UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

MEETING ON VOLCANISM

September 14, 1992

Alexis Park Hotel
375 East Harmon
Las Vegas, Nevada 89109
(702) 796-3300

BOARD MEMBERS PRESENT

Dr. Clarence R. Allen, Chairman,
Structural Geology & Geoengineering
Dr. Dr. Edward J. Cording, Member ,
Nuclear Waste Technical Review Board
Dr. John J. McKetta, Member,
Nuclear Waste Technical Review Board

Also Present
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

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P R O C E E D I N G S

1:00 p.m.

DR. CLARENCE ALLEN: Let me welcome you here on behalf of the Nuclear Waste Technical Review Board. This is a meeting of the Board's Panel on Structural Geology and Geoengineering on the subject of volcanism at the candidate, Yucca Mountain repository site.

I am Clarence Allen, chairman of that panel. Let me introduce to you the other Board members and Board participants who are here.

The other Board members are Edward Cording, John McKetta. We have consultants and advisers, Bill Melson from Smithsonian Institution, Kip Hodges from MIT or the Eastern Branch of Cal Tech will be here one of these minutes, and Senior Staff members, Leon Reiter and Bob Luce. In the back, Linda Hyatt and Donna Stewart the staff that are there to help you with administrative details.

This is the second formal meeting of our Board on the subject of volcanism. Many of you, perhaps most of you were present at the meeting on March 1st, last year, and were anxious to see what kind of progress has been made since that time.

It was clear at that time there had been some convergence of viewpoints on some of the scientific issues of volcanism. There were also still some major differences and it is going to be interesting to see whether we have had further convergence or further divergence on some of these particular issues.

There were differences of scientific opinion last year on some of these issues; it is clear that there are still some difference of scientific opinion.

Tomorrow afternoon we will have a round table discussion and it is on the agenda. But, I would just like to remind you what the four questions for the subject of that round table are because I hope you will keep these in mind during the presentations, during this afternoon and tomorrow, and keep in mind questions or responses that might be apropos regarding these questions at that time.

The first question is: On which issues is a consensus developing, if any?

The second: On which issues are there serious remaining differences?

Three and perhaps the vitally important question to the Board: Are these issues important with respect to site suitability and public health and safety? Which, after all, is the primary focus of our concerns.

And fourthly: How can these issues, the remaining issues be resolved?

So, I hope the speakers will keep those questions in mind, and I hope that you were invited to participate, of course, in the meeting, and the audience will also keep these questions in mind in terms of possible questions you might wish to pose.

Let me remind you that this meeting is being recorded and that any people who wish to make a comment or a statement or have a question must approach the microphone and must announce their names to make it legible for the record also. Particularly for those of you at the front table, please speak close to the microphone so that your voice can be amplified and it can be recorded.

From the agenda I see that we have three introductory statements to be made; one by the DOE; one by the State of Nevada; one by the Nuclear Regulatory Commission. So, may I ask that, I guess Jeanne Cooper is going to make the statement for the DOE.

Jeanne Cooper.

DR. JEANNE COOPER: Thank you.

As Clarence said, I am Jeanne Cooper. Some of you probably don't know me. I have been on the Yucca Mountain Project now about a little over a year, I guess. I work managing the Volcanism Task for Yucca Mountain. And after this meeting, I will be taking over Ardyth Simmons' job of managing the interactions we have with the Technical Review Board.

I'd like to make just a few brief introductory remarks. I think we have a lot of interesting data to present today. I would like to get on with that data and we are pretty excited about this meeting.

Just as an introduction, I would like to remind everybody that the primary focus of the volcanism studies is the valuation of the potentially adverse condition of igneous activity. And, as mandated by law, DOE must look for evidence that would disqualify the site early in the siting process.

We believe that the only future igneous event that could potentially be capable by itself of disqualifying the site is an extrusive event though the repository. So in other words a volcanic eruption through the repository.

We also believe strongly that a probabilistic approach to risk assessment is the appropriate way to approach this problem. I think we have consensus on this from most of the groups involved in this issue. And, because we think that an eruption through the repository is the only condition that could disqualify the site, as again I say by itself, we have necessarily focused initially on this issue of disqualification due to an eruptive release.

We are currently involved in an effort to resolve some of these issues related to eruptive releases so that we can move on and expand our studies to the effects of igneous activity on the entire waste isolation system. So, not only eruptive events, but also intrusive type events.

We are excited, as I said, about some of the data that we will be presenting today. The volcanism studies are progressing and we are very happy with them, and we are satisfied with them also. Progress was slowed somewhat in fiscal year '92 because of a limited budget. But, we are still following our strategy that was laid out in the site characterization program baseline and also in the three study plans.

I would like to emphasis finally that this meeting is a progress report. It is a progress report of accomplishments that we have had since March of '91, which is the last time the Board heard about the topic of volcanism. In the next two and a half days, you will be seeing a snapshot of some of the current activities and current studies that are going on.

