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STRUCTURAL GEOLOGY & GEOENGINEERING PANEL MEETING

VOLCANIC HAZARDS AND VULNERABILITIES

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Days Inn
88 East Broadway
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SG&G PANEL MEMBERS PRESENT

Dr. Clarence Allen, Chairman
Dr. Donald Langmuir, Member

Dr. William D. Barnard, Executive Director
Dr. William G. Melson, Sr. Professional Associate
Dr. Leon Reiter, Sr. Professional Staff

I N D E X

P R O C E E D I N G S

DR. CLARENCE ALLEN: Good morning, ladies and gentlemen. I'm Clarence Allen, Chairman of the Structural Geology & Geoengineering Panel of the Nuclear Waste Technical Review Board. This is an announced public meeting of this panel. It's being recorded and I would remind those of you in the audience, in particular if you have comments to make as we hope we'll have an opportunity for during the day, to please identify yourselves when you go to the microphone.

Let me just introduce the other representatives of the Board here. One other member of the Board on my right, Don Langmuir, is present. In addition, we have Bill Barnard who is the Executive Director of the Board; Bill Melson who is a consultant of the Board from the Smithsonian Institution; Leon Reiter on my left, one of our Senior Professional Staff; and then, Davonya Barnes and Helen Einersen are helping at the table outside. I don't think we'll go through and introduce individual members or individual people who are here, but you'll be hearing many of them during the day.

The purpose of this panel meeting is obviously not to wrap up the volcanological issues at Yucca Mountain. There are still many things yet to be done, but I think our hope is that we can focus on the questions that are listed in the agenda for the roundtable discussion this afternoon. And, at

3:50 this afternoon and for an hour or so, we hope to have a roundtable discussion that at least initially will include all the speakers during the day and then we hope to also make it possible for others in the audience to participate.

The four questions listed there, I think, are the subjects of primary interest to the Board at this meeting. Number one, on which issues is a consensus developing? Secondly, on which issues are there serious differences? Three, are these issues important with respect to site suitability and public health and safety? And, four, how can these issues be resolved? And, I hope our speakers and commenters during the day will be focusing on those four questions and working towards answers to those items.

I should also point out that at the end of the morning program, starting at 11:00 o'clock, we have a slot there called additional comments from the audience. We know there are several speakers who want to say some things and we will make sure they do. Anyone else in the audience within the time constraints is certainly welcome to make comments at that time.

We have a very tight schedule and I'm going to be fairly ruthless in trying to keep with it. In particular, the coffee breaks are limited to 15 minutes. Now, that's about half the time we've normally spent on coffee breaks and I'm going to try to reconvene even if you people are still back there yakking at each other. Otherwise, we're going to lose control and I think it's particularly critical that we get to these and not penalize people later in the program and particularly not prejudice their opportunity for the discussions later this morning and at the end of the after- noon.

So, are there any other items from the staff to bring up?

(No response.)

DR. ALLEN: Okay. Let's proceed then directly to the introductions by some of the principal players in the scene here. The first one listed is Ardyth Simmons from the Department of Energy, but as I understand it, Dave Dobson is going to stand in in her stead. Dave?

MR. DOBSON: Thank you, Clarence. As most of you know, my name is Dave Dobson from the Department of Energy and this has been an issue near and dear to my heart for a long time. So, I thought we'd take this opportunity to start off by saying a few things.

Just from a sort of Yucca Mountain and performance assessment perspective, I wanted to make the point that we believe very strongly at the project that a performance based probabilistic approach is appropriate for evaluating volcanic hazards at the proposed potential repository during the post-closure period. And, I guess I wanted to make a--this is more of a personal statement than a DOE statement, but I've been watching the probability calculations for about four years now and, having read the literature back about five or seven years before that, I think it's fairly clear that although we have learned a lot in the intermitting period, the estimates, the probability estimates, that we've come up with have not changed significantly with the addition of more field evidence and the conditional probabilities, and you're going to hear a lot more about this from Bruce and his attempts to bound this problem throughout the rest of the day and probably also from some of the other speakers. But, the probability that we estimate generally run in the range from somewhere around 10^{-8} to 10^{-10} and those probabilities, in general, do not include consequence analysis. We think it's pretty unlikely that those things are going to get much higher than that and you'll hear some of the reasons why we think that.

We also believe that the current estimates are reasonable and from a geological perspective, in particular, they tend to be conservative. And, that is to say that every time we do an estimate, for example, of probability, we assume a random distribution of the source of volcanic centers within a given area. We have yet to do a probability calculation that actually considers that the probability is higher in structural basins and valleys than in ranges in the basin and range and we think that's strictly from a geological perspective an observation that lends conservatism to our kinds of analyses. In addition, as I mentioned before, we have not really considered consequence analysis. So, we basically make an assumption when we get to the end. The numbers that you've seen us publish--and Bruce, in particular --have been probabilities of a volcanic eruption through a repository.

On this next viewgraph, I want to kind of preface with a remark and that is that I don't want anybody to take it as an attack or any kind of a personal comment. I'm going to look at John when I say this because there was a quote in a paper recently in the Reno Gazette Journal that was attributed to John and I put it here because it so perfectly illustrates the kind of problem that we've had with volcanism. The quote that was attributed--and I don't even know if it's true, so I'm not making any assertions and certainly John has had plenty of chances, we've had plenty of chances to interact with him--but the quote basically said something like the chance of volcanic eruptions at Yucca Mountain is funda- mentally an unanswerable question. Well, that's a very different standard than attempting to do a probabilistic estimate of hazard. And, if that is true, then that's a very different standard and we need to know that because it would really affect the way we go about doing business. We don't think that it is true. We think we can bound the probabilities at Yucca Mountain from a variety of different perspectives. We have tried cone count estimates. We've tried volcanic rate estimates. We tried basically every way that Bruce can think of and you'll hear some other more original ways of doing it today. But, we do think it's important to try and keep that risk in perspective and you have to include the consequences and the probabilities.

And, so basically what I wanted to say was that we anticipate within the next year we're going to be essentially in a position to start writing down the Department's position on how we go about resolving this issue. That doesn't necessarily mean--in fact, it particularly doesn't mean that the studies are going to stop. There are other reasons. I think it's clear from many of our ongoing activities that there are other reasons that you need to understand the site from a scientific perspective and from a confidence perspective and simply even in a regulatory perspective from being able to adequately characterize the site. But, from a risk perspective, we don't think that there's a very high likelihood that these numbers are going to change unless we start seeing the thermals on Yucca Mountain and 200 degree per kilometer thermal gradients or something like that. We think that's a very small probability.

So, I just wanted to kind of preface the remarks by saying that, first, we do believe that a probabilistic approach is correct and I want to endorse in advance what Bruce is going to say later on today and that won't surprise many people.

Okay. I don't know if you wanted me to talk through the agenda, Clarence. Everybody has it. And, so I think we'll just let it go. Okay, good. That's all I had then.

DR. ALLEN: Thank you, Dave.

MR. CARL JOHNSON: For those of who don't know me, my name is Carl Johnson. I'm the Administrator of Technical Programs for the Nevada Agency for Nuclear Projects. Our agency is responsible for Nevada's oversight of the Department of Energy's program at Yucca Mountain.

The risk of future volcanism at the proposed Yucca Mountain High Level Waste Site is a critical issue to the ultimate determination of site suitability. The DOE in its 10 CFR 960 siting guidelines considers the presence of volcanic activity to be an adverse condition.

To their credit, the Department of Energy recognized in the early 1980's the need to place priority on the study of basaltic volcanism and volcanic features adjacent to the Yucca Mountain area. However, the results of these early studies led to a false sense of security that the hazard of volcanism was not a problem. As a result, the priority waned in the late 1980's. However, here we are in 1991 and the issue of future volcanism and volcanic activity is still with us and, I might also add, alive and well.

The state has long recognized the critical importance of the potential of young volcanic activity at Yucca Mountain and the risk of that activity to public health and safety. It became readily apparent to us in our review of the environmental assessment for Yucca Mountain that the Department failed to adequately address the issue and that their approach to resolution of the issue led to a false conclusion. As a result, in 1985, the state sponsored Dr. Eugene Smith of the Center for Volcanic and Tectonic Studies, University of Nevada-Las Vegas to conduct an independent study of the risk of young volcanism at Yucca Mountain. The approach was a regional one. First, to understand the regional setting of basaltic volcanism and the processes which led to that activity and, second, to define the character-istics of individual volcanic centers and their eruptive history. With that approach as a foundation, then a study of volcanic centers in the Crater Flat area adjacent to Yucca

Mountain could be attempted. Since 1987, progress on these studies have been greatly constricted due to funding limitations imposed by the Department. However, Gene has made progress, some of which has been presented to the Board in the past. Today, Gene plans to update the Board on the progress he's made during 1990.

Late in 1989, the state determined that sufficient progress had been made in the regional volcanism studies that an assessment of volcanic risk could be initiated. Dr. Chih-Hsiang Ho of the Department of Mathematical Sciences, University of Nevada-Las Vegas was funded to develop a probability model of volcanic risk at Yucca Mountain in concert with the volcanism studies that were being conducted by Dr. Gene Smith. Ho has initially focused on understanding the statistics involved in the occurrence and recurrence of volcanic events in the natural environment. His work to date suggests that traditional methods of calculating probabilities of event occurrence may not be adequate for naturally-occurring events over long time frames. And, another point is that we don't believe that presently there is sufficient data by which we can calculate reasonable probabilities. Today, Ho will report on his progress in 1990.

Finally, the state continues to be skeptical about the DOE approach to characterizing the volcanic features in southern Nevada and developing a reasonable probability of risk. The Department's recently issued study plan for volcanic features provides further evidence for our skepticism. The state submitted comments to DOE on that study plan last week. I would recommend that the Board review and consider those comments as part of its deliberation of the volcanic issue.

At that point, that concludes my remarks and I look forward to the discussions today.

DR. ALLEN: Thank you, Carl.

Next is Ron Ballard from the Nuclear Regulatory Commission.

MR. RON BALLARD: Thank you. Okay. I welcome the opportunity to meet with the TRB's Panel on Structured Geology & Geoengineering and discuss with you NRC's views on volcanic hazards at Yucca Mountain. Dr. John Trapp of my staff will be giving a presentation a little later on in the program. I'd like to take a few minutes right now to briefly outline the agency's safety philosophy as it relates to the volcanic hazards issues.

Those of you who attended the waste management conference this past week will find in your copy of the proceedings a paper that describes NRC's overall approach to safety in substantial detail that was presented on Wednesday of that session. I will just briefly touch on certain aspects of its application to the tectonics issues today.

The NRC's safety philosophy is based on the concept of "defense in depth" and can be described as a three-tiered structure, the first level of which requires conservative design for expected operational conditions. The second level is to incorporate redundancy and safety features into the design to accommodate unplanned incidents. By unplanned incidents, I mean such things as perhaps off-site power failures, plumbing failures, perhaps operator errors. The third level requires additional safety considerations for unexpected, but still plausible events, and it is within this third category that issues related to volcanism will very likely be considered.

This philosophical structure takes the form of multiple barriers in actual regulatory practice. Most of you are aware of the reactor situation where barriers include stable fuel form, fuel cladding, emergency cooling systems, and ultimately the containment structure. The repository follows a similar process in 10 CFR 60 by specifying subsystem performance objectives for particular barriers after closure. These include such things as the waste package containment and dissolution controls after the period of containment, and ultimately the geologic repository that requires a slow groundwater motion.

The staff's role in the regulatory process is quite familiar to most of you. It makes use of a wide range of information, primarily applicant submittals, literature, and also alternative interpretations of the information that's collected at the site. The staff positions will typically take on a conservative view when there are issues of public health and safety or of waste isolation. The staff's evaluations, along with the Department of Energy's safety analysis report, will become a part of the evidentiary record that will be considered in the hearing.

A unique complicating feature of the repository program is the duration of the license period, a minimum period under present regulatory structure of 10,000 years. Attempts to project repository performance over such a long time period have introduced uncertainties which have led to the development of the probabilistic standards under which we are currently working.

As DOE develops its technical record for the license application, it must demonstrate that the Part 60 technical standards and criteria are met. The matter of volcanic hazards will almost certainly have to be treated in the demonstration of compliance with the total system's performance standard. NRC staff certainly recognizes the difficulties that will be associated with obtaining and evaluating volcanic data and expects that there will be substantial reliance on expert judgment in this process. The use of expert judgment, I would say, is to be expected in a developmental program such as a repository. However, whenever expert judgment is to be an important factor in a compliance demonstration, it will be important for DOE's supporting analyses to clearly reflect the quality of the data and the reasonableness of its assumptions and the logic behind it's reasoning.

I'd like to make one additional observation on the formal use of expert judgment, specifically on the use of expert elicitation techniques for purposes of compliance demonstration. DOE is using such approaches and I believe they can be very, very helpful when it comes to management decisions for documenting technical data bases and for reducing expert bias in expert opinions. However, they should only be used when there are no other means readily available for demonstration. And, their use, in particular, for combining diverse expert opinions into a single measure for compliance demonstration purposes should be only considered of limited value for the licensing issues unless those opinions of each expert are evaluated on their own merits.

The above cautions notwithstanding, the staff has reviewed one of, my understanding, about 20 or more scheduled study plans related to volcanism. We look forward to the opportunity to review those remaining study plans and I suppose we'll be hearing quite a bit about them today. But, we also are looking forward to the results of the field investigations which we certainly hope will be starting soon. We're all interested in those.

That completes my introductory comments.

DR. ALLEN: Thank you, Ron.

And, finally, representing EPRI, Michael Sheridan. I guess Bob Shaw had hoped to be here, but is not able to be here, and Mike will stand in for him.

MR. MICHAEL SHERIDAN: Bob Shaw would have liked to have been here, but he had another appointment. My name is Michael Sheridan. I'm at SUNY-Buffalo and I will this afternoon present the details of the EPRI methodology for volcanology, but I would like to go very, very rapidly through the back-ground on the EPRI Project because many of you here have not heard this story before, although some of you have. So, let me quickly go through.

The purpose of the EPRI methodology is to develop an integrated methodology for early site performance evaluation and to identify and prioritize the critical issues. And, this first phase of the study was completely funded by EPRI, but

EPRI envisioned further development and not necessarily presenting a total project and certainly not an assessment, but to lay a framework which could be subsequently modified and improved upon to integrate a probabilistic approach to the understanding of all of the problems related to site safety and evaluation.

The team involved in this study was necessarily small because of the scope of the funding and involved participants from various universities and also from industry with different levels of expertise. It was decided to limit the study to the migration of waste through the subsurface water system and gases were not considered, also surface intrusion was not considered, and other factors were not considered. But, most of the major issues were considered, so that a demonstration model could be formed.

The progress in this project went according to schedule with, first, a meeting for brainstorming, selection of the individuals who could work as a team, and then in December of 1989, the problem was defined. And, the problem was defined by using, first, influence diagrams to understand all of the impacters and looking at their relationship and then structuring these impacts into a logic tree approach because a logic tree seemed to be a useful means to evaluate the problem and also to assess the sensitivities of the various aspects of the problem to the migration of radioactive waste from the site. A model was formulated, we put it together, and the model was completed in July/August and the EPRI report is now available. And, if people are interested in obtaining a copy of the EPRI report, please write to Bob Shaw at EPRI and these copies will be available.

The tree that was developed contains several nodes, as you can see here, 11 nodes, and on each node is a branching. For example, here is the flux which is the water that would be moving through the site related to rainfall which was highly dependent on climate. So, a climatologist was involved in this part of the development. Volcanology that we will consider down at this level was integrated behind earthquakes and seismology because there is a clear dependence or relationship between faulting, earthquakes, and migration of magma.

In the volcanic activity, not only the direct effects or lack of them, of volcanos, but the effect of subsurface volcanos on water systems was considered. So, that a rise in the water table, of course, would cause a change in the accessible environment and increase the probability of materials moving through the water system and out. Please, excuse my speed for going through these because I don't have time here to explain in detail.

At each of the nodes that you saw in the previous diagram, there were branches based on probabilities and a critical part of the EPRI approach is to involve all of the models currently existing and to be able to incorporate new models into this program so that all factors could be considered. It's not exclusive. It's a completely inclusive framework for other models to be incorporated. So, EPRI was not a model itself, but it was a framework for other models to be incorporated or even a model for future models to be developed from. And, this is especially true for the volcanology model, but I'm not going to say more about that at this time. But, I could say that all existing models that I'm aware of could be incorporated into the EPRI scheme and the probabilities assigned to various types of scenarios by expert panels could then be used to evaluate the effect of volcanism on the site.

From the various probabilities for events in the whole tree, a series of cumulative probabilities for different scenarios could be developed and, from these then, a cumulative release calculated based on these scenarios. It's possible to change probabilities and then these curves would also change and the cumulative probability would change. And, this is one of the advantages of a probability tree and then it can be used to evaluate changing conditions or new conditions.

Just briefly, this is one of the models using all of the possible branches for cesium-135 and what we can see is some possible paths, which we could say the worst possible cases greatly exceed the current standards now set. But, the probabilities for those types of events are low, so that their effect on the system has to be evaluated by knowing what these probabilities are. Other paths are very low and would not be important. From the point of view of funding, it's important to understand which aspects are critical and where funding should go to resolve issues and which parts of the program are not essential, at least at this moment and can be put on a back burner until the model is further refined.

Giving an example of how this could be used, this is also for the cesium-135, you can see the cumulative probability of escape of this radionuclide into the accessible environment is shown by the probability of normalized release rates and the draft EPA criteria is shown here. For this particular example, the cesium would fall below the standard set and would not be a problem, but we must remember that this type of calculation would be done for every hazardous material in the site to determine if the criteria could be acceptable --above the limit acceptable.

Finally, it's possible using the case of flux to look at the worst case/best case scenarios and see what the effect of flux is on the cumulative probability of the escape and again this is used for cesium-135. And, in this case, we can see that flux or rainfall or climate has a huge effect on the transport of this radionuclide and it turns out that for every one, flux is an important element. So, we can say the climate is a factor that should be studied. And, I can't go into any sort of evaluation. What you must remember is the numbers you see here are numbers from a demonstration model. They're based on data we had available in the literature and there are many, many people in this room with much more data, much better data than we had, and we would certainly welcome the use of improved data in this type of model. But, our purpose was not to solve the problem, but to present a framework from which the problem could be solved.

So, in conclusion, we found that the multi-disciplinary approach to solving this problem was especially beneficial because there are ideas that come from people in other disciplines that are new, and embarrassing in some cases, and very useful in solving this and that a structured approach is required. And, our conclusion was that workshops should be formed to evaluate the risks involved from different aspects. And, as I understand it, such workshops are now underway. They're underway for seismicity and, although this isn't a workshop, this is an open forum, that a workshop in volcanology would certainly go a long way towards resolving this issue if the most respected people in this field were to get together and to resolve or at least to rank the various scenarios in terms of probabilities.

The logistic tree seemed to be a convenient way to represent the data and to deal with it. We found that the results should be iterative, that the method should be folded back on itself. So that we found that the methodology was very successful and we would hope to see a methodology like this one adapted for the future.

Thank you.

DR. ALLEN: Okay. Thank you, Mike.

Let's proceed directly into the main part of the morning program which is emphasizing progress during 1990. You'll note that in the agenda each speaker is allotted 30 minutes. That actually includes time for questions and so forth. So, after 20 minutes, I'll at least make a note and at that point you can decide how much further you want to talk. At the end of 30 minutes we are going to cut things off. We just have to.

Okay, this is Bruce Crowe from Los Alamos, representing the Department of Energy.

MR. BRUCE CROWE: Let me make a few introductory remarks.

I didn't realize volcanism was a contentious issue. This is a surprise to me.

It's been about a year and half since we talked to the Board, and we really enjoyed our last interaction with you and appreciated some of the positive comments you had about our program. What I really want to emphasize today is to update you on where we've gone since we last talked to you.

The agenda for this meeting expanded a bit beyond my original expectations and what I would like to avoid is treating this as kind of a defense of the volcanism program. We really would like to tell you what we've done, where we are going in trying to get on with trying to resolve this problem, and that's the main emphasis of the talk.

In the first part, what I'll be talking about is progress. In a probability talk, I'll talk more about kind of a strategy of trying to resolve this issue.

Before I start let me just show you the second view graph first, because, I want to show you the numbers of people that are involved here and I don't want to represent that I'm the one doing all the work here. And when there is any doubt I would like to refer to the people who are working for me and who have a lot more expertise in some of the items. But, as you are probably all aware, this is a tough project to work on. It's hard to keep good motivated people. And it's really for three reasons that I want to just briefly touch on.

First, there is certainly a lot of sensationalism related to volcanism and it does make it difficult sometimes to keep a proper perspective when you are subject to reporters knocking on your doors and trying to look for nice things to go in the newspapers. This is a pressure we have on us constantly.

Second, what we have really asked everybody involved in this project to try to maintain as a sense of neutrality. As scientists, we really have to look at this issue with no biases, neither positive for the site nor negative for the site. We really want to try to be guided by what is the science telling us about volcanism, and try to be as analytical as we can about the problem. And that neutrality often is jeopardized by conflicts and the sensationalism of this issue.

And third, there's really an issue of burden of proof here that affects us in ways that scientists were not normally affected, and that is, that some of the work that we've done we feel could be easily published in the normal literature without much comment. And yet, we are asking for levels of proof that far exceed what we would call kind of normalcy in science and that often has caused some conflicts. In fact, you'll hear about some of those conflicts today.

But, we maintain that there really is a pressure on us to try to prove things with as strong confidence as possible, and in many cases that far exceeds what we'd normally do for writing papers and literature.

I'm going to cover three things today; the progress we've made since we last talked to the Board a year ago last November; some new work we've done on probability calculations particularly emphasizing trying to bound the uncertainty of our probability calculations; and third, the new work that we've started on, the consequences of volcanic activity, what would happen if a volcanic eruption actually penetrated the repository.

I think the message I'd like to state right up front is that we think we are fairly close to resolution contrary to some of the State comments. We think we really have a pretty good data set. We think we can bound the uncertainty in ways that I think are fairly impressive. If, and this is a big if, you allow us to refer to the geologic record as our bound on uncertainty. If the uncertainty bounds are that anything can happen, this is very thorny problem. But if you allow us to appeal to the analogy of what happens in the geologic record, what happens when we observe volcanic processes, what are the bounds in volcanic processes, we think we can place some bounds in this and we think we are approaching resolution. So, with that, I'll jump into this.

I am not going to try to go through, for time's sake all the people involved, but I do want to show this view graph up front so that you do appreciate that there are a lot of people involved with this. A fair number of them are here. And if we come to questions for some of these specific topics, I'd like to try to refer to the people in the audience to perhaps expound on some of the topics that we are working on.

To start off, let me emphasize the approach we are taking as Dave Dobson mentioned, we are taking a probabilistic approach to this, and the way we have tried to define this question of the issue of whether or not Yucca Mountain is qualified or disqualified is the conditional probability, and we call it the probability of disqualification, PR_{DQ}. And basically the way we have defined that is with three parameters.

First of all, what we call E1, what's the recurrence rate of volcanic events? How often do volcanic events occur?

Second is what is the probability that given a volcanic event that that event would impact the repository? That's what I call the bulls eye ratio, that given an event in order for it to have an impact for direct releases it has to bulls eye the repository.

And third issue is given E1 and E2, what is the probability that the releases associated with a disruption would exceed the regulatory requirements? That's a perspective that all the work that we are doing is structured to this conditional probability.

We divided the work roughly into three areas. The major data collection is in study plan, I'm not going to try to read the number, we call it "Characterization of Volcanic Features", and we provided copies of that to the Board prior to this meeting. And that study plan has been accepted in informal review by the NRC. And as I understand their technical review has been generally acceptable for the study plan.

The probability calculations primarily for the parameters E1 and E2 are in the second study plan, which is called "Probability of Magmatic Disruption of the Repository". That study plan is either there or trying to get to the NRC right now. I'm not quite sure where it is, but it is somewhere back in Washington.

UNIDENTIFIED VOICE: Headquarters.

MR. CROWE: Headquarters, okay. Whatever that means.

And the third one is "Disruptive Effects", which is looking at what are the potential effects of volcanism should it impact the site. That one is about 90 percent written and Greg Valentine here and I are trying to get that out in the near future.

What I want to emphasize on our probability calculations which I'll talk about mostly in the second talk is there's really a two-fold perspective you have to use when looking at this. We would never stand up and say that an individual probability value is a hard, firm value. It's an estimate. And, there is a fair amount of uncertainty with an individual value. But what we feel is that if you look at the bounds that we can put some pretty firm constraints in the bounds. If you are going to tell me that 20 percent, 30 percent, 50 percent uncertainty in my individual value is unacceptable uncertainty, then we would probably have a problem here. But if we look at the bounds and we look at the bounds in a regulatory perspective, then I think that we can make strong arguments that we can resolve this issue.

Our guidance in terms of trying to resolve this issue is 40 CFR Part 191. And basically this is what I call our magic target, currently. And the way we view that regulatory guide, is that this basically states that if you can demonstrate that the probability of occurrence of an event is less than 10^{-8} this doesn't require further work on this particular issue. It doesn't mean that it is licensable or not licensable, it means that as we read Appendix B, it says that this issue doesn't require further examination. And what we will show you in the later session is that we think that this 10^{-8} goal is very obtainable by the calculations that we have.

Okay, what I want to first do is--

MR. DOBSON: What we would just like to note that the 10^{-8} value on 40 CFR 191 is not a failure criteria and of course Appendix B is releases and there simply are some statements in Appendix B to the effect that if you can demonstrate that the probability of an event is less than 1/10,000 over 10,000 years, then you don't really need to consider it further. Like I said it's unlikely that we are going to lay Bruce off next week even if we find that the probability is less than that.

MR. CROWE: That's okay with me, actually.

Okay, what I want to quickly do is go over accomplishments and basically problem areas in the first two view graphs and then we will delve into some of the science of what we've been doing.

I mentioned the study plans and I invite you to look further at those. There's a lot more details than I can really cover today in the study plans.

The major areas of accomplishments that we see is that we've really been wrestling with the geochronology methods. And as you remember when we talked to you a year and half ago this was one of the more thorny areas. In particular, the progress we can report is, we solved what we thought were some pretty thorny problems analytically with doing both uranium thorium measurements and helium. We have some of the first numbers out on those and I'll report those to you. We think we've made a lot of progress in those two areas.

Perhaps some of our most major progress was we've really been very pleased with the results of the thermo luminescence studies or the TL studies as we'll refer to them. And we think that this is going to be a major method in trying to establish the chronology of volcanic events in the region. Particularly the quaternary and towards the younger part of the quaternary events.

Les McFadden who is here has been looking at the use of Carbon 14 dating methods for data particular horizons in soils. And we think that this shows some promise based on his preliminary work. There has been some new K-Argon results particularly in the area of doing $^{40}Ar/^{39}Ar$ methods. And I want to talk a little bit about paleomagnetic studies noting that this work has been done in conjunction with this project, but it wasn't funded by the YMP project and it wasn't done under the Quality Assurance Program.

We finished mapping the Sleeping Butte volcanic center which we helicoptered into in our last trip. We have the maps issued for that and a report is in press. I put it in the package that I made available to the Board. These two items I won't talk about now because Frank is going to talk about these, so I will just skip them for sake of time.

The sixth thing, let me just move this up a little so you can see this. The sixth thing is polycyclic volcanism which we talked about at the last meeting. I'd have to say from our perspective that we do have considerable more data on this interpretation and it does appear to be continually supported by the kinds of data that we have gathered. This is a controversial issue with some of the USGS participants.

I have two sevens here, I forgot why that is. Anyway, maybe it is because I got stopped by QA. I am proud to say and Rich Morely is in the audience here and has dragged me through many times where I complained and he told me that I had to do this for QA and I argued and moaned and complained and he suffered through those and listened to me and made me do things anyway. We are proud to say that we have done quite well in the audits and we have passed all our audits. In fact, we just finished the first audit at Los Alamos without any deficiency reports which we consider to be an item of pride. So we are here to say that QA is a pain, but we are functioning under QA. And, I still maintain it is a pain.

And then what I really want to emphasize in my second talk is we began particularly this year on probability to examine the uncertainty. There's an perception that this uncertainty is just gigantic, it's unbound and we have this horrible problem. And I'm here to say that we don't. And in my second talk, that is what I want to emphasize. I'll save the thunder for that talk.

What are our problems area. Well the quaternary chronology problems are clearly problems. We've gone slower than I would have liked to have gone. We think we are making progress but it's been painful and often not as easy as we'd like it to be. And, what I've found from my perspective, I've tried to maintain some sense of neutrality as a PI, because I don't actually make the measurements. But, I've found that each person that makes the measurements really stands by their measurements, and when we question them, they take it quite personally which is probably what they should do, but it does make for a very interesting task here.

We have some disagreements over data interpretations and in large part, I think those disagreements are because we are really asking for as conclusive of data sets as we can gather. And I think some of the data sets that we are still questioning would certainly be accepted in the standard literature without question. But because this issue relates to Yucca Mountain, we are asking for standards of agreement and converges of data that are certainly above and beyond anything else that I think exists in the literature.

There is also an issue of conservative versus conventional. We really are trying to err if we make errors, we want to make sure that we err to the conservative side so that we don't underestimate volcanic risk at Yucca Mountain. We have had some, because of these concerns, we had some personnel problems with the USGS people involved with this work. I don't want to pretend that we haven't, and I think that the Board needs to be aware that we have had some problems. You'll see some papers in the handouts that we gave of basically disagreeing opinions particularly about the chronology of Lathrop Wells and I welcome you to read those papers. I want to make sure that you are aware of what those differences are. I think the differences are not significant, but I want you to see what other people's views are.

Quality Assurance software has been a problem area. Particularly, poor Frank Perry has not be able to get an analysis of a volcanic rock for what, a year and a half now, I think. And it does make it a little hard to do geochemistry when you don't have any geochemistry data. He has some new data, but we have been slowed down by this. We think we are close to resolution, but I've been hearing that since a year ago February. And it's always been, like, a month or two away. That has been a problem.

Our most serious problem is, particularly for, Lathrop Wells and Sleeping Butte where we have some chronology problems, if we can get in there and trench, we think we can solve the problem within three months. And we really think that a lot of the contentions and the fights would end if we could trench. It's an emotional issue with me. And, I don't know what we can do to get through this. You are probably aware of all the politics; the State is basically stopping the permitting for trenching. If we could trench, I really feel three months might be an over-estimation, but certainly in six months we could gather the kind of data that would really rigorously test the different models on chronology. We simply can't gain access.

On the positive side, what I'll show you next is what we call the truck which is basically a bribe to Les McFadden to keep him into this program. It's pretty hard to keep a soil scientist in a program when you can't dig a hole. And Les has patiently been waiting for three years now to dig a hole. And what I want to show you basically is what we have to keep Les going. We do think though that the success of the TL technique in particular which is very dependant on the trenching has been good enough that if we can start trenching, we can really begin to crank the work on this.

Here's what we bought to keep Les happy. This is what we call the truck and we joke that as part of having this role we are all going to start chewing tobacco and getting tattoos for using this out in the field and trying to grow beer bellies. But, anyway, what this is is basically a four-wheel drive mounted back-hoe. And for the kind of problems we are dealing with where we are trenching in very unconsolidated alluvial sediments in blown eolian deposits, we can go in with this piece of equipment and we can excavate the bottoms of lava flows and apply our dating techniques very, very rigorously. This thing is warmed up, it's fully gassed and setting in Albuquerque and we are desperate to start to work with it. We think we may permission to start to work in the Cima Volcanic Field in California. And we are hoping actually this month to start work with it, but this thing is ready to go.

Okay, let me jump into the actual geology. This is the good old Lathrop Wells volcanic center. The geology of this has kept us all interested, occupied and entertained now for a lot of periods of years. I don't want to go into all the details. We helicopter landed at about this area right here (indicating), and walked over and looked at some tephras in the sequence at our November meeting. What we basically see here is we can divide the geology of this center into three packages. A group of aligned fissures we call the Qs_5 unit here and associated lava flows called Ql_5 form this package here. We think these are the oldest units at Lathrop Wells. We don't know how old they are. We have some suspicions and we have some arguments over how old they are, but we do have general consensus that they do see to be overlain by the other deposits here, so this is part of the older.

What we have identified and particularly Frank Perry's data has emerged is that there is a unit that we are now calling Ql_6 which we think may be the oldest unit here and I'll show you in a later view graph what that is. We think that this may precede the Ql_5. We don't have much data other than geochemistry on this unit.

There are two packages of lava flow sequences out there, what we call Ql_3 and Ql_4 that are related to two fissure systems, a fissure system here (indicating), and a separate fissure system here (indicating). What Duane Champion has shown is that these have about the same field magnetic direction and we think it makes some sense to lump those, although there are some new chronology data that casts some suspicions on those, not enough yet that we would get very excited about that. So we would lump these two as a package. And then what we see is we think the growth of the cone has post-dated all of these, not the entire growth of the cone, we don't know exactly how the cone has grown, but certainly the deposits from the cone overlie Ql_5 and there are airfall deposits on top of these lava flows from the main cone.

Associated with the main cone is a pyroclastic surge unit that we didn't look at, but largely curves to the northwest. Now one thing that has emerged, we showed you the tephra deposits in this general area right here (indicating), since we last saw you, there's been considerable more quarrying at Lathrop Wells particularly in this zone. What we've been able to do because of that quarrying is that we now feel that these tephras which we through were from the main cone probably are not from the main cone. They have essentially explored some quarry areas here and we think that probably these came from a separate side. We are a little bit worried that they came from a side that has not been quarried out. And I'll show that actually in the next view graph here.

