UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING
MEETING ON VOLCANISM

Alexis Park Hotel
375 East Harmon
Las Vegas, Nevada

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<u>BOARD MEMBERS PRESENT</u>

Dr. Clarence R. Allen, Chairman
Structural Geology & Geoengineering Panel

Dr. Edward J. Cording, Member
Nuclear Waste Technical Review Board

Dr. John J. McKetta, Member
Nuclear Waste Technical Review Board

<u>ALSO PRESENT</u>

Dr. Leon Reiter, Senior Professional Staff
Nuclear Waste Technical Review Board,

Dr. Robert Luce, Senior Professional Staff
Nuclear Waste Technical Review Board,

Dr. William Melson, Consultant

I N D E X

P R O C E E D I N G S

(8:30 a.m.)

DR. ALLEN: Okay. May we come to order, please? This is the second day of the Panel on Structural Geology & Geo-engineering of the Nuclear Waste Technical Review Board, a meeting on Volcanism.

I see I'm scheduled for 10 minutes of introductory remarks. So, we'll start off 10 minutes early and I'll turn the meeting again over to Jeanne who, I think, has one final speaker on the DOE program.

DR. COOPER: First off this morning will be Dr. Greg Valentine and he'll be speaking about physical processes of magmatism.

DR. VALENTINE: I wanted to preface this talk by saying that these studies in the effects part of the volcanism task are really just getting started. They've been funded at a relatively low level over the past year, about a quarter of an FTE and we've focused about half of our time on doing initial scoping studies with modeling and field studies trying to kind of get an idea of what problems we need to look at and how we want to look at them. And, the remaining half has been focused on writing study plans and getting the administrative details in place. So, this is really a very initial progress report.

This is an outline of my talk. I'll start off by introducing what this study is all about. I'll go through the study plan which is currently in the final stages of revision, almost ready to go to DOE within the next month, hopefully. I'll talk about how we interface with performance assessment, and finally, I'll go through some technical progress and approaches. Again, these will be very initial type of studies, but just examples to show how we're doing things.

This study is basically comprised of two parts. The first part is to assess the possible effects of magmatic activity in or near the repository and this includes not just eruptive effects which are sometimes also referred to as direct effects, but also includes subsurface effects, such as hydrothermal activity and so forth which I'll be talking about more. Subsurface effects have also been referred to as secondary or indirect effects. We're in the middle of a terminology change. So, it's kind of confusing. This part of the study feeds most directly into performance assessment and licensing.

The second part is to assess the physical processes of magmatism just to give us a good framework for how magmat-ic processes work in the Yucca Mountain region using a combination of all the data that we're collecting from the various studies and some theoretical constraints. This is in response partly to what Don DePaolo discussed yesterday, as far as having a magmatic framework model.

The study plan is broken into three activities. The first one is eruptive effects; the second, subsurface effects; and, the third is magma system dynamics.

The goal of the first activity is simply to estimate the probability that regulatory release limits could be exceeded by eruption of waste onto the surface. And, this is probably what most of us think of when we think of volcanic hazards for Yucca Mountain. In all of the parts of this study plan, we combine modeling and analog or field studies. It's crucial to combine theoretical and observational work clearly. But, in this activity, we're relying mainly on analog studies and the reason is because we're looking at processes of possible entrainment of waste if there were an eruption through the repository. We're looking at the possible processes of entrainment of waste and how it would be carried to the surface. And, we feel that theoretically this is a very difficult thing to get a handle on, and besides, the most straight forward thing to do is to look at field analogs. So, that's what we're going to focus on. And, we're looking at several centers in the western U.S. that are similar, that have volcanoes similar to what we see at Crater Flat.

The main aspect that we're focusing on is measuring quantities of erupted lithic material that are ejected in these volcanoes and identifying their depths of origin. And, we'll use this to estimate the fraction of material that could conceivably be carried up from the repository horizon. In conjunction with this, we're also studying the geometry of deeply eroded centers for further constraints. We're combining looking at lithics in the actual tephra or lava deposits with looking at the geometry of deeply eroded centers because when we look at deeply eroded centers, we're seeing the final products of a lot of processes that are overprinted on each other, many of them probably during the closing phases of the eruption. When we look at lithics that are erupted during the eruption, we're looking at the process as it happened during the eruption. Some of the modeling that we'll do will be to estimate vesiculation and fragmentation depths if we need to because these might further help us understand this process.

The second activity, subsurface effects, has the same goal, estimate the probability that release limits could be exceeded, but as a result of subsurface processes of magmatism. And, of course, we understand that any eruptive process has an accompanying subsurface process, but you can have subsurface processes without eruptive processes.

Here, we're combining modeling and analog studies again, but there's more of an even balance. For analogs, we're focusing on basaltic intrusions that actually occur in silicic tuffs and I'll be showing some examples of this later on in the talk because this is what the case would be if there was an intrusion in Yucca Mountain. We're quantifying the ranges of geometry, sizes, et cetera, and studying the effects of these intrusions on the wall-rocks. Modeling will be mainly geared toward looking at hydrothermal processes in the vadose zone. For example, one of the first things we need to do is quantify how far an intrusion of a given size can be and not effect the repository. Maybe I phrased that wrong, but in other words, what is the zone of influence that an intrusive body would have on the repository because this plays a role then in probability calculations.

The third activity, magma system dynamics, has just a very general goal of providing a theoretical framework for magmatism in the Yucca Mountain region incorporating all the observational data that we're gathering. To some degree or another, we're trying to look at all aspects of magma dynamics starting with melt generation and segregation in the mantle through magma chamber dynamics, magma transport through the lithosphere, and eruptive processes. As Don DePaolo said yesterday, this is a very active field of research and there are many theoretical constraints that we can use to help us understand what we observe and give us a more physical basis for probability calculations, for example. The common theme between all these three activities is that they focus on physical processes.

Now, this study is our main avenue for interaction with performance assessment and I've spent quite a bit of time in the last couple of years working mainly with performance assessment people from Sandia on this. For example, George Barr and Ellen Dunn at Sandia have worked up an event tree which describe various scenarios that can accompany magmatism. I've worked quite closely with them on that.

I've also provided parameter values and expert opinion for specific calculations that they've done. For example, I guess it was about a year ago Sandia and a couple of other laboratories were working on a total systems analysis which I think has been presented to the Board earlier in this year. The information that they used for volcanism came mostly from me.

I also work somewhat with people who work on other repository components such as the waste package and I'm involved at a relatively low level, but I'm involved in some of the ideas for different thermal loading designs for the repository and so forth that are going on now. All of this feeds back into the whole system CCDF.

And, basically our interaction is described by this last bullet here. We provide the physical effects at the repository horizon. PA takes this information and looks at how it affects the waste packages and the resulting radio logical release.

Okay. Let's spend the rest of the time going through examples of the technical work that we've done to date. The main purpose here is to give you some idea of the methods and approaches that we'll be using over the next few years in this study.

First of all, everything that we do eventually goes into the volcanic release probability with which you are probably infinitely familiar. This involves the proba- bilities E1, E2, and E3. E1 is the annual igneous event probability in the region; E2, the probability that such an event affects the waste isolation system; and E3 is proba- bility that such an event in the repository block or near the repository block has consequences that exceed the regulatory limits.

I'll start off by giving some examples of eruptive effects work that we've done, and then subsurface effects, and then finally some examples of magma system dynamics work. I wanted to start off by showing some just general schematic cartoons that illustrate some of the processes that we're trying to account for and also that define what we call an igneous event.

This shows the propagation of a magma field fracture which we call a dike through the upper part of the lithosphere. I have a laser beam today. This is a dike here propagating upward. It's driven by its own buoyancy. It's similar in many ways to a hydro frac. When dikes, such as this, propagate through intact rocks, we can have joint sets that form parallel to the dike plane. And, this is observed, as I said, mainly in intact rocks such as on the Colorado Plateau and there have been some classic studies with dikes propagating up through large sandstone units and so forth that produce these joint sets. This is all related to the stress field around the tip of the dike as it's propagating up. But, clearly, we want to understand that such joint sets would form in a Yucca Mountain setting because that might affect the long-term hydrology and so forth. Yucca Mountain is already quite fractured. So, I'm not sure if there would be a strong effect, or not, but we really don't know at this point.

The flow of magma in the dike is balanced by momentum balance and energy balance and I've just shown the energy balance up here where the heat advection plus the latent heat from crystallization plus heat that's produced by viscous dissipation, which I've shown schematically down here, has to be greater than or has to balance, at least, the heat that is lost to the wall-rock.

Now, in the idealized sort of situation, these small basaltic eruptions are thought to begin with fissure eruptions where the dike or set of en echelon dikes initially intersects the surface of the earth. And, as I say, this is idealized and is not necessarily what happens at every center in the Crater Flat or Yucca Mountain region. But, ideally, initially we can have a linear fissure eruption from where these en echelon or single dikes penetrate the surface with magma flowing evenly up along the entire length of the dikes. As time goes on, for various reasons, flow begins to be focused at some depth, so that we have focusing of eruption into a few locations along the fissure. Now, what this means is that there is a bulge in the dike below the vents and, as soon as you get some bulge or roundness in the dike structure, then it can exert tension on the surrounding rocks and cause propagation of radial dikes. And, these complexities are important for effects studies.

Eventually, ideally, almost all the flow can be focused into one central vent which can form a large scoria cone or a lava flow, whatever. But, one of the things I'd like to illustrate here is that many different types of vents and many different types of pyroclastic facies can form during the course of a single magmatic event. We would call this a single magmatic event from an effects and a probability point of view, even though it can possibly produce many vents.

Just again to illustrate that we are looking at various degrees of complexity, this shows examples of some scenarios that we might look at. The simplest is probably a monogenetic cone or with a lava flow possibly that forms above the repository, penetrates the repository with a single dike or single en echelon set of dikes. And, the complexity that commonly occurs in the upper conduit is all at a level above the repository in which case these complexities probably don't matter so much. A more complex case for a monogenetic cone would be where some of these complexities, such as radial dikes and possible lateral intrusions, occur at or below the repository level.

When we have the possibility of polycyclic eruptions, we have to look at least at the possibility of having two dikes separated in time by whatever the repose period is for these things and we also can have varying complexities in the feeder system.

We're also looking at intrusion complexes and here, I've drawn some dikes and sills that form. In this case, they form at or above the repository horizon, but we also believe that complexes that form below the repository might actually be important because of the hydrothermal flow system that can be set up.

Let's go through a very simple example of how we have used eruptive effects calculations to help us guide our work. See, what we're looking at here is the amount of waste that could possibly be erupted during a scoria cone forming eruption. The information is all going to be based on analog volcanoes. The first assumption that we're going to make is that waste will be entrained and erupted in a manner that is very similar to wall-rock fragments, so that we can use erupted lithics as an analog for waste. There's a strength here because waste is much denser and especially if it's in its intact form, it would be very difficult to entrain into a narrow dike. A weakness is that--and, we're aware of this and we're trying to find ways to solve this problem is that we realize that the repository horizon will be different from the surrounding rocks. It will have backfill and so forth. And, we're trying to find analogs that might give us some handle on the sensitivity of erupted processes to these complexities. Okay. We're going to say that the volume of erupted wall-rock is proportional to the total erupted volume.

Just based on early data, this was actually still out of the paper that Crowe, et al., wrote in 1983. If we use a conservative erupted volume of $10^8 m^3$ in a conservative lithic content of .06%--that's the volume fraction of lithics in the eruptive products--the average depth to the repository is 300m and the repository horizon, at least in the design that I was using here, was about 5m thick which is 1.7% of the total depth. And, we're going to assume that this .06% volume fraction of lithics was derived evenly from erosion of the conduit or dike walls all the way from the surface down to the base of the repository horizon. And, if we do this, the release that we come up with is about equivalent to one or two waste packages, which means that our E3 parameter is about 1 for this case.

The most critical assumption that we've made, we think, is that of constant wall-rock erosion with depth to repository. You know, we have a lot of evidence, I think, that most or many lithics or much wall-rock erosion in these plumbing systems actually occurs in the very uppermost part of the system. So, what we're going to do is try to quantify actually how much of that happens at depths above the repository. If most of it happens above the repository, then it doesn't really--then it will bring this release way down. So, this is where we have come up with a series of field studies to look at the variation in wall-rock erosion with depth.

Just to show you a couple of examples, this is at the Paiute Ridge area, which I'll be talking about later, where we have dikes and sills that intrude into Paintbrush tuffs. So, it's a very good analog for the Yucca Mountain site. This is actually on the eastern border of the Nevada Test Site.

Here, we have a long dike that terminates in a plug system. The plugs have radial dikes. So, this probably is an eroded remnant of a feeder system for a small basaltic volcano. So, this is an example of an ancient eroded center that we would look at; in this case, 8 million years old.

Here's another case which is really quite scenic. On the rim of the Grand Canyon, there is a volcanic field; a north/south-trending volcanic field that intersects the western Grand Canyon and it's commonly known as Vulcan's Throne. There were many cones that erupted up on the flanks of the canyon and lava flows came down and actually dammed the canyon up and so forth. And, where those lava flows spilled over, we now have good rapids. But, one of the reasons we're interested in this site is because we have these volcanoes like this one perched on the edge of the canyon. Part of the conduit systems looks like it might be accessible here. And, this volcano actually is a tuff ring or a hydro-volcanic center that was later filled by a scoria cone. So, we actually have the varying eruptive styles preserved up on top, but we also have the conduit and plumb-ing system exposed down here. And, even where it's not accessible, the basement stratigraphy is relatively straight forward here. So that if we find a lithic up here, we're hoping that we can match it to its depth of origin. So, again, this will give us some idea of how conduit or dike erosion varies as a function of depth.

Let's look a little bit at subsurface effects. Subsurface effects really can be divided into two aspects. First, a short-term effect which includes everything that goes on during the thermal pulse of an intrusion. Long-term effects happen after the intrusion has cooled down and it's basically a hydrologic problem.

Things that I'll illustrate here are factors that are affecting intrusion, geometry, and location. The mechanical effects of intrusions on wall-rocks, such as jointing which I mentioned earlier. Hydrothermal flow in the unsaturated zone which, I believe, is not a very well-studied process. Of course, there's been much work on hydrothermal circulation in the saturated zone, but not in the UZ. The flow of vapor or air in response to intrusions and I believe at the last TRB meeting a year and a half ago, I showed you an example of a calculation showing how air would flow in response to emplacement of a dike. And, we're also interested in the volatile releases from magma.

This is a simplified geologic map of this Paiute Ridge area where basalts, shown in the white pattern here which is a really bad color for basalt--it should be black--intruded into Paintbrush Tuffs and some later welded tuffs from the Timber Mountain caldera. These linear forms are actually dikes and again this is about 8 million years old. The linear forms are dikes and their thicknesses is much exaggerated on this map. Then, these more extensive bodies are actually sills that we think formed at about 300m depth or so. Is that right, Frank? Yeah, 300m.

So, the next slide will be this cross section. The solid line here shows the present day topography with a vertical exaggeration here of about 4.1 to 1. So, out to the west here is Yucca Flat which looks like the moon, and up here, is the Paiute Ridge area. Up here in sort of a ghost pattern is my interpretation of what the terrain and the geology was like 8 million years ago when these basalts were in place. For example, there's evidence that there was a scoria cone here and a lava flow field here. Note that dikes came up along normal faults which has been describe by Gene Smith and co-workers. This system basically was sort of a miniature horst and graben at the time. So, this is a good place to look for the factors that control dikes versus sills and the effects of basaltic intrusions on wall-rocks, even though we don't know if this is really a good analog tecton-ically for Yucca Mountain. This shows an example of a dike marching across. Here, it's coplanar with a normal fault and the dike is actually only about one-third of the thickness that you see there. The rest is talus.

Now, that's a little out of focus, but what we're looking at here is the margin of the dike, basalt on one side and tuff on the other. The tuffs were originally unwelded, but when the dike was in place, it heated the tuff and exerted pressure onto the tuff and caused welding adjacent. And, we've sampled extensively to look at variations in welding away from the margins of these dikes because welding clearly is closely related to hydrologic properties and so forth.

For long-term effects, we want to know the effect of intrusion properties, size, and location on the long-term hydrologic flow field as it re-establishes itself. Paiute Ridge is also a good example here. Here, we have a spring, although I wouldn't drink out of it. It's pretty gross looking. And, you see that there's a dike. You can see this dike I showed in an earlier photo. It kind of marches over the back of this hill. In cross section, the same cross section I showed before, this spring occurs right here and it's just up-gradient from this dike and it's possible that water is being perched as a result of that dike. The formation of local saturated zones clearly could have a very strong impact on radionuclide transport over the long-term. The water table, incidentally, is at least 1,000 feet below the surface here. So, this is perched way up high.

DR. ALLEN: Is this a hot spring?

DR. VALENTINE: It's not a hot spring, no.

Okay. To talk a little bit about magma system dynamics, again the idea is to maximize our understanding of volcanic systems in the region. We have all this data: petrology, geochemistry, geochronology, volumes, geophysics eventually, eruptive styles, and just general field relations. And, we need to combine this with theories of melt segregation, magma chamber dynamics, ascent in dikes, and eruption dynamics.

So, I'm going to talk about examples of magma chamber work that we've done, so far--again, this is just sort of scoping to kind of see what we can do--and also eruptive processes. We want to understand the thermal and chemical processes in small, question mark, deep basaltic chambers. We have evidence that some of these magmas have undergone protracted fractionation, but they erupted with a low crystal content. As Frank presented yesterday, we have separate small magma batches. One thing we want to look at in terms of magma chambers is are there--what sort of eruption triggers are there at the magma chamber level. Frank mentioned the importance of density yesterday which probably is a possible eruption trigger, as the density of the magma varies as they fractionate. And, also, we want to try to understand open versus closed system behavior of each batch.

This is an example of a fluid dynamic calculation of a very small magma chamber of a very small sill that's actually about 10m high by 60m wide and this actually is similar in size to field examples that we have of Paiute Ridge. And, the reason for choosing this site was two-fold; one is because computationally we want to start with some-thing simple and also, secondly, because of the examples at Paiute Ridge. We might actually have direct field examples with which to test the numerical calculations. Interestingly enough, as far as I can tell, this kind of calculation has never been done for magma chambers, although a lot of people study the dynamics of magma chambers.

We emplace magma at 1100 degrees, basaltic magma, into the sill and it's surrounded by cold, rigid wall-rock which conducts heat. The Rayleigh number for those of you who are into these things is fairly high, 2×10^7, which is in the full turbulent convection regime. Now, the interest-ing thing about these systems is that because it's surrounded on all sides by cold rock, you can kind of think of the sill as being two parts. The bottom part is actually stable. It doesn't want to convect because it's cold on the bottom, colder and denser on the bottom, and hotter, less dense in the middle. The top half does want to convect because it's cold on top and warm on the bottom. So, what happens is along the bottom here we just have a stable layer that sort of analogous in some ways to just stably stratified atmos-phere. We have plumes that come down off of the top surface. these are temperature contours, by the way. And, they come down and when they--this is a good example here. When it encounters this stable layer at the bottom, it just slowly spreads out in the same manner as smoke from a factory stack would just reach a stable layer and spread out. So, what happens is the stable layer just slowly grows at the bottom until it consumes almost the entire sill. It's very dif-ferent. It's a relatively short time during which this hap-pens compared to the overall cooling time.

This snapshot down here shows the temperature structure of the sill at the moment when the magma has cooled to the point where it does not flow anymore. You can see the original outline of the sill here. The temperature contours go out into the surrounding rocks because of conduction. One of the interesting things here is that we get this portion here in about the upper one-third level where it is hottest for the longest period of time, but it also has a lateral structure to it. And, although we haven't carried out detailed petrographic studies yet, there are indications at Paiute Ridge that some of the sills might preserve this lateral structure and the way it's preserved is by crystal size variations at a given level.

Okay. Another example I want to show is eruption dynamics. We want to use eruption dynamics to constrain volatile contents and ascent dynamics, magma discharge rates, eruption durations, and we're basically looking--for what I'm going to show here, we're looking at two types of eruptions, Hawaiian which is steady fountaining or Strombolian which is bursts or short-lived fountains.

One of the things that we have access to in the field are the pyroclastic facies. This is a photograph of the A-cone or the Black Tank Cone as it's now called in the Cima Field.

The next photo is the inner wall of this crater. This cone has welded spatter, at least along portions of the inner wall of the cone. Contrast this to the Lathrop Wells crater which has, as far as I've been able to see, zero welding, at all. It's just all loose scoria in the cone. Now, this variation in facies distributions or facies occurrences has to be related to eruption dynamics, somewhat. I think that by looking at the types of variations that we see throughout volcanoes in the Crater Flat Volcanic Zone that we can get an idea of the types of different discharge rates and volatile contents that are realistic for these eruptions. And then, this again, would eventually relate back to the processes that are going on at depth.

It's been known for a long time that welding in scoria deposits depends on the accumulation rate of the clasts on the ground and the initial clast temperature. In other words, no matter what your accumulation rate is, if the clasts are cool enough that they're solid, then they will not weld or stick together. If you have very high accumulation rates at high temperatures, the clasts will totally coalesce and form lavas, either lava flows or ponds.

There's an intermediate zone here; for example, here where you have varying degrees of partial welding. And, what I've done is I've modeled the cooling history of basal-tic clasts when they hit the ground and various things and I've come up with this diagram which just shows under a certain set of circumstances the variations in facies that you get and how they can be matched to the accumulation rate and the initial clast temperature. While this looks purely esoteric now--and, maybe it is--but we want to do is simulate the eruption process as a whole and find out how the accumu-lation rate and the clast temperature vary with varying erup-tion conditions. And, this shows an example of a hydro-dynamic simulation where I've just chosen a couple of things to plot here. This is the accumulation rate of material along the ground as a function of distance from vent. Notice that the peak in accumulation is about 40m away from the vent. And, this shows the temperature of the clasts when they hit the ground as a function of distance from vent.

Now, what we do with these kinds of simulations ultimately is we want to make artificial facies maps that we can match to what we see in the field and at least get an idea of the relative variations that we're seeing. In this case, we have a very abrupt transition from fully welded scoria within about 50m of the center of the vent and then just totally loose unwelded scoria outward. In particular, the position of this kind of transition is very sensitive to the discharge rate and the temperature of the clasts as they hit the ground is very sensitive to the volatile content of the magmas. So, that's just to give you an idea of where we're heading there.

So, to conclude, I believe that at this point we have identified most of the problems that we want to address and we've come up with strategies for addressing them. We've carried out some initial scoping studies, some of which you've seen here. The study plan is basically written and is in the final internal review at Los Alamos and, hopefully, ideally will be to DOE around the fiscal year, the beginning of the fiscal year. My highest priority is to complete the acceptance process for this as soon as possible, even though I know it's going to be a long and grueling process.

The technical goals for the next two years are listed here in order of priority which doesn't necessarily correspond with the long-term importance of these problems, but it's just the order that we laid out for addressing them. First, we want to complete the lithic distribution study which I described and, hopefully, barring any surprises, will lead to a final analysis of eruptive effects.

Second, we're going to perform sensitivity analyses to come up with the subsurface zone of influence for hydro-thermal processes, for example. We want to complete the eruption dynamic study which I just showed you. It was the last thing I showed you. That's very close to being com-pleted. Continue our analog studies of subsurface effects and begin looking at melt segregation and magma chamber modeling in more detail.

And, with that, I will quit.

DR. ALLEN: Okay. Thank you, Greg.

Are there questions from the Board? Bill Melson, first?

DR. MELSON: Greg, is there any way on getting at how much near the surface water is released in these eruptions by stable isotope studies that would go along with these studies you're planning or how do you plan to address the things which you can't see?

DR. VALENTINE: Um-hum. I guess I am not an isotope geochemist. So, I don't--Frank, do you have any ideas on that for using--

DR. PERRY: Nothing comes to mind.

DR. VALENTINE: Frank was also a co-PI on the study.

DR. PERRY: You asked how much water would be released?

DR. MELSON: Well, not in water, but the interaction with the canister.

REPORTER: You're going to have to be closer to the microphone.

DR. MELSON: How do you address the volatile components, in general, that would possibly be affected by a shallow intrusion? I mean, I see what's happening here, an excellent approach to the lithic components and wall-rock, but I'm not clear from the approach. As it's planned, it's supposed to go into or could go into assessing volatile release, in general.

DR. PERRY: The only thing that comes to mind right now from a place like Paiute Ridge where you have access to the wall-rock and the intrusions, you can get an idea from both radiogenic isotopes and stable isotope studies how much interaction there is between wall-rock and magma and how much effect there is into the wall-rock from the magma. And, you know, certain relation to groundwater and that type of thing. So, I think that's the only thing that comes to mind using that type of approach.

DR. VALENTINE: Um-hum. From a modeling approach, I think the first thing we would do is take some conservative estimate of the volatile content of these magmas. And, say, if we wanted to look at the effects of volatiles released from a sill of a given size, we would just release all of that volatile and track it in the hydrothermal flow as a passive tracer just to see. And, if the modeling indicates that it is diffused or dispersed out to the point where it's very dilute by the time it reaches the repository, then we wouldn't worry about it. So, I guess what I'm saying is there would be both modeling and possibly some geochemical tests for that.

DR. ALLEN: Bob Luce?

DR. LUCE: I was impressed by the number and length of these topics that you put up there that are going to be studied. What kind of resources are going to be put to this? And, just offhand, I find it difficult to feel that you're going to get all this done in time for the schedule that DOE has. It's a very impressive list you've placed there.

DR. VALENTINE: We've carefully thought out how we're going to lay out our milestones and progress. As far as resources, we don't have any problem with resources. We have the computational and the analytical research--

DR. LUCE: It's almost like there's several lifetimes of work and so forth.

DR. VALENTINE: Well, I think there could be, but we're going to be very careful to keep it addressed to licensing problems and not--there are going to be a lot of basic questions and problems that will come up along the way, of course. But, we're going to try to keep it focused on licensing problems.

DR. ALLEN: Ed Cording?

DR. CORDING: I had one question regarding the Vulcan's Throne area and the Grand Canyon area. Is your understanding that the initial volcanism there occurred--did it occur at the bottom of the canyon or was there already--initially, when the first flows occurred, was it coming into the bottom of the canyon or was it coming up on the plateau?

DR. VALENTINE: It was up on the plateau. I think this Vulcan's Throne area is just a small part of the north/south chain of volcanoes and it just happens that the Grand Canyon was just there, right near the end of that chain. So, you have a few volcanoes on the south and then the main part of the chain is to the north of the canyon. I haven't been down to the bottom of the canyon there, but if I remember field guides correctly, there might be one conduit type structure down close to the river, but by and large, most of them came down--erupted up on the plateau and came down. And, if you ever fly eastward over the Grand Canyon, it's really striking. You can really see it well.

DR. CORDING: If it were a sill across the bottom of the canyon, you would probably assume it was the sort of feature that came up there, rather than up on the side walls. It's almost like the way you showed the model of Paiute Basin where the volcano, at least in your diagrams, was coming up into a lower area where stresses would first be relieved. Is that a trend that you think is important or--

DR. VALENTINE: Um-hum. That's a good question and we don't know how important that trend is. At Paiute Ridge, we do observe that the erupted products seem to have been in the original graben part of the structure, although there were shallow intrusives out within the horst that didn't actually erupt. So, if we were back 8 million years ago looking at it, all we would see is a volcano in the middle of the valley.

DR. ALLEN: Greg, why do you state that the acceptance of the study plan will be a long and grueling process? I can understand that the work itself might be long and grueling, but why should the acceptance study plan be long and grueling?

DR. VALENTINE: That's just what I hear from old experienced people that have put their study plans through the process. That's a year or two type--maybe I'm wrong, I don't know. How long does it--Bruce, do you want to--

DR. CROWE: I think the safe thing to say is that the worst place to be is the first one to get a study plan through. The initial study plan and characterization of all planning features was a grueling process. We probably had over 200 comments to respond to and it was a fairly--it took about six months to actually go through the review process, but it's been streamlined dramatically since then and I think it will be much easier this time through. At least, I hope it will.

DR. VALENTINE: Maybe it won't be long. Maybe it won't even be grueling.

DR. ALLEN: Leon Reiter?

DR. REITER: Greg, you know that the Board has looked at the issue of earthquakes and placed a lot of emphasis on using seismic vulnerabilities to help determine which are the most important loads of seismic failure and what are important things to do. For example, it's no secret that at its last meeting, the Board heard a lot saying the ground motion effects really are relatively minor, and faulting, particularly large faulting, may be a more serious issue. I'm not quite sure as to how well your kind of studies are geared into that to help--or if it's possible to come out of it with guidance or understanding. Is the actual behavior or the interaction of the volcanism with the waste package simply at your doing or is that something that somebody else is doing?

DR. VALENTINE: That's a different study plan that we would interact with very closely.

DR. REITER: It seems to me that that feedback--when we talk about vulnerabilities, that's a very important feedback. And, it may find that volatiles may be really extremely important and you may decide that you really want to do a lot of investigation.

DR. VALENTINE: Um-hum.

DR. REITER: While other things may not be that important.

DR. VALENTINE: Um-hum.

DR. REITER: Maybe, it's a question for Bruce, I don't know.

DR. VALENTINE: Well, no. I guess what I would say at this point, especially for subsurface effects--mainly for subsurface effects is that these processes really haven't been looked at almost, at all, in the past and so there's going to be some ranking of the importance as we go along. At this point, I wouldn't want to say what subsets are more important than others. But, part of the reason why I try to keep my ties with the performance assessment community very strong is specifically so that down the road we'll be able to exchange information very--you know, to maximize the exchange of information.

DR. REITER: Let me just point out one of those questions about the magma dynamics. I'm not a volcanologist. It seems that's an extremely exiting, interesting area and, as Don said yesterday, a lot of people do work in there. And, I guess from one point--the best of all possible worlds, a little bit of work in that area could give you a lot of insight vis-a-vis the hazard. And, the worst of all possible worlds, a lot of work in that area could either lead you to conclusions which are irrelevant or lack of resolution. And, is that sort of sensitivity as to--how do you feel that that's going to work out in this case?

DR. VALENTINE: I think that it's going to be more the former case where a relatively small amount of work will give us a lot of good information.

DR. REITER: Vis-a-vis, the hazard?

DR. VALENTINE: Yes.

DR. REITER: And, particularly, the key thing is the eruption sequence?

DR. VALENTINE: For example, one aspect would be giving us a range of alternative conceptual models or, you know, giving us more of a handle of the range of possibilities for understanding the mechanisms of polycyclic volcanism in terms of the current tectonic setting, I think, will--

DR. REITER: Yeah, I know Bruce is going to say that. That if it turns out that the effects of different models of polycyclic volcanism have relatively little impact on the hazard and that might make that study less important.

DR. VALENTINE: Um-hum.

DR. REITER: In keeping your eye--I guess the bottom line of the whole position, in keeping your eye on how this information is going to be used or what impact it has.

DR. VALENTINE: Yes. Another way to phrase your question, are we going to go off on our own tangent or just basic research tangent. We're going to be careful not to do that and, as we go down the road as I said before, we'll be geared toward what we see as being most important for the licensing program at the time. And, because we haven't really started many of these studies, I can't really say what those things will be.

DR. ALLEN: Greg, as I understand it, there's some seismological evidence from earthquakes from the Marianas (phonetic), as well as some petrologic fuel, I must say. Sills have sometimes been in place with essentially explosive velocity. I mean, sills over long distances of seconds or tens of seconds without necessarily eruptive vents. Does that have any bearing on this, at all, on the Yucca Mountain situation, the hazards there, or is it irrelevant to this particular area?

DR. VALENTINE: I guess it could be relevant if a sill actually occurred at the repository horizon. The details of the sill emplacement could be very important for how it might disturb the repository. But, other than that, I don't think --I think, in terms of all the other processes that are going on, the emplacement of a sill--I mean, we'll just treat it as an instantaneous kind of thing. I don't think the details of it are--we know that dikes propagate at centimeters to meters per second and I would expect a sill to propagate at the same kind of rate.

So, does that answer--

DR. ALLEN: Well, so did we except we've been surprised by some of the seismological evidence. It indicates those things really take off explosively--

DR. VALENTINE: I don't know. I'm not familiar with that.

DR. ALLEN: At least in some parts, maybe not here.

DR. VALENTINE: Um-hum.

DR. ALLEN: Any questions from the audience? Yes?

MR. CADY JOHNSON: Cady Johnson with a reply. If entrainment of the waste containers is sufficiently credible to receive analysis, I would ask if criticality issues resulting from mechanical sorting of the wastes are also considered credible?

DR. VALENTINE: I don't have an answer for that off the top of my head. I guess I hadn't thought about that. That's my answer, no answer.

DR. ALLEN: Yes, Bruce Crowe?

DR. CROWE: Let me just make a quick comment to respond to some points that Leon made. Some studies that I did outside of Yucca Mountain--I've actually done something outside of Yucca Mountain--I think give us some insight that probably the most important process--do you remember the diagrams that Greg showed of the different models, particularly the subsurface intrusion models? What we were probably the most concerned about is the disturbance of the hydrologic system from the thermal input from an intrusion and then the degassing of the volatiles from the magma system. What we learned from Hawaiian work is that these degas large quantities of halogens and form hydrochloric, sulfuric, and hydrofluoric acids that could be very, very corrosive. And, probably that is going to be one of the major things we'll focus on. In fact, one of the things that Greg is doing is sampling the wall-rock to look at the zone of influence of how far we can see signs of alteration from degassing and hydrothermal effects. We think that that scenario may end up being the most important scenario of the ones we're looking at.

DR. ALLEN: Yes, Carl Johnson?

MR. CARL JOHNSON: Carl Johnson with the State of Nevada. I've got a whole series of questions, but I think I'll limit it to one at this point in the interest of time.

I'm trying to better understand what the objective and the goal of the study plan is. In at least two of the activities, the goal is to estimate probability that regulatory release limits could be exceeded. Yet, further on, you made the statement that in the interactions with performance assessment the goal is to provide physical effects and the performance assessment people will estimate radiological releases.

DR. VALENTINE: Um-hum.

MR. CARL JOHNSON: Who is going to estimate the radio-logical releases, you in this study plan or the performance assessment people?

DR. VALENTINE: Performance assessment.

MR. CARL JOHNSON: So, how does that meet with the goals of your first activity?

DR. VALENTINE: Well, even what performance assessment does will be the volcanic release probability. So, we're feeding information to that. For example, if we want to understand the consequences due to hydrothermal activity resulting from an intrusion, the kind of information that we would give the performance assessment might be the flow field and the temperature field at the repository horizon which they would then take to look at the effects on the waste packages and so forth and turn into specific radiological releases.

DR. ALLEN: Okay. I think maybe we ought to get on with the program here. Thank you, Greg.

That completes, at least for the moment, as I understand it, the DOE presentations. We now have a couple of presentations, possibly the State of Nevada. Carl, perhaps, you would like to introduce these people?

MR. CARL JOHNSON: The first presentation is going to be Dr. Eugene Smith. Gene is the director of the Center for Volcanism and Tectonic Studies at the University of Nevada-Las Vegas.

DR. SMITH: What I'd like to do today is present you with a progress report of all activities since the last meeting with the Board in March of 1991. I have a handout. It may be somewhat difficult to follow. I was playing with my new Power Point software package and I have now learned how to put six transparencies on a single page. So, you may need a magnifying glass to see the transparencies, but I'm sure I saved the forest by doing it this way. But, this is not intended to be a position statement. It's intended to be a progress report.

The last five or six years, as Carl mentioned, we've been taking the tack of trying to evaluate volcanism in terms of regional perspective, in terms of looking at analog studies, and then trying to apply those analog studies directly to volcanoes in the Yucca Mountain area. And, in fact, Professor Ho at the University of Nevada-Las Vegas has used the models that we proposed for Crater Flat and Lathrop Wells to calculate disruption parameters. The purpose of our studies for the last year have been to provide geological data that can be used in risk assessment and this, of course, is the bottom line. This is why we're doing this.

Now, just to refresh your memory, we've proposed a slightly different view of how one would look at the Crater Flat/Yucca Mountain area in terms of areas of most recent volcanic activity. We've proposed a larger zone of impact which we call the area of most recent volcanism. This differs from the Crater Flat zone that Bruce Crowe has proposed in that it includes the Buckboard Mesa Cone which is a 2.8 million year old volcanic center located in the mode of the Timber Mountain caldera. We've also proposed a series of hazard zones and recently, as I just mentioned, Professor Ho has calculated disruption probability values based on the hazard zones that we've proposed.

Now, it's very important in order to justify this sort of a model. It's very important to take a look at some of the assumptions. For example, the larger hazard zones are derived from some of the analog studies that we've done. Therefore, it's very important to determine the analog areas that we're studying are the appropriate analogs to the Crater Flat/Lathrop Wells areas. It's very important. We have to have some way of evaluating whether analogs are appropriate.

Important, but not critical to this model, is the northeast trends of volcano alignments. Do these represent regional trends or are they related as secondary structures to northwest striking faults related to Walker-Lane. That is are the northeast trends the important tectonic element? Do these represent the important tectonic element in the region or is the northwest trend the important tectonic element? So, these are the two things that I'd like to try to focus on today and this has been really the focus of our studies for the past year or so.

Now, in terms of analog studies, we've looked at two areas; the Reveille Range in central Nevada and the Fortification Hill area in northwest Arizona. And, the question is are these appropriate analogs to volcanic systems in the Yucca Mountain area? Also, structural control of volcanism, northeast versus northwest trends, which one of these is the regionally significant trend?