I don't want to dwell on the agenda. You all have copies of the full agenda. This is just a synopsis of some of the presentations by the Yucca Mountain Project people that you will hear over the next day and a half. And, this afternoon, you will be hearing Bruce Crowe and overview of the task. And then an independent view of the chronology activities from Don DePaolo. This will be followed by some information on paleomagnetism by John Geissman and then cosmogenic helium dating by Jane Poths.

Finally, today we will hear from Les McFadden and Steve Wells on soils and geomorphic studies both at the Lathrop Wells center and also some information from the Cima Volcanic Field. And, we will conclude today by hearing from Frank Perry some really interesting results from his recent data on petrologic studies.

Tomorrow, we will hear from Greg Valentine on some of the efforts to expand our studies into the physical processes of magnetisms and effects of these on the repository. And finally, from Bruce Crowe both on probability studies and then on a summary of our progress and future areas of study.

And, of course, interspersed tomorrow between some of these talks will be information from the State of Nevada, and also from the Nuclear Regulatory Commission and other independent scientists.

So, with that I would like to conclude and get on with hearing some of the interesting results.

Thank you.

DR. ALLEN: Next we have an introductory statement from the State of Nevada by Carl Johnson.

MR. JOHNSON: For those who don't know me, my name is Carl Johnson. I am the Administrator of Technical Programs for the Nevada Agency for Nuclear Projects.

The Agency for Nuclear Projects is the state agency responsible for the state's oversight of the DOE High Level Waste Program.

Over the next couple of days you are going hear presentations by some of the researchers that we have employed looking at the volcanism issue. Dr. Eugene Smith from UNLV and Dr. Chih-Hsiang Ho also from UNLV. You'll also hear a short presentation by one of Dr. Smith's research assistants, Mark Martin.

An important point, I think, that was brought out by Jeanne's just concluded opening remarks that I want to make clear to everybody, and that is that the burden of proof in this particular issue in defining whether there is a volcanic hazard at Yucca Mountain or not lies with the DOE. It does not lie with the State of Nevada; it does not lie with the NRC; and, it does not lie with other independent researchers, specifically the USGS.

I would take exception with her remark and that is what prompted my remarks, is that I don't think there is consensus that there should be a probabilistic approach to dealing with a volcanic hazard problem. As we understand the requirements of the NRC is that the approach will be a deterministic approach. Now the DOE is also allowed to consider a probabilistic approach, but the main emphasis should be a deterministic approach. And, we in the State in Nevada have a similar viewpoint. So, I don't want everybody in the audience to feel that the probabilistic approach that is being promoted here by the DOE is the "consensus" view of everybody here as the reasonably approach to resolving this particular issue.

With that in mind, let's start the discussion.

DR. ALLEN: Thanks, Carl.

The final statements will be by Keith McConnell of the Nuclear Regulatory Commission.

MR. KEITH MCCONNELL: Thanks, Clarence.

For those of you who don't know me, I am Keith McConnell. I am a section leader in the Division of High Level Waste Management for Geology and Geophysics.

We appreciate the opportunity to attend and participate in the Board's meeting on volcanism. We have found both meetings like this that the Technical Review Board has plus the working group sessions that the Advisory Committee on Nuclear Waste has, good forums for issues like this to be discussed and perhaps some common ground found in appropriate approaches to address some of the issues.

The only other thing I would like to mention or at least bring up at this time is that one of our concerns over the years has been the integration of the geophysical program with studies like this on volcanism and also in the area of faulting. In looking at the agenda, it doesn't appear that geophysical studies, outside of perhaps paleomagnetism will be addressed and perhaps it is outside the scope of this meeting, Clarence.

But, it is a longstanding concern on how geophysical activities are going to integrate into the topic of volcanism and deciding whether volcanism is a serious concern or not. And, we would appreciate it if in the future, either in this meeting or out in the field, we could perhaps bring that up as a topic.

That's all I have.

DR. COOPER: The first speaker this afternoon will be Dr. Bruce Crowe. He will be presenting an overview of the task status.

DR. CROWE: I want to introduce myself, because I feel like if I do, there might be another person in the audience who will decide to review my program and we have had plenty of people doing that.

Let me just give the Board an overview by saying that we had all of our studies fine tuned to present in October and you moved it up a month. It probably impacted Mike Morrell the most because he kept spectrometer time and he doesn't have any data to show you. But nonetheless, we will proceed.

The main focus as we do this next two and a half days is to provide you an overview of the progress. And I want to emphasize that this is an overview of progress. We are not presenting a program defense, we are presenting what we have done since the March, '91 meeting that you last attended and emphasizing that. We are assuming some level of past information on the part of the IAC or study plans or a variety of documents.

While we will be emphasizing Lathrop Wells, since that has been one of the major points of contention in our studies, I also want to point out up front that Lathrop Wells is not the whole program, that there is a lot of other things we are doing besides Lathrop Wells and we will be trying to talk about some of those.

I think perhaps the bottom line that I would like to make right up front is that we feel we have been making progress, particularly in the last six months in the geochronology field and petrologic studies are beginning to come together. And we are quite anxious to present that information, in fact we are quite pleased with the progress we have made.