Here is a picture of the south flanks of Lathrop Wells. Where we looked at the tephra sequences right here (indicating), we originally thought that it came from this event, but now they have quarried out extensively this part of the cone, and we find no signs that those tephras trace over to that part of the cone. And in fact, we are now suspicious that this area that is all removed may have been the source for those tephras for two reasons. One is that in talking to the quarry people, what they look for is very, fine grained tephra and there probably is a good reason why they did a lot of quarrying right out of here. This is the first target that they went into. And then second, what we are now doing is we've committed a person from SAIC to look at and try to find historic photographs of Lathrop Wells to try to figure out what this thing looked like before it was quarried. And we think that that may be a key part of trying to understand how this field has evolved.

Now one of the controversies over these tephras that you'll be hearing about is some of the USGS people feel that these are debris flow deposits derived from cone slope erosion of the cone. And what I would like to point out from this view graph is, what we have reiterated numerous times, these deposits are out here (indicating), first of all there is no cone slope apron on this cone. There have been no signs of debris flow deposits affecting these, so it is a little hard to propose apron deposits when we see no apron deposits. Second, you'll notice that there is absolutely no evidence of debris flow processes operating in the cone, so we feel this is a fairly difficult hypothesis to present when there is no evidence for it in the geologic record.

Okay, again, here are the tephra deposits in the quarry section we looked at, for time I am not going to say much about them, but in our packet that we handed out you'll notice that we wrote a geology paper and there has been a comment to it that we replied to. And I want you to be aware that some of the survey component think that these are not airfall tephras. They are not primary tephras. They think that they are reworked tephras. And I just want to show you that we still feel very strongly that these are tephras. There is an important record here of multiple soils in between tephras. And, I don't want to belabor the point of why we think they are tephras, but we are quite confident. I would refer you to that comment reply section in the packet that we handed out.

Noting here in a close-up of the tephras that they show classic airfall tephras. They have basal contact, flat basal contacts that drape the topography of the area. We can see that they have inverse grading, they have unsupported fabric. There is some confusion about the presence of silt in these materials. That silt is infiltrated silt from less deposits on top of these. They are not fine, grained ash. They are introduced largely quartzofeldspathic components from eolian activity.

One of the key features that we see on the north side of the cone, this is the north flank, in particularly this is a little bit light to see this, but this is the main cone and what we see--can we dim the lights up here a little bit? Is that possible to do? All right, does anyone have control of the lights? Could we try to just dim it a little bit up here so we can see it better? Anyway, I'll start talking about it.

What we see, this is the main part of the cone. What we see up in this part of the cone is there is an erosional surface here and if the lights were better, what you can see is there is rilling in what we think requires a time gap between these deposits in the main cone. And this is the evidence for it right here. I have an air photo that I brought along, and this is very hard to see. I can show you on the air photo how obvious that evidence is. But, this is a key bit of evidence here. This is what we think are some Qs_5

units, and the cone overlies them and there is an erosional surface here. This is a key target of where we want to trench. We think this will contain some key bits of evidence for trying to resolve this issue of the age difference between some of the units.

And then I just quickly wanted to show you some lava flows on the north side. Here is the side--this is what we call a Ql_4 here which has been--there is paleomag data on it. We've got a uranium-thorium disequilibrium age of about 150,000 years, and we just got some brand new helium results in of about 40 to 50 thousand years. We have a TL date on a baked soil underneath the Ql_3 over here of about 25,000 years. And these are all new data that have been gathered since we last talked to you. I am going to just kind of go over the data quickly and then I'll kind of critique what we think about interpretations of it in some summary view graphs. And again, this is the southwest flanks of the cone.

What is important here, I want to point out this lava flow unit right here (indicating). This is what we call perched flow and Frank will be talking about it in his talk. What we think is important here is we have been able to excavate with a shovel the base of this flow, and if you look at the modern pavement surface or the present pavement surface out here, where it is not modified by the road, this unit sits about three feet about that modern surface, whereas we find that all of the other lava flows particularly the Ql_5 are ingrade of that surface and we suspected this could be an older part of the sequence here. This has not be dated by any method. There is no paleomag for this flow and we think it is a key part of the record that needs to be investigated.

Okay, we can probably--well let's leave the lights low because I have some other view graphs.

I don't want to try to go into all the K-Argon data, but you have papers in your package that certainly make the case. What I tried to do is just put together a summary table of the K-Argon results. They are varied; they are assorted; there are some controversy over the results. I think all I'd like to emphasize is that if you look at the ranges they are quite extreme. We've got everywhere from negative ages to ages exceeding 700,000 years by some laboratories. Brent Turrin and his co-workers have made some arguments that they think they can extract a mean age out of this data set that their best guess, as I have emphasized in the bolder numbers here are variance weighted methods of about 137,000 or about 160,000. This is their latest results here.

The State has dated one locality, Gene, do you know where that sample was from?

MR. GENE SMITH: Ql_3.

MR. CROWE: Ql_3, 67,000--this is done by Arizona, the University of Arizona. They have some new Argon $^{40}Ar/^{39}Ar$

results, with these sorts of numbers and they would like to emphasize these particular values here which are attained by the variance weighting. We have obtained a number on Ql_4 of we would call it greater than 120,000 years. The isochron is actually 150,000. I'll show you that in a second. And, to emphasize the positive sides of these data, what I think is really interesting is despite the broad range in the ages, there is a reproducible mean value both with $^{40}Ar/^{39}Ar$ with the K-Argon that's intriguing. I mean, these have been dated over a period of about a decade and the ranges still are large, but if you use various ways of weighting it and I prefer really a mean value versus a variance weighting. I'll talk about that in a second. But there is certainly a reproducibility of a mean value here somewhere in the 120 range for these lava flows, 120,000 years. And there is some possible confirmation from the uranium-thorium and soil tephra correlation, so these are positive points to be made from the K-Argon side of things.

Now there are other sets of data that since we last talked to you, Fred Phillips has done some ^{36}Cl work. What is interesting about these data is they seem to cluster into two poles. Either they are older than 100,000 or much younger than 100,000. Fred Phillips' Chlorine 36 is the only thing that comes in the middle, and we find this entertaining. These are numbers that Fred gave to me by the phone. I understand he's at a Penrose conference. He's moved these up just a little bit more toward 105,000 to about 60,000 in his lower range. But I just want to show you these values to show you the kind of range in values that we are extracting out of these kind of lavas.

What we think is real important and I'll show you in a later slide is that Jane Poths has recently produced some Helium dates. The helium numbers are on--this is the top of the cone. We did an experiment where we said, okay, let's go to get the best surface analysis we can of Ql_5, Ql_3 at the top of the cone and let's see what their differences are. These represent the top of the cone, I'm sorry, the Ql_4 lava. And the numbers are like ±20 to 25 percent. But what we think is important is that there does appear to be a difference between those two units that is reproducible by helium.

Then perhaps the most interesting results we've gotten is Steve Forman has done quite a bit of TL work. Steve couldn't be here. He's moved to Ohio State University and he is midway through his move right with this meeting and we decided that we would allow him not to come. But, in the quarry section of the tephras, we dated the lower soil down there and we get reproducible numbers around 9,000 to 10,000 years. We have day-to-day baked contact underneath the lava flow, the Ql_3 at 25,000 years. So what we think about these are basically that we do get good agreement between the TL and the helium. We are not in the position where we would argue that these are vastly differing numbers here. We think that they are surprisingly good agreement.

We don't know what to say about the chlorine and it is kind of a little bit unfair to put Fred Phillips on a spot here, but right now, particularly this oldest number comes from a surface bomb that we have no idea where it came from. It's just, Fred grabbed a bomb that was setting out on a bedrock surface and we don't where it came from or what it is or what it represents. So, it is a little hard to try to tie it into the stratigraphy, whereas these two numbers we can tie to the stratigraphy. This one is from the top of the cone, this is one from Ql_5 lava.

Okay, what I want to quickly show you here is this is just the isochron that Mike Morrel has obtained using uranium-thorium. And what I really want to keynote here is that Mike has done some remarkably great work. I mean if you take the conventional way of doing these kind of analyses, this cluster of points here, if Mike did conventional alpha counting, this would be one point and the analytical uncertainty would overlap, you couldn't do anything with it. It is only the new precision that he's got out of using these solid source mass spec that allows us to see this.

The good news is that Mike has overcome the analytical problems with doing these measurements. The bad news is that the rocks have not be cooperating the way that we want. The spread of uranium-thorium values, the ratios across here are not as much as we'd like. But, Mike, nonetheless can extract a 150 isochron from those. We are currently debating really what that means and it's part of this kind of bi-polar distribution or bimodal distribution that results. But I think we have to say that this is a real value, a real measurement, and we can't explain it away currently by any other process. It seems to represent something that may be real. I would like to think that it might represent some sort of inherited chamber edge for this, but Mike convinced me last night that that is not a valid interpretation.

Here's a little bit more information on the helium. Two bombs in the summit have given reproducible numbers. We are quite pleased by the plus or minuses you are seeing on this. This is a good reflection of kind of the analytical uncertainty that is available with this helium technique. We are very pleased and these are very reproducible numbers in our opinion from the top of the cone. And what we would emphasize the most is that the helium would suggest that the top of the cone is a different age than the lava flows, and I think that is a pretty firm conclusion from that. We are a bit older than the TL results, but nonetheless we feel that this is substantial progress that we are making on the helium results.

Let me expand on the TL a little bit. One thing that we have done, we sent Steve Forman up to Snake River Plain to try to kind of test the TL technique. And what he did is he looked at ^{14}C localities and the K-Argon localities. And he went to places where we had good soils data and either ^{14}C or K-Argon and then he's been able to remarkably in fact, reproduce the numbers from those localities up there, which gives us some confidence that we are heading the right direction on TL. He's also done some work down at the Cima Volcanic Field. In particular looking at the A Cone flow, which we stopped at our trip last November. And here we have a real clear discrepancy emerging between K-Argon in other techniques. What we see is a variety of techniques converge on numbers, we like to say kind of in the 10,000 to 20,000 range. And what Brent Turrin gets is $^{40}Ar/^{39}Ar$ of about 110,000--I'm sorry, about 119,000. And what everybody else feels strongly about is, you know I've talked to Steve about this at length, he says I could be wrong by a factor of 2, maybe even a factor of 5, but not a factor of 10. And he feels that we have a very clear dispute. We are impressed by the convergence of these techniques and we suspect that there may be an unappreciated air term of K-Argon that is beginning to emerge as we do these studies.

The same thing is developing for the I Cone lava flow, which is one that we didn't look at. It's another younger flow but we have helium results from two independent laboratories; one from Australia and this is Jane Poths' results from working in her laboratory. We are impressed that they independently have reproduced the same number of about 19,000 to 20,000 years. Again the plus or minuses are about at least 20 to 25 percent, probably 5,000 to 6,000 is a better number to give for a plus or minus there. Seeing the same number the K-Argon gives numbers of about 100,000 a year. So, again we are seeing this factor of about 5 difference between other techniques and K-Argon.

In your package I just showed this letter from Steve to me summarizing his results. I just wanted to give that for completeness to you. I don't think I'll bother talking about it.

But, what I do want to point out is we have some calculations that Greg Valentine has done for us. What the TL technique is dependent on, in order to tie the TL technique into geochronology, you have to have an event that resets the TL clock. Basically you have to burn off by some method the earlier TL signals as acquired from gamma decay of uranium-thorium and potassium. And what we had been hoping would work was by baking out that contact, by lava flow contacts, and what Steve has shown in the laboratory is when he heats samples at 300° he gets a very flat TL signal and can show that these are very baked out and zeroed samples. And what

Greg has calculated here assuming different thermal diffusivities, is what the position of the 300° isotherm is with time. And what you find is that the position kind of goes up above the same spot, it just takes longer depending on the diffusivity. But, the point is that at Lathrop Wells, we sampled at 30 centimeters from the lava contact. What we show is we are looking at 3 meters or so position of the 300° isotherm.

And similarly Greg has done some calculation looking at lava flow thicknesses, different emplacement temperatures, and again what we see is that we feel very confident that the samples we collected for TL are well above the 300° isotherm and so should have zeroed that signature and should be reflecting the emplacement age of the lava flows.

Okay, now what I want to do is editorialize a bit here. The position that we have is that we are not going to argue that we know the age of Lathrop Wells yet. We basically have three working models of what it could be, and what I want to try to do is just talk a little bit about the strength and weaknesses of it. Here's the first model that I think the survey people as you read their papers would emphasize. This is a simple monogenetic center and all the units are greater than 100,000 years. And the evidence for that would be the K-Argon including both conventional in $^{40}Ar/^{39}Ar$, uranium-thorium results and they have a tephra correlation that is described in their paper. We would consider it kind of neutral to be a paleomag. The paleomag just tells you whether units can be clumped or unrelated. It doesn't really give you an age of anything. And we kind of put the ^{36}C just floating in the middle saying, you know, ^{36}C can go either way, which is kind of nice, I guess.

And then the Con evidence would be the geomorphic soils, the TL, the helium, the stratigraphy--I don't want to read all those off. And here is kind of in terms of how it relates to the project, here is the strength and weaknesses of those. The strengths are that a monogenetic classification of these kind of centers is the classic interpretation from volcanology. I mean that has been the strength of what volcanologist have called these things for years. And it kind of sets with conventional interpretation.

You could argue that the lack of convergence of chronology methods may simply be that these are developmental. These are brand new techniques that we are doing a lot of kind of the pioneering work, as well as, other people are at other places. But the dispersion of the data may simply be that these are brand new and we haven't tested them well enough yet. Certainly the K-Argon has been the most accepted dating method used in the literature for decades.

A key thing is that if we take this model, it really simplifies our chronology studies. We basically can almost tell you that we are done if we take that approach, and there is a certain amount of satisfaction to taking that approach. There's some appeal from this. And in fact, this would have, for most of the models, this would have the minimum impact on Yucca Mountain in terms of volcanic risk. This basically says that this is an old event. And at least a pretty conservative assessment of volcanic hazard.

Now, on the weakness side, we feel that this ignores the major group of data here suggesting that there could be a younger age. It requires special and presently, we feel unspecified explanations for the lack soil development, the lack of geomorphic modification and particularly, the TL and helium results. The tephra correlation that that's been used, we regard as unproven, and in fact, we've examined the tephra. We can't tie it to any unit at Lathrop Wells. The tephra that they are using is 5 kilometers from Lathrop Wells and, in fact, we think it may be a re-worked tephra from some older units and completely unrelated. We've done PIXE and microprobe work on those tephras and our current conclusion is we don't think it is from Lathrop Wells. If it is from Lathrop Wells it is not tied to anything in the stratigraphy of Lathrop Wells.

And finally, we kind of argue that you have to be very careful about getting what we call a false positive, that in terms of the volcanic hazards we don't want to make the mistake of thinking that the cone is old when it is really young. We can err to thinking it is young when it's old, but we think in terms of hazards it is a potentially dangerous error to think that it is old when it is really young.

The key thing is that we would point out for the K-Argon in particular is that there really is a proof of principle applied here. We believe the data set is a good data set. There is nothing wrong with the measurements. We have complete confidence that they were done well. They were done to high scientific standards. What we disagree with is how they are variance weighted and how they get a mean value. And we regard that nobody has really rigorously tested the meaning of an average number. Is it meaningful to average multiple analyses? Does that weighted average or does average mean more than individual analyses? And that's something that needs to be proven.

Now, the problem is, what do you test it against, and that is one of the major problems with this whole thing. We don't have any absolute golden spikes to tie our chronology to. But we will regard that averaging is an interesting approach but not proven. And then we would have some minor differences in map units, but I would underscore that if we could get it entrenched, we can resolve those very quickly.

Model II is a simple kind of a compromise model. We would say well maybe things are young, both young and old out there, that this is a polycyclic center, meaning multiple events and that some events are old and some are young and that is what is explaining the kind of polarity of the chronology results. We see some results in the stratigraphy that do support this. This interpretation, we don't think has a major impact on volcanic hazards. The weaknesses we see for this model, other than that it keeps people from fighting with each other, is that we don't see two distinct groups of soils, but we caveat that saying that we haven't been able to dig. And, so we haven't looked as comprehensively as we would like to look for those soils. And we have to also say that there are some geomorphic differences between the units, particularly the Ql_5 and the Ql_6, but they don't appear that significant and so we are a little bit uncomfortable with saying that. And it requires again this polycyclic model is an unconventional interpretation of these kind of centers. In terms of areas of disagreement there are none, but trust me, we'll find some the way this project goes.

And then here is the third interpretation, and that is that this is a polycyclic center, that all the units are young and by this we would regard the K-Argon as basically saying and a number of chronologists that I've talked to would present the same interpretation that they would argue that trying to date rocks younger than about 200,000 to 300,000 years, K-Argon is not the method to use, that you are basically going to be getting analytical noise when you are analyzing those, and there is, I would only want to classify it as the majority or minority, but there certainly is an opinion that that is true, that this is the wrong technique to apply for this problem.

And if you take that approach, we would then argue that the combination of the geomorph soils, TL, stratigraphy--Frank will talk about the geochemistry, even the paleomag data in these tephra-fall units between the soils that we showed you on the trip, would argue that the cone, the composite cone is quite young.

Again the con, the one that I want to emphasize is that we have no reason to expect uranium-thorium value is not a real value and we can't give you any explanation of anything wrong with that, other than we would like to have a better spread in the uranium-thorium values.

And so, the strengths are pretty much what I have talked about. One of the things that Frank will be emphasizing is that these are pretty strange basalts. We have strong evidence in the geochemistry that Frank will talk about that they have two-stage ascent history. They clearly had to pause about the crust mantle boundary, fractionates substantially and then ascend rapidly. And we think that that possibly could be one of the reasons why we are getting such variable results. We don't know, it is very speculative.

But, this does identify a potentially sensitive area of volcanic hazard assessment, and in the next view graph, I'll talk about what that is.

What are of the weaknesses of these interpretations? Clearly, this polycyclic interpretation is an unconventional interpretation of these kinds of centers. And, we'd be the first to say that they are. I certainly was skeptical originally, but I think the bulk of the data that we see emerging does seem to support this. We really are flying in the face of this apparent convergence of K-Argon, uranium-thorium. As I mentioned, I am not strongly impressed with the tephra correlation, but nonetheless, we really have to say that K-Argon has been the standard method used in science, and we are really kind of flying in the face of that standard method, and you have to keep that in mind.

Alternative uranium-thorium disequilibrium models for that 150 isochron have not been tested, and Mike feels very strongly are probably not right. And the main thing is we really need more trenching. I mean we really feel that we could solve this problem if we could go out and trench and get like eight or ten TL measurements, eight or ten helium measurements; do the soils; et cetera; we think this problem would emerge. So, what we really want to emphasize is that we are still into this thing and we don't have any clear answers here.

The areas where we really disagree and the disagreements are primarily with the USGS workers. In general the State and the NRC have both been--have accepted it. I mean they probably won't accept it because we are saying it is young, but nonetheless we don't have a disagreement with the NRC. And I don't believe we have a disagreement with State, and Gene or Carl can correct me on that if they would like to.

But, I think the USGS people feel that they don't believe the TL results and they feel the soil and geomorphic correlations are incorrect. They disagree with the interpretation that these deposits we showed you are fall deposits and they disagree with the concepts of polycyclic volcanism. So those are areas that we still disagree over and probably will continue to disagree.

There are academic arguments, and I have to honestly say that having gone to the probability calculations when I asked myself, are these differences important, what is the difference between say 40,000 and 120,000, it's not that important. What we see is that if we use simple event counts, we've already counted this is an event, and so the chronology doesn't change it. If we use volume time plots, what becomes important is where the chronology differences fall. If some of the small time events, the younger events, it really is not important at all in terms of our rate calculations, if the whole system out there is say younger than 20,000, then we might get some sensitivity in our rate calculations. It basically is a slope of a volume versus time calculation.

But, I have played around with the sensitivity. We are talking about sensitivities in the order of a factor of 2 to 5. And as I'll show you in the later talk we are willing to accept that. It's the bounds that are important. What is important though is this polycyclic model, for this very simple reason that if you believe the polycyclic model, it leads you to two predictions of what might be the most likely scenario future volcanic activity. If the polycyclic model is correct, we would argue that the highest probability of event that might occur in the next 10,000 years or in the future, would be a recurrence in eruption at either Lathrop Wells or the hidden cone of the Sleeping Butte centers. And we don't have enough chronology data that we can say what the probability is, but in over 10,000 years we would regard it as a relatively high probability. It might be somewhere between 1,000 and 100,000--one in 1,000 and one in a 100,000, but it becomes large in terms of 10,000 years. We can't quantify that until we resolve our chronology models.

The important thing is that we would argue that that has no effect on Yucca Mountain in terms of coupled effects and I'll talk about that in the later talk.

The other issue that we'll be talking about from probability is that the key issue we think in terms of impact on Yucca Mountain is what is the probability of a new volcanic center forming? An eruption in an existing center, we think can arguably be demonstrated; it has no effect on Yucca Mountain.

So here is where we stand on these things. We want to continue to work under chronology and polycyclic events and we basically really have kind of a yes/no approach to this. If our polycyclic model and our young chronology is wrong, we have really simplified our risk assessment, and we have a lot of confidence that we can come to you and say we are basically done, that we don't too much more work that needs to be done , and so this has a lot of appeal in some respects and would be nice to bring this issue to bed.

If the answer is yes, then we really have to do a little bit more testing of our alternative models of why do these exhibit multiple eruptions, what's the recurrent rate of those eruptions, and what is the controlling mechanism? Do we have a lot of confidence that these things still erupt at the same sites? What's driving these? Why does it behave like this? And that is the uncertainty that we think still remains in the chronology of things.

Okay, let me just quickly go through a little bit on some of the other work that we've done. I mean, as you can see, Lathrop Wells is probably near and dear to the hearts of a lot of us here. This is just a quick geologic map of one of the two Sleeping Butte centers up about 47 kilometers to the northwest of Yucca Mountain. And here is one that we don't disagree over, which is kind of nice. This basically is a simple scoria cone, lava is vented from multiple flank sites in the vent from a site here, site over there, it has a nice cone slope apron shown in the blue. There has been some K-Argon data on there. The Argon data shows very large errors, but the numbers tend towards the 200,000 to 400,000 range. The soils that we have looked at, that Les has looked at, the geomorphic degradation all seems to fit roughly with that 200,000 to 400,000 year assignment. We don't see much problems there. We even could agree that this could be classified as a monogenetic center, although we note that some of the deposits that we are interpreting at Lathrop Wells to indicate polycyclic activity have been removed there so we may have lost part of the record, but there basically is no dispute over this center.

Here is the second center, Hidden Cone, we are calling it, and we landed in the helicopter just to this side and walked up to this locality here (indicating). What we see in a large part is this is very much like the other center, a typical Strombolian cone, vented lavas from radial dikes that come out from the flanks of the cone in multiple sites. There are three sets of lavas here. But, what we do see here is there is a blanketing event that really puzzles the heck out of us. This blue unit. What we see out in the flanks and in the cone is an older unit that has a good soil developed on it and again the K-Argon numbers are about 200,000 to 400,000 years and the soils are consistent with that in these flank deposits, but we see a draping event that we simply don't understand. The geomorphic preservation and degree of soils in that would indicate to us that it is a much younger event. And let me--this is kind of a washed out slide, but I want to at least show you, this is good to have a washed out slide, maybe.

What is puzzling to us, is here is the actual contact between what we think are the two units, and what we see is there is virtually no rilling, there is a very poor soil developed on this part of the thing, and yet down here we have a good soil and this is actually part of a cone slope apron that is not part of the original cone. It's part of an older cone that has been eroded and transported down to the bottom. And there is a marked contrast. Now, we need to go in and trench here, but what Steve and Les have pointed out to me repeatedly is, once you start to form a cone, that all the processes that start affecting it, lead to rilling and development of topography on this cone. There is no process that we could envision that would lead to smoothing. And therefore, we find no way that we could explain the geomorphology of this in the soils of this unit without appealing to multiple events. We don't know what the age of the second event is, but we are basically asking for an event that came from the same vent and draped but didn't completely covered the whole vent. And we think that it has to be a strong age discrepancy between the main part of the cone and this later event. So, again, evidence of this polycyclic history.

I think I am going to skip this second view graph in your package here and move down to the A Cone in Cima. What we've seen since last talked to you, we've done some very detailed work now down at Cima. Now we have been able to identify three, clear sequences at Cima. This is part of an older mass that has been dissected enough that it exposed dikes in the cone mass here. We expect that this has to be older than about 500,000 years. There you see this sheet of scoria fall deposits here, it is extensively rilled. We have traced that to a source here. This is an old spatter cone formed right at the summit of this older cone and then breaks out on this side by erosion. And we see a soil on top of that. And here's the latest event here which form the main mass of A Cone. The only point I want to emphasize here, we are working out the details through TL soils of the sequence here, but what we view is just completely unequivocal evidence of three events at this center and in support of the polycyclic model.

And then to quickly take you through G Cone, which we waved our arms at in November off in a distance, what we've puzzled over, in fact one of Steve Wells' students was the first to see this, this is G Cone and if you look at it, there is a segment of the cone right here that has less rilling than the other part of the cone off in this direction. And we speculated that this could be a small fall event that was younger than the main mass of the cone. And what we found once we climbed up here, if you notice there is an asymmetry in the profile of the cone, there is a bulge right up in this area, and coursing deposits. And what we discovered when we climbed up and looked at this, is in fact this is agglutinate, and this is core spatter infilling the vent and what we find is there is a soil underneath this on top of what we think is the older vent there. And this is what we've been proposing, if our polycyclic model is right, we've been proposing that this is what we should see, that you should see a surface with a good soil and then more deposits on top of this. We haven't done the paleomag which we think is important for this site, here, yet and Les has not described the soil. Also, one alternative interpretation that Steve Wells noted is the possibility that this soil could be an influx of material down through this course of agglutinate and accumulating at a finer grained locality there.

DR. ALLEN: Bruce, you've been talking for 50 minutes, so in another ten minutes we are going to have to go onto the next speaker.

MR. CROWE: I'm just about done. I think I have two left and I would like to have time for some questions.

I want to just mention Duane's paleomagnetic studies, because, what is important here is particularly that, well, he has done Sleeping Butte and 1.2 and 3.7 million year old center at Crater Flat. He finds that they have grouped directions, that there is some evidence of two sets of directions that maybe the center is much like what we see at Lathrop Wells, but they appear to be clustered. And he has argued that these represent that in the clusters, say the four cones at Crater Flat, that they look like they could have formed almost simultaneously or with small time intervals. And this is important because it could lead you toward a model that would basically have clustered volcanic events. Very analogous to say clustered seismicity that when an event occurs it is not one simple event, but it is a cluster, and we would argue that you might have what we would regard as episodic events. Long periods of no activity and then when activity occurs it occurs in a burst of activity where multiple centers form. And this becomes important in some of your models of how you would accumulate and use probability calculations.

Here is my last view graph and I hope I have some time for questions. To summarize basically, we feel we've made progress. We are still puzzling over the chronology, but the bottom line is that perhaps these inconsistencies we are seeing are expected with these kinds of new techniques. We are sufficiently encouraged that we think that we are making enough progress that these are solvable problems. We don't regard these as the imponderable questions.

We think that there are some exciting scientific spinoffs if either the K-Argon is right or the geomorphic and soil processes are right. Either way, it leads to some interesting scientific conclusions that are largely beyond the bounds of the Yucca Mountain issue, but that are quite interesting to the program--to science in general.

The polycyclic model, we feel, continues to be supported. The USGS workers do not, and you should be aware that, they feel quite strongly that it is not. It is important in our opinion in looking at potential future volcanic scenarios.

And then finally, this cluster model could become important because it deals with how you would count events in doing probability calculations. I'll stop there and hope we have time for a few questions.

DR. ALLEN: Okay, thank you, Bruce.

First of all, let me ask if there are questions from Board people here or consultants or staff. Okay, Bill.

DR. BARNARD: You have used several different techniques to date your samples of various investigators. Have you ever taken one sample and split it up into ten parts and everybody date it using their own techniques?

MR. CROWE: That has been done with the K-Argon technique that I didn't talk about. Scott Sinnock in 1982 did a blind testing of the K-Argon technique, where he sent the same sample to three different laboratories and reported results and we got remarkedly wide results varying from 80,000 to 500,000 for those.

We haven't yet done it--well, we are converging on that with the techniques we are using, but no we haven't rigorously done tests were we have taken all of the independent techniques and gone to the same site. There are some differences in how we sample that does make a difference. The cosmogenic techniques require surface samples, which tend to have a little bit more alteration. We would prefer not using those for uranium-thorium and K-Argon.

In large part we do have good control of where the samples are and we feel that we are cross-checking these pretty effectively.

DR. ALLEN: Bruce, you conclude you are making progress. In rationalizing these different dates, it's not quite clear to me that that's the case even though it is scientifically exciting and some day, obviously we are going to resolve these issues, but what is the evidence for saying we are really make progress that is going to have closure in this issue during the life of this project?

MR. CROWE: The evidence I guess is my own prejudice that I think the bulk of the evidence is leading toward a younger age for Lathrop Wells. And in the last six months the TL and the helium results in particular have led to convergence toward the younger age.

If the issue is; can we prove beyond a shadow of a doubt the K-Argon is wrong and these others are right, I can't say that we can resolve that. It's beyond the scope of this project. But, it's going to come down to a burden of proof. I mean, we are going to have to make some decisions and it's not clear how we will make those decisions, but we feel that if we can trench and if we can do--I'd like to do ten sites with helium, ten sites with TL, that we will have a data set that we feel we can present conclusions. That's not to say that there will never be disagreements over that data set, but we think that if we make prudent decisions that they could lead to an acceptable conclusion.

DR. ALLEN: Yes.

MR. DOBSON: May I just add that from a perspective of resolution of the issue, if you will, I mean that is a regulatory concern, not a scientific concern. And what we need to understand is how the probabilities are affected if it is a monogenetic center at 120,000 or a polycyclic center that is less than 50,000. So that is the perspective that Bruce and also that I was under. I am not sure that we will absolutely ever have complete consensus that the K-Argon ages are right or that the TL ages are right or whichever technique is the most appropriate.

I think we could say pretty firmly now that almost everybody agrees that it is less than 150,000 years old.

MR. CROWE: Yeah. Maybe 200,000.

MR. DOBSON: And I think almost everybody agrees it is not, you know, it is at least 7,000 or 8,000 years old, something like that. And then you just have to study what your estimates of the risk are within that range of age estimates and that is the perspective we are coming from.

DR. ALLEN: Let me point out that both Brent Turrin and Duane Champion are in the group here, and as I understand they are each going to have a short comment here later in the morning.

Are there other comments at this point from anybody, or questions?

DR. MELSON: Bruce, this is more of a comment than a question. Cerro Negro volcano is a small cinder cone in Nicaragua which is erupting almost every tens of years, say 20 to 40 years. It is very much like Lathrop Wells, except the scale is very rapid. So looking at recent volcanism there is no reason not to expect that there wouldn't be polygenetic cones with perhaps longer periods of repose, even though I agree with you that the geological community is not thought of that way, but there are good examples of that sort of thing within the past 200 years.

MR. CROWE: I definitely agree. It certainly is a novel concept. I have to admit that when the idea was first presented to me I was quite skeptical and it has required a fair amount of data for us to view it.

Let me state though what I think is important in terms of our perspective is we really are trying to, as I mentioned, we are trying to avoid this false positive approach. We really are over emphasizing the potential young age because in most models that has the most negative impact. And so, we are really trying to wrestle with it. Our position is that until we can disprove all the evidence against the young age, we really ought to look at that as a the most conservative approach to this problem. And again I have to underscore that if we were doing this in kind of normal literature standards, we probably wouldn't be anywhere near the levels of dispute. I mean it is the Yucca Mountain perspective that's leading us to these levels of dispute.

DR. ALLEN: Okay. I think we are going to have to go on here to stay on schedule. The next speaker is Frank Perry from the University of New Mexico, speaking on New

Geochemical Data.

MR. FRANK PERRY: The purpose of the petrology and geochemistry studies are to try to come to an understanding of the underlying mechanisms of volcanism in the Yucca Mountain region.

And currently we have two goals. The first is to understand the overall magmatic evolution of the Crater Flat field from 3.7 to the youngest event of Lathrop Wells. And the second is to understand the nature of polycyclic volcanism at individual eruptive centers. And all I'll talk about today is the Lathrop Well center. We are also studying the ones at Sleeping Butte, but really don't have any data yet.

Okay, this is a simplified geologic map of Crater Flat. I just want to point out the three cycles; the oldest cycle at 3.7; an intermediate cycle at about a million; and then the youngest event at Lathrop Wells. The important things to note in terms of the evolution in the field is that first the volumes decline systematically through time. The volume at 3.7 was about a little bit less than a cubic kilometer, and by the time you get to the Lathrop Wells event, the cumulative volume there is about a 20th of a cubic kilometer. So there is a systematic decline in volume through time. There is also a change in a fusion through time, that also decreases.

At 3.7 you had a fairly higher fusion rate, and what you see are sheet-like flows that travel in some cases several kilometers from the vent. But at the intermediate and the youngest cycles, you have stubby aa flows that are confined to about a kilometer from the vent. So both of these suggest a decreasing magma flux through time.

Okay, so there are two lines of evidence for declining magma flux. First is the field evidence I just spoke of. The second is petrologic evidence. There is evidence of a deepening of magma reservoirs through time. And this evidence includes different phenocryst assemblages and trace element contents, and I'll go through that, and then increased magma evolution through time. And both of these are consistent with a decreasing magma flux which cause magmas to stall at deep levels and then they could only erupt once they had undergone protracted fractionation leading to the lowering of density which then allowed these to move on up through the crust.

This is a characteristic--it's a slide of growth rate versus time for a Hawaiian volcano, and this is characteristic of Hawaiian volcanoes. This example is Mauna Kea. What you see is the normal waxing of the volcano and increasing growth rate and then a decrease and a waning stage of volcanism. And this presumably occurs as the volcano approaches and then passes a hot spot in the mantle. What we are interested in is the very last waning stage of volcanism in these volcanos, the alkalic stage because we see some remarkably similarities with what we see at Crater Flat. And we think by analogy that Crater Flat is also in a waning stage, and whatever heat source was supplying Crater Flat, is now decaying.