Now, in our mind, in order for an area to be a good analog, it must meet at least three criteria. First, we feel that--I'm sorry, I should step one step back here. Analog studies provide us with some important information. I just want to review very briefly what they provide us and this has been touched on by several people already during this meeting. Number one, it gives us information regarding structural control of volcanism. Number two, we can get some information regarding vent geometry and this is because many of these analogs are really deeply eroded. And, also, we can estimate the volume of magma erupted to the volume of the magma in the subsurface. This is also very important in terms of risk assessment.

Now, I'd like to just mention some of the criteria that we have to honor in terms of deciding whether analogs are appropriate. First is that analogs should have a very similar structural setting or tectonic setting to the area that we're comparing to. Also, the volumes of individual volcanoes within a field should be similar to the Crater Flat/Yucca Mountain area. And, last, the chemistry, both the isotope chemistry and also the trace and major element chemistry should have similarities.

Let's take a look at these fields one-by-one. Fortification Hill field in northwestern Arizona, this is a Pliocene alkali basalt field with a total volume of about $1km^3$. There are 25 vents within this field. And, individual volcanoes have a volume of less than $.05km^3$. So, the volumes of individual volcanoes within this field are similar to those in the Yucca Mountain/Crater Flat area.

This volcanic field occurs in northwestern Arizona, as I said, just to the east of Las Vegas and to the south of Lake Mead. Regional extension in this area occur between 12 and 9 million years and was accommodated by northeast-trending strike-slip faults, northwest-trending strike-slip faults, the Las Vegas valley shear zone, west dipping detachment faults, and north striking high to normal faults. The volcanoes, the Fortification Hill volcanic fields shown in this dotted pattern, is related to the Black Mountain horst, a north-trending horst bounded on both sides by high angle normal structures. Volcanoes occur on the west side of the range, on the east side of the range, and three out of the 25 vents occur in the range interior.

Now, in terms of estimating or terms of evaluating tectonic setting, we can do this in one of two ways. We can look at this on two scales. We can look at the surficial structures or we can take a look at how the lithosphere behaves during the volcanic event. And, I'd like to concentrate on number two because I think this is very important because these magmas are derived fairly deep in the mantle and I think we have to understand how the lithosphere changes and how the lithosphere behaves during the event that we're talking about. Now, we can look at this in two ways; we can look at geophysics or we can look at isotope studies. And, for the last year, we've been concentrating on the isotope studies.

Let's take a look at this in a little bit of detail. This is very similar to one of the plots that Frank Perry showed yesterday afternoon. So, I won't go into details as to what these particular numbers mean. This is Epsilon Neodymium, initial strontium. Basalts that fall into this region here are similar to basalts that were generated in the ocean. These are ocean-island basalts normally assumed to have formed by partial melting of asthenospheric mantle. Basalts that fall in this area here have lower values of Epsilon Neodymium and higher values of initial strontium and are generally accepted to have been produced by the partial melting of the lithospheric mantle. The lithospheric mantle's signature is quite variable. In southern Nevada, the area around Yucca Mountain, Farmer, et al., suggested that basalts generated from lithospheric mantle for faulting generally fall in this general range. However, in other parts of the Great Basin, for example, in the transition zone between the Colorado Plateau and the Basin range, we can find basalts thought to have been generated at least having a lithospheric mantle component with Epsilon Neodymium values as high as -1 or possibly as high as 0. So, it's quite a variable isotopic range.

But, the important thing about this particular graph is the basalts of the Fortification Hill field fall into two groups. One, up in this area right here, characterized by basalts that were generated by partial melting of asthenospheric mantle, and Group 2, down in this area here, basalts that are thought to have formed by partial melting of lithospheric mantles. We have two isotopic groups and that's the important thing on that particular graph.

Now, let's take a look how these isotopic values are geographically and temporally related. If we look at the Fortification Hill field pre-9 million years ago, this is Lake Mead, the Lake Mead fault system, and the Las Vegas valley shear zone. The important thing to notice about this particular plot is that region-wide we have relatively high initial strontiums and relatively low Epsilon Neodymiums except for one point right here. The general conclusion from this particular plot is that most of these basalts probably were generated by partial melting of lithospheric mantle region-wide.

Now, let's move to the same area, but post-9 million years. This is sort of a complicated chart, but sort of concentrate to the area to the south of Lake Mead. You'll notice a very dramatic shift. Many of the basalts now have lower initial strontium values and higher Epsilon Neodymium values. These numbers in italics are older basalts. In reality, this is showing a gradual shift from the higher values pre-9 million years and sort of the lower values of Epsilon Neodymium pre-9 million years to the higher values post-9 million years. But, we get a shift and now we can say that most of the basalts in this area here are being generated by partial melting of asthenospheric mantle. But, to the north, we are still getting approximately the same ratios, relatively how Epsilon Neodymiums and relatively high initial strontiums. So, it's a major difference running roughly to the northeast parallel with Lake Mead.

So, what is this indicating? What we're suggesting is that during regional extension between 12 and 9 million years ago, the lithospheric mantle to the south of Lake Mead was thin and replaced by asthenospheric mantle. However, to the north of the Lake Mead fault system, the lithospheric mantle remained intact. So that contrasting behavior to the north and south of this boundary produced a Miocene domain boundary in the tertiary in the Lake Mead area.

Now, this type of behavior can be shown on this cartoon. This is prior to the major phase of extension. We had a relatively thick lithospheric mantle. Alkali basalts were being generated from the lithospheric mantle region-wide, period of regional extension, thinning of the litho-spheric mantle, replacement by asthenospheric mantle. So, that after the major phase of extension, we are melting asthenospheric mantle mainly to the south and lithospheric mantle to the north and the domain boundary was produced by differential behavior to the north and south of the location of the Lake Mead fault zone.

So, what we're suggesting here is that this area represents, in reality, a rift zone. This is very similar in behavior, for example, to what has been described the Rio Grande Rift and other rift zones in the western United States. We feel that this is a rift zone, but we feel that it's being sustained by passive upwelling in the mantle rather than active welling because we have sustained magma- tism for about five million years so that the heat source in the mantle must in some way be coupled to activities in the crust. So, the reason I'm doing this is I'm just showing you a way that we can determine how the lithosphere behaves during a magmatic event.

Now, let's move on and take a look at the Reveille Range. Let's get closer to an area of interest here. The Reveille Range in central Nevada, this is also a Pliocene alkali basalt field. You're up to between 5.9 and 3 million years ago. Total volume of about $9km^3$. There's 72 vents. And, individual volcanoes have volumes of $.13km^3$ or less. The Reveille Range falls in the northern part of a belt of volcanic rocks. Originally proposed by Vaniman, et al., back in the early 80's, they called it a Death Valley Pancake Range zone. It appears right here. The volcanic rocks in the Reveille Range occur on the west side of the range, on the east side of the range, and Mark Martin will describe in a bit more detail the structure control of volcanism in this area, but approximately 10% of the volcanoes occur in the range interior.

Now, let's take a look at some of the geochemistry, especially the isotopes. The last time I spoke to the Board, I mentioned that volcanism occurred in two episodes. So, I'm not going to go through that story once again. In Early Episode 1, between 5.9 and 5 million years ago, there seems to be a lithospheric mantle component in the source, relatively high initial strontiums, and relatively low Epsilon Neodymiums. Not as low as the one that I showed you in the previous area, but this suggests a lithospheric mantle component in the source. In Late Episode 1 and Episode 2, between 5 and 3 million years ago, strontium values are now lower and Epsilon Neodymium values are now higher. This suggests that we're now melting asthenospheric mantles. So, there's a shift with time between having lithospheric mantle component in the source to asthenospheric mantle. Now, Ken Foland recently proposed an alternative model where he suggests that this signature is due to crustal contamination. I, at the present time, do not like that particular model, and if Ken wants to discuss that with me later on, I'm very happy to talk to him.

So, the question is what is the structural setting of the Reveille Range? How does the lithosphere react during this volcanic event? Are there any similarities, for example, between this and the Fortification Hill field?

Now, I'm going to try to discuss the Reveille Range as part of this belt of volcanic rocks that I showed you in one of the previous slides. This belt of volcanic rocks was originally described by Vaniman, Crowe & Gladney in 1982.

It's a belt of Pliocene volcanoes extending from at least as far to the south as Crater Flat to the Pancake Range. And, a very important point, probably as important as this observation here, is there's little or no Pliocene mafic volcanic activity between this belt and the margins of the Great Basin. So, it's isolated. This belt is isolated in the central part of the Great Basin.

And, just to refresh your mind as to what we're talking about here, this is the belt, extending from at least as far to the south as Crater Flat to Lunar Crater. Very little volcanism between; in fact, almost no Pliocene mafic volcanism between here and the eastern margin of the Great Basin and between this belt and the western margin of the Great Basin.

DR. ALLEN: Gene, is there further activity still further north?

DR. SMITH: No, there doesn't seem to be any--until you get into north central part of Nevada, there does not seem to be any Pliocene activity. So, in reality, I'm not really sure why, but activity seems to end at least in this belt at Lunar Crater.

Let see then how the mantle is behaving in the northern and southern part of the belt. The northern part of the best, at least according to the model that I'm proposing, we have lithospheric mantle early, asthenospheric mantle late. This is associated with a long period of extension in central Nevada. Extension began probably as long ago as 30 million years and it's continuing through the Pliocene, at least. The southern part of the belt, the oldest volcanic rock that we have isotopic data on is 10.5 million years, but from 10.5 to the Lathrop Wells cone, it appears that we have a lithospheric mantle source with no asthenospheric or very little asthenospheric component in the mantle for the source.

Now, just to show you a map to show you what I'm talking about here, it's sort of an index map. This is from a paper by Axen & Taylor. The red line is the volcanic belt that we're discussing. To the south, we have a strong litho-spheric mantle component. To the north, a lithospheric component in the source is indicative of the high initial strontium and the relatively low Epsilon Neodymium. This is along the entire belt. And, this, by the way, is between 4 and 2.8 million years. Now, if we take a look at post-2.8 million years, we see a very dramatic change. We're still melting lithospheric mantle to the south, but we have an asthenospheric component in the source to the north.

I will propose a model looking something like this whereas between 4 and 2.8 million years, we have a relatively thick lithospheric mantle extending north/south under the entire belt. It could be that the lithospheric mantle is slightly thinner in the Reveille Range, so that here we're melting right at the boundary between the lithospheric mantle and the asthenosphere. Post-2.8--and this is exaggerated a bit. We probably didn't have this much thinning. Lithospheric mantle is replaced by asthenosphere. Over to the south, we still have lithospheric mantle.

Now, the question is what does all this mean? How can we explain these patterns that I just showed you? Are there any similarities at Fortification Hill field or are there other explanations for this? Now, I'm sort of repeating myself several times here, but this is a very important point. In the Great Basin, Pliocene volcanic activity has very interesting distribution. There are three prongs of activity and this has been known for a long time; on the eastern margin of the Great Basin, Pliocene and Quaternary volcanic fields, the Grand Wash field, the St. George fields, for example; western margin of the Great Basin, Independence fields, you can read the rest; and then in the central Great Basin, this volcanic belt that extends from Crater Flat up to Lunar Crater.

And, I will just show you a couple of maps. These are maps from Fitton, a fairly recent paper by Fitton. This is the transition zone between the Colorado Plateau and the Great Basin. These are volcanic rocks between 16 and 5. We have volcanism along the eastern margin, along the western margin, and then in the central part of the Great Basin.

Notice the almost total lack of mafic volcanic activity between the Reveille Range and the eastern margin and the Reveille Range and the western margin of the province. And then, last here, this is 0 to 5 million years, pretty much the same pattern, east, west, and the Lunar Crater/Crater Flat zone. So, just to bring home this point once again, this belt is isolated relative to other volcanic fields of similar age in the Great Basin.

Now, are there any other features of the Great Basin that also parallel this belt? Is there any other major geologic features that are coincident with it? And, many of these things were pointed out a long time ago. In fact, many of these things were pointed out in the original Vaniman paper. One important feature that's sub-parallel to the belt is that the western margin of the Precambrian craton based on neodymium data of Farmer & DePaolo lies just to the west of the belt. And, I'll show you a map of that in just a second.

Also, there is a concentration of tertiary calderas located along the belt and something that's really quite interesting is that one of the deepest basins in the central basin and range, Railroad Valley, lies adjacent to the Pan- cake Range/Reveille Range and long axis of that valley parallels the axis of the belt.

And, this very poor transparency. This is from Best, et al., a recent paper and a guidebook. He's plotted known or suspected caldera in the Great Basin. You have to be careful of this particular type of diagram because many of these calderas, or at least some of these calderas, probably do not exist, but this is his diagram showing calderas that are either known or suspected to exist. This is the volcanic belt we're talking about and there does appear to be a concentration of calderas along this belt from the Timber Mountain area up into the Reveille Range.

DR. LUCE: Is that a cross section line you have there or--

DR. SMITH: No, this simply represents the volcanic belt just to give you the location of the belt, volcanic belt, that I've been talking about extending from the Pancake Range down into the Crater Flat area.

DR. ALLEN: Well, Gene, this is your line, not Best's line, right?

DR. SMITH: This is my line. I superimposed this line on the diagram just to indicate the location of that volcanic belt.

And, this map here shows Railroad Valley just to the east of the Pancake/Reveille Range.

Also, just to go a little bit farther, this belt corresponds to a zone of crustal thickening during the Mesozoic and the Paleozoic. Sonoman orogeny, Antler orogeny, and thrusting during Jurassic-Cretaceous event resulted in crustal thickening in the general location of this Central Nevada Volcanic Belt. Also, a COCORP line across northern Nevada to the north of the belt suggests that this crust or the crust remains thicker in the central Great Basin after tertiary extension.

And, I won't show you all of these diagrams, but just this one. This is from a paper by Axen & Taylor just to show you the location of the central Nevada thrust belt and this is an area of crustal thickening during the Mesozoic.

And, I'm going to show you this interpretation from the COCORP line. Very dramatic changes in crustal thickness along the western margin of the Great Basin, along the eastern margin of the Great Basin, and significant changes in crustal thickness in the central part of the Great Basin.

So, the point of this, in case you're wondering what the point is, is that this volcanic belt corresponds to an area of the Great Basin with a fairly unique tectonic and magmatic history. These Pliocene volcanoes are simply the last episode of an area that's undergone a very complicated history since the Paleozoic. And, so to me, this suggests that these volcanoes are not simply randomly located in the belt, but they were looking at control by features or geo-logic phenomena that have occurred earlier.

Now, I would like to make some suggestions in closing as to what the cause or why these volcanoes are here.

You know, what actually controls the location of these volcanoes in this particular locality? I think we've already established this is an important tectonic and magmatic zone, at least in my mind. Now, this sort of verges on speculation here, but I'll do it anyway.

There's several possible explanations as to why these volcanoes are found along the axis of the Great Basin and these are just three. There are actually many more. First is that this may represent a Pliocene extension and, to me, the similar behavior of the lithosphere during the volcanic event to what we saw in the Fortification Hill field to what other investigators like Frank Perry, for example, have described in the Rio Grande belt suggests that, in fact, we might be looking at an extensional belt. Also, evidence from the Railroad Valley area where there is approximately 4.5km of sediment fill, much of which might, in fact, be Pliocene, suggests that there may have been considerable Pliocene extension at least in the northern part of the belt. So, this is one possibility that this may represent a zone of focused Pliocene extension and we may, in essence, be looking at a rift zone superimposed on the overall geometry of the Great Basin. This would be interesting because that would suggest the rift zone, in reality, is opening to the south.

Also, another possible explanation is that volcanism may, in fact, be controlled in the Great Basin by areas where the crust changes thickness in a significant way. This may be why we find volcanic rocks, Pliocene volcanic rocks, around the margins of the Great Basin because this is an area where we have significant changes in crustal thick- ness. Possibly, the crustal thickness changes in the central part of the Great Basin related to the over-thickening during the Mesozoic and the Paleozoic might have caused differential behavior of the over-thickened crust as opposed to the thin- ner crust during extension and this may have caused zones of fracturing and may have allowed these volcanoes to arise to the surface or the magmas to rise to the surface.

Another possible scenario is that the central part of the Great Basin may be an area where west-dipping detach- ment faults that break away along the eastern margin of the Great Basin, these attachment faults may enter the middle to lower crust beneath the central part of the Great Basin.

Let me just show you a cartoon and I have to empha- size that this is a cartoon. This is a cartoon from a recent article. And, modify this considerably, but let's just assume this is the eastern part of the Great Basin. Detach- ment faults may, in fact, project into the central part of the Great Basin. Here, they would reach to a depth of 30 to 40km into an area where the crust is behaving in a ductal fashion. This may be enough to generate magma in this area. Now, I have to warn you that many of these detachment faults are a lot older than the volcanism that we're talking about. Most of these detachment faults are Miocene in age. However, they may have been responsible for producing some of the Miocene calderas. They may have actually prepared the ground, they may have pre-heated this particular area of the Great Basin preferentially, which may have allowed the Plio-cene volcanoes to preferentially occur in areas where there has been a large amount of previous magmatism.

So, these are just three speculations as to why we have volcanism in this particular area. I'd, personally, prefer the first model, but we're currently working on doing a lot of work to try to determine which one of these models is correct.

Now, in conclusion, we feel that the Reveille Range and the Fortification Hill field are appropriate analogs to volcanoes near Yucca Mountain. Individual volcanoes have approximately the same volume. The lithosphere behaved in a similar way during volcanism. And, I forgot the last one here. That's right. The volcanoes in both of these areas have similar compositions. They're alkali basalts. They have similar isotopic compositions to those in the Crater Flat area. Also, the Crater Flat and Lathrop Wells volcanoes are part of the Central Nevada Volcanic Belt. In my mind, I feel like this is the most important tectonic feature, a very important tectonic feature of the central Great Basin. In fact, I feel that this is what is controlling the positions of the Pliocene volcanoes in the central Great Basin. And, I think that this Central Nevada Volcanic Belt has to be considered in any models, any structural models, that evaluate risk.

Also, the north and northeast-trending vault and vent alignments that we see at Crater Flat--I should have pointed this out earlier--but throughout this entire belt, volcanoes are controlled by north and northeast-trending faults. This is the pervasive pattern throughout most of the central Great Basin. It's not simply a local pattern related to Walker-Lane structures. I feel--and, I didn't have much time to develop this--but I feel that these north and northeast-trending fault and vent alignments have regional significance. They're not simply minor faults related to Walker-Lane structures.

And, I think that that's all I have to say.

DR. ALLEN: Thank you, Gene. I think just for a little overlook, let's go ahead with the next talk which is closely related and then see if we have any time for questions.

Carl, would you introduce your next speaker?

MR. CARL JOHNSON: The second part of our progress report will be Dr. Mark Martin. He has recently joined Gene Smith's group and we'll focus his remarks on some structural control studies of volcanism that he's been involved with.

DR. MARTIN: I'm going to be discussing basically ground truth, what we see in the field in the northern Reveille Range, and I think this is applicable to the talk that Dr. Valentine gave a couple of discussions earlier.

DR. ALLEN: Can you speak closer to the microphone, please?

DR. MARTIN: Okay. Basically, what I'd like to do is address the question, do older shallow crustal basement structures control the eruption of recent basaltic volcanism? And, in order to do this, we have chosen the Reveille Range. It's a lightly older analog. It consists of Pliocene basalts, as Gene has discussed earlier. And, basically, what I'd like to do now is sort of show you where Reveille Range is on a geologic map and give you an idea of what it actually looks like on the ground.

Reveille Range is located right here. Proposed Yucca Mountain repository site is about right there. You're looking over the northern Reveille Range here, looking to the northwest out over the western portion of the Pancake Range. We've chosen the northern Reveille Range as an analog and we feel it's an appropriate analog to study for a couple of reasons. One reason is that in order to understand older structure as it affects the influence of basaltic volcanism, we have to be able to see it. We have to be able to see the basement that these rocks are coming up through. At least, down in the Nevada Test Site and the proposed repository site, you don't get a good glimpse at this older basement, what it looks like, what the structures actually are that are exposed in them. The northern Reveille Range, we have a good cross section and we see good both structural depth and stratigraphic depth beneath the basaltic volcanism and there's some of the Pliocene basalts here erupting through and over land by Oligocene and Miocene volcanic rocks and also Paleozoic marine sedimentary rocks. Another reason for going to the northern Reveille Range is if you look at pre-existing mapping in the area, specifically Ekren, et al., a USGS group that published a map, a USGS map of this area at 1:48,000, you're led to believe that this area is just riddled with faults. And, some of their interpretations, if you look at the map, are that some of the Pliocene basalts are indeed coming up along older faults. Well, we feel that these two reasons; one, that you see deep basement upper-crustal rocks, you see deep into and beneath the Pliocene basalts, and the fact that there's apparently structure there for which these basalts to come up through if they so choose, are two reasons why the Reveille Range is an appropriate analog for this type study.

Now, I'd like to present some of the conclusions here and this is also an outline for the talk. One, detailed mapping at 1:24,000 that I have done in the northern Reveille

Range indicates that previous interpretations represented by previous workers, Ekren, et al., in 1973, in this area are not particularly unique. All right. And, I'm not saying that their mapping was poor by any means, but at that time they were covering a lot of ground and this is a good reconnaissance map, but if you look at it in more detail, I think you see that their interpretations are probably not the correct interpretations in many cases.

Secondly, I should point here that wherever you see Plio-Pleistocene in these slides, one, Pleistocene is misspelled and what I really mean is Pliocene basaltic eruption. And, this is hopefully correct in those handouts. That Pliocene basaltic eruption in the Reveille Range occurred along both range crests and at the range margins. This is important because DOE workers have suggested that Pliocene basaltic eruptions generally only occur in alluvial basins and at range margins and very, very rarely occur within the central portions of the range.

And, then, finally, Pliocene basalts in the northern Reveille Range do appear to utilize older shallow crusted structures as conduits for eruption. So, this is again sort of an outline where this talk is going.

So, in order to understand if basaltic volcanism is taking advantage of older structures, we have to know what these older structures are. And, the Tertiary lithologic nomenclature and the placement of lithologic time tags presented by Ekren, et al., in 1973 in a map that was done at 1:48,000 from Tertiary volcanic rocks is generally not questioned. I don't question their nomenclature and I don't question the placement of their contacts. What I do question, at least in this instance, is that they have called upon strike-slip faults and reverse faults and in many areas the different Tertiary volcanic units. My mapping indicates in a more detailed mapping that these contacts--and having a better understanding now than we did 20 years ago of the Tertiary lithologic succession in the area, finds that many of these contacts are depositional and that in geometry these contacts as a function of topography associated with calderas at the time of deposition.

What I have here is a very simplified geologic map of the northern Reveille Range. I'd like to say again--well, these are Pliocene basalts, 3 to 6 Ma in age and they're in gray. The older volcanic units are in greens and orange and lavenders and the Paleozoic marine sedimentary rock should be on here in blue. This is a line or section that I'll show you in some of the upcoming slides. But, in general, if you look at Ekren, et al., map of this area, what you see is most of these contacts and the older Tertiary rocks and the Paleozoic rocks with Tertiary rocks or a lot of Paleozoic rocks are fault contacts. And, in most cases, I believe that these are depositional contacts and that you have the effects of topography and related caldera eruption during the time these Tertiary rocks were deposited.

Specifically, I'm going to show you an example in the next couple of slides of this contact here. This contact, as I've shown in the legend here, I believe, is better interpreted as a caldera wall. Ekren, et al., previously showed this contact as being a left lateral strike-slip fault with associated thrust faults, presumably kinematically related to the left lateral strike-slip faults.

So, to look at this contact, what it looks like in the field, this contact that I just showed you basically comes right over the crest of this hill, right about there where you see the Pliocene basalt vent coming. That's a basalt vent actually. The contact, what I interpret to be a caldera wall, what was previously mapped as a left lateral strike-slip fault, separates a younger tuff, a tuff in the northern Reveille Range here, and it is apparently the--the field data indicates a very good interpretation of this is that this is intracaldera fill and that the caldera that erupted--that caldera is erupted, filling its own caldera by the tuff in northern Reveille Range. The high topographic relief here is an older tuff, the tuff of Goblin Knobs which is known to be 24.5 million years in age.

I've generalized a cross section across this as a generalized cross section and I don't mean there to be any scale. It's just to show you what my interpretation of this contact looks like. And, here, we have the intracaldera fill in the northern Reveille Range, the caldera which erupted this tuff, the margin. The structural wall for this caldera is here separating these two different compositional units and in age.

Again, there's no compelling evidence along this contact for a left lateral strike-slip fault either. There's no sign of brecciation and there's no sign of lateral offsets between the different units. However, there's quite a bit of field evidence to suggest that this is indeed structural wall for the caldera that erupted the tuff in the northern Reveille Range. Some of the field criteria that I've used for identifying this is a structural wall. I'd like to state that this is not an exhaustive list of criteria. These are the criteria that I find in the northern Reveille Range. There are many other criteria to use for identifying calderas in the field. These are just the ones that I see in the northern Reveille Range.

Contact here, this is looking south along the same contact that I infer to be a structural wall for a caldera. These are the older tuffs at Goblin Knobs. This is the intracaldera fill, the tuff in northern Reveille Range. These tuffs are highly silicified. This is basically what I would infer to be a buttress unconformity at this point. The tuffs here within the caldera are highly silicified. They're innerbedded with surge deposits and they're also innerbedded with mega-breccias. The mega-breccias are apparently eroding off the structural relief and these mega-breccias contain large up to 5m class. That would be older tuff in Goblin Knobs. It's this type of criteria that I would use to say this is, indeed, a structural wall for a caldera and not a left lateral strike-slip fault or associated thrust faults.

To look at some of these examples in the field, this is a large lithic class approximately two feet in diameter. The older tuff in Goblin Knobs that was within the intracaldera fill of the toughened northern Reveille Range. And, here are some silicified surge deposits and again inner-bedded with these surge deposits very near the structural wall of the caldera you see innerbedded mega-breccia deposits that are eroding off the wall rock of this caldera.

Field studies in the area, in the region in general, also corroborate the interpretation that I'm making that bears, at least, one and I think probably two caldera centers in the northern Reveille Range. And, this is out-lined by a photograph that Gene showed. This is from Best, et al., in '89 showing known calderas. They're relatively good field evidence for these calderas and inferred calderas in the region. Okay. And, again, the Nevada Test Site down here, the Reveille Range. I would infer from my field studies in the northern Reveille Range to date that there are probably two caldera centers that should be placed on this map now.

So, to summarize what I've just told you about the ground truth, we can look at older maps that may be 20 years old that are reasonably good maps for reconnaissance, but doesn't substitute for really good, more detailed mapping. And, if you were to look at Ekren, et al., map now, you would just sit in your office and look at it and you would say, oh, yeah, it's quite faulted prior to the eruption of Pliocene basalts and it looks like some of the faults are actually serving as conduits for emplacement of these basalts. I would say from the field studies that I have done that this is not the correct interpretation. That, in fact, many of the contacts between different Tertiary units are depositional, but that the northern Reveille Range area is an area of Miocene caldera eruption. There are probably two calderas there and that it is these structures that are related to caldera eruption data probably serving as conduits for basalts to come up through the upper-crustal portions, the upper shallow levels of the crust.

Now, where are the basalt vents located in the Reveille Range? Well, as you can see in this slide here, most of the basalt vents of approximately 70 or so are located either in the alluvial basins or along the range margin, but approximately 10% of these vents are actually well within the range interior and a few of them are actually on the range crest which is outlined by this dashed line. The orange area is the exposed bedrock beneath these Pliocene basalts.

Crowe, et al., (1991) have stated that basalt vents tend to occur in alluvial basins or at range margins and that in analogous geologic settings to the Yucca Mountain site basalt vents have not been documented in the central portions of ranges. Furthermore, he has stated that it is important to document that basalt eruption does occur within the ranges to understand why this occurs. Well, I think my mapping clearly indicates that approximately 10% of the Pliocene basalt vents or the 70 or so occur well within the center of the ranges and that I can document at least two of the vents that appear to be sitting right on older pre-existing caldera walls.

A few examples shown here that you see in this photo already, it's a different angle. You're looking north across the northern part of the Reveille Range into the Pancake Range here. Nicely stratified outflow deposits for a lot of the calderas that I showed you in Best, et al., map. Come across Road 375 which is down here beneath the relief topography and you're in another world. You don't see these nice stratified outflow deposits. You see chaos for the most part. But, I believe that chaos is simply related to we're in a nested caldera complex once you go across the highway.

But, anyhow, this is Dark Peak, a basalt vent. It's located right on the topographic range crest and it is apparently located also on a contact that I infer to be the structural wall for a caldera. If you're standing on Dark Peak, as we are in this slide, looking a little bit more to the northwest, this is looking more to the northeast, you see that basically this is the contact that runs through here; the tuff of the northern Reveille Range, younger, inferred to be an intracaldera fill; older, tuff of Goblin Knobs; there's a vent here, as well as a vent up here. These vents appear to be located right on the caldera structural wall.

All right. I sort of introduced the structural control of basaltic emplacement in the Reveille Range. What I'd like to say here is that my mapping indicates that faults were not found to serve as conduits for basaltic eruption in the northern Reveille Range, as you might be led to believe if you looked at Ekren, et al., map from 1973. I'm not saying that there are not faults in the northern Reveille Range. There are faults there, but I think they're relatively minor and I think they're relatively minor structures as far as serving as conduits for eruption of the Pliocene basalts, at least in the central portion of the Reveille

Range. I'm not talking about range bounding faults which may be serving as conduits for the basalt vents that we see on the range margin. I'm simply talking about structures within the bedrock, within the central portion of the Reveille Range.

However, pre-existing north/south-trending tectonic joint sets in the Miocene tuffs do appear to guide basaltic dikes that feed surface basalt flows and vents. That pre-existing Miocene caldera wall has appeared also to serve as conduits for basalt eruptions. I've shown you some of those slides already. I'd also like to make a disclaimer. An equal number of occurrences of basalt vents and dikes were not found apparently to be associated with pre-existing structures. And, this is only what you can see on the surface. This doesn't tell you what might be guiding the occurrence of these vents and dikes in the immediate subsurface.

The slide on your right is a slide where you're looking to the west, basically from the alluvial basin on the east side of the Reveille Range. You're looking up to the range scarp and into the range crest area. And, you might not be able to see, but these are some basalt dikes here. There's a couple large basalt dikes that kind of come in up through here and you can see them coming into here and, in fact, several of these dikes come right up to the vent here and this vent has erupted, one of the larger basalt flows found in the northeastern flank of the Reveille Range.

I'd like to also state here that the exposure we see here, this is--from here down to where this photo was taken is approximately 300 to 400m of relief. We see deep levels here. We see the basement. We're seeing approximately 200 to 300m of stratigraphic section within the basin and this is rather unique for this portion of the basin range and specifically for studies that are going on down in Nevada Test Site.

If we look at these dikes up close, this is what we see. This is a basaltic dike. These juniper trees are five to six feet high. To scale, this dike is approximately a meter in places, a meter and a half thick. It is coming up and it is clearly feeding this basaltic vent. There's no question about that in my mind. What is interesting here is that you see a very prominent north/northwest-trending, what I infer to be a tectonic joint set. Now, Dr. Valentine earlier said that these basaltic dikes can also propagate their own joint sets. However, I infer these to be pre-existing tectonic joint sets in the Miocene tuff here. One reason why I say this is that these dikes don't just always come up along these joint sets which would be the inference here; they're sub-parallel. But, you see these dikes cross-cutting these joint sets at high angles, as well. So, this leads me to believe that this joint set is very reasonable.

It covers quite a large area. You can see it very prominently exposed in air photos. I believe this dike--at a closer scale, we see what I infer to be tectonic joint sets and, indeed, these basaltic dikes do appear to be coplanar with these joint sets. So, the inference here is that I believe these dikes are being--that these tectonic joint sets are serving as planes of weakness for these dikes to come along and feed the vents that ultimately erupt the basalt flows in the northern Reveille Range.

I've already addressed this, but I'd like to bring this home a little bit more. This is what I infer to be a structural caldera wall. At least, three of the vents that are lined up along the topographic range crests sit on top of this older contact. And, to me, this implies that the fragmentation that you had during Miocene caldera eruption in this region, this fragmentation serves as an appropriate conduit to bring these basalts to the surface in the shallow crest. This is a very up close view of this contact. Again, this is the tuff of the northern Reveille Range. It's intra-caldera fill. Contact between the tuff of northern Reveille Range and the older tuff in Goblin Knobs is not more than two or three meters to the right of this slide and actually these rocks down here are the tuff of Goblin Knobs. You cross a boundary. You're into a thick sequence of intracaldera fill and the basalt dikes that feed this vent appear to be coming up along this contact.

And, again, just to give you a generalized cross section in the northern Reveille Range, inferred caldera margins again based on more detailed mapping, I think these are good calls of the caldera that erupted in the tuff of northern Reveille Range. This contact, there is apparently at least three basaltic vents that sit on top of this contact. My interpretation is that these basalts are actually taken advantage of this overstructure. This is my interpretation and I'm not saying that my interpretation is any more unique than what Ekren, et al., had for this contact, but I think the field evidence more clearly supports a caldera structural wall at this location than any--there is apparently no compelling evidence for a strike-slip fault at this contact. And, also, the tuff of Goblin Knobs also appears. Its caldera wall must also exist some place between the northern Reveille Range and the southern Pancake Range. But, this contact is unfortunately covered by Pliocene basalts and this is also an inferred contact. This, you don't see, at all, but you can place it well just by distribution of the tuff of Goblin Knobs and older tuffs in the area.

DR. ALLEN: Mark, let me make sure I understand this. The arrows there point to two different caldera margins.

DR. MARTIN: Yes. One, this is--

DR. ALLEN: And, in both cases, the other margin is somewhere off to the left of the cross section.

DR. MARTIN: Yes, you don't see that.

DR. ALLEN: Okay.

DR. MARTIN: So, again, in conclusion, detailed mapping 1:24,000 in northern Reveille Range indicates that previous interpretations represented by older mapping in 1973, Ekren, et al., in this area is not particularly unique. And, the warning that should go along with this is that field inter-pretations are seldom unique at any scale and that pre-exist-ing mapping can always be improved upon. However, more detailed studies--and, I really emphasize this, you need to get down and you've got to put your nose to the rocks--and better understanding of the regional geology will lead to fewer equivocal interpretations.

Secondly, Pliocene basalt eruption in the Reveille Range occurred along both range crests and alluvial basins, but it also occurs in the northern Reveille Range approxi-mately 10% of the time. If you want to put numbers on it, well within the central portion of the range and on the range crests, unlike what DOE workers have suggested. All right? This needs to be taken into account. And, the warning here, again I've already said that.

Thirdly, Pliocene basalts in the northern Reveille Range do appear to utilize older structure, shallow crusted structures, as conduits for eruption. And, the warning here, existing faults are not the only structures that basalts can utilize as conduits. Pre-existing shallow crustal fragmentation example at least from the northern Reveille Range, existing calderas in the area, and existing tectonic joint sets are equally suitable as conduits for eruption of Pliocene basalts in the region.

Thank you.

DR. ALLEN: Thank you, Mark.

We're a little bit over the time here, but any quick questions?

(No response.)

DR. ALLEN: Let me declare a recess here and we'll try to reconvene at 10:50.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Could we reassemble, please?

Okay. The next two speakers on the program, both from U.S. Geological Survey, requested to give presentations here. The Board felt that the data they had was relevant and we welcome them here.

The first one is Brent Turrin of the U.S. Geological Survey. Brent?

MR. TURRIN: Good afternoon. We'll get started and try and keep this on a quick pace. Lights, please?

This is just a quick slide to show you where we are in case you've forgotten all the fun we've had today. Another geographic location for you--hang on for a second-- the one million year old complex, the 3.7 million year old complex, and the now infamous Lathrop Wells. An aerial photography of Lathrop Wells--and, the reason this is in here is this is the Unit Ql_3 and this is basically where Unit QS_5 resides and we've used a simple criteria in developing our map, scoria mantle deposits as opposed to unscoria mantle deposits. This is the map that we've come up with. It's basically a modification from Crowe, et al. You can't see them very clearly, but those are the K-Ar 40/39 sites and the paleomag sites, again unmantled deposits and scoria mantle flows. Using that simple criteria, the paleomag data that Duane will talk about essentially gives this kind of distribution which is essentially an ordered distribution.

A brief history here is that in 1978 the USGS generated some K-Ar data on Unit Ql_3 and obtained a weighted age of 127 plus or minus 15. If you don't like the weighted age, we give the arithmetic age up here with a standard error of the mean. In 1985 to 1990, the USGS in a collaborative effort with UC Berkeley did a much more extensive dating study and developed and obtained this age, 137 plus or minus 13,000, but there was distinct evidence of some contaminating component, in particular, in one sub-site in the Unit Ql_3. If you combine these data, you'll see that statistically you really can't separate these three analyses out and the errors are also large. So, in the weighted average, it has very little impact and, circa 1985, this was our best estimate of the age, 133 plus or minus 10.

Okay. Now, we'll jump to the other unit that I've introduced and that was Unit Ql_5. In 1978, the USGS obtained three analyses with a weighted average of 240 plus or minus 40 for the scoria mantle Unit Ql_5. In 1979, funded by a study by Sinnock & Easterling, UC Berkeley obtained an age of 97 plus or minus 13 for the same unit. In the 1985 to 1990 study, we obtained this series of K-Ar dates, 176 plus or minus 60. If you combined all the data, you'd get this weighted average as a 115 plus or minus 12 and you'll notice that the distribution is essentially Gaussian.