At the same time we have also moved, particularly in the last year, into other ares, looking at the other Quaternary Centers in the region, particularly trying to do more work on synthesizing structural models at the Yucca Mountain setting and how they tie into a probabilistic approach to the problem, and then also following the Board recommendations they made at the last meeting, we began to add more emphasis on the effects of volcanism.

And up front, I want to repeat that we view the goals of the Volcanism Program. They are very similar to what Clarence pointed out in his summary comments.

Basically, our goals for this project is to try to define the risk of volcanism for the potential Yucca Mountain site. As part of doing that we are required by the program to gather data sets which support the basis for those conclusions under risk assessment, i.e., that they need quality assurance requirements of the program. That is an extra challenge of not only do we have to produce the scientific data, but we have to make sure it is fully pedigreed under the quality assurance requirements and will meet the standards that we required to proceed to the licensing. And then third, we are required to present our conclusions to the NRC so that they can judge the suitability of our conclusions.

As part of that, on those goals, we want to emphasize that we are not that concerned over which methods we use in the approach to the problem. Our interest in solving the problem and looking at a variety of methods. In fact, the best way that we think to solve these structural problems we are up against is to use multiple methods. Because if we can get convergence of data where we have looked at information from a variety of perspectives, we think that that has ultimate confidence to succeed and address problems with research goals.

Now, as part of our mission here, we feel that it is our mandate to be reviewed by everybody. Let me just show you our survival model that we have evolved. You won't find this in your package, but it is something that we use to try and keep our sanity.

What I would like to talk about then is let me give you an overview. What I would like to present is our goals for this meeting. Since March '91, I am probably not surprising anybody if I say there were elements of the meeting that we found quite pretentious and perhaps unnecessarily pretentious. And so when we left that meeting, we felt that it was really important to try to bring a perspective of the problem and see if we could minimize, or at least bring into bounds our unreasonableness. And, I'll list the most important that was utmost in our mind was the perspective of chronology of Lathrop Wells site.

Along with that, you probably heard a lot of frustration that our inability to put trenching in the site, so our goal was, can we get trenching started? Can we begin to test for stratigraphic models and data to constrain our speculation to see what are the differences that exist, are really two differences and are simply bound by the lack of information that is clear. And also we felt it was really important to try to separate the data where we have assumptions that require to generate the conclusion versus what data are truly definitive.

The second goal was we wanted to look at what we could do to try to achieve a consensus or try to gather more agreement on some of the different options that we have for looking at these problems. Our view after the meeting was that our study plan was reasonable, of applying multiple geochronology methods and that the best way to proceed was to gather more data to try to achieve resolution through data gathering. Rather than argue about it, let's just gather data and try to see if the data itself will help us get through some of these problems.

The key issues we felt were identified as what data might help us to resolve issues. Specifically why differences exist. NOt necessarily to focus on what the differences are, but try to get the fundamental foundation of how there are data differences, what differences there are and how interpreting data can lead to differences of opinion. And finally, keep it all within the perspective of what does it mean to volcanic risk for Yucca Mountain. Then our third goal was really not to neglect the fact that Lathrop Wells is not the volcanism program.

So, what I will be reporting to you now, is what we are trying to get on with in trying to establish our goals.

The first thing I did after the frustration of the last meeting was I decided it was really important to try to bring in an external reviewer to look at our geochronology program and provide an independent view of what we are doing in this controversial area. We feel very pleased that we were able to entice Don DePaolo into the program. And he conducted a field and literature review and interviewed just about every participant involved across the program, both Yucca Mountain participants and other participants and we circulated a summary report to you, I believe last spring, sometime last spring which we made available to the Board.

He also is providing a series of interim reports.

We are pleased to see that Don is continuing his role as a geochronology adviser. I want to emphasize two points. One, he is not personally involved in our work. We have asked him to give an independent perspective applying his expertise in geochemistry and geochronology and provide his advice both where he agrees with us and where he disagrees with us and try to keep that advice independent. Basically, we are looking at him as a neutral party to review it.

We also instituted a series of organizational changes. As the program is divided into two study plans it is also divided into three PI's. Frank Perry is running the Characterization of Volcanic Features, and Greg Valentine is focusing on the Magmatic PRocesses/Effects.

The primary reason we have done this is we really wanted to expand our perspective. I particularly have been at this program for a long time and I think it doesn't hurt to bring in fresh insights and different opinions. And I also was fighting to spread the work and to try to preserve my sanity as well as some of the sanity of other people involved.

As part of this we also did this organizational change to try to bring more of a focus on effects on the repository, magmatic effects. Again, one of the recommendations that we very much agree with that the Board made in their last report to the President. So, what you will be seeing on this from Greg's talk today is some of the emphasis that we put on three topics: the potential eruptive effects; the potential subsurface effects; and, a new topic that has grown out of Don DePaolo's recommendation to look at the whole possibility of magma dynamics and to see if that can help us to understand both the project in terms of both geochronology program and magmatic processes that produce the past record and might produce future volcanism in the region. Again, Greg will be talking about this extensively.

I just want to show you how this looks. This is our present organizational structure of how we are set up to run the volcanism program. One thing I want to emphasize first is that Jeanne Cooper is the task manager for the DOE and then we set up Don so that he reports both through the PI and also has independent reporting to the DOE so he maintains his sense of independence.

