actually started data collection several years before seismic hazards got back, or the whole tectonic geologic data collection process got back on track.

So, actually, the number of volcanic centers was pretty much known, and has been known for some time. The actual problem of dating, of course, has been a real problem, but that's not, in a way, the major volume of information that Bruce needs for his calculations.

We're starting--so, there is another point. The volume of data needed for a volcanic hazard analysis is, in my opinion, probably an order of magnitude less than what we will be using in the seismic hazard calculation. We got out

100 kilometers from Yucca Mountain, and we have to evaluate every single major lineament, and then categorize it as a potential relevant source. So, we have a heck of a data collection process to go through.

Then we have a whole interpretive phase of actually picking out fault parameters, ranking relevant seismic sources, the ground motion modeling, et cetera. All is actually beginning to be done now.

Furthermore, because of the state of the knowledge in the early eighties, the site characterization plan had a very strong deterministic orientation. It was called the so-called 10,000 year characteristic earthquake, and with the new team that came in in the last couple years, we've discarded that completely, and that's one reason why the topical report was written.

There were open items from the NRC looking at the site characterization plan. We wanted to revise our approach and use and take advantage of the work that's been done through the eighties, and, so, this is something that really couldn't be done overnight like that. So, all those factors, I think, tell you why we are a couple years behind, but we have collected a great volume of data in the last couple years, and, as I said yesterday, I think that we will be very close to beginning our actual hazard analysis this time next year.

DR. ALLEN: Okay, thanks.

Any other comment on this?

MR. SULLIVAN: Tim Sullivan. Let me just follow up and summarize on John's remarks.

DOE recognizes that there's a significant misfit between the Board's expectations of a seismic hazards program, and the schedule and program planning that we laid out yesterday.

In response to that, it seems that I ought to carry back to my management a recommendation to reconsider the prioritization of activities within the seismic hazards program, and perhaps overall site characterization activities, and I will do that.

At the same time, we will pay close attention to the evolution of the volcanism program as it moves into its assessment phase, and learn what lessons we can from that program.

DR. ALLEN: Okay. Buck Ibrahim had a comment on this subject.

DR. IBRAHIM: According to the statement yesterday about the topical report, correct me if wrong, that the final report will be coming around 1996, or maybe take more than that, and we are sure now that the ESF is progressing, and whoever has gone through the ESF like to have some kind of design to proceed with their ESF, and if are waiting for 1996 and, God knows, maybe take until 1998 to come out with that kind of value.

I know you have the preliminary value, which you mentioned yesterday, about .4, but, again, if you read some of the other reports, they are coming out with a value of .75, and I really don't know which one DOE is trying to dictate or tell the engineer to design for, so unless we progress fast, a little bit like what Paul Pomeroy suggested yesterday, I think some of that will be waiting until 1996, 1998 to come out with what is our value trying to tell them.

MR. SULLIVAN: As I mentioned yesterday, and DOE didn't present this in detail, we have completed a preliminary, simplified probabilistic seismic hazard assessment in support of the underground portions of the ESF design, and maybe I ought to ask Rich to just take a minute and describe that. It will be issued or released within the next couple of months. It's intent is to support the underground portions of the design, and, particularly, those that are to be incorporated into the repository.

DR. ALLEN: Yeah. Rich Quittmayer.

MR. QUITTMAYER: Yeah, I can just say a few words about that. The approach that was taken is a probabilistic approach. John Whitney, yesterday, during his presentation, showed some of the inputs that went into that, the source characterization, which was summarized in one of the tables in his presentation, and he also showed one of the hazard curves that has come out of that, showing the contributions of some of the sources.

As Tim has said, you know, we're in the final stages of writing this work up. It still needs to be reviewed, and then should be out within the next couple of months.

DR. IBRAHIM: Just one simple question about some of this preliminary data. You discovered recently a new fault. Is that new fault considered in this analysis, or it will be updated, or what is the situation?

MR. QUITTMAYER: No. When the analysis was done, the Sundance Fault had not been identified as a specific future, and that is not included as a specific source. There is a background source to accommodate seismicity that is not occurring on identified faults, so I guess the answer was yes and no.