To address the contamination issue that we saw in the one sub-site, we applied a laser of 40/39 single grain dating technology. This is just a little viewgraph showing you the apparatus where we actually in this sample container we fused single grains of the Lathrop Wells material. This was published in Science. This is the Unit Ql_3 and it clearly identifies a contaminating component. Now, these components are identified both on compositional information we obtained from neutron activation of potassium-calcium and, in a statistical approach, addressed these as definite out-liers. With these data set included, this black portion in here, you can see that there's not a normal distribution. With these data set removed, you do have a normal distribution and the ages are given up here. Again, this has been published already. For Unit Ql_5, we were able to also date samples using the laser on samples QS_5 on Ql_5 which paleomagnetically have been correlated. And, they both, separately and together, give nice calcium distributions.

This is just a quick reappraisal of the isochron plot presented in our paper in Science. And, there has been a claim that there are influential data points in this data set and I presume this is the one they're talking about here and these ones they're talking about there. In an A&S paper we presented in 1991, isochron data are presented for each of the sub-sites and, in addition, when we did these analyses, we removed these points to see if they were influential and they were not influential. They do not change the isochron data.

So, at that time, we thought the best age circa 1991 was the combined ages of the K-Ar and 40/39 ages which was for Unit Ql_5, 136 plus or minus 8, and 141 plus or minus 9 for this unit. Given the standard errors, the analyses are indistinguishable from each other. They're essentially identical in age based on the K-Ar 40/39 data set.

There has been some concern expressed about systematic errors and/or excess argon in these rocks. This is essentially zero aged K-Ar data that we obtained at the time we did the initial K-Ar analysis. You can see there is no systematic error term. It's essentially 14,000 plus or minus 10,000 years which would statistically say it's as good as zero. In addition, we went and analyzed just only--at this point, we've only analyzed six samples of Tabernacle Hill and this is unique in that Tabernacle Hill is known to be 17,000 based on stratigraphy and Carbon-14 ages with Lake Bonneville shore line. And, moreover, what's important is that this particular basalt flow has erupted through pre-Cambrian crustal material about 1.7 billion years, I believe. And, as you can see in this data set, excess argon hasn't manifested itself.

Jane Poths presented some information yesterday about all of the excess argon she observed in olivine. If you do the mass balance calculations, that amount of excess argon she reported essentially would add about 1 to 2% to the ages that we've obtained on the units from Lathrop Wells. The calculations, you can simply go through and that's--you take her values and you ratio them out by the percent of olivine in there and by the measured argon in our data results and you'll see that that is about a 2% variation and that equates directly to age.

Okay. We've recently have done some what's called 40/39 step heating results with a newly developed furnace at

Berkeley and these are the final results we've obtained. This is on Unit Ql_3, sample for reference 186. As you can see, there is a nice homogenous plateau spectra that includes essentially greater than 90% of the gas. And, if you look at these--this axis has a lot of information on it. So, don't get confused. This is apparent age up to about that point, this is the potassium-calcium content as determined by neutron activation, and this is the percent radiogenic.

A real important point I'd like to make is this number right here in this column. In the conventional K-Ar and in the 40/39 dates, we generally obtained essentially .1 to 2% radiogenic argon in the individual analysis. And, this method, the percent radiogenic--I don't know if you can read that slide, but that's about 8% right there. We increased the radiogenic argon by as much as a factor of 10 in this particular sample, and yet, the age hasn't gone up proportionally. Moreover, if you look at the tests in calcium information that we get from each incrementally higher step, there is no function of age displayed with the potassium-calcium ratio of the rocks. This is another unit, Unit 486, Ql_3, as well. Here we've increased the radiogenic yields up to 20%, and yet again, no variation with ages as the radiogenic argon goes up and comes down. I will discuss a little further on what this diagram actually means. The point to note here, this is the age as 125 plus or minus 5. The isochron ages on the plateau data set are 122 with an air intercept of essentially air ratio and a standard weighted deviance of about one. Again, these are outstanding--would be considered outstanding textbook examples and virtually eliminate any possibility of excess argon influences these ages.

This is sort of what I call my crippled run. This new system is fully automated. So, you program the incremental steps and let it go. However, there is still a potential for operator error and I lost the steps from about 400 to 800. And, so this is only really a partial plateau diagram, but this is for Unit Ql_5 and again the data are 183 plus or minus 38. The isochron ages indicate air ratios and an age of 160 plus or minus about 100,000. This is not a very good run and we're going to redo it because there was some technical problems with it.

I'm going to go through these really quickly because they're not of very good quality. This is essentially an ion microprobe image of Lathrop Wells material. From about there to there, is 100 microns. This is iron. That's the olivine with a little bit of brown mass olivine. This is magnesium. This is aluminum distribution. As you can see, there's no aluminum or very little aluminum in the olivine phase. This is sodium distribution. This is the calcium distribution and you can notice there's some microcrystals or probably pyroxenes growing along that grain boundary. The thing to watch is in these dark areas. That's the potassium distribution, again 100 microns from here to here. The point that I want to make is that there has been some claim that some ages have been obtained from Lathrop Wells that are "superior" because they're plagioclase separates. Well, the data from those analyses are 2% K_2O, and if anybody knows the nomenclature, plagioclase or the chemistry of plagioclase, you can't have that much potassium in plagioclase. So, what they've done essentially is a ground mass separate.

Okay. I promised you we would explain this diagram a little more carefully and I'll try and fulfill that promise. I'll leave this up here for reference. This is another ion microprobe. This time, we've learned the technique a little better. This is a sodium. As you can obviously see, this is the plagioclase lath, this is the interstices between the plagioclase. This is calcium. Again, you can obviously tell the calcium because of their calcic rich plagioclases. And, key area right in there. Again, this is about 100 microns across. That dimension right in there is probably 20 to 30 microns. There's the potassium in this rock.

Now, concern was made about the recoil of argon-39 out of these sites. If you've read the literature on recoil, the mean recoil distance of the energies that potassium-39 that is converted to argon-39 is about .02 microns. As we can see from this image, that's about 20 to 30 microns, much greater than the recoil distances reported in literature. And, what happens in recoil is that a potassium-39 atom through an np reaction was converted to agron-39 with a recoil distance of about .02 microns. And, very, very fine grained material, .02 microns or less, the 39 can recoil into high K phases. That means you can get unsupported 39 recoiled into this calcic phase.

Now, do we have recoil problems with these rocks? Absolutely not. Again, we look at this value right here, the potassium-calcium ratio of this particular analysis. As we increase the temperature of each incremental step, we are essentially doing a Bowen reaction series to basaltic composition with a eutectic value apparently around 750, 800, a dry eutectic. And, what you can see is that this is where we're outguessing the highest potassium phases which correspond to the 20% radiogenic yields we got out of these three consecutive steps. Now, we're starting to outguess higher calcium phases and the radiogenic yield is going down, but the age is staying identical. Again, higher and higher calcium phases, radiogenics going down, ages stay identical, except for the last 2% of the gas and that's--I don't know what that represents. That's the last refractory phases that we were outguessing. If there was any significant recoil from this part of the domain of these rocks, you would not get a plateau like this because as unsupported 39 is recoiled into that material, these ages would decline monotonically generally. This has been well-documented in the literature in several different papers. And, in our recent comment reply in Science, the reference was given there and there were some reprints in the back.

Okay. What we're going to do here is now what is the conclusions we can make from this early 40/39 step-heating date we've made? And, the conclusions we've made is that the original K-Ar ages are essentially acceptable or correct. There is no evidence of excess argon. There is no evidence of recoil. And, what little excess argon is found in the highest-most olivine phases is volume percentage negligible and this can--let me digress a bit. This is the plateau age, this is the total fusion integrated age. And, if there is any excess argon, it's less than 5,000 years, 6,000 years worth of age. And, that's what we get from using Jane's data to do the calculations on what the effect of argon would be. It turns out to be 1 to 2%. So, again, we can dismiss the excess argon argument again by the isochron data and by the mass balance calculations from the data that Jane has provided us.

Okay. I promised you some new data and I obtained cuttings when they drilled the buried anomaly in Armagosa Valley at every 10 feet. And, this is just one of the 15 or 20 analyses on the buried anomaly. It's typical plateau age. This was done using a laser. It's old enough that we could use much smaller samples. So, we don't need the furnace. The most percent radiogenic is running up here at 80/90% radiogenic. Potassium-calcium ratios show the typical pattern we see in basalts, climbing in the high refractory phases. The plateau ages is 4.3 million years plus or minus 35,000 years. The integrated age is 4.3 plus or minus 130,000 years. The isochron data are 4.38 plus or minus 73 with an intercept of 281 plus or minus 18 and an MSWD of 1.6, an acceptable value.

You may be concerned as to why is this value so large when the error is so large. In an isochron plot, if you plot this data, considering that these are very high radiogenic, you have a very long regression in an isochron plot back to the initial composition. Therefore, the errors are quite large.

This is a newly obtained step-heating date on Hidden Cone. This was done with the furnace. There are the temperatures, the apparent age, potassium-calcium, percent radiogenic, and isochron information. The best age estimate for Hidden Cone would be 382,000 plus or minus 16,000 years. This is concordant with the isochron ages and essentially an air intercept again at a 2 sigma air confidence interval.

This is a summary of the K-Ar ages I obtained at Hidden Cone. This is the older underlying basalt at 9.85 plus or minus 190,000 years. And, this is the Hidden Cone data, 366 plus or minus 74,000 years. As you can see, they're almost perfectly concordant with the step-heating data.

So, what does this mean? Well, the dates that we've obtained at this point for the basalt flows in Crater Flat are essentially one million years even for the little cones, Black Cone, Red Cone--I might have those backwards--and northern-most. And, 3.7 million years for this complex of basalt flows and vents here. And, this is just a map of that air photo and then we have an age--right now, if I had to take an age to the bank for Lathrop Wells, it would be 125 plus or minus 5 for Lathrop Wells.

And, that's the end of the talk and I'll entertain any questions now?

DR. ALLEN: Okay. Thank you, Brent. Are there questions from the Board?

(No response.)

MR. TURRIN: Either that's a good sign or a bad sign, I don't know.

DR. ALLEN: Any other questions from the front table?

(No response.)

DR. ALLEN: How about from the audience? Yes, Don DePaolo?

DR. DEPAOLO: I have a question, but actually I just would like you to clarify a couple of things. The first set of new data you showed was on Unit Ql_3 and you gave this 104 plus or minus 15.

MR. TURRIN: What's that? Could you repeat that, please?

DR. DEPAOLO: Unit Ql_3 is 104 plus or minus 14 was the isochron age?

MR. TURRIN: Was the isochron--hang on, I think I have the papers here so I don't have to go through the slides. Okay. We obtained an isochron age of 104 plus or minus 14, one standard error. And, an intercept of 297 plus or minus 1, one standard error. An MSWD of 1.9.

DR. DEPAOLO: Okay. And, the age of 122--

MR. TURRIN: 123 plus or minus 10 was the preferred plateau age.

DR. DEPAOLO: All right. The other--

MR. TURRIN: The other Ql_3 unit was published in the Science reply. That's the--

DR. DEPAOLO: Actually, I was more interested in the new one that--

MR. TURRIN: Okay. That one is 125--the plateau age is 125 plus or minus 5. The isochron age is 122 plus or minus

5. And, the intercept is 296 plus or minus 1.5. MSWD of .7.

DR. DEPAOLO: And, was that on Unit Ql_6?

MR. TURRIN: Ql_3.

DR. DEPAOLO: That's also Ql_3?

MR. TURRIN: Yeah. These are the same samples that we essentially did the study on starting in 1985.

DR. DEPAOLO: Okay. One comment. You made the statement that there's no evidence for any inherited argon-40, yet the age for these two is about--well, it's about 30% younger or 40% younger than the means that you have from before of about 150.

MR. TURRIN: Means before were 136 and 141 plus or minus 8 and 9,000 years.

DR. DEPAOLO: In any case, the initial of 297 is a little higher than air and that would indicate about a half percent of radiogenic--

MR. TURRIN: The intercept is actually indistinguishable from the air given the analytical precision.

DR. DEPAOLO: Yeah, but it's half a percent higher which was approximately your total radiogenic contribution in those samples that you'd measured before. So, the amount of inherited argon was not less than 50% of the amount of--

MR. TURRIN: Oh, I see what your point is. The amount of inherited argon you could hide in that ratio would--yeah, okay. I can--

DR. DEPAOLO: I mean, my interpretation of what you've shown is that, in fact, the data that you have originally contains a small component of radiogenic argon, but now that you have a small component of inherited argon and now the new data because you've got higher radiogenic proportions, can separate the radiogenic from the inherited better, and you can still see the age. The age is slightly younger than it was from the other data and it's better defined now.

MR. TURRIN: Yeah, I'd agree. That's why I stated that my preferred age would be 125 plus or minus 5 now for Unit Ql_3.

DR. ALLEN: Other comments? Bill Melson?

DR. MELSON: Brent, in your various papers which I've read and kind of enjoyed, probably more than the participants in some of the controversy that's going on. Could you speak rather directly to what I consider the very clear cut evidence of some of the formations at Lathrop Wells in particular, but in general, which would seem to strongly indicate and I think you would agree that there are multiple events. But, perhaps you could address that directly?

MR. TURRIN: It depends on what you're using for the evidence of the multiple events. In our Science reply, we presented a composition, grain size information, and we also have some other information that Duane will present of why we think that those deposits may not be volcanogenic in origin.

And, therefore, any ages that they obtain on those deposits are irrelevant to the age of eruption of Lathrop Wells.

DR. ALLEN: Other questions or comments?

DR. FOLAND: I have a question and then I'd like to make a comment and have you respond. First, what is the identity of this potassium there--potassium regions? Mineralogically, what is it?

MR. TURRIN: When you look at them under high-powered petrographic microscope, it first appears to be completely extinct, but if you look very carefully you'll see it's probably an intergrowth of alkali feldspars. And, the next phase now is to take the SCM and look at these things in a much finer scale and see where the potassium really resides. This is not a new study. This kind of study was done by Dalrymple & Mankinen and presented in EPSL circa 1974. And, those kind of textures have been shown to produce reliable K-Ar ages that I've demonstrated in those micro-graphs.

DR. FOLAND: Okay. Secondly, the comment is that essentially the radiogenic argon is dominated by the potassium-bearing phase. All of the isochron analyses, including the step-heating, is dominated by that effect. So that, in effect, if one looks at the isochron analysis, it seems to me it is difficult to rule out conclusively excess argon if, in fact, one is dealing with a mixture of atmosphere and, let us say, argon in this fine grain potassium bearing phase. I'm not sure how one can rule this out because that dominates the entire argon.

MR. TURRIN: Okay. Right. Okay. You're saying--if I understand, let me try and summarize your statement then. That you can't rule out that some finite phase of excess argon distributed in that sample, right?

DR. FOLAND: That phase of argon has to be associated with potassium. I think that's the requirement. Otherwise, one can describe the isochron diagrams to mixing between argon and this matrix fine grain phase and atmosphere.

MR. TURRIN: I don't--

DR. FOLAND: --dominated by atmosphere.

MR. TURRIN: It's dominated by atmosphere?

DR. FOLAND: Your mixture is dominated by atmosphere.

MR. TURRIN: Yes, that's correct. The mixture is definitely dominated by atmospheric argon. And, in the step-heating experiments, we've driven off a large portion of that excess air argon and what we do see is that no matter--if you look at the potassium-calcium ratios, you get essentially concordant ages all the way across the compositions that you're melting as you go through the sample. So, if excess argon then would have to be evenly distributed, but if it was evenly distributed, you wouldn't get isochron ages that give you air intercepts and concordant isochron ages that are concordant with the plateau.

DR. FOLAND: I think the essential point is what when you do see a rise in the calcium-potassium ratio, what proportion of that argon, in fact, is due to this calcic phase? That's the critical point. If it's a large fracture, then your point about excess argon is well-taken.

MR. TURRIN: Well, let me see if I can find the viewgraph. If you look at this, you can see that our steps are essentially between--it's easier if I look up here. Each individual step is about a 10 to 20% cut of the total gas. Well, this one is a little bit larger. But, you can see that each increment we look at is pretty much equally distributed throughout the whole volume of argon relative to 39. And, again, these are essentially potassium-calcium ratios you would get from pyroxenes and calcic plagioclases. And, the errors are tremendous because there's very little 39, therefore there's very little radiogenic argon in there and that's why these error bars have grown. But, they aren't--this particular last gas basically has some excess argon in it. This is where we might be cracking the olivine fluid inclusions and releasing that last little bit. But, overall, there is no distribution of excess argon in these rocks that can--you can't simply explain these results if it's homogeneously distributed.

DR. FOLAND: Do you want to comment on the plagioclase analysis that you referred to? I'm not familiar with this.

On your data, I guess, or someone else's.

MR. TURRIN: What do you mean the plagioclase analysis?

DR. FOLAND: You mentioned that there was a plagioclase analysis of the--

MR. TURRIN: Oh, there's been reported, I believe, a "plagioclase analysis" that's about 2% K_2O of, what, 60,000 years or so for Unit Ql_3. And, Paul Damon's lab has touted this and has reported these results as plagioclase analyses and I'm not aware of any stoichiometry where you can get that much potassium in plagioclase.

DR. FOLAND: Just the K-Ar analysis?

MR. TURRIN: Yeah.

DR. ALLEN: Okay. Bruce Crowe?

DR. CROWE: What I'd like to do, Brent, is try to bring us into perspective. The issue really here isn't the analytical methods. The issue is the data reduction methods and basically you were not here yesterday, but there was a range of information presented that showed dramatically younger ages from a combination of helium and other work. What I have difficult with is two things. One is the statistics of how you do your variance weighting. But, basically, you argue that there's no contamination, no excess argon, and yet you eliminate samples that you screen as being contaminated. And, that's an inconsistent statement, basically. Second is that if you just do a normal mean, the error bars basically overlap with all the other methods. And, so what we really need to focus on here is your confidence that your variance weighting method is correctly applied to these samples.

MR. TURRIN: You have several statements in there and I'll try and address them one at a time. Basically, if you don't weight the data, you go with your analysis just taking a simple mean of the data set, the standard deviation simply refers to the width of the Gaussian distribution of a data set. It has nothing to do with how well you know the central tendency. The parameter you're interested in and how well you know the central tendency is the standard error or the mean. So, it's incorrect to say how well you know the age based on the standard deviation. You have to factor the square root in in that analysis.

DR. CROWE: Well, that's not addressing the question. The issue is--

MR. TURRIN: Well, I mean, in your data sets that you-- in your handout, if you take the standard error or the mean of the numbers you've presented, you get standard errors on the order of 20 to 30,000 years.

DR. CROWE: All right. Let me try this a little dif- ferently. If you read the literature on what you see is one point that many workers make, Harrison in particular, is that you have to be insured that all your errors are analytical and you have to have a Gaussian distribution. Yet, you've got a distribution that you showed was non-Gaussian. You had to remove data in order to make it Gaussian. What's the basis of your extreme confidence that you've done that correctly and, therefore, have eliminated any non-analytical term?

MR. TURRIN: I thought I addressed that in the talk and that was based on compositional information from the neutron activation of potassium-calcium. The anomalously old ages have potassium-calcium ratios which you would expect from rhyolitic glasses and that's again published in the Science paper. Those data are available. And, moreover, if you use a number of different rejection criteria, one that we prefer to use is the one by Ludwig, it identifies the same four points that we've rejected as being not a part of a population.

DR. CROWE: Sure. A standard outline method does that, as we pointed out--

MR. TURRIN: That's right and when you--

DR. CROWE: The point is really--is not the details of these arguments. Basically, if you take the standard mean with the standard errors, we don't really have a conflict here. What is in conflict is your assertion that you can define this age so precisely. And, again, I have to question how you can do that when there is some evidence of excess argon. Your comment about Jane is a bit unfair because what is important is volumetrically how large that olivine component is.

MR. TURRIN: We've done that calculate--the comment about Jane's presentation was is making the volumetric corrections. You take her values, which I believe is approximately 10^{-9}cc STP, you divide that by 22.4 liters to get moles per gram. You get 10^{-13} moles per gram. Then, if you take the volume calculations she lists which is 2%, you get 10^{-15} moles per gram and if you--

DR. CROWE: But, that's not the point--

MR. TURRIN: No, let me finish, please.

DR. CROWE: Sure.

MR. TURRIN: And, if you take that and compare to the radiogenic argon we've measured in these rocks, we get on the order of 210^{-13} moles per gram--

DR. CROWE: No argument with that--

MR. TURRIN: --is essentially 1%.

DR. CROWE: I'm not arguing with what you're saying. You're completely missing the point. The point is how do you know what the volumetric component is of all of the--or, perhaps, even glass phases that you probe with your argon/argon method? You don't know that. You're assuming that the weight percent of the total olivine distribution sample is the only component contributing to that. How do you know that what you actually probe and fuse has that same abundance as the total rock?

MR. TURRIN: By doing a lot of analyses and then plotting among the isochron. You get error ratios. Analytically, you get error ratios. Whatever excess argon is hidden in there is small and is not observable in the volumes that we're dealing with.

DR. CROWE: Well, unfortunately, you missed some of the talks yesterday where there seems to be quite a bit of evidence that there is excess argon. But, the whole point is--I mean, we can argue about--

MR. TURRIN: What evidence? I read through the handouts, Bruce, and Jane is the only one that presented any information about excess argon.

DR. CROWE: Well, let me try on more tack and I'll give up here. One of the difficulties I have is that you are so willing to discard all other sources of data. You dismiss the helium, you dismiss the TL, you dismissed the geomorphic and the soils data, and the issue that we're trying to deal with here is we have to gather a number that we can confidently go into a licensing arena and say this is the right number. And, one of the points that I have required all workers who work on the DOE project to do is very carefully point out the strengths and weaknesses of their method and the uncertainty and the assumptions that you make when you do this. And, yet, you've been willing to discard large volumes of data, field relations, soils relations, geomorphic relations, et cetera. What's the basis of your confidence that you can do that?

MR. TURRIN: There is no conflict in the age data you have, Bruce. The helium ages are minimum ages. They're entirely concordant with an age of 120,000 years. They're exposure ages, they're not eruption ages. So, all those deposits based on your helium data are that age or older. And then, your uranium ages, Murrell has reported and you report in your paper are concordant with our ages. Another known chronometer gives you an age of 100 kilo annums; I believe it was 150 plus or minus 30 to 40,000 years. On your solid, well-documented, well-published chronometers you get concordant ages of about 100 or 120,000 years.

DR. ALLEN: Well, I wonder if we might move on?

MR. TURRIN: Okay.

DR. ALLEN: I appreciate these are technical issues and the purpose of this Board is to listen to technical input and we appreciate the discussions we've had here. We do have other--and, hopefully, you might think about some of these issues that are appropriate to return to this afternoon at the roundtable.

I'd like to introduce Duane Champion.

MR. CHAMPION: Good morning. In my talk this morning, I would like to bring you up to date on our volcanic hazard work in the USGS as it relates to the suitability of the Yucca Mountain Repository. I have now completed all laboratory work on post-Miocene basalt centers near the NNTS and can relate those results. In addition, I will discuss some paleomagnetic work at Sunset Crater, Arizona and Hidden Cone, northwest of Beatty, which has precipitated some geologic re-evaluation.

I will also use the paleomagnetic data we have generated to evaluate two models of episodic behavior which have been presented for the million year old centers of Crater Flat and for Lathrop Wells. I will finish with a discussion of the characteristic of the southeast flank of the Lathrop Wells cone. By the end of my talk, I will show that the paleomagnetic data still fails to find the signature of remnant directions recorded in these basaltic centers signifying the misleading nature of the polycyclic model.

John Geissman explained yesterday the paleomagnetic analysis that is useful to the study of these volcanoes. So, I won't repeat much except the basics for those who may not have been here. Paleomagnetists collect directions of samples in volcanic units to ascertain the inclination and declination values recorded in those units. The technology is over 30 years old and very robust. The fact that John Geissman and I agree as well as we do, I think, is tribute that we're not fluttering on the edge of technological development.

Can I have the first slide? Records of the directional change through time, such as this from Hawaii, for the last 3,000 years document the rapid and seemingly random nature of this geomagnetic secular variation. Don DePaolo, I'm sure you correct in your analysis of the secular variation that you measured from Robin Holcum's 1986 paper with about one degree per year of annual variation, but this record which is 10 years--well, benefits from 10 years of additional data generation has a rate more like 8 degrees per century. Robin and I had an unresolved debate about the way he averaged data together in that SV record.

It was suggested yesterday that there's a need to be conservative in the perception of secular variation. And, this conservatism can only arise or cause difficulties under one direct circumstance and that is if secular variation is suspended. I will show a lot of data that shows single directions of magnetization from individual volcanic units and I suggest that that means groupedness in time. If secular variation is suspended temporarily, it would undo this conclusion. But, it's unlikely that such a suspension can occur because I view secular variation as fundamentally weather of the earth's core. It's one of the most fundamental indicators we have of inner and outer core processes and it's like asking the weather not to occur someday. I mean, you can suspend it subtly, but not indefinitely. We have 200 observatories around the world that take measurements every few minutes because this is such a dynamic, an important variable of the earth's behavior.

If we go to paleomagnetic records, we find evidence that this continually varying signal is recorded. This record from a model lake from 13 to 36,000 years shows directions of declination and inclination varying back and forth. It includes the model lake excursion here at about 30,000 years. This is an eight meter section of wonderful lake sediments that did a particularly good job recording secular variation. Another record of inclination from an ODP core in the Gulf of California from Levi & Karling shows 60,000 years of inclination variation and it just varies and varies and varies. It just keeps going. And, the longer record of secular variation recently presented by Kerr and colleagues from Lac du Bouchet shows 120,000 years of secular variation from a lake in Europe. And, again, the inclination and declination columns just show continuous variation.

This is essentially our entire time frame of our consideration today. There is no evidence in this record that secular variation was suspended for any time frame of any importance.

If you lack detailed time control, then you can still collect directions of magnetization and not be able to draw a path of variation through time, but still examine the overall range of the data. On this diagram of Holocene directions, I've outlined the usual outer range of secular variation as plus or minus 25 degrees of angular variation with this roughly triangular shape which will appear on several subsequent diagrams. Theoretically, paleomagnetism views the variation of circularly distributed and uses an inverse measure of dispersion called kappa to describe the variation. Here, we see the dispersion for kappa=30 which is a good number for secular variation through time and we see that it's clotted toward the center as the real data was on a previous slide with 95% of directions within 30 degrees of a mean direction.

As Don explained yesterday, similarity of magnetic directions from a number of volcanic units can be used as evidence that they have been formed at the same time while different directions are hard evidence that the sampled units are not the same age. Polycyclic volcanism which is thought to manifest in eruptions at volcanic cinder cone center that are separated by up to tens of thousands of years should produce multiple magnetic directions in the volcanic pile aggregated through time.

Let's review the basaltic centers of post-Miocene age located near Yucca Mountain and see what they record. We'll look at the centers oldest to youngest.

A recent re-evaluation of the stratigraphic position Thirsty Mountain has suggested to Dave Sawyer, Scott Minor, Rick Warren, Bob Fleck, and the other members of the USGS DOE Weapons Project that the 8 million year age associated with that shield volcano is incorrect. They have embarked a chemical and stratigraphic study which is being assembled in manuscript form now. Fleck, who by the way is here today, has also done three pairs of tests of K-Ar ages shown in this table which documented an age of 4.63 million years for the shield.

Mark Hudson of the Weapons Project and I have collaborated on getting some paleomagnetic data from the southern, western, northern, and summit areas of the shield and the mean directions are shown in this diagram. They are reversed in polarity and all show south/south-easterly declinations and moderate inclinations. Use of a statistic presented by Bogue and Coe to evaluate the randomness of a population of magnetic directions suggests only one change in 100,000 that these five directions are randomly acquired in time. So, we can show that large volume basaltic eruptions have occurred in the general vicinity of the repository site long after the silicic eruptions were finished and that the duration of the eruption was short compared to the rate of secular variation. There is no hint of polycyclic volcanism in the data. We note that Bruce Crowe has already included

Thirsty Mountain in his Crater Flat Volcanic Zone.

No polycyclic model has been presented for the relatively voluminous basalt eruptions in the southeast corner of Crater Flat which occurred at 3.77 million years. Under the assumption that it may be offered in the future, we have taken six sites in these lavas, including one in the up-faulted block on the west side of Yucca Mountain. These lavas are eroded and sometimes buried by alluvium, but good paleomagnetic sites were found in vent and lava flow facies. We find reverse polarity in these sites appropriate to the Gilbert Epoch and again we see well-grouped data, but this time close to the limit of usual secular variation. By moving 10 to 12 degrees from the average reverse polarity direction, the Bogue and Coe statistic begins to suggest powerful correlations with the odds of these six sites being a random draw of secular variation at one part in 100 million.

Buckboard Mesa with an age of 2.92 million years is the next youngest basaltic center near Yucca Mountain and it, too, is relatively voluminous. It has been included or excluded from volcanic hazard consideration depending on who you read. So, we felt compelled to collect paleomagnetic data to evaluate its possible polycyclic nature. The reason cited for its exclusion from the Crater Flat Volcanic Zone were its slightly evolved chemistry and voluminous nature.

As Thirsty Mountain has been included, the reasons for excluding Buckboard Mesa no longer exist and the Crater Flat Volcanic Zone needs to be further redrawn.

Three sites taken in southern, central, and northern areas of the flows all record a steep, westerly declined normal polarity direction, appropriate to a time frame within the Gauss Epoch. These directions or at or outside the usual range of secular variation. So, although we only have three sites, the odds they are random in time is only three parts in 100,000.

Again, we have evidence of a short duration eruption at Buckboard Mesa. One of our sites was taken in a flow located just north of Danny Boy Crater, a flow which was discussed at the last panel meeting as possibly locally vented and not from Scrugham Peak. The lava we collected was oxidized and vesiculated in a manner that suggested a flow of remobilized spatter, though no obvious vent structures presented themselves. Its direction was identical to those from the main flows. So, it shares the same eruptive episode even if it is locally vented.

If we jump to the western side of Yucca Mountain, we find the next youngest basalt centers, those of the 1.1 million year vents on the floor of Crater Flat. There are four principal eruptive centers up to 12km apart, aligned in a gentle arcing trend to the north/northeast with the northern-most vent at the western foot of Yucca Mountain itself. Samples have been taken in vent areas, from perched lava lakes, dikes, and from lava flow outcrops. Until recently, no geologic maps for these centers have been presented, but polycyclic models were suggested at the last meeting of this panel based on satellitic cone scoria and on a single anomalously high K-Ar age date.

The directions of magnetization found from 20 sites in these 1.1 million year centers have reversed polarity with somewhat steep inclination values and they group well. By comparing the least common and most common directions, it is possible to bracket the range of probabilities of randomness in this data and they range from 10^{-16} to 10^{-34}. A more conventional geologic model would suggest that the four different basalt centers would each be monogenetic and independent eruptive events.

This plot shows the mean directions of averages for individual sites at each center and demonstrates, once again, the very strong grouping. They group so well, there is only four chances in a million that they are randomly selected from secular variation. Not only do we have evidence of polycyclic eruptions, but we also have evidence that the four 1.1 million year centers share the same age. This paleomagnetic conclusion was heralded by earlier conventional K-Ar dating, in that data from 26 extractions shows very similar age results for the four centers and an overall age of 1.04 million years.

Geologic maps of the Red Cone and Black Cone centers were recently presented at a meeting of the ACNW on Quaternary dating and also in a journal article. In the article on hazard probabilities, there was concern expressed in regard to ascertaining episodicity within these basalt centers. And, yesterday, we heard Frank Perry say that the northern and southern flows of Black Cone were not chemically relatable and indicated they must be due to time separated episodes of polycyclic volcanism. I have shown our paleomagnetic site distribution on this geologic base and can report that four of the indicated volcanic units at each center have at least one paleomagnetic site in them. We have already seen that the directions of magnetization are very grouped. There were only six chances in 1,000 that the mean direction of Red Cone was different from Black Cone. The Bogue and Coe statistics on the mean directions calculated for each of the four map units at each center are three parts in 10,000 for Black Cone and three parts in 100,000 for Red Cone. There is little possibility that the new stratigraphic units at Red and Black Cones are separate in time. For those keeping a tally, we are not in consensus if significant time division is required for Frank's interpretation.

Care must be taken in the volcanic hazard evaluation of the 1.1 million year centers when you tally the number of eruptions. There are four if you are evaluating a spatial term, but there are only one if you are counting episodes in time.

The two relatively low-volume cinder cones located just northwest of Thirsty Mountain are included within the northwest-trending Crater Flat Volcanic Zone, though they are 47km from the repository site. What you see in this photo is Little Black Peak on the left center and Hidden Cone perched on the northern flank of the older and more silicic Sleeping Butte. Geologic maps of these cones have recently been presented and I have shown the location of my sites on that format. I have taken samples from the cones themselves, shown in pink, and from sites in spatter, dikes, and lava flows. Polycyclic eruption models based on detailed geomor-phic and soils analyses have been suggested for these cones with episodes at 285, 200, 100, and 10 kilo annums.

The distribution of normal polarity directions is again limited, although the color coding suggests that some difference in direction exists between Hidden Cone and Little Black Peak. Looking at mean directions calculated from the individual site means for each cone, we find they have a small 4.5 degree angular difference. Our randomness statis-tics suggest there's only a 7% chance that this difference has any significance in time. We can support the idea of a single essentially monogenetic eruption episode if we look at the existing K-Ar age data for samples from the two cones which Bob Fleck has averaged for me. With 14 extractions from Little Black Peak and 12 from Hidden Cone, a single episode of 353,000 years is indicated. The new $^{40}Ar/^{39}Ar$ age that Brent just described also supports this age assignment. The paleomagnetic data constrains this episode to about a century of time.

Recent geologic work by Dave Sawyer and Bob Fleck of the USGS DOE Weapons Project has added to our understanding of the Hidden Cone eruption story. They noted the existence of a flow to the north of Hidden Cone not reported previously. This air photo from BLM sources shows this young flow with tongues reaching off to the northwest. Here's Hidden Cone here, the flow reaches off to the northwest, and a separate flow arc shown here comes off the northeast. Basically, the outline of the new flows is here.

Three new paleomagnetic sites have been located in these northern flows and they are shown here in green against the backdrop of the red mean direction for Little Black Peak and the blue mean direction for Hidden Cone. What you should note is that the new northern Hidden Cone sites agree better with Little Black Peak mean direction than they do with the previous sites from Hidden Cone.

I have two interpretive choices. I can embrace these new directions as a manifestation of dispersion with Hidden Cone and just average them in with the other six sites. This will drastically reduce the angular difference between the means of the two centers and improve the interpretation of monogenetic origin common to both centers. The interpretation I prefer is that the northern Hidden Cone flows represent the same exact episode as that which produced Little Black Peak and that subsequent eruptions emplaced the eastern flows at Hidden Cone. I say subsequent because the site at the cone rim of Hidden Cone, and presumably one of the last eruptive products, agrees with the eastern flow.

The grapevine has suggested that a high degree of resistance to the discovery of these northern flows exists, tied to the though that 9 million year flows have been mistaken for the flows of Hidden Cone. This is analytically impossible as the new $^{40}Ar/^{39}Ar$ age that Brent reported is on these northern flows. And, the older flows are very well behaved with a range of ages of between 9.70 and 9.29 for a stratigraphy of three different basalts sandwiched under, between, and over the Paiute Mesa and Trail Ridge Tuffs. Confusion is impossible here. A new geologic map of Hidden Cone is being prepared. The photogrammetry is done and it is being inked. It will be released as either an Open-File Report or a miscellaneous field studies map in the future.

I won't dwell on the Lathrop Wells data that is now complete except in quick review. This map shows the location of the 26 sites taken in these flows and spatter deposits. There are seven different eruptive units mapped here. A simplification of the map suggests there are two easily recognized units; one older and mantled by cinders and a younger flow to the east shown in green which is essentially unmantled by cinders.

These are the directions of magnetization derived from Lathrop Wells units. They are color coded by unit assignment, but cluster so strongly they are difficult to resolve. Little overall magnetic variation is indicated for these sites. If you average by geologic unit, small angular differences emerge. We feel these differences are real and require some 50 to 100 year duration for the Lathrop Wells eruptions. If we assert these means are significantly independent in time, we can limit that possibility to one part in 10,000.

As John Geissman pointed out yesterday, all is not goodness and light in paleomagnetically sampling the Lathrop Wells center. This photo shows the blocky outcrop of flow Ql_6 in the vicinity of the old cribbing on the southwest side of the center. I wish I had had a D-8 dozer to rearrange the outcrop for I could only place the site in the biggest and deepest route blocks in this area under the hope of obtaining a coherent result. This hope was not realized, as you can see by these results and the very large circle of 85% confidence. Detailed step-wise thermal demagnetization revealed no partial thermoremanence in these samples, indicating this dispersion occurred in cold blocks jostling on the surface of the flow. The overall result is not without some coherence as the mean direction is clearly of viable normal polarity, but it is utterly useless to secular variation study.