I won't go over the details of what sits under everyone. I assume that this is in your package and you have looked at it and this is in the study plans.

So, we have done as far as field studies go to try and implement our goals, we are happy to tell you that we have initiated our trenching program. Before I tell you a little bit about it I want to tell you some of the work we had to go through to do this and particularly acknowledge a lot of the work done and the long hours getting it ready to be able to trench.

Basically, about last June the issuance of a needed permit by the state allowed trenching to start, and we implemented a very quick and rigorous program to get through all the procedures we needed to go out in the field. Basically, we used the safety procedures of Los Alamos National Laboratory and all the environmental procedures of the DOE. And we also had to develop operating procedures to be approved by both DOE and our own organization, and I don't want to underscore how important that work is.

Rich Morley, in particular, put in a lot of hours doing this, and we're pleased to say the volcanism program was part of the first surface disturbing activity that occurred in July of' 91. Since then we have completed about 35 trenches primarily to look at stratigraphy soils work, geomorphic work and trying to do some petrology. We will be showing you some of those trenches in the field on Wednesday.

We have one large trench that we just finished last Thursday, where we brought in a big DA Caterpillar and have constructed a very large trench in the quarry where we think there are very critical stratigraphic relationships.

We modified our stratigraphy, remodified stratigraphy and we continue to modify our stratigraphy. We are finding the more we do this the more we are learning. We are not there yet, but we think we are making progress.

Also, we started trenching at Cima. We put in 9 trenches in Cima and in a recent coup we think that we have gotten full permission of one of the quarry owners to start work, just this month, in the quarry activities. So, we feel like a lot of the hurdles that were in front of us when we last talked to you have been passed and we are on stride and proceeding with the program. So, our bottom line is, we're not done. We still have problems, but we are quite pleased with the progress we've made.

At the same time we have started field mapping at Crater Flat and Frank has been extending his petrology studies to Crater Flat and the Sleeping Butte site.

This is a map of Lathrop Wells where I show our trenching sites. In some places we have very detailed trenching so we don't show all the trenches. But this shows you basically where we have done our trenching. We concentrated on the north and west side working around and coming to the quarry.

We have good exposure around the fringe out here, so it's not going to take as much trenching, and our next tests will go in to the quarry. Now we have structured a very detailed, a very large trench, what we call a buried flow right in this part. We can't go into the trench because of safety concerns, but we will talk about the reason we put the trench here.

Basically, just to show you our rig, this is what we have. We have a truck mounted back hoe that sits in the back of a one ton, four wheel drive truck and allows us to dig holes. In fact this is exactly where we were trenching into the buried flow on the north side.

You can see down to about eight or ten feet and you can also see sideways. It's not fully versatile, but it turned out to be a really valuable tool for implementing our geochronology program.

Okay, what we have done in trying to work through and gain more consensus or at least trying to understand the range of multiple views, we continue to use the multiple geochronology methods and in this meeting more than we did last time, we'll be hearing from individual investigators that have done the work. And we'd like them to actually speak to you on what their methods are and what their progress has been.

We feel that using multiple methods is our best shot in trying to reach consensus. We have finally been able to dig holes in some of the soils. And then a big addition has been John Geissman. He has joined the program and has some new paleomagnetic data. John's work and some of the reviews of Don DePaolo, I have to say that certainly in my mind I feel a lot better understanding what the paleomagnetic differences have been in perhaps some of the uncertainties that have revolved around paleomagnetic data.

But I think we are much closer to resolution in my mind. I am not sure we will have an agreement over this, but at least I feel a lot better than I did a year and a half ago at understanding why there are differences over paleomagnetic identifications.

What I think is interest is that I think we are seeing first signs of convergence. We think that some of the data coming in at around the 65,000 to 70,000 range for much of the main volatile sequences, and this is something that you'll be hearing from each of the individual speakers.

The other thing that has come out of this is we found that by looking at the program in detail and also from some of Don's comments, that it has become really important that when you look at data from each data set that you look at data from three perspectives. First, what data are truly definitive in giving you some firm constraints. Second, what are data that require assumptions to lead to conclusions. And, third, what are data that are really based on speculations. They may be valid speculations, but they are based on a foundational surface assumptions that may or may not be true.

And when we clarify that very rigorously and ask each investigator to do that, we think we have clarified a lot of the problems that have been there a year and a half.

Another thing we have put a lot of effort into was after the meeting we decided it was really important to get as much of our information out in the literature that we presented to you in the March of '91 meeting. And so we made a major effort that really peaked in about December of '91 to get everything down on paper for the symposium. And we produced four papers and much of what you will be hearing today is a summary of those papers and the additions of the new data we gathered since December of '91. But with that combination of reviews and examinations, we feel we have a lot more confidence. We still have problems. We are not there, but we have problems that will receive resolution from steady progress.

By a state of resolution, it doesn't mean that everyone is going to be satisfied with the kind of numbers we came up with, but we think we can bound the problem to allow what is required from a regulatory perspective. We do still have some problem areas, QA software has been a giant burden, primarily at Los Alamos.