Frey in a recent paper has listed several important characteristics as you go through the last waning stage from the basaltic substage to the hawaiitic substage. What you see and you also see all these things at Crater Flat, you see a change from more primitive compositions to more evolved balsatic compositions, the decrease in volume through time, a change in the petrography from porphyritic to aphyric basalts. And you see changes in two important trace elements. One, you see an increase in strontium through time and a decrease in scandium. And these two trace elements are important because they trace the role of plagioclase which strontium goes into and pyroxene which scandium goes into. And these two phases are in turn important, because by looking at the relative role of plagioclase and clinopyroxene, you can get an estimate of what the depth of fractionation is and see if there are any systematic difference in fractionation depth as you go through time.

There's been many experimental studies and thermodynamic studies done on alkalic basalts. This is one example from Knutson and Green on an hawaiite. And what you see and what is typical of all these experiments, that at low pressures, plagioclase is stable near the liquidous, but as you go to higher pressures, and this would be about the base of the crust, plagioclase gives way to pyroxene show here by the circles as a more important near liquidous phase. So you would expect an alkaline basalt at moderate pressures, there would be more plagioclase involved in fractionation, at deeper pressures more pyroxene involved.

Now we don't have our own data for all of Crater Flat yet, because of our analytical limitations. What I've done is taken data from a paper by Vaniman, et al in 1982, and reinterpreted some of that data.

This is a plot of strontium versus Mg number. And Mg number is a fractionation indicator. It is magnesium over magnesium plus FE^2+. And primitive basalts that represent a melt in the mantle have high Mg numbers and then as you evolve and crystalize the Mg numbers get lower.

What we see are two different fields. The oldest Crater Flat basalts are low in strontium, but both the intermediate and Lathrop Wells centers have very high strontium. And what we propose is that this represents two different fractionation trends. One, a low pressure trend where plag is important and this buffers in effect the strontium contents so you never get very high strontium. And then we see a higher pressure trend where pyroxene is more important, and strontium is in essence incompatible if there is nothing for it to go into. And so you get very high strontium contents in these basalts. Also notice that the 3.7 basalts are, as a whole, of higher Mg numbers and are more primitive than the more evolved, intermediate and Lathrop Well cycles.

Okay we see a very similar thing for scandium which traces pyroxene fractionation. See again the low pressure trend for the 3.7 basalts where plag is important, pyroxene not as important, and then a high pressure trend where pyroxene is important leading to lower scandium for Lathrop Wells and the intermediate cycle. So both of these are consistent that fractionation is occurring at deeper depths as the system becomes younger.

So the model that we propose is that at 3.7 million years, the fluxes were relatively high. You create a melt in the mantle and the flux was high enough to go through the crust mantle density boundary and maintain chambers in the mid-crust. This led to more voluminous eruptions. There was a higher average throughput to the system so you on average erupted more primitive basalt. So it is more vigorous system.

Then once you got to the intermediate and the youngest cycle, the flux decreased quite a bit. And here you create melts but the flux was so low that you couldn't penetrate the crust mantle boundary. These then at this point underwent high pressure fractionation and it was fairly extensive, at least 30 to 40 percent fractionation. And then you could only erupt after the density because you are fractionating high density phases. Once the density became low enough, then they could ascend on through the crust and erupted rapidly and probably in several pulses because of this polycyclic model of which I will go through in the next part of the talk. The question mark is, we really don't understand the high level structures that are controlling the ascent of these basalts.

The rest of the talk I'll talk about Lathrop Wells and what the geochemistry says about the polycyclic model. You've seen a geologic map of Crater Flat. What we've done is for every eruptive unit we have identified in the field, we have taken multiple samples. And this is to ensure that any internal heterogeneity within a flow or a scoria unit is less than any differences we see between units. So, we want to make sure of any chemical differences we see among eruptive units is a real difference and doesn't just reflect some heterogeneity.

So first I'll show data for the four flows, the Ql_6 perch flow and then on around. The two flows on this side, all the flows are in blue, but we believe that these--the flows on the southwest side are older than the two flows on the northeast side. And this is based on their morphology and flow fronts and things.

Okay, the first thing we see is that are systematic chemical differences among those four flows. And this is just an example using two incompatible elements. So, the question is what leads to these geochemical differences? Currently, I can find the literature of two models to create geochemical differences at simple monogenetic small volume systems. The first is fractionation of a single batch and that is not uncommon if enough study is done at a monogenetic center. The second is mixing between two independent batches of magma. And this seems to occur in high flux systems where there is a lot of magma present. You see this at Kilauea and also a field in western Saudi Arabia that is quite active. So given enough flux there is a tendency to have enough magma around where you do get mixes in batches.

What we proposed at Lathrop is what we are seeing are temporally discrete pulses. That when a second magma comes up there's been enough time that whatever magma came up before that is completely frozen out and can no longer interact with the system. And we believe these are separate partial melts and I'll show you the data for that.

Okay, first I'll try to rule out fractionation and mixing as a cause for this. If fractionation was going on, then on a plot of two incompatible elements, the unit with the highest concentrations would be the most evolved. So in this case Ql_5 would be the most evolved fractionated product.

Now this is Mg numbers versus the number of units for Lathrop Wells. All the flows lie in here and have a very restricted range of Mg numbers. So that is a constraint that doesn't allow much fractionation between units, because they essentially all have the same Mg number. This is a scoria unit which I'll talk about in a minute.

Okay, the other thing is if you look at a different variation diagram, this is two compatible elements, things that go into the phenocryst phase. If Ql_5 is the most evolved due to fractionation on the other plot I showed you, then it should also occupy the most evolved composition on this plot which would be down here at low MgO. But what you see is that it occupies an intermediate spot between these two units. So there is no consistency between plots which means it cannot just simply be one batch of magma that is fractionating. Likewise this could be a mixture between these two batches, but again, that would be inconsistent with the other plot I showed which shows Ql_5 at an in member position.

So just using these real simple arguments, you can rule out either fractionation or mixing as accounting for all the variation you see.

Instead, what we see, going to trace elements is that there are systematic differences in incompatible trace element ratios. On this type of a plot, lanthanum is slightly more incompatible than samarium, but they are both incompatible. And in a simple basaltic system, you wouldn't, you cannot change that ratio by small degrees of fractionation. But you can change this ratio by differences in the degree of partial melting. We see a systematic difference between the older two flows Ql_6 and Ql_5, which have lower lanthanum and samarium and then the youngest flows which have higher. We can plot different trace elements here and no matter what you plot you always get the systematic relationship with the younger ones having higher ratios. And this also indicates that if these are younger, if we are right about that, that these represent slightly smaller amounts of partial melting.

So the conclusion of this is that these systematic differences lead you to a model that they are separate, partial melts that retain their identity and erupt to the surface as discrete pulses.

This is the same plot for scoria and bomb units. And again we see--well, what impresses me about this is that in most cases things cluster very tightly. These are bombs from the Qs_4 fissure system. These are bombs from two different mounds on the Qs_5 system, they also cluster very tightly. These are scoria from the quarry area, and, based on geochemistry at this point, we don't know where those quarry scoria come from. The main cone, interestingly enough, has the biggest range. And to me that suggests that we maybe sampling more than one event in the main cinder cone and it may have a more complicated history than we know at this point.

So the conclusions really are that eruptive events at Lathrop Wells represent separate melts, and the apparent lack of melt interactions between these separate melts indicate that there was some time interval between eruptions. So it is not a simple monogenetic center and it is consistent with a polycyclic story.

The needs for the future are really--it's very important that we integrate the chemistry with the stratigraphy by trenching, because we need to know how systematic are any chemical changes through time and can that give us a better handle on what the mechanism is. And analytical capability refers to QA, just our frustration of not being able to get much data. That's it.

DR. ALLEN: Thank you, Frank. Are there any questions or comments from our Board people?

Any other questions or comments?

MR. DOBSON: Frank, I have an easy one.

DR. ALLEN: Yes, Dave.

MR. DOBSON: On the plot that you showed of the cone variation, are those just a group of different bomb samples from the cone or are those--

MR. PERRY: Two of those were scoria samples that were at the high end. The rest of the scatter was all in bombs.

MR. DOBSON: So, how did you separate the scoria samples to make sure you were getting like eolian stuff in there too?

MR. PERRY: They were just clean samples. We did some ultrasounding to get rid of any particular matter and they came out clean.

MR. DOBSON: Okay.

DR. ALLEN: Mike.

MR. SHERIDAN: The question is have you examined the strontium isotope data which would have some bearing on the differences between those two stages, one with a large crustal magma chamber which presumably would have a higher strontium isotopic ratio?

MR. PERRY: There is some very limited strontium and neodymium that has been done in lead, but you know only like one per cycle or something like that. We are going to do strontium neodymium in lead in the future, hopefully this year to test some of that and also to see if we can see variations within the Lathrop Well series that might give us more clues about what is going on.

DR. ALLEN: Don?

DR. LANGMUIR: Frank, have you tried to apply this same general approach to some other cones in the vicinity, some of the other ones which were discussed earlier today in those systems?

MR. PERRY: The only one we really have data on is one of the cones at Cima. And there things seem to be related by fractionation. And my feeling is and I don't know if other people would agree, but I think that if that is a polycyclic cone, what we've sampled, it looks like it is related by fractionation, covers a shorter time span than what we may have a Lathrop Wells. So in that sense it would be more consistent with fractionation of some batch of magma. But it looks different than what we see at Lathrop Wells. We have samples from both Sleeping Butte centers but those haven't been analyzed yet.

DR. ALLEN: Further questions?

(No response.)

DR. ALLEN: Okay, if not, let's start our break and we'll have 20 minutes. We'll reconvene exactly at 10:15 a.m.

(Whereupon, a recess was had off the record.)

DR. ALLEN: Okay. The next presentation this morning will be by Gene Smith of the University of Nevada, Las Vegas, who has 45 minutes if he wishes it.

MR. EUGENE SMITH: Thank you.

I don't have a handout--I didn't prepare a handout, however I would be very happy to mail copies of the slides to members of the Board.

DR. ALLEN: We would like one copy for the record, please.

MR. SMITH: I would be very happy to do that.

DR. ALLEN: Thank you.

MR. SMITH: What I would like to do today is describe the work of the Center for Volcanic and Tectonic Studies at the University of Nevada Las Vegas. During a period of time during 1990, and mainly what I want to try to do is try to provide some geologic constraints for the models that are being proposed by a variety of different individuals. I think all of these models have to be based on good geologic data and I think we are beginning to gather that data. But I think we are just in the initial stages of this data-gathering effort.

Major purposes of our activities during 1990 is first to determine the source of the basaltic magma at Crater Flat and some of the other volcanic centers in the southern part of the Great Basin. The second part of our effort has been directed toward determining the mechanisms of ascent of this magma from the source to the surface, primarily dealing with the types of geologic structures that may in fact be controlling magma ascent in the middle and upper part of the crust. And lastly, volcanic hazard assessment in sort of a qualitative way in the Yucca Mountain area.

This presentation will be divided into three parts, three topics. I'm not sure if there will be flow from one topic to the other. They are three separate discussions.

First I would like to talk about some of our new isotopic data that we've recently obtained for Crater Flat and other volcanic centers in the southern Great Basin. And then talk about magma ascent, and then lastly discuss our risk assessment, and this is really divided into three parts. First are our definition of what we call the area most recent volcanic activity. This is the area that we consider to be of primary interest in terms of gathering data for probabilistic studies. We want to talk about the position and the importance of Buckboard Mesa which is a volcanic center within the mode of the Timber Mountain caldera. I will talk about the importance of this in terms of the area most recent volcanic activity. And then lastly define or describe to you some of the qualitative risk zones that we propose within the AMRV.

First let's talk about the isotopic studies. We are going to look at volcanic fields in several different parts of the Great Basin. I want to show you data for the Crater Flat area which is located right here (indicating). Yucca Mountain is approximately right there (indicating), at the tip of my finger, some isotopic data for Sleeping Butte and Buckboard Mesa, the Reveille Range, right up here (indicating), and also the Fortification Hill volcanic field in northwestern Arizona and southeastern Nevada, a place that we've been doing work for quite a number of years.

Now, this is a sort of a diagrammatic plot of lead isotopic ratio, lead 206 to 204 plotted against the initial strontium ratio, strontium 87 over strontium 86. Plotted on this diagram, we are basically looking at two isotopic systems at one time. Plotted on this diagram are several different types of possible source material, source material from the asthenosphere, which falls in this particular location, this is called prema or prevalent mantle. We have several different types of lithospheric mantle also plotted on this diagram. We have a type called HIMU which may in fact be lithospheric mantle that represents subductive oceanic plate without any appreciable sedimentary component that is a pure basaltic oceanic slab, EM_2 enriched mantle 2 and enriched mantle 1, which represents different types of lithospheric mantle, possibly containing a certain degree of sedimentary component.

The other parts of this diagram, the yellow dots on here represent samples from the Crater Flat, Sleeping Butte and Buckboard Mesa areas. They form a very tight cluster with relatively initial strontium between about 707 and 708, and lead 206, 204 about 18.5. Points that fall here (indicating), probably represent lithospheric mantle as Frank, Farmer, Perry and several others have suggested a long time ago, it may be lithospheric mantle that's a mixture of some primitive asthenospheric material with a lithospheric mantle that might have some of this EM_2 component in it.

The blue dots represent the Fortification Hill volcanic field. This is really quite exciting, at least to me. The early eruptions from Fortification Hill, these are mainly phyllitic basalts, and some alkali basalts, have a very similar isotopic signature to the basalt at Crater Flat. With time and these white lines represent decreasing time, these are the oldest lavas at Fortification Hill, these are the youngest lavas (indicating). The lavas appear to be tapping a more primitive source. Finally the youngest lava seem to be tapping a source that is a mixture of the asthenosphere and possibly a little bit of this HIMU lithospheric mantle component.

This trend toward melting lithospheric mantle early and more primitive material later, may represent a deeper source, that means melting a deeper source with time. Or, it may represent a situation of lithospheric erosion. That means as the continental and mantle lithosphere is extended, lithospheric mantle is thin and it is replaced by an asthenospheric plug. So we are not really sure which one of these two is valid, but it's a very interesting pattern.

The Reveille Range falls up in this area here (indicating). Most of the Reveille Range rocks are very similar to this HIMU component and it is possible that we might be melting lithospheric mantle of a different composition than we melted at Fortification Hill or Crater Flat.

MR. DOBSON: Gene, what's the length of the time involved at Fortification Hill?

MR. SMITH: Yes, the latest--the oldest lavas are 5.8 million and the youngest are 4.6.

Now just very quickly, I don't want to dwell on this, we do have some neodymium and strontium data. It's interesting that the rocks of the Crater Flat area, Buckboard Mesa and Sleeping Butte fairly tightly cluster on this sort of a diagram suggesting very similar type of source. Though I haven't looked at individual centers or individual flows from the same center to determine whether or not we have small changes in the nature of the source with time, but I am impressed by the fact that we are getting a very similar type of source for not only Crater Flat, but also Sleeping Butte and Buckboard Mesa. You can see the same change in time at Fortification Hill and the very distinctive isotopic signature for the Reveille Range rocks.

So what the isotopic studies suggest very briefly is that Crater Flat is formed by the melting of the lithospheric mantle as suggested earlier by Farmer and others.

The Reveille Range we feel, based on the isotopic work may also be melting a lithospheric mantle, but of a different composition.

Fortification Hill, we see a change from lithospheric mantle early to a mixture of asthenosphere and lithospheric mantle, this HIMU component with time. Whether this suggests lithospheric erosion or a deepening of the source with time, we are not really sure at the present time.

Let's move onto the second topic, magma ascent. How do we get the magmas from this deep lithospheric mantle or asthenospheric source to the surface? And the conclusion that I am going to draw is that that alkali basalts which are common in Crater Flat, common in the Fortification Hill field, common in the Reveille Range, are associated with high angle faults or other types of structures that penetrate deep into the crust. And this provides the channel-way for bringing these materials up.

Now, what I'd like to do to bring across this point is to describe an analog study that we've done in the Fortification Hill field. Fortification Hill field, like I said is located just to the east of Las Vegas, extends through the area just to the southeast of Hoover Dam up to the Overton arm of Lake Mead as far to the north as Black Point. Now there's a series of phases of eruption in the Fortification Hill field. There is only one that really has an interest to us but I would like to go through the other stages just to sort of set the scenario for the eruption of the alkali basalts late in the history of this field. And I'll show you diagrammatic cross sections to sort of bring across the point in a section, but let's just go through these series of stages first.

Phase one, between 18 million and 12 million years we have large volumes of intermediate calc-alkaline magmas. Some of these magmas produced large plutons and these plutons were in place very rapidly over periods of less than a million years in placing cubic kilometer or in some cases several cubic kilometers of magma over very, very short periods of time. We feel that this is related to ductal extension in the lower part of the crust.

Phase two, 12 million years ago, we find the first evidence of upper crustal structures, and we feel what is happening here is that these upper crustal structures are tapping the end-member magmas that mixed earlier to form these intermediate magmas. So we produce rhyolite and granite volcano superimposed on top of these andesitic volcanoes that were produced during stage one or phase one.

Phase three is the major phase of upper crustal extension. This is when we find the major detachment structures, large amounts of back rotation of strata and large values of upper crustal extension. However, the really thrilling thing is that during phase three, there is almost no volcanism at all. We find very little volcanic activity, and that volcanic activity is mainly very small volumes of balsaltic andesite.

Stage four is the phase that we are really interested in, is very late, between 7 million and about 4.7 million. We have high angle faults that cut the detachment faults and we find alkali basalt volcanic activity. So it is not until the last phase that we produce these lower volume alkali basalts and they are related to the formation of these high angle faults.

Let me just try to demonstrate this on some cartoons. This is phase one, ductal extension in the lower to middle-part of the crust, partial melting of asthenosphere or rocks deep in the lithospheric mantle, melting of crustal rocks, the mixing of these two, possibly in chambers controlled by the location of these ductal shear zones and the eruption of the intermediate magmas to the surface forming a low broad stratovolcanoes and the emplacement of fairly large plutons representing the subjacent chambers for these stratovolcanoes.

Stage two, this is very diagrammatic of course, the first upper crustal faults, the tapping of these basalts had rhyolites that were mixing earlier and the superposition of these basalts and rhyolites on top of this earlier andesitic stratacone.

Stage three, the initiation of large amounts of upper crustal extension. I'm not going to get into this in any detail, but we don't feel that these upper crustal faults are the typical types of detachment faults as described by Brian Wernicke in some of his classic papers. We sort of--we now feel that these detachment faults are originally high angle structures. We feel that: we've had footwall uplift; isostatic uplift; the back tilting of some of these angle structures close to the surface; and, the penetration of new high angle segments to the surface, having the effect of transferring rocks from the hangwall to the footwall producing offsets, for example, in this case, a volcano and its pluton, by as much as 20 to 25 kilometers. It's a very complicated model.

The important thing here is that during this major phase of upper crustal extension there is very little magmatism and that magmatism that does occur is basaltic andesite, probably with the source either in the asthenosphere or the lithospheric mantle with a little bit of crustal contamination, but very small volumes.

Finally, and this is the part that we are interested in, we have high angle faults that chop this entire terrain up. These are very planar high angle faults. For the first time we are picking up alkali basalts with mantle xenoliths, suggesting a very deep source. These basalts are very similar to the basalts that Frank was talking about. They lack plagioclase phenocryst. They must have come up very quickly, and we suggest that they are coming up along high angle structures that penetrate very deep into the lithosphere and possibly crustal penetrating structures. We feel that these are the major controls for these basaltic andesites.

Let's look at the geologic map just to sort of emphasize this. This is the Fortification Hill field. The major high angle structures are shown in white. The major volcanoes are shown by these off-yellow dots and the flows are shown in green. These volcanoes are found to associate with the range margin faults, both on the west and the east side of a very prominent horst that extends through this region right here (indicating). We also find several vents that are located in the central portion of the horst. They produce a chain that's about 35 to 40 kilometers long and about ten kilometers wide. We have very, strong structural control. I have to emphasize though that the volcanoes are not occurring directly on the structures. They are occurring either on the footwall or the hanging walls of the structures, but there is a very strong relationship between the location of events and structures, even though they are not coming up along the structures directly.

The same thing, just to show you this occurs other places, here is the Reveille Range. Volcanism occurs both on the east side and the west side of the range, predominately associated with the range margin structures. In the Reveille Range we have several different volcanic chains tens of kilometers long and five to ten kilometers wide, both on the east and the west side of the range with fairly strong control by the range margin structures. We also have volcanoes occurring in the center of the range.

So, the basic conclusion of this particular part and I'll try to go into this and indicate its implications for Yucca Mountain in a couple of seconds, is that alkali basalts are associated, we feel, with high angle faults that penetrate deep into the crust.

Now what are the implications for Yucca Mountain in this particular section? We have already said this. What is the nature of the high angle faults in the Yucca Mountain area? Well, we don't know to tell you the truth. They could represent a high angle segment of a detachment fault like those that I showed you in stage two of my model. It could be a strike-slip fault, a northwest trending strike-slip fault, and the volcanoes may be coming up along high angle normal faults that are secondary faults to this strike-slip structure. It could very well be that the package of faults that cuts Yucca Mountain may at depth, represent a high angle crustal penetrating structure.

So, we are not sure which one of these three is correct, but we do know that, based on our analog studies, that what we have to suggest is there has to be a high angle crustal penetrating feature in the vicinity of Yucca Mountain to control the emplacement of these volcanoes. And I think the work that Frank Perry mentioned earlier suggested a very rapid rise to the surface and I think that would agree with the fact that there has to be some sort of a channel-way to bring these things to the surface.

Something else that this brings out is the distributions of volcanoes and we'll show you this in just a second in more detail, is not random, so that we feel very strongly that the structural control of volcanism must be considered in any probabilistic models. Distribution of volcanoes is not random. They are very strongly controlled by structure. They occur in volcanic chains, and that has to be factored into any models that predict the locations of future events or any models that predict the probability of future events.

Okay, let's go onto the last topic here, the area of most recent volcanic activity, Buckboard Mesa and the risk zone. Now it's fairly well known in the literature that volcanism migrated to the south in the Great Basin between about 43 million years ago and about 6 million years ago. However, in the time interval between 6 million years and the present, migration trends are non-existent or very vague. What we've done, and I've showed some of these slides before in other talks, is we've divided the time interval between 0 and 6 into three time slices and show the distribution of basaltic volcanism within each one of these time intervals.

Between 4 and 6 million years, volcanism is spread from the Reveille Range to the north into the Cima field in California and as far to the east as the Grand Wash Cliffs in northwestern Arizona. There is no volcanism within 50 kilometers of the Yucca Mountain boundary of the proposed repository.

Between 2 and 4 million years, volcanism has waned a bit, there is not quite as large a volume, fewer centers, but we still have a very wide distribution from the Pankake Range to the north down at the Cima and as far to the east as the Grand Wash trough. We are now beginning to pick up some volcanic activity within 50 kilometers of Yucca Mountain. We have the activity in southeastern Crater Flat and the activity at Buckboard Mesa.

Between 0 and 2, again a very wide spread of volcanic activity. We don't see any--at least I can't see any obvious migration trends here. From the Reveille Range down into Cima and in the Death Valley area of California, we now have several different events within 50 kilometers of Yucca Mountain. The asterisks represent centers that erupted within the last 300,000 years with activity at Lathrop Wells and activity in the central portion of Crater Flat. These are the 1.1 million year old centers that were shown in slides earlier, and we also have volcanic activity occurring at Sleeping Butte.

Now the way we define our area of most recent volcanic activity--do I have control of the focus or not? There are so many buttons on this thing, I'm not sure which ones control what. I hope this is not the only--this may be the only one out of focus. Thank you.

This is a satellite photograph of Yucca Mountain, Timber Mountain caldera area. We feel that when one makes measurements to include in probabilistic studies that one should look at all of the events that have occurred within the last 4 million years within 50 kilometers or so of Yucca Mountain. So what we have done is we have simply outlined all of the volcanic activity, every event that's occurred within this time frame and we label this as the area most recent volcanic activity. This is the area of interest for future studies. This includes the Lathrop Wells cone, the volcanoes in Crater Flat, Sleeping Butte and Buckboard Mesa.

I should mention that there is no obvious migration trends even at this scale. Volcanism started in southeastern Crater Flat at about 3.7 million years ago and may have continued to about 2.53. Volcanism was going on about the same time at Buckboard Mesa at about 2.8, then shifted back down to the southwest at Crater Flat at about 1.1, and then we had roughly--we had coeval activity at Sleeping Butte and Lathrop Wells starting in the hundreds of thousands of years and continuing--well we are not really sure how long they continued. There seems to be quite a lot of debate on that, but may have continued to the point where the latest eruptions at Lathrop Wells might be less than 10,000 years, but I don't want to get into that debate right now. So the area of most volcanism includes all post 4 million year old volcanoes within 50 kilometers of Yucca Mountain.

Now how does this compare with some of the other zones that have been proposed in the past? Vaniman and others a long time ago proposed the Death Valley Pankake Range, volcanic belt that extends from Death Valley all the way up to the Reveille Range and Lunar Crater area in central Nevada. Our AMRV which is outlined right here falls completely within that zone. Crowe and Perry, fairly recently proposed the Crater Flat zone that extends from Sleeping Butte to Lathrop Wells and also includes some aeromagnetic features, aeromagnetic anomalies to the southeast of Lathrop Wells.

Our AMRV is very similar to that. There are two major differences, however. Our AMRV includes the Buckboard Mesa center, but does not include the aeromag anomalies. It does not include the aeromag anomalies because we simply don't know what they are and once we find out we may decide to include them in this AMRV, but right now, we don't know whether they are basaltic, we don't know whether they are felsic, we just don't know what the age of these features is.

Now, the major point of difference then between the Crater Flats zone and the AMRV is the Buckboard Mesa center. I would just like to present a little bit of evidence to suggest that Buckboard Mesa should be included within this area of interest to Yucca Mountain. Buckboard Mesa sits, like I said in the mode of the Timber Mountain caldera. There is one major event, a cinder cone complex located right here (indicating). The regional gradient was down the mote of the Timber Mountain caldera, so most of the flows traveled down the topographic gradient in this direction here (indicating). There is a very prominent ridge that you can see extending from the cone to the southeast, and there is a small amount of scoria located in the chemical explosion pit at that particular point right there (indicating).

This has made several investigators suggest that the Buckboard Mesa center is controlled by some sort of a rift or fracture that extends to the southeast parallel to the mode of the caldera with one center here and one center here (indicating). However, our recent work at Buckboard Mesa suggests that this probably is not correct.

Let me just give you some background. This is the geologic map of the Nevada test site that was recently published, a nice map for showing regional structure. This is the Yucca Mountain area and Crater Flat, some of the cones within Crater Flat, the Timber Mountain caldera with Buckboard Mesa and Pahute Mesa located to the north. One of the really spectacular things is the very prevalent northeast trending structural grain both at Yucca Mountain and then on Pahute Mesa. It seems to be interrupted by the Timber Mountain caldera. And we feel that this is the regional structural grain and most probably the primary structures within this area. These structures continue to the north for many miles. They are not simply associated with the Yucca Mountain area.

Let's look at a close-up of this. On the geologic map of the Nevada test site, you can see the northeast trending structures here on Pahute Mesa. One of the authors of the map has continued one of these structures down across the northwestern part of Buckboard Mesa. It shows offset of the Buckboard Mesa lavas and in fact this particular fault offsets the ash-flow tops that lie beneath the Buckboard Mesa lavas both here and over in this area here. There is a little bit of debate of whether this is the same fault as that. There is a reverse displacement. This fault is down to the east; this fault is down to the west. However, as I showed you before both types of displacements occur on Buckboard Mesa. So it looks like according to this map that one of these faults cuts right across the northwest part of Buckboard.

Looking at an aerial photograph, it is quite obvious that there is a structure there. Here is the cinder cone right here, and here is this northeast trending structure cutting right across--it's actually quite complex. There is a series of en echelon faults both down to the east and down to the west. It's our contention that the cinder cone is controlled by this northeast trending structure, rather than any sort of structure that extends to the southeast down the axis of the mode of the Timber Mountain caldera.

This is a nuclear explosion crater here (indicating), this is the chemical explosion crater that I've talked to you about earlier and if you look very carefully you can see this lobe extending down into this area.

Now let's go on if I can. This is sort of a sketch map of Buckboard, the major northeast trending faults, the main scoria cone located here and this lobe extending to the southeast, the nuclear explosion crater and the chemical explosion crater. I would now like to sort of focus in on this box. Getting a little bit closer, major northeast trending structures, when one looks at the cone in detail, it is very interesting. What one sees is a series of small intrusive plugs and spatter cones and other types of vent material aligned on a north northeast direction parallel to this regional structure. Also there are dikes. Dikes of course have a variable attitudes but many trend to the north northeast. It looks like most of the secondary centers here are controlled by this northeast trend. Here is that lobe that I talked about earlier and this little chemical explosion crater with some scoria. We feel that this lobe represents one of the latest flows on Buckboard and that this particular center here may be a rootless center related in someway to tubes, lava tubes that extend from the cone to the terminus of the flow. We don't feel that this is a major volcanic center. We feel that the centers are controlled by these northeast trending structures.

And this is quite common. Lutton back in 1969 on an article on the Buckboard Mesa center described the very similar sort of geometry as being very common on Buckboard and you can see this if you go out there and look at the flows. Most of the flows are composed of individual toes or lobes that are overlapped by other toes and lobes. And we feel that that last ridge is simply one of these toes that has extended for distance down the regional gradient along the axis of the mode of the Timber Mountain caldera.

So, in terms of including Buckboard Mesa within our area most recent volcanic activity, we feel that it is the northeast striking structures that control the location's events. Admittedly, the location of that must be controlled by where those northeast trending structures intersect the mode of the caldera. But, it's a northeast striking structure that we feel is the major controlling feature. This is similar to the structures that control the locations of events at Crater Flats, as I'll show you in a second. We see no difference in structural control of similar isotopic composition in source, we showed you that earlier on both diagrams. They seem to be melting the same source. Now they are somewhat similar in mineralogy and chemistry. There are differences. The Buckboard Mesa center is higher in silica. It's about 51 percent silica as opposed to the high 40 rocks in the high 40's, 46-48 for Crater Flat and Sleeping Butte. But, if one looks at the rocks carefully and hand specimen in thin section, one sees plenty of quartz xenocryst. And this could be what is causing the increase in the silica content.

And lastly, it's an event that occurred 2.8 million years ago and it's only 33 kilometers from Yucca Mountain. So we feel that it should be included within this AMRV.

DR. ALLEN: Gene, excuse me, there are ten minutes remaining of your allotted time.

MR. SMITH: Thank you.

Now let me just go into some of the details of structural control and I'll go through these quite quickly. We'll be talking mainly about the cones in central Crater Flat and mapping of these cones. And I'll do this--I won't go through all the details, it suggests that for example at Red Cone which is the central cone of this 1.1 million year old cycle, we have the cone itself then a series of scoria mounds scattered over the area to the south of the cone, we find two northeast trends alignment directions of these scoria mounds and a subsidiary northwest trend. Go to Black cone and see exactly the same thing, two northeast trends.

Looking at a map of the Yucca Mountain Crater Flat area we see these northeast trends here on the cones. Detailed geologic mapping in the southern part of Yucca Mountain has revealed that the major structural trends as has been known for a long time are north northeast. However, we do find northeast trending segments that connect these north northeast trending high angle normal faults. These northeast segments are parallel to the alignment of the scoria mounds on Black Cone and Red Cone.

So it is possible that these northeast segments may be the structures that are controlling the positions of the scoria mounds. But what evidence do we have that there are faults that lie beneath let's say Black Cone? Well one piece of evidence is simply an observation based on field mapping that we have the Tiva Canyon member of the paintbrush tuff exposed just a couple of hundred meters to the east of the flows on Black Cone. In hole Vh_2 which is located approximately right here (indicating), this same unit is found at a depth of 800 meters. We feel that this requires at least one structure, at least one fault to explain the nearly 800 meters of the displacement across a kilometer and a half of distance between these exposure and the hole Vh_2. So a possible cross section going from the summit of Yucca Mountain to Black Cone here, is that exposure just to east of Black Cone requires at least one fault in order to keep the same geometry. We place several faults beneath Black Cone, and these could be the faults along which the Black Cone magmas are arising to the surface.

This would be a hypothetical geologic map with the Black Cone scoria mounds being controlled by these northeast segments. Just to show that other people have a very similar impression, this is the cross section from the geologic map of the Nevada test site going from the summit of Yucca Mountain across Northern Cone, again a very similar sort of geometry. Here they show the magma coming up and leaving the fault close to the surface and coming up into the hanging wall block.

Now why are these northeast segments so important in controlling volcanic activity? Well, if you take a look at work done in the Death Valley area by Wernicke and others, the regional extension erection that they find in the Black Mountains just to the southwest of Crater flat is about north 60 west.

And other work by Stock and Healy and region work suggests that north 60 west and north 65 west as the regional extension direction. These northeast segments are aligned perfectly to this extension direction. They are at right angles to this extension direction, and therefore they have the structure that would be the most susceptible to allowing magmas to arise.

Now the last part, the risk zones within the area of most recent volcanic activity, we have a lot of assumptions and many of these assumptions are based on studies of our analog areas and I just simply don't have time to go into all the analog studies. We do assume that volcanism was controlled by north northeast striking structures. These are the same structures that control the location of Crater Flat and Lathrop Wells volcanoes. The volcanic chains vary from 12 to 35 kilometers long and between 1 and 15 kilometers wide, and we feel that future eruptions may occur preferentially at sites of previous volcanism within the same structural zone that controlled those previous volcanic eruptions.

So we then construct risk zones preferentially on some of the most recent volcanoes within the area of most recent volcanic activity. The two most likely sites are at Lathrop Wells and Sleeping Butte. The inner rectangle represents the length of the Crater Flat chain shown in blue. The outer rectangle represents a conservative estimate of the length of the volcanic chain from our analog areas. We have aligned these risk zones parallel to regional structure. We feel then that the highest possibility for eruption is in an existing cone like Lathrop Wells or Sleeping Butte.