DR. IBRAHIM: No, but I'm saying, I mean, it seems to me that unless we go ahead and try to characterize the site and do the investigation which has been asked by NRC for a long time, we are going to really be delayed by going through this licensing this site.

DR. ALLEN: Well, in defense of the DOE, this was a fairly recent discovery, quite recent discovery. It's under intensive investigation right now. It's not clear that it is a seismic source, so at the moment, it's preliminary to talk about using it, it seems to me.

DR. IBRAHIM: But I thought that that fault was seen in 1984, as is indication.

DR. ALLEN: John Whitney can better explain this.

MR. WHITNEY: Yeah. Let me give you a little bit of information about what we've done.

The detailed surface mapping over the repository block is going on along in strips along several of the ridges that overlie the block, and, also, along the projections of the Ghost Dance Fault, and during that mapping, a northwest feature that did contain a, or structure that contained some breccia in it was found, and the amount of offset on that feature is not really known at this time. It's not known whether that structure is a volcanic--a collapsed feature from within the block, and it's extremely old.

So, basically, we're kind of talking about unreviewed, raw geologic data, and in a program like this, we're kind of in a situation where we're damned if we do and we're damned if we don't. If we find something and don't talk about it, we're told we're covering up new information, and if we put stuff out before we've studied it--and I can't tell you exactly what that fault means--it makes it look like we haven't done our work. So, we're in this kind of no man's land with new information here.

The thing that is important to realize with these earthquake faults, as they're called in the newspapers in Las Vegas, is that the total offset on a structure that cuts a 12½ billion-year-old volcanic unit is less than 30 meters, and there are no surface expressions in rock of these structures having moved in welded volcanic tuffs. If there has been any movement within several hundreds of thousands of years, we expect to see some sort of topographic expression of these things. It's not there.

So, we have a program this year to evaluate the history of these structures as best we can, and we will, unlike most places in the world, we should be able to see some of these old inter-block structures underground, as well as on the surface, but our first analysis will be on the surface. We will actually try to use exposure dating to show how long the surfaces that have been cut across these structures have been unaffected by any kind of movement.

Also, there is a possibility that we have, within our program already, analog studies of inter-block activity out in Midway Valley, and, for example, we may decide to look at the geophysical anomaly called the Midway Valley Fault, which everyone knows has to be there from the cross-sections of the geologic maps, and our detailed gravity profiles to pick it up, so we kind of know where to trench.

We know that the materials that cross that structure are hundreds of thousands of years old. That's not going to be a debatable point. Whether it's 400,000 or 550,000 years old may be debatable, but that's where the debate will lie.

We can see no surface expression of this fault. We don't even know where to trench. We have to ask a geophysicist where to trench.

DR. ALLEN: And I think it's only fair to point out that these ongoing field investigations are under some financial straits because of urges by some groups that they get underground very quickly.

We only have some 25 minutes left, and we are going to cut it off at four o'clock so people can leave.

Let me ask if there's some people in the audience who have any urgent statements or questions or comments they would like to make? You're welcome to do so.

(No audible response.)

DR. ALLEN: I can't believe it.

MR. SULLIVAN: While the audience considers their comments, DOE didn't invite the speakers here today, yet, I thought I heard a majority of those who discussed seismic hazards conclude that probabilistic seismic hazards analysis is appropriate for assessing seismic hazards at Yucca Mountain, and, further, that that's the best way to describe those hazards.

DR. ALLEN: Well, would anyone disagree with that?

MR. McCONNELL: Yeah. I wouldn't characterize our response in those terms. I think we'll hold our response until we see the topical report, or the series of topical reports, and if I could just make one other statement.

It was brought to my attention there might be some misperception about the NRC desire for a topical report. We have not taken a position on whether the topical report approach for seismic hazard methodology is the appropriate approach to take. We just have no objections if DOE chooses to take that route, so we're not promoting it or--

DR. ALLEN: I feel if Ellis Krinitzky were sitting at the table, you might have some response from him.

MR. SULLIVAN: If I could respond to that, Keith, just quickly.

Keith, you did send DOE a letter identifying seismic hazards evaluation as an appropriate topic for a topical report.

MR. McCONNELL: That's a little bit less than endorsing it. It doesn't say one way or the other whether we accept that methodology. It just says that as far as a topic for a topical report, it's fine, and--well, I'll leave it at that.