Probably the person most impacted by this has been Frank Perry. We still do not have resolution to that. We have not been able to get any major element data for any of his rocks. We've only been able to get trace element isotopic data and we are still wrestling with that.

Los Alamos was visited by a TIGER team during the review period, so you can imagine we had some environmental issues that came up. We shut Mike Morrell down for a period of months as he generated waste that we weren't sure we could generate for awhile.

I have to underscore that we have faced a large technical challenge. A lot of the work we are doing is state-of-the-art and involves a lot of method development and we have to constantly examine what those method developments are, what the assumptions are, so we still are working on that.

Meanwhile, we'll talk about this in the other sections and again we will be hearing lots more on the effects of volcanism. We have reviewed structural models and then one of the things that we've made some progress on is trying to get an overview perspective on the teleseismic tomography results that Evans and Smith talk about. Some of the recent work will contain a seismic gap some people have felt could be a magmatic gap.

Further, what we have published in our papers was that we put out the first probability tables where we've done E-1, E-2 calculations that we talked about in the last meeting. We have identified a couple areas where we think we can focus on where there are differences of opinion that we might not be able to carry to resolution. In fact we would like to hear some of the Board recommendations or some of their thoughts about these areas. And in many areas where we have differences, some of them I think can be resolved and some may not, for example the device of what recurrence models you use for future events.

How do you define what a volcanic event is? This whole general issue of what constitutes conservatism in calculations has been a longstanding problem. In fact, one of the reasons why you see probability values that differ somewhat, in large part is focused on what each individual person's definition of conservatism is. I'll tell you tomorrow what we think should be done, and we would like to hear from the members of the Board and ask them what their thoughts are.

And finally we have another area of what I call model bias. It brings in some of the suggestions the Board did give us in this area, we would call ugly words from the reaction by the NRC when we bring up these words, and that involves expert opinion. We are desperately waiting to hear what you have to say. I'll give you some ideas that I have on how you might implement it, but I would really like to hear what you have to say about it.

Okay, finally one of our major activities that we have done in the last year and a half is we have begun the process of developing an issue resolution report and interacting with the NRC. In fact, we have a rough draft of the report finished, and we hope to have, probably late this month or next month and we would like to submit a copy of that to the Board as part of our review process before we formerly submit it to DOE.

Basically, what we've started is a series of changes in the NRC that express the opinion that the concentration of our work here...and we feel we can make a very strong case and would like to go ahead and have NRC take a look at our case, of the issue of direct effects involvement can be demonstrated, we think, from our data sets, and we'd like to try and see if we can convince them, whether our data stands up to their scrutiny. And, if so, our recommendation would be again to move on more in looking at the effects of volcanism.

Here is a photograph that I have some black and white copies of for the Board. One of the things we looked at, one of the areas of contention has been what did the cone look like before the quarry. And we actually had a person in the program who spent a lot of time looking through historic photographs in newspapers, and after about a year of no progress, and finally I got a phone call about three months ago and they located this photo.

I'm not an old car expert, but I've been told this is probably early 1930, mid 1930 vehicles. And, lo and behold, here is the south part of the Lathrop Wells cone. We now have a snapshot of what it looked like and we also have some early '50s area photographs we have had blown up.

Another area that we think is significant outside of our program was in the letter sent to you by Brent Turrin and Duane Champion and their new K-Argon results, which we have not seen. We've heard of them via the rumor mill, but we have not actually seen it on paper. And, that is the two new Pliocene sites of volcanism that have been recognized in the region. We have looked at these in the field and we are basically in agreement that these are probably Pliocene centers. Two are up in an area we call Thirsty Mesa which is this big mesa sequence of lava flows that USGS has some dates about 4.5 million years. We've gone out and examined them and we completely agree with those dates.

We've submitted them to our quality assurance process and we expect some dates back on the results some time in early October. I see no reason not to completely agree with an age of about 4.5 million years.

Equally, a site we've been trying to drill since 1986 was drilled by a private company looking for petroleum which I find surprising. But, nonetheless, that's what they were looking for. We got samples that were submitted, Brent Turrin has also got a date, about four million years. We also agree with that date. We think it is reasonable. And basically what it does is add two new points that we think will help define further the Crater Flats volcanic site. I'll be talking more about this tomorrow.

The only point, a minor point of disagreement is...there was a note made of a very extensive committee meeting on the topic of volcanism. And what we see from the magnetic data in a new report just issued from Langenhelm by the USGS Open File Report, this is basically an aeromag anomaly. What you see, this is looking in to the southern part of the Amargosa Valley; this is the southern part of Yucca Mountain. You can just see the edge of Lathrop Wells. And what it shows you is basically the area surrounding the magnetic anomaly.

We think that this is not an extensive sheet, or extensive area, and basically represents an isolated center. In fact, it has a somewhat circular area here with lava flows off to the south. It has a dipole effect, probably because...our thoughts are that you had enough sedimentation coming in along down the axis of Forty Mile Wash that you have a Pliocene center.