The next highest probability is within the blue rectangle. The next highest probability is within the yellow rectangle and then the lower probability within the area of most recent volcanic activity. These of course are qualitative estimates. However, I think regional structure, and this is what we are trying to do is to factor in regional structure and data from our analogs, I think has to be factored into any probabilistic study.

So our summary then in conclusion of all this is first that the source for alkali basalt magma appears to be deep in the lithosphere and/or asthenosphere. And we are talking about several different areas. For Crater Flat we feel that it is the lithospheric mantle.

Magma rises along crustal penetrating structures. In the upper crust, Magma may leave these channel-ways and intrude either the hanging wall or the footwall. It may target an existing vent, or it may rise along one of the northeast fault segments. We feel that in the upper crust, at least in Crater Flat, it will focus itself into one of those risk rectangles or focus itself at one of the existing centers.

And it is quite possible that in the upper crust, magma may follow structures with a different orientation than the master structure that it follows as it rises from its source in the lithospheric mantel into the middle crust. So in the upper crust, we may be looking at different orientation to structures than the structures in the middle to lower crust.

Thank you.

DR. ALLEN: Thank you, Gene. We have about three minutes or so here.

First of all, are there questions from Board members or consultants or staff members? Bill?

DR. MELSON: You've taken your idea that the most likely hazard zones are going to occur along the northeast zone from these other areas, and plus the presence of the faults in the Yucca Mountain area.

DR. SMITH: Right.

DR. MELSON: But it seems to me if you look at the trends of activity as they have occurred in the Yucca Mountain region, you see a northwest trending zone, and you keep using the phrase, "factor in these other areas." It seems to me that Yucca Mountain has its own signature of centers, which are more or less northwest oriented. They may be following northeast oriented zones, but where the magma is finding its way to the surface is along this northwest trending region, and your hazard assessment doesn't really take that into account. And yet, to me, that's what the Yucca Mountain story is saying above all else, is that there is the zone, most of it in the valley, and, as you say, Lathrop Wells is probably the most likely one to erupt, and then where I would see some difference from your interpretation is when you say it's going to be along these long northeast trending zones. I would say probability favors it in the valley, along the northwest trending zone, based on what's happened at Yucca Mountain.

DR. SMITH: Yeah. There is, you know, very definitely, as Bruce has pointed out--and that's the basis for his definition of the Crater Flats zone--a northwest trend, at least in terms of the youngest volcano, Sleeping Butte and the young volcanos in Crater Flat and Lathrop Wells, and it's quite possible that, as I mentioned, that in the upper crust we may be looking at emplacement of magmas along faults that have different orientation than the master controlling structure, and it could very well be that a northwest trending strike slip fault, or some sort of a northwest trending structure is the master structure, and that the basalts are simply following secondary northeast trending high-angle faults once they reach into the upper crust.

However, the risk zones are based on what happens in the upper crust, and the volcanos, I think, are very nicely aligned in that northeast direction. We might find another one, for example, someplace between Sleeping Butte and Lathrop

Wells. Another volcano may erupt within that northwest trend, but we feel that what will happen is that if one gets more than one volcanic eruption, that volcanos will be controlled by northeast structures at that particular point. That's the master structure in the upper crust.

So those risk zones are based on locations of present vents, and if there's another eruption associated with the present vents, then where will it occur? And we feel that the northeast structures will be the controlling factor, rather than the northwest structures. This is what has controlled these vents in the past; for example, in Crater Flat.

We have a single vent. The next vents occurred along the northeast trending orientation. Looking at the 3.7 million year old vents, they're controlled by north-south to northeast trending vents. So we feel that in the upper crust, that's what's going to control the emplacement of the magmas. So it's based on the present structural grain, the northeast structural grain, and it's based on where we feel the next volcano will occur, based on the structures that one sees in the upper crust.

I'm not sure--I'm sort of talking around you.

DR. ALLEN: Okay. Thank you, Gene. We may have a chance to come back to this during this next half hour, but initially, we've had a request some time ago for a couple of short presentations here, and to introduce those, let me introduce Carl Hedge of the U.S. Geological Survey.

MR. HEDGE: In his discussion of the geochronology this morning, particularly of Lathrop Wells, Bruce Crowe made reference to his difference of opinion with the USGS on some of these ages, and I'd like to elaborate on that a little bit by saying that many of the uses that Bruce made of those numbers, I simply cannot accept. He treated his numbers as though they were all based on the same principles and the same processes, and this is simply not the case.

The two age methods that produced the oldest ages there at Lathrop Wells, the potassium argon and the uranium-thorium are based on fixed rates, fixed and known rates of radioactive decay which are the principles that have underlined the vast majority of absolute geochronology for the last 35 years. This is what the age of the moon and all these other good things are based upon.

Some of the other methods that he's applying, while it's admirable to apply them, these distinctions have to be made. Some of these other methods are based on such vagaries as cosmic ray production at some unknown time in the geologic past. But more importantly, many of the younger ages that he presented were clearly surface processes, and time and time again, that distinction was not made.

The most glaring example was the Carbon-14 date of the desert varnish at Cima dome. Obviously, that desert varnish did not form at the time of volcanic eruption. It had to have formed at some unknown, probably protracted period of time much, much later, and it's not fair to mix that age and compare that age with some of those by other methods.

Thermo luminescence certainly has value, although at the same time, it has its own problems. I thought it was rather amusing that he proved the value of the thermo luminescence by comparing it to a 95-million year old potassium argon age that was produced by a USGS lab, the very same data that he--type of data that he tries to discount at Lathrop Wells.

Obviously, these intense differences are not going to be resolved in the few minutes we have here today, but I would like you to spend a couple minutes looking at some of the data sets that some of the USGS people have generated.

DR. ALLEN: Let's let him go ahead here and get through with his presentation, if we may.

MR. HEDGE: Brent Turrin.

DR. ALLEN: Brent, let me remind you, by agreement we said ten minutes, I think, so...

MR. TURRIN: Well, I can do it in five, I think.

Okay. I don't want to dwell on this particular view graph, other than to give you the impact that there have been a lot of determinations made. These are a compilation of the

K-Ar ages from the USGS done in 1978, from the Sinnok and Easterling report done in 1979, and from recent 1986 K-Ar studies at Lathrop Wells. These ages were given in Bruce's presentation as preferred weighted averages for the data.

As you can see, all of the data aren't of the same quality. For example, here's an age of 120, plus or minus 20 kiloannums, and when we try to figure out the central tendency of these data, why should we give that age equal weight as to an age like this, for example? Weighting data is a standard technique used in a variety of scientific fields. I have discussed this personally with Brent Dalrymple and Marv Lanphere as to the validity of weighting data, and have gotten some input, as well, from Marco Bukuwinski (phonetic), a mathematical geologist at the University of California, Berkeley. And, as I said, weighting data is a standard--variance weighted data is a standard way to deal with data of varying quality.

So I'll move on to the recent article we submitted to Science, which is a compilation of 40/39 ages from Lathrop Wells on the exact same splits and the exact same samples we've already looked at.

This is the Unit Ql_3 in 24 determinations. That's less these four analyses right here, and I'll explain a little bit about those later. Arithmetic mean of 170, plus or minus 87,000, with a standard error of the mean of about 20,000 years. The weighted mean for the same data set yields 183,000 years plus or minus 21,000 years. These other points right here are actually outliers caused by xenoliths from probably the Topopah Tuff. Obviously, this one analyses is very different, using the 40/39 ratio--39/37 ratios, we can look at potassium calcium ratios. Based on potassium calcium ratios, all these points are anomalous. They're not the kind of ratios you would expect in basalts. So this explains some of the variability we see in our K-Argon ages. There's simply some tuff xenoliths that get into the 10 to 20-gram samples you do in conventional K-Ar.

All these analyses, I should point out, were done by laser, single grain, 40/39 analyses where we actually analyzed probably a grain a millimeter by a millimeter, up to two millimeters by two millimeters; actually, 2 x 2 x 2. These are cubic, you can think of them as volume.

These are the results from Units Ql_5 and Qs_5. I have subdivided them. These are the Ql_5 analyses seen in purple; the Qs_5 analyses seen in blue and black down here on these cumulative sort of--these are sort of--can be looked at as cumulative age plots. They give you an idea that there is some normal distribution to the data sets because you get the typical S-curve in a cumulative plot. But as you can see, the ages are analytically indistinguishable, and when we combine them, using weighted variances, we get 144 plus or minus

35,000 years.

Some questions have been pointed out about the possibility of residence time or excess Argon. Well, another advantage that we can do with 40/39 age dating is do isochron plots. These are the conventional 40-36, 39/36 plots, and these are the inverse plots. The isochron plots give essentially identical ages to the weighted mean ages. They also show that these have essentially a 40-36 ratio intercept, initial ratio of 295, which is the air ratio, so that would discount the argument of excess Argon being incorporated in this population.

This is a statistical parameter. MSWD is mean weighted deviance, and it should be one, if all your air in this number can be accounted for simply by the analytical dispersion around the point. As you can see, we passed that test as well.

This is the same kind of diagram for Units Qs/Ql_5, and, again, the ages are essentially identical to the weighted ages. Air intercepts, again, and mean weighted deviance of one or less; again, by the K-Argon community, these are very acceptable, outstanding dates to be contended with.

DR. ALLEN: Your five minutes are up.

MR. TURRIN: All right. Well, I've got two more view graphs, and they're quick.

Bruce mentioned some supporting evidence for these ages, and I'll just leave these up there and I won't bother reading them to you. You can probably read them faster than I can read them out loud to you, but essentially, the 120-140,000 years for Lathrop Wells is supported by uranium-thorium ages on the alluvial deposits that contain an ash that we believe has the most likely source to the Lathrop Wells cinder cone. Uranium-thorium isochron age of 150,000 on lavas from Lathrop Wells, and the Chlorine-36 exposure ages, which were lumped into the questionable, not interpretable data category, are actually quite interpretable. Given that these are cosmogenic surface exposure ages, and subject to surface spallation through time, they have to be looked at as minimum ages, and if you look at the minimum ages, these were presented to us at a Penrose conference in October; 69 to 105 kiloannums by Fred Phillips, so if we look at those as minimum ages, we know that the flow is at least--is older than 105 kiloannums.

So from this very abbreviated presentation, we can draw these conclusions. Now, you'll hear about the paleomagnetic studies in the next slide shot.

DR. ALLEN: That's three minutes.

MR. TURRIN: The paleomagnetic data indicate that there are only two events at Lathrop Wells, and I won't say any more because I don't want to detract from the next presentation. The combined weighted average from the K-Ar and 40/39 analyses indicates that these events occurred 120-140,000 years ago. This age is concordant with other isotopic geochronometers.

In conclusion, this brings up an interesting point, is that the geomorphic evidence and soils profile evidence obtained by Wells, et al., indicate an age of 20 kiloannums, and our conclusion is that they're incorrectly calibrated, and moreover, the geomorphic and soils evidences are in a bit of a predicament, because these parameters are essentially--are calibrated from the Cima volcanic field by 56 potassium argon agents that I published in 1985, so the question I have is, at what point do the geomorphologists and soils scientists basically step away and suggest that their data are more valid, essentially, than their calibrated numbers?

DR. ALLEN: Okay. Thank you, Brent. I think your conclusions are clear.

MR. TURRIN: Thank you very much.

DR. ALLEN: Duane Champion? You had a few things you wanted to say.

Brent, can we get copies of those view graphs?

MR. TURRIN: Yes.

DR. ALLEN: Thank you.

MR. CHAMPION: I've used geomagnetic secular variation studies to try to assess the duration of eruptive episodes in the volcanos near Yucca Mountain.

Geomagnetic secular variation is an after view to the earth's geomagnetic field that bears quickly over a relatively limited range, but it is a neat way to establish duration of geologic phenomena. Basaltic lavas record these different directions of magnetization with a high fidelity, and the techniques to obtain data from these rocks are standard. They're 30-year-old techniques. I am nowhere near the cutting edge of the technology. This was all invented by Dick Knoll and Allen Cox 30 years ago.

If you go into a region like the western U.S. and collect directions of magnetization, this is a equal area plot and you'll see several of them. It's a stereographic projection to plot unit vectors. Vertical downward would plot here; horizontal northward would plot at zero. These are 77 directions collected from Holocene Age lava flows in the western United States, and they show a significant dispersion. In a time frame of a hundred years, the field typically moves as much as five degrees in a seemingly random pattern, probably because we're too stupid to figure out the order in it.

It's possible to assign an error limit on the range of those kinds of variations, and I've done it here in this box; ±25° in declination, and ±20-25° in inclination. If we go to Lathrop Wells cone for the--you've seen the geology--27 sites record only--well, those are directions of magnetizations. Those are the means and alpha-95's from those

27 different outcrops, 388 cores. There's not a lot of dispersion reflected in those geologic units.

If you average them by geologic unit identification, you see this distribution for four different mean directions of magnetization. In particular, this direction here, Qs_5, one of the older perceived units, and this unit, Ql_3, differ by about 4 1/2° in angular distance. The kind of a time frame it could do that in is as little as a hundred years. Also, Qs_1, which is perceived to be the very youngest unit, and is the cinder cone itself--I've sampled the rim of the cinder cone from bombs, and, in fact, its direction of magnetization is completely blanketed by the Qs_5 direction. They're only different by .27°. On a statistical basis, you can assign a probability that they're different of their internal characteristics of only one part in 10,000. I think the odds are excellent that they represent the same geologic event.

If you go to the Sleeping Butte cinder cones for which, at one point two years ago, there were as many as five different eruptive episodes suggested; two different ages of cinder cones, two different ages of lava flows, and at least one in 10,000-year cinder cone, 13 sites reflect--mean directions of magnetization reflected by only those two different means. One is for Little Black Peak here, and one is for Hidden Cone. They're about 4 1/2° apart. Again, possibly five times, separate volcanic events occurred, really reflected with minimal directional variations. Suggestive that, in fact, they constitute two separate, but closely spaced eruptions.

If we go to the million-year centers in Crater Flat, we've taken now as many as 20 sites in the Crater Flat lava from the Little Cones, Red Cone, Black Cone, and northernmost. I have not seen or had in my hands a geologic map in order to do the analysis of these different centers, but when you look at the 20 directions of magnetization, they look like this. In this case, they're reversed polarity because they're 1.1 million years old. They're part of the Matsayama Epic. The distribution is here and, again, in a very tight kind of a way, and this is a pretty interesting possibility because all four centers, when you mean on the basis of the four centers, they, in a sense, show the same direction of magnetization for all four centers.

The larger alpha-95 circles, or two standard errors of the mean circles a confidence for these two are for northernmost and Little Cones, from which I could only get three sites to sample. They're so small, unless you want to sample on top of yourself, you're really not in an independent location.

The point being that this small dispersion of overall possible directions--and again, here's our envelope of possible secular variation. In this case, it's been reflected on the upper hemisphere, upward directing reverse magnetizations--suggests that all four centers in Crater Flat were essentially erupting at the same instant of geologic time. I'm not seeing more than a hundred years of total time duration in all those eruptions. The outcrops include north, south, east, west, rims of cinder cones, dikes in cinder cones, lava lakes in cinder cones, as many different looks--in the absence of a geologic map--as I could possibly collect.

The raw statistical odds that those four directions are a random selection from all the possible variations is one part in 10,000, so at least the event from Crater Flat, I think, has to be imagined as a simultaneously eruptive 11 1/2 kilometer long structure. Preliminary data from the 3.7 million-year centers reflect the same form of a group, a different reverse direction--in this case, in the Gilbert reversed epic.

The data that I have shown essentially reflects the traditional concept of monogenetic volcanic eruptions. This figure from Chuck Woods' 1980 paper on the morphology of cinder cones was used to suggest the notion that the average eruption duration is only on the order of a month, and that 93 per cent of eruptions are over within a year. A couple of exceptions were Peracotene, which took 3300 days, I think, to do its whole thing, and a volcano named Harulo that took 15 years.

The data that I seem to get from the Yucca Mountain centers suggests that this diagram perhaps should be extended yet another order of magnitude. There are three and a half orders of magnitude of time variation on this diagram. By extending it one more, to on the order of 36,000 days, we can encompass other monogenetic volcanic eruptions, depending on how one exactly wishes to use the term.

I think we should pay close attention to how radical a suggestion polycyclic volcanism is on the basis of what the conventional thinking was before, and I'm not finding, in my data, reflections of the polycyclic manifestation.

Thank you.

DR. ALLEN: Okay. Thank you, Duane. Both of those were very clear, concise presentations.

Carl, do you have any--before we go into questions, do you have any further summary statement you want to make?

MR. HEDGE: No.

DR. ALLEN: Let me ask if either Duane or Brent have any burning need to say anything more here at the moment. I realize they've been very short.

First, let me ask if there are questions or comments from members of the Board or the staff.

(No audible response.)

DR. ALLEN: Okay. Bruce, you previously asked to say something. I think it's fair you--again, limit yourself to a short time, though.

MR. CROWE: What I want to try to make clear is this first two points. I think it's a little bit unfair what Carl Hedge had to say because the people doing the helium and the TL, the uranium-thorium are very, very aware of the differences between potassium argon and cosmogenic versus other. It is really kind of an insult to their professionalism to suggest that we're not. We clearly are well aware of this. I didn't go into that simply for time constraints. It's in my study plan.

But the point I really want to emphasize in just a brief comment is I touched on the kind of passions that this problem has ignited, and the perspective I'd like to talk from is that we really are looking at this in a different perspective than classic science. We're looking at it, what does this mean to Yucca Mountain? And we have over-exaggerated the potential concern: Is there anything young there? We're not attacking K-Argon. We're not attacking paleomag.

What we're saying is, is there any evidence that these centers could be young, and how would we factor that into a risk assessment? And we really would like to try to avoid these contentious difficulties between systems. We really would like to look for convergence and try to work things out, but we have to recognize, as I mentioned, the danger of a false positive. If there is any chance that those things are young and we say they're older, we've made a serious error. But if we say that they're young and they're older, we've just made a conservative error, and that's the perspective we're coming from.

We really don't--are not trying to pit things against each other and say, which process is best? We would be the first to admit that potassium argon is the demonstrated technique that's been used for decades, but we think we're asking questions that perhaps have not been asked at the same scale ever before this time.

DR. ALLEN: Okay, thanks.

Mike?

MR. SHERIDAN: Yeah. I would like to make a comment about Duane's data, and that is that if his data and interpretations are correct, then what he's saying is that there has been only two volcanic events in the last one million years, and in the last four million years, only three events--four events, five events, which means there's one event per million years, which is one in a million, which seems to come back to what we've been talking about.

I think what Bruce is suggesting is maybe the events are possibly more frequent than that, but smaller and less dispersed. But from the point of view of what I think we should be considering as a community is, what is the risk implications of the interpretations, and I think that the scientific difference is not really affecting that aspect between these two parties.

DR. ALLEN: Well, I think that has yet to be shown, whether it does make a difference.

Duane, would you agree with Mike's sort of summary statement?

MR. CHAMPION: In principle, I would. The principal differences I see between the two models is if mine conventionally interpreted premises correct, we know relatively little about the spatial predictability of the next volcanic eruption as far as the risk assessment goes. Mother Nature has not erupted many times here. There's only been five events in the last 3.8 million years.

If Bruce's polycyclic model is correct, it erupts more often, and we have the big advantage of spatial predictability. We know exactly where. We have excellent odds where the next one ought to be on the basis of what's happened in the past, and so it's a fundamental--the scientific difference does relate, I think, to the risk assessment. It's difficult to separate it out, and I see tremendous confusion now in how the probabilistics can be handled when we have--I see a fundamental division in the phenomena we expect.

DR. ALLEN: Brent, what do you want to say?

MR. TURRIN: Yeah. As Bruce has mentioned in the probability calculations, the age is probably pretty well buffered, but let me make an analogy here. If someone tells you that you have a 20,000 year old volcano in your back yard, what is going to be the public visceral perception of that as opposed to if somebody tells you that you have a 130,000 year old volcano in your back yard? So there are some political issues as well, and we want to make this decision based on solid data. So I think it is important to establish a truly defendable age for the cinder cone, and not take the conservative approach, but take what is the best defendable data for the age of the youngest event.

DR. ALLEN: Thank you.

Other comments or questions? Yes, please. Les McFadden, University of New Mexico.

MR. McFADDEN: This question is directed towards Brent and Duane, and I notice in your presentation that you put up some of Brad Phillips' work on Chlorine-36 dating, and you've noted that that supports your ages. So do you generally accept Chlorine-36 dating, or--

MR. TURIN: With a caveat.

MR. McFADDEN: What's the caveat?

MR. TURIN: It's a minimum age.

MR. McFADDEN: Okay. He now has two populations of ages of Chlorine-36 on bombs and flows that differ in age by about

30,000 years, which indicates to him that these are definitely polygenetic or polycyclic eruptions. So, Duane, do you accept the Chlorine-36?

(Inaudible response.)

MR. McFADDEN: So you don't accept that they're polycyclic on the basis of the two clusters of Chlorine-36, but you do believe, in general, that it can be used to support the K-Ar.

DR. ALLEN: Duane, would you--legally, we're required to record this. Could you stand up? And you're being recorded. All of you are being recorded legally, whether you like it or not.

MR. CHAMPION: I understand. The essence of another difference in those two ranges is they're different physical materials, and this is an attribute of a physical material. And so the properties could change from identical aged materials, and so the different populations, you know, may be different kinds of minimums.

MR. McFADDEN: But you don't think he can tell the difference between 40,000 years, at the level, say, of 160,000 years old, even given that it's a--

MR. CHAMPION: Chlorine-36, of the exposure techniques, has done the best job so far trying to cross-calibrate to proven chronometers. Helium's been trying to do it; Mark Kurtz and helium, or in that Carbon-14 dating and such, but there has been minimal effort in a lot of the exposure techniques yet to cross-calibrate, and as we heard at the Penrose meeting, the production rate for helium is still largely up in the air. There's a significant division there that they have to resolve. You know, I see Chlorine-36, helium, and Beryllium-10 and all these others as developmental techniques. They're in their developing stage, and we're asking a lot of them to throw them into this question.

So I conditionally embrace the Chlorine-36, but understand they might be different minimums, and they're not necessarily reflective of polycyclic volcanism, although they might be.

DR. ALLEN: Brent, I guess, wanted to say something else.

MR. TURRIN: You maintain that there's two separate populations, but there's no upper bound on those populations.

MR. McFADDEN: Brad maintains it on his own.

MR. TURRIN: Well, it's--okay. I mean, my interpretation of minimum ages is that his cluster at greater than 60,000 years -- -- greater than 105,000 years. You can't see two populations, given those parameters.

DR. ALLEN: Other comments? Les, I think since they've been speaking and you haven't been speaking into the mike, if you want to say something into the mike, please do; briefly.

MR. McFADDEN: The only reason I say this is you asked a question directed specifically to me and Steve Wells on soils and geomorphology. You've made the comment that we calibrated soils using potassium argon age techniques in the same volcanic field. In fact, the soil age at the A-cone or the A-blow was based on an estimate long before K-Ar dates. It was done in 1983 and was based on calibration with Holocene alluvial fan deposits and terrace deposits. It was not calibrated on the basis of K-Ar, so that is in error.

DR. ALLEN: There was one other hand up.

MR. VALENTINE: This is for Gene Smith.

DR. ALLEN: Okay. We're going to have to call this off presently, but if you have a question for Gene and we don't want to pursue this, go ahead.

MR. VALENTINE: Okay. My name is Greg Valentine from Los Alamos, and it's fairly well accepted for fracture propagation of magmas that the plane of a dike is perpendicular to Sigma 3, the minimum of stress. You were suggesting that the intrusion patterns would be perpendicular to that direction. Do you have a mechanism for that?

DR. SMITH: I was suggesting that the dikes are being emplaced along preexisting structures, and that the fact that the northeast segments are oriented at right angles to the Sigma 3 direction, so that these structures would be the most conducive for rising magmas to then use as channel ways. I'm not suggesting that the dikes themselves are propagating fractures. I'm suggesting that these are preexisting structures, and these structures would most preferentially be used by the rising magmas.

MR. VALENTINE: Structures on the Colorado plateau suggest that dike intrusion can only be controlled by preexisting fractures within a very narrow range around the orientation of Sigma 3, so it might be ±10-20° off the perpendicular from Sigma 3, but I don't think you can put it at right angles without coming up with a good mechanism for that.

DR. ALLEN: Okay, thank you. We have one--

MR. SHERIDAN: Could I just make a comment on this? That at the site, Sigma 3 is located to the northwest/southeast, so that I think the direction predicted by Gene would follow that pattern.

DR. ALLEN: Okay. One final comment or question by Bill Melson.

DR. MELSON: Yeah, I have a question for Duane about your calibration of your scatter of the data, which you then used to compare with these paleo data. That was based on direct measurements of pole locations within the past one hundred years or not? I'm--

MR. CHAMPION: The very first figure that I showed you?

DR. MELSON: Yeah, which you used then to show how tight the later data was.

MR. CHAMPION: Those were based on Carbon-14 dated

Holocene Age lava flows. That was 10,000 years of secular variation, the dispersion cloud due to that force.

DR. ALLEN: Okay.

DR. MELSON: So then that was--say that again.

MR. CHAMPION: Well, when you can reconstruct secular variation--and I have a view graph I could show from Hawaii if they call for it later--it forms kind of a smarmy path, a pile of worms on a plate kind of a path through time. If you lose track of the order of directions of magnetization, they just appear as random distribution of points within the broadest range of the distribution, as I kind of reflected by that quadrilateral shape. In the Holocene, from either Carbon-14 or soils, you know, different kinds of ways of declaring a lava flow to be Holocene, 77 units made that cloud of data. I mean, if polycyclic chops up time in big pieces, we ought to see those kinds of jumps, yet we always see the two directions, perhaps, at a center, but it's always right nearby, and the probability that it's random is quite small.

DR. MELSON: Okay. Thank you.

DR. ALLEN: Okay. I think we'll call it quits. You'll recall that on the agenda we're reconvening again at twelve-thirty, which we will do so. And let me thank the speakers the last few minutes here. There's been a very, I think--I appreciate the candor, and so forth. I realize that there's some contentious issues. I also think it demonstrates Yucca

Mountain offers some exciting scientific opportunities, although we must keep in mind that our obligation in the long run has to do with the suitability or unsuitability of the site.

Okay, thank you.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

12:30 p.m.

DR. ALLEN: Okay. The first presentation this afternoon, starting our probability discussions, is by Chih-Hsiang Ho of the University of Nevada at Las Vegas.

DR. HO: This provide us a good excuse to fall asleep, and I forgot to state a theorem. The theorem is: if I say something which is geologically nonsense, that's for this purpose.

Now, let's go back to the business now here. The title (indicating)--and that's me, and I specially designed it for this talk. Okay, now, start the talk with the goal. Why am I here today? Number one, I want to estimate the recurrence rate; and second, I want to estimate the waiting time for the next eruption, and then calculate probability of at least one future eruption during the next 10,000 years, and then the probability of that disruption. But I'm here (indicating). So far, I'm here. I need more money to continue the fourth one.

Now, approach number one is that we need a model which captures the basic elements of this study. The first one is objectivity, and the second one is the trend. I think the trend is very important. I will show that in the next couple minutes. And then the third one, the model should provide the ability of prediction, and the fourth one is I want the model to be as simple as possible, but not over-simplified.

So now we are talking about a time series generated by stochastic phenomena, or events. For example, I have a data recorded versus time, 34, 14 and a plot--this is a kind of dot program, very common in statistical analysis. That's the first step to doing the business. For example, something happened at the first circle, and that's called an event; it can be an earthquake or a volcanic eruption or something else. Depends on your interest.

For example, the next one provides an easy understanding about what are we doing now. Suppose now next time you go fishing and that you have nothing to do while you are waiting than record the time of the catches, one-by-one, then you generate 14, 34, 42, and so on, but once I show this to my student, and my student told me, "Oh, gee, it's time to go home because now the last waiting time is so long; 244 minutes or seconds, whatever." So that means these slides imply, even based on five data points, you see something, and that something is the trend.

But unfortunately, somebody just give you this one. Piles of fish, and they ask the question, "How about the probability of catching at least one fish during the next 24 hours for me, please?" Gee, I will be in trouble. I have to understand a lot of things. Seems like what? I have defined what is a single event; a fish or whatever. So in order to answer this question, I have to ask, "Please let me know what's your fishing technique?" or "In the volcanological study, what is the eruptive process?" We have to understand this first before we start anything.

So, for example, this is very intuitively easy to understand. You use a single hook or multiple hook, or a net, a fishing net, you catch a bunch of fish one time, then how can you measure that? So your model should fully--are based on the understanding of fishing technique.

Once you know the fishing technique and the definition of single event is defined, then you say: "What is the variable of interest?", in order to model the probability. So, for example, so many things can measure all the fish; length, weight, volume, or age or--and here, I believe, is what--how fresh are the dead fish, or how dead are they? Right. But now you have to understand, do we have a reliable, consistent technique to measure the freshness of the fish? If we don't, we are going to have a debate, just like this morning; end up to nowhere, and no agreement to anything, so why do that?

So now the whole thing can be summarized thus: Define a single event, and then what? Measure each event, count them all, and then you generate a time series and you are ready to find a suitable model to tell your story.

Now, let's go back to the time trend. I used the same data and I kind of intentionally permute that one into three cases. Suppose these three cases, representing the eruption of three volcanos, do you see something out of these three time series? Sure; why not? This is the story.

We see that a waning volcano, a random volcano, and then a developing volcano. Is that convincing? I think so. So the trend must be one of the important factors in model the volcanic activity.

Now, let's talk a simple Poisson model, which is the basic, simple, elementary model in modeling the stochastic process, but unfortunately, the Poisson model ignores the time trend and assumes a constant rate. Now, the rate of occurrences--which is denoted lambda on there--the best estimate for that lambda, if you have data is the number of events divide by observation time, or the reciprocal of average inter-event time. So that's the standard way to estimate, and that's the best way to estimate the recurrence rate of a time series data, and that's important.

Okay. Now, since Poisson does not have ability to detect the trend, so we modify that one parameter to be a function of time. So you let time take care of the rate. So it becomes a function of time, and now you are talking about I'm going to model the number of events during time zero to time t, for example. These t's can be reversed, saying that ten minutes ago up to present, so you have a time trend, and then you're talking about counting the numbers of events during that process.

The theory indicated that you have another Poisson process, which is called a nonhomogeneous Poisson process; depends on the choice of μ(t). You have different methods of this process. So, now let's see. The choice that we make is important. This choice is based on our objective and the purpose of doing the modeling. We chose lambda (t) equal to this one, which give us a μ(t) equal that one. That implied the model is a well-known Weibull model with two parameters, one more parameter than the Poisson, and the reason that we choose this one is because: there is one parameter which indicated that whether the trend is increasing or is greater than, which is Poisson; or which is decreasing. So that's our goal. So we stay with this one and show you how it works.

The whole idea is you collect the data, or you record the time of eruptions at t_1, t_2, up to t_n, that this t is cumulative time. For example, the first two eruptions will give you the sum of two repose times, and so on, so you can estimate those two parameters, and then, also, the lambda t or the lambda hat. Later on I will define what is a lambda hat here. So this step indicated that those parameter estimations is your first step to quantify the activity of the volcano, or the Yucca Mountain volcanic activity. So we will quantify that one.

Now, how it works. For example, those three volcanos, if I apply those parameters, estimation technique, we end up with β = .63, .99, and 5.4. That is complete agreement with our Weibull analysis. Less than one indicate a slowing down, close one indicate it's random--that's a characteristic of the Poisson process--and the third one is developing, and that give us a strong signal saying that something has begun, and this actually is quite useful in modeling the quality control, and you want to see a machine need to be tuned up, then you say, go ahead, replace the machine, or do something else.

Okay. Now, this technique has a very nice application, which is when you say $\beta = .9$, is this .9 significant enough to say that it is equal to one or not equal to one? So we can actually do the hypothesis testing, saying that this is the Poisson, versus, now, this time series is not a Poisson, or in terms of beta, is equal to one or not equal to one. That's a good indicator and it can be statistically tested. Not just say, oh, I see that this is increasing or this is decreasing. You've got to provide scientific evidence through the statistical analysis, and that's what we're doing.

Okay. Now, we're talking about the rate, lambda. I said before, a lambda is your model--you collect your data. Now your present time is (t). This model allows you to calculate the lambda hat. Lambda hat indicate this is the instantaneous recurring rate. The process has been going all the way up to here, and you want to measure the intensity at that moment, and try to use this one to predict the visual.

You are not taking the average over all, you just take some-- cumulate everything up to now. That's the most informative information you've got at this moment. So lambda hat is important, so we're going to estimate that one for the Yucca Mountain site.

Okay. Now it's time to talk the Yucca Mountain. Exciting here. Now, t, t is the time frame. When you talk about trend, you can now say, okay, I started with two million years ago or three million years ago or 3.7 or 6. You've got to be objective or whatever. You've got to have an idea, saying that, when is your starting point? You cannot pick a random starting point and say that the trend is increasing. If you moved either back, your count decreasing, so it should be kind of a convincing term of t, but I'm a statistician, so I just go for the literal research, and now a lot of people talk about a Post-6 million years of volcanism or quaternary volcanism, so I will base on this, too, to see whether the results will provide some consistent information. If it's not, then we do something else.

Now, again, we go to Yucca Mountain. Let's go back to the goals, things we have some knowledge about, those terminologies, so recurrence rate, waiting time, the probability of so and so. Now, let's see how to identify a single event.

Identifying a single event, again, is as controversial as the dating. You can say, okay, my single event is determined in a cluster of centers, volcanic centers, or the whole volcanic belt, fine; or a volcanic center, a single center, or maybe a main cone, or maybe a small vent. Either one will be fine to me, or to any statistician, as long as you can well-define this single event based on geological reasoning. And I'm not qualified to do that. Somebody certainly is qualified to do that, and I assume this reasoning is okay.