John Geissman has produced coherent Ql_6 data for other outcrops and they show a direction that is insignificantly different from our earlier work. A new tripartite chronostratigraphic framework for the Lathrop Wells center was presented by Crowe, et al., this spring which grouped previously designated geologic units. They are shown here with the color coding of blue for the eldest, green for the intermediate and most broadly distributed, and red for the youngest unit, essentially the cinder cone itself. It was incorrectly asserted that my distribution of sites, shown on this figure as red and blue dots, were inadequate to evaluate possible time differences between the three chronostratigraphic units. This assertion is untrue with a possible exception of the cone for which I have only two sites of 43 samples total.

I have re-averaged my sites in accord with the new stratigraphic model and they are shown here with the same color code as the previous slide. The angular range of the

Lathrop Wells data which once was 4.5 degrees has been reduced to 2 degrees with a zigzag pattern of movement through time. Jack Evernden, here's a perfect example of the concerns you expressed yesterday. The declaration of differences is directly tied to how they were tied to geologic units. The signal comes and goes as a function of geologic assignment.

The Bogue and Coe statistics suggests there was only one chance in 10,000 that these new chronostratigraphic units are random samples of secular variation. We prefer our original analysis reported in Science with the identification of a very short time break in eruptions which all occurred at about 125,000 years ago.

We have studied other youthful basaltic centers in the vicinity of Yucca Mountain, such as those at Pisgah and Amboy Craters. The pattern is now easily recognizable to you all. Both centers group well with the three steeply magnetized sites from Pisgah non-random at one part in 10,000 and the five very shallow sites in Amboy non-random at six parts in 100 billion. These are not polycyclic lava fields.

And, just to show that others have produced the same sort of paleomagnetic work from basalt centers, we have John Geissman and his students' work at the Albuquerque volcanoes as an excellent example. This geologic map by Vince Kelley shows vents in an alignment of at least 4km long and flows broken into eight geologic units. John's directions document extremely unusual, but tightly grouped directions for lava flows of Brunhes Epoch age as they are over 120 degrees from the expected normal polarity direction, here shown by a star. My computer algorithm for the Bogue and Coe statistics shows only six digits of information and when I ran the Albuquerque volcano data the answer came up all zeros. I can, thus, say for sure that the probability of random acquisition of these eight directions is less than one part in 10^{-42} Other trials suggest the real number is less than 10^{-300}. I think we know from this data that this center was formed during a quick monogenetic event and the polycyclic model is not indicated once again.

Yesterday, Steve Wells presented a new geologic map for vent area of the Cima A cone and compared the stratigraphy there to the Lathrop Wells cone. This vertical photo shows the youthful appearing cinder cone and flow of Cima A. Six sites were located in the lava flow and vent facies outcrops including the cone rim. Three sites were located in deposits proximal to the QV-2 vent which is thought to be a source of tephras older and separated by a soil from tephra deposits of QV-3.

The paleomagnetic results dictate that all younger Cima A eruptive products share a single episode of eruption and a single direction of magnetization. A randomness test on these directions which are 25 degrees shallower than the average normal polarity direction returns odds of 1 in 100 trillion they are random.

We are in direct contradiction to Steve's interpretation of two young Cima A eruptions. We feel that data taken from Sunset Crater, Arizona are excellent analogs to the short eruption duration of basaltic centers near Yucca Mountain. New sites have been located in the Kenna flows and the flows of the Vent 512 and they have confirmed and tightened our short record of stratobound secular variation. Here, we know these directions were acquired in a time frame between 1065 and approximately 900 A.D., 200 years, 150 years. Tree ring dating confirms enduring eruptions as disturbances in the growth pattern of trees northeast of Flagstaff.

Thousand of years of polycyclic volcanism are not available to produce the directional variation. The regional record of archeomagnetic variation passes right through the directions in the proper chronologic order.

A few comments on the Lathrop Wells cone, itself. It was stated yesterday that Brent and I came to declare a cone apron for Lathrop Wells on the basis of our granulometry measurements. It was actually through an entirely different route. This is the topography of the 1982 seven and a half minute topo map. In pink, I've shown the area of quarrying.

By 1987, the rate of quarrying accelerated. So, we asked a topographer in our national mapping division to re-contour the quarry area. Shown in blue is the pit area where the controversial tephras, shown as a red line, lie. These deposits thicken to the northwest along the line of the outcrop, more or less thicken in that direction. He also contoured using 1959 air photos before quarrying to produce this map which has the then quarry shown in pink, an area of sand overburden which was being skinned off with a dozer in yellow, and in blue is the location of our pit which I'm sure we'll visit tomorrow, circa 1987--well, this year. You can see that it is below the break in cone slope, but still close where contours follow cone shape. This is the position where cone aprons form and we relate the controversial beds to the processes which usually form cone apron deposits. This proximity is the explanation of the northwestward thickening of the tephra enclosing stratigraphy.

Paleomagnetic studies in basalt centers near Yucca Mountain systematically fail to show the paleomagnetic directional diversity that thousands or tens of thousands of years would produce. Coincidence cannot explain this failure given the total number of trials I have performed. The eruption durations must be much shorter, on the order of years to a century. The petrologic models which are now insisted to need thousands of years to produce variation require re-evaluation. At this point, the contradictions reveal little consensus and the vitality of polycyclic volcanism is in the balance.

Thank you.

DR. ALLEN: Thank you, Duane.

Are there comments or questions from the Board? Bill Melson?

DR. MELSON: Duane, I remember last year when you presented this tight cluster of paleomagnetic positions and how convincing your arguments were. Really, this is more of a comment than a question. It seems inescapable and I don't know where that leaves us because I think some of the previous information and the work that's been done before is equally competently done and so I think it's important that we resolve the really elegant story I think you have with the other results. I hope we can do that.

DR. ALLEN: Leon Reiter?

DR. REITER: Duane, you sort of dropped a point which I didn't quite get and I think it's maybe quite important. That is the--based on the inclusion of Thirsty Mountain, Buckboard Mesa, it now has to be included in the Crater Flat zone. This is a strong difference between stuff that Gene is doing and the stuff that Bruce is doing. What's the rationale why Buck--could you just repeat that again?

MR. CHAMPION: As I read, I believe the '89 or '90 paper in which that zone was defined, I mean it was to include the young centers of volcanism and Buckboard Mesa at that time was excluded because it was somewhat higher in silica and alkalis and was voluminous; and therefore, the Crater Flat Volcanic Zone off in the Las Vegas shear zone would include buried anomalies for which we didn't even have samples at that time, would include the Hidden Cone and Little Black Peak centers up by Sleeping Butte, but would exclude Buck-board Mesa because it was voluminous and somewhat higher in silica. I did not at that time understand the rationale for that particular declaration because I didn't think a basaltic voluminous dike was any less dangerous to a potential reposi-tory than a less voluminous basaltic dike would be. Now that Thirsty Mountain has been included and really has to be--there's already evidence that its chemistry has the same evolved aspects of Buckboard Mesa, it's older, it's 4.63 million years--I see no reason to exclude Buckboard Mesa from active consideration for at least hazard zone declaration. Whether you still call it Crater Flat Volcanic Zone is some-what superfluous.

DR. REITER: Where does Thirsty Mountain begin?

MR. CHAMPION: It's immediately this side of Hidden Cone and Little Black Peak. It's way at the end of the long trend, way to the northwest end.

DR. ALLEN: I have the sneaking suspicion there might be some comments from DOE. Don DePaolo?

DR. DEPAOLO: I'd just like to say that I think this work that you've described and also what Brent described before looks like excellent work to me and I think that the data will and should influence the final best fit interpretation of what's happened at Lathrop Wells, as well as elsewhere. I find it, you know, fairly convincing that short time periods may be involved. The apparent contradiction with the petrology is something I think, you know, it should just be left in limbo for the time being because we don't know the time frames that different magma batches could come up in the time of 100 years. We don't know that and that's one of the reasons I've emphasized that we ought to be able to--we ought to dissect what we know about time scales and volcanism from the magma forming in the source to when it appears at the surface. If that time scale somehow is forced to be 5,000 years, which I sort of doubt, then you've got a contradiction. If it could be 100 years, then there's not really a contradiction. So, I'm not sure that there's a need for polarity on this yet. But, I think what's happening with these new data which I think are extremely important is that the focus is sharpened. It's not that we're necessarily are going to end up at a far different point than where we started, but we're going to be more confident that we know what happened.

I wanted to ask you one detailed question, though. You showed the paleomag SV curve that went back 120,000 years. And, one thing I noticed on there was that the bottom part of the record seemed to be a little more quiet than the more recent time. I mean, is that real or is it a digenetic degradation or do you think it--does it affect--I mean, do you take that into consideration that the weather might have been there, but not as extreme?

MR. CHAMPION: It's concerning the record for declination drifts off to non-zero values. The overall signal mutes either by digenetic process or some such thing. Sediments are imperfect recorders. They're not like lava flows which do an excellent job of recording directions of magnetization. Sediments integrate the signal through time. So, sedimentation and digenetic parameters can be destroying what once was an excellent record. But, there are hints of variation. We don't get flat-lining. Flat-lining doesn't occur in secular variation records. Variation is the constant.

DR. DEPAOLO: But, do you have an idea of how much difference in K that represents when the amplitude of the SV variation gets to be small back about 80 or 100,000 years or so?

MR. CHAMPION: Oh, yeah, it would go up near 100, probably. Instead of being 20 and 30 which are the sorts of values people imagine for secular variation, when I spouted off 40 for my Holocene work in my dissertation, they're going to say, oh, you didn't sample all the frequencies. You know, it's lower than that. So, that record would be more like 60 or 80, not like a good--well, an adequate paleomagnetic site gets you up to 100.

DR. DEPAOLO: It would increase, and maybe not enough, but it would increase the possibility of there being random selection--

MR. CHAMPION: Coincidence

DR. DEPAOLO: Four or five degrees, say, difference or something like that.

MR. CHAMPION: It could increase the potential for random coincidence, you bet.

DR. DEPAOLO: Okay.

DR. ALLEN: Other comments or questions?

MR. GEISSMAN: Duane, first of all, I'd like to reiter-ate Don's comment. It's great to see all of these data. It's a rather impressive amount of work, first and foremost. Second, thanks for bringing up the subject of the Albuquerque volcanoes. To my knowledge, nobody has argued that that is a representative polycyclic field and in that discussion with my close colleagues certainly we've never had any sort of conclusion toward that line.

There are a couple of issues that I'd like to bring up just for the record. And, I understand the mechanics and the background of the Bogue and Coe algorithm and their model. It, indeed, is a model based on the best guess for secular variation averaged over a long period of geologic time. It's a best guess. And, there are a number of problems. One is that the geomagnetic field in any locality is, in all likelihood, not circularly distributed. It is elliptically distributed. So, that complicates the issue to a degree. Number two, we now have evidence that averaged over periods of time the geomagnetic field does not behave like an axio geocentric dipole. So, the expected axio geocentric dipole direction that we might calculate for a locality is a best guess, but it might be in error. If you look at the Schneider & Kent DSDP data which they report for the last 4 million years or so, we do see some inclination biases depending on what polarity--

MR. CHAMPION: Of what--

MR. GEISSMAN: Don't misunderstand me here. I'm not saying that this should drive us. I'm just saying that the calculations that you use might have some further limitations to them. Okay?

Your issue of secular variation, I don't think anybody argued with you or implied that secular variation is ever suspended. But, the amplitude of secular variation clearly, as you've shown in these diagrams, changes with time and I think one point that needs to come out and it's very important is that there is a probability, especially when you take into consideration the errors associated with our sampling and measurement techniques that the geomagnetic field as we record in the spotty record of young lava flows will come back to itself, so to speak, if you understand what I mean. You look at your secular variation records which you're reporting here and there is an average. The earth's magnetic field does come back to something of a near similar spot which we oftentimes, if we're allowed enough information over geologic time, refer to as the time averaged axio geo-centric dipole direction. But, what is that? And, we still don't know enough about the geomagnetic field behavior. As you've pointed out yourself, we need good information from good recorders and sedimentary rocks don't always give that good information. The volcanic is always inherently spotty.

So, I think the calculations that you used, the probability calculations, need to be done with a little bit of, if you will, hesitancy in terms of the actual accuracy of those probability calculations because they're model-depen-dent.

MR. CHAMPION: Sure. No, I understand your points. I enjoyed using the Bogue and Coe statistic because it was the answer to my persistent dilemma of describing to a geologist, well, gee, how different are they or how similar are they?

MR. GEISSMAN: Oh, sure. Right.

MR. CHAMPION: I finally had a clean algorithm, a simple algorithm, to make that declaration. Your points are well-taken that we don't know the dispersion parameter that should be applied in that equation. The other two things are hard angles that we can know from our own data, but in the trials that I've done I've discovered it's not particularly sensitive to the kappa choice; a 20, a 30, a 40, they all do about the same. I just chose 30 just to be--the problem with the choice is if you use a low dispersion, a 20 for kappa, you make sites near the center near an axio dipole direction less random. And, if you go for a high kappa, well, then, sites that are out at the periphery become more random. So, I just elected to go with a lot constant figure.

MR. GEISSMAN: But then, just to re-emphasize, the calculation of the expected direction is also based on a model. It's a fundamental model in paleomagnetism and that is the axio geocentric dipole. Whether it's pertinent for a time period, even a million years back in the geologic record, it's still open to debate.

MR. CHAMPION: Yeah, but deviation from axio dipole inspection is still a very small number of degrees.

MR. GEISSMAN: Not necessarily.

DR. ALLEN: The next commenter is Bruce Crowe. Incidentally, we're running well beyond. I don't want to cut off discussion, but--I know how important it is for a final conclusion. I think it's important to go through this even if we have to push our discussion a little later this evening.

Bruce Crowe?

DR. CROWE: Thank you. I'm going to make a comment, Duane, and not a question. So, I just thought I'd warn you beforehand. The concern I have basically is this. We've worked hard to try to reconcile your paleomagnetic data in all of our interpretations. What we really have to plead for is some objectivity on your side at being sure that you explain carefully what your assumptions are, what your limitations are, and argue about what's permissive versus speculation versus definitive data. I think the difficulty I have is not necessarily with your data set, but with the presentation that's geared primarily toward confrontation and disagreement. I mean, actually, there's quite a bit of areas where we agree in these things and what I find difficult is that you're focusing on everywhere where we can disagree.

I think if we're going to reconcile the different views, we have to learn to work together with professional objectivity. The bottom line is how do we bring a data set together that we can convince the scientific community we've done a correct job? What I find the most difficult on both your part and Brent's part is your willingness to dismiss alternative data. Now, you may be right. I will not argue that you're absolutely wrong, but I find it difficult to understand how you can dismiss when I think we have lots of types of data we would like to reconcile. And, the way to do that is to work your data together and work somewhat compatibly. I mean, there's nothing wrong with disagreements. That's healthy. But, when the disagreement becomes so polarized that you lose the ability to try to resolve and work to consensus, then it becomes a real problem.

And, I think what I would like to just finish that with is a plea asking for more objectivity.

DR. ALLEN: Any further comments? Duane or anyone else? Jack Evernden?

MR. EVERNDEN: As far as I'm concerned, consensus in scientific investigation has to be one of the stupidest concepts I've ever heard of. You seek truth, you throw people out, you cast concepts to the floor, but you seek the answer. You do not seek consensus. I think that's just crazy.

DR. ALLEN: Would anyone else like--

(Laughter.)

DR. ALLEN: Frank Perry?

DR. PERRY: Duane, one of the most important pieces of evidence, I think, for a long time between magma batches is the quarry exposure where you have a basal tephra separated by a soil, as interpreted by Les, and a tephra above and those are clearly two--you know, I believe, two separate magma batches.

MR. CHAMPION: This is in the pit?

DR. PERRY: Right. Two tephras separated by a soil which I think are clearly two different magma batches. Also, the upper tephra, I think I showed clearly yesterday, is not derived from the main cone and it cannot be any type of cone apron deposit. It's simply not the same scoria as exists on the cone. I just want to hear your comments on what you think this soil--what you think of the soil interpretation and how the fact that this is not the same tephra as is present on the cone fits with this mass flow model?

MR. CHAMPION: The point of your--I think it was sample number 78 which was high in the tephra section in the pit not coming from Lathrop Wells cone itself ties with the reassignment of the Lathrop Wells cone to a 65,000 year age according to the helium dating. You're then left with no source for your tephra. You've failed to solve the source for your tephra problems.

DR. PERRY: Right, but it's clearly not from the main cone.

MR. CHAMPION: So, where is it from?

DR. PERRY: We don't know the source of every tephra, but we can rule out some sources.

MR. CHAMPION: And, yesterday, I was just--that was great. You found strong correlation between one of the bombs that hung out of the wall low in the main tephra sequence and correlated that with the Ql_6 flows which are, I think everyone would understand to be, relatively low in the overall stratigraphic sequence. So, it's neat to find direct chemical ties, there's the fragmental part of that batch, there is the flow part of that batch. But, my memory is that there was never any evidence found of any strata form break or soil break or erosional unconformity within the main cone sequence. Has that story changed now?

DR. PERRY: No, that story is--I'd have to let Les answer that. That story is still--

MR. CHAMPION: I think it's still intact. So--

DR. PERRY: But, what--do you think that that's not a soil between those two tephras?

MR. CHAMPION: No, I'm talking about the soil--you know, the absence of stratobound break between Ql_6 and the cone results on one of your plots. The cone results were way off, Ql_6 was down lower. You've got a bomb from low in the cone now that you use with the Ql_6. You argue that they're separate batches in time. Please, show me the break in time in the cone sequence then.

DR. PERRY: That's a perfectly legitimate question. We want to know where the soil is, we want to know why--

MR. CHAMPION: And, the fact that it's not been recognized isn't a problem, but if you're correct, it should be recognized.

DR. PERRY: Right. I'm not sure. I'd have to, you know, give that to Les. But, where there is a soil between two--what we interpret as two different magma batches, do you agree it isn't soil or--I mean, either it's a soil with a lot of time represented or it's not.

MR. CHAMPION: I still have doubts that the deposits are volcanic products. I'm not convinced at this point from what's been presented that they are volcanic eruptive products.

DR. PERRY: Okay.

DR. ALLEN: Okay. Thank you, Duane. Incidentally, it would make it much easier for the reporter to keep track of what's going on if we could written data from you, the same way we have from all the other participants.

MR. CHAMPION: Okay.

DR. ALLEN: Thank you.

I think we'll forego the Kip Hodges talk at the moment and come back to that right after lunch. It's going to push us a little bit later into the afternoon, but I think this discussion was worthwhile. We appreciate the presentation by the two of you and your participation.

So, we'll try to reconvene at 1:15.

(Whereupon, a luncheon recess was taken.)

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

DR. ALLEN: May we reconvene, please?

Some people have asked about the logistics of the trip tomorrow. Jeanne, could you perhaps brief us on where we're going to be in the morning and so forth?

DR. COOPER: The latest information that I have--I'm sorry, but Ardyth Simmons really has done a lot of the organization still for this meeting. But, I think the plan is still to meet at the Valley Bank Center in the front. I believe we're going to meet in the horseshoe area right in front there where the fountains are. We'll have about, I would say, 10 4-wheel drive vehicles.

DR. ALLEN: And, we're meeting at 9:00 o'clock?

DR. COOPER: 9:00 o'clock is the estimated arrival time at Lathrop Wells and it's a two hour drive.

DR. ALLEN: Okay. May I remind the people who stay in the hotel here that box lunches are available, but not in significant quantities from the restaurant here.

MR. CARL JOHNSON: Jeanne, is the schedule firm that we meet at Lathrop Wells cone at 9:00? So, if for some reason we don't go to the Valley Bank Building, we can meet at Lathrop Wells at 9:00?

DR. COOPER: Yeah, 9:00 o'clock, we're going to meet at Lathrop Wells for those groups that have--

DR. ALLEN: Yeah, at the point where the entrance road leaves the highway, not at the cone.

DR. COOPER: The plan is to have a short overview and gathering right at the gate where we drive up the road to the cone. So, I guess I would ask if anyone is going to make plans like that to drive and meet us at the cone to please let Ardyth Simmons know that so that we don't wait for you at the Valley Bank Center.

DR. ALLEN: Okay. Any other statistical or administrative announcements?

DR. COOPER: Can I say one more thing?

DR. ALLEN: Please?

DR. COOPER: Just so people know this, it's a long day. The scheduled arrival time back in Las Vegas is 7:00 p.m. So, keep that in mind.

DR. ALLEN: Okay. To finish up the morning session, we have a presentation by Kip Hodges of MIT. Kip was one of the independent outside reviewers of the DOE's early site suitability evaluation familiar with the program and some of the controversies for that point of view.

Kip?

MR. HODGES: Thanks, Clarence.

I want to start out by saying that I have not worked a single day at Lathrop Wells, but I do think it's a really interesting problem from a scientific perspective and from a societal perspective. And, as I always tell my graduate students, when you really want to understand a problem, the best way to understand it is by effectively writing a research proposal. In other words, how would you approach this problem were you doing the work? And, what I'd like to do today is I'd like to take you with me through some thoughts that I've had basically over the last couple of days before I came here. So, this is part of the reason why this isn't in the formal packet.

But, just some thoughts I've had over the last couple of days about how this problem ought to be addressed from the perspective of, most specifically, $^{40}Ar/^{39}Ar$ geo-chronology and then, hopefully, at the end, I'll make some comments that might be pertinent to other dating techniques, as well. I have a really simple-minded way of going about things and so I prefer to begin at the beginning. And, if I insult your intelligence or bore you, please bear with me through that period. But, I just want to make a couple of comments right off the bat about potassium-argon technique and the $^{40}Ar/^{39}Ar$ technique that I think will put us all sort of on the same basis when we consider these data further.

As I think most of the people in here know, the potassium-argon geochronologic technique is based on really one of the two branches of the radioactive decay of the isotope 40 of potassium. And, the one specifically we're interested in is 40-potassium going to 40-argon which is only a very small part of that total decay of potassium-40 and it has a half-life of about 12 billion years or so. Especially considering the time, I'm not going to belabor the discussion of these age equations, but one thing I think is really important in this from the perspective of the discussion we've had in the last couple of days is that in order to calculate an age from these data, we have to make some inference about the ratio of radiogenic argon-40--that's the asterisk after the argon in the numerator--and 40-potassium. And, the standard procedure for potassium-argon is that we measure potassium separately using something like flame photometry and measure argon in a rare gas mass spectrometer.

But, the problem is that argon-40 that's measured in that particular sample, some fraction of it is the amount of argon-40. Some of that signal is argon-40 that was in the sample prior to closure of that sample and some of that 40 is radiogenic, produced by radioactive decay of potassium-40. And, the standard operating procedure is to believe that the ratio of argon-40 to argon-36 in the sample at the time of closure was 295.5 which is the accepted number for the atmosphere today. This is a requirement of potassium-argon geochronology. There is no way out of that. If you want to assume another ratio, you're free to do so. You can calculate an age any way you want to, but you have to assume a ratio.

Now, basically, there were a number of problems with the conventional potassium-argon technique that prompted people to develop a little more sophisticated technique nearly 30 years ago and this is the $^{40}Ar/^{39}Ar$ technique. And, some very intelligent people made the observation that if I took potassium-39 in a particular sample and bombarded it with fast neutrons, I could convert some part of that potassium-39 into argon-39, release a proton and a little bit of energy which becomes important as we'll talk about a little bit later, and in doing so, I could use simply a gas source mass spectrometer and I could measure all my argon in the gas source mass spectrometer. I didn't have to physic- ally split my sample. And, basically, in order to determine the amount of potassium in a sample like that, we make this calculation which is that the 39-argon induced by the neutron bombardment is equal to the amount of 39 in the sample to begin with times a factor which has to do with the neutron dosage of that particular sample integrated over an energy spectrum of the fast neutrons involved in the reaction.

Now, I'm going to skip over some parts of this derivation because, after all, what we're not doing today is a class in isotope geochemistry. But, the important thing is that the age equation looks something like this in that we have a term which has to do with the decay constant, of course. We still have a ratio just as we did before which still has a radiogenic component in it and it's multiplied times a quantity J and that J is effectively a value which takes into consideration the neutron flux to the sample over the energy spectrum. And, the way we monitor that in natural samples is by taking samples of known age, irradiating them along with our unknowns, and then making a calculation for J.

Now, the important thing again is that in this age that we calculate using this simple-minded technique, we still have to have the value for radiogenic argon-40. So, on the face of things, we still have to make an assumption about what the initial ratio is. The beauty of $^{40}Ar/^{39}Ar$--and, I cannot say this more emphatically--is that it gives you a direct up-front opportunity to establish whether or not that number was 295.5.

So, depending on how you use $^{40}Ar/^{39}Ar$, you can either make the assumption that it was 295.5 and go ahead and calculate your age, or alternatively, you can look at the data on isotope correlation diagrams and directly assess this problem. So, the very fact that there may be excess argon components in Lathrop Wells, I don't think basically is telling us that $^{40}Ar/^{39}Ar$ is not the technique to use or that it's even an insurmountable problem for these particular samples. So, that's the one thing I want to get out of the way. This is not the principal issue, but it's one of the things I want to talk about.

Now, if we're interested in dating something like a sample from Lathrop Wells, the first question we ought to ask ourselves is what are the limits on a sample size? And, those limits are different depending on the analytical technique that you use. We've got two ways, basically, to liberate argon from an unknown. One way we can do it is by simply incrementally heating it in a furnace or heating it all at once in a furnace, but anyway, using a resistance furnace. And, the other way is to essentially fuse it with a laser or incrementally heat it with a laser. And, these two different techniques have different strengths and different weaknesses.

In my opinion, the principal strength of the laser approach has to do with what I'm talking about up here above and that's the blank at mass 40 for a laser is about--for my machine, anyway--1 x 10^{-16} to 5 x 10^{-16} moles. It's very straight forward to characterize what that blank is. And, by blank, all I mean is background in the system. In other words, no matter how well I pump my system, that's about how many moles of 40 I find in my system.

So, at the same time, if I look in the furnace part of my system, I find that my blank is about 1 x 10^{-15} to 5 x 10^{-15} moles or about an order of magnitude poor from the perspective of getting age information out of a sample. And, so I can make a straight forward calculation and I can say if the age of the sample is about 100,000 years and my signal to blank ratio is 10 which is about as small a signal to blank ratio as I would ever want to work with. And, the percent potassium of my unknown is about 2, which is a little bit like the basalts at Lathrop Wells, then for a laser sample I'm going to need a minimum of about 4 to 17mg and for a furnace sample about 35 to 173mg for my particular system in order to run that sample. Now, these numbers are variable. I mean, it assumes basically perfect ionization of all the material I get out of the sample. It means I get all the material out of the sample, all the argon out of the sample. It doesn't take into effect what argon-40 might have been in the sample that's non-radiogenic. But, basically, these are pretty good ball park numbers.

And, to give you a feeling for size of something like a chip of basalt, 4mg is on the order of a block 1mm x 1mm x 1mm. Okay? Something like that. So, that's pretty much, for my machine anyway, about the theoretical limit and the blanks for my machine are not radically different than the blanks at places like Berkeley or places like the USGS.

Now, let's ask ourselves the question, too, about the possible sources of error in $^{40}Ar/^{39}Ar$ geochronology. The principal source of error for samples that are older than about 10 million years is J; the uncertainty in our neutron influence because that plays back into uncertainty in the monitor minerals that we use. The J value can be monitored using several well-characterized standards and those of us in this room who do $^{40}Ar/^{39}Ar$ could probably get into a great argument about what well-characterized means in this case. But, we can appropriately distribute them in the sample container and depending on how well we distribute them and depending on which monitors we use, we can probably estimate J very well, I would say.

The other one has to do simply with the blank because not only is there a blank, but there's also an error in the blank. And, in order to really understand your blank, you're going to have to measure it again and again and again and again and again. This is a fairly straight forward thing to do with a laser. It's not mechanically difficult to do with a resistance furnace, but the big problem with a resistance furnace is that the blank itself is a function of temperature within the furnace. So, you can't simply subtract a blank for all of your samples and get away with it.

There's also a potential error associated with signal extrapolation because what we have to do is we have to take a signal that's changing with time because of fractionation in the system and we have to project it back to the inlet time. So, if we have non-linear gas evolution for whatever reason, that calculation becomes more difficult. There are also interfering reactions associated with it because when we're making argon-39, we're also making things we don't want to make like a little bit of argon-40, for example, and other isotopes. But, these are things that we can generally monitor by using natural and synthetic salts.

And then, finally, there are some potential of recoil effects which Brent talked about a little bit earlier and I'm not really going to go into because of that. I basically agree with Brent's assessment of recoil effects and their significance in the Lathrop Wells' samples.

You'll note that in this diagram, I did not include the excess argon problem because in my opinion the excess argon problem is tractable. I agree with Bruce's argument that excess argon cannot be ignored, but I also agree with Brent's argument that it isn't clear that excess argon is that big of problem. And, what I would advocate is I would advocate a systematic approach to trying to understand where the excess argon is in these samples and what kind of a significance would it make rather than backing off in one direction and saying it's not a problem and in another direction saying it is.

Okay. Now, given those kinds of sources of error, let's ask a question. What kinds of uncertainties are we likely to get in these kinds of studies and what's sort of the best we can do with the uncertainties that we're likely to get? Now, there's been a lot of talk in here and certainly in the interchange in Science about the various usages of mean weights and weighted means and all this sort of thing.

And, to me, what all this boils down to is that it's a parametric argument. Right? And, my problem is I think that the problem itself is basically non-parametric. Non-parametric techniques are principally used to try and make inferences about a population from few and messy data. I would argue that the data for the Lathrop Wells volcano are both few and messy. And, so what I'm going to do is I'm going to go through what amounts to a back-of-the-envelope calculation and try and give you some ideas of what these numbers are.

Now, what I'm going to do today is something that's sort of a pseudo non-parametric treatment of the data and we can go into this later if you want to when I'm not taking everybody's time here. But, I'm basically going to use a Monte Carlo error propagation technique to look at Brent's data, the laser data that was published in Science about a year ago or something like that. What I'm about to say has obviously no bearing on the furnace data.

Now, the simplest way of looking at this is for some three measurements of a value. We'll call them X1, 2, and 3 with some assigned standard deviations of sigma 1, sigma 2, and sigma 3 in this case. We're going to create three bins of data, B1 through 3 and each one of these bins of data contain a large number of synthetic data where a large number is perhaps 1,000. In the ones I did it was about 1,000. And, each one of these bins is going to be created such that the mean value for all of the elements in this bin is equal to X1 in Bin #1, X2 in Bin #2, and X3 in Bin #3. And then, I'm going to say that the standard deviation of the values in each one of these bins is equivalent to the standard deviations that I had previously assigned to the actual measurements, X1, X2, and X3.

One of the nice things about a Monte Carlo air propagation technique is that you don't really have to--you can easily explore the significance of your assumption that these things have a standard deviation. In other words, that this is some kind of a Gaussian situation because I did exactly the same analysis that I'm going to show you using effectively randomly distributed values within that box. So, instead of using sigma 1 of the sample as sigma of the bin, I also used it as a complete range and I doubled it to use it as a complete range. And, I can tell you--you're welcome to the code if you want to--it doesn't make a whole lot of difference in the results that I'm going to give you.

Well, then, what we do is from each one of those bins, I'm going to pick randomly a sample and I'm going to take those samples and calculate simply a mean. And, that's why I say partly this is a pseudo parametric or pseudo non-parametric way because some of the things I'm going to do is look at parameters. So, I'll calculate the mean and I'll do this for a large number of times (r), in this case about

1,000 for the samples I'm going to show you today, such that you have a population of synthetic means. And, from that, I can determine the mean of the population which is just the arithmetic mean of all those different values and I can calculate a standard deviation, as well. I can also calculate a median which I'll show you in a minute.

I'm also going to calculate a term, which I refer to here as the mean absolute deviation. There are a lot of different names for this and this is sort of more preferable in many ways if you don't really understand what the distribution is, if you don't know that something is a Gaussian distribution. And, all it is is the absolute deviation of the difference between a particular measurement and a mean divided by the total number of samples that we look at. So, that's what ADEV is. It's probably the one that you may not have heard of in what follows.

Well, in the next diagram, this is the view of the samples Q3, from Unit 3, that Brent worked on. And, this is the result plotted simply as a histogram with frequency versus age bins that I took arbitrarily at being 25,000 years for all of the data in the synthetic population that I looked at. And, as you can see, as Bruce and co-workers have pointed out in their papers and as Brent said originally this is a decidedly non-Gaussian distribution. There's a tremendous amount of skewedness toward older ages in this particular sample, but the mean of all those values is 266 with a standard deviation of 59 or 60, to use the proper numbers. And, you can see that there's a difference between the median and the mean. The mean is about 266, the median is about 258 in the study that I did in this case. So, the deviation between the median and the mean is basically a reflection of the fact that this is not terribly Gaussian in its distribution. The number I'd like to promote as probably the best estimate of the error of these particular numbers is that average deviation at 48,000 years. It's in the upper right hand corner. So, I think this is a much more robust measurement of the error on those particular points than some weighted error, okay, in a parametric way.

This is the example for the same Unit 3 where I've dropped the contaminated samples that Brent didn't like in his earlier paper. I've dropped those and I've just looked at the data without them. And, also, as i pointed out, this is a much more gaussian distribution. As you can see, the mean and the median in this particular case are identical. But, at the same time, the average deviation is still about 45. And, I think that's probably also a pretty reasonable approximation of what the mean deviation is for those particular samples. I think it's probably better than the standard error of the mean, weighted standard error of the mean.

This is the same kind of example, exactly the same thing done with the Q5 samples, the collection of Ql5, QS_5 samples. And, in this case, again this is a pretty Gaussian distribution. The mean is 133, the median is 133, and the average deviation in this case is 53,000 years.

Now, there are a number of questions that you can ask that are particularly easy to ask, as it turns out if you use this kind of non-parametric approach. So, I'm going to hide the answer there unless you guys have already seen it. And, I'm going to ask the question, are these ages reason- able? Now, one question that somebody is likely to ask you is they're going to say, okay, look, I have a bunch of datas from something that I know from geological grounds is older and a bunch of data from something that I know is younger. And, the mean age of the younger ones is older than the mean age of the older ones. Okay? So, the question is, is this reasonable? And, Brent's approach to that is to say, okay, look, these things overlap at the 95% confidence level; therefore, there's no inconsistency there. And, I, in gen- eral, agree with that, but there's a question that is so seldom asked in those kinds of parametric studies and that's what's the probability that Q5 in this case is older than Q3 based simply on the data alone. Okay? And, so the way we can do that is we can say, okay, from these bins of mean ages produced in the previous exercise, I'm going to select a pair at random. And then, I'm going to ask the question, is the 5 sample older than the 3 sample and I'm going to repeat those two steps 1,000 times and I'm going to calculate something that's like the probability that Q5 is older than Q3 based on the existing geochronologic data. And, the answer to this is that about 46 in the examples that I did--46% of the selections resulted in the correct--the geologically based age relationship.

Now, one of the things I like to do with statistics is I like to look at these things from a common sense point of view because I think the guts of something, especially non-parametric statistics, has a lot to do with common sense. So, let's try a little test. Suppose next week when you get home, you flip on the radio before you go to work one morning and it says on the radio there's a 46% chance of rain. Now, without thinking about it, raise your hand and tell me how many people in this room take their umbrella with them? That's about half. Okay. Now, I think that's a really interesting point because what that means is that good enough for you? Is it good enough that you can convince half the people in a crowd that these two things are giving you reasonable age relationships?

So, the other end of that, the slip side of that, is are these data consistent with a 20,000 year age? And, again, that's something that I can easily use the synthetic populations that I've dealt with before and I can ask that question. And, I can say it's not very likely. And, the reason is that less than 3% of all the samples in this synthetic population had ages of less than 30,000 years. Now, older numbers have been proposed at this meeting, 65,000 or whatever. But, the point is simply that these numbers-- perhaps you're not saying they're the best numbers that we can get, but on the other hand, they're not very consistent with something that's about 20,000 years old. So, you can take that pretty much for what it's worth.

Now, another way of looking at this problem, what I would do if I were writing a proposal is I would say can we do better than that? And, there's one way to deal with that and I have to apologize, there's an error in this slide. I sort of mixed myself up with Zs and Cs down here and I'll try to get us by that. But, let's say, if we expect an average standard deviation of Z for each analysis of a sample, how many samples would we have to analyze before we achieve an acceptable uncertainty, however we want to define that acceptable uncertainty on the age of a very young sample?

Well, one way to approach this problem from a non-parametric perspective is to say, okay, let's choose an age and for this exercise I said 20,000 years. And then, I'm going to create a bin with a large number of synthetic ages, again using the same techniques I've used before where the standard deviation of that bin is equal to in this case Z, not C, but Z, the standard deviation for each one of these analyses, what I think is a reasonable average standard deviation. And then, I'm going to choose C ages at random from that bin and assign each one of those ages a standard deviation of Z. Okay? The same as what I consider to be the average standard deviation. And, I'll perform a Monte Carlo procedure that I've described previously and I'm going to repeat this for other choices of C and Z and see how these look in space, whatever space I want to plot them in.

And, the two spaces I want to plot them in in this case are these. First, we'll look at average deviation for a particular sample versus the total number of samples that we would have to analyze in order to beat down the statistic which in this case is the average deviation. And, the dark solid lines that I've shown here are contours for the average deviation in the measurements. So, for example, S is equal to 50,000 years, that's if I say, okay, look, I can measure this quantity to an uncertainty of 50,000 years. And, given that uncertainty, how many samples am I going to have to analyze before I can beat down the average deviation of that population to a point that I find it acceptable? And, what I've done with the dash line here is I've arbitrarily drawn a line of acceptability of 5,000 years. This is average deviation in thousands of years. And, I took that sort of randomly to say, well, let's say, two average deviations is equal to the lifetime of the repository. Okay? So, if I do that, you can see that if I have an average deviation of about 50,000 years, I would have to run on the order of 50 samples before I could beat that statistic down. But, if I had an average deviation of 100,000 years or 150,000 years or 200,000 years, I would have to do a great many more.