Okay, then quickly I want to talk a little bit about the field trip. We now expect we'll be exceeding sixty plus, and needless to say that's a little big bigger than I was anticipating. We were hoping, perhaps, that we could spend a lot of time in the outcrop like we did when we helicoptered in, but with sixty people that might be a little difficult. So, I just wanted to say that the logistics are going to be a little more difficult than we thought. We will be on four-by-four roads that are very sandy, and you stand a really good chance of getting yourself stuck, so we'll have to ask everybody to cooperate and follow our directions on the site very carefully.

The second thing is that this is private property we'll be on. We have a very good relationship with the quarry owner, and we'd like to keep it that way. Particularly we'd like to not have anybody run over the loader and not have the loader run over them. They are quarrying actively, and we don't want to interfere, and we also want to be very safety conscious when we've out there. So, when we drive through, we want to make sure everybody stays together so we don't have to worry about safety.

Also, we would like to try to ask you, at least at one point, to look in detail at the quarry exposures, because I think we've got a lot of new data. So when we go into the quarry spend maybe twenty minutes examining the different containments and outcrops, so we can focus differences right on the rocks.

Okay, and the last thing, there are some unstable cliffs we'll be around and we have a big trench right there and we're going to ask that nobody enter the trench because we're not exactly sure what the safety hazards are. We can stand on the edge of the trench and look in, and we'll have people who are certified to go into the trenches and describe them.

Our goals this field trip are to show you the progress in trenching, to show you the stratigraphy as we now see it, show you what we know, show you what we don't know, show you what some of our thoughts are. Examine the tephra units, and particularly, we now have exposed soil. You probably remember Les's plea for letting him look at the rocks in the field. He now has a hole in the ground, and we want to show you what we found in the soil.

So, quickly, let me just show you where we'll be going. This is the famous quarry site there's been so much written about, probably more than any outcrop. It looks a lot different than this now. There's a huge soil pile, and we're almost losing this quarry exposure, so it's quite critical that we get in to look at this on this field trip. We're worried that it might go away in the next six months or so. This is on the north side. I'm sorry, this is on the south side.

We'll also be going to the north side, and there is a very important buried surface here that if you look at it, this is the main part of the north cone, and as Steve Wells has pointed out for many years, there is no sign of erosional rilling in that cone, and yet it covers an erosion surface here, and terraced these surfaces up into the cone, and it's covered with non-welded material.

We now have three or four trenches into that area. We'll open up two of them for the field trip. We'll want to walk across and show you what this unit is, and show you what we think it represents. We think that this erosional surface can be traced northeast to what we call a buried lava sequence where we've dug a really deep trench.

Where we'll be going is walking down across here. We have a really large trench, a couple hundred feet long, over thirty feet deep in some places. It runs from the buried lava flow right here. Basically, where we have seen our most important stratigraphic problem, we felt that in order to resolve the issue we had to dig a large trench. We haven't logged it yet, of course. We just finished Thursday, but we would like you to see what we're doing and how we're going about trying to solve this problem.

The very last viewgraph I'm going to show you and we can talk about is that we have a strategy for trying to proceed with resolution. I wanted to show you, but this is so busy, so if you have insomnia tonight it will probably help you get to sleep. But, basically we do have an overview strategy of how we proceed through both direct effects of volcanism, what we call the eruptive effects, and subsurface effects where we might have effects without volcanism erupting the surface, although the effects might be increased by subsurface geometry.

I think I will stop there. Are there any questions.

DR. ALLEN; Are there questions from the Board?

DR. REITER: Bruce, I have a question for you. Could you please clarify your view of the direct effects of volcanism. Did I understand you to say that you thought that was the primary concern of volcanism?

DR. CROWE: When we say direct or eruptive effects what we are talking about is an event probably fed by a linear dike system or perhaps one dike or multiple dikes that penetrate directly through the repository and erupts through the surface. The gravity force for potential release are the volcanic rocks themselves. And, basically, compared to a ten thousand year time period it's an instantaneous process.

The distinction we made with subsurface effects is, if you have some sort of a subsurface geometry, you could perturb the waste isolation system and the area of disturbance would be larger so that you have to redo both the E-1 and E-2 calculations and make them correct.

Now, what we feel is that the eruptive effects could, on itself, disqualify the repository. Now, what we would argue, and we think we have enough data to argue. We're pretty close to saying that that doesn't occur. What we want to get on to is the more difficult problem of how does it perturb the waste isolation system. We still have two steps there that we're facing.

Actually, I didn't think we had agreed on what we would call a volcanic event until after our conference with the NRC a few weeks ago. Gene Smith and I talked, and I think we're in agreement, and I'm actually enthused to say that at least Gene and I agree. I don't know how many others agree, but that's a step forward.

DR. REITER: I thought that Jeanne Cooper had made a statement that she thought that the main problem is a direct intrusion, and that the other thing is not only secondary, but of secondary importance also.

DR. CROWE: No, I don't think she meant to say it that way. What she is saying is that eruptive effects could, on itself, disqualify the repository. Whereas a secondary, we're not sure it would disqualify, but I think, more importantly, it would become a component of the CCDF for the whole site. So, in that effect, it has to be coupled with the system. But direct effects could potentially disqualify the site.