"A main cone is the final stage of a single eruption, and a single eruption could have several small vents to accompany the main cone.", Crowe and others, 1983, so the following analysis is based on this crucial assumption. I do not take the responsibility of whether it's wrong or right.

So now, based on that definition, we count each widely recognized main cone as a single event, but do not require that the main cones in each center be of separate ages. Is this (indicating) understood here? It's very important.

Let's see, here we have a lot of good talks in the morning--therefore, I don't need a picture or a map--3.7 million basalts (indicating). We count four main cones (indicating). This is from the estimate of a nice, a good geologist, Dan Feuerbach, okay? And then Buckboard Mesa, 2.81, but I'm not sure one is right or not (indicating).

Again, (indicating) Red Cone, Northern Cone, Black Cone, Little Cones, you probably have different ages, but so far this is in the literature, so that's the only thing I can count on. If you don't want debate, fine, we have more things to go and to do.

The next one is Sleeping Butte Cones (2) (indicating), Lathrop Wells Cone is hot here, .01. I just picked the middle point of zero and .02 in the middle, so that's a robust estimate, if you are not certain about anything. So we took that one, but just count one. I go back to polycyclic. So now, the summary is we got a data set that we can try to model that, so that now here (indicating), I wanted to make a point.

The data in the Yucca Mountain is sufficient for probability calculation. Statistical techniques have ability to model the probability if you give me three. That's enough. But whether this probability calculation, or the result, fully reflects the story of the Yucca Mountain, I'm not sure. Depends on the completeness of the data collection. So, so far, there are two stories. We can do it, but the result, value or not, is questionable.

So now, based on these two data sets, one is a set, quaternary (indicating); second one is Post-6 million years. We found that Beta hat for this Post-6 Ma is 2.29. That indicates an increasing trend. This p-value indicated that--

small p-value indicates the trend is significant. The volcanic activity is increasing and developing, and lambda hat is a recurrence rate. This is an instantaneous recurrence rate estimate at the present, current, time if this volcano is alive. This is the power. This is the intensity this Yucca Mountain is going to provide, and now I'll go back to the quaternary. You have a moderate increasing trend, but it's not significant, but look at this, too. Those estimates are very close; 5.5 and 5 x 10_{-6} per year, per year. This is rate. This is not a probability. This is rate.

MR. DOBSON: On your previous view graph, you said that the beta value, 2.29, indicated an increasing rate.

DR. HO: Yes.

MR. DOBSON: Now, obviously, you don't have--

DR. HO: 2.29, yes.

MR. DOBSON: You don't have any volcanic--you don't have any volume--you don't have any kind of rate included in that, so you're just dealing with number of events.

DR. HO: Okay, yeah. Because when you're talking about Poisson, this is a very--

MR. DOBSON: Well, no, I'm just trying to understand.

DR. HO: Yes. So far--I understand your question. So far, the only thing I measure is the time, associated with number of eruptions. No magnetic volume involved, nothing else. So far, all I need is number and date. If you are talking about the rate, that's another story. You cannot model the recurrence rate based on the volume, just like you want to model, what's the probability I catch the next fish on just a measure of the volume of the fish. That's different story. You can catch a very big one, or you catch ten small ones. So be careful here, volume, I didn't touch volume yet. Maybe in the future.

Did that answer your question?

MR. DOBSON: Well, not exactly, but it'll do for now.

DR. HO: We'll go back to that sooner or later.

DR. ALLEN: We've got about ten minutes left here.

DR. HO: Okay, good.

So now, the estimated rates of this one, it represents the instantaneous eruptive status of the volcanism at the end of observation time t, which is present time, as I indicated before.

Okay. Now, time to do some predictions. Based on the quaternary, we observe so many years, which is a long time. Now we're going to predict into the future, which is just 10,000 years. Now, this is a tiny time period. So, one, the projected time is just about 0.6 per cent of the observation time, and is only 5 per cent of the average repose time, so that means, now, I feel comfortable to shift from the non-homogeneous Poisson to a Poisson model, because I can assume the constant rate within that small, tiny interval.

To make it easier, not only that, but also the following: mathematical simplicity. We got Poisson, then we have recovered the simplicity of that one, but we didn't oversimplify that. And second one is objectivity here, should the trend continue. The answer is yes or no. In the literature, you see that different volcano have different reviews, up and down, up and down, so I'm taking a neutral position up to now, while I see I can find the lambda hat, but in the future, assume it's random. So that's called objectivity, and also, the trend for the quaternary just slightly, is not significant, so why bother?

DR. ALLEN: But this trend and this rate have to be for a specific area that you have defined; right?

DR. HO: Define what?

DR. ALLEN: It would have to be for a specific area. It doesn't include Death Valley, it doesn't include--

DR. HO: The area, I believe, according to--yes. According to Dr. Gene Smith's talk, AMRV is the area.

DR. ALLEN: Oh, that's the area?

DR. HO: That's the area.

DR. ALLEN: Well, that's important; that's important.

DR. HO: Because the time, all the time is from those volcanic centers from around there. Yeah, so we assume that is a constant rate in terms of time, and giving--talking about a spatial distribution of those volcanos. That will be the next--in the future here.

Now, once we determine to go to the Poisson, the number of eruptions during the next t_0 years 10^4 is this one. So now we are ready to do the predictions.

The average time to the next eruption is a reciprocal of the rate, and the probability, based on this simple Poisson model, everything comes from that one, and now we have--the sum over here is for these two periods; β hat, and we talked about that, and then the waiting time to the next eruption is a point estimate which is .20 Ma or .18 Ma. A common interval, which is 90 per cent, is .11 Ma up to .45 Ma.

So this is a interval, if you want to estimate the time that we have to wait in order to see the next new eruption. And then isolation period, either the first year or 10,000, or, a little bit more aggressive, up to 10^5. And now, common interval on this one, 5 per cent, you're going to see at least one future eruption in the Yucca Mountain, based on that data set.

MR. DOBSON: In the AMRV?

DR. HO: Yes, in that region. Suppose that is the territory of interest.

Okay. Now, we got the result based on that one. People talk about a polycyclic volcanism here. Assume that this is right. Okay, again, how would that affect the probability calculation? The answer is certainly yes. I assume that if you have additional three eruptions associated with a single volcano, at Lathrop Wells there, then you see how β have increased from 1.09 to 1.5 and p-value is smaller. That indicates the trend, increasing developing trend is stronger. That makes sense. The addition of three events is an indication of a more strong increasing trend, and then how about the instantaneous recurrence rate? This shows increase because you increase three numbers. Three numbers easily double the recurrence rate, double it. You need three, only, to double the recurrence rate. You need three to make the waiting time 50 per cent sooner.

So that means the calculation, the probability calculated before, is what? Incomplete, and that probability is the lower bound, not upper bound; the lower bound, because what you see, it's there, but another thing that people don't see, how many eruptions associated with polycyclic volcanos? If you graph them out, count them, I believe the probability will be much, much higher. So some data collection certainly is required to tell the whole story.

DR. ALLEN: Okay, thank you.

Do we have questions or comments from the group here? You look like you have a question, Bill.

DR. BARNARD: For the dates for the Lathrop Wells, you used .01?

DR. HO: .01.

DR. BARNARD: What if you used .1?

DR. HO: .1, certainly, we can easily modify the model and do that, and so that's a very good question; just like why I add three more, and you see the change. If you change from .01 to .1, you'd certainly see some change. Which direction, I'm not sure; depends on you do the plotting, you see whether the trend is increasing or decreasing. There probably are some slight changes. So that point indicate the date. The accuracy of the date is crucial, is crucial.

DR. ALLEN: Bill Melson?

DR. MELSON: Your analysis would apply either to a single vent or many vents in this case, so what we have here are scattered vents, which you treat as one--well, you don't treat it as one, but you're looking at a regional picture. And what we want to know is not only the time, but the space; in other words, where will they happen?

DR. HO: Okay. Now, the space certainly--spatial distribution or spatial modeling is more important in the probability calculation of site disruption. Then they rate calculation. So rate calculation, you can lump them together, but when you consider about the site disruption, then that's certainly a big factor, and this is in progress now. I'm going to model that one.

DR. MELSON: But you need more money for that?

DR. HO: That's for sure, yes; that's for sure. The more I do, then, let's say, you have to do that and that. That's true.

It's much harder, much harder, because you need to know the geological stuff. Without that kind of knowledge, you cannot just put the models in. That's the model. So I have to do more research in the literature, reading, and certainly need the -- information and geologically.

DR. LANGMUIR: Your polycyclic concerns, what if they apply to all the other past dates as well as to the modern dates? It just swamps it all in, doesn't it? Aren't you back to where you started from?

DR. HO: Exactly, exactly. The reason that I show just Lathrop Wells, because that's the only volcano that being studied and identified as polycyclic. Other volcanos should be researched and studied and provide evidence whether it is polycyclic or not, and then we can incorporate that one. For example, this 3.7 can be extend to ten 3.7. Actually, 3.7 is not enough. It should distinguish each single eruption 3.85, 3.9, and so on, but so far the dating technology is handicapped by the accuracy and consistency. So that's a good point. You've got to study any and all the volcanic centers, and then list all the data set; have the complete data set, yes.

DR. ALLEN: All right, Leon?

DR. REITER: I know you haven't done the calculation, but you could--under the assumption that there is no structural control, that everything is random, and under the assumption that you have an average area that would be associated with one eruption, what's the likelihood of Yucca Mountain being disrupted?

DR. HO: I don't quite understand your question. You say that you lump several events into one single event?

DR. REITER: No. Assume that there's no areal control, that it is Poisson, that it's a homogeneous Poisson process areally, okay.

DR. HO: Yes.

DR. REITER: Okay, then I can then--if I assume--if there's an area of average size associated with an eruption, what's the likelihood of getting--affecting the repository?

DR. HO: Okay. Now, actually, when we're doing the modeling, that--this one over here, 1.09, is already indicated that it is a Poisson. It's close to one. So that's based on the Poisson in that area already, one slightly different, but statistically not significant.

DR. REITER: I'm not sure you understood my question. I'll repeat it again.

According to this, there's a 5 per cent likelihood that you'll have an eruption in the next 10,000 years in the AMRV.

DR. HO: Yes.

DR. REITER: Okay. Now, let's go one step further. Assume that there is no structural control, that everything is completely random. If I assume that an average size affected by a volcanic eruption, what would be the likelihood of affecting the repository?

DR. HO: Here, again, you are talking about a different issue here. The Poisson applied to the time only. Time only, have nothing to do with the area.

DR. REITER: I'm asking the question. Let's assume--I want to see the implications of this, of assuming spatial randomness.

MR. CROWE: Can I jump in and try to just give you some bounds on that? That's what we've done. It turns out that it--because the repository is buried at roughly a thousand feet, what your--it becomes the dike dimensions of those depths, but then that factors out and becomes the area of the repository divided by the area that you have defined your rate, and, you know, that's what we did in 1982, including Buckboard Mesa, not including Buckboard Mesa. Our numbers with the random model fall in the range of 10^{-3} to 10^{-4}.

DR. ALLEN: Okay. We'll come to that in your presentation. I think we just have to move on here if we're going to maintain any kind of schedule, so may I call on Mike --thank you. A very clear presentation and well within your time constraints. Thank you.

MR. SHERIDAN: What I would like to do is to first present the method that we developed for EPRI to make some estimates of exactly some of the probabilities we're talking about, to fit them into the tree diagrams to calculate the effect of volcanism, and I would like to take about half of my period to do that, and I would like to then take the rest of it to discuss some ideas that are my own ideas, and I will be speaking as a professor from a UV, rather than as an EPRI representative of a group, and this is certainly non-reviewed off the top of my head, but I think it's much more interesting than the rest of it, which has been surpassed by recent ideas.

Okay. First of all, discussion of the branching at the volcanology node. There are four main branches that we made at this node, and what I should say is that the estimate of the probabilities that you see assigned to these branches are based on the pinning down of various factors, and we tried to make these branches as simple as possible, although they could also be expressed as probability distributions, but in almost all cases, they're expressed as branches.

So we can see that there would be no effect of volcanism, that volcanism would affect the hydrology. In other words, dike intrusion nearby the site would add a heat impulse or would add a stress impulse that would cause changes in the water table, and we then put those changes in the water table at either no rise, or 80 meters, as being sort of not significant or some sort of significance. So we pinned everything down.

And the probabilities in many of these cases are just drawn by flipping a coin. This is a 50 per cent probability, so that's exactly flipping a coin. Other probabilities are based on existing data, and what I was going to do is throw out a big disclaimer and say that these numbers that are down here are just numbers we drew from the literature, but from everything I've heard today, all of the numbers are exactly consistent with what we've seen in the past.

The third branching node down here is magmatic release, meaning that the magma intrudes the repository site, and there's a release of material onto the surface because of that, and then the fourth is a hydro-magmatic, meaning a steam blast-type of explosion.

Now, the probabilities assigned here are conditional probabilities. It's a probability, that given a volcanic event, that that event will intersect the site. So we're taking the material that Professor--the estimate that Professor Ho has given at something like 10^{-5}, 10^{-6}, and then saying, given that sort of an event, what is the probability that event will intersect the site? So that's the sort of thing that Leon--the question that Leon was asking.

I want to give some of the background, because I don't think that we can make these kinds of estimates without a firm understanding of the geological processes involved; that we can't just--not all fish are equal in this case, and I think we have to take into account these processes and evolve this from solid volcanological and tectonic thought.

The problem relates to two considerations. One is a subsurface consideration, that there are many events at volcanos were nothing is produced at the surface, but the volcano swells up, there are lots of earthquakes, and then nothing happens. Well, we all know that magma is moving up towards the surface, but it doesn't break onto the surface. And this is a diking or a fissure-filling event.

If you look at areas such as this in Canada, where dike fields have been mapped out, we see there's a great regularity in the pattern. There's a great regularity in the pattern, and conventional wisdom at the present time is that the diking events are perpendicular to Sigma 3 at the surface, and this means that for the Yucca Mountain site, we have quite a bit of data on stress release from earthquakes, and we have, also, data from stress within boreholes, and from this data, we could predict what the near-surface orientations of the dikes are most likely to be, and these should also be parallel to the orientations of fissures at the surface, to linear alignments of scoria cones on a single cone, and also to chains of scoria cones, and we find that that data is perfectly compatible, and that's more or less as Gene Smith had pointed out this morning.

What does a dike look like? Well, there apparently has not been much National Science Foundation funding in this area, so only three or four people in the whole country are interested in mapping out dikes and understanding how they work. But those people are very good, and we have some good data on dikes. Here's a case of a dike near the Four Corners area, showing that dikes generally occur in branches or segments, so that a single dike could be very long, but there are segments of dikes. And this has to be taken into account in considering the hydrological effect. A dike isn't going to dam the whole aquifer and cause a massive rise in water. There's going to be a certain amount of leakage through the dike.

Also, in terms of the portion of a dike that's going to--what is a dike going to be like if it does intersect the repository, and I know that a lot of work is going on in this area. It's not something that we really got into, but it was something that was necessary in formulating the model. So we can see that, typically, or at least we can see conventional wisdom has it that a dike enters from depth along a single opening, but as the dike approaches the surface--and that opening is affected by the regional stress field and the deep stress field--but as the dike approaches the surface, the topography, local stresses, inhomogenates of the rock, and so on, cause a rotation of the stress field, and the dike breaks up into dike segments.

And this has been seen in exposures of dead dikes. It's also been seen in development of dikes at active volcanos, and I should mention here that I study active volcanos and the time framework that I'm mostly concerned with in predicting volcanic activity is the time frame of, say, 400 years or 500 years, historic record at a volcano. So that to predict for 10,000 years or 100,000 years into the future is a very, you know, just to predict for two months is difficult, but to try these long predictions is very difficult. So that's the first part of the spatial consideration, is dikes.

And in our study, we looked at all of the published data on dikes, wherever we could find them. We found out that the dikes were consistent with the regional or near-surface stress field, and that these dikes--the orientation of dikes didn't deviate by more than ±15° from whatever the trend was. So that in the model, in the EPRI model that we use for volcanology, we're able to say that if we knew where the center was going to be, we could put the feeding dike for that center by giving the dike a certain length, with a standard deviation of lengths, and with a certain orientation, a standard deviation of orientations, and then by Markov simulation, we simulate large numbers of dikes for various distributions of volcanos within the field in order to calculate the probability.

And we had a little algorithm that would calculate, is the dike going to intersect the repository? We just digitized the repository and we calculated those intersections, and just a matter of letting it run on the PC for a few minutes and you get that data.

But what about the surface expression? Well, let me say that the distribution of volcanos, the spatial distribution of volcanos is significantly dependent on the size of the event, and I think there are many analogies to be drawn between seismicity and volcanism, and therefore, volcanologists, we need to come with some sort of moment measure of the events, and if we had a moment measure of a volcanic event, we could plot the longer the frequency versus the moment, and probably get a straight line for energy production. In fact, people have worked on this.

When we look at distribution, the gross aspect--and let me say that we really chose a simple model. The gross aspect of distribution can be seen in the Pinnacates volcanic field, which is less than a million years old, has more than 400 cones, and you can see it's significantly larger than the ones we're talking about. And what you have to keep in mind is the size of the volcanic field at Crater Flat is tiny compare with other volcanic fields. It's really tiny, and that the magma production rate there is very small and it's a very important thing to determine. It's an aspect that has not received sufficient attention, but measuring the volume relationships with time tells you about the energy production.

Well, if we just look at this, we can see that there's a scattering of cones in this field, and we say that each cone represents an event, which is the assumption. There is a concentration of those near the center, and they're more widely spaced farther out, and for our simple model we chose a two-dimensional Gaussian distribution of vents for which we could specify an elongation direction with a deviation, an aspect ratio, and the number of vents that we would generate to generate--to simulate volcanos associated with different fields.

I'm going to skip down to the results we came up with, which, the idea of our model was to have an inclusive model. I said that this morning, it would be inclusive. Anybody who would like to use their particular theory in this model could do it, so that they could develop the probabilities associated with whatever sort of distribution they came up with. However, it would have to suffer the slings and arrows of peer review.

This is the distribution of--in a relatively--it's a very simple way, where the repository's shown in green, and the various volcanic areas shown here in blue, and if we took a Gaussian--if we assumed this to be a two-dimensional Gaussian distribution of vents, we would see that it was essentially north-south oriented, with an aspect ratio of about two. And in this respect, it's similar to almost all of the other volcanic fields in Nevada, and those in nearby areas in the western USA and northern Mexico, and we looked at all the ones for which we had data.

Now, what we could say, what does this mean? It probably means that the feeding system, the deep feeding system for these volcanos has this sort of an orientation, but as the magma comes up, the stress field rotates that so that the dikes near the surface are oriented to the northeast.

So taking this overlay, I'll show you some models. Assuming this is the most plausible--which can be debated--arrangement, I'll show you what we did, or what I did. That was to generate 5,000 events, called vents here, with an aspect ratio of two, and using a standard deviation in this case of 4,000 meters. And you see that the predicted events or vents more or less coincide with the ones that are presently visible, and then what I did was generate dikes, using the average length of dikes, measure from the literature, and I gave them the orientation to the northeast to correspond with drill hole data, and with stress release on faults in the area. And we can see that, in counting, there were no intersections, so that the probabilities are very low.

As a matter of fact, I could have done 10,000 or 100,000, and probably there would have been no intersections. That's the distribution using this particular model, but you can use any model you want, to generate them.

So then, the logical thing is to say, well, suppose the field was bigger? So I increased the size of the field--in this case, doubled it, so that the standard deviation is eight kilometers. We find then we have some intersections; two intersections for vents, and, in this case, 14 dikes cut through the repository, and so on. So I made another model and made it even larger. This one's twelve, and the number's gone up to 18 events in the repository and 38 dikes, and so on. You might say, "Wow!"

So if we get up to this big area outlined by Gene Smith as the area of recent volcanism, you say, "Wow! It's going to have a very large number," but here's a very interesting result. I ran a number of these and I found that up to some number, up to some distance, there was a linear increase in the frequency of intersections, but after reaching some point that maxes out, and it tails off. And the reason for that is, as the field gets larger and larger, of course, the events get farther and farther apart. So there's some maximum, and what this does is set a bound on the frequency. So regardless of the size of the field, there is an upper bound on the frequency. And we can see that this frequency here is; about five in a thousand of these centers would intersect, if we chose this sort of distribution.

Of course, we could choose other distributions, and I'd like to comment on other types of distributions and their significance shortly. If we look at the dike data, we can see the same sort of thing, that as the fuel grows, the number of dike intersections increases, but it maxes out at about--say a field is 30 kilometers long, and after the field increases beyond that, there is a tailing off of dike intersections, and that frequency is close to one in a hundred.

So if we took the probability--using this model--the probability that Bruce Crowe developed that was like one in a million or 10^{-5}, 10^{-6}, or that Professor Ho gave us that was halfway between those, that's what I used. So we multiply those two together and that gives the probability of an event in any one year. Multiply that by 10^{4}, and you get the probability in 10,000 years, and that's what we used in our diagram.

So getting the probability for affecting the hydrology, we just use a larger area, and these are just numbers for demonstration. They're not solid numbers. I'm having a little more faith in these numbers now, but you could determine what would be the effective distance from the repository where a dike intrusion would have some effect on the water table, and you can imagine--I just took one kilometer.

And the probability I took for hydro-magmatic release was that we'll use the same probability of hydro-magmatic release that we've seen in the deposits that are there, and it's about one eruption out of ten is hydro-magmatic, and that's a pretty conservative estimate, so we just multiply that by 100, and the difference is that a hydro-magmatic event will probably produce an explosion to the surface and a lot of extraneous material will be dispersed; whereas, a magmatic event will incorporate very little country rock into it, or country radioactive material and bring it upward. But our problem wasn't to make that sort of estimate.

Now, I haven't heard the ten-minute warning yet, so I'm going real strong here. I better slow down because I'm going to have some questions if I keep going at this rate.

So there are some very important questions that have to be resolved for modification and improvement of this particular model, and one of these questions is: What are the geometric and temporal rates that determine the distribution of volcanic events? And related to that: What are the geologic processes that control these rules?

Now, I think these are really significant questions. These are very important questions for this problem, which is one of the, in radioactive waste disposal, one of the huge problems that faces us in the next ten or twenty years, and also for volcanic risk, and this is a decade for reduction of hazards, including volcanic hazards. But the problem is, there's very little money out there to help researchers to work on these problems, and it's really too bad, because I think that they can be solved.

So what we have to think about in these spatial distributions is, what types of distributions? And the one I used was a Gaussian-type, or you could use an exponential or a power law or whatever you want of decrease in volcanism with space. I want to make an analogy here to a paper that just recently came out in geology regarding mineral deposits, and in mineral deposits, here are three types of distributions.

The first one, A, is a Poisson distribution; in other words, there is a random distribution of events. And B is a special type of distribution called a Neyman-Scott distribution. It's a two-phase cluster distribution. First of all, you choose a cluster location, and then you distribute your events about the center of that cluster, and you can see that in this sort of a distribution some areas are more highly probable and other areas are least highly probable. And the third one down here is a seven-level recursive Poisson distribution; in other words, you choose a cell, and then you go into that cell and you choose another one on a much smaller framework, and you keep choosing them on smaller frameworks, and you can see that this sort of clustering leaves wide spaces where there's very low probability, but there are other zones where there's a high probability. This begins to look like volcanic fields, and what I would like to say is probably the distribution is something like this, it's something like a fractal dust in space, and probably it's like a fractal dust in time, and there are some reasons for that.

I think what we have to do is understand the mechanism, and I think there's a power spectrum that we're looking at, and if the exponent of the power spectrum is -1, then this can be described as a self-organized, critically active regime, and there are systems that are like this. For example, volcanic tremors follow this pattern, and earthquakes in general follow this pattern, and if we understand that the fracture pattern, which controls mineral deposits in the western United States, is also of this general nature in space, probably volcanism follows this, too.

So I would suggest that future research--and it won't take a long time to do this--would be profitably directed towards understanding the spatial distribution of past volcanism in terms of some sort of moment measure; for example, the volume of material released in these periods of time, and relating it to these types of distributions.

DR. ALLEN: Mike, in No. D there, is that diagonal line the California-Nevada border?

MR. SHERIDAN: I really thought so. You know, I really thought it was, and I don't know if it is, but it could be the Sierra-Nevada, and those could be national parks. You know, this could be Sequoia National Park over here. I don't think this is--I think that the border is somewhere over here, but this could be the Sierra-Nevada. I'm not sure where that is but, you know, if you--don't worry about that. Think about this part.

DR. ALLEN: Well, mining laws in California and Nevada are different, so...

MR. SHERIDAN: Oh, yeah. Well, you've got to find the basic laws that control the distribution of these things.

DR. ALLEN: Basic laws of statistics.

MR. SHERIDAN: Let me put this one away.

Okay, let's look at the distribution. Some of these slides we've all seen many times, but if you just look at the distribution of the volcanos, and let's compare them with this Distribution C. That's very bad, forget it. What you can see is there are large spaces without volcanos, and then other spaces where volcanos are concentrated.

Now, this is at a very large scale. It's like maybe one-tenth of the basin range you're looking at here, but if we look at different scales, what is interesting is we find the same type of relationship. Here is that area of most recent volcanism or whatever you want to call it, and in this area we see there's a cluster of volcanos down here, and one or more volcanos here, and then some more volcanos over there. They seem to be clusters, but there are large areas without volcanos, and it's not a matter of that being covered with alluvium or whatever it is. It's really there are no volcanos in that area.

So now we're going from this larger scale down to this smaller scale, and I want to say just on perhaps the largest volcanic field on the North American plate is this Michoacan-Guanajuato Volcanic Field with more than a thousand volcanos, and in this field you can see there are clusters of these volcanos, and within the clusters they have a similar sort of composition, and so on. So they have something related in their genesis, but there are areas without volcanos, and these are all quaternary volcanos.

But you can see, also, this linear arrangements. The volcanos are controlled by near-surface phenomena. So I'm thinking that this could be studied in terms of fractal dust distribution, and then that sort of distribution could be applied to the Yucca Mountain area. This is a little bit larger than the Crater Flat area. This is 15--so that's probably about 45 kilometers in length. These are north of the Colorado River in Arizona, and these are fairly recent scoria cones, some of them younger than 10,000 years. They seem to occur along lines, but they're clustered and within that area there are areas where there's low probability, actually, within the volcanic field. There would be a low probability of a cone at some of those points.

And if we take it down to even this ridiculous level, I think we've seen this slide of Gene Smith's, of these little scoria deposits on Red Cone, we see that they are not randomly distributed. Those are clustered, and they seem to be--some of them seem to be--it's very easy to make lines. I should have gone back and showed you on that Poisson recursive distribution. You could put lineaments. You know, you get three or more cones, and you can start putting in lines.

But I think that these fracture orientations are probably realistic, and it's related to this near-surface stress field, but even within this area, the probability of cones is not uniform, so that uniform probability can't be used.

Now, the justification--I'm winding down, still haven't heard that ten-minute warning, so I'm really in deep trouble.

DR. ALLEN: You've still got ten minutes to the ten-minute warning.

MR. SHERIDAN: I've got ten minutes, okay.

The cumulative volume model, this is a model for volcanos. As I say, volcanos are like people. They're more like people than earthquakes are, because volcanos have a birth, and then they have a period of youth, they have a period of maturity, like I'm in right now--very active--they have an old age period where they start slowing down, and then they have a death. There is a period beyond which they are no longer active, and if we look at the cumulative volume model for almost any volcano where it's been studied, you see this sort of relationship, and what we can see from this model is if it's in this period of maturity in the center, that it has a more or less constant rate of production, and that constant rate of production is higher than the rate of production if it's young and growing, or if it's old and dying.

Models, then, to take a conservative point of view, would take into account the volume production rate, and it's very easy to do this, at least at order of magnitude levels, and to get the ages, also, a little bit better than order of magnitude, but to get the ages and build this sort of chart. And using this as the moment, probably, because for volcanos the energy is the thermal energy, and since the properties of most lavas are pretty much similar, it would be a matter of just calculating the volumes, and the volume is directly proportional to the amount of energy that was produced.

But what I want to point out is that this represents, for most volcanos, what comes to the surface, and you have to realize that there's a cut-off level. There's a lower limit that the volcanos in a given area--there's a very low probability that a volcano will be smaller than a certain size, and the reason for that is it takes a certain amount of magma to fill the conduit, to fill the dike. So there is a cut-off at the lower level, and we can say that small events are more frequent than large events. So that these events that produce dikes are more frequent than the events that produce lavas at the surface.

So it's possible to set some bounds. There's also an upper limit in size of volcanos, the probability is very low, and you could use this same sort of graph just to graph the frequency of the volume of material erupted to set some probability limits, and I think the point I'm making is that-- I didn't make it yet, but I will make it right now--is that the spatial distribution of volcanos depends on the scaling factor of size. The very big volcanos are going to be widely spaced. The very small volcanos are going to be close together.

So that if we're talking about a large number of events, of small events, they're going to be very close together and they're probably going to be very close to an existing volcano. The new volcanos generally don't start with a little pop and a little bit of scoria comes out, and that's it. They start with a big event, and then they decrease.

So I think all of these are very important issues to be considered because they are going to have a key bearing on the probability of radionuclide escape from evaluation of the risk of volcanic activity at the site.

That's my last slide. Thank you.

DR. ALLEN: Thank you, Mike.

Questions from the Board members or staff?

DR. MELSON: Mike, when you showed the growth of the volcano, saying it starts off slow, is that really so? Some volcanos seem to start growth very rapidly in the early phases, and then slow down. You showed them slow at the beginning and then slow in old age.

MR. SHERIDAN: What volcano are you talking about?

DR. MELSON: Oh, I don't know a volcano. What about Hawaii? What do we know about it?

MR. SHERIDAN: I think we know that it must have started with, like --, and small numbers of events. I think when I'm talking about a volcano, I'm not talking about a single cone, or a cone appearing in a volcanic field. I'm talking about sort of the growth of a much larger area; for example, a composite volcano.

DR. ALLEN: Mike, early in your talk--maybe I misunderstood you--you sort of implied that one of the difficulties here is we've got such a long time period, 10,000 years. Yet if you asked me to predict when and how often and where earthquakes will occur in California in the next year, I'm in real trouble. If you give me a 100,000 years, I think

I can do a pretty good job. In other words, the length of the time period gives you an advantage.

MR. SHERIDAN: Well, there's a big difference because you have so much data, so much seismic data to work with, and with volcanos, the data that we have is based on evidence of a short life period, and volcanos also undergo different styles of activity, so they have a sort of Markov--they're split personalities. They have a Markov probability of being in some explosive phase or some effusive phase or something like this, but--

DR. ALLEN: But wouldn't you feel more comfortable?

MR. SHERIDAN: I was just saying that this is what I've been involved in, so that this, for me, was a new task. But maybe it's easy, you know, if we assume this data is good. Historic data isn't all that great for volcanos anyway. Maybe field data's better than what someone said they saw.

DR. ALLEN: Other questions or comments from anyone?

(No audible response.)

DR. ALLEN: Okay. Let's proceed, then.

The next presentation will be by Bruce Crowe for the DOE, and you're on.

MR. CROWE: All right. I'm not going to try to cover all of the things in my view graph package, but I want to focus on, particularly, the--try to give you what I think are some bounds on the probability calculations, but first, let me tell you a little bit about what we've been doing since we last talked to you.

We did finish our study plan on the probability of magmatic disruption of the repository, and that's described in the study plan with this number, and one of the things I wanted to point out is in one of the options we listed in there is a consideration of perhaps using expert opinion in a final--trying to assemble the final judgments on the probability of distributions.

Our current plans right now are not to do any more revised calculations than we've already done numerous times, and everyone else has done for us, until we get a little bit more volume data and try to settle the chronology down a little bit, if such a thing is possible.

I wanted to also just mention to you that there is this issue of the possible presence of magma chambers, and there is a teleseismic tomography model, that John Evans in the USGS has, that suggested there might be some evidence that areas to the south of Yucca Mountain might have a magma chamber. We've been in correspondence with John and we've been talking to him, and the USGS is actively involved in doing this, and it's something that's going on as part of the study plan activity.

But what I really want to focus on--entirely, in fact--is the evaluation of uncertainty, because I think there is a widespread perception that the uncertainty is just astronomically unbounded, that virtually anything can happen, and what I'd like to argue, that individual values, again, are estimates, but if we turn to the geologic record, we can put some pretty firm bounds.

Okay. Let me make a real important point that I think came up in several of the other talks. When we talk about scenarios of future volcanic activity, when we do the probability calculations, we divide it into these two things. We have this, as you saw, a very active polycyclic debate, and then we have what I consider is the thing that we're trying to formulate, and it says: What is the probability of a new volcano forming? Because the new volcano is the one that could move around to a new spot and could have an effect on the repository.

The definition of a polycyclic event is that it occurs at the same vent, and so once an event forms, you're not--it doesn't have a probability of intersecting the repository. It just says it's going to erupt again at the same spot. That's a really important concept, because you should factor the polycyclic concepts into the consequence analysis, not into the recurrence rates. This is the definition that we're after with our probability calculations.

And this, just to really quickly show you again, this is the structure that we're working in, this conditional probability, and I emphasize E1 here. It's the rate of formation of new volcanic centers, and I also want to emphasize this 10^{-8} is what we look at as our potential bound of the definition of processes that no longer have to be considered if you can demonstrably fall below that value.

Okay. I also wanted to say a few things about what I call behavior roles when dealing with conditional probabilities that I hope, if you believe these, then we begin to have some hope of maybe answering this problem. Again, the individual values are estimates. If I give you a number like, say, 2×10^{-8}, and if you ask me, "Could that be 4?", and if it's four, then you don't know what you're doing. Then I'm in trouble, because we don't emphasize those individual values. What we emphasize is the bounds, and we think that you can bound those by basically looking at physical limits of how volcanic processes express themselves in the geologic environment.