MR. WHITNEY: Following on that sentiment, and remembering what Paul Pomeroy suggested in his presentation, how would you feel if we just did our own probabilistic seismic hazard analysis without giving you a topical report or a study plan, just go out and do it?

MR. McCONNELL: Well, I think, while study plans aren't statutory documents, they are derived from a statutory document, and there would be an expectation study plans would be forthcoming.

I think the approach to doing a seismic hazard analysis is up to DOE. We've tried to put out, in both our letters to you and in this presentation what our policy would be on approaches to determine seismic hazard. How you then decide to go, well, how DOE decides to go is really up to them, but we've also made clear that you should come to us and discuss these things early so that we don't get into a situation where we get to licensing and we don't agree, and there are significant problems with the approach.

So, again, I'm not saying that we don't, or that we object to a topical report. I'm just saying that that decision is DOE's decision, and that we neither endorse it nor detract from it.

DR. IBRAHIM: I just like to ask question here. Is the topical report coming out with anything new than what exists now everywhere in the literature?

MR. QUITTMAYER: The topical report describes a methodology that is based on experience in seismic, probabilistic seismic hazard assessment over the last decade, so there are no real new concepts involved. The application of probabilistic approach to fault displacement estimation is not as well-developed as it is for vibratory ground motion, so, in some sense, that may be a new application of it, but...

DR. IBRAHIM: I would say, really, if there is not much difference between what was existing now in the literature and what you are coming soon, I didn't how--why it will take so long for two years to come out as a report.

DR. ALLEN: Any comment?

MR. QUITTMAYER: The question was why it takes so long?

DR. IBRAHIM: Yeah.

MR. QUITTMAYER: Because we had too many people review it.

(Laughter.)

DR. ALLEN: There's an answer for you. You're probably right.

Allin Cornell?

DR. CORNELL: Yeah. I think to that same question, I suspect strongly the models used are probably not going to be unusual or unexpected. I think what is probably not well-resolved is how you use experts in that process. There's a number of us in the room that are still kicking that problem around seriously for the next--what to do in the next ten years, so I wonder what DOE has in mind in its topical report with respect to that, and whether you anticipate doing something new, different, of the possibilities open; that is, basic previous studies, which of those might you be following, an EPRI-like, Livermore-like, whatever. Has that been resolved?

MR. QUITTMAYER: Well, I think the short answer is no.

DR. ALLEN: Incidentally, vis-a-vis what you said, I agree with you. I think the ground motion problem is not really a serious issue. In my opinion, the fault displacement problem could be very, very tricky, because of the multiplicity of faults we're now discovering, the width of these fault zones, if they eventually have to be declared potentially active.

It's very easy to say, yes, we can put in drift emplacement. It's not quite so easy to say exactly how that's going to solve this problem, and I guess my judgment is that in the area of earthquake hazard, it's the fault displacement problem in the long run that's going to be the bigger hangup, and then trying to convince people, if that's what we're trying to do, that that is a solvable problem.

DR. IBRAHIM: Also, it seems to me we are concentrating also here on the larger vent, or the maximum earthquake in the area, and I'd like to say that, also, we must put some attention to the small earthquake, because within--after post-closure, the canister will deteriorate and will be crusted, and maybe the load from the small magnitude earthquake will affect the capability of the canister to hold the waste, so we should really be concentrating on some of the small events, the multiple events, of course.

Small events occur like, what we know, that two, three months or something like that, so the load from this two event per month, what would be the effect on the canister after, for example, 1,000 years, and we should take that in consideration.

DR. ALLEN: Other comments?

DR. HO: Can I make one more comment?

DR. ALLEN: Dr. Ho.

DR. HO: I have a little bit uncomfortable feeling about the way people present their results, their final probabilistic results. For example, the annual probability of site disruption was 10^{-8}. What did that mean? Specifically, what did that mean? The annual probability of site disruption is 10^{-8}. Can any of you answer that question? What do you mean by annual, annual 10^{-8}? Every year we have a constant probability of disrupting that site, or what, or the first year? And can we use that throughout 10^{4} years?

DR. CORNELL: The answer to that, of course, depends on whether you think the process is stationary in time. It depends on whether you think the process has memory or not, and it seems to me in the earthquake area, we've addressed both those questions, and sometimes yes and sometimes no.