DR. ALLEN: You say indirect effects. What do you mean?

DR. CROWE: Okay. This is what Greg's whole talk will be about. There are problems like thermal effects, we have circulation effects, the release of volcanic gases, increase in corrosion rates of the canisters, and Greg will be showing diagrams of all this.

DR. MELSON: Keith McConnell raised a point, and it's an interesting one about bringing more geophysics into play. Would you speak to that?

DR. CROWE: This is an issue we've had many discussions with the NRC. I agree with Keith in principle, but one of their concerns is how thoroughly the geophysics department is integrated across the spectrum of topics. Volcanism being one, tectonics being another. A whole range of topics.

In our study plan, when we wrote the probability study, what we proposed doing was bringing in separate consultants to overview all the geophysics work that's been done to make sure that all of the key data that might be important to volcanism has been examined, and make recommendations to others who are more focused on volcanism.

We feel that we are following an orderly process. The NRC has made the point that they think integration is so important that they would like to see more, or see it in a more timely way. But the problem basically comes down to...there's a lot of data out there, but it's been looked at largely in overview, where it's not been focused on a problem solving perspective. Is that a fair statement, Keith?

MR. MCCONNELL: I guess I'm more or less in the dark about the extent and nature. I know we've had some discussions and the intentions are there. They are fairly far along in their investigations, and we are talking about issue resolution.

DR. ALLEN: Any questions? Carl Johnson.

DR. JOHNSON: It's more of a comment than anything else about the question that Bill Melson just asked. We are concerned about the integration of the geophysics program, and I think it's from two perspectives. One, we don't have good confidence right now that DOE program has identified all the volcanic centers in that area. This could g a long ways in helping to accomplish that. Secondly, we've had a lot of concern about the relationship of volcanism to geologic structure. Geophysics can go a long ways to help to resolve that also.

DR. CROWE: Let me just respond to that real quickly. We feel that we have done a lot of identifying of volcanic centers, and have a very extensive aeromagnetic data base that has proven to be very valuable. We've drilled three or four mounds already and we feel we have had a lot of success.

DR. ALLEN: Any other comments or questions? Thank you, Bruce.

Jeanne, will you introduce the next speaker?

DR. COOPER: Let me introduce Dr. Don DePaolo. He has had a long and distinguished career in the field of volcanic geology. He's currently the Chair of the Department of Geology and Geophysics at U.C. Berkeley.

DR. DEPAOLO: Please let me apologize. I have come down with a cold. I'm running on fewer cylinders than normal.

What I'm going to try to do is give you basically my perspective on what has happened in the geochronology aspect of the volcanism program.

My view of what is happening in this program, and to some degree this will be an iterative process, and there may be some errors in what I say, and misunderstanding, but this is the way, based on my background, that I see it happening.

I've sort of broken the program down into an organization that doesn't exactly fit the way Bruce has described it, but it's the way I think of it. And we are essentially at the quantification of the relationship between geochronology and the assessment of hazard. And I think this comes out initially in two parts. One, is the sensitivity tests of geochronological interpretations. And the second program as it applies to a place like Lathrop Wells.

And basically, I think that the quantification of the relationship between geochronology and hazard is at this point insufficiently known, so that there is perhaps more controversy about geochronology than there need be. On the other hand that's not necessarily the case, it might be the other way around. In any case, that's required. And, secondly, the problem shifts into another realm. It appears that the results are discrepant and the investigators cannot agree on a reasonable interpretation of all the results. So, therefore no real knowledge of the hazard.

The geochronological data, in my view, break down into two types. One, is the quantitative age information, which includes the argon-argon data, the potassium-argon data, most superceded by argon-argon data, cosmogenic Helium-3, the uranium-thorium data and the thermoluminescence.

Then there are what I call qualitative age modifiers or hazard modifiers. We have qualitative age modifiers or hazard modifiers. We have paleomagnetic direction data, soils stratigraphy and developmental stage data, volcanic stratigraphy data and volcanic geochemistry and eruption rate data.

Then, finally, my view again is that the synthesis of the data and hazard implications has to be done in the context of the volcanic process framework model, and I just want to give you some idea of what I'm talking about. And, describe geochronological data in some meaningful way.

The way I see the relationship between geochronology and hazard. This involves the age distribution of volcanism, and presumably, the Lathrop Wells center becomes somewhat more important than some others closer to the present, and that there is the general issue of the age distribution over the past ten million years or so, and then there is the subset of the issue of the age distribution over the last hundred thousand or so years.

The recurrence interval of volcanism. Again, we can break that down into the longer time scale and the shorter, more recent time scale and the spatial distribution. And my recommendation is that there needs to be a clearer quantification of the relationship between disruption probability and the geochronological results, and that way you can define what issues apply to Lathrop Wells and define what is needed in order to finish.