The alternative models that we can develop for these, in my opinion, what we have to focus on is trying to look at: Do these models change the bounds? If my job is to look at every model that's proposed and try to discriminate in those models, or maybe my job is just to settle on a firm chronology, we basically are designing a retirement program here, because we're not going to be doing that. And so the test I try to use is to basically say: Do these differences in models change the probability bounds? If the answer is no, then I think we really don't have to necessarily fight over these things. What we have to focus on is what changed the bounds.

There's a second thing that I want to caution people against, and that's what I call propagation of worst case assumptions. Because a lot of the parameters that we put into these probability calculations are inter-correlated, you can't go over and change one, say I'm going to use this rate, and not look at the others. Often, if you change your area ratio, you have to look at what volcanos are in those areas. If you change your definition of the distribution of volcanos, your area ratio changes, and so you can't look at those in isolation and still do correct calculations. In fact, what often happens, if you don't have a reality check of the geologic record, you can end up with actually coming in with some calculations that are physically implausible.

And then, as we well heard this morning, there are some difficulties at gathering data for probability calculations. We really have a burden of proof requirement, and again, we try to look at it in the perspective of look at the bounds, don't fight over the details; otherwise, again, we'll never get done.

Now, one thing that I want to mention is I think if we bring this probabilistic perspective into this arena, that we can--I think we have a chance for an answer, and John Trapp and I, as you probably all know, have agreed to disagree over the calculations, but what I want to complement the NRC on is that by formulating their calculations in a probabilistic perspective, we can see what parameters we disagree over. We can go and look whether or not we like the--how we use the rate, or how we select the dimensions and things, and it gives us a common grounds for airing our disagreements, and if we keep that probabilistic perspective, I think we can solve this problem. But if we end up just kind of a battle of, "I don't like this number you used for your edge," and these sort of things, those are unresolvable, and so it's kind of a message leading to this issue consensus. I'd like to urge that if we do agree on this probabilistic perspective, I think we can manage this problem.

Now, I'm not going to talk about recurrence rate models. I was real pleased to see Mike's comments about cumulative volume versus time. I really have to agree with those comments. We have put together some cumulative time curves for a variety of fields, and we think that this volume predictable model has a lot of potential importance in here. I don't want to go into--I don't think I have time to go into all these different types of models, other than there are multiple types of models where we can do recurrence rates from simple Poissonian counting, to trying to look at time series, trying to look at volume predictability. This is one that we think might be kind of interesting and we hope we can talk to you more about it, but what I find is that the probability bounds are not sensitive to these recurrence rate models, and I think I can try to demonstrate that by looking at some bounds from major volcanic fields.

What we see is we look at the Lunar Crater volcanic field, which you've seen in the map is--here's the kind of way volcanism expresses itself, and what we did is quaternary vent counts, again, being careful to try to be liberal in what we call quaternary. That number could be 100, it could be 60. I don't want to quibble over that number, but what we see is we see 82 centers in 28 clusters, and there's about something greater than 60 km^3 of magma, and your vent density of the field is about .33.

If you look at the Cima volcanic field--and these are two of the bigger volcanic fields in the region--we saw about 29 quaternary centers in 22 clusters, greater than 20 km^3. Vent density's about .1.

What we see at Yucca Mountain is seven centers, three clusters. I think we can agree that there are seven centers, and the clusters are another issue, but as Mike pointed out, this is a very small field compared to dimensions of others. We've seen about a half a cubic kilometer of magma in the quaternary. We have a vent density that's quite low, and to put this in perspective, I looked through the literature, and here are about the largest vent densities that you get. Mauna Kea has a vent density of about .39, et cetera, and so these numbers up here are not that far out of kind of looking at extremes.

Well, let's look at those rates in terms of we just count those in a simple Poissonian model, we can begin to look at the thing. Let's say we take a rate 9number of vents per year, of 10^{-4}. Propagate that for two million years, and you'd be looking at a field that would have 200 events. We can look at Lunar Crater and say, well, we've got 82 there, so Lunar Crater has to be somewhat less active than that. Same way with Cima. Cima's got to fall somewhere--Cima actually falls somewhere close to 10^{-5}. What we would argue is just simple numbers--and I think we might even be getting toward a consensus on this--we've got to fall somewhere between 10^{-5} and 10^{-6}. I don't want to quibble about where it is. What I'd like to emphasize is that this is a pretty firm bound. Physically, we don't see volcanism in continental fields acting up more than that. If they did, we'd have more volcanic centers in these fields. And so, I'd argue that this is a pretty firm bound, that you're going to have a heck of a time changing those numbers unless you want to propose that anything can break loose and we've got a volcanic field that's been virtually unprecedented in the record of the southwestern

United States.

I'm sorry, (indicating chart), that's vents per square kilometer, thank you. I forgot to put that on there. So we see that 10^{-5}, 10^{-6} as being pretty firm bounds no matter how we do this.

Now, the structural controls issue is one that Michael really touched on. Well, we've got what we've always defined as this Catch-22. If we had more events, we'd probably have a better pattern to look at controls. But if we had more events, E1 increases, and we probably wouldn't be here talking about this today. So we're kind of stuck with the kinds of data we have. I think what we may have is perhaps some unconstrained models. I'd like to argue that Mike's point of view is a good one, bring your perspective of kind of the orderliness that you see in volcanic fields, and I think that brings a good perspective. We strongly agree that we think there is a northwesterly trend to these volcanic fields, and now what I want to look at is a little bit of what other fields look like.

I don't even think I want to look at--I can show you this real quick, but I think we've seen enough of this that you probably are tired of it already. I have a minor model that I want to talk about a little bit. Well, maybe I don't want to talk about it. I think I'll just skip that. I could get long-winded on that.

Let's go back to Lunar Crater and let's look at what Lunar Crater is. I did an exercise with one of the decision analysis groups, looking at trying to bound probability calculations, and they asked me, well, what could happen? What's kind of the worst case? And I said, well, probably Lunar Crater or Cima. It's probably a physical problem of going from Yucca Mountain-type or Crater Flat-types, we have the Lunar Crater in 10,000 years, because the scale of process, as we see, is on the order of about 100,000 years.

But let's just say--I mean, what he asked me to do is he said, "put the repository right in the middle of Lunar Crater and what kind of numbers would you come up with?" And so I did that, and the numbers surprised me a little bit. Here's what Lunar Crater looks like. What I did is, these are all the identified quaternary vents. This is based on Scott and Trask's work and our mapping that we've done up there, and there's K-Argon data from a variety of sources, that we were pretty liberal in assigning the vents, and you notice that there is a strongly structural controlled grain to that, and so there's a variety of ways of kind of looking at the distribution of centers in there.

I did a simple--what we did is took the latitude and longitude, and then did what's a simple weighted least squares just to look at, "is there linearity to this field?", and, of course, that's not a hard question to answer and the data clearly shows that. And so, then we fitted it with an ellipsoid, basically, at different confidence intervals. We chose the 50 per cent, because you can kind of do a test of normalcy of data. It should roughly divide the number of events in half, and what we see is that this does broadly fit that. And so, what I decided is I'd do a calculation to say how far away from the field we'd have to go to, say, be at the 99 per cent confidence that we'd be outside of an event occurring in that field. And this is what we calculated, using a package called SYSTAT.

What it shows you is, with the kinds of data that you have at Lunar Crater, you can go from here to here and drop two orders of magnitude in the probability of intersecting the repository, and that distance from here to here is about five kilometers. And so what it says is that there's kind of an--it's a fairly orderly process we have here. These things just don't shoot up everywhere like crazy. They have pretty discrete boundaries.

We decided, okay, let's try to look at one that's not quite as well structured or controlled, and it turns out that Cima is perfect for that. Here's the vents again. This is from John Dohrwend's publications, about 29 sites, some clustering, not as much clustering, but there clearly--you'd have a harder time picking a pattern out of that, and it's starting to approach a random model, and when we do the distance weighted least squares, it also says that. It basically says you're going to have a hard time fitting a linear model to that data, so we'll buy that.

We do the same sort of calculations here and say, okay, does it fit the 50 per cent? And, in fact, it divides it perfectly in half, so gives us some confidence in that. And then we draw the same circle and say, where do you have to be to be in the 99 per cent confidence that you're out of the --that you'd be out of the predicted, based on the variance in the XY position of these, and here's this. This circle, you'd have to be 11 kilometers from the middle of the field to get outside of that circle.

And so that's a perspective that I think we can bring from the geologic record when we look at this. It's not an unbounded problem. Both the rates have some controls, and these are not chaotic structural features. As Mike pointed out, there's some form in geometry and there's some physical boundaries on fields.

So then let's see--where am I leading to on this? What I'm going to do is make an argument that I think we're assuming some fairly conservative values. When we do a Poissonian model, as several of us have pointed out now, we're assuming a steady state system. We think there's pretty good evidence, as Frank presented, that things are waning, that this system is shutting down. And Frank presented the main bits of evidence. I think a key piece of evidence that Mike touched on as well is there is really a duration to how these continental basaltic volcanic fields look.

If you go out in the literature and you look at how long these kinds of fields are active, you see numbers in the range of three to about seven million years. And what I think is significant at Crater Flat is this thing didn't just start yesterday. We think it started about 3.7 million years ago, and we don't know when it stopped, and obviously, we're going to debate that forever. But what we see is that duration is approaching the lifetime of normal fields, and so I would argue that if this thing was going to start cranking up, it's really probably had a shot at doing that, and so we would argue that, again, assuming steady state is a pretty conservative assumption.

And then, second, on the disruption ratio, we would argue that it is a fairly conservative calculation. That if we look at past patterns, what we see is things do stay in this fairly narrow zone. It's--and I look at it as nature's taken a few shots at Yucca Mountain and missed, and it missed in that zone. It missed largely up at Buckboard Mesa. It really missed there, but the other zones, it stayed right in a narrow defined zone, so again, there's some patterns to these things.

And we also think that there--it's pretty rare for basalts to erupt into the range interiors, and so, again, we would argue that the random calculations that I used are pretty conservative, because we allowed the possibility of an eruption occurring to the repository.

So what does all this do? This is the reason I'm kind of rushing through this. I think this is a key view graph here, and there's a lot of information, so I want to go through this fairly slowly. It's my last one.

Here's what this says: What I've done is: where I've done quaternary vent counts, assuming a Poissonian model; I'm looking at the annual probability of disruption. This is the recurrence rate and the likelihood of hitting the repository. And let's take a look at where Lunar sits. If we look out here, I used two methods. I used the vents and I used the cluster, and so in Ho's talk, I basically decided, let's just look at each range and get a feeling of the sensitivity of those.

Here's the kind of numbers you'd see. Here's what I calculated to pull your two orders of magnitude over here. It would take you 5 kilometers difference, distance from the main part of the field, and here's the key thing: At Lunar Crater, moving 5 kilometers out, we could make an argument that we would meet this 10^{-8} target that we're talking about. Cima, since it's less active and there's less clustering, there's not as much range here, but again, just moving out 12 kilometers puts you crossing this middle ground.

And here's the calculation that I did, the new one that I did for here. I said, okay, let's take this little narrow zone that we defined in this Crater Flat volcanic zone we called it, this northwest trending zone. I said, let's stick the repository right in the middle of that. Here's the values that you would see, ratioing the area of the repository to the area of that zone. In fact, I used a slightly conservative area for this. It might be a little bit less than this is. And then, here's where Yucca Mountain sits over here, based on my random model, and the random model, the numbers I come up with, that I published with Dick Beckman and Mark Johnson way back in 1982, was in the 10^{-3}, 10^{-4} range, and that's--I took a midpoint of that and pulled it over there. So you really are stuck in--

DR. ALLEN: But then that's based on your northwest trending zone, though.

MR. CROWE: No. Actually, the random model incorporates just distributions of volcanos. It includes the repository. It's the same thing that we used in 1982. I didn't assume distance from this zone. I basically just did an area ratio of a random model.

DR. ALLEN: I see. So it's different from the other two.

MR. CROWE: Yes, it is. Yes, it is. Exactly. That's a good point and it's important that you see that. I also decided to look, you know, is there someplace else we can get a perspective on just how busy can volcanism be, and I chose the Snake River plans, where you have episodic formation of rifts, and here's the kind of events that you--ratios you get up at--if we put this, the repository right in the middle of the most active part of the Snake River plans, which has been the Great Rift, which has erupted about 12 times in the last 15,000 years, it has a lower vent density. That's why it's not too far off of Lunar, because it tends to be tholeiitic and they space themselves out more, and basically very much like what Mike said. They tend to have more volume, but more space in between centers. Here's the kind of numbers you see. I didn't bother trying to do the calculations.

But the bottom line here, in my opinion, is this: You have two choices, really, of trying to show that Yucca Mountain would not meet this 10^{-8}. You can say, let's move it over in the middle of a volcanic zone, and yes, you would come up with numbers like this. Your other choice is, change the rates, and if you change the rates, you've got to physically bound it by that table I showed you. You really can't go, logically, much beyond 10^{-5} and 10^{-5} will not pull you over from any expected value by where we do all of our calculations.

So basically, there's a lot of sensitivity of chronology of the structural controls models, but if you start to bound the end effects, you end up with numbers that fall dramatically away from 10^{-8}, and I kind of wanted to echo the comments that Dave Dobson made at the start. We've played with these numbers since 1982. We've looked at them kind of upside down and backwards, every which way we could. We are not getting them to change, and I would basically put out the challenge that I'd like to see anybody show me that they can change without violating basic physical processes of how volcanos behave, and that's my pitch.

DR. ALLEN: Okay, thank you.

Questions from the Board? Leon?

DR. REITER: I think you went over pretty quickly some important conclusions. Could you just reiterate very quickly the assumptions on the top, the Yucca Mountain? What were the two assumptions that you made there?

MR. CROWE: Okay. The Yucca Mountain rates that I'm getting are based on what we said in 1982. Well, what I said, I basically bounded it between 10^{-5} and 10^{-6}. For this calculation, I took the midpoint of that range, so 1×10^{-6}. I'm sorry, no. It would be 5--in fact, it would be the same rate that you used, 5×10^{-6}.

For this calculation here, I said, let's put Yucca Mountain Repository in this zone, and so that ratio is the area--which turned out to be about 6 kilometers--versus the area of that zone, and the zone's about 500 km^2. For the random model, what I did is I basically took our distributions where we tried to bound area ratios by--we looked at every combination of distribution of vents, and the occurrence of where Yucca Mountain is, and we fitted minimum area circles and minimum area ellipses to those distributions. We took models of just quaternary cones. We included Buckboard Mesa, we dropped Buckboard Mesa. We went as large as all the way up to Lunar Crater, just innumerable combinations, and what we found is those numbers were bounded in the 10^{-3}, 10^{-4} range. I again took a midpoint for that value, which was--I just said 5 x 10^{-4}.

DR. REITER: What I'm trying to get at is supposing you assumed the zone that Gene assumes, what kind of probabilities would you get out of that?

MR. CROWE: Gene's number, if you assume his worst case--and here I would argue that I don't think it's physically plausible, but let's take it anyway. It would push him to the 10^{-2} range, and so you would drop down, let's see, you would drop down about into this area here for your disruption ratio.

DR. REITER: 10^{-2} times--

MR. CROWE: 10^{-2} is his area--it's the area of the repository to the area of his disruption zone.

DR. REITER: So 10^{-2} and then this 10^{-5}, 10^{-6}, wouldn't that make it less than 10^{-6}?

MR. CROWE: Well, what it is, here's your 10^{-8}. If you take a rate of 10^{-5}, you're getting--and add that, you're going to be in the seven times--I'm sorry, it would be in the 10^{-7} range for the lowest. For 10^{-6}, you'd then be at 10^{-8}. So that is the one sensitive area that you could play with there.

Frankly, I mean, actually, I'm with Gene all the way up until this last corridor, and basically--and I think it's the same point that, Bill, you were making. When he does his first two corridors, he's looking at the dispersion that these centers show in the modern stress field, when you get a clustering of an event that occurs. When he has his longer corridor, he's basing it on distributions of centers of multiple ages, and what we don't see out there is that amount of dispersion for an individual event. It's a key point, and --but I have no arguments with Gene at all on his first two zones there. I mean, in fact, we could take those same calculations and we would not be in disagreement.

DR. ALLEN: I was going to say, Carl, I saw you shaking your head at one point during the talk here. Is there something you wanted to say?

MR. JOHNSON: I don't think it was anything significant.

DR. ALLEN: Well, he was talking about his northwest trending zone, and I had a feeling that you didn't buy a northwest trending zone at all, but...

MR. JOHNSON: Well, I think that's the point, and that was the point of Gene's remarks this morning, is that if you look at the geologic structure, it doesn't support the northwest trending assumption. It supports a northeast trending assumption.

MR. CROWE: No, I--go ahead, Bill. I'll let you handle it.

DR. MELSON: The northwest isn't an assumption. That's how the vents are distributed. That's how they are ranged right now. That's how they go.

MR. JOHNSON: But if the purpose is trying to identify where the future events could occur is, you need to be looking at what the geologic structure is telling you, and the geologic structure is telling you you need to be looking along northeast trending systems.

DR. MELSON: I think any kind of model in predicting the future, I think, as we've talked about, you have to look at where things are now and how they've grown in that context, and what that says is it's northwest, not out way up to the northeast. That's not the way the field has grown. It hasn't grown that way. Volcanos are not oriented that way. They're disbursed along the northeast axis very short distances, not great, long distances, as they are in some other volcanic and organic fields.

DR. ALLEN: Do you want to respond to that?

DR. SMITH: I guess the point is here I agree that what you see here is a northwest trend. Volcanic centers appear to be oriented to a northeast trending--a northwest trending zone, from Sleeping Butte down to Lathrop Wells. If you plot the volcanos, you know, that's what you see. I think everybody today has mentioned--Mike has mentioned this, I've mentioned this, and Bruce has mentioned this--that there is a hierarchy in terms of risk; that the first priority, in reality, is that the next eruption, if it's going to occur, is probably going to focus in on an existing cone, and then it may, in fact, you know, miss. It may be--it may deviate a little bit. It may not exactly hit the target.

Now, what's important here, then, if it's going to--and we're talking about very small cones, we're talking about very small volumes, so we're talking about small cones that are going to cluster. The thing is, what's going to control these clusters? And this is the basis for my risk zones.

What's going to happen is that the local structure, the structure that we see mapped on Yucca Mountain, the structure that we see at Pahute Mesa, the northeast trends are going to be the important structures controlling the positions of these volcanos within these clusters. So my risk zones are based on the fact that we're going to focus in on an existing cone, or we're going to have a near miss, and if there's a near miss, the volcano is going to locate itself along one of these upper crustal faults or structures, and it would be along a northeast trending structure.

And I think, you know, I think it's simply a problem of scale. You know, you're talking about a very large scale. Mike is talking about a very large scale. I'm talking about what's going to happen right at the repository, right at Lathrop Wells, right at Crater Flat, and I think the northeast trending structures are the important structures to look at in terms of hazard to Yucca Mountain.

MR. DOBSON: Yeah. Gene may want to stay there for a minute in case you want to comment, I'm not sure. But I just wanted to make a point that addressed Leon's original question, which was, you know, what do the total numbers end up looking like, and if you--Bruce's example where you showed the values for the Crater Flat volcanic zone basically assumed that you moved the repository inside his northwest trending thing.

The point that I was going to make was that if you put Yucca Mountain back where it belongs, and then expand the risk zone to what Gene has used, the area of most recent volcanism, the expanded area that you get by including Busted Butte ends up giving you, assuming a random distribution of hazards now, a number that's very similar to Bruce's number there in the red zone. The question is whether the number can get smaller than that, because of what Gene summarized, whether essentially, what's the probability that Lathrop Wells is the first of a new cluster that's going to be long enough to reach Yucca Mountain. That's essentially the question that we're dealing with, and I guess I--I think we would argue that that proposition or that assumption has a probability of something less than one, and the question is, how much less than one? Is it an order of magnitude or two orders of magnitude, or what?

MR. CROWE: It is a good thing to focus on because it illustrates, in my opinion, what will be a standard grounds of debate: Do you use the geologic record, or do you use an "anything could happen" sort of approach? And so, I mean, Gene's right, there are structures there, and certainly we know that basalts like to follow structures, but we have--nature has taken its shots, as I mentioned, and it likes this zone. There's clearly something about that zone. I mean, we speculated that it might be some sort of an old element of the Walker Lane there since the strike-slip faults might tend to be a bit more penetrating and might be better guiding paths. But we have to say that this thing has taken its shots and it hasn't shown any inclination to head toward Yucca Mountain, other than small dispersion, as Bill pointed out, which is this direction, amount of dispersion along this northeast trending, which is perpendicular to the extension direction, which is very common to what you see in volcanic fields.

So, I mean, the debate comes down to is that if you can draw a zone that says--just circle the last volcano in

Yucca Mountain, you can get a number that gets down to 10^{-2}, but you have to ask yourself, is that geologically reasonable, and is there any indication in the record that that will happen, and clearly, there could be a structure there. I mean, this is a well-faulted area. We can find lots of structure, but the point is, we've seen no signs that basalts want to do that, and that may become the point of the debate.

MR. JOHNSON: Bruce, let me respond to that. I agree that it is a well-faulted area, but I'd like to know what your evidence is that there is some geologic structure involved there.

MR. CROWE: Well, I would cite your own state worker, who proposed a buried strike-slip fault, Bill Schweikert.

MR. JOHNSON: That's the Schweikert model?

MR. CROWE: Right. I mean, he's pointed out this linear area and suggested that it could be controlled by a concealed strike-slip fault. I don't think we know, and I'm--I've always been a little uneasy about running a circle all the way up to Sleeping Butte. I mean, there's numerous ways that we can divide these combinations, but I have to echo what Mike said and what Bill said. When we see the patterns, when we go down to the individual vent center, Gene and I agree, we see a northwest and a northeast conjugate sets.

When we go further and look at the fields, they expand in the northwest, but not that much in the northeast. I mean, and so there is this scaling regularity, or what Herb Shaw calls self-similarity in these things. I think that's nature telling us this is how it wants to behave, and so, again, the debate becomes, do we allow how nature's behaved guide us, or do we take a "what-if" or "anything could happen" approach? And I think prudent decisions would lead us to let's use nature.

MR. SHERIDAN: Can I make a comment here on the EPRI perspective on this? And that is that the EPRI model, or a model that evolves from the EPRI model, would be able to accept these two individual models as branches on volcanology, given the probabilities that could be reasonably assigned to them and agreed upon by a panel of experts. And I think that this is exactly the way that such issues should be resolved, because I think that getting two scientists to agree exactly on every detail of the other one's model is going to be very difficult.

DR. ALLEN: Well, but you also have to recognize that merely picking a panel of experts and having them vote, doesn't necessarily mean you've solved the problem.

MR. SHERIDAN: No, but that's the best conclusion you can reach, given...

DR. ALLEN: After you've been through a very thorough investigation.

MR. SHERIDAN: Yeah; right.

MR. CROWE: In fact, what I would encourage you to do is, in the study plan I basically bring this up, and what I--my recommendation I made for the study plan is that basically we assemble models in a catalog approach, and the only thing I would ask is, let's make them physically plausible. Let's not appeal to things that would violate kind of how volcanos behave. But let's assemble the models, and then let's produce a probability of distribution based on assembling those models. At that point, what I have suggested we do is we invoke expert opinion not to choose which is the single preferred model, but rather weight all the models according to geologic credibility, and that's basically the strategy I presented in my study plan for trying to resolve the issue.

DR. ALLEN: Leon?

DR. REITER: Gene, have you ever done some calculations--I know you've looked at your zones and given them--rated them various levels; one, two, three, and four. Have you done some calculations to what the likelihood of hitting the repository would be using those zones?

DR. SMITH: No, not yet.

DR. REITER: Do you have any feeling as to what that--how that would come out? What I'm trying to look for is what kind of difference is it going to make?

DR. SMITH: I really don't have any--up to now, the zones are purely qualitative. This is one of the things that I think--hopefully, we will look at it in the future. Dr. Ho, I think, that's one of the things that he has planned to do. So right now, I really can't answer that question.

DR. ALLEN: Gene, while you're up there, let me ask another--maybe irrelevant--question.

You emphasize the importance at the present time of northeast trending, deep-seated structures, structures going down to some depth. In your opinion, does this sort of rule out the shallow detachments as being possible seismogenic sources in the area? I realize this is sort of irrelevant to this meeting.

DR. SMITH: In the model that I proposed this morning, I didn't say that the northeast structures are the ones that penetrate deep. I said that the northeast structures are the ones that are now acting as the channel ways in the upper crust for the magma. I mentioned that magma could rise along structures that have a different orientation, and then in the upper crust, they could rise along features that have a different orientation from the master structure.

I'm not sure if, in terms of the shallow detachments, if in the model that I showed this morning, I'm using a model of detachment that was proposed by Roger Buck, where the attachments are, in reality, high-angle structures. So the seismogenic portion of the detachment would be the high-angle fault. The low angle segment is inactive, so there'd be no seismic activity related to the low angle segment of the detachment fault.

DR. ALLEN: Okay. I realize that's not the primary subject of this meeting.

Are there any other questions or comments from anybody? Yeah, Dave?

MR. DOBSON: Clarence, just one more comment, and that was that--maybe it's kind of a repeat of something I said earlier, which is that that 10^{-8} line is not any kind of a criteria, suitability criteria or failure criteria. The 10^{-8} line is a line that the EPA suggested was a line below which you didn't have to put it in the CCDF, and I guess I'm going to--I'll take some slight issue with Bruce, in that he made a statement to the effect that we didn't have to consider it. It doesn't mean we don't have to consider it. It may mean that it doesn't go in the CCDF.

But the fact is, if the value is 5 x 10^{-7} or 5 x 10^{-8}, excuse me, or even 5 x 10^{-7}, but I'm not sure, that what's important from the overall perspective is the probability that you're going to get releases that affect public health and safety, and we haven't even started talking about releases yet. We're just talking about the probabilities here of getting a dike into proximity to a repository, and so--

DR. ALLEN: Well, through the repository.

MR. DOBSON: Pardon me?

DR. ALLEN: Not just in proximity, I think through.

MR. DOBSON: Yeah. Well, we haven't yet, but we haven't added in, you know, any kind of a factor that entrains waste and moves it to the surface or anything. No, we haven't done the consequence analysis. I guess Greg is going to talk a little bit about that this afternoon, but that's--there is yet another step in this analysis.

DR. ALLEN: Okay. Thank you, Bruce. We're exactly on schedule.

John Trapp from the Nuclear Regulatory Commission is next.

MR. TRAPP: Actually, after what you've heard today and following one of Dave's comments this morning about newspaper articles, this may be a minority opinion report. I believe that was a quote from Clarence, or at least attributed to Clarence.

What I'm going to try to do today is take a look at this issue of volcanism from a regulatory perspective. We've had a lot of scientific discussions today, but Bruce, I'm glad to see, keeps on trying to at least bring the point out that what we're dealing with is a question of, can we license this repository? It's very nice to sit and have these good scientific discussions, to sit and try to obtain scientific consensus, but if we don't license the repository, this is all a waste, at least for the project that we are trying to accomplish.

And it also was mentioned that, gee, some of these things are different than putting together a scientific study, the "proof" is slightly different. You better believe the proof's different. The burden of proof on this is what the DOE, on being able to prove with a reasonable assurance that this site is safe. There is no requirement on anybody to come up and prove that something is wrong. This is a totally different type of level of proof than some of the people seem to be dealing with.

However, when you're going through this burden of proof, et cetera, there are three main things we've got to take a look at: What can affect the repository; how likely it is; what's the consequences? You've got to answer those no matter what type of standard you've got. We may change the EPA standard, or EPA may change the standard, but you're still going to be looking at these type of questions.

And if we start taking a look at the first one, what can affect the repository, where there has been a series of probabilities brought out today, I submit that the probabilities basically are not looking at the whole problem. The probabilities we've been talking about have really only been dealing with this first part of the issue, direct release. We really haven't gotten into the probabilities associated with all these other type of things that have got to be considered in the overall phenomena of volcanism and how they can affect the repository.

So with that in mind, what I'm going to go through in the next three slides is basically take a look at a series of things that you can see out there, you can discuss out there, and start asking some questions. I think one thing that Bruce and I do agree with totally, is that if we're going to answer this question, that you've got to base it on the geologic data, and I guess my point is we really don't have enough data right now to make this assumption.

We can start with the cones. If we didn't have those cones sitting out there and weren't staring at them right in our faces, the question probably would not be raised. But as far as I know, the repository is planned not to be sitting on the surface, but someplace buried in the subsurface, so what we've really got to talk about on the cones is not only the cones themselves and doing some point common and this type of thing, but we've got to start taking a look at the plumbing system that's feeding these cones. And if we're taking a look at this and taking some assumptions, et cetera, what you find is most people will make some type of assessment that this is some type of dike system that's controlling it.

Okay, let's take that for somewhat of a given. So what do we know about dikes? Well, right now, there isn't a whole heck of a lot of information as far as studies in the Yucca Mountain region on dikes, but there are a few points that need to be brought out. Number one, there are some dikes in the area of Yucca Mountain, right along the Solitario Canyon. I believe these are old ones. I think they're about 10 million years, aren't they, Bruce? But they're also, if you take a look at the boring logs, there are several examples where you're going through the boring logs and you start intersecting basalts in the area of Yucca Mountain.

The point here is we've got these dike systems sitting some at the surface, some in the subsurface. We really don't have a good distribution because we don't have the information yet as to where these are. But sometime in the geologic past, the conditions were ripe for dike formation. Are they still?

Let's carry that a step farther. In this area, we've got a whole series of these calderas, and I'd like to discuss them for three points. Number one, if you take a look at these--the whole history of volcanism in the area of Yucca Mountain, starting about 13-14 million years ago, you started with some very highly silicic volcanism. Roughly, I believe, at 10 million years you came in with your first basalts, and as you carry this thing through, you start picking up more and more basalts until our present regime.

There's an awful lot of analogs that are used which basically are trying to make an analog with the stuff that's going on at the Hawaiian Islands, and the real question is, are these analogs valid? How much of the analog is valid? Which part of a--what type of a volcanic cycle do we have to look at?

Another point. If you take a look at, for instance, Timber Mountain, or where the Tram or Claim Canyon calderas are, or these other ones, these weren't benign-type features. There was some extreme energy put into these systems, and there was quite a bit of structural disruption through the area. I have yet to see one cross-section that goes from Yucca Mountain north and shows what the structural relationship is between these things and Yucca Mountain. Matter of fact, I haven't seen it on the northwest trending ones. So what is the deep structural relationship that we've got here? There's a bunch of structures that haven't been accounted for.

A third point, which may get into this thing, is if you take a look at where these different calderas, you see that they're right at the north of the site. They're right at the west of the site, and you also note that north and west of the site there's a very strong groundwater gradient, groundwater anomaly. Is this a coincidence? I don't know if it is or not, but it's a very strong coincidence if it is.

There's been a lot of discussion on faults already, and different structures in the area. We've got the north trending faults, or basically--strongly north, northeast, northwest, and we really have got very little information on these. Plus, we also have this assumption that we've got these detachment faults that people are able to draw and run all the way across the test site, take them on over into California.

If we're running these things through, somehow we've got to tie them in to this vertical plumbing system, and I haven't seen anybody show me a real good way that fits in yet, and we also got to tie these in somewhere to these other features.

I'm not going to discuss age determination. I think my point on the question of age determination and where we sit on that has been adequately discussed this morning.

So we've got these features, some that we can see at the surface. Are there other features we're concerned about? Well, there's a bunch of indirect evidence that gives us a lot of questions that we really want answered. It has already been mentioned that there are aeromagnetic anomalies around the area. You take a look at these aeromagnetic anomalies, and they suggest there's a whole bunch more buried basalt bodies in the area than the ones that are simply at the surface.

If you take a look at the study plan that Bruce has mentioned, that came in, there are studies to start investigating these, but they haven't been done yet. If you talk, or listen to what Mike Sheridan was talking about, there was a discussion about how many of these actually get to the surface and how do you actually factor these into your overall probability calculations? So I'm saying there's a bunch of information that's suggesting there's a lot more to the basaltic story than has been brought to the table yet.

Teleseismic. This was also brought up slightly. There is this very large teleseismic anomaly in the site area. Is this some type of magma chamber? Is this--well, I don't really know what the thing is, and I haven't gotten anybody who really can tell me what it is, but what is its relationship?

Heat flow. We're sitting right at the edge of what's called the Eureka Low, and there are also, if you take a look at some of the stuff that SASS has got, there appears to be some type of anomaly centered right about approximately G2 rather than Yucca Mountain. The question is, are we seeing something here that is related to tectonics, or are we seeing something here that's related to groundwater flow? I personally tend to believe that this is more of a groundwater flow phenomena that we've got right here, but it's telling us something about this site which hasn't been really taken a look at.

What about seismic reflection/refraction? Well, we've only really got one line in the area, and that's the test line down in the Amargosa Desert, and I think this line raised more questions than it answered. The specific one, question I'm going to bring up, is the fact that right in the center of this line there was a nice big bright spot, and this bright spot appeared just about looking exactly the same as the one that's under Death Valley. Is this a magma chamber? What is it? Nobody has basically come up with a story yet.

How about the leveling data? There's another piece of evidence that makes you kind of wonder what's going on in that area. There has been a series of lines run through the general area, and then resurveyed. If you take a look at this leveling data, what you've got--at least in one good spot right over in Beatty--is a very strong vertical change. There's no seismicity associated, but there is a strong vertical change. This would seem to me to be telling us something about the tectonics of the area. Right now, I have no idea what it is. But somebody is really going to have to take a look at that information.

Let's carry it a step farther. What are the relationship of these structures to volcanism? Well, first off, we're not even sure what structures we've got. We don't have enough data, but we carry it a step farther, and how do you tie these all together? Carry it a step farther, what about the crustal magma involvement? What type of magma chambers do we need? How do we bring this stuff from deep below on up to the surface? And we've got this whole stuff on volcanic cycles. Are we at a waning cycle? Are we in the middle of a cycle? There's also information, at least in the western United States, that would suggest that we have gone, in the western United States, into a period of greater volcanic activity, which would suggest that if you take a straight projection, that all the numbers that we've got so far really are low.