I also see that as exactly what you're kicking around in the volcanic area, also. The question is, most people seem to be posing their answer not in terms of the annual probability, but the probability in 10,000 years. I mean, what is the probability of one or more events in 10,000 years?

Now, so the question is, do you--that, of course, that answer could be obtained either by multiplying the annual rate by 10,000, if everything is stationary, et cetera, et cetera, et cetera, and non-Poissonian, and Poissonian, provided the numbers are small, all those caveats, but I think the other question that we haven't heard answered is what context, in what format do the assessment people and the criteria people want their answers? I mean, we haven't heard that, it seems to me, answered finally, and I guess we don't have an answer to that yet, do we?

DR. HO: Yeah, but if we assume that the rate is constant, the probability is constant, then we can use that. It's that kind of standardized number to do the final decision, but in this Yucca Mountain risk assessment, my analysis indicated that the probability and the time is not linear. So, therefore, you cannot divide the time in saying that this is annual rate.

For example, one easy example may be best analogous to this one. The doctors use the survival rate to let the patients make decision whether--suppose you have a lung cancer, and they say your five-year survival rate after surgery is 85 per cent, 85 per cent. And now, if you divide by five, what does that mean? It doesn't mean anything, doesn't mean anything.

You don't make a decision based on the one year, even though it is meaningful. Right. People say that your first year, survival rate is 98 per cent, but when you hear about a five-year survival rate is only 50, 45, then you hesitate, so in that case, the way we present the final result in terms of probability terminology should incorporate time frame into that one, no matter whether it's linear or not, and at this moment, my calculation indicates it's non-linear, so it's not go linearly with time. So, the first year times 10^4, they mean the risk for the four are 10,000 years.

DR. CORNELL: I don't think anyone disagrees with you, Dr. Ho.

DR. HO: Thank you.

DR. ALLEN: Peter Wallmann.

MR. WALLMANN: Bruce may want to make some comments about the rate, because I think, geologically, we have to decide--a consensus has to be reached of what is the rate doing. Is it waxing, is it waning? Data is not specific one way or the other, but, as Dr. Cornell mentioned, how do performance people want this, I can't speak for the program, I can only speak personally.

It's a distribution. That's all you can do. It is possible to update that distribution as time goes on, if necessary. So it can be done, it can be treated that way. It need not be constant through time, but if one is going to make it non-constant through time, I feel you need a mechanistic argument to do that. You don't make models more complicated just because it's fun to do. You do it with a reason.

DR. MELSON: I would like to leave this just for a second and add a new thing for you to think about.

Now, it seems to me the consequence analysis is really moving to the fore. I think the importance of that was certainly brought out by Gene's slide showing the Tolbachik eruption, the great Tolbachik eruption and a heated discussion which followed before lunch.

I think now it's time to be very specific in a new way about the event, and what kind of event is it going to be. Will it be a pyroclastic eruption, like the great Tolachik eruption, which I think is highly improbable, or will it be a smaller eruption that we see at some other cinder cones? But I think this is a very important issue, or will it simply be lava flow, with very little cinders at all? So now we have a new probability within the consequence E3; that is, what's the probability of certain kinds of eruptions, and then that has to be integrated with design characteristics to give us some idea of release.

If we have a great Tolbachik eruption through the repository, I think we would all agree we have a very serious problem.

MR. CONNOR: I think a PDF for explosivity is the logical thing to go for, and I think everybody's going to try to go that way, but now that we're back on probability, I asked a question before. I probably didn't phrase it succinctly enough.

It is that: When do you decide to reject a model in your probability estimate? And, specifically, I want to say that we've shown that there is statistical significance to spatial and temporal clustering, even though there are few vents. It's statistically significant with 90, 95, 99 per cent confidence. It's very statistically significant.

I've shown that some models, heterogeneous models, or non-homogeneous Poisson models, Markov models do a better job at predicting where events are going to be based on what's happened in the past than the homogeneous Poisson model does.

Well, if we, in the end, need to incorporate a range of models, which I think is appropriate, when do we decide not to incorporate a model? Normally, in spatial statistics, I think, you study the distribution, and that gives you clues as to which model you want to use and which model you don't use.