Sort of a reasonable estimate for the laser samples that were in Brent's paper is about 100,000 years. Okay? And, so for 100,000 of those samples, this kind of an analysis would tell you that you'd probably have to do about 200 analyses in order to beat that statistic down to some reasonable level. Now, 200 analyses sounds like a lot, but it's not so bad in the world of laser art on 40/39 geo-chronology because we can do a sample about every 15 minutes or so. So, it's certainly not unheard of that we could do very, very, very large numbers of samples to try and address this problem.

Now, another comment that I think Don made yester-day, which is a very important one, is that there's a big difference between the precision with which we measure some-thing and the accuracy of that measurement. And, part of that is indicated in this other diagram, sort of in a back-handed way where I've looked at this in terms of number samples contoured again in terms of the standard deviation or the average deviation for these particular samples. And, I've said, okay, of all these Monte Carlo problems, what percentage of the answers, the values I got, were within 10,000 years or 20,000 years? So, in other words, how many of them were in the range from 10,000 to 30,000 years? And, so then I can also take an arbitrary percentage off of that and I can say, okay, I want 95% of those to be within 10,000 years, of 20,000 years. And, when I do that, I find that for each one of these individual average uncertainties, I have to do many more analyses to try and get that kind of information. So, for example, with an average deviation of about 50,000 years, I'd have to do upwards of 150 or 200 analyses.

Now, this kind of a mental exercise, I went through as it pertained to $^{40}Ar/^{39}Ar$. But, I could go through this same mental exercise as it pertained to helium or thermal luminescence or uranium-thorium disequilibrium and all you have to do, if you believe these kind of analyses--and, like I say, they're very preliminary, they're back-of-the-envelope kinds of calculations. I think the lesson that you learn from this sort of thing is that with the uncertainties that we can establish for a lot of the systems that we're looking at here, whether it's helium, argon, whatever, we have to do a lot of analyses. A replicate analysis is not enough, 10 isn't enough, 50 isn't enough, and I would argue that hundreds are necessary before we can really beat back these statistics to the point that you can convince people scientifically.

Then, we're going to back off and this is the part where you would write the proposal. And, in this part, I'd ask myself, what do we do now? What do you do now? If I'm DOE, what do I do now? And, the first thing I would do is I would decide how important this problem is. Okay? This has been beat around a lot in the last couple of days, but it's really fundamentally important here. How well do we need to know the age of the basalts at Lathrop Wells? And, this is something that I think Bruce is going to talk about a little bit later. And, if we agree that it's sufficiently important, then we have to decide how well we have to know the ages of these pulses of activity. Not do we need to know the age, but how well do we need to know the age? Do we need to know it within 50,000 years, 100,000 years, how well do we need to know it? And, this also pertains to the questions of polycyclic eruptions. How well do we need to know the cyclicity of those? And, if we decide that, okay, this is worth spending money on, then what you do, I think, is you apply all possible dating techniques that (a) have the capability of providing ages with sufficient precision and I would argue that $^{40}Ar/^{39}Ar$ is one of those. I would argue that all of the techniques that were discussed by Don yesterday fall into that category. And, (b) can do so at reasonable cost and that's the pragmatic part of this argument which some people may not agree with. But, how pragmatic is it to do 200 helium measurements? How pragmatic is it to do 200 uranium-thorium disequilibrium measurements? And, I don't have a good control on that. It's not my job.

And then, finally, what I would do is I would establish some appropriate statistical treatment and, as I think you can determine, my tendency is to always go with non-parametric techniques for all the methods of age determination that we're looking at. As Bruce said earlier, sit down and critically evaluate the uncertainties in a lot of these systems to a level that we've never done--well, not that we've never done, that were done by some of the people sitting in this room here for argon 20 or 30 years ago. But, they need to be done again and the reason they need to be done again is that the whole analytical ball game is real different than it was back then. We have to take another real critical look a these things. But, let's establish some appropriate statistical treatment for these methods and arrive at some realistic estimate of the true age of volcanic activity. Or, as Jack said, let's try and find out the truth.

And then, finally, sort of my last two cents worth, how would I approach the 40/39 aspect of this? Now, I'm at a little bit of disagreement, I guess, with Brent in that I think that I would use the laser fusion more than I would use the resistance furnace. I think that's his way of thinking right now. And, the reason I concentrate on that is because the laser system has far more predictable blanks. It may be that you can get blanks down better in the resistance furnace system than 1 x 10^{-15}, but they're more predictable in a laser system. And, because they're more predictable, we have a better handle on them through time. And, I also think that it's much better able to evaluate the severity of contamination by older components and that gets back to the question of excess argon in these systems. And, I think by judicious use of isotope correlation diagrams, that's a surmountable problem, certainly tractable.

Then, I guess what I would probably do is I would probably establish a low blank which was essentially dedicated to dating very, very young samples. And, basically, this minimizes the effect I'm going to have of any kind of memory in the mass spectrometer. Then, carefully, evaluate the sources of error, particularly the J values and the blank level. We talked about that a minute ago before. And then, perform a very large number of replicate analyses. And, I would say, at the very least, greater than 200 replicate analyses in order to reduce the effect of uncertainty to some kind of acceptable level. And, in this case, I'd say, maybe an average deviation of about 5,000 years.

Okay. Thank you very much.

DR. ALLEN: Thank you, Kip.

Any quick questions from the Board?

DR. MELSON: Just real quickly, Kip. Suppose you had in your large number of samples a minor event that had a different age. You started out, I guess, with the three different bins, but I wasn't clear how a small unit of quite a different age might get hidden in the--

MR. HODGES: Yeah, in the basic approach that I use here, you're assuming that you have three different measurements of the same thing, obviously. And, if you have more than one thing, then you're going to have to go to increasingly more sophisticated ways of doing this. I mean, this a really simplistic way of looking at things from the non-parametric perspective and that's why I mean you have to establish some appropriate statistical treatment.

DR. ALLEN: Okay. I think we better move on. Thank you very much, Kip.

If it's okay, I guess I'll introduce the speakers from here on out. Bruce Crowe is next in the program with the Status of Probability Studies.

DR. CROWE: Well, let me thank Kip for giving what I thought to be a very informative talk and ask him if he wants to apply for the PI position.

Before I jump into the probability talk which will change the subject a little bit, let me see if I can provide a little bit of perspective on what we're doing here. I had kind of feelings that this was Tucson '91 and we're right back there again after the last series of presentations. And, hidden in there, is something that I think maybe didn't come out and that is if you look carefully at Brent's data that he presented--and, I think Don made this point when he asked some questions--is he's been moving his age down a little bit. I mean, we were 150, 140 to 160, in '81; and now the numbers I see were like 110 to 120. And, if you look at the helium results, we've moved things up to about 65 or 70. And, it could be this is the way to win; by a process of attrition, we're beat each other to death and we'll get to the answer. But, the point is I think there has been some progress here and try not to lose this. This is not a contest to see who is the most right and who has got there first. The problem is can we bring a perspective to this, can we eventually go to the NRC with some data answers, and I hope that we're all still surviving after we get to that point.

Okay. What I'll be talking about now in this talk is the status of probability studies and what I'm going to do is basically try to start from where I left off in Tucson in '91 and introduce some new topics that I've been working on since that point.

What I emphasized in my Tucson talk was basically how we were trying to bound the uncertainty of the probability calculations because the problem that we have here is these really shouldn't be called calculations; they should be called estimations. We don't have enough data that we can do anything robust statistically with these and so we have to search for ways to bound them. And, the approach I was advocating in Tucson and which I still advocate is that we may have to appeal to kind of what are the natural bounds on how volcanism behaves as a process, what are the kind of ranges of behavior we see in terms of events per, say, the Quaternary. And, I still feel that way. I want you to remember that's where we've gone and what I'll be talking about is some different aspects now, the probability problem for this talk.

Okay. Just to overview it, I want to talk at least enough about this issue resolution we've been going through because it is a major part of what we're trying to do. We are interacting with the NRC and we had an amazing meeting in August. It's the first meeting I've ever been to--actually, it was a video conference, maybe that's why it worked--that we all walked away fairly happy. And, I'm still convinced that something is wrong. This hasn't happened in the history of the program before. But, for whatever reasons, we are seeming to make some progress, at least trying to get a joint understanding of the foundations of how we're approaching this problem and I think that perhaps it would help if we can try to do that in other things.

I will quickly show you the probability model, yet again, that Greg showed. I'll talk a little bit about where we're going on structural models and how it might help us to discriminate models, particularly the northwest versus the northeast-trending models, and what their impacts are. And then, I will make some comparison with other probability models. Particularly, one request the Board made was to compare our calculations with the State of Nevada calculations. And, what I'm trying to emphasize is areas of agreement and areas of disagreement and then emphasize topics where we may be at somewhat of an impasse and would welcome recommendations. And then, in your packet you should see the latest tables of E1 and E2. And then, finally, I'll finish with discussions of this issue of the possible presence of subsurface magma and the effects that it might have on the probability approach.

This is very hard to see, unfortunately, and so you're going to have to probably look at your packets to look at this. But, basically, we're trying to provide some logic to how we want to proceed with resolution. And, the way we're doing this is we're asking this series of questions. First of all, do we have to worry about volcanism as an issue and, obviously, we're here and we're worried about it. So, I think we've answered that question yes. The second question then becomes do we worry about it actually erupting; if we do, we go off into this side of the diagram. Or, do we worry about what Greg talked about, the probability of non-surface eruptions?

What I want to point out on the non-surface issue is that we intend to carry that down to performance assessment for resolution. And, so we're going to have to go through an iterative process of trying to look at the combination and try to give them some probability inputs, but then the final resolution will probably end up where do we violate the release standards?

Now, if we go down this side and we look at the probability of surface-breaking eruptions or a surface-breaking magmatic event, there's two questions we have to ask. First, where does it occur and the possible answers are it could occur outside of the repository or it could occur through the repository. If it occurs outside of the repository, again our view is that we're probably going to have to go through consequences. We again have to come up with a probabilistic term and so we'll have to come up with some spatial resolution that Greg talked about of looking at how large the effects could be where you don't have a direct hit and again we'll iterate down to performance assessment. The other direction we've been going here and emphasizing for most of our studies is this issue of what happens when it goes through repository? And, our decision points are basically this which is very hard to see here. It's this 10^{-8} value. We argue that if we can demonstrate that the likelihood of the event occurring and having significant effects is less than 10^{-8}, then we would argue that we have resolved this. And, the big caveat there is the uncertainty of that resolution. And, so we have a couple of other points that we can go down to. If we feel we can't demonstrate that with satisfaction, then we will again have to proceed into releases to handle that problem.

So, this basically is the strategy of what we're following and most of what you'll be hearing about today deals with this arm of this issue resolution. But, I want to emphasize that this is not the only part of the problem; that, eventually, we have to look at the contribution of volcanism to the CCDF and basically whether or not that would push it either above or below the regulatory limits.

Okay. Here is another modification of the probability calculation that the statistician has been working with me used. I like it because it looks longer and it impresses people more than what I was using before. And, so if that works, I'll take it. And, we also now call it a tripartite probability because that sounds more impressive, too. But, basically, it's the same old stuff you've been hearing about. E1 is recurrence rate, E2 is the probability of disruption, E3 is the probability of exceeding the regulatory limits. We are still applying the probability model that we used back in '82.

Ho, in his papers, has suggested that this can also be phrased as 1 minus; basically ours, we agree with that completely. And, he's also pointed out an important point, I think, that within the 10,000 year time frame, we're generally in an area of linearity of this exponential equation. Again, lambda is the recurrence rate, p is the probability of disruption, and r is the probability of exceeding the regulatory limits. We've gone over this so much that that's all I'll say.

Okay. Now, what we've worked on since we last talked to you is this distinction that we talked about a little bit in the introduction and that is that there really are two scenarios to this. The first scenario is the release scenario and that is the green arm that went down off on this side. And, basically, here we're looking at again what's the potential for rapid or catastrophic releases of radionuclides from penetration of the repository. And, here, we're formulating E1 as the likelihood of formation of a volcanic center and that has to do with how we define a volcanic event and Greg touched on that. I'll show you basically his slide to show you how we define that.

E2 becomes the probability of intersecting the repository or the probability of it intersecting the controlled area. We try to do both calculations as part of our study plan approach where we have it as a ratio of a/A where a is the repository or the area of the controlled area and A is the area of the event definition. This is fairly important to emphasize because we do have some differences in how we define that event definition. That basically has to do with the different approaches to defining where you derive your volcanic rays.

E3 again here, is the probability of exceeding eruption releases and so it's what--the release is entirely from the eruptive component in this case.

Okay. Now, the important concepts here are this linear dike model that we do have a general bit of agreement on and that is that basically basalts are fed by linear dikes. We have a lot of analog data from looking at dikes and eroded terrain that we know that what the general shapes of dikes are. There's a lot of theoretical work that suggested movement of magma through dikes is the most efficient way to move basalt up to the surface.

Now, a real important point here comparing the controlled area versus the area of the repository is "a" approaches the area of controlled area or you're moving further and further away from the repository. E3 begins to approach zero because you have less and less potential for incorporating waste and carrying it to the surface. So, the further you extend this area and the further the dike occurs away from the repository, the less likely E3 is to be a significant component. And, that becomes important in the way we couple these together. So, actually, basically what becomes important there is the potential for consequences reduces as you move away from the repository.

Okay. And then, these wonderful words that we've had so much to talk about, polycyclic versus monogenetic is important primarily because of the effect in the E3 term and I'll talk about that in a later slide.

Okay. Now, how does this differ from subsurface effects? Greg talked about this and I want to emphasize it again. Here, we being to define E1, E2, and E3 a little bit differently and these are important. E1 becomes the likelihood of intrusion of magma through or near the repository system and this is why Greg emphasized that what we're trying to find out is what's the area of influence of subsurface intrusions. We do not yet have numbers on that, but that probability should be higher than the E1 that we do for the eruptive scenario since you involve a larger area.

Second, E2 is the probability of affecting the system and that again becomes larger because you're looking for secondary effects. You're not only looking at penetration, but you're looking at can you perturb the waste isolation system. And, again, we're working to try to define that.

And then, E3 becomes a bit more complicated because the releases in this case are driven primarily by the subsurface or coupled components and this is where we have a direct feed out to performance assessment. And, to defend kind of some of the questions about the work Greg is doing, yes, we realize there's a very researchy aspect of this, but we have focused all of our work on trying to have it deal with this feed to performance assessment. That while we have a range of topics we're working on, they've all been focused on trying to feed that information that we think is key to performance assessment.

And, here, the important concept becomes what's the area of intrusions? And, as Greg talked about, we have a variety of different forms. And, the second thing is what is the affected area where you can have a combination of effects from the thermal loading, the hydrothermal effects disturbing basically the component of water around the dike, and then the release of volcanic gases. We basically have not gone very far in trying to formulate this, but we want you to understand that these are the basic two components that we have to look at in doing the full probabilistic assessment.

And, basically, just to quickly show you the same diagram. I mean, again, I'm basically just showing this in diagram form. And, why we feel that monogenetic versus poly-cyclics is so important is basically it begins to complicate the area of intersection if you can have more than one pulse. Now, after hearing Duane's talk, what I'm feeling like is we really almost have a semantics argument here and I'll get into this in my last talk today that actually both of us are saying that there could be multiple pulses. We're just disagreeing on the time frame. And, frankly, the repository doesn't care. All it cares is that it could be multiple pulses. And, so that is one area that I think we might be able to clear up in the later discussion that I'll have. And, again, here's the general geometry that Greg talked about with intrusion complexes.

Okay. I drew this up probably with a bit of opti-mism, but what the heck, I'll go through it anyway. Here, I'm focusing on the agreement that I think exists between the YMP program, the NRC, and the State of Nevada. I didn't put the USGS up here because it would complicate this consider-ably.

Okay. The first thing is that we think--at least, until Carl Johnson made some comments yesterday--there is a general consensus that a probabilistic approach looks like a reasonable way to tackle this problem. And, basically, what we like about it is it provides us a way to bound or test a problem against the regulations. The regulations tend to be probabilistic, but I've heard philosophical arguments about whether they truly are or not and I don't care to enter into that. We're trying to do a step better than the simple subjective ways that hazards have been approached in the past where you just basically say here's where the volcanoes last occurred. They're probably going to occur somewhere around the places where they last occurred. We're trying to take them a little bit further, but recognizing that these are estimates, not detailed calculations.

I think we actually have some consensus on the number of volcanic events. I know Gene Smith and I agree on this pretty much and we probably will be able to reach resolution that there isn't too much disagreement over where the vents are. We have these new Pliocene vents that have been recognized and we probably will have to do a little bit of work to see how we might want to talk about those. But, basically, the very phenomena that we're dealing with shouldn't be too tricky. Now, I do disagree with the statement that Carl Johnson made yesterday that there could be these buried things out there that we're missing. And, John Trapp has made the similar statement of the possibility of hidden intrusions. And, I think those are unlikely for this reason. These are what I call John Trapp's sneaky intrusions. Basically, what we have is pretty detailed aeromagnetic data out for the--at a 1 to 24,000 scale that was formed by aircraft. And, that allows us to see quite dramatically where basalts are. I think I have that. I hate to put this up because I can never tell which way is up in this thing and it looks very much like some sort of a psychological test.

But, anyway, basically, here is the chain of the four 1.0 million year basalts running roughly right along through here. Let me to go to this thing to see. What you see is this center is probably Red Cone, Black Cone. This is probably not the northern-most cone. I think it's up in this anomaly and then--wait, I have that wrong, I'm sorry. Here is the four anomalies in this line here, but the point that I want to make is that these things stand out very dramatically because they're much more magnetic than the surrounding alluvium and the Paleozoic rocks, and largely, they have different signatures than the tuffs. And, we have a pretty good ability to see those. So, it gives us some confidence that we can spot them and we've been busy running around. This just clipped off the lower part of this anomaly that Brent Turrin just dated, about 4 million years down here. Here's Lathrop Wells. Since it's normal, it's a positive color here. The point is that we can see these things. We think we can see them in the surface pretty well and I haven't been able to convince an aeromag person to tell me how deep we can see, but I've seen some work that Gordon Bath did in the early parts of the program where he was talking about intrusions down to the depth of several thousand feet. So, we think that we have the ability to see there. In order for an intrusion to sneak up on us, it would either have to be very small, in which case its effects would be small, or it would have to find kind of a narrow zone to where it intruded just deep enough to affect the repository, but not deep enough to see by aeromag. And, that's why I joke with John Trapp that these are his sneaky intrusions. They may be there and there is work we can do to extend the detail of this aeromag base, but it does give us a pretty good handle in this problem, I think.

Okay. Again, I already mentioned about the linear dike model and I think Greg touched on it quite nicely. So, I won't go into it very much. Other than to point out that, in general, conduits probably form above the repository. We might be able to come up with a mechanism where they can propagate downward through time, but something that we really have to concentrate is on these intrusions like we see at Paiute Ridge and how common they are. We don't think they're real common, but we still recognize them and we don't have a good handle on how common they are.

Okay. And, here's my overoptimism. I thought we might actually be reaching a consensus on the polycyclic model. I think, in general, from my conversations with Gene Smith and with John Trapp that we are in general agreement that it's something that should be considered for the program. Whether or not it's a viable model will remain to be tested through time. But, certainly, I would argue that by testing it, we're making sure that we don't err toward underestimating hazards.

And then, finally, I think nobody would disagree that the geochronology data problem is a difficult problem and I don't want to go any further than that.

Okay. Now, where do we disagree and why do we have some differences in numbers in the probability calculations? Well, first of all, there is some disagreements in how we factor polycyclic events into there. It basically affects lambda occurrence rate, but this is something that I need to spend some time with Ho talking about. We think that it's a little bit more complicated to factor in there because basically the way we serve the probability is we have independent events that the recurrence rate, the likelihood of disruption are independent. When you have a polycyclic model, what it says is that once a center forms, there's a likelihood that another center is going to form in the place. And, so you then have a dependency and so you can't do it as a simple conditional probability. We haven't yet come up with a method how we would like to factor that in. In fact, I'm very interested whether Ho has some comments and how he might do that. But, the point is that we can assume that lambda would be reduced by some function which is the recurrence time of polycyclic events. And, right now, we're just assuming and we'll just take the recurrence rate and, for conservatism, take it as that value. What that means though is you have to be careful with the way you define events and not count polycyclic events as formations of new volcanic centers. And, we have a little bit differences in how we do counts. That generally gives you a factor of two or three differences.

Now, the way we do volcanic risk assessment is we factor this issue of the polycyclic model into the releases. We don't factor it into the recurrence rate. And, some of the end quotes by the USGS people that we do do this are just simply incorrect. We are only worried about the problem of the formation of a new volcanic center. So, what we're doing is the way Greg showed you. We define E3 as if this thing is polycyclic or monogenetic is probably secondary to the issue of how much waste these analogs bring up. So, in a sense, we've blended that problem, thankfully, in the way that we do this.

Okay. The third thing is that we do have some differences in how E1 and E2 are derived. What we tried to point out in our '82 work was you really can't vary one or the other without looking at how you've done that. If you change E too dramatically, you have to go back and change your rate function to make the two consistent. And, we do have some difference over that and I'll try to elaborate what those are.

Okay. And, let me emphasize this because I think this is also an area--can be an area of disagreement. These values are estimates and we really have this profound paradox that I have described since 1980 when I first was crazy enough to start into this program. And, that is that we basically don't have a lot of events. And, by virtue of the lack of events, we have a fairly low risk of another event occurring within the 10,000 year time period. But, because we have so few events, we have a lot of uncertainty in defining the likelihood of future events. The corollary is we could say, okay, let's go get more events. But, what happens is our risk goes up, but we can define that risk more carefully. And, I don't think there's anybody that would disagree with the statement if given the tradeoff between these two directions, we would probably prefer to keep the repository at Yucca Mountain versus putting it in the middle of Lunar Crater where we might be able to define the risks more dramatically. The point is that we're always going to have this uncertainty. We never will get around it. There's just nothing more we can do to reduce the uncertainty of an limited data set.

Okay. We also have some disagreements that I think are mild disagreements, frankly, in our recurrence models. And, it deals more with how you choose to come up with your models. Basically, there's a range of what I think are very valid distribution models that can be applied to this. I happen to like the model that Ho has proposed, his Weibull model, because it does allow you to factor in waning versus waxing volcanism. Unfortunately, with the kind of data set that we have, it's hard to come up with a beta factor that's significant, again because we have limited data. I think at places where we have like lots of historic eruptions is a great way to do this.

We would like to be able to do more sophisticated distribution models, but our data set doesn't allow this. And, so what I tried to discuss in some detail in my '92 Las Vegas Symposium paper was why we want to use a Poisson model. Now, I recognize that there's always going to be debate over which model is chosen and we're probably not going to resolve this. I think about all I can do is make two points. That, one, I think a Poisson model is probably an honest or simple approach to use when you have a small data set. And, the second is that we think that you can keep a handle on your error term when you use a Poisson model. And, the way we do this is because the number of events are so small that you can't really do distribution testing, we use a cumulative magma volume curve versus time. And, what we're interested in looking at is what's the shape of that curve? Does it show any indication that volcanism is steady-state, waning, or waxing? And, a point that we want to emphasize is we feel that that curve and the petrology work that Frank has done gives us some confidence that we have a waning system here in which case we feel that you can argue that a Poisson model is conservative.

Okay. So, what are actual differences and how significant are they right now? We do, as I mentioned, have these differences in a volcanic event and, basically, Greg touched on this and this is and this is Greg's slide. He draws much better than I do. The point is that from the per-spective of--let's say the bottom of the plane is our reposi-tory. What we're interested in is how much we penetrate that repository. And, the geometry of branches above it are not significant unless they propagate effects downward into that repository. So, the number of events we count up here may not have a strong effect down here. And, I say may because we're not completely convinced we can always rule out propa-gation downward and so we want to be a little bit cautious there.

The second thing to point out that Greg talked about is that what first happens is we're pretty confident from a lot of historic eruptions that these things erupt along linear fissures and then focus down to one central conduit. So, when you look at effects, you really look at an initial break across a linear feature and then concentrating into probably conduit flow. How that happens at depth, I don't think anybody has really worked on that problem and how these things flare upward and at what depth is something that we're working on through this other work.

So, anyway, if you look at where we stand in the literature, there is some differences. Again, it's a factor of about two to three. And, in the discussions I had with Gene Smith just a few weeks ago make me feel like we're not very far off on trying to come to this agreement. What we were discussing is basically pretty good data on the physical dimensions of dikes. And, if you apply those data to the distribution of events where they violate the geometry likely of simple feeders, you probably have to go to multiple events for those. And, I think, using that approach, I think we may head to resolution on that.

Now, one of the things in Ho's latest paper, we do have some differences on his calculations and those differences are two-fold. One of them is that he uses a 90% error bound to propagate his worst case of his--I'm not sure worst case is the right term for that--but anyway, he uses a

90% error bound on how he does his E1 calculations for the area of most recent volcanism as defined by Smith. And then, he defines a small chain model based on a subset of Smith's model that uses an area of 75km^2. And, what I would argue is that that's not necessarily incorrect statistically, but it leads to what I would argue is a physically implausible model because you end up--basically, the numbers that I've looked at, if you apply the rate to this area, you end up with event density that's roughly the same as putting a repository in the middle of Lunar Crater. And, I think you can argue that, while you can't prove it numerically, that you can argue from physical processes that you can bound that lower limit of your rates a little bit more robustly than what Ho calcu- lated. Now, I'm not saying his calculation is wrong and I want to be careful here because we don't need any additional arguments in this program. But, the point is that I think we might be able to come up with some bounds by looking at physical plausibility of processes and that's something that I think we can work together toward and try to gather some resolution.

Okay. And, here's a diagram that I just wanted to show that shows a dissected center. This is the Silent Can- yon, one of the Silent Canyon basalts about 8 million years old. And, here's a perfect example of what we think a linear dike system looks like. In this case, it filled fault plains, northeast-trending faults, and then here's the main conduit area and part of the surface scoria cone. Now, the big question is what happens underneath this, but I think unquestionably, we have strong support from field data that the linear dike model is a correct model.

Okay. Let me just show this. I think Ho will talk about this in his next calculation. And, the only point I want to make is that basically the way he propagated his numbers is he used a rate E1 for this whole area and then applied it to a segment of the chain calculation. And, what we would argue is that rate probably doesn't apply there unless you have some physical reasons to think that that rate in the future will be the only one that applies. And, I would argue that it does not based on this argument and let me run this by you.

Basically, one test of the plausibility of models of risk areas is what sort of predictor do they provide? And, so what you might do here, let's take the Gene Smith model of these northeast-trending zones and his chains like this and let's say that we put one around Crater Flat and we have a new data point down here around the Armagosa Valley center. Can we draw those and then say, okay, how well do they predict the next step and, since we can see in time, we can see where they occur. And, what we see with the north-east model is everywhere the chain occurs has never been the site--everywhere Gene has his boxes has never been a future site of another event. And, so when you test it as a pre- dictor, it basically fails because the record shows that there's not that simple predictability. And, that's one of the fundamental reasons why we would argue that we prefer this northwest-trending zone. Although, in fairness, we've done calculations in every way. I mean, poor Buckboard Mesa gets really abused. Everybody thinks we're excluding it. Back in our '82 calculations, we had Buckboard Mesa in. We've done it in our papers. We have not left poor Buckboard Mesa out and we're not neglecting it. But, the point is that we think that there is some spatial predictability to this zone that if you look at it defining it particularly by these Pliocene events, what's interesting was all events except one in Buckboard Mesa have fallen in this zone that we have drawn around roughly a 4.0--I've forgotten exactly what Brent's number is--and Thirsty Mesa about 4.5. And then, all events have now fallen into that. So, basically, the point I want to make is we have a better record of predictability as a test of how well we're doing at this and we think this prob- ably is a better model. The next step is to try to under- stand why it's there and that's what I'll talk about in the next step.

Now, here's where I'd like to solicit some help from the TRB. Basically, we have two areas that I've run into a lot of difficulties with. The first is how do you choose your values for propagating when you have like a three part probability and do you choose a worst case, do you choose a mid-range, what do you choose? And, in my '82 work, I basically used a slightly worse case. I call it a conser- vative in order to bound the problem. A lot of statisticians argued with me about that that what you've done when you've used that value is you've introduced an undefined conserva- tism because what one man's conservatism is might not be another. And, they said why not use a mean because that's been demonstrated for a lot of years to be a good descriptive of the central tendency of data. And, so we have a basic problem of what do you choose?

Now, what I would argue is I think propagation of mean values may be the best way to do these calculations and then apply conservatism in your distribution function when you set up your final probability tables by propagating your mean values. I don't think you're ever going to get a con- sensus on this. I would welcome insight from anybody who wants to contribute to this. Basically, that is one of the major reasons we get differences in our probability calcula- tions. So, if we can somehow agree to what's a good way to do this, we might have the ability to at least tighten up our range of where we see.

Now, the second thing is that there is this model weighting problem that I talked a little bit about in Tucson, and which we're beginning to deal with. When we put together ranges of models, there's going to be a range of physical possibility of these models, and how do we judge that in how we put our data together? I naively thought, back four or five years ago, that perhaps we could accommodate all view-points, and I now have to say that we cannot do that. Our spread will be so gigantic that we wouldn't have solved anything, and so we're going to have to figure out some way to do this in the way we use our probability values. I know a mechanism that I could bias them. If I wanted to disquali-fy the site, I would just load my catalogue with models that gave you high probabilities. Conversely, if I wanted to qualify the site, I could just load it with ones that give you low probabilities, and I'm not satisfied with that.

I think we have to figure out some way of fairly weighting or accommodating the range of views, and I have proposed expert opinion, and that sends up some real red flags, but basically, what I want to try to distinguish, what I'm asking to do here is I'm not using expert opinions to produce the probability distributions, which I think is one area of major concern that I share. I'm trying to use expert opinion that once you've produced some values, to have the experts give you some independent feedback on how plausible are those models that you used in terms of the tectonic setting of Yucca Mountain, and the volcanic processes that you're trying to model, and that's where I think expert opinion may help us in beginning to come to some sort of a uniformity on these calculations.

Now, where we've decided we're going to go is, we think that if we try to search forever for consensus, we're never going to get it, and what we're going--we've decided we're going to do with this issue of the resolution process is we're going to take what we think is a very reasonable position, and there will be lots of difference in definition of what's very reasonable. But we also are going to very carefully document how we take our positions, and make sure that as we make each step, our assumptions are spelled out, what we did was completely documented, and then begin to send that on, that that's the only way to get off of being stuck forever on just letting everybody bang away at these calculations.

So that's the strategy that I have been pushing and I think we're going to do through--we're going to proceed with our calculations. We're going to try make some reasonable assessments of what we think are good values to propagate, and then we're going to present our numbers and present them to scientific and technical review. So any things that the TRB would like to comment on that, I would welcome them. I would love them, frankly.

Okay. Now, and I'll just--I won't even show you. What I did is, this is the latest compilation of this. I haven't even begun to try to break the data down, but I have tried to show you all the different range of calculations. I've tried to define the rate model, and here I've done something that I call Quaternary events. In order to kind of give your perspective on how you're looking at your calculations, I've turned these rates around and I said, okay, let's take these rates and propagate them for the Quaternary, and again, you can do it for 1.6, 1.8, or 2.0. I don't care. It doesn't change this a lot, and when you do that, you do see some anomalies. I'm one of them right here. I had one calculation that I did in '89 that would end up with predicting 73 events, and I'm not very comfortable that's a very plausible model. So again, this is the kind of screening that we will probably do when we go through this, and I present this primarily to show you where we stand on trying to sum up the data.

I've done the same thing for the E1 calculation, again trying to add some things. Since I've published this, I've added Sheridan's work with his Monte Carlo simulations of dikes. I've added Ho's latest things, and I thought we were doing pretty well, but I have to admit that Ho's 8 x 10^{-2} is a new number that has caused me some distress, and I'm anxious to hear him talk about it some. But the point is that we do have some tails on here, but when we do a whole bunch of models, including putting Buckboard Mesa in and out of these calculations, we don't get a lot of variability. We're pretty much somewhere around the range of 2 to 5 x 10^{-3}, and that may be a tight enough range for us to live with through propagating our calculations.

Okay. How am I doing on time here? Because I don't want to get--

DR. ALLEN: Well, you had 45 minutes, so about ten more minutes.

MR. CROWE: Ten more minutes? Okay. I don't want to get too caught up in these structural models, but one other thing that we've tried to do is begin to bring a regional tectonic perspective of the Yucca Mountain setting, and what I want to just briefly show you is that there are a range of models for Yucca Mountain that range from the Detachment Models, Caldera Models, what we call the Kawich--this is supposed to be Kawich, not Kawick, although that's kind of a cute name that Will Carr has proposed--the Amargosa Desert Rift Zone, a new model of Wright's; what we would call the Strike-Slip Basin Model, and I'm not sure I spelled Sweikert right. We really labored over it. I didn't do it, okay. I didn't think so. We didn't try--this is a compromise between two end numbers; and then what we would call a Pull-Apart Basin Model, and let me make a couple of points about Yucca

Mountain that we think we can see in the data.

First of all, if you look at a satellite view of Yucca Mountain, what you see is that Yucca Mountain itself is cut by generally north-south trending basin range faults, generally down to the west. As you go south into the block, there has been evidence of rotation and increased displace- ment across Yucca Mountain, and occurrence of strikes of left slip faults that seem to be related to the Mine Mountain Spotted Range sequence that runs roughly through here. This has been described in a lot of different models.

There's paleomag data in the Tiva Canyon formation that does suggest there has been post-Tiva rotation that has been explained as some sort of oroflexural, oroplanal folding that may be related to strike-slip faulting. The other important feature here is Crater Flat. There's just been a pile of models of what's causing Crater Flat, and it's of obvious real importance to us because that's where the basal- ts are, and so let me just show you the models quickly and talk a little bit about what they are, after I show you this one other diagram.

Two points. First of all, the extension that shaped Yucca Mountain, we have a pretty good amount of data suggesting that it mostly predates and slightly involves the Timber Mountain Tuff. It probably peaked about 11.4 million years ago, yet some still continues to exist, but basaltic volcanism that we're worried about unquestionably postdates the major phase of extension.

One other point I want to make here is that with respect to detachment faulting, I'm not real enamored with entering into this because I'm not convinced the detachment faults are reasonable pathways. When we look at dike systems in the field at a lot of structural levels, we do not see any evidence that they're following low-angle structures. These are steep structures, and I'm not convinced that the detachment models are that important.

Now, here's what the different models are, and what I want to just show you is how they might relate to your implications of Yucca Mountain. First of all, Will Carr has a Caldera Model for Crater Flat, and this has been the subject of a lot of debate, but what's important is the edge of that caldera runs just to the west and perhaps might cut into the north part of Yucca Mountain, slightly north of the expiration block. The reason this is a potentially important model is we have identified ring fracture zones of calderas as potential pathways, and so under this model, we have to worry about the potential for magma sending along that structural margin.

Okay, and kind of a compromise model that Will Carr put together, he proposed the Kawich-Greenwater Rift Zone, and this is really a variant of the old Lunar Crater, Pancake

Range, Death Valley rift zone. What he's pointing out is that if you look at a combination of the calderas and rift depressions extending through the Amargosa Valley down into Death Valley, you can define a zone that he's defined like this, and this is in his 1990 paper, and he argues that this is a structural zone that has some significance. He makes many points that Gene Smith made in his talk, and there isn't a real simple correlation between where basalts occur with this model, but what Will would argue is that the basalts are occurring primarily within the area, and perhaps along the margins of his rift zone. So this basically is an indepen-dent--is another model that suggests that we cannot ignore the northeast trending model based on Will Carr's structural model.

Okay, a new one that I think is pretty important is what's called the Amargosa Desert Rift Zone. It's not very dissimilar from Will Carr's, except--and this is Wright's model. I can't remember if it's '89 or '88. We have the two different dates, I notice, but what he is saying--and this is largely related to detachments--is that he thinks he sees a series of rifts that has opened up along strike-slip faults, and the primary evidence for them is in the gravity field shown here, where he thinks that these are basically pull-apart basins that are forming en echelon zones extending through the Amargosa Valley, then he actually wraps them down into Pahrump Valley, Stewart Valley, along what's been called the Stateline Fault here.

What's important there is when you look at the spatial association between these kind of rift structures and basalts, basalts tend to occur along the margins of these rift structures, and that may be one way--I mean, it's possible that this upper part of the Crater Flat rift here could still be controlling where basalts occur, and when we look at our, again, our Crater Flat volcanic zone, I think two points are of interest here.

Number one is if we take the rift model, you might argue that there's a spatial association. What we see is the dispersion of how far these basalt vents go, shows a relationship to the proximity to where we think the strike-slip fault is here; that the most dispersion, the 1.2, kind of an intermediate level is the 3.7, and Lathrop Wells show no dispersion, as does--and there's no dispersion shown by this, so there might be an actual spatial association across this zone between proximity to the routing strike-slip fault and how much the basalts dispersed as they emplaced themselves into the crusts.