I threw these two up because these are two areas that are just basically starting to get going. They were brought out a little bit by Mike Sheridan, but they are basically--I'm not sure they're totally new concepts. They may be old concepts wrapped in a new guise, but the concept of self-organized criticality has to be looked at, and I've got down power laws simply because I personally hate the term "fractals". I've never understood what a fractal was, but I understand what power laws are.

And if you take a look at this, for instance, is one, the relationship of volcanic centers to size of eruption that has been proposed in the literature. Is this a good one? At the present time, I don't know. And then the final question of analogs. Which analogs should we use, and how should we use them?

Now, if we're sitting or pretending that we're going ahead and presenting this material to a licensing board, the licensing board is going to be swamped with information. They're going to have some information which is basically stuff that you can lay your hands on and measure, et cetera. They've got a bunch of information that's going to be coming from indirect measurements, and they've got a bunch of assumptions, theories, et cetera, that are based on this data that's been brought together. And somehow, they're going to have to try to organize this information, and this is my first attempt at trying to organize this the way it would have to be looked at by the licensing board.

If you start on this side, there's data that's measured. You can go out and put your hands on it. On over to the other side where you've got inferred data, where you're talking about experts' opinion, you're talking about the decision of methodology. If you want to carry it far enough, basically, you're talking theology.

There's certain information we can measure, but if you take a look, for instance, at the way we've got right now, there are a lot of questions that come up. We've got this age date for features, and I notice I put down here the actual age of the features. Basaltic cones, we can measure part of this, but the whole thing we can't. Surface structures; deep crustal structures.

The point, as you go through this thing, notice that the place that we've got to make the decision, the probabilities and consequences, are based totally at inference on everything that feeds into it, and right now, what I see us doing is try to go through this thing and start from here, and get to here without going through this whole sequence. These have all got to be run through.

Now, this is a section on probability, and here's one that we're throwing up kind of for discussion because it brings out a couple points.

DR. ALLEN: John, you've got ten minutes; ten minutes remaining.

MR. TRAPP: What probability numbers right now, with the data base we've got, does the NRC think they can support? Well, we came out here, and what's the probability of volcanism occurring someplace in the Yucca Mountain region sometime in the future, say in the next 100,000 years, 200,000 years, et cetera? We couldn't really come to a good idea what the region was, but we came that--it's high. I would have said that it was a lead-pipe cinch myself. People said that was a little bit exaggerated.

The point here, though, is there is the mistaken assumption that the EPA standard and the Part 60 go out 10,000 years and then everything stops, you forget about it, and that is not the regulation that we're dealing with. The 10,000 years was selected as a surrogate to basically describe the future. So we have got to be concerned about more than just the next 10,000 years. I don't think anyone would want a site that works beautifully for 10,000 years, and at 10,001 it all went to pieces.

The probability of volcanism occurring in the Yucca Mountain region in the next 10,000 years, well, it's obviously less than any time in the future. What's the probability of affecting a repository at Yucca Mountain? Well, until we get some site data, get site characterization going, we really don't know, but there's enough things happening that we've got a lot of concerns with it. The probability of volcanism occurring at Yucca Mountain? Now, if somebody could come with data and say it was zero, we could throw this whole thing away. But I haven't gotten anybody to--in any of the discussions, to say there is no possibility of this happening. So, somehow, we've got to narrow this down tremendously.

What are our basic conclusions? Yeah, we think it's a significant concern. If we get to licensing, you've got to have a reasonable conservative data base. At the present time, this data base is limited and, in many cases, it's basically non-existent. We've looked at a lot of the stuff that DOE is proposing, particularly in the SCP, and they appear to have a program which is aimed at looking at all these general concerns, but that's--in the SCP we're really talking an overview of the whole thing.

We've mentioned the number of 20, 22. There are something like 22 study plans which appear to feed into answering the volcanism concern. So far, we've seen one of them. Most of these programs haven't been implemented. So basically, what we're saying at present, we don't have a number that could even come close to answering the question.

What do we need? Well, I guess what we need and what we're aiming for is the same thing we were talking about in the SCP. We need some site characterization data. We need some information. Until this is brought in, this is a totally open issue, and I think it's going to be an issue that's of tremendous concern all the way through the program.

That's basically it.

DR. ALLEN: Thank you, John.

Are there questions or comments from Board members or staff? Yeah, Bill?

DR. MELSON: John, just on a minor point, you mentioned that the frequency of eruptions is increasing, or there is some evidence for that in the southwest? What is that evidence?

MR. TRAPP: What I'm basically talking about there, there are a couple reports which are suggesting--not in the southwest, but in the American west--that there is an increase in volcanic activity from the start of quaternary to now, and

I'm just saying that this is one thing that when we're taking a look at these probabilities, these type of statements on the cycle, et cetera, have to be considered when we're trying to determine what the real answer is.

DR. BARNARD: John, as you pointed out, there's certainly a lot of existing uncertainties, and I think that's the reason why the Department of Energy wants to get on site to try to resolve some of those uncertainties. Do you think these uncertainties will be resolved to your satisfaction, your personal satisfaction?

MR. TRAPP: I'd like to kind of beg off on that one, if possible. There's been a lot of flak about this, which actually is directed at something totally wrong. My concerns are based on our ability to get enough information to license this site. That is different than starting to talk about these scientific problems.

When Bruce and I sit and start discussing science--which is very few times--but when we actually do, we don't have that many differences. Where we have differences is basically, yes, can we get to a license or not? But I just got done last week on a--not last week, week before--on one on systems engineering, and there were a couple slides in there that were rather interesting. I think one of them kind of sums everything else up, which says, without data, you're just another damn fool with an opinion, and I think that's where we sit right now.

DR. ALLEN: John, you list any number of important geological and geophysical studies relating to the Yucca Mountain region, with sort of the implication that we have to understand everything about everything before the site is licensable. It seems to me that it's incumbent on us to try to identify those issues that are important and relevant. I can't necessarily be convinced that a bright spot under Death Valley, or a surveying tear at Beatty necessarily has any relevance to the suitability of the site, and yet your implications seem to be that we have to understand all of these things.

MR. TRAPP: It's not a question of understanding, to total degree, all of it, but it is a question of having sufficient data that you can make a good, logical choice. There is no geophysical seismic reflection data, for instance, in the area of Yucca Mountain, but there is a line just south of Yucca Mountain--the one I talked about--which shows the bright spot. This bright spot is centered in the vicinity of the town of Lathrop Wells.

So I'm not talking Death Valley. I'm talking about a feature that's in the region of Yucca Mountain, which appears similar to another feature. I'm talking about the teleseismic anomaly which is suggesting there may be something down there that could be interpreted as a magmatic body.

If you're asking what I think we need to do, I think we need, first off, a tremendous amount of geophysics. I think that's really going to be the key in not only this issue, this part of the issue on volcanism, but in the whole structural faulting area, and we don't have it yet. I am not sure how it's going to come out because there have been some problems in running different geophysical techniques in this area. I'm hoping that they can be resolved, but we just don't have the information so far.

DR. ALLEN: Well, I certainly don't disagree with you that we need to know a lot more, but I think the first question we have to ask is, how is what we're going to find relevant to the suitability of the site? And a lot of things we've done, in my opinion, haven't necessarily been terribly relevant to answering that question, and I hope in the future we can ask that question before we spend a lot of money doing things just because someone wants to know. And many of these are questions we're not going to answer, beyond the state of modern science. Intriguing questions, but we have to face up right now to the fact that some of these are questions that in ten years or 50 years we're not going to have a complete answer to.

MR. TRAPP: And at that time, the licensing board is going to have to make a decision if this is of significance concern, that, therefore, you cannot license the site.

DR. ALLEN: If it's of sufficient concern, yes. Don't forget, this Board goes out of existence after 10,000 years.

DR. BARNARD: Well, not necessarily. One year after we open a repository.

DR. ALLEN: Are there other comments or questions? Yes, Mike?

MR. SHERIDAN: I have a comment. John, on one of your slides, you--that says, how can volcanic activity affect the release of radionuclides? You said that this, for example, modification of natural systems, groundwater level, thermal environment, hydrothermal environment, geochemical environment, modification of engineered barrier systems, you said that that hasn't really been done, but this was the whole thrust of the EPRI methodology, was to develop a means for evaluating the probabilities of the effect of volcanic systems on these. Not that we came up with a solution, but that using data that's available, numbers can be generated and the uncertainty associated with those numbers can be tested.

Have you read the EPRI report?

MR. TRAPP: You're right, I've read the EPRI report.

MR. SHERIDAN: And do you feel that that is a satisfactory method, or--

MR. TRAPP: What I see there are a bunch--what you've got right now are a bunch of scoping calculations, but first off, DOE themselves has done very little along this line. If DOE is going to assign EPRI as the person who's going to take care of this, then maybe we do have a start on it, but I wasn't aware that that was the situation we've got.

MR. SHERIDAN: You're correct.

DR. ALLEN: Okay, one final comment or question. Dave?

MR. DOBSON: Just a couple remarks.

One is that I think, John, we do certainly intend to include all of those in the total system performance assessments. You're correct that the numbers that Bruce has been talking about today have been direct release and transport numbers, but certainly, if you look in the total system--and I'm not sure if Greg's going to talk about any of these this afternoon, but certainly, the intent is to assess the impact of volcanic hazards on total system performance.

I wanted to provide a couple of pieces of information. One is that there is seismic data at Yucca Mountain. There's a line that runs up Forty Mile Wash that a number of people here have seen.

The second is that I guess I just want to kind of ask you the question, because it follows up on some of the things that Bruce said earlier. Certainly, currently it's our strategy that the principal thing that we rely on when making calculations of volcanic hazard is the geologic record of the volcanic rocks in the area, and if given the option of relying on that versus the, you know, the teleseismic data, for example, in which we have a velocity level that extends from Southern Amargosa Valley to Las Vegas, in an area with no historic volcanism since the Cretaceous, using that data to, you know, and folding it into a probability estimate is a very soft sort of a thing.

I mean, I don't disagree that it's something we need to be able to provide a response to, but I also don't expect to be able to prove to you what that is, now or in the near future, because I don't think that it's likely that we're going to have that level.

So I guess my question is, do you not agree that the basic geologic record of basaltic volcanism is the main basis for your probability calculations?

MR. TRAPP: As you--has been pointed out, the total record of basaltic volcanism is not at the surface.

MR. DOBSON: I understand.

MR. TRAPP: As also has been pointed out and brought out in some of the discussions, the assumptions you make on structural control make a hell of a lot of difference in the final number. Which structure I'm dealing with is going to give me a totally different number as far as probability than a random-type distribution. I don't agree that it's going to be conservative to assume random, not if the structure is right underneath Yucca Mountain.

DR. ALLEN: Okay. Thank you, John. I think we ought to move on. Appreciate it.

The final presentation--

DR. BARNARD: Clarence, could I just say one thing there momentarily? In response--in partial response to your comment, I don't think that the lists that John put up there were things that we feel have to be resolved to some scientific level. As he had indicated earlier, the primary job of this is DOE and we assume the TRB will provide that--this help in this kind of advice as to what is really relevant. Those are merely examples of problems that support the general concern.

DR. ALLEN: Thank you.

The final presentation before the break will be, again, by Bruce Crowe, beginning the consequence analysis section.

MR. CROWE: Okay. I only have, really, a few brief comments I want to make and largely I'll introduce Greg Valentine, who'll be doing most of the talking.

But again, as Dave Dobson emphasized in the question at the end, we've been stepping through this conditional probabilities and now we're coming to E3. All of the calculations in the discussion has focused on the first two, which is the probability of disruption.

I might just quickly add--to address a point that John Trapp made--that the secondary effects are rolled into what we call intrusion of the controlled area, and that is -- part of the performance assessment calculations that is part of our program. It's not something that's being ignored, but it's being done as secondary, and so there is activity in that area.

Now, what I just want to focus on primarily is just conceptually what kind of a beast we're dealing with here when we talk about magma intruding on a repository, and there's a couple of key things that we can start to put some limits on, that we've touched on in various parts of the talk.

First of all, we know something about dike widths and dike lengths from observations of eroded centers, and in fact, I think we're in pretty good agreement between our position and the state's position on this, that these are pretty narrow things. I've put a range of about a half a meter to two meters; that they have finite lengths that are generally defined by their aspect ratios, in the range of one to four kilometers; and so you end up with, even intruding a dike right through the middle of a repository, you have a pretty small cross-sectional area that's available to actually disrupt the repository.

And the key thing is that we're not looking at effects up here for entraining wastes. We're looking at the geometry of a dike as it intrudes through the repository. What we observed with dikes in the field is--and what's been some of the models of propagation of dikes--is that they are--magma has a hard time making it up to the crust. The velocities are pretty low and most buoyancy models have a hard time getting magma to the surface, and a key constraint is the loss of heat to the walls.

And what we observe when we look at dikes in the field is they generally insulate themselves with their own walls. You have chilled margin in dikes, and the general thought in the literature now is those chilled models are key to allowing the magma to retain its heat and move through the crust, because if you drop the temperature just a small amount, it's going to solidify and we'd never see it at the surface.

So, in general, we think at these kind of depths, that there isn't a major effect. I mean, there are unknowns. We do see in some cases that basalt dikes come up sill bodies. We showed you on our last trip an area where this has occurred in east of Yucca Flat, but the general model, we think, is that these are narrow dikes that have virtually no effects down there.

Now, we do know that we start to see effects when they start getting -- volatiles, and with the water contents of the basalts that we're dealing with, we think these are pretty shallow levels. This is something that Greg Valentine will be working on, but our guess is that where you begin --

is somewhere in the last few hundred feet approaching the surface.

Then the other key ratio becomes, when does the magma actually fragment? And the current thought in the literature is that it begins to fragment when the over-pressure in the bubbles exceeds the yield strength of the magma, and again, those calculations suggest that it's at a pretty shallow interval, and we've got some handles on that from the previous studies that we published in 1983, where we looked at the amount of lithic fragments in erupted material. We concentrated on the pyroclastic component, because that's the most explosive of the eruptions. We didn't look at the lava flow.

What we see is that there's about .03 per cent by volume of lithic material in the Lathrop Wells scoria cone, and that the sizes of these things are pretty small. They're like millimeters to a few tens of centimeters. They're not big fragments. And so, just conceptually, if we take the most likely scenario, which is intrusion of a dike through here, and we're dealing with waste packages of dimensions of meters, we think that it's going to be a pretty hard situation for magma to carry waste.

The standard assumption often, in these calculations where we do all three conditions, is that the area of the dike is equal to the area the waste inventory is capable of carrying to the surface, or that there's a probability of one that the worst case could occur, and we would argue that that's not true. There's a whole range of scenarios, and what we're going to probably focus on in our work is just what are reasonable ranges there. We don't think that the assumption of direct contact and immediate release is a valid assumption. We don't know what the right number is, but we don't think one is the right number.

So now what basically I want to--Greg will be basically talking about what our capabilities are for kind of modeling and constraining these processes, and let me just kind of put this into logic. I think I want to ignore the upper part of this, because I probably wrote this at a point of unfair optimism or delusions of something.

How do we go about trying to look at eliminating this as an issue? We really have two pathways open to us now. We just discussed that in my previous presentation. We can really begin to look at the probability distribution of E1 and E2. We can look at model sensitivity, and we can possibly invoke expert opinion and begin to refine those distributions. And then we end up, we're going to probably be negotiating with the NRC as to what is a critical cut-off value? It's very likely they're not going to accept a mean value, but what's a reasonable value? Is it two sigma from the mean? Is it a 30 per cent, is it 10 per cent, whatever? We can begin attacking that number, and we may be able to make an argument that our distribution leads us to values below 10^{-8}. We can't yet say that with certainty. We really have to go through these processes of kind of fine-tuning that distribution. That's one way to take on this process.

The other way, possibly, would be if we want to fold consequences in, would be to look at what those consequences are, and again, we have one or two ways. We might be able to put some bounds by looking at the lithic fragments and looking at the probability of incorporating material at repository depths to bound that, but there are ranges of other scenarios; for example, some sort of a sealing (phonetic) complex in the repository and ejection out, say, vents or access vents or whatever that we, again, can't rule out, but we might be able to make arguments are improbable.

And more than likely, we think we're probably going to have to go to expert opinion to make those kinds of judgments. I just pulled some numbers off the top of my head. We, based on calculations that we've done in--actually, I didn't do, but a Sandia group did, with my input--about the amount of releases for a typical Strombolian scenario. It was actually quite small, considerably less than the regulatory guidelines. If I had to make an estimate of the range or the probability of exceeding the regulatory releases, I'd say it's probably somewhere between one in a hundred to one in ten thousand. I don't know where it is. I don't know whether I might be pushing near one in a hundred, or maybe even--maybe one in ten is reasonable, but the key point is that if we accept conservative values on the first two parameters--and we just ended up saying, let's just accept there's one chance in ten of exceeding the releases. That puts us well below that 10^{-8} value that we've been talking about through this whole thing, and as Dave correctly pointed out, is not an elimination, but it certainly argues that it doesn't have to be in the CCDF.

So we're really at a key point here where we've played with the numbers and we have a variety of ways to go, but we really see that we're right on the sensitivity of potentially arguing that we're well below the 10^{-8} and we have several ways to go.

What Greg will talk about in his talk is kind of the techniques that we have available at Los Alamos to begin to try to put some bounds on consequences, and that's all I'm going to say on this.

DR. ALLEN: Leon?

DR. REITER: Yeah. I just want to make sure I understand the last thing. The probability of exceeding the 10^{-2}, 10^{-4}, is that of exceeding the CCDF?

MR. CROWE: That's a direct release.

DR. REITER: For the direct release, okay.

MR. CROWE: That's for direct release, right.
DR. REITER: And what's--and what would it be if it's ten times direct release?
MR. CROWE: That I haven't calculated.
DR. REITER: Because there you used 10^{-3}; right?
MR. CROWE: I'm sorry, go through that one again. I got lost.
DR. REITER: What's the--where are the EPA--
MR. DOBSON: Was that done by the TPT group?
MR. CROWE: No, that was not.
MR. DOBSON: I don't think that they went into--I don't think they did separate assessments or elicitations at a, you know, Table A and ten times Table A.
MR. CROWE: No; right.
MR. DOBSON: It was just a rough estimate of exceeding the EPA standard at some point along the distribution described in the EPA standard, and those are just judgments. Those are not hard numbers.
MR. CROWE: Those are judgments. Those are not hard numbers that we put together by a panel or anything. Those are just some--
DR. REITER: And the last one, the last line says: 10^{-1}:PD_{dq} a lot less than 10^{-8}. Say that in words.
MR. CROWE: What that says is we go back to here, and if we take E1, E2, and E3, if you can add just one order of

magnitude to the numbers that we think are pretty reasonable for E1 and E2, we have a lot of confidence that we're below 10^{-8}. The issue is whether or not, through a consequence analysis, we can demonstrate. I mean, the argument that we could make is--I'm pretty confident we could get almost any panel together and they would give very strong support that the probability of exceeding releases is much less than one in ten, and what I'm pointing out is that one in ten really buys us a big buffer from that 10^{-8} line that I showed you as the last slide on my previous talk.

Does that help?

DR. REITER: I'll talk to you afterwards.

DR. ALLEN: Other comments or questions?

(No audible response.)

DR. ALLEN: Dave, you look like you're ready to say something. Anything else?

(No audible response.)

DR. ALLEN: Okay. Let's call a break for twenty minutes, which will mean at three-fifteen we'll--well, let's make it three-ten--or pardon me, three-fifteen we'll reconvene.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Well, may I just say a word about the round table discussion that's going to start in another half hour or so. The Board is very much interested in sort of getting your opinion, particularly the result of today's meeting on these questions we ask here at the end of the agenda, and let me just say who I hope will participate and sit up around the table here. It includes most of the speakers, and a few other people; Dave Dobson, maybe Ardyth, if you have something to say, Ardyth Simmons, Ron Ballard, Mike Sheridan, Bruce Crowe, Frank Perry, Gene Smith, from the USGS, Gene Roseboom and Duane Champion, I guess; John Trapp, and Greg Valentine.

And what I'm going to do is ask each of you, if you are wiling--if you're willing to take on the assignment--to just--oh, and Chih-Hsiang Ho, I guess I didn't mention your name--if you're willing to, maybe in two or three minutes, just very briefly summarize your opinions on the four questions here, and let me just again reiterate those:

On which issues is a consensus developing? And maybe as a result of today's talks, you might have changed your ideas on that. On which issues are there serious deficiencies or differences? Are these issues important with respect to site suitability and public health and safety? That, I think, is a critical question; and fourthly, how can these issues be resolved?

Now, if each of you talks ten minutes, we're going to be here until seven o'clock, but I hope maybe you can summarize very quickly, in two or three minutes, what some of your primary opinions are in those areas, and the Board would greatly value hearing those.

Okay. So for the final presentation, we have Greg Valentine, and you're on, Greg.

MR. VALENTINE: Okay. As John Trapp and Dave Dobson and Bruce Crowe and others have alluded to, one of the directions that we need to go in volcanism studies is actually assessing the effects of vulcanism penetrating the repository, and we tend to think about the eruption process or the subaerial explosive kinds of events that can distribute waste, but in addition, I will be talking about subsurface processes, also, because they're very important.

This is an outline of my talk. I'll start off by casting a framework for what we're doing, and then go through subaerial processes. There are basically three kinds of subaerial eruptions that we can get. Those are hydrovolcanic; Strombolian eruptions; or lava flows. Then I'll go through the subsurface processes, which have to do with dikes and their thermal effects, and then the long-term effects of dikes. And this is really a new work that's just getting underway, so I'll be showing some preliminary calculations, but they are preliminary.

The purpose is simply to assess the amount of waste that can be transported to the accessible environment as a result of volcanism. This interfaces with the other parts of the volcanism task by providing--I wrote P_3, but Bruce has had E3 on his view graphs, so--the probability of exceeding the release limits. I spent a lot of time interfacing with other performance assessment people, like from Sandia. Sandia people mostly work with the hydrologic factors of the repository. I also worked with people from Livermore, who know a lot about waste packages, so we're looking into the effects of intrusions on waste packages and such.

In general, especially for the subaerial processes, we want to base the conclusions, as much as possible, on observations at volcanos around Yucca Mountain or in analog volcanos. And theoretical modeling will come in when it can really provide a key to help us understand what's going on, and, also, when it can be used in a predictive mode.

At the bottom, I put the level of accuracy for effects is going to be partially--for effects studies is going to be partially determined by the probability of those, of given events, and I'll talk a little more about what that means later.

In order to study release effects from volcanism, we need to come up with scenarios, and we based the scenarios on observations in the Death Valley/Pancake Range volcanic field. And, as we all know, these are small volume basaltic centers that are produced by combinations of hydrovolcanic eruption, which involves hot magmate reacting with ground or surface water. Another component is scoria cone, which is more of a fire fountaining kind of process, maybe pulsating, like we all see in the footage from Kilauea, although much smaller, obviously. And also, we have small volume lava flows that are associated with the eruptions.

So if a typical basaltic center forms in the repository block, how much waste will get out, and by what mechanisms? And mainly, what I'm going to be doing is showing strategies for addressing these problems, at least to start with here.

For hydrovolcanic explosions, we need to understand the mechanisms, obviously. Most hydrovolcanic eruptions happen in valleys where the groundwater is shallow, or where there's surface water, so we're not sure what would happen in Yucca Mountain, where the groundwater is relatively deep, but we need to understand more of that process, and I think the literature probably has the information that we need to understand whether or not we can get very explosive magma/water interaction at depth. But this is important, because hydrovolcanic eruptions, as Mike Sheridan said, can eject large quantities of radiolithics, or whatever they are; wall rocks from the intrusion.

Now, the main thing to do is to go out to the hydrovolcanic eruptions that are recorded in the Death Valley/Pancake Range field, and look at the lithics that have been ejected, the wall rock fragments that have been ejected during these eruptions, and what depth those fragments come from, because obviously, that's a very crucial thing to understand, is how much will be coming--how much could possibly come from the repository depth.

In order to do this, we'll need to use analog studies where the basement stratigraphy underneath the volcano is very well constrained and is very easy to trace vertically; for instance, in the Colorado Plateau, where we have very nice, smooth layers of sandstones, limestones, we can go look at the lithic fragments in volcanos on the Colorado Plateau and match the lithic fragments to some depth.

Okay, and I already mentioned theoretical considerations of depths of magma/water interaction and the effects on surrounding rocks.

Now, next, I'm going to talk about the Strombolian or Hawaiian eruption, which is what forms the main scoria cone construct of these volcanos, and I've started to do some initial work on that. Again, these are characterized by steady to pulsating fire fountaining, big clots of magma being thrown up, and the eruptions are driven mainly by expansion of magmatic volatiles, instead of explosion of groundwater. Typically, these scoria cones contain less than 1 per cent lithic fragments, and actually, usually less than a tenth of a percent lithic fragments. So that, right away, gives us some handle on how much repository material could possibly make it out, and Bruce Crowe and others considered this in a first order way in their '82 or '83 paper.

Again, the approaches will be to make field measurements of the eruptive products and their facies, and use analog studies to determine the conduit erosion process. Theoretical modeling will be used in parallel with this to help understand the eruption processes and the depth of fragmentation and other things that Bruce has mentioned.

So right now I just want to go into some of the features of these Strombolian or fire fountaining eruptions. This is a damn good numerical model, with an explosive eruption. No, this is Kilauea, somewhere on Kilauea, I think; a picture taken out of a book.

Just to key you in to the features, there's a tephra cone that's formed around this fire fountain. This fountain is very well collimated; in other words, it's very narrow and it stays very straight as it goes up. The core is very hot, and the particle concentration is very high. It's optically thick, and cools very inefficiently, so the material in the core of the fountain stays very hot.

As you move out away from the core of the fountain, the particles are able to transfer their heat to the air by radiation and convection much better because the concentration, particle concentration decreases, so the particles get dark and as they cool down, of course, they become more viscous, and eventually brittle. So what's happening is, material is rising up, and then falling back to the ground, and above the main fountain there's a very low concentration cloud of ash that rises blatantly into the air.

Now, we can use the deposits from these scoria cones to help us understand the parameters affecting the eruption dynamics. This is from a recent paper by Head and Wilson, in Journal of Volcanology and Geothermal Research, and they were discussing how the exit parameters or the vent conditions in one of these eruptions affects the deposits, and possible ways to work backwards from properties of deposits back to the eruption dynamics.

And they discuss this all in a very qualitative way, but basically at the vent you start off with some gas content and some volume flux--also some temperature, of course. This produces a fountain structure, which is collimated to varying degrees, and this affects the opacity and how well material can cool. The wind profile also comes into that. The wind can bend the column over or expose the inner part of the column by stripping away the outer parts and affect the cooling.

These vent conditions also affect the clast sizes, and the clast sizes determine partially what the clast temperatures are when they hit the ground as they're deposited. They're also affected, of course, by the fountain dynamics. The fountain structure and the wind profile directly affects the low clast accumulation rate, and what happens is you end up with this accumulation rate and the clast temperature being the primary factors that determine the characteristics of the deposit at a given point. So they've qualitatively put down here that this is the--on the bottom axis would be the clast temperature when it hits the ground, and the vertical axis is the accumulation rate.

When you have very high accumulation rates at very high temperatures, the clots coalesce and can form lava flows. When you have lower accumulation rates and lower temperatures, you get brittle cinders hitting the ground and forming scoria slopes or scoria fall sheets.

Just to put this in the framework of what a scoria cone would look like, faulting--okay, never mind. So when you have one of these eruptions, you have a fire fountain forming and the center of the fountain, the inner fountain is hot, dense, high particle concentration, and material that falls directly out of the inner fountain falls at a high rate, accumulates at a high rate and it's very hot and, in the extreme, can form lava flows when it hits the ground. As you get farther out, the particles are lower in concentration and cooler, and form just normal scoria layers.

Sort of an intermediate condition, where the particles are semi-hot and they're landing semi-fast, gives you welded spatter, which is commonly observed right up on the shoulders of these scoria cones, and in the inner slopes of the craters. Now, this is all leading somewhere.

Now, one of the things I work on is two-phase flow numerical calculation of volcanic eruptions, and what I'm going to be showing here is a numerical simulation of particles in gas flowing together under the conditions appropriate for a Strombolian eruption, as one combination of parameters. What you'll be seeing is an eruption of basaltic magma. The clasts are about one centimeter in diameter, and the temperature of the eruption is 1200° Kelvin, a gas content of, let's see, .6 per cent or something--.06 per cent; very low. And what's actually being animated is the particle concentration, with red being very high concentration, going down through black, and to gray being very low concentration.

(Whereupon, a video was shown.)

Okay. So while it's stopped, I'll just orient you. This red region, there's a vent with a diameter of 20 meters here. The red region is very high particle concentration and what's actually happening is particles are flying out and fountaining back down, all within this red region. The scale is 700 meters by about 700 meters on each side of the vent. And what's happening is above this there's a cloud of ash that is very low concentration that rises because it's buoyant with respect to the atmosphere, and that's the component that would give us a widely dispersed fallout sheet, and if it had any radioactive material entrained, would probably be something we'd need to address. So let me see if I can get this going. Okay, so it's going to go over again, and it's actually showing only about 70 seconds of the total eruption.

So the point is, this is an example of some of the techniques we'll be using to address the release of material from volcanic activity. Ignore the no caldera rim, but this isn't a caldera eruption.

So just to real quickly go through what that can tell us, for this eruption, I've plotted along the ground the accumulation rate in meters per second, so this is meters per second of deposit building up as a function of distance from vent. Within the vent is--has a 10 meter radius, so it's just this shaded region right here. Okay, there's a peak in accumulation rate at a distance of about 40 meters from the vent, and the base of the cone that would form from this would be about 100 meters out from the vent. And it turns out that this--presumably, this peak in accumulation rate would correspond to the rim of scoria cone, and this matches very well what's observed in scoria cones where the radius of the crater is typically about 40 per cent of the radius of the entire cone.

So we have the accumulation rate and the particle temperature, so these will vary with varying eruption conditions, so we'll be able to use this kind of modeling in combination with observations of the deposits to backtrack some of the dynamics of these eruptions, and then go into a predictive mode.

Let's just real quickly go through some of the subsurface effects. I've been working a lot with performance assessment people on this, and we've been studying a basaltic dike intersecting the repository. We do this in three stages. The first is to analyze the intrusive event itself, which primarily focuses on the magnitude and duration of the thermal pulse, and the dynamics that result in the mountain from this thermal pulse. Then we look at the effects on waste packages, and that's mostly people from Livermore, waste package designers. And then, finally, we go back--once we know how much material gets out of waste packages under these conditions, then we go back and allow this material to be transported by various means through the mountain, by vapor phase convection or by liquid flow, whatever.

I'm going to show you an example calculation. This is like a chunk taken out of Yucca Mountain, with the groundwater level simplified to be constant at 750 meters elevation. The top of the mountain varies in elevation from 1100 meters at the south end, to 1220 meters at the north end. The sloped face over here corresponds to the slope on Solitario Canyon. The pressure on this boundary is set at hydrostatic, and then what we do is we allow the repository level to heat up according to the heat loading from the waste packages themselves, and then at some arbitrary time--in this calculation, it's going to be 5,000 years--we emplace a dike along this one face--this is sort of a symmetry plane here--and allow it to cool, and allow the air, the vapor in the rock to convect from the heat. And it turns out that air in the unsaturated zone convects a lot, from a very small amount of heat, so you can imagine that the input from a dike can cause a lot of air flow in the mountain, and some radionuclides are transported in the vapor phase, so it's important to understand how the vapor phase flows.

This is just to show you what the waste package heating looks like at the repository horizon. We smoothed this out, but basically what it is, is the waste package is heating up to about 200° C. at 20 or 30 years after emplacement, and then slowly cooling off. Emplacement of the canisters; closure of the repository. Somebody thought we needed to understand what the background heating was before we did the dike.

Now I'm going to show you another animation, and this is kind of difficult to see, but the graphics put this prism that I showed you into this rectangular box so that the actual computational domain is only a prism that fits within this box, and you'll be seeing contours of temperature. The two temperatures that you'll be seeing correspond to the volatilization temperature of two radionuclides that are in the repository. Okay. This will be real quick.

(Whereupon, a video was shown.)

This was done with a porous flow code that was written by George Zivloski (phonetic) at Los Alamos. This is showing temperature for those eruption simulations, so sorry about that. I just need to fast forward through these.

DR. ALLEN: Well, Greg, I think we better call it quits.

MR. VALENTINE: Okay.

DR. ALLEN: Concluding remarks, or--

MR. VALENTINE: Okay. Well, in conclusion, we're working with the performance assessment people and waste package design people to study these effects, and in the future, the immediate plans are to focus on the conduit erosion for the volcanic eruptions, as I spoke about before, and also to continue working with site suitability and performance assessment people.

DR. ALLEN: Okay, thank you.

Do we have questions from members of the Board; from anyone else? Mike Sheridan?

MR. SHERIDAN: Greg, I've got a question or comment, that in our--in the EPRI evaluation of the waste packages, we took into account that there would be a probability function of waste package lifetime, so that after a given number of years --which is a few thousand years--or starting from a few thousand years onward, that there would be some waste packages that would be completely ruptured. Is this the method that you used, assuming that some--a certain fraction of the waste packages would be intact, and another fraction would be completely ruptured? Because this would have quite a different effect on lava flows or other types of volcanic disruption.

MR. VALENTINE: The people who work on waste package aspects assume various kinds of distributions of waste package failure. We basically haven't gone that far. I mean, they're working on that right now.

MR. SHERIDAN: Okay.

DR. ALLEN: Okay. Thank you, Greg.

May I ask those people that I named earlier if they would come up here and I'll ask the other TRB people to leave the table to make some room, if you don't mind, Bill. Don't worry too much about the name tags.