When I saw Peter's talk, where he defined a whole bunch of different areas, I thought, yeah, this is a good indication of the problems when using a homogeneous Poisson model. Why not just move on? A lot of groups have in other disciplines and it's, you know, why use--why stick with a homogeneous Poisson model if the statistics show that's probably not the best description and doesn't behave in the best way through time?

DR. CROWE: I'll make it quick.

It really isn't a question of one or the other, and that's what I've tried to emphasize in my presentation. It's an issue of how do the probability distributions change, and I think the only way--and we've tended not to reject it, and if you noticed in my summary tables, I included your non-Poissonian models in the distributions.

My position, from having been in this program for awhile, is that if I make the decision to reject something, the howls of protest drive me out, and so that's why I really feel it's so critically important to go to expert judgment. I think you have a variety of options under the rigorous process of expert judgment.

You may not have to reject models. You may just go to a weighting process, but I think the real bottom line is incorporating a range of models and looking at their effects and distributions, and if they're not sensitive, then you don't need to worry about them. If they become sensitive, then you use the expert judgment issue to begin dealing with them, or perhaps send it back to the option of saying gather more data to try to help us discriminate.

DR. WALLMANN: I mean, for one reason, you used it because it was simplest and you weren't getting paid for your time in doing it, but I think the other thing is, I don't really think it's going to change the probability of disruption in the repository a great amount if we cluster at Lathrop Wells or if we cluster at Black Cone or in that area unless we can devise a mechanism for generating 20 to 30 kilometer long dikes.

I would be happy if polycyclicity continued at Lathrop Wells. I think it would have zero impact on the repository.

DR. HO: I have a comment here.

I demonstrated that if I throw in two more data set into that data set, the probability risk increased about 42 per cent, 42 per cent. If I change the prior, and then the probability increased about 800 and something per cent. So that is a demonstration saying that model data make a big significant difference.

DR. WALLMANN: I've got to say one thing, Dr. Ho. Your model was the least geologically defensible of any model. You cannot geologically say the events will only occur between Lathrop Wells and the northeast. The probability of something going southwest in that area is just as great.

DR. ALLEN: We only have five more minutes here. I'd like to ask if anyone has some other final statement they'd like to make.

Dave Tillson, for example, on behalf of Carl Johnson, do you have anything you would like to express in a general way regarding this meeting, or anyone else?

DR. HO: Do I have a chance to answer this question?

DR. ALLEN: Excuse me. Out in the audience. Could you make your name known, please?

MR. GIESE-KOCH: Gus Giese-Koch from the NRC.

I've been involved with the probabilistic seismic hazard analyses both in Lawrence Livermore and EPRI. What we came up with in the de-aggregation of the seismic hazard was that the controlling earthquakes for the eastern United States were very close to those earthquakes we used to define the SSEs for all the plans, meaning that the probabilistic studies gave results that were very close to what the deterministic results gave. That gave us a good feeling.

Now, my question is this: If your seismic hazard or volcanic hazard studies, probabilistic studies do not give results that would be comfortable to those people, those experts who say, this is possible, this is not possible, which way will you go?

DR. ALLEN: I'm not making the decision.

Any response to that? Keitti, you've thought about these kinds of things.

DR. AKI: I am not expert in these things.

MR. STEPP: I think, from a regulatory point of view, the power of probability approaches is that you can answer those questions, and you can determine what it means to the outcome by saying, this is not credible, and giving it zero weight; or, this is absolutely the only model, giving it a weight of one. That's what probabilistic modeling can do for you.

DR. ALLEN: I would only say that if you have a major difference within your probabilistic and your deterministic, you damn well better find out why and not go for either one until you have some understanding of why those differences are the way they are.

DR. CORNELL: I would point out that this notion of the deterministic earthquake is not a unique--there's not a unique definition of it. In California--that's what I tried to say in my opening remarks yesterday.

In a California situation, where we have high recurrence rates for earthquakes near the maximum value; that is, you know, what you obtain by looking at the fault length and back-figuring a maximum earthquake, yes, you have a deterministic earthquake, and the earthquake that controls the hazard is the same and, hence, you're going to get out of a probabilistic, de-aggregated probabilistic analysis. You ought to focus on what used to be called the deterministic earthquake.