The second point that I want to make, in terms of structural models, particularly with the addition of these new two points, what we see is there's been a fair amount of episodes of basaltic magmatism injected along this zone, and what we think happens is that it's following some sort of a structure. We're not clear what it is yet, and then we think it re-orients. The dikes that follow it are re-orienting to a northeasterly direction following the modern stress field.

The point is that we've had enough injection that we might be able to actually test this model with geophysics; that we should be able to see with detail, looking either through some sort of seismic methods, or aeromagnetic meth- ods, whether or not we see a route zone that would reflect this zone, and that's something that might become a testable model.

Now, where that becomes important is that for this model, what's important is that if it's correct, we don't--it suggested, actually, volcanism would not impinge into the Yucca Mountain area, whereas the northeast trending models has Yucca Mountain in the interior of those zones, so you can see how they're important.

When I actually do my E2 calculations of the dif- ferent models, it turns out that they're not that different, depending, again, on how you define your models.

Okay, quickly let me just touch on the Evans and Smith controversy over basically what they have talked about, is that there's evidence of a low velocity teleseismic anoma- ly extending south of Yucca Mountain into the Amargosa Valley and extending down toward, roughly, Indian Springs. They have argued that this could well represent a magma body, and they've gone further in their recent paper and argued that it could be some sort of a residual plume trace like the Yellow- stone plume trace and the Jemez lineament down in New Mexico and Arizona.

Coupled with that is the information about a seis- mic gap that Parsons and Thompson have pointed out. They've argued that one possibility is that magmatic activity in the Crater Flat area could be absorbing strain and is an explana- tion for the seismic gap.

This is an area of future investigations that we'll be emphasizing, and particularly, what we're going to do for our first stage step is examine the range of geophysical data that are there. We already do have some seismic lines that I've talked to Walter Mooney very briefly about that do cross part of where Evans has proposed his magma chamber, being down in the 25 to 30 kilometers site. They have not actually published that line, but according to Walter, he does not see any signs of a magma body from the seismic reflection refrac- tion data. But basically, we want to examine existing data and test it for consistency with these interpretations, and then bring in an external consultant. We hoped to start this last year, but because we were funding limited, we're going to start it next year; have him review the data and then make some recommendations to us about what needs to be done to begin to resolve this issue.

I want to point out that there are some difficulties with this magma model that really weren't touched on by Evans and Smith, and perhaps the most important one is what we're dealing with down here in the Yucca Mountain area in the south is an area that's been called a magmatic gap, and it's significant because we had voluminous silicic volcanism throughout the whole Great Basin, but the southward migration of it terminated here, and we have a broad area that crusted, extended dramatically, yet did not respond with volcanism. And we also note from isotopic studies that this is an area of thickened lithospheric mantle, so it may well represent a thick, cold mantle that was incapable of responding through very voluminous extension.

The point there is I have a little bit of difficulty trying to find a driving mechanism under the Evans and Smith model that would produce an anomaly. The anomaly they are proposing is a big anomaly. It's like equal to a hot spot anomaly that you would see with Hawaii.

Now, one alternative interpretation that I'd like to present and has been presented by a couple of other geophysicists, is if you look at reconstructions of the plate configurations based on Severinghaus and Atwater's work, they make two points; that not only do you have to worry about the geometry of the plate configurations through time, but also the age of the subducted oceanic crust; that is, the crust gets very young, it's still hot and it doesn't subduct well, and so they identify what they call a--I can't read that here--a slab-something, incoherent slab there, thank you.

The point is that about 20 million years ago, the position of the incoherent slab was roughly down close to the southern Nevada/Arizona area in here, and then if we look at where it was ten million years ago, we have a succession of silicic volcanism. It is right about at the boundary when you propagate the Mendocino Pioneer effects here at southern Nevada.

Now, one proposed mechanism or explanation for this anomaly may well be that it represents old subducted crust that was not assimilated into the mantle and didn't respond by de-watering and producing magma. So the point is, I think, that I want to say is that this is an important concern. If we have a young magma body in the region, it would call into question the whole basis of how we've done probability calculations, but before we panic and jump into worrying about this, we're going to start a systematic review of all the geophysical data and bring in a consultant to look at this and look at a range of interpretations and begin to test the model before we just assume that it's there.

Okay. I think I'll stop there and open it to questions.

DR. ALLEN: Thank you, Bruce.

Questions from the Board?

(No audible response.)

DR. ALLEN: Any questions from anyone else?

DR. CROWE: Have we exhausted everybody?

(No audible response.)

DR. ALLEN: Without any questions, let's move right ahead, if we may; save a little time here.

The next speaker is Chih-Hsiang Ho from the University of Nevada, Las Vegas.

DR. HO: So when I knew that I have only 30 minutes, I trimmed my overheads into half. Now, after hearing Dr. Bruce Crowe's talk, I think I need one hour to make my presentation a little bit clearer, such that people don't misinterpret the approach. So now here is the title and I will define the risks in the talk.

The goal of this presentation is to first estimate the recurrence rate of the volcanism. The second one is the probability of site disruption during the projected time frame, which is 10,000 years.

We're starting with a data set to begin with. In order to form a data set, we have to clear the two-three things nicely. First, we have to define the single event, which is not easy. Second, after the definition is clear, we then measure each event based on the initial single event.

The third one is count them all and then form a time series. Over here, we need the dates for the single event.

Now, how do we define the single event? There are so many ways to do that, but in this presentation I just show one way, which is I adopt Dr. Crowe's definition, using the main cone as a single event, which is consistent with what I presented the last time in Tucson.

Based on that definition, the data sets for the first one is post-six million years volcanism, starting from 3.7 million year basalts, to .01 Lathrop Wells cone.

DR. ALLEN: In what area?

DR. HO: Okay, in what area. When you see 2.8, you see that this is very similar to the MRV defined by Dr. Smith, so that is the area we're talking about. In this data set we do not incorporate the polycyclic volcanism into this study because that issue has not been resolved yet. So, so far there's no incorporation with that. So the question that Dr. Bruce Crowe asked to be answered may be in the near future.

Now, once we see the data set, then what kind of model can we use? So the second major topic is find a suit- able model, and I think that's important here. In finding the model, we chop into two stages. Stage No. 1 is estimate the recurrence rate. How often does the eruptions occur? The rate of occurrences.

Now, in order to select a model which is variable to this study, we have to mention about what are the basic elements of this set to characterize issue studies. First, we talk about a trend. There's two kinds of trends. First there's time, the time trend; and second one is spatial trend, or we say the trend in terms of time, the trend in terms of space, or we can say that geometry of those volca- nos, so that's the first one.

The second one is, does this model provide a good ability to predict into the future? So we call it predict- ability. The third one is, we make assumptions for the models, something as--even a simple Poisson model, we have to make assumptions, but once we make an assumption, what if the assumptions does not fit? Then how would that affect the result? So therefore, that model should be robust to your model assumptions in case there is violation about those assumptions.

And the fourth one is there's tons of models avail- able in volcanology and earthquake modeling, but we hope that that model is simple; hopefully so simple as a simple Poisson model.

First, why do we need to see the trend, and why a data set like this one, as Bruce Crowe mentioned about a small data set for Yucca Mountain data, a single Poisson model is the best way to do, but now let's see here. If we don't see the data and you are here, and you see that after

14 minutes you catch the first fish, you'll probably say, "Okay, I'm going to assume that the model is Poisson." That's fine. You don't want a data with no history. The best reasonable way of using the Poisson is random, but once you see the data set and if you still assume that the rate of recurrence is dependent on Poisson, and assume that it's Poisson to model the official event, and that probably give you the proper conclusions.

And look at here, we have only five data points, and this five data points is even smaller than the data set that I had for the Yucca Mountain region, and I wanted to show that even for five data points, the model I choose can handle this one nicely.

So the whole idea is let that constant rate, which is being assumed by the simple Poisson model indicating that it is independent of time, but now I let that rate, lambda, to be a function of (t), what's called a lambda (t). So we generate a Poisson model into a nonhomogeneous Poisson model, so the data will take care of the lambda. If it's dependent on time, then it shows; otherwise, you go back here to Poiss-on.

So the lambda (t) I chose is called a Weibull model, or a more general Weibull-Poisson model. Why do we call Weibull-Poisson model? Because this Weibull-Poisson model generalized the Poisson model. Therefore, it does include the Poisson model. So if someone say that Poisson model is good enough for the Yucca Mountain study, then the Weibull model is not going to be worse than the Poisson model, because it include the Poisson model. So if people really understand what Poisson-Weibull model is, they all say that a simple Poisson model is the honest model to model the volcanism in Yucca Mountain.

For example, the trend will be showing in this prime, the beta. If, after you see the data, you ask me the beta, and beta is close to one, you know that the data shows no trend. If it's greater than one, you have increasing trend; if less than one, you have a decreasing trend. So that can model three cases.

And the estimating process is based on the cumulative time of occurrences, t_1 up to t_n. t_1 is the time at the first eruption, and t_n is the time for the last eruption, and β1 is called the shift parameters. You can estimate that one and see the scale parameter and the lambda is the rate that I point out we're going to estimate the recurrence rate. We use that one to estimate the recurrence rate.

Pay attention to this recurrence rate estimation. For example, if time series from t_1 over t_n, and t is the current time, and at that time we can evaluate the lambda (t) which will be used as an estimation for the recurrence rate, and we have a special name for that one, which is called an instantaneous recurrence rate I'm just showing. For example, go back to what are promised for the small data set with five data points. I can rearrange that one in increasing trend and decreasing trend, and also in random pattern, and when you see beta, 0.61 indicate a decreasing trend, and .99 you see that there is no particular trend in the middle, for the middle one, but for the last one, 5.4 indicating a strong increasing trend, so based on five data points, which is extremely small, it shows that there is trend there.

So now for our data set, which is preliminary, (A) Post-6 Ma volcanism, beta indicating that is increasing, so the trend is developing and is statistically significant; p-value indicated, .005; small p-value indicated is significant. And then lambda instantaneous recurrence rate is estimated 5×10^{-6} per year. So the rate based on the year is fine, and B is for recent volcanism, Quaternary volcanism. Now here, beta is close to Poisson, 1.09, slightly higher, slightly developed trend, but not significant, and lambda is very close to the one for data set A.

So now this lambda is the recurrence rate, instantaneous recurrence rate. It's a point of estimate, and it represents the instantaneous eruptive status of the volcanism at the end of the observation time t, which a simple Poisson model cannot produce.

In addition to a point of estimate, we like to present our estimation in terms of a confidence interval, which incorporates uncertainty about the data on the estimation. So this confidence interval indicating that a range of that one is that, so that's our confidence, which is 90 per cent, which is better than the point of estimate.

So we are done with the first stage, estimation. The second one is predicting the future eruptions. In order to predict future eruptions, we have to say that, what kind of model are we going to use? So maybe we can use the same Weibull model to model the future trend, but in this case, since the projected time frame is so small compared with 1.6 Ma in the Poisson, the data set B is slightly developing, but not significant. So, therefore, I switched that one to a homogenic Poisson model. Use the history to estimate a recurrence rate, but use that instantaneous recurrence rate and assume it is constant for the future time.

So once that assumption and model has been selected, we are ready to model the volcanic disruptions. So before we actually model that one, I have to make it clear what am I doing, so I define the risk. There's so many ways to define the risk. You can say the risk that are they going to be injured, is the risk going to be death or whatever, but over here I defined the risk that probably lists one disruptive event during the next t_0 years. This t_0 is 10,000 years here, so once the risk is determined, now we can say, okay, the number of those kind of events during the next t_0 years is quantified by $X(t_0)$. This is very common in statistics.

But now I have to make a comment here. We define a single event based on a main cone. Therefore, that risk only accounted for eruptions which form a main cone.

The next one is p. My notation for p is the probability that any single eruption is disruptive. You have a new future eruption. What's the chance that this eruption would disrupt the site? That chance is noted by p, probability. So it's between zero and one. If you seat the repository on a volcano which happened to erupt, then p is one. If that volcano is farther away from the candidate site, then p is zero. So the permissible value of p is between zero and one.

So the remark I make is written here. So the approach, the variation, the estimation is based on the first one. I only consider eruptions which directly hit the repository, directly hit the repository. The -- event is considered here only. The second one is, I ignore what we haven't seen, because I define the single event, which is a main cone. Therefore, I ignore things like series of dikes, plugs, and sills, and then some other things which may affect a repository. So, again, I want to make a comment here. Is a probabilistic approach enough, and is this the only approach we can evaluate the suitability of the site? I say that probability approach is one way, but not the only one. We have to accompany with a qualitative assessment because we have these kind of things there. Something underground should be valued maybe qualitatively.

So now the risk, once we have p there and we define the risk as at least one disruptive event, and these risks I simplify into this equation. Now again, Dr. Bruce Crowe mentioned about I used the wrong risk to AMRV to estimate a recurrence rate, but use a rectangle to estimate p, which is wrong here. The reason is why I say it is wrong because maybe he didn't quite understand what the Bayesian approach is.

The Bayesian approach, assuming that the probability of disrupting the site is a function, is a function, that function ranges from zero to a particular value. So I show that in detail here, but before we go to the detail, the whole idea is I assume that that p followed distribution, we call a prior distribution because the Bayesian approach, you need a prior distribution, the permissible range of p between zero and one, as I said. One is the maximum, zero is the minimum, but if you treat the whole Yucca Mountain as a black box, that we ignore geology there and saying that, okay, zero is the minimum and one is the maximum, so our people used a noninformative prior, saying that uniform (0,1) is distribution for p. That indicates to me the point is half, which is

50 per cent of the time that a future eruption would disrupt the site. I think people will question about that point. So we cannot ignore the geology in the Yucca Mountain area.

So we need scientists to answer this question. So when I was contacted by Dr. Smith, I think in the year 1988, I said, what I need is something like that, something like that; quantify the risk in that area such that we can use that one to evaluate site disruption. We cannot just say that, okay, the whole area is this one and the area for the repository is eight, so take the division and you've got a fixed point estimated for the p, which is E2 in Dr. Bruce Crowe's presentation.

So the approach here is, now look at here. So the whole area is AMRV here. You have a future eruption here, then the probability of site disruption is what? Zero. Zero; definitely zero. You cannot assume it is A over A for this future eruption here. But for the future eruption here, 7 is the probability, much higher than the future eruption here.

So just like, okay, we'll make it easier here. We have a group of people here. Some people will live longer than the other, some people died at birth, so at birth means that you have a probability of zero to disrupt the site. And then p over 1 indicate that that guy, just like George Burns, lived almost 100 years. So use that idea. I say that my p have a lower bound zero, but the upper bound I don't assume is one. I assume that this half of that rectangle is A, as defined in Dr. Bruce Crowe's paper, and the area is the numerator, and take the ratio, and I use that one not as a fixed point estimator, but just the upper bound for my distribution for the Bayesian prior. So that answers the question, saying that I use everything to estimate a recurrence rate. I also use everything to model the p; zero up to here. This is the high risk zone here.

Okay, superimposed, that one is here. So this is a rectangle that gives us the worst case, but I don't want to assume that the worst case is all the cases.

So now over here we have A equal to 75 km^2. That's the half of the area of the rectangle defined by Dr. Smith and others, based on his AMRV paper here, and a, I still use the same estimation of Crowe's and others, 1982 paper, that's now probably modified to seven or maybe six or maybe something else, and actually, we can increase the a to any--maybe a distance with three or four or five km^2, to be the area of the repository. So geologists have to evaluate, saying that what kind of area will be affected as a way to modify that one.

So now three indicates that. That is future, pi(p); p was the probability of site eruption for any given eruption. It's uniform, 0 up to 8/75, so I didn't use 8/75 for every single future eruption. I use a function, a distribution which is randomly from 0 to 8/75, so the maximum is 8/75, minimum is 0. So 8/75 is the upper limits for p, and I believe this is very reasonable and that actually is the beauty of the Bayesian approach.

So now we have p determined, we have lambda determined, so therefore, that function can be evaluated, and then something nice about this one is a confidence interval for the instantaneous recurrence rate can be carried over to the risk, and that risk after our analytical integration, we got this probability, lower bound and upper bound with 90 per cent confidence interval. So this is roughly one in a thousand--to us, it's one in a thousand, and then two, about seven to a thousand within a time frame of 10^4 years. So this is dramatically different from what people years ago were saying, that one in 10 million or one in a billion, and I just don't understand whether that mean one in billion or one in 10,000 or one in whatever. So this is the result that I got based on my definition of single event, and then definition of risk, but certainly, this is not the final result for other definition of risk, other definition of volcanic occasions.

So this is just one demonstration about how we may approach the same problem by using the different definition, so I don't think there is a unique answer for this important and hard question. This just demonstrates you can have dramatic results based on different reasonable interpretations.

Thank you for your attention.

DR. ALLEN: Thank you.

Questions or comments from the Board? Bill Melson?

DR. MELSON: I'm wondering if maybe you could clarify the issue about--well, an issue raised by Duane Champion, with interesting results, which is let's consider a model where, in fact, instead of looking at these as discrete events, we're looking at a fissure swarm--

DR. HO: I missed that part.

DR. MELSON: At a fissure swarm of en echelon fissures coming up and creating several cones all at once, so that what we're dealing with then is a--something that has length distributed in time.

DR. HO: Okay.

DR. MELSON: And then, I mean, this is a, I think certainly a valid model, and perhaps more valid than the possible model or possibilities you were using.

DR. HO: Yes. So again, as I mentioned, that this presentation is one way and then everything is preliminary. If I have the full support of geologists, for example, a accurate base, and then also indicating whether this cone is polycyclic or not, I would definitely use a different approach, and the approach I'm thinking about and very similar to what you mentioned there, is treat that cluster as we have the main shock, we have the after shock. So a main cone probably is the main, treated as the main shock, and then the others may be treated as after shock.

If you want to treat that one as the main shock, certainly we can use the Weibull model and then assume that you have several machines, several repairable systems, and model different systems the same time, and Weibull model can do that easily. So over here we just treat anything as one repairable system, but I think the accurate way and the better way is using--treat AV cluster, AV centers as a single system, and parallel and model everything. But unfortunately, we don't have sufficient data.

DR. LUCE: Yeah, I'd like to ask a question about something I've seen several times, I guess, mainly today, and that's that drawing of the high risk zones with the northeast trend by Smith, I guess?

DR. HO: Yes.

DR. LUCE: I don't see how you can get a trend when you have one point on it, and they're very long rectangles even for the ones that have more than one point.

DR. HO: Okay, yeah. Dr. Smith can answer that one, but I will follow up that.

DR. LUCE: You have to have a second point in the big trend that goes through it.

DR. SMITH: Can I address that since I'm the person who designed these? They are not designed based on the number of volcanos within each trend. They have two parameters in-volved. Number one is the alignment of faults, the fault trends within the Yucca Mountain area. They're aligned, elongated in the northeast direction, parallel to the struc-ture or region. That's what controls the long axis of that rectangle.

The lengths, the sizes of those rectangles are based on the sizes of volcanic chains. The smaller rectangle is based on the size of volcanic chains at Crater Flat. It's a relatively small chain. This is the--about 12 kilometers long, a couple kilometers wide. This is how wide volcanic chain in Crater Flat--this is how long the measured crater chain is, so all I've done is I've taken of a crater chain that you can measure in an area adjacent to Yucca Mountain, elongated that chain in the direction of regional structure, and that's the origin of the inner rectangle.

The outer rectangle is based on the lengths of crater chains in the analog areas. This is determined to be, at least in my mind, the largest--it's determined to be sort of a worst case scenario, in that we're trying to determine how large a chain can be and we're looking at, for example, some of the analog areas, the Reveille Range, the Fortification Hill area that I mentioned this morning, and this larger chain is what I consider to be the largest plausible volcanic chain that can form.

That means if we have another volcano, how far to the north along regional structure, how far to the south along regional structure can it form? So I have to have some way of bounding that, so I'm using chains from analog areas to form the larger rectangles, chains from nearby volcanic areas in terms of looking at Crater Flat to form the inner rectangles, and the alignment direction is the direction of regional structure. So it's not based on the number of volcanos.

Now, I should mention, just to answer Bruce's comment, if I can, it's sort of unfair, what Bruce said, that the rectangles cannot predict any eruptions, because in reality, I've gone one step farther than Bruce in terms of the Crater Flat zone. It's sort of unfair because if you draw a line around all existing volcanos, of course, it's going to, you know, if you were there 100,000 years ago, you're going to be able to predict, you know, a future eruption because you're drawing a line around all volcanos that are known, and my AMRV does exactly the same thing. All the volcanos fall within the AMRV.

I've gone one step farther in terms of trying to identify potential hot spots within the AMRV. What I would like to challenge Bruce to do is to find, within his Crater Flat zone, I would like you to design high-risk zones within your Crater Flat zone, because I feel that those risk zones are actually going one step farther in reality than simply defining--simply drawing a line around all existing cones.

DR. CORDING: Just one more question on that. Is the turn, then, is that parallel to the Solitario Canyon Fault?

DR. SMITH: It's pretty well parallelled with the regional faults, yes.

DR. CORDING: But specifically the Solitario Canyon or the Ghost Dance or--

DR. SMITH: Yeah. There was--I don't recall the exact strike of those faults, but they're roughly north, south, north 20° east.

DR. CROWE: The Solitario and the Ghost Dance are north/south trending, and Gene's is a little bit more north-northeast.

DR. ALLEN: Let me ask this, Gene. If we instead drew this box so we'd find a different trend, so somehow Yucca Mountain fell just outside rather than inside, what would that do to the risk?

DR. SMITH: Well, these zones are--these zones have in a way sort of--I tried to make the zones as narrow as possible. I tried to be as conservative as possible in terms of the widths of the zones. In the Reveille Range, for example, some of the volcanic zones are nearly twice as wide and what I have shown, but I tried to make them as narrow as possible. If you change the orientation by 5 or 10°, possibly Yucca Mountain might fall outside of that risk zone.

DR. ALLEN: Well, in your analysis, if this zone had been drawn 5° different so that Yucca Mountain was outside rather than inside, what would the risk then be; zero, or not?

DR. HO: No, I don't think so, because actually, that rectangle is designed to evaluate the upper bound for p; that upper bound, the upper limit, that the maximum probability that any given eruption would disrupt the site. That upper bound somehow has to be evaluated, and if Yucca Mountain is not in that rectangle, we cannot say that is zero. We cannot say it's zero, but we have to think of some way to find an area such that it reflects a reasonable estimate for the upper bound for the p.

DR. REITER: I'm trying to understand what happened. I read the article and I'm really trying to understand a couple things.

If I understand it, the way you got your 90 per cent confidence limits, essentially you took the interval estimates of lambda?

DR. HO: Lambda, yes.

DR. REITER: And multiplied it by essentially the average of the uniform distribution?

DR. HO: Not--because now here, according to the risk equation, risk equation, that p has been averaged out, it's averaged out. The only thing is lambda, and lambda had a lower bound, so you have a lower bound, and upper bound, you have a upper bound.

DR. REITER: But the average of p is four events, 4/75?

DR. HO: No, no. The average of p is the mid-point of zero to 8/75. Yes, that's true. That's the mean of the uniform distribution.

DR. REITER: Right. So essentially your calculation simply assumed, take all the events in the AMRV, okay, and dump them or somehow put them into half that particular zone?

DR. HO: Um-hum. Your rough calculation is right, yes.

DR. REITER: So I think the question that I have is not that your bounds of zero and 8/75 are appropriate, but whether the assumption of a uniform distribution is appropriate.

DR. HO: Okay.

DR. REITER: Because if I remember Gene's article, he gave various priorities to where different--how the volcanic --future eruptions would appear in here. He, for instance--Gene, correct me if I'm wrong--I thought you assumed there was some low probability if it occurred randomly within the AMRV, then you had three different locations, and within the locations you would have both a higher probability and a lower probability.

And it seems if I wanted to take Gene's model, I would take all those various assumptions in there and I kind of feel I would not get a uniform distribution between 8 and 0.75. I'd have a distribution that was much more skewed to the zero. So I think that your prior is not--it doesn't seem to represent what was intended in that article. It really would be a lot lower than what you're presenting.

DR. HO: That's a good point. That's a good point here. Prior is always an issue in using the Bayesian approach, which we have an analysis called a sensitivity analysis. You assume different prior and you evaluate different results, which actually will be my next project.

DR. REITER: The only point, again, is I think it would be very useful for us to see the results of Gene's model, taking into account not only that one rectangle and putting all the weight on that, but taking into account all the other assumptions that Gene has made about the AMRV, that there are various centers and there are smaller rectangles, and then see what that leads to. I think that would be a very informative project.

DR. HO: But at least we have done that upper bound there, but you say that probably zero is more skewed to zero. It may be more skewed to the 8/75. Therefore, I don't know directions. Therefore, I put uniform distribution there.

That's the best I can do.

DR. REITER: I kind of doubt that, but it seems to me that 8/75 is not your upper bound, but the upper bound is assuming uniform distribution.

DR. HO: No, no.

DR. REITER: I think so.

DR. HO: No. My point is that the probability is between 0 and 8/75 randomly or uniformly, meaning that you have some percentage below that, and some percentage below that point. So the whole distribution is a flat one. But you say that maybe it's towards zero. It can be the other way around, because if the future eruption is towards the repository, then the direction will be that way.

So what I have used for the uniform is I'm using the assumption towards the middle, which is uniform, and you may argue that it's towards zero, and some other people--actually, Dr. Smith's MRV rectangle also indicated that future eruptions will be very close to Lathrop Wells, and in that case, it holds to the 8/75. He points out that future eruptions will be somewhere around there, so if that's the case, then that uniform zero one is too conservative, maybe an estimation of the--

DR. ALLEN: One final question here; Kip Hodges.

DR. HODGES: I just had a quick point. I think the beauty of this technique is that you can use any distribution you want, and so to do the sort of sensitivity analysis that he's talking about, he can sit there and play with as many distributions as he's interested in, and that's the beauty of this basic approach.

DR. REITER: But, Kip, the point is that we're taking only part of Gene's model, and I think that we are interested in seeing what Gene's model presents as a totality, and that Gene has listed various probabilities of earthquakes occurring within that particular zone. I think it would be very useful to see somebody trying to capture that, and then see what the probabilities are.

DR. HO: But so far we have captured the overall direction already, and for the details, I don't think that paper already provided detail about what kind of priors do we use. That's my interpretation.

DR. ALLEN: Okay, thank you. Unfortunately, we're getting very late.

I'd like to suggest we take a fifteen-minute break right now, and then, Jeanne, I'll let you see what happens after we're reconvened.

(Whereupon, a brief recess was taken.)

DR. ALLEN: If we may reconvene, please?

Jeanne Cooper has a couple more words about the field trip tomorrow.

DR. COOPER: It occurred to both myself and Ardyth--and

Ardyth, please contribute if I forget something here--that we really ought to clarify some things about the field trip tomorrow.

First of all, again I'll remind you for some of you may not have been here yesterday when Bruce tried to present a little summary of what was going to happen out there, that we really do have a large logistical challenge here with over 60 people going on the trip, so we would appreciate your cooperation.

Right now we have 11 four-wheel-drive vehicles that will be transporting people out to the site, and we would request that all 11 of those vehicles please meet us at the Valley Bank Center, and we plan to leave at seven, so we'd appreciate it if you could be there, say, at 6:45 at least tomorrow morning.

Also, a few hints about the trip. You need to bring your own lunch. We will be providing water, but if you want something else to drink, you should bring that along, also. You should wear sturdy shoes, preferably boots that at least come up over your ankle. You need sunscreen, sun hat, and probably a wind breaker of some kind.

Ardyth, please add anything that I've forgotten.

DR. CROWE: Let me make one quick point there, Clarence, on the field trip. If we could get everybody to agree to make sure you don't go past the gate, we just--we'll meet at the gate because we want to try to just control how we go into the private property, and so please don't go past the gate if you get there early. Just wait at the gate and we'll all assemble there, and then move on from that point.

DR. ALLEN: DOE has made a special request to make a very short, less than five-minute presentation here on some new geo-chronologic data. Mike Murrell, I believe, is going to present that data. I was guaranteed in five minutes or less.

DR. MURRELL: I wasn't planning on presenting any data at this meeting, but I have some recent results that Bruce has asked me to present to you. Unfortunately, Bruce made the transparencies for me over lunch and they're rather faint. I'm not sure if that was intentional or not, but bear with me--and this will be less than five minutes.

I'm Mike Murrell. I'm from Los Alamos National Lab, the Isotope Science Group there. We've been looking at the uranium thorium dating of young--of volcanic events. Just to spend 30 seconds on refreshing you what's going on here, we're looking at the uranium decay series, Uranium-238, Thorium-234--or Uranium-234 and Thorium-230. The half-life is about 75,000 years. When this system is left alone, the activity of the daughter to the parent, thorium to uranium, is usually one. If you have some kind of chemical fraction- ation, such as partial melting and crystallization, you can disturb that, and as this returns back to this ratio of one, you have some idea of time.

In regard to dating the crystallization age, the systematics of this are something like this. You look at the activity ratio of uranium to Thorium-232, plot that versus the daughter, Thorium-230 versus Thorium-232, at t_0, if you have minerals that have different thorium-uranium ratios, they plot along this line here, and with time, they rotate back on to this slope equals one line. At any point, the slope of this white line here defines an isochron, which is the crystallization age of that particular rock.

About a year and a half ago, we presented data at the previous TRB meeting on QL4. It defined an isochron of 150,000 years for QL4. There was some concern that the spread in the mineral separates pulled out of this rock was not very large, about 4 or 5 per cent. That led to fairly large errors--this is why the errors are so large--and there was some concern that because there was not very good separation here among the uranium-thorium ratios, that maybe these weren't clean separates and, in that case, you had a mixing possibly between something that was young and something that was older, giving you an apparent age of 150,000 years.

We have looked at QL6, working very hard to make pure mineral separates and more of them to try and attack this problem, which is what I'm talking about today.

This is the data on the thorium-uranium ratios of the various separates I've pulled out of QL6. In general, they confirm the data of QL4. There is not very much spread in thorium-uranium ratio. For example, the plagioclase separate is exactly where the whole rock is in terms of thorium-uranium ratio, which is very similar to one of the olivine fractions. We did get some separation in a fairly unusual phase. There's a second component of olivine here, the largest olivine, which is different by about 20 per cent, and the magnetite contained within that olivine is also different from the whole rock by about 25 per cent, but what we have here are two populations of olivine, larger and smaller, and two populations of magnetite here and here which have different thorium-uranium ratios.

This is the isochron that results from plotting that data. The fine-grained phases from the matrix of the magnetite, pyrixine in the matrix itself, the whole rock, and the fine-grained magnetite from the large olivine all plot on a isochron, giving an age of 120,000 years, plus or minus 20,000 years. There's a large spread here, and so the errors have gone down quite a bit.

This basically confirms the data of QL4 in that the matrix material shows a very, very small spread in thorium-uranium, and I think that's real. We're helped here by this magnetite from the olivine.

There is a mixing line. The large phenocrystic phases, this large olivine, and the plag fall off this line, and I believe they're interacting with some other material that's out here that's pulling them off the line. I think this is a magma chamber process, and it's interesting in its own right. It's giving us some information on what's going on in the magma chamber before eruption.

DR. ALLEN: Okay. You've had five minutes. Do you want more?

DR. MURRELL: Okay. That's it, then.

DR. ALLEN: Okay. Thank you. I'm sorry to cut you off here, but clearly, we're running very late, and the next three speakers are basically perspectives and we'll start off with Bruce Crowe on volcanic studies; progress and future directions.

DR. CROWE: I'm going to hope that I might be able to keep this just a little bit short and bring us into perspective, if that's possible.

Okay. What you've heard for the last two days is a series of overview talks by individual investigators, presenting a whole range of new data, and I think what I would like to emphasize is that I think we've made a lot of progress since we last talked to you in 1991, March of 1991. Obviously, we still have contention over parts of the issue, but I feel like the addition of new data has helped clarify this issue and somehow, if we can get a perspective of trying to worry about what the different data sets mean and less about who's right, that we may progress even further on this.

In general, I think we're starting to see some signs that we're moving toward resolution, if we can just keep ourselves from getting too caught up into the polariza- tion of differing methods, and I have been trying--since I don't do geo-chronology, I have been trying to keep a per- spective here of trying to do that, but I'm not sure it's totally working. But the bottom line message that I'm hoping that we have gotten across to you is that we have made prog- ress. We're not there yet, but I think the way to solve this problem is with data, not with rhetoric.

Okay. I'm not even going to bother going into this. I think you've heard so much about the plus and minus- es of K-Ar that I don't want to jump back into this again. I think we'll just further bind ourselves up. But let me make a few points on future directions.

Number one is that I think Don DePaolo's comments are very apt for this problem, that we can look at the argon work as a upper bound, somewhere around, say, the 150 range, and what we will end up having to resolve is how much of an excess argon component, what sort of data biases, and then what's the best way to sample the data set, or to present the data set, but probably we can safely conclude that we're probably younger than 150,000 years.

A problem that we will have to deal with is the quality assurance pedigree of the data set. None of the work that's been done outside of the program meets quality assurance, and so we have the double issue of trying to come up with a quality assurance data set that we're going to have to deal with.

One thing that we didn't mention that we have submitted to Ziegler, is trying to date lithic fragments. These are high potassium lithic fragments where you have small fragments that range from fist-sized to finger-sized completely immersed in basalt, and our hope is that they may have thoroughly reheated and re-equilibrated and we can get a--basically a basalt crystallization age out of those. We hope to have some data on that in the next couple of months, actually.

The biggest thing that I think I want to emphasize is what I keep struggling with when I try to understand all these data, is just the mixing of assumptions and uncertainty, and trying to wade through prejudices and things. I mean, I think the individual workers have the best understandings of what the strengths and weaknesses are in their data set, and if I can make a plea, it's that please, try to present the data in terms of what you can truly conclude, what you think are reasonable speculations to proceed from that, and what is true speculation, because simply just hammering at only the strong points or the points you want to make and not presenting the full spectrum of possible interpretations, I think has really caused a lot of difficulties in this problem.

I'm going to skip the next couple of pages and not even bother getting into them. We've made these arguments about the statistical concerns, about the weighted mean, and I happen to like Kip's data approach. I basically think beating this with data is probably the best way to proceed on this.

Basically, you saw the uranium-thorium. We actually--I mean, this is brand-new data. QL6, we think, is the oldest unit out there, and so it's interesting that it gives us the 120 age. We obviously have further direction to go. One of the problems with uranium-thorium is that while Mike has done a lot to overcoming the analytical problems, we still are faced with the--the mineral separations have been difficult, and it's an expensive, time-consuming measurement, but again, we think we're making progress.

We sat down last night trying to decide where to go with this, and I think the consensus is that we think we should keep going; that, again, more measurements is the way to resolve this, not sitting and arguing from different--throwing stones from each side of the spectrum, but just gathering data and see where the data pushes you.

We think that there's been a lot of progress in the cosmogenic helium ages. I think that perhaps they have--there has been some over-exaggeration of the potential for the minimum age concerns here, but we can solve that problem by basically sampling and gathering more data. What we try to do when we use helium is find as pristine a primary surface we can find, with enough topography on the outflows that we're above the active areas of deposition. Our major concern is deposition with the helium. And frankly, if these are minimum ages that are substantially younger, then with enough sampling, we should get a trend back toward older ages. That's solvable by basically a standard working hypothesis of gathering data. This can be tested, and I think you've interrupted this, or we were presenting this in the initial stage.

I basically strongly disagree with just throwing helium results at its minimum ages. I think we have to test models to be thorough and be complete, and anything else but continuing to test them, I think, would be foolish. Basically, we feel that the technique looks promising. We've overcome all the QA hurdles. We do see some signs of a 65,000-year conversions, particularly on the helium in the initial ^{36}Cl results, and the one potassium argon mineral separate that has been done, surprisingly or coincidentally--I don't even know what to make of it--has given us a 65,000 ± 35,000. That's the QL5 lava site.

I'm not going to make a big deal out of it. I think, again, you're seeing a snapshot of a problem that should be resolved by data, not by rhetoric.

Now, with respect to thermoluminescence, I feel I have to really defend Steve Forman here because Steve has been out of the country. When we rescheduled this meeting, he was unable--he already had prior commitments and couldn't come here, so we really have gone lightly on that.

I talked to him--he just got back into town Friday and I talked to him a little bit about the discrepancies between the helium in the thermoluminescence results. What Steve has been working on--and we'll present them at some other time, or we can actually make his data available to you--is he's done a lot of testing of the TL method up at the Snake River Plains against Carbon-14 sites, and he's been able to show good reproducibility between the TL and the Carbon-14.

The big point, I think, that Don really emphasized is that we really are in an unknown area when we calibrate beyond 30,000 years, but the one thing that perhaps we have to emphasize is that there has been good results with TL where we have dated soils buried by tephras, and right now, we have no firm basis on which to discard the TL age for the tephra sequences in the soils.

The alternative interpretation that the USGS has presented, that these are not measuring volcanic processes, I think, is an important one. I happen to disagree, but I think it's something we can go out in the outcrop and at least begin to understand why people have a difference of opinions. I think we've gathered enough data now that we can demonstrate that their premise of these being cone-slope apron deposits is physically impossible for two reasons:

Number one, we have the historic photograph of Lathrop Wells. There is no evidence of mass waste in the cone slope enough to produce a cone-slope apron. The cone lacks a cone-slope apron. What they showed on their topography as a cone-slope apron is simply a sand ramp blanket. It is not a cone-slope apron. There is not a cone-slope apron out there, so you have a bit of a physical impossibility of doing that.