Incidentally, I appreciate that the NRC people here are in a little bit of an awkward position. They're the ultimate licensing authority here, and consequently, to the extent that they wish to decline to say anything at this point, they're certainly welcome to. I appreciate your position's a little bit awkward.

So why don't we start with someone we haven't heard from yet today; on my left, Gene Roseboom of the U.S.

Geological Survey, and any thoughts you have on this will be welcome.

MR. ROSEBOOM: Certainly, looking at the four questions you have for the round table discussion, it seemed from this morning, at least on the issue of the geochronological ages, that there are at least some apparent differences. Some of that may be, at this stage, the sort of differences that could be sorted out with a little more time, and a workshop, and some assorted senior scientists with experience in geochronology sitting in. I think that would be the first step to try to explore those differences.

I think beyond that, at this stage, Bruce also pointed out the next way to go, which is to consider what are the alternative models, then, for the Lathrop Wells center. Is it really a simple monogenetic center, or a polycyclic center? And explore, then, what the--given the different models, explore what would the possibilities be in terms of the performance of the site, particularly the first item, the recurrence of a volcanic event. The different models will give you some different recurrences, and so you could at least, at that point, get a sense of which of the models might present the most serious problems in terms of the ultimate performance of the site, and then focus on the parts of the models that seem to present the major problems.

DR. ALLEN: Let me just ask, already some pretty senior scientists have made their opinions well known on the geochronology issue. What reason do you have to think that a workshop is going to resolve this, rather than five more years of research?

MR. ROSEBOOM: Well, it might not resolve it, but a workshop would last about two days, and five more years of research would be five more years, so that at least it might be worth trying to--if there is not a consensus, at least to get agreement on where the disagreements lie, and at that point, maybe one can proceed with what that implies in terms of different models.

DR. ALLEN: Well, isn't it pretty clear already where some of the disagreements lie?

MR. ROSEBOOM: Yes.

DR. ALLEN: Well, okay. We'll come back to this.

Gene, why don't you give us the benefits of your thoughts?

DR. SMITH: I think the important thing that we have to resolve is Question No. 3, whether or not the suitability of the site is affected by a volcanic eruption, and in order to do that, we have to calculate a number that'll indicate what the probability of eruption is, and we have to gather--there's a lot of information that has to be gathered in order to come up with that number.

And as I've been sitting here today, one of my concerns is that we're coming up with numbers in a variety of different ways. Bruce, Ho, Mike Sheridan have all proposed models. My main concern is we're producing models without having the data base that we need to make those numbers meaningful, and I think we know a lot. I think there's a lot of stuff that we know. There's agreement, for example, in terms of the chemistry. We all know that the basaltic volcanism is what we have to worry about, but there's a lot of things that have to be resolved. For example, we don't really know what the age of these features are. We don't know whether they're polycyclic or monogenic. We're not even sure what structures control these features. There's a lot of things that we still have to know.

I'm not saying we have to know everything, but there's some very critical things that have to be known before the numbers that are being thrown out will be meaningful. So I think in terms of, how can these issues be resolved, I think we have to do some good, basic geologic work in order to try to gather some of this information before at least I will feel comfortable with any of the probability numbers that I'm hearing.

DR. ALLEN: Let me ask this: Let's assume Gene's magic workshop resolves all these differences between the various chronologists, geochronologists tomorrow, and that issue is resolved. What more do we need beyond that, then? A great deal more sampling, or what?

DR. SMITH: Well, I think that we have to understand the nature of one of these eruptions. Are they polycyclic? Are they monogenetic? My feeling is that these features are polycyclic. My field work tells me, my basic geologic work that I've done suggests that they are polycyclic, but obviously, this is an issue.

We have to know how many vents there are. We have to know whether these vents are controlled by northwest trending structures or northeast trending structures, and that's very important. If they're controlled by northwest trending structures, there may be less of a chance for the repository to be intercepted by a dike or a center. If they're northeast trending structures, that might be more of a chance that that might happen. So I think we have to know more about the structure, have to know about the nature of the eruptions.

DR. ALLEN: Do you have specific field work that you would like to do in this regard that, given permits and so forth, you could do that would help answer that?

DR. SMITH: Yes, I think so.

DR. ALLEN: Okay, thank you.

Duane Champion?

MR. CHAMPION: I see a significant division of opinion in the basic chronology, in the basic perception of the phenomena we're trying to project and the certain probabilistic consequence argument. I would hope, from a personal standpoint, that the panel at this point would at least be confused about what the right answer is.

DR. ALLEN: The panel being what, this Board, or what?

MR. CHAMPION: Yes, sir.

DR. ALLEN: Or the DOE, or--

MR. CHAMPION: Right; this panel of the Board, yes.

DR. ALLEN: What makes you think we aren't confused?

MR. CHAMPION: Well, I would hope that you are. You've been presented with a--from the DOE side in this discussion an argument of a completely unprecedented style of volcanic activity which seems to have a lot of data to support it. I don't concur with that.

In a situation where you are projecting a new model to explain things, it's typical in science to require extra proof of a new and radical departure from previous thinking. You have to convince people to change their minds. But if you can--I mean, if we can assemble the data to prove that polycyclic volcanism is, in fact, a viable mechanism, then it would seem to me that the probabilistic arguments, from what I understand of them, suggest we're on the fringe of having a non-important phenomena, or a less important phenomena than might be viscerally perceived. Ooh, volcano close, let's be scared.

If that is, in fact, the case, if we can slide the probabilistics by either being able to spatially predict or time predict where the volcanism occurs and at what vents it occurs, then perhaps we can de-emphasize this aspect of the overall study, and here I rise to a statement you made earlier today, you know. We're trying--we, you--are trying to make a decision about what we should or shouldn't be doing, and that relates to Question No. 3. Is there a clear evidence of hazard to the public over this particular phenomena?

It will be decided by time. Science is decided by time, by a peer process through years of time, but what is right is what most of us think. It isn't an isolated opinion. Do you want to wait for that time frame or not? If you want to wait five years, these opinions will bang against one another and the truth will be what most people think in the end. If expediency is of an issue, in order to shepherd resources towards problems that are identified as being more critical or more dangerous, well, then perhaps you need to know sooner where the center of this thing is going to come down, and to accomplish that, I suggest that you assemble a peer review panel to look into the very large--I mean, it isn't that we don't have much data. We have a lot of data. There's a lot of data sets that can be looked at. What relates to the essential issues of volcanic hazard? When? How much essential physical phenomena that we're dealing with?

Some of the studies that are going on strike me as somewhat oblique. They don't really focus on those essential issues of volcanic hazard from an engineering standpoint, and I don't speak for the Director of the U.S. Geological Survey. Perhaps Gene does, but we have three observatories of volcanic hazard experts that we loan regularly all over the world, and I'm sure if you ask Dallas Peck, he could maybe loan you a couple of those guys for a meeting or two, and I'm sure Bill Melson could suggest, from an academic, you know, university standpoint, university people with solid credentials that could give you some advice about where the center of this data is going to come down, and not wait for a five-year or ten-year or fifteen-year time frame.

DR. ALLEN: Although, isn't it true that we don't--we're not so concerned about where the center of the data is, as where the tails of that curve are and what we have to guard against in a conservative, very conservative outlook towards the repository?

MR. CHAMPION: I'm not, you know, I mean, there was a comical allusion earlier to, you know, get a panel of experts and it'd be like a weather vane in a high wind, it'll just point all over the place. But it seems to me that's the best shot you've got. They can tell you about the vitality of all the data sets, and clarify the confusion, if that's what it amounts to. But I guess I urge you to seek expert opinion if you want to speed it up. If you don't need to speed it up, then time, time will resolve it.

DR. ALLEN: Okay. Thanks, Duane.

Greg, do you have any words of wisdom for us? You were one of those who's on the firing line of the consequences end of this thing.

MR. VALENTINE: It seems to me--this could come as a surprise, because I'm relatively new, I guess, to this project--

DR. ALLEN: You're not biased like the rest of us, are you?

MR. VALENTINE: Excuse me?

DR. ALLEN: I say, you don't have the biases the rest of us have by having been associated for 10,000 years already.

MR. VALENTINE: That's right, and let me say this...

The problems with whether or not these volcanos are polycyclic, I think some of the physical aspects need to really be worked out as to whether small volume polycyclic volcanos can work. I mean, from what I've seen in the field and the arguments I've heard, I think they do exist, and it behooves us to work out the mechanics of how this goes on in order to help support our position, and this needs to be tied in with geochemistry, such as what Frank's doing.

And it seems that there is a consensus that some of the effects do need to be worked on, so I hope we'll be pursuing that.

DR. ALLEN: Okay, thanks.

Frank?

MR. PERRY: I still think one of the essential pieces of data we need to resolve the polycyclic versus monogenetic story is to get a hold on the basic stratigraphy at Lathrop Wells. Once that's understood, then that constrains all the other data, and that's going to take trenching. But that will, you know, will constrain the geochemistry, the soils data, the geochronology. And I really think until we get that, things like peer reviews are just going to kind of go around in circles, because no one's going to agree on a lot of things, but the stratigraphy will really pin it down.

One thing is, another part of petrology, I'm wondering if people would agree to a consensus that regionally, at least in the Yucca Mountain region, that volcanism is waning in the sense of a decay of a regional heat source and in the volumes being erupted, and that can be extended all the way back to the caldera forming eruptions. Those, you know, are phenomena, ultimately, of basalt coming into the crust, so there must have been large volumes of basalt at that time, and then, you know, we've seen from 3.7 on that's continued to decline, and I don't know any evidence that things could be going the other way.

So I think just as a framework to put the volcanism in, that maybe we could agree that it is waning.

DR. ALLEN: Let me ask you this, Frank. In the absence of formal permits of the type we need for trenching, are there realistic things we can do right now, or could do that would help resolve the question? Or have we done everything we can without--

MR. PERRY: We've pretty much done the basic field mapping. There's really critical places where we can't see the relationship between units, and I think that's going to take trenching, and then that will be the strongest constraint to many of these models and arguments.

DR. ALLEN: Okay, thank you.

Dave? Speaking for the Admiral.

MR. DOBSON: No, not really; speaking for myself.

I guess I'd just sort of endorse what several people have already said. I think the critical question is whether or not we can bound our estimates of the risks to--about the performance of the repository and public health and safety.

I guess I just--I have a couple of perspectives, and one is that having been involved in about a half a dozen peer reviews over the last two-three years at the DOE, I guess I would guard against the notion that assembling a group of experts in one room is likely to result in a 100 per cent unanimous consensus, and I think that we in, you know, we in the Department think it's extremely important that we build credibility and an acceptance in the technical community among all the organizations that are involved with us and that oversee us, including groups like the Board and the state, and I think we can move in that direction, but nobody has yet defined that, you know, what the standard is. You know, if 81 out of 100 experts agree, but the other 19 don't, nowhere is it written what the standard of reasonable assurance is in a regulatory sense, and so I think that's something that we need to continue to interact with the NRC about, and I'm sure we will over the next couple of years.

I guess I have--I do have one other perception, too, and it's just sort of a comment on some of the discussions. At least based on all the interactions I've had with the public, I'm not sure that they discriminate between 10,000 year cinder cones and 100,000 year cinder cones very effectively, and that's why I think I would kind of endorse what Gene said earlier, that the real issue is what, you know, what likelihood there is of something that's going to cause problems in terms of public health and safety.

And I guess kind of a last remark is I'm not sure that there is a lot more data that we need in order to address this question in terms of risks. I mean, there's--I know there's one very critical piece of data that Frank just described--and I think Duane mentioned, too--and that is the stratigraphy of Lathrop Wells, but I also think that Bruce showed calculations that whether you change that number from 10,000 to 100,000 doesn't result in a large shift in the probability estimates, and so I think that's significant.

We have a considerable amount of data on the orientation and location of basaltic rocks, and we have a fair amount of geophysics, and we will get considerably more. Certainly, you know, the--figuring out the ages of all what we presume are buried basaltic rock zero mag anomalies may have some effect on the volume calculations, but we can, you know, we can bound that already as well. We can assume they're all young basaltic centers and calculate probabilities.

DR. ALLEN: But isn't it also true that, despite Bruce's statement that we can absorb a lot of uncertainty, that from the public point of view, the more the scientists disagree with each other, the more they will lack confidence in the whole process?

MR. DOBSON: Oh, absolutely, but the reason that I brought up that topic was that I think that, at least from my perspective, the goal isn't necessarily to get everybody in the technical community to agree that the age is 108,000 ±4. The goal is to get the technical community to agree that volcanic hazards don't represent a significant risk to public health and safety, and that's--

DR. ALLEN: Either to agree they do or they don't.

MR. DOBSON: Exactly. And so, you know, we sometimes--

and, you know, I've--like I say, I've been involved in half a dozen peer reviews on unsaturated zone hydrology and reviews of tectonics and hydrology and all kinds of things, and, you know, it makes for a very vibrant and, I think, healthy discussion of all the different interpretations of data, but I would be very wary about ever trying to say, well, here's a point. Now, I want everybody in this room to agree on this point. It's a difficult way to do science in the first place, and it's not necessarily really productive.

So, I guess sort of my summary is, I do think that we have made progress. I was--I don't know if you would exactly agree, but I was very interested to see that Dr. Ho's numbers on the rate calculations are comparable to the numbers that Bruce presented, and they're based on a separate and different assumption of the distributions. We can take additional steps in terms of trying to get people together on the structural models, and I think maybe that's a significant next place to go, because it appears to me from this meeting that that's where the biggest disagreement exists right now, is what's the number that--once you've established a rate number, what's the probability that something within a volcanic zone is going to encounter a repository?

And I--but I, frankly, I don't see that we're that far apart. I don't see those numbers even there ranging more than, you know, an order of magnitude or two in the different hypotheses that I've found--I've heard, so I think we have made a fair amount of progress and, you know, I want to credit all the individuals that have been involved in terms of encouraging a healthy discussion and keeping the issue sort of in the forefront of the discussion. But I think that there's been quite a bit of good science done by all parties, and so from our perspective, that's a good thing, and so, like I say, but I think we should be a little hesitant about trying to get everybody to agree on every interpretation of every data point.

DR. ALLEN: Okay. Ardyth, you've been very quiet today, which presumably means you've been thinking very deeply.

MS. SIMMONS: Well, the only thing that I would like to reemphasize is that I think we do have to keep things in the proper perspective for what the real problem is that we're trying to solve, and my feeling is that getting an exact age from the Lathrop Wells cone is not what the real issue is here, and--nor is it trying to bound the geology by one particular model. I think that we are always going to be dealing with uncertainties, and that we have to be aware of those and look at the range of models that can be used to bound those uncertainties.

And I think that we are at a point where we have gotten all the, or almost all of the data that we can at this point, and perhaps we need to focus on some other studies in order to be able to deal with the volcanism issue in a bit of a different context from what we've been looking at so far. And perhaps that only then can we come back to this issue and maybe reassess it, because--

DR. ALLEN: When you say other studies, what kinds of other studies?

MS. SIMMONS: Well, for example, some of the things that John Trapp has been mentioning. We do have a number of other geological studies related to structural features at Yucca Mountain, other geophysical studies. He mentioned 22 study plans that discuss volcanism, and it's true that the work that Bruce has been talking about today is the center of the volcanism studies, but there are many others that are related, and maybe we need to get on with some of that work, and then come back and re-look at the volcanism issue, because it seems that we are not really gaining the consensus on volcanism as a risk.

DR. ALLEN: Okay, thank you.

Carl?

MR. JOHNSON: To come back to a point that I made in my opening remarks, and that is that the volcanism issues are still alive and well, and I think this whole meeting has certainly supported that.

There's a lot of things I could talk about. Let me focus on a couple of things, though, and the interesting remarks that were made by Brent Turrin and Duane Champion, and that has to do with public perception; Gene's remarks that whether we have a 10,000 year event or we have a 100,000 year event, in the public's eye, it's all the same thing. Active volcanism is active volcanism, and that in the context of that, I would, at this point, be very cautious about convening an expert panel.

There was a lot of differences of opinion as to whether we really had gotten most of the data that was necessary to address the volcanism issue or we hadn't gotten all the data. I think we've heard the DOE say that, yes, we have all the data, but I think the other parties here had a different opinion, and I think it would be premature to convene a expert panel and have that under the scrutiny of the general public and debate back and forth these issues when we don't even have the data base to really provide the necessary credibility to those arguments that are really required.

Secondly, I think that the general public has a skeptical eye to scientific opinions in the first place, and at least in Nevada, the public has a much lower credibility point of view of Department of Energy and what it espouses, so I think we would be very cautious about convening a panel before we have a lot more information about whether to deal with it.

And the second point that I want to make in all this is we are kind of all, with the exception of John Trapp's remarks, forgetting that all of this information and all of these opinions and everything are going to have to be put in the context of regulatory requirements in a regulatory hearing. The burden of proof for all that is going to lie with the applicant. They're the ones who have to present all the information. Gene Smith and Ho and Mike Sheridan, they don't have to present anything. It's all the Department that has to present anything, and they have to make the convincing case to the regulators that volcanism is not an issue, and that case is going to have to be made on data. It's not going to be made on expert opinions of those individuals in a workshop or a panel situation.

DR. ALLEN: I wish you could help us get more data in terms of the licensing problems, but that's another issue, obviously, and not actually up for discussion here.

Ron, you would just as soon remain silent, or anything?

MR. BALLARD: Well, I will defer to those far more expert than I on the issues of volcanism and all. I would just like to mention one programmatic statement that I think would be helpful to us.

I had indicated earlier--and John Trapp did, also--that there are numerous study plans in the mill that relate in one way or another to the volcanism issue. I would put out a plea for perhaps some sort of a program description that would relate the various aspects of your studies so that we could have a better feel, as these study plans come in piecemeal. I don't know that that helps the panel much, but it would certainly help us as reviewers over in the NRC.

DR. ALLEN: I don't quite understand. What--

MR. BALLARD: Well, I had mentioned that there were a number of study plans. We have seen one. We have seen no more. You read a study plan and you're asked to comment on it and determine whether it's adequate or not in the--when there is no description available of where the other pieces fit of their program that relates to volcanism. So the point is that some better description of the overall programmatic approach, as it relates to volcanism and tectonics in general, would certainly be a lot of help to us, because as it is now, as we review it, we almost end up with a large number of questions because you don't know the rest of the program. These come in in very narrow increments.

So the point is, I think it'd probably be helpful to everybody to have a little--a programmatic description that would relate the various aspects. That's about it.

DR. ALLEN: Thanks, Ron.

Mike?

MR. SHERIDAN: Well, one of the advantages of being near the tail is that everything else has been mentioned, so that in terms of the four questions that you wanted us to respond to, I think the responses covered most of the items that I had listed on the paper ahead of time.

But it seems to me that various people were talking to different types of issues, and, for example, on the question of a panel, some people seemed to think that a panel was going to say that volcanos are a problem or are not a problem, and that would resolve the question. Whereas I see a workshop or a panel would be a means of conducting sensitivity studies to identify the key issues: issues where there was accordance; issues where there was discordance, and to determine, say, research priorities for advancing. Which I think is a key issue, and it's an issue that EPRI has sort of criticized DOE for among ourselves, is that it seemed like there was a whole shelf full of steps to be taken to resolve this question, and they don't seem to be taken in any sort of progression.

Whereas, if the research could be put into priorities, we see general areas where there's order of magnitude agreement, which I think is the critical issue, and that can be put in--order of magnitude differences can very well be put into a probability study, and those where there's two or three orders of magnitude difference, those are areas either where there can be no resolution and put it away, or a scenario where additional research could be profitable. I think that those areas have to be decided.

So I would say that that type of workshop would be very useful, but not a workshop to resolve the issue. I think that the volcanology should--I can't agree with Ardyth saying that the volcanology issue should be sort of put away because there seemed to be a lot of different viewpoints. I think that it--from public perception and from perception within the people here, that this could be the so-called show-stopper. It could be a critical issue. It could kill this site, or it could be an issue that, at another level, has to be considered and the effects have to be considered, but that it's one of many issues, such as groundwater or climate, and so on; or, it's an issue that is so far removed that we can just forget about it, and that means we can put our research resources elsewhere.

I think those three levels would be very important to decide, and I think that that could be decided in this sort of a panel if it were structured correctly.

DR. ALLEN: Do you think it would take a workshop, or would a workshop help decide that, or is that going to be decided in the course of events here?

MR. SHERIDAN: No, it will not be decided in the course of events, as far as I can see. I think that a workshop, a carefully structured workshop could key to those types of issues and resolve points of--and group together points of commonality and points of dispersion, and I think that would be very useful for all parties.

But in terms of work to be done, that's--some people are putting in plugs for these things, and I will do that, also. I think that one of the areas of--where there does not seem to be a convergence of opinion is in the spatial relationship of the volcanic process, and I know that Professor Ho is going to be working on this, and Gene Smith will probably be working on this issue with him. I think it's an area of key concern, and that a variety of different inputs from different sources would be useful here, and probably that question could be resolved in a year or two or three.

DR. ALLEN: Will you also continue to work on this?

MR. SHERIDAN: I would be very interested in working on this problem, and from a different perspective, but I think it's one of those issues that that, combined with the effects of the volcano--of volcanic events on the site that Greg is beginning to conduct now, these are very important things and the multiplication of those three probabilities is the bottom line.

DR. ALLEN: Bruce?

MR. CROWE: Actually, I think I want to address what I consider could be a misconception out of the meeting, and that is that we've got these gigantic areas of disagreements that are overwhelming us. I, frankly, am impressed more by that we have some fairly good agreements, and I would list just a few: One being that the states' discussion of the recurrence rate and ours is very, very compatible. I'm quite pleased by that.

I think we have general agreements on kind of the distribution of the volcanos, the kinds of volcanos they are. Gene and I are only off on a few details. I think we maybe are overreacting a little bit to the areas that we disagree, which is pretty common, particularly--I think I come from a different perspective than most people on the chronology problem. I'm not a chronologist. I don't profess to be a chronologist. But what I have discovered is chronologists are incredibly turf sensitive, and you begin questioning their techniques and they get very, very fidgety and very uppity, and very honestly, when I look at the data sets, there is not that much disagreement.

If you add plus or minuses to many of the techniques, we're not that far off, and I have to underscore what Frank said. Let us trench, and I think we're going to solve this problem. I think we can solve it in an orderly fashion. I don't think we're going to have any major catastrophe. I think, perhaps, what's been a little bit misguided here is people have gotten too polarized and basically it's this technique versus that technique. As I look at the data set, I see some areas of disagreement, but I see some signs of conversions that I think are fairly favorable. I personally think this can be resolved, and I think it can be resolved if people will be professional and try to back away from their kind of turf consciousness, and look at the total issue.

DR. ALLEN: You think we can resolve this without a great deal of further site exploration?

MR. CROWE: In fact, I would suggest that we basically continue in an orderly fashion of trying to follow our study plan, that the NRC has generally been favorable toward the characterization of volcanic features, with the exception of we can't--we need trenching to work the stratigraphy out. We really need trenching to implement TL effectively.

Jane has made major progress in helium. We can crank like crazy with helium now that she's gotten to where she is. We take surface outcrops for that. Uranium thorium work can proceed without requiring trenching. The key thing is the stratigraphy and the TL, but let me underscore, I think we have some agreements here, and I think the chronology is really just a minor part of a disagreement that's perhaps been overblown and over-sensitized, in my opinion.

I'm impressed that there seems to be somewhat of a consensus as to what a probabilistic approach to this problem, and I'm impressed because I think that's the way to attack it, that we can work out things probabilistically. We keep the perspective, as Mike just said, of what are the effects of these three conditions.

I would urge that perhaps we might want to look at a debate about what do we use. Do we use the geologic record as a perspective, or can we say because we don't know this, almost anything can happen? And that continually comes up as a key issue that we have to address.

In my mind, going down through what we've seen, I'm more encouraged than discouraged, surprisingly, after kind of an interesting start this morning. I think perhaps the key issue in my mind may be structural controls, and the reason I say that is we have a limited record, as I mentioned in my talks, and there may not be too much more we can do to bound this problem. I think we're close to having about as much data as we're going to get.

I mean, John mentioned the uncertainty about dikes. One of the nice things we have about the dikes here is that we have beautiful aeromag data, and all of the basaltic rocks are very magnetic. We're pretty confident. We've got drape aeromag at a scale of 1:12,000, and it's been very carefully looked at by a lot of geophysicists, and I think we probably are pretty effective at identifying any unknown dikes and those sorts of things, and so basically, I guess my pitch is, with the exception of structural controls--which I think we may want to focus on--that we're not in that bad a shape.

DR. ALLEN: If DOE comes in with a perfectly sewed up probabilistic treatment, do you think the NRC will be satisfied with that? Do you think the public will be satisfied with that?

MR. CROWE: Is that fair to ask me?

DR. ALLEN: Well, there are other people.

MR. CROWE: You know, the public perception issue is one that's always there. I mean, when we look at this polycyclic model, and again, I really try to detach myself from proving the model versus looking at its impact. I mean, we really shouldn't have that much vested interest in who's right on this. It should be a matter of what's right for the program.

I think there is a concern that if we end up with a polycyclic model, that there is a fairly high probability that there could be an eruption and we have to recognize that that's a very sensitive public issue. I mean, let's say we had an eruption in the next ten years. Even if we could prove that another one wasn't likely for a very long time, I think we'd be in trouble. So that's a very politically sensitive issue. That's one of the reasons I've focused so much effort on addressing this young volcanism in the polycyclic model, because of public sensitivity.

But as a scientist, I have to kind of try to separate the public from what I can give you analytically, and I can just go so far with your data, and I think I'd like to underscore that I think we have gone a long ways. I think there's a fair amount of data we can gather to resolve the chronology, but frankly, I've really played with the sensitivity of the probability calculations and I have to say out of honesty, I don't think we can pretend to tell you we can do much better than we've done, and it really--we ought to really focus this issue and say, if the kind of bounds we're placing on it are unacceptable, we need to know that now out of prudence to this program.

DR. ALLEN: Okay, thanks.

Chih-Hsiang?

DR. HO: I'd like to comment on my concern with another analogy. We all know that it takes about nine months to produce a healthy baby. If one tried to have nine women pregnant at the same time and tried to deliver the baby at the end of the first month, certainly, it don't work right. So--

DR. ALLEN: At least the probability is very low.

DR. HO: Yeah. So now, after this whole days' discussion, I think we have created a lot of interesting problems. Now it's time to solve. We know that lawyers has a bad reputation. They create more problems than the problems they solve, so I think geologists are better than lawyers. So now, how to achieve this kind of pioneer study?

I believe that it's time that we should concentrate on our study, concentrate in the sense of listing all the problems. I personally like panel member, NRC member, John

Trapp's spectrum of thoughtful questions and concerns, so based on that, we probably can have some plan and answer all the questions and when we answer these kind of questions, we certainly cannot expect everyone to understand everything, so teamwork is required.

When we talk about teamwork, sometimes people do have their own prejudices or something else, so I kind of stress that as a researcher, we got to have our mind, leave it open, open in the sense to accept some different argument to a certain degree. So once this door is open, then some solutions can be produced. So once those solutions can be produced, then we can sell this product to the public, to the audience.

When we sell that, we got to use a very technical way to kind of sell our product; for example, I'm trying to make another analogy, things like we do research in a scientific, efficient way, but to be objective, you should kind of trade your place to the other side and then put some thought for the people who propose different opinions. For example, if you want to buy a house, you want to buy a house. Are you considering buying a house near the airport or near the cemetery? Right. So that's a good analogy, saying that why this issue is so complicated and so important, and certainly deserve serious concerns.

Even though the probability is zero that somebody's going to see a ghost near the cemetery, even though the probability is close to zero that somebody living nearby the airport is going to be hit by the airplane, but they are concerned about why should I buy a place--a house that's near those places? It's kind of the concept. So somehow, people will have to understand this kind of concept, and then when answer questions, I do suggest that people try to stay kind of cool and try not to be too emotional, and that's exactly what I got from my advisers when I graduated and get a job at UNLV. And they say, "Hi, Ho, stay cool at Las Vegas." That exactly what I do. So try to be a little bit considerate and get things done.

So now, let's go back to Dr. Bruce Crowe's comment about those compatible results here. I do see some difference in concept here; for example, my recursion rate is about 5.5×10^{-6}, but I make a point that that recursion rate is my lower bound. That means my calculation, or the actual calculation shall be at least that much because the data we see, not all the story that telled (sic) or recorded by the Yucca Mountain. On the other hand, Bruce Crowe's calculation is the upper bound, so that's quite a different interpretation and concept.

So we do have some important difference here. That means a lot of things have to be addressed again, and seriously. So before any conclusion can be concluded, I hope that some of us could be done, just like I suggest, teamwork, and then concentration, and then try to push forward the whole project in a very smoother way for both side; people in Nevada, and people not in the State of Nevada, and then everyone is happy and we are doing things in a very professional way and we spend our time and we make our contribution, and I think then we deserve kind of compliments from people on either side.

DR. ALLEN: Thank you very much.

John, do you have anything you want to say; John Trapp?

MR. TRAPP: Talking about your points here, there's only two I actually want to talk about, and then there's just a few light points I'd like to go through.

What issue is consensus development? I think the only real issue that we've got the consensuses developing is that we need to go out and do some field work to get some data so that we can actually address these questions in a logical manner, which gets us to four, how can you resolve these things? Basically, you start the site characterization program.

In going through this, there is one other point that I would like to make, and it's kind of a philosophical point. There has been much discussion around the table about "use of expert opinion." First off, my--well, my graduate work was on statistics in geology, and there are some real things that come out of there. Number one, no geotechnical sampling program will ever get the complete range of the variable of interest. If you start taking a look at expert opinion, what you find is basically the probabilities from expert judgment are just about always expressing a range which is much less than the values warrant.

DR. ALLEN: Than the values what?

MR. TRAPP: Than the values warrant.

Also, there's another little homily, which is, basically, there is no data in the future. So what we're dealing with is a question where we've got to use Baysian statistics or basically the statistics of belief as to what we think is happening. We aren't really dealing with data. We're taking the scientific data and trying to figure out how our beliefs fit in some type of projection.

The standards--and I'll keep raising them--Copernicus was correct, but he would have failed on an expert panel. So would have Galileo. Thirty years ago, if I was sitting and trying to talk about plate tectonics, I would have been thrown out of the room. These have to be recognized, and in dealing with the expert opinion which we know we're going to get in the licensing process, it is imperative that this expert opinion be built on good, hard scientific data. Without that, licensing is going to be extremely difficult.

DR. ALLEN: Thank you. Presumably, if the person is an expert, by definition--hopefully--he will have that kind of data, he or she.

Brent, I apologize for not naming you, and so forth. Do you have any comment? You were one of the presenters today. Anything you want to say?

MR. TURRIN: I think all the comments have been exhaustive, and any more comments would just sort of be rambling.

DR. ALLEN: Okay. I would like to ask people in the--we have a little time left here--people in the audience. You're welcome to express any views you have, briefly. In fact, we would welcome them.

Yeah; Les McFadden.

MR. McFADDEN: I'd like to direct this to Carl Johnson, and I keep hearing that you seem to think that we need more data. I can take a truck with a back-mounted backhoe right now and go to the Cima volcanic field, trench and get lots of data. I cannot go to the fields in Black Cone, Red Cone, other centers in Nevada and do anything. I take it you will now support our efforts on the assumption that you regard us as good scientists to begin this work?

DR. ALLEN: Carl, you can respond as you see fit. I realize you are not the Governor of Nevada.

MR. JOHNSON: Yeah. I'm not the Governor of Nevada, so I'm not going to make any comments relative to the Governor's viewpoint or those of his regulatory agencies. We are not a regulatory agency within the state.

But let me bring around to the point that I think I tried to make and Gene Smith tried to make in his presentation, and I think--I believe that John Trapp alluded to in his remarks--is going out and spending a few days trenching at Lathrop Wells cone will not resolve the issue of the volcanic hazard at Yucca Mountain. There is a whole lot of other data out there that need to be collected.

John alluded to a number of things that need to be looked at and need to be considered when evaluating the volcanic hazard and the risk, and these are the types of data we're talking about. It's not spending a couple of days with a backhoe mounted on the back of a new truck, trenching at Lathrop Wells cone.

MR. McFADDEN: How else do you get stratigraphic data?

MR. JOHNSON: But there's a whole lot of other data that also needs to be collected and included in that analysis. It's not just stratigraphic data.

DR. ALLEN: Okay, thank you. I don't think he's arguing against your data. He's just saying it's not enough in itself.

Other comments from the audience?

(No audible response.)

DR. ALLEN: Any rejoinders from individual members here who would--some of the initial ones here spoke more briefly than those at the end, and you're quite welcome to say anything else.

(No audible response.)

DR. ALLEN: Okay. In that case, I'm inclined to draw things to a close here. It has me speaking for ten minutes at the end. I don't have anything to say for ten minutes, believe me. I think--

SPEAKER: You've got five left.

(Laughter.)

DR. ALLEN: I would just like to terminate on behalf of the Board, and I assume, incidentally, other Board people here have nothing further to say? Bill Melson?

I'd like to thank the people who presented today. I also particularly appreciate the candor that people spoke with under somewhat difficult circumstances during the day, and so forth, and I think we all appreciate that very much, and I certainly have found this very valuable and I think most of the rest of you have in terms of getting a better understanding of what we have done in the last year, what the challenges are, in terms of resolving this issue or working towards resolution of this issue in the future.

And on that basis, then, I'll call the meeting closed. Thank you.

(Whereupon, the meeting was concluded.)

CERTIFICATE

This is to certify that the attached proceedings before:

UNITED STATES OF AMERICA

NUCLEAR WASTE TECHNICAL REVIEW BOARD

In the Matter of:

PANEL MEETING

STRUCTURAL GEOLOGY & GEOENGINEERING

VOLCANIC HAZARDS AND VULNERABILITIES

Location: TUCSON, ARIZONA Date: MARCH 1, 1991

was held as herein appears, and that this is the original transcript thereof for the file of the Board.

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