The confirmation that Gus refers to that we got in the east is, in fact, for a very different situation. There, the deterministic earthquake was, as someone, I think, Kevin described, sort of the maximum historical, plus maybe half of a magnitude unit. It was pushed a little ways from the site because we didn't think it was reasonable to put it right under it. I'm sure Leon will confirm that we know--we have no reason to believe this is the worst thing that can happen, and it was a reasonable judgment, and it was fives and five and a halfs.

If we go to what--if we look at what the experts in Livermore and EPRI studies said the maximum possible earthquakes were in those zones, they are, as has been shown by O'Hara and others at Yankee, a good half a magnitude larger than--that is, what we're saying is that the deterministic max is a good half a magnitude larger than the deterministic design basis earthquake that was being used in the east, and that was quite right because there were low recurrence rates.

DR. ALLEN: What you're saying is--

DR. CORNELL: Now, the problem is we have Yucca Mountain, which is in neither--doesn't fit naturally into either of those two real situations.

DR. ALLEN: What you're saying is that the maximum dreadable earthquake is somewhat higher than the maximum credible earthquake?

DR. CORNELL: Exactly, exactly, and so it's not--I don't know which deterministic procedure you should use in Yucca Mountain, but I think probability will give the same answer.

DR. BUDNITZ: I think the slide I showed yesterday is living proof that a deterministic doesn't make any sense at all. Using deterministic, some of the SSEs were 10^{-3} per year and some of them were 10^{-6} per year, using exactly the same procedure for a six or eight-year period between 1967 and 1975.

DR. ALLEN: Leon Reiter's shaking his head.

DR. BUDNITZ: I know he is, but I'm just telling you that that tells me--and some of them were sites that were only 30 miles apart.

DR. REITER: But, Bob, the greatest impact upon determining the SSE has nothing to do with which earthquake, but the ground motion attenuation, and that's the main reason for the dispersion, not the choice or the size of the earthquake.

DR. ALLEN: Well, I'm going to call it quits here after one final statement from a member of the audience, or a question.

MR. LANG: I'm Hank Lang from the M&O in Washington.

I'd like to get back to the topical report, and, actually make a couple points that, number one, the topical report really helps us resolve issues. It's an issue resolution format to make differences aired, to have the experts say yes, no, maybe, and come to a bottom line, because it drives into the 10 CFR 60 regulations, X, Y, Z, however many, and that feeds into the NRC.

And the topical report will also feed into the annotated outline, which is driven then as the license application end result for the FCRG, the reg guide.

Now, if suddenly we take Paul Pomeroy's suggestion and say, DOE, time out, stop. Use EPRI, use probabilistic, deterministic, bring in the experts, and as I have listened to the experts here, they're going to have to resolve their own issue resolutions, because they can't seem to come to a point. Do we save any time by following your approach?

MR. POMEROY: In the interest of time, yes, I think you do.

DR. ALLEN: Well, in the interest of time, I'm going to call it quits here, and I want to end by thanking, on behalf of the Nuclear Waste Technical Review Board, thanking all of you, particularly thanking the consultants who came here for us, thanking the DOE for being tolerant and allowing us to hold this meeting and take some of you people away from your work on the site. I particularly appreciate the participation of the NRC, which I realize was difficult, in some degree awkward, and for everyone here, from the M&O, and so forth, many, many thanks.

I've personally found this very revealing in terms of understanding some things we know, some things we don't know, some areas where, clearly, we have some further understanding to make.

John Cantlon, as Chairman of our Board, would you like to say any final words?

DR. CANTLON: Well, I would just follow your lead and thank all of the participants. I think these kinds of panel exchanges are really the raw material that go into the Board to make our comments to DOE and to Congress, and I think this last discussion of why seismic lags behind volcanic, it's interesting. We didn't know that in any kind of detail until just a few minutes ago, so I think these kinds of frank, candid exchanges at the end of the panel meetings are absolutely essential to keeping the Board posted so that we don't go off half-cocked.

Thank you.

DR. ALLEN: Yeah. And, finally, let me thank Leon Reiter for doing all the work and many of the ideas that led to this workshop.

Thank you very much.

(Whereupon, at 4:00 o'clock p.m., the meeting was adjourned.)