Frank's new data is fascinating, in my view, that he has identified that chemically, that these tephras did not come from the main cone. So that's a second line of evidence that raises those difficulties. The problem we have is, we don't know where it came from. But how do you solve that? You solve it by gathering data, and that's a process that we've asked Frank to continue on, and I'll talk about that in a second.

So basically, we can't discount that evidence, and what I feel very strongly about is you just can't throw out data because you don't like it, particularly when that data would lead to a younger age for Lathrop Wells. We have to be religious about making sure we conduct the right studies to make sure we can bound the lower edge of Lathrop Wells by as many techniques as possible.

On paleomagnetic data, I really think we have a bit more discrepancy here than I realized on the quality of the data set. One thing that I would like to show tomorrow is we have some concerns about the sample data set collected from the summit of Lathrop Wells, and from the scoria mountains that have been sampled because they are covered--the cone summit is basically a non-agglutinated scoria, and I'm concerned about whether you can really extract a good paleomagnetic direction out of that. But what we have to basically do is let's go look at where the sites were collected, and have Duane show us his individual data sets.

He has argued in the literature that he had to discard samples. We'd like to see what he discarded, and show us his full data population so we can make a judgment of whether he's discarded samples reasonably, or whether he might be measuring something that's so disbursed, you can't gather good information from it.

The only point I'd like to make is one thing that we've recognized just recently from our trenching work is that we can get through this problem. We can now trench into the scoria mountains and if we--we can find places where we have good agglutination, and I think that would add less noise to this issue. Right now, we see that there's a pretty serious paleomagnetic sampling issue that hasn't been addressed. We'd like to talk about it on the field trip tomorrow, but what we can agree upon, I think, is that we have a means, through trenching, of going ahead and getting data where we think that we can eliminate that variability, and that's something that we want to do.

The soils and geomorphic studies, my biggest concern here is that I just don't understand how you can throw soils and geomorphic data out and just discard it. That causes me great distress. I spent a lot of time with Les and Steve in the field. They are well-recognized, very capable workers. What I see in the rocks tells me that there's strong evidence that these things could be polycyclic. I find real difficulty dismissing it until I can find some physical mechanism explaining why the outcrops I see aren't correct.

If we can trench them and open them up and the geometry changes, fine, but right now, out of honesty, as being on this investigation, I cannot see any other explanation for these deposits besides requiring polycyclic eruptions. You just do not form soils with distinct horizon development in short periods of time, and I'm perplexed and disturbed, and I honestly don't understand why the paleomag does not seem to be reflecting this, but I'm not willing to just automatically discard a wealth of information provided by soils and geomorphic data.

We think that that is a really valuable cross-checking tool, and I cannot dismiss it until I find evidence to dismiss it. I think the rocks are telling us something. Maybe we've interpreted them wrong. If we can find out why we've interpreted them wrong, I'm happy to accept that, but until we do, we have to look at multiple working hypotheses. Anything else would neglect the challenge in front of us for this project.

I think Frank has demonstrated some really significant breakthroughs on the geochemistry studies, and what I think is important there is that he seems to have very, very strong evidence that these things had to have formed as physically separated magma batches. The issue of the age difference, I think, has been unresolved, but there does seem to be awfully strong geochemical evidence, and again, I just cannot understand dismissing that data as just minor variation. Basically, he's presented a testable hypothesis and he's examined it with data. I cannot come up with an alternative explanation for that data. I think we have to consider it carefully.

We plan to extend his work to other centers, and what we think is that his method of testing stratigraphy is a very cost-effective way. Before we throw all this geo-chro- nology data at the sites, we would like to use Frank's ap- proaches as a testing method, and that's what we plan to do as we extend our Lathrop Wells studies.

Okay. Now, what does all this mean? This is perhaps the bottom line here, and what I have to say is when we look at this in terms of probability models, we can't give you a lot of insight. We cannot say that this is really important, unfortunately, because there are so many assump- tions that go into probability calculations, but let me tell you exactly where we go.

The bottom line is that I cannot demonstrate a lot of risk sensitivity to the age of Lathrop Wells. I think the bigger issue here is more like a public confidence, scientif- ic confidence. We'd like to put enough work into this that we think we've got a pretty reasonable answer. That answer may well be bounding the problem, but I think that that's the essence of what we need to do there.

The more important problem, though, is this whole issue of polycyclic, of multiple events versus a single event, and that's by far the more sensitive issue. Okay, now let me show you the basis on which I say that:

What we have tried to use to look at time sensitivity and tests of waning versus steady state versus waxing processes is a cumulative magma volume curve versus time, and here I've drawn up these new data points from Thirsty Mesa, the Amargosa Valley site, 3.7, and I've made two curves. In one I included Buckboard Mesa, which would include this data; in the other, I have not. And what you see is basically this just shifts by about a cubic kilometer.

The point that's important here that we have made repeatedly is the shape of this curve through time is what's important. What we see is that you almost have a Pliocene magma production rate or magma output rate, and what's important is that it's been dramatically lesser in the Quaternary, depending on which way you want to do this.

Now, the only way for Lathrop Wells to really change this dramatically would be to move this around, and you cannot do it by chronology. That actual end of that arrow point there is 150 and this is supposed right at ten. You can see that there's not a lot of sensitivity. The only way to get that to move would be if you had a lot more material out there, so in terms of the risk models, this is just not a sensitive issue. I think perhaps the stones and rocks and the fighting have overshadowed what it means.

But we do want to be able to establish credible ages that we think we can defend going both before the public and before licensing, and that's probably the major issue here.

Now, the other issue here really is this issue of monogenetic versus polycyclic, and what I'd like to propose today is that let's throw out the terms monogenetic and polycyclic, because they've gotten such emotional impact. The issue is, do we have one event or do we have multiple events? We really don't care what the time is between those, because the important event is what goes through the repository, what has the potential to bring material up. And what I find amazingly perplexing to me is that, basically, Duane's paleomag data provides one of the strongest lines of evidence that there are two events at Lathrop Wells. The only difference we have is 100 years versus longer years.

I have some real reservations on how Duane gets the 100 years, but that's not the critical point. The critical point is that we could have two events there, and what I see is two groups that are both saying the same things. We just are arguing over the time frame. So what I actually see, is I really feel that this--from a repository perspective, the penetration, upward-carrying of waste is what's important, not how long it is between those intervals. It actually has some effects on how the thermal load would be distributed from the thermal pulse of injection, but the more important thing for our eruptive model is one or multiple pulses.

What I think is perhaps significant is if you look at the geometry of the feeder systems for both Lathrop Wells, and taking the state's data that Gene Smith has worked up for Red Cone and Black Cone, I have a hard time fitting that data with any one single dike system. It looks like we have a requirement of multiple dike systems.

In some earlier data that I've seen Duane show, he does demonstrate that there might be two somewhat close, but slightly different positions for some of the Crater Flat stuff. We're ending up really arguing different views of what's getting to the same point, that there could be multi- ple pulses, and that's the essence of the point that we're trying to look at.

In large part, what I do have to say for Lathrop Wells is I'm not convinced that Duane's data is conclusive that there are two positions, and that's something that we can discuss on the field trip; that he basically gets his position in part from the cone, and then from the lava flows. Now, the data set for the lava flows looks very good. There's good precision with that data set. I'm a little bit amazed that you can get a good data set from the cone sam- ples, and I'd be more than willing to have Duane basically explain how he can get that data set.

The issue that we don't know is the timing, but the way we are factoring these calculations is we are doing this

E3 through an analog study, which doesn't care about the timing. We're basically looking at the potential for the quantity of waste that can be carried out, and so if there's any way that we could kind of declare a truce, that we dis-agree, but the risks don't differ that much, and let's hammer out with data what the interpretations are with more data and less rhetoric. I would certainly welcome that.

Now, what are my summaries? I actually think, despite, again, ending up very polarized in this, that we have made some progress. Some of the high end ages have moved down a bit, some of the low end ages have moved up a bit. When you begin to look at error bars, we may not be that far off. We may have to beat this to death with a lot of samples to do a good job, but the way to do that is to do it with data. I think that it makes some sense that in order to maintain some credibility in both the public regime and the scientific community, that we ought to make slow, cau-tious progress rather than emphasizing our differences, and I'd love to see that happen.

We think that with access to the quarry we can trench some areas that we still need to look at and test some of the models. In general, I'm encouraged by the progress that we're making despite some of the disagreements.

Finally, again, I have to repeat my plea. You've got to somehow maintain a sense of objectivity. This is not a contest to see which method is best. I don't, frankly, care how old Lathrop Wells turns out to be because I'm not a geochronologist. I would like to see a data set that we can present without half the room coming unglued about what they see about the data set, and the importance is to learn to work together as mature scientists, and we can disagree without being polarized. There's nothing wrong with dis-agreements. In fact, let me underscore that I think it's healthy. I think the fact that we have disagreements means we're being very rigorous on how we're going about this process. I would just like to try to ask us not to hurt each other so much in how we take apart our disagreements.

To quickly summarize, I think where we would like to go is I think there is the possibility of wrapping up the major issue of Lathrop Wells, depending on how you look at this risk sensitivity. In some respects, I like Don's point that we may be somewhere bounded in the range of, say, 65 to 150. We could probably accept that if we didn't kill each other over whether we accept that. That level of resolution may be sufficient for us as far as a chronology issue. It will not be as far as the multiple versus single events.

We plan to extend some of these detailed studies now into the Sleeping Butte/Crater Flat area to try to refine the chronology there. We don't think we have the major discrepancies, with the exception of the possibility of a young event at the hidden cone center. We plan to extend our field studies. We've been doing a lot of new work at trying to gather volume data which will help us refine some of our calculations, and we do plan to do some additional work on these Pliocene centers that have been newly recognized; particularly, the Thirsty Mesa probably requires some mapping, although from what the USGS said, they may have done sufficient mapping that we can use that as part of our studies.

We also plan to do quite a bit more work on this issue of resolution. We'd like to try to yearly update our E1 and E2 tables, and basically try to refine those and come to grips with how to best present those tables. Greg presented a lot of data. We hoped, say in the next meeting, to be a lot further along on our field analogs and our E3 constraints to be able to answer the question about can we bound the eruptive probability of the eruptive scenario based on those, and as I told you, we hope to institute the geophysical data review and are very much sensitive to the fact that we would like to see a more complete and thorough integration of geophysical data across this.

I think I'll stop at that point.

DR. ALLEN: Okay. Thank you, Bruce.

Any quick comments or questions from the Board?

(No audible response.)

DR. ALLEN: Incidentally, I think we appreciate your plea for more data and less rhetoric; on the other hand, I'm also impressed at this meeting. I've heard an awful lot of data, awful lot more data than rhetoric compared to last year, so I'm encouraged.

DR. CROWE: Great. Thank you, I appreciate that.

DR. ALLEN: So I guess next on the program is John Trapp with the Nuclear Regulatory Commission with an NRC perspective.

MR. TRAPP: When I was asked to put together this talk, I was basically asked to cover two points, just so that most of you people can understand where I'm coming from.

I was asked, first off, to basically give kind of a repeat of a talk I gave last spring during the Waste Management Conference, and I was also asked to provide some comments on the various study plans, et cetera, that the NRC has looked at, and to describe what our problems were, where we were coming from, where we stand on this resolution.

On this thing, I'd like to--the title, I'd like to point out two things. Number one, this is the "NRC" perspective. This isn't the John Trapp perspective. There's been more people at our agency that have looked at these damn slides than I care to talk about.

There is another point, too, and there is a perspective and it has to be understood. We've heard an awful lot of technical data, et cetera. The NRC perspective really is not so much the technical, but we're interested in the licenseability of the site, and the licenseability of the site is a legal problem, it's a political problem, it's not a technical problem. What we've got to do in this whole thing is make sure that the data, the whole program meet the legal constraints so we can use the technical data to solve the legal problems.

Now, the basic question that I'll be trying to answer at least at the start of this thing is in our investigations, what are the regulatory requirements that control the level of investigations? What level of proof's needed, or, you know, what the hell do those guys want?

In going through this, it allows us to look at a couple spots in the regulation which are quite important. The three things that we need to take a look at is the overall system performance objectives and volcanism, if you want to believe it, has one simplifying factor over many of the other things that I could be talking about. In volcanism, I am really only looking at the overall system performance objective. I'm not looking at concerns for the pre-closure. So I'm not looking at 60.111.

I'm also not looking at the ability to meet the sub-system performance requirements, so I'm not looking at 60.113, so I can narrow my comments, but my comments that I'm going to be going through can be carried through to these other areas. This gives us a chance, also, to take a look at the PACs, the potentially adverse conditions, and try to understand why these are in the rule and what they were intended for; and most important, it gives us a chance to take a look at 60.122(A)(2), which really is what I want to be spending most of the first part of this talk on, because this is the qualitative description of the what the NRC is bound by as far as how we are going to be looking at these investigations.

I will point out one thing, this 60.122(A)(2) is most likely going to be taken from the part of the rule that it is and moved to another, because the lawyers feel it should be better in another part of the rule. It doesn't change anything that I've got to say, but if you see it being changed, that's why.

If you take a look at the overall system performance objectives, the cumulative release of radionuclides to the environment, there's a couple points that need to be brought out, and this is an area that there's kind of some disagreement or misunderstanding between what we seem to be asking for and what we seem to be getting from the DOE.

What we're interested in is the probability for categories of processes and events, not the probability for an individual process and event. We are not interested in the probability for a single cone, a single dike, et cetera. What's the probability of magmatic disruption of a repository of all types? This is the bounding numbers, and when we're talking about consequence, what we're interested in is the required summation of all releases that happen during the 10,000-year period, not just the releases that happen from the volcanic events itself.

The buzz words that get thrown around are basically these scenarios have to be complete, comprehensive, and mutually exclusive. What this really kind of amounts to is if you take a look and try to plot this out in, say, a two-dimensional graph, you've got to have a graph which stretches out 10,000 years in one direction, and in the other direction, basically covers all the processes and events that can lead up to some type of release to the environment, and you've got to make sure that when you are done, that you have got 99.9 per cent of all these things accounted for.

If you sit and start moving events, processes, et cetera because, well, gee, I can take a northeast dike and throw that out because that's a got a low probability, then I'll throw out a northwest dike because it's a low probability, what you end up with is a sequence going through here where you have removed some of the probability and you no longer have the 99.9 per cent amount of processes and events that are left.

It's the reason that that one chart that Bruce showed, where he's showing the philosophy, or I'm not sure exactly what. The charts came down to two different things, where you say, we'll throw this one out for eruptive and we'll throw this one out for disruptive; no. It's right up at the top. If you get below a certain probability limit there, yes, but once you get into that loop, you carry it through the performance assessment.

Now, if you take a look in the rule, there is a whole series of potentially adverse conditions, something like 24 of them listed. Now, when the rule was originally put together, most of the people at the NRC that did the calculations basically said, hey, if, you know, you've got a good site, you don't have any real problems with it, it should be real easy to demonstrate compliance with all the system performance objectives. And then they said, well, what could change this? What could cause us to lose confi- dence that we can really make these projections?

So they sat down and said, well, you know, if there's volcanism, that could cause it; faulting could cause it; significant earthquakes could cause it; some geochemical anomalies could cause it; and they sat down and listed them. So these are all in there because they are things which, like I said, lose confidence in the ability to make these projections for the time period, and it's real important to note that all these PACs do have to be considered in performance assessment. It doesn't mean that they all have to be carried through to the last calculation. What it means is you've got to get down to this process and event, this breakpoint of the process and event, and at that time, you can cut them out.

So what, basically, do we need? Well, what we require is a three-step process, and there's a couple points here that really need to be brought out. The first thing is basically establish the degree of resolution of the investigation. The important thing that a lot of times gets forgotten is not only what did you see, but what didn't you see because you didn't look hard enough. In other words, what could be present and undetected?

One of the easiest ways to explain this is if you're talking about a geophysical survey, you'll normally say, well, I've got resolution of X number of meters, and you may say, okay, I've got resolution of 50 meters. So I found, say, three faults that have 50 meters or more of offset on my geophysical survey. Could there have been faults of 40 meters? You're damn right there could have been. What effect would these have on the calculations?

If you want to carry this into some of the things that were discussed today, Bruce brought up his aeromagnetic side, and basically he said we've got all the centers and all this type of thing detected, or he thought he did. I would agree that we've probably got all the major ones, but how big a center, how big a dike, how big something could be present and not detected, and what effect could these have?

When you do the analysis, one of the things you've got to do is demonstrate the sensitivity of all these different things and assure that you haven't underestimated the effects. Notice, I didn't say that you've got to overestimate, but you've got to make sure that you didn't underestimate the effects, and finally, you've got to demonstrate that it's insignificant, it can be compensated or remedied or mitigated. That last point is really just to say that there is some flexibility in how you demonstrate compliance.

So, what was the basic question? Well, the response is that the regulation does have some qualitative guides; not quantitative. It's now going to sit down and say, hey, you know, if you do 100 borings, that's great, or if you do ten miles of seismic land, that's great, but it's got some qualitative guidance. You've got to describe the program of investigations, the resolution obtained, features, characteristics detected, and most important, what you could have missed. And you've got to do your assessments with a balance of knowns and unknowns to make sure that you don't underestimate the effects.

Now, is this new? Is this new to the High Level

Waste Program? No. I used to do stuff legitimately for a living as a consultant, and one of the things I worked on was the Byron Station. On the Byron Station we had a Category 1 pipeline that passed over a whole series of karst topography. We went out and mapped this area. We did some geophysics. We did some borings, and we put a report at the NRC which said, you know, this is what we've found. These are the size of the features, this is what they look like, all the other kind of things, and the NRC came back and said, hey, how big a karst feature could you have missed? And so we sat down and worked it out, and the NRC basically came back and said, hey, design for that size feature.

The philosophy we've got here is basically the same type of philosophy. It's a philosophy that's been carried through in many other NRC licensing actions. So let's talk about some example concerns--and these are much simplified, but let's just try to bring them in and maybe tie a few things together.

We're really concerned about the interrelationship of the surface and subsurface features. Now, we've got our primary data base, which is on the study of surface features, and we've got fairly high resolution. Subsurface? Dikes, sills, all this other kind of things? What is in the subsurface? We've got relatively low resolution on.

If you sit and take a look at most of your volume frequency calculations, if you take a look at some of your calculations that have been done so far on effects, they're all really based on these surface volume relationships, and if you take a look at something like, say, the Hawaiian Islands, what you've got there is normally a three-to-one ratio of one-third of the material comes out in the surface, about two-thirds is in the subsurface. Now, is this the same type of a relationship that we've got at Yucca Mountain or in the basin and range, or does it even make a difference?

What we need to do is basically find out what this relationship is, does it make a difference in our calculations, and make sure we don't underestimate the effects.

Structural relationship to volcanism. There has been a whole bunch of discussion about the northeast trend or the northwest trend, or people like to say the Bruce Crowe trend versus the Gene Smith trend, and right now the data isn't such that you can discriminate between the two. We do know that if you sit down and do an analysis, that our effects are normally greater when we're talking on the northeast models than when we deal with the northwest models. Now, if we can't discriminate, the NRC is stuck with a point where they've got to decide which model to accept. Generally, if there is no basis for discrimination, we'd be taking the northeast model.

Now, I was very pleased to hear that there is a whole series of other models being considered because, very honestly, by the time we get to licensing, I don't think any one of these models will really be there. But the basic point will stand, can you discriminate between these models? How do you evaluate what you do? Hang onto that a little bit, because I want to go into it a little bit more later.

I'm not sure how much I want to say about this because it keeps on getting beaten to death in all these meetings, but when we're talking about this, the age of events, we need this kind of information so we can sit and decide, are we talking waxing, waning, are we talking polycy-clic, monocyclic? How do you define an event? Was Crater Flat one event? Was Crater Flat four events? Was Crater Flat 25 events? How many events were in the basic area? Can we see them at the surface, or are there something like another hundred events that were so much smaller, didn't get to the surface, that also need to be brought in?

We need to get some resolution on this age deal and try to figure out where we're going, or we need to come up with some agreement on some type of bounding analysis which will meet the other requirements so we can move forward on this.

We've got some concerns with the presence of magma chambers in the site vicinity. Now, this has been discussed a little bit, also. You've got the seismic refraction lines that was run in the Amargosa Desert which said there could possibly be a magma chamber there. There's a whole hell of a lot of other explanations possible, and we've got a low degree of resolution.

If you take a look at the study plans, et cetera, what you basically see is this could affect your probability calculations. If we don't have more data so that you can sit and somehow decide whether this really is a magma chamber or not, put some resolution on it, then you've almost got to assume that the magma chamber's present. Bruce was saying more data. I agree; a lot more data.

Now, the consequence analysis, I am a member of MSA to date, and I'm also talking about what's been officially submitted to the NRC. We basically, so far, think we've seen things which underestimate the effects because they haven't taken into account the secondary effects, the fracturing, the change to the groundwater, the flow paths, the heating, this type of stuff. They generally assume a single feeder dike. Now, if you take a look at Red Cone and all this other kind of stuff, there are more than one feeder dike that appear to be present.

The models assume that the repository will react the same as a country rock, and it's not, it's an anomaly. There's a totally different tensile strength here. It may have a different weight, but there's different tensile strength of the canisters, et cetera, and if you start putting a dike through there, what's going to happen? And the possibility of a hydrovolcanic stage has not been considered.

Like I said, to date, these have not been brought in. Hopefully--and I was quite encouraged with a lot of the things I saw in the proposed study plan, or the study plan that's in house at Los Alamos, because it's starting to address an awful lot of these concerns. However, the NRC has not seen it yet, so I really can't comment more than that.

This thorny one, expert judgment. The licensing board will use expert judgment. You know very well they will. But the licensing board has also got to make sure that they're guided by 60.122(A)(2). They've got to live by it, also, and it doesn't increase the data base. If it's undetected, it's still going to be undetected, and it doesn't eliminate the requirement that you make sure that you don't underestimate the effects.

What it really does is give you a more informed decision, and we keep on saying DOE should be cautioned about reliance on expert judgment if they can get some data reasonably. Now, a couple of points just to carry this through. This weighting, biasing, all this other kind of thing keeps on getting thrown out, and if you took a look, for instance, at those, quote "two structural models," we had before of the northeast and the northwest, let's assume that when we got to licensing, those were the only two models we had.

Now, they're incompatible models, so if one's right, the other is wrong. Let's say we decided to weight this and half the people thought that this one was the right one, the other half thought it, so we gave them each a 50 per cent weight. Now, what we've got in a situation like that is 50 per cent of 100 per cent and 50 per cent of zero. You try summing this up and putting it into any calculation, what you've got is a calculation which only covers half the probability space. What have you got? You've reduced the uncertainty under a condition like that, but weighting in that manner has basically reduced the uncertainty because you know very well you've got the wrong answer.

Conclusions. Well, basically, at present, we think there's a very limited data base. We think there's an awful lot of models that can be used to explain the date base, and we think there's a lot of areas of concern that have got to be resolved.

60.122(A)(2) does provide some qualitative requirements that'll get you--should be used by DOE when they're putting together this program and trying to decide which areas that they need to go into a little bit more detail, because in licensing, you've got to demonstrate compliance with the performance objectives and you've got to address the PACs by describing the investigations which were including those things that could be undetected, and assure that your analysis used is not likely to underestimate the effects. And like I said, don't rely on expert opinion. If you can get the data, if there is any way that you can use data to support it, it's going to make the case much, much easier to be sold.

Well, this is going to be a second part of this presentation, and summary of the NRC concerns with the SCP and the DOE study plans on volcanism. I wanted to note--and I've already alluded to it--that I'm going to be discussing two study plans, and at the beginning of this thing there were three study plans discussed. We haven't seen the third study plan, so I can't comment on it, aside from saying it appeared to be encouraging from my listening to what was being discussed.

If we go back to the SCP, there are several points that need to be brought out. One of the basic comments that came through in the cover letter was we felt that investigation of the tectonic phenomena, et cetera, should receive early attention, high priority.

Note B, integration of a site characterization program, there was a tremendous amount of concern expressed with this problem. We had specific open items relating to volcanism in three areas: In geophysics, we were talking about the integration of the geophysics program. We had questions on the sufficiency of the geophysics program, and we had specifics on the geophysics program as it was related to volcanism. These are open items. They have not been resolved.

We have questions on the DOE performance measure, parameters, et cetera, versus the NRC performance objectives; in other words, are the studies being put together in a manner that really will resolve the licensing concerns? These have not been resolved.

And we had concerns that the volcanic rate calculations appear to be independent of the knowledge of the process. That also has not been resolved.

We received a study plan on the characterization of volcanic features. In general, we took a look at it and said the work outlined in the study plan appeared needed. It was what we basically described as a necessary, but not sufficient part of the program. We had some minor questions on age dating, basically what techniques they were going to use, why they were going to use them, this type of thing. That's been resolved. We had some questions on why some core was going to be oriented, why some core was not going to be oriented, et cetera. It was really kind of a nonsense question, but that one's been resolved. And we had questions on analogs; which analogs were being planned, where they were being planned. That has not been resolved.

We basically stated in the cover letter an ongoing concern with the integration of the program, and we were told in the cover letter coming back that, read the SCP. Now, if we had thought it was in the SCP and if we'd found it in the SCP, we wouldn't ask the question, so this definitely is still an ongoing concern.

We recently finally got out the comments on the probability of magmatic disruption of the repository, and we had a video conference, not conferences, on August 25th where we sat and discussed these things. We basically had a con- cern about having the sufficient and necessary data to be able to do these probability calculations and to be able to understand magmatic processes. The emphasis here was on the geophysics program.

We had a concern on the use of expert judgment, emphasis on weighting, and then we had a series of specific comments, and basically, the main theme of these was the scope, we thought, was too narrow to solve the regulatory and technical concerns.

Now, before I go into these I just want to make something clear, at least according to my understanding of what came out of this conference. In reading the study plan, in reading the scope, in reading the title and the whole thing, we had certain expectations on this study plan as to what it was supposed to cover. We were basically told that our expectations were not what the study plan was intended to cover. It doesn't mean that the concerns are less, it just means they're supposed to be discussed someplace else, and I right now don't know exactly where they are. I think some of them are going to be in this third study plan, but that is not all of them.

According to my best guess, if I take a look at the total program of all the study plans that are necessary to understand everything about volcanism so we can put it to bed, I came up with 22 study plans, and I have no idea what the progress of these study plans are or when they're going to be available. Very few of these study plans are under Los Alamos's control.

Well, we had a concern that the events of concern are more than just cone formation, because the study plan seemed to be totally aimed at direct disruption of the repository. We were basically told that some of our concerns would be discussed in other study plans. I believe this is going to be mainly discussed in the effects one; right, Bruce?

(No audible response.)

MR. TRAPP: So most of this, hopefully, will be found in the next study plan. I don't think all of it's going to be there, because I think an awful lot of it's got to be found in the performance assessment study plan. I think an awful lot of it's got to be found in the ones dealing with waste package disruption and this type of thing.

We had a concern about extrusion rates used as a surrogate for magma production rates. I'm not going to go into that. We had a concern about crustal bodies needing evaluation, magma bodies. I've discussed that a little bit previous, so to save time, I'm not going to go into that any more.

We had a concern that the probability calculations did not appear to include all the significant processes and events, and again, they didn't. Basically, the probability calculations, the way this was put together, appeared to be more aimed--and this kind of goes in one of the later com- ments--at meeting the requirements of 960 than it did in meeting the requirements of 10 CFR 60.

We had a concern about the validity or recurrence models without a range of input parameters, because if you sit down and take a look at the cones that you've got, if you're using these as your basic data point, you've got a very limited distribution of parameters that you're dealing with. If you're sitting there and trying to put something together like a seismic recurrence curve, you need some smaller events so that you can kind of put this whole thing together and give yourself a nice line, a nice type of curve that you can use to start figuring things out.

We had a concern--and this one maybe should have been a question instead of a comment, but it was basically a recurrence rate versus the average recurrence rate. Bruce contends--and I think he's probably right--that this has been beaten to death in a lot of earlier papers. Our real point is we want to make sure that when you're talking about the validity of these calculations, that we make sure that we put the right caveats on the calculations so that we know what we're really discussing. What are the assumptions? What are the uncertainties, this type of thing, that go into all these calculations?

We had a concern about the full consideration of non-Poissonian models. Now, I agree that with the data set that we've got, it's much easier to work with the Poissonian, but we do need to look at the possible non-Poissonian models to find out if these models can possibly better explain what's going on. We need to look at stuff like self-organized criticality. We need to look at fractals. We need to look at the whole thing and try to put together some models which describe the dynamic processes which were discussed earlier.

We basically said the disqualification formula appears invalid, et cetera. Well, we're talking about 10.60, and I contend that the disqualification formula wasn't valid for 10.60. I'm not going to say if it's valid for 960 or not. If it's a 960 disqualification formula, fine.

We basically said, again, that we thought the input geophysics program appeared too limited. This one, it's not limited to the volcanics program. It's basically the whole DOE program, the program of tectonics, everything. If we're going to go ahead with anything dealing with licensing this site, somehow we've got to go ahead and start getting some good, solid geophysical data, and this data has got to be used in all these different disciplines and brought together.

It was agreed that this was an area that we perhaps needed some more discussion. We had one discussion dealing with the White Paper, and we didn't get too far on that, but it's an area that we've got to go ahead.

The next two basically deal with some questions on the mathematics, et cetera. They appear to be all taken care of. They should come back in the others, and we had questions about the use of weighted models and bias reduction. We had questions on the use of expert judgment.

Supposedly, there's going to be a series of papers coming on back or a report, not a report, but a response to these things, I believe, sometime by November, and at that time we can find out if we're closer or farther apart, or actually where we sit.

Now, this is something I think needs to be brought up for several reasons. These three different things, we feel, are kind of significant in the whole discussion of not only the volcanic problem at Yucca Mountain, but the structural problem as well, because in this Evans and Smith paper, there basically were three points that needed to be brought out.

Number one, if you take a look at the area around Yucca Mountain, directly under Yucca Mountain there was a very strong offset noted, a very significant offset noted. If you take a look at the old gravity data, the gravity data basically shows an offset under Yucca Mountain. If you listened to some of the talks today from the state, there was a discussion about caldera boundaries. One of the things that's been discussed on some of the possible interpretations of this offset is a caldera boundary. If caldera boundaries can focus volcanic events, and if we've got this offset underneath Yucca Mountain and it's a caldera boundary, then we've got something under Yucca Mountain that can focus volcanic events.

Another thing that came out of this is, if you take a look at it, there was basically a difference in the area under--to the north under Timber Mountain versus the area at Crater Flat; difference in velocity, et cetera, in those two areas, which basically suggested that the Crater Flat area, et cetera, is a much more likely area to have volcanic activity.

The one that's gotten the most attention, though, is this north--I mean, this east/northeast trending low velocity zone which is basically talking about three parallel trending zones, which he's interpreting as zones of low partial melt. The basic significance of this Humpreys paper is it's another paper that was presented, or he's noted the exactly the same thing, these same three zones, and he came up with one of the possible interpretations as exactly the same thing that Smith's talking about.

What we've got here is a possible zone at approximately the same depth that was discussed by Frank Perry as being the depth that most of this stuff, he thinks, comes from. So we do have the possibility of being able to say that this really is the source for this volcanic material. Now, it might not be. It might be something else, totally, and that's why this last one's so important, because in the last one Evans sat down and he talked about a whole bunch of different studies that could be done to help resolve some of these concerns, and I guess our points--the NRC's point would be that this is a letter, this memo, that should be read very closely because it's an area that we think needs to be looked at, needs to be considered and studied.

Presently, what's our perspective? Well, actually, our present perspective right now is kind of negative. We haven't seen an integration of the site characterization program. We haven't seen where the geophysicists, structural geologists, the earthquakes boys, the volcanologists, all these are working together for a common end. We haven't seen integration of the volcanic program. We haven't seen where Los Alamos, the USGS, and all these other guys are working together to get to the end.

We don't see a geophysical program that's sufficient, and that part of the program which you've actually had submitted doesn't appear to be sufficient to resolve the regulatory concerns. Hopefully--and on this last one--hopefully, the other study plan that's coming in and some of the work that was discussed, or the possible discussion on geophysics will resolve these concerns and we can get a lot closer together.

That's it.

DR. ALLEN: All right. Thank you, John.

Are there any quick questions from the Board?

(No audible response.)

DR. ALLEN: I think we'd like to move right ahead. I was mistaken a few minutes ago when I said the State of Nevada was going to give a perspective here, but we do now have time for closing remarks by DOE, State of Nevada, and the NRC, and we'll start off with the DOE. That's Jeanne Cooper.

DR. COOPER: Clarence, I think I'm going to hold a couple of my remarks for the round table discussion, in the interest of time. I'd just like to make one point, and that is that I'd like to make a plea for the importance of all the aspects of volcanism studies. I realize that some of the technical details may be hard to swallow at times for some of the aspects, but I'd like to try for us to keep a perspective on the geochronology issue, in that while it's exciting and controversial, it is not the entire volcanism program and, in fact, it may not be the most important issue for us to look at.

I would also like to thank all of the presenters for their high-quality presentations and the interpretations of data they've presented over the last day and a half.

DR. ALLEN: Thank you, Jeanne.

State of Nevada, Carl Johnson.

MR. JOHNSON: Carl Johnson. I, too, would hold most of my closing remarks to the round table discussion.

DR. ALLEN: I'm going to--let me warn you, this round table discussion, I can't guarantee exactly who's going to get how much time.

MR. JOHNSON: Yeah. Given the time schedule and the round table discussion may be somewhat abbreviated, I think there's one point that I'd like to bring out to put at least our studies into perspective, and it gets to the statement I made in the opening remarks.

It's not the duty of the State of Nevada to assist the DOE in characterizing Yucca Mountain. We are here to provide oversight, and in that, it is our responsibility to ask questions. The studies that we are doing that we commented on here today was to provide us a better understanding of the volcanic issue so we can be in a better position to ask the more appropriate questions, and that is actually what our role is, so we are not here to assist the DOE. As I said before, it is the responsibility of the DOE to prove that whatever position they take is, in fact, the appropriate one.

DR. ALLEN: Thank you, Carl.

For the Nuclear Regulatory Commission, is it you, John, or Keith McConnell? Keith, please.

MR. McCONNELL: From our point of view, I'd just like to say one thing, and that is our perspective is just that we're in pre-licensing, and what we identified--what John identified as concerns are strictly concerns. It's areas where we think that the program is lacking, at least from the basic information that we've gotten to this point. So it's not necessary, I don't think, to read too much into some of our concerns if studies further down the line will provide the information that we've identified is necessary for licensing.

DR. ALLEN: Okay, thank you.

Let's take about a five-minute recess, because we have to rearrange the tables up here, but I would ask that all the people who've spoken during the meeting be prepared to sit around up here and have at it.

(Whereupon, a brief recess was taken.)

DR. ALLEN: May we reconvene?

I think the audience is outnumbered by the people at the round table here. I regret to have to report that we are still being recorded, and therefore, people, please identify yourself, even though your names are in front of you. And if you're speaking--what happened to our micro-phones from the audience? Well, if the audience wants to participate, please come up and use Gene's.

I might--I say I regret this because I'm personally opposed to this kind of routine of the court reporter for this kind of a session, and I have voiced that objection to our Board, but apparently our lawyers demand otherwise or some such thing. I don't quite understand it.

I'd like to start out, although it's not on the program, by asking Bill Melson to make a few comments from his perspective. He's asked a number of questions. A number of you have wondered, perhaps, what some of his thoughts might be. He's our, the Board's, consultant in volcanology, and I wish, too, Bill, though, that you would, if you can, try to address these four questions of the issues on which consensus is developing, on which there are serious differ-ences, which of the issues is important in terms of licensing, and then how can these issues be resolved. So, please go ahead.

DR. MELSON: Well, I don't know if I can answer all those questions or even speak to them, but I'll make just a few comments.

I think that we've seen a lot of progress since our last meeting. I think the age differences, although there are still some problems, are getting certainly resolved, and there may be a question about how much more we really need to do to resolve some of those.

I think the magma dynamic modeling is moving along very well and is certainly important, and it'll be interesting to see that continue to evolve.

There has been a lot of really good work here and I don't really want to review it all, you've all heard it. I think John Trapp's final comments were very insightful in that all bases have to be covered, and I see that happening progressively every year, and I don't know where the end is or what it'll look like, but we're moving toward it.

I would say one aspect, which I just had a conversation with Chuck--what is your last name--with Chuck Connor about, and that is the area of interest to me, which is active volcanism, meaning new volcanos, and these provide something we're not really looking into; that is, what can we learn by looking at volcanic eruptions like might happen at

Lathrop Wells and the Lathrop Wells area, and certainly Sierra Negro has been mentioned, which was recently in eruption, and what do we actually observe happening to the water table in these particular circumstances?

And it seems to me my role at this meeting has often been to move us further from resolution by suggesting things of this sort, but nonetheless, looking at thoroughness of approaches, I think this is a question which Chuck raised, which I would certainly agree with him is a very relevant area of future investigation.

I think I'm going to stop at this point, because I could continue rambling, and just say I think we are making major progress, and I think this is going to continue.

DR. ALLEN: On which issues do you see consensus developing?

DR. MELSON: Certainly, we're constraining the ages of the cones a lot better. I think the hazard assessments are being very circumspect. I think that Gene's models are certainly, I would suggest the worst case, which we have been invited to look at in terms of the hazards. I'm not sure there's a convergence there, and I'm not sure there's a convergence, either, about how we're going to calculate probabilities. I would say that there isn't--we aren't close to, in my opinion, at looking at whatever realistic probabilities are. We're seeing a lot of divergence there.

In terms of convergence, I think we're seeing convergence of our data. We're seeing the data slowly being reconciled, and it is becoming a question of what models fit that data better.

DR. ALLEN: On one previous occasion I remember we went around the whole table and asked everybody to make some sort of a statement. I'm not inclined to do that, because we'd be here until eight o'clock.

What I'd like to do is make a couple of sort of hypothetical statements, and ask to what degree people might --if anybody disagrees with these, and let me start off with this statement:

It is credibly possible that at Lathrop Wells the volcanic event or events, all of them, occurred between 65,000 and 140,000 years ago. Would anybody say that's not credibly possible? Particularly the, say, the geomorphic people. Would you agree that it is possible that that event or those events all occurred between 65-140,000 years ago?

DR. WELLS: I'll start on that.

DR. ALLEN: Steve Wells.

DR. WELLS: Steve Wells, and I'll let Les pick up a little bit on that.

As Les pointed out in the presentation, we argued for the date of 20,000, based upon the use of soils and varied soils where we had radiometric calibration, such as on beach ridges, and we had C-14 dates, and so that's what directed us towards saying that, and I think what's been lost so far in this discussion at this meeting is that we still feel that 100,000-year-old flows are not incompatible with what we said in our geology article, which was that the youngest event at Lathrop Wells, we thought, was incorporated in that 20,000-year time frame, and that we did not rule out that there were older lava flows, and in fact, the more stratigraphy that's done, it would seem that there is an older history there.

So my personal feeling is, based upon the geomor-phology--and Les can address the soils--I would have a hard time believing that you could push it way back, say 50-60,000 years, without seeing some kind of modification. So I would still stand by the fact that the quarry site which we'll see in the field has a youthful age to it; i.e., less than 20,00-0, which has been supported, in part, or is compatible with the work that Steve Forman has done on TL dating.

DR. ALLEN: Okay, so my statement, or hypothetical statement that it is credibly possible--and I don't say physically possible, because that gets a little bit absurd, but whatever credibly possible means--that all of the volca-nic activity occurred between 65-140,000 years ago, you would not accept that?

DR. WELLS: I don't feel completely comfortable with that, no.

DR. ALLEN: I'm not asking whether it's likely. I'm just saying whether it's sort of credibly possible. You don't feel that's credibly possible?

DR. WELLS: I hate to ask you to define what you mean by credibly, but...

DR. ALLEN: Well, like the maximum credible earthquake. God knows what it means, but--

DR. CROWE: Clarence, let me just interject something. Maybe we could caveat your statement by saying could we accept that the flow sequences could be between 65 and 140, and I think there the answer would be yes.

DR. ALLEN: That's a little bit different question, the flow sequences alone.

DR. CROWE: Yes.

DR. ALLEN: Would that--

DR. WELLS: Yeah, that doesn't bother me because a lot of the scoria sits on top of that, and that's where we have our soil bounded in conformity. So that doesn't seem to be unreasonable.

DR. ALLEN: Okay. Well, that's an important, I think, an important point.

DR. DePAOLO: Yeah, I wanted to make that comment, too.

DR. ALLEN: Don DePaolo.

DR. DePAOLO: That most--almost all the geochronology that you've seen applies to the flows. There's a little bit on the cone, in addition to the soils data, and there's--I would go along with your statement, but with the caveat that there's still a substantial possibility that the cone is younger than 65,000 years.

DR. ALLEN: The non-flow aspect of the cone, yeah.

Let me make another statement and see whether there's agreement on this, and I suspect there might not be. Any credible further volcanic activity within Crater Flat in the immediate proximity of the existing late Quaternary activity would not pose a threat to the safety of the repository.

Carl, would you agree with that?

MR. JOHNSON: I think based on the work that we have done and our viewpoint of what we read, both the Part 60 requirements and 960 requirements, we can't agree, cannot agree that it does not pose a hazard.

DR. ALLEN: Okay, in the immediate vicinity, in Crater Flat; in the immediate vicinity of existing late Quaternary centers?

MR. JOHNSON: Yes, in the immediate vicinity.

DR. ALLEN: Okay. Let me rephrase it. The same thing, but talking only about direct effects. Is it possible, credibly possible in your mind that any kind of volcanic, reasonable volcanic activity in Crater Flat, in the immediate vicinities of the existing late Quaternary centers would have--could have a direct effect upon the repository in terms of its safety?

MR. JOHNSON: I think that centers in Crater Flat could have a direct effect on the repository itself. Given all the unknowns of how volcanic processes occur in the Crater Flat, I would say yes, it could have an effect.

DR. ALLEN: Could you explain how that might be? I'm trying to use direct and indirect effect in the same way that various people up here used, a direct effect being--well, as it was expressed, you know, something going right through the repository, which clearly is not the case; an indirect effect, quite different, groundwater changes, heating, thermal--all kinds of things related to proximity but not to direct penetration.

MR. JOHNSON: I would agree that in the definition that you're throwing out, direct effects meaning that you have a disruption of the repository itself, then that does not mean it has to go to the ground surface, but it's a disruption of the repository itself, and I still continue to agree, or say that it is possible. We don't know enough about the processes of magmatism that goes on there to discount that at the present time.

DR. ALLEN: Any further comment on that question by--yeah, Bill Melson?

DR. MELSON: Yeah, I'd like to speak to that just briefly. The kind of effects we want to address in your question would be tephra fall, air fall deposits hitting the external structure and doing damage to it. We might imagine a base surge, say, as it occurred at Lathrop Wells, which would have absolutely no effect, and presumably, given reasonable construction, the air fall would also have no effect.

As far as the effect on groundwater, say, let's assume some distance of two or three kilometers in the valley, in the Crater Flat region, if we look at eruptions that have occurred, their impact nearby on groundwater in most cases is minimal, so I think we could work toward a consensus that something within Crater Flat would have minimal impact on the site.

Let's be more specific. Let's say at three to four kilometers from the site, so we can remove immediate impact on the groundwater effects and things of this sort. The closer in that it gets, this of course becomes a question.

MR. JOHNSON: Clarence, I thought the question dealt with direct effects. Secondary effects, which, I think, is what Bill addressed, is something completely separate and I would agree with all of the points that Bill just brought up, and maybe even add a few other ones dealing with the effects on geochemistry, the effects on the stress regime in the area. Those are just what come off the top of my head;

secondary effects.

DR. REITER: Carl, I didn't quite--could you give us some idea as to how, say, an eruption at Lathrop Wells could have a direct effect--direct effect meaning intrusion of the repository? Maybe--I'm not quite sure I understand.

MR. JOHNSON: I think the point that I was trying to get out is we don't know enough about the magmatic processes to be able to constrain an eruption to the immediate area of Lathrop Wells and not discount the possibility of some diking heading towards the repository itself, especially given the zones of weakness, faulting, fracturing that we know have trends in that direction.

DR. ALLEN: So it is credible in your mind that we could have an eruption, again, let's say, at Lathrop Wells or close to it, with a dike that extended right through the repository?

MR. JOHNSON: Yeah, that's what I'm saying.

DR. ALLEN: Not likely, but credible, huh?

MR. JOHNSON: Credible.

DR. ALLEN: This afternoon I heard a statement someone made. I forget who made it. Let me see to what extent there is agreement on this. A statement was made, if I heard it correctly, that polycyclic eruption or the polycyclic scenario was necessarily--it necessarily involved more risk to the repository than monogenetic.

Would anybody agree or disagree with that statement? Jack, we appreciate your input, but it's got to be through the microphone.

MR. EVERNDEN: This gives me the opportunity to ask Bruce. I would have sworn--and I may be wrong--that you said yesterday that polycyclic added no significance to what was happening, and also you said that when I asked what's the point of worrying about the date in detail at Lathrop Wells, that it had some statistical significance.

Today I think you changed both sentences around. You said it was significant that it was polycyclic, and you said it doesn't make any significance statistically exactly what the age at Lathrop Wells is, and all this is is to try and clarify what I think are my understanding of what were contradictory statements by you.

DR. CROWE: I'm sure happy to address that. What I talked about yesterday was the issue of recurrence rates, and what I said is that we do not factor polycyclic events into the recurrence rate calculations; therefore, it has no effect on the recurrence rate calculations. It's the same thing I said today, basically. There's no contradiction there.

What I said was, the potential for polycyclic can affect E3, which is the releases, and therefore, could have a potentially greater effect than the monogenetic. So maybe there just was a misunderstanding that I was referring to the actual calculations of the recurrence rate. I hope I've been consistent. I know I have in my own mind, and that is the difference.

MR. TRAPP: John Trapp. Just one comment.

If you take the polycyclic model, et cetera, and you also take some of the statements that were made about Lathrop Wells--and maybe I'm misinterpreting them, but basic statements that say that Lathrop Wells is at its last stage, or it's basically going through an ending phase, unless you know the cycle, what you're basically saying is it won't occur at Lathrop Wells, it'll occur someplace else, and you are back at a totally different probability, the probability of some type of event, a new thing happening. So it's really necessary to understand where you are in this whole cycle, this whole volcanic cycle before you can really make that type of statement.

DR. ALLEN: Anybody else wish to comment on this?

DR. PERRY: Frank Perry. Can I just respond to what John said?

It'd be nice to know if, you know, where Lathrop Wells is in its stage and, you know, some of the evidence I presented suggests it might be waning, but we don't--we'd like to compare that with the other centers in Crater Flat, that they also have that type of behavior. Until we compare it with other centers, you know, we can't confidently say what stage it's at. There's just a hint that it may be waning, but that may have no significance.

DR. ALLEN: My own hunch is that we'll never be able to prove that and demonstrate it adequately enough to the scientific community, not to speak of the public, to use in a licensing to somehow quantify the probability in a licensing arena.

DR. CORDING: Was is the field that was waning? I saw some diagrams showing Buckboard Mesa and all. That was the field that was waning. Do you also have something on Lathrop Wells itself?

DR. PERRY: At the last TRB I presented evidence that the field as a whole was waning. The volumes declined from four million to the present. There's changes in chemistry that suggest chambers are getting deeper. Then, you know, yesterday I presented some evidence that Lathrop Wells by itself may also be waning, but that's more speculative, and we'd need a lot more data to, you know, to have much confidence in that.

DR. ALLEN: Let me ask this. If we cannot agree whether we have polycyclic or a monogenetic scenario there, and it might well be, judging from some of what we heard today, this argument could go on for the next ten years, does it really make any difference? Why shouldn't we be spending our money somewhere else and just--does it really affect the risk analysis to that extent?

MR. TRAPP: This may be kind of an aside, but I think it might help. If you take a look at the EPA standard the way it's structured right now, it does make a difference. Now, one of the things that has been tried with the NRC and the EPA is basically a modification of the EPA standards, what is infamously known as this three-bucket approach, which nobody really understands. But the third bucket, really, instead of the straight probabilistic-type analysis, gets into more of a deterministic-type of analysis where you can do some bound-ing-type calculations. You agree on what goes into this type of thing, and you might have a better chance of resolving this thing with some bounding calculations, than going on the straight probabilistic-type of thing that we're presently working on.

DR. CROWE: Clarence, let me respond to that a little bit. It really depends on our method of resolution. If we make our argument purely on the occurrence probability, then polycyclic is not important because we're not using it in the calculations. But if we are forced to go into the release part of it, then it does become important. So as we went down through that logic of the issue resolution diagram, it really depends on--if we could make our argument solely on the basis of the occurrence probability, we do not use poly-cyclic at all in that calculation.

DR. REITER: Why is it important in the release?

DR. CROWE: It's important because it means that you just don't have one dike system feeding through the reposito- ry. You have multiple penetration. Basically, you can look at it--if you take the typical dimensions of a dike, say, it's a few kilometers by a few meters long, if you have one dike, that gives you one area; if you have two dikes, you have double the area; and triple, and so on. And so basical- ly, it increases the area of the waste magma interaction.

DR. REITER: But maybe the calculations if you have one interruption, you fail the criteria, so what difference does it make if it's one or two?

DR. CROWE: Well, that's correct. If one would fail, that is correct. That's a good point.

DR. REITER: So have you done those kind of calcula- tions?

DR. CROWE: Greg presented them. Where we did them was basically assuming that all the waste that was contacted was brought to the surface, and yes, that does fail.

DR. REITER: I think those kind of calculations are really important because if it turns out that one will fail, then even for--it'll cause you to fail, then it's not really necessary--and, you know, assuming polycyclic, then even on assessing the impact, polycyclic makes no difference.

DR. CROWE: Except that the important point that Greg emphasized is what is the reduction with depth of derivation of material; that should we assume that everything that a dike encounters gets carried to the surface? And we feel that's an extremely conservative, probably overly unrealistically conservative calculation, and the whole point of the analog studies is to try to address what level of change of frequency of occurrence happens with depth. And so, I mean, I think Greg's calculations are important to show that at the worst case, you could have E3 of one, basically; assume that it's one. But there are other ways that it could be less than one, and that's where we're trying to bound with these analog calculations.

DR. SMITH: I think whether the volcano is polycyclic or monogenetic depends on how you define the term event. Now, as Bruce has reminded me several times today, I have agreed with him in terms of geologically speaking, what a volcanic event is, but I was clearly quite impressed with Frank Perry's geochemical data, and I think he's shown very nicely that there's a series of independent magma batches, and it may, in my mind just thinking about this today, it might be better-- each one of those magma batches is equivalent, in reality, to forming a separate monogenetic volcano. It might be better in terms of defining events to define each magma batch as a separate event, rather than defining the formation of the entire cone as a single event, in terms of risk studies.

I agree that in terms of volcanology, the formation of the entire cone is a single event, but in terms of risk studies, in terms of what Frank just proposed, maybe each pulse might, in fact, be better defined as a single event, and this has profound implications on probability studies.

DR. HODGES: I have a question. Maybe it's sort of a point of departure for this sort of thing. Can we agree or can we ask the question, does it make a difference if it's "polycyclic," does it make a difference if it's episodic or periodic? Because I think that goes back to some of the questions John Trapp was getting at. If it's periodic, then there's a higher probability that you can predict something from it.

DR. CROWE: Let me just take a first shot at that, then maybe Frank wants to comment.

I think we have no basis to make an estimate of that right now. We just--we're so unconstrained on the timing between events, about all we would say is from the soils data, if we are correctly interpreting the soils and the tephra relationship, that we have to have at least a few thousand years between events. On some circumstances down at Cima, and perhaps the Hidden Cone, we may have as great as exceeding 100,000 years between events, and so we're almost completely unconstrained now on whether it's periodic, what the interval is between reoccurrence, and those sorts of things.   Out of honesty, I don't think we could say anything.

Frank, do you want to comment to that?

DR. PERRY: Yeah, I agree that we're pretty unconstrained, but I think it would be nice if we could get to the point where we knew the duration of a polycyclic center from beginning to end, and I think that would have an impact on probability. If it turned out that Lathrop Wells had--all of its activity, say, was between 100,000 and 50,000 and nothing's happened since, then you'd expect that nothing is going to happen in the future; that all the activity was at that time.

If it turns out the last event--and then you would --that would be factored into an event jumping over to the repository, in a sense. If the last event was less than 10,000 years and, you know, you can show there's been semi-continuous activity in several events, then there might be some reason to expect the next event to be at Lathrop Wells, and that would also have to be factored into the probability of an event intersecting the repository.

So I think it's important to know the duration and have some understanding how long-lived these centers are.

MR. McCONNELL: It relates to what Gene was saying, and the--I guess something that Bruce had said earlier, and that relates to the controversy between monogenetic and polycyclic. I thought he said that the only difference between he and the USGS was the time between intervals. Therefore, it's not a question of how many times you necessarily intercept the repository and bring things to the surface, and shouldn't have an effect on the probability calculations unless the interval is greater than 10,000 years.

DR. CROWE: Yeah, and the basis of that is let me ask Duane if he wants to stand by his data set, where he has two discrete positions at Lathrop Wells. I mean, I do have some concerns about whether we can believe the position you've gotten out of the cone, primarily because of the preservation of the cone and your lack of showing your data.

MR. CHAMPION: The two directions of magmatization relate to the QL5/QS5 combination and the QL3 unit by itself, and the cone rim results are independent of the identification of the original two directions.

DR. CROWE: Okay. Let me expand that question, then. What's the basis on which that you can derive good directions off of scoria mounds? You've not shown us that data, nor have you demonstrated that those are good recorders of paleomag data. Do you have a data set that you'd like to present to show that?

MR. CHAMPION: Right now?

DR. CROWE: Well, you can mail it to us. That'd be fine, too.

DR. ALLEN: Not right now, no.

DR. CROWE: No, I just would love to see your raw data. What we have found--I'm being slightly coy with you because it's a point we're going to beat on you tomorrow with--is that some of the sites that you have sampled are floating in loess, and we find it very difficult to imagine how you can get a coherent position out of those, and I'm not saying that you cannot sample good sites, but I can take you to your boulders where you have your drill holes and I can show you that they're completely floating in loess.

And so, you know, I would like to challenge you to show us your data.

MR. CHAMPION: I'm a little astonished that they can be truly floating in loess and have given me the coherence that they gave me. I'm just astonished. I can barely wait to see the boulders.

DR. ALLEN: That would be remarkable.

Bruce, in your game plan diagram with the triangles, some of which were green, and so forth, I'm a little bit confused. At one point on there, you said if the probability was less than 10^{-8} at this point you were home free.

Carl, you made the statement in your introductory comments that probabilistic approaches were not apropos, or at least could not be controlling here. I don't understand this in terms of this 10^{-8} thing.

MR. JOHNSON: The point that I was making was not that probabilistic approaches were useful. It is that Bruce had made the statement that there was an agreement that probabilistic approaches are the way to resolve the volcanism issue, and what I was referring to is the point that the NRC has made over and over and over again, and that is while probabilistic approaches are useful and may be acceptable, deterministic approaches are also important and must be used.

DR. ALLEN: Okay, but that 10^{-8} criterion still must be met, right?

MR. TRAPP: One of the points that I was trying to make in the talk--and I guess it didn't quite get through--is that 10^{-8} the way Bruce has got his charts put together, we disagree with strongly. We disagree strongly with Bruce.

Basically, what we are saying is that 10^{-8} we'll accept is right up at the top. If you've got a 10^{-8} up there on magmatic processes or however you want to define these things, events that can affect the repository, we'll accept that. But when you come down this thing and you start breaking these off into these sub-processes, and then try to use a 10^{-8} to get them out, no, because what you're doing there is you are totally removing bits of the probability space that have to be covered.

So the 10^{-8}, yes, at the very top; as you're going down that chart, no.

DR. REITER: But there's two points. Carl is making--stating that probabilistic won't hack it, and you're saying that probabilistic will hack it if it's done correctly.

MR. TRAPP: Well, I hate to say this, but Carl, I think, is going a little bit off base here because what he's talking about is a series of things that we've talked about seismic studies and unfortunately, with the present structure of the EPA standard, we're basically stuck with the probabilistic numbers. Yeah, we'd like the deterministic to go through, and that's one of the reasons--again, going back to this three-bucket approach--where we think we might have a better chance of getting at it.

But if we stay with what we've got with volcanism, unfortunately, it's got to be a probabilistic answer totally.

DR. CROWE: Clarence, let me see if I can help you on this a bit because it is part of our discussion that we had in our teleconference, that the basic difference there is John is correctly pointing out that you can't ignore the direct releases in terms of the total CCDF. The point that we're making is that if we meet the 10^{-8} criteria, we could argue that the site would not be disqualified solely on the basis of volcanism. So it's a subtlety, but it's an important point.

DR. ALLEN: Solely.

DR. CROWE: Solely, exactly; that's the key point.

DR. MELSON: At the expense of departing ever so briefly from your questions, I would like to put out a question, and the question is--

DR. ALLEN: Please do.

DR. MELSON: --what are the probabilities that we will be sitting around this table five or ten years from now talking about these same issues? I mean, I'm being serious. I mean, what are the predictions about how this is going to be brought to some state of yea or nay and get beyond maybe.

DR. ALLEN: No 10^{-40}, hopefully.

DR. MELSON: I think I'll address that to John and the people in the regulatory business.

MR. TRAPP: One of the things, if you're talking about convergence, going back to this whole thing, and we have been harping for a couple years on the need for certain things to be done; the geophysics, et cetera, all these other kind of things. I haven't seen the direct evidence that these things are getting there, but I'm seeing some indirect evidence that we're getting at that point.

We do need this data. We need the geophysical data. We do need to start getting into these effects calculations. Hopefully, if somebody can light a fire under whoever the appropriate person is in DOE to get started on these investigations and, like Bruce said, to start getting some data to resolving these things instead of discussing hypothetical instances, which is what we're doing most of the way, five years from now we can start answering these things. If we don't have the data, no.

DR. MELSON: Just very quick, Clarence, I'd like to continue; just very quick.

I was really going back to your initial statement where you said this would be decided politically and socially, and I think you didn't really speak to that. You spoke more to getting to the technical completeness. I guess I'm asking you to talk, at least very briefly, about the political and social situations that would precipitate a decision on this issue.

MR. McCONNELL: I don't think we could comment on that. I think that that's an obvious observation, but I would like to speak to your first question, and that is, what the NRC sees as a mechanism for coming to closure, and issue resolution in a pre-licensing stage is kind of a misnomer. The only thing that the NRC is trying to do with its guidance and its input is to come to some sort of convergence on acceptable approaches to resolving the issues at Yucca Mountain. What are the approaches we need to get to the end point? We can't resolve the issue at this stage. We can't resolve it until we get a license and we get to go before the licensing board, and they resolve the issue; we don't. We come to agreement.

DR. ALLEN: John, let me play the devil's advocate for a moment. I'm, at least in part, a geophysicist. You've pleaded for much more geophysical work. Several times you've mentioned the bright spot, or the alleged bright spot underneath Crater Flat. My judgment is that seismologists can play with this for the next ten years and they're still going to disagree on whether or not there's a bright spot there, from the tomography. They're going to disagree on whether it really represents a magma body. But even if it does, let's assume that's a magma body there, as we probably have a number of places around the west. Does that have a direct influence on the licenseability of the site, or upon the safety of the site in comparison to what we know much closer in from the recent volcanic history there of the future of volcanism?

MR. TRAPP: If this bright spot is truly the source, and if this bright spot is active, then we are starting to get to a better definition of the process that John was talking about.

If we are sitting there and saying, okay, we've got this thing down at, say, 60 kilometers that Evans was talking about, if you tie this into this bright spot and start understanding the process, maybe you're closer. Now, the geophysics is not going to give you a definitive answer, but if you use multiple geophysical methods, you can get a lot closer and you can, again, bound the possibilities.

DR. REITER: Based on my own experience, I think, you know, you use the geophysics, it's certainly going to give you a lot of insight, but in the end, you're going to have to rely an awful lot on expert judgment and interpretation, and there's no way getting around that.

DR. CROWE: Just to interject a comment here, Clarence, I discussed this issue with John Evans at the Las Vegas symposium, and basically the question I asked him was, I think, related to the point that you're making here, and that is, if this magma body truly exists and it's existed for a fair amount of time, then I would argue that it hasn't imprinted itself significantly on the surficial processes, and therefore, it isn't a dramatic problem. If it is a new body that's just started to form just when we decided to look at Yucca Mountain, then all bets are off.

And so, John's answer was that he does not think it's a new body. He says that if it is magma, he interprets it that it's a plume trace that's probably been there for a long time, and so in one respect, that's a somewhat satisfying answer, if we then accept the geologic record as our indicator of what's gone on in the past. His only other point to that was that you have an unlimited reservoir for generating magmatism, and he would disagree with the waning volcanism models on that basis.

DR. ALLEN: I guess my concern is we spend our limited money--and it's getting very limited--in the areas where we can learn the most in terms of safety of the site and then environmental concerns, and I guess I'm more concerned about learning exactly what's happened to Lathrop Wells and the other centers very close to Yucca Mountain, where we have direct geologic evidence, than spending a lot of money trying to understand a geophysical phenomenon some distance away that we probably never will understand and we'll all, to some degree, be debating.

MR. TRAPP: Let me point out that when we're talking geophysics--and this is a point that we discussed during the video conference--it is more than the volcanism program. It's the whole program dealing with tectonics, the faulting, the volcanism, the earthquakes, et cetera. It's basically understanding the subsurface in the area of Yucca Mountain.

Right now we have got all these models which--well, for instance, if you take a look at some of the models that have been put out by--on detachment faulting, these different types of things, one of the things that never shows up, for instance, is that big strong offset under Yucca Mountain. Somehow you've got to explain this. Somehow you've got to put the data together and get a coherent model that explains the volcanism, explains the faulting, explains the earth-quakes. You can't have these all separate.

Now, there's got to be some data someplace, and we just don't have it yet.

DR. REITER: John, you just made a big jump from an interpretation of evidence that there might possibly be something underneath Yucca Mountain, to explaining why it's there. I think that's a rather large jump, and I think you have to be careful about that.

I have a question that I want to ask to the volca-nologists, and I want to take as a parallel what we did in our seismic vulnerability. The statement was made--and with a concern that with direct faulting in the repository--that if we, you know, the Board really believes in going under-ground to see what's there. If we go there, when we go there--

DR. ALLEN: As we mention once again.

DR. REITER: --as Don Deere sends us telegrams every day, and we find essentially that very few--unfractured rock, and the assumption is that--we make the assumption that this thing hasn't fractured during the Miocene, then there's very little chance that it's going to fracture in the next 10,000 years, new fractures.

Is there anything applicable to that in volcanism if we go down there and find that there is no evidence of intrusions of dikes, of sills, and I guess the rock is--somebody said, is it eight million years old? I've forgotten what somebody said. How old is Yucca Mountain? Twelve to fifteen million years. If we find there is no evidence of that, can that be used in any manner to assume that the likelihood of lava or magma reaching the repository horizon is so small it can be neglected, based upon experience around the world?

DR. ALLEN: That's really a question for the volcanologists.

DR. MELSON: I don't know if that means me, because there are all sorts of volcanologists, but I can tell you that--actually, I'm going to throw it back into the domain of tectonics. What you're really asking is, given an extensional environment with a magma available either directly from the low velocity zone or at somewhat shallower depths, what's the likelihood, given the particular stress configuration which you don't know, as to where the fracture would occur which would permit the magma to ascend?

And I can see cases where if it's extremely solid, let's say, and there are weak zones, say, off to the side from the normal faulted zone, that would be the zone that would give and, therefore, the magma would tend to rise off into the fractured zone. But you could imagine stress scenarios that would break the block itself.

DR. ALLEN: But that normal fault's been there for a large part of this 13 million years.

DR. MELSON: Well, but you're talking about new--

DR. ALLEN: What's the probability of it happening in the next 10,000 years?

DR. MELSON: Well, because the magma's not just--this is not a passive, in my mind, not a passive phenomena. The rise of the magma and the faulting are part of the same parcel of extensional events. The fault can be sitting there quite quiet and quite dead, but under a new stress regime, would be the opening under which the magma would prefer--or which the break would happen and, therefore, the magma would rise.

I guess I would say that that needs to be looked at in terms of the hazards to the repository. Now, we saw the cores from the repository, as you recall. They are fractured to beat hell, so I'm not sure this scenario of unfractured rock would be what you're going to find in the tunnel.

DR. ALLEN: So you mean you think we should be worried, or at least investigating the possibility that the stress regime is changing right now and will be changing over the next 10,000 years, as compared to what's happened during the past 13 million years?

DR. MELSON: What I have said a number of times is strain in the region ought to be monitored. There should be EDM networks in the region just ongoing, and not only for the repository effect, but just to understand thoroughly what the regime is. I would say that it would be precursors to any renewed rise of magma, possibly. There's a good chance. We don't know. In modern--in volcanos we see erupting, there are deformational precursors. They're quite different than these little bits of volcanos, which I think we're being generous, almost, in calling them volcanos given things like Hawaii and whatnot, but nonetheless--

DR. ALLEN: Do you mean a precursor, like a 5.6 earth-quake?

(Laughter.)

DR. MELSON: No, a precursor like a few millimeters of extension across the rift valley.

DR. HODGES: Putting on my structural geology hat, I would just like to echo those comments. I mean, basically, if we're willing to accept the possibility that Lathrop Wells is active and that there is ongoing volcanic activity, clear-ly, if there is volcanic activity, that's going to have a local effect on a stress regime, and I think it would be inappropriate to make an argument saying, along the lines of, well, nothing has happened in the last X million years or whatever, based on the overall stress regime, when you could have the current stress regime buggered up, if you will, by these relatively recent events.

So I would agree. I don't think it's a passive process, either.

DR. ALLEN: But, of course, the stress regime has also been buggered up during these various eruptions of Lathrop Wells and the adjacent cones.

DR. HODGES: Sure.

DR. ALLEN: During this 13 million year period.

DR. HODGES: But the structural control, I mean, another question is, I mean, if there are structures that are relatively old that appear to be controlling some of the eruptive activity, that does not, of course, necessarily mean that those structures did not move again at the time that the volcanism occurred. So it's not so--I would argue it isn't as simple as saying that, well, those structures were there before and they were just being used as conduits--

DR. ALLEN: Indeed, there is some evidence that some of the faulting has been contemporaneous with some of the eruptions, as I understand it.

DR. CORDING: Looking at the other part of that, is if you have major structures in the vicinity of the repository, and you have, mainly for the hydrologic and transport reasons, offset yourself from those major faults, if there were an event that were--magma event that were to cause something to come up in the vicinity of the mountain, would it tend to go in those major faults?

MR. TRAPP: Just a comment. There was a recent paper in Science where, was it Ellis, I believe, put together--where he was putting together a model where the fault itself was not where some of these things were going through, it was parallel to the thing, and he was showing how he could model this thing as coming through this intact rock parallel in these compressive regimes where you wouldn't expect it to come out; in other words, in the mountains itself and not in the basins. So there are models which have been proposed which explained what you're talking about. A lot has been done.

MR. MARTIN: Your question is if there are structures existing--and there are structures existing, I gave examples of that. They may not necessarily be faults, but even if they are faults, I don't know that we have any answer to the fact that they will or they will not take advantage of these older structures. Sometimes they do and sometimes they don't, but I think the point is, is that we do see it occur-ring. Therefore, it can happen again.

I don't know that anyone can say what are the chances--I mean, it's very hard to really put percentages. It happens 50 per cent of the time. It's not quite that straightforward, but we do see it occurring, and I think getting to the point that John Trapp is making, that seismic studies should be done, I think that's a very appropriate path to take. It doesn't have to be deep seismic, it can be shallow, crustal seismic, but we really don't know what's under the Lathrop Wells cones. We know that they're sitting on an alluvial basin, but what's the bedrock beneath that, and can we see if there are maybe dikes, basaltic dikes that are feeding those cones, or can we see some heterogeneities in that shallow crust? That gives us some idea that there may or may not be, and again, you're right, it depends on expert opinion in many cases, but to a certain extent, I think we have to rely on that.

DR. SMITH: I just wanted to make the observation that in areas like the Fortification Hill field where we do see to depths of 300 meters or 400 meters beneath the surface, it's quite common for dikes not to occupy faults, but to come up adjacent to faults or to come up in the foot walls of faults, and the volcano then erupts on the foot wall and the flows then cascade over the fault itself. So in the upper several hundred meters it's quite unusual, at least from our experience or from my experience, to find a dike or a vent coming up directly on a fault itself. It's either in the foot wall or the hanging wall, and more commonly in the foot wall.

So for some reason, I'm not sure why, these conduits leave the fault in the upper part of the crust and normally come up either in the foot wall or the hanging wall. So just because a block may be relatively coherent and not contain structures, or at least mappable structures apparently, it's still possible for them to--or a dike to enter this relatively structurally coherent block.

Also, the comment that Greg made, that many of the dikes themselves propagate fractures, I think, is very important.

DR. REITER: Kip, I'd like to ask a question. You were part of the ESSE Peer Review Panel, and that peer review panel issued a consensus, the earth scientists issued a consensus statement, and part of it was that you felt that many of the phenomena would defy quantitative resolution, and at the end you would have to rely upon expert judgment and "peer review" to--I don't know if I--I think I quoted correctly, but could you tell me to what extent you feel the volcanism issue will fall in that?

DR. HODGES: Well, gee, that's a loaded question, Leon. My role in that was basically to make, as you know, to make comments and I didn't have to receive agreement from--certainly from the people involved from this "consensus report."

My only concern is that, to put it simply, is that I don't believe myself that geology is a predictive science, and the big problem to me is that it's very difficult to ask geologists to do something that they're not accustomed to do, and that's predict the future. So I think that there's always going to be some level of uncertainty, and certainly levels of disagreement that I don't think, frankly, have even shown up yet because I don't think when this process gets much, much closer to the point that we can really define some of these parameters a lot better than we can now, and we can really form some kind of convergence on the probability, the probabilities that people like Bruce and Professor Ho calculate, then we're going to be--or estimate--then we're going to be in a situation where it's going to come down to value judgments by a lot of people, and the people that are going to be involved, I think that probably the people at the NRC, for example, would be quite surprised at the lack of consistency in the interpretations of those data regardless of what the probabilistic answers are.

And that's--I guess that's my only fear. At this point, I think it's premature to, of course, throw up our hands and wander away. I mean, something must be done, but on the other hand, I guess I'm not so hopeful, personally, for the future. I'd like to be proven wrong, but I don't think I'm so hopeful.

DR. MELSON: I think that I would tend to disagree. I mean, perhaps it's difficult to predict, but when you're in the area of what's called geology and geophysics and volcano prediction, of earthquake prediction, you're at--or of a horror prediction or a flood prediction, many people, that's all they do, so there is a discipline, as best they can perform, of predictive geology, and--

DR. ALLEN: Let's leave earthquake prediction out of that group.

(Laughter.)

DR. MELSON: And there have been great successes and great failures, but the successes have been such, certainly in volcanology, on cases, that literally tremendous disaster has been averted by being right, and a lot of times where the predictive process hasn't worked, it's been because of a time factor. Like in Nevada del Ruiz, it was clear that there was--the village was going to be buried, but somehow the mechanism of getting the message out didn't get there.

So I think what we're up to isn't quite that bleak, and what I see happening is should, in fact, this end in this being an improved repository based on this kind of a process, I feel very comfortable, based on what I've seen in predictive geology, that it'll be better. It'll be safer, and it's a worthy process, and it's difficult. This is far more difficult than most predictive geology I've ever been associated with.

DR. HODGES: My only response to that is that I agree with that, with most of that. My biggest problem, I think, is that in most cases in which this kind of prediction is done has been under circumstances where there is a volcano there, and now we're talking about a situation where there is no volcano there, and this is a whole new ball game. So I would argue that what we think we know about our predictive capabilities under those circumstances may not be a guide for this problem.

DR. ALLEN: It's past six o'clock. It's been a long day. Let me just ask if there are any final statements anybody up here would like to make, or any final statements or blasts anybody out there would like to make before we close? We'd like to hear from anyone who has particular opinions.

DR. DePAOLO: I should forego the opportunity, no doubt, but I just wanted to comment that the point that you brought up, I think, is an interesting one; that you look at a place like Yucca Mountain. Nothing's happened in a long time and you expect that nothing probably would happen and it seems reasonable, but the way I see it is that the idea of a 10^{-8} per year probability is not within the human intuition range. That's about the probability that one individual person will win the California lottery in the next year, and you can be pretty sure that you won't if you buy a ticket every week.

So this is, I mean, the problem is totally analytical and it has nothing to do with intuition.

DR. ALLEN: Any other final comments? Yes, Ardyth Simmons.

MS. SIMMONS: I would just like to say that with regards to the comments on the integration of the program, we have a new effort that has been undertaken in the last several months or so, recognizing the comments that the NRC, particularly, but also other groups have levied against DOE and its lack of integration in the geophysics program, and a group of knowledgeable experts has been convened from across our program to examine all of the geophysical data that exists, taking into consideration the information that was presented in the White Paper and subsequent to that, and then to look at what a feasible program would be to resolve some of the uncertainties, and keeping in mind the cost benefit associat-ed with doing additional studies.

And certainly, some of the things that we will take into consideration are memoranda like the one that you men-tioned written by Evans, so even though you haven't seen any results of that yet, you'll probably be hearing about it in the next few months or so.

DR. ALLEN: Thank you.

In conclusion, let me just thank everyone for participating, particularly those at the front table, but everybody out there. We look forward to seeing you tomorrow --most of you tomorrow, at any rate.

I might say that in terms of some of these ques-tions, for example, on which issues is a consensus develop-ing, you know, it's hard to point out right now a great convergence on ideas, although I agree with Bruce that there is some movement towards convergence on some of these, but nevertheless, what's impressed me is that we've heard a lot of very good science, and also I haven't heard much rhetoric. I've heard a lot of science, and I'm impressed with that, and as long as good science is being done, I think we're on the way to resolving these issues; for example, whether these things are soil layers or not, clearly, if good science is being done, we're going to resolve that issue.

And so, all in all, I come out of this somewhat more optimistic than I initially felt yesterday morning, and so I think it's been a very useful interchange, and certainly, I've learned a lot, and I thank you all for participating, and that's it for today.

Thank you.

(Whereupon, at 6:10 p.m., the meeting was concluded.)