Now, with regard to the volcanic process. This is an interesting and very active field. Earth science research has made quite an advance in the last five to ten years, and there are several aspects to it, and this comes into the issue of what a volcanic event is...five parts to this starts out with some solid material down in the mantle probably at about fifty to eighty kilometers...back up to shallower levels and you would release some pressure and it starts to melt. This typically characterizes the melting rate and the compaction time, and in order for the melt to travel upward the solid material has to compact to let the melt rise toward the surface...and one ends up with a layer of liquid that is mostly separated.

Then there is transport of that material through this region here, which has no porosity and very little permeability in terms of the drain scale. It has to be transported through cracks, and the intermediate reservoir, as show here, is optional. It doesn't necessarily have to exit, but it presumably does, and there is a time scale associated with that, but there is probably fairly rapid transport to the surface through a dike, and resulting in an eruption.

The thing to keep in mind is that through the geochronology program one can tell when eruptions occurred. One can't have any volcanism unless there is melting in the mantle. To some degree one needs to know something about melting rates and magma production in the mantle, and other information on the geochronology will come into that. In fact, some of the work that Frank Perry has been doing in geochemistry, probably some of the only information that will relate to that. Another way of getting at that might be, although this is a little bit uncertain, the geophysics, basically the travel time information from the deep part of the crust and mantle.

Now I would like to talk briefly abut some of the quantitative information that comes from the various dating methods. I'll start with argon, then go to helium, uranium-thorium, very short on thermoluminescence and paleomag.

I want to say two things. First of all, these are not exhaustive discussions of these data. They are sort of examples of the way I look at data. And secondly, there may tend to be a few mistakes in some of the things I've done here. I'd have like to pass my discussion of 40/39 data past Brent Turrin, but I have been away for two weeks and I haven't had a chance. And, similarly with the paleomagnetic data I was intending to talk to Duane Champion about it ahead of time but didn't have a chance.

Now the way I see the 40/39 data is it has advantages particularly in the situation that has legal ramifications and that is has a long history of use. It has been shown for the most part to give correct ages in many instances,in other words ages that are consistent with other information and so on. And if there are pitfalls, many of them have been identified and methods further addressing the pitfalls have been developed to a large degree.

The problem specifically with the application to Lathrop Wells, and in fact some of the other young center of the Basin & Range as well, is that because they are very young, the lavas are very young and they are not particularly potassium rich, so there is not very much radiogenic argon. And there is also typically known to be some problem with fine grain basalts in that they don't always get equilibrated with air on eruption and have some excess argon. There are a number of examples from the literature and I think there is evidence in the Lathrop Wells data of the existence of excess ^{40}Ar on eruption that hasn't been de-gassed.

Again, basically the way I think of the way this system will work is when the lava flow, this irregular thing here that is supposed to be a lava flow that has come out over a pre-existing surface, and it has a few little xenoliths that are pieces of rock that it picked up on the way through the crust that don't belong to the magma initially. The idea is, as I understand it, is that the initial argon that this lava has, which may have an ^{40}Ar 40 content that is not the same as air escapes from the flow and the air argon dissolves in the flow. At the same time xenoliths de-gas the argon that they have. In the case of Lathrop Wells the tuff xenoliths have a large amount of ^{40}Ar in them. So, what one has here is a dynamic system where there is a transport in and out and from internal sources during the history of the lava flow as it goes across the surface and cools.

And, I think the important thing to recognize is that the time scale for argon diffusion is at least an order of magnitude or two faster than thermal diffusion. So, there is always a chance that the lava will solidify without the argon equilibrating. And, it is important to be able to establish this. I can't think it can be established a priori.

A typical diagram that is used to understand the data is this one, which is a plot of the $^{40}Ar/^{36}Ar$ isotope ratio versus the $^{39}Ar/^{36}Ar$ ratio. And this ratio here in a way represents the amount of potassium in the sample going off to the right. This is the air ratio of about 295. In the Lathrop Wells area there is miocene tuff that the basalt comes through which has a fairly high ratio. I have just estimated here. And there is Precambian Gneiss that makes up about 30 kilometers thickness of basement which has extremely high 40/36 ratios presumably because it is typically potassium rich. So there is abundant ^{40}Ar around for the lava to pick up as it comes to the surface. And the idea of measuring small age differences from zero in these samples, these are a few samples of basalt based on Brent Turrin's data from one of the samples he's measured. Basically, one has to be very careful that all of that potential incorporated radiogenic argon has gotten out and again it is a difficult problem.

Now if you look just in detail at a couple of the samples that have been measured, these two are from the QL3 Unit which is a lava flow which is from the intermediate age conostratographic unit. Here is the 40/36 ratio here with the argon left off and the 39/36 ratio. The air ratio is at 295, which is across here. And here is one sample and you can see that there is pretty evidence there as it trails off with no change in 39/36 that there is a little bit of inherited argon in one of these samples. And from my point of view, I can't see how you can tell that these don't have any in them. They might have a little bit too.

If one looks down here again this is the difference in slope between zero age and 100,000 years. Here are the data points with the estimated uncertainties. Now, there is a chance that I made a mistake in estimating the uncertainties. They were done by taking the age uncertainties and assigning it to an uncertainty in the 40/36 ratio. So, this is the sort of thing, the way the data look.

Here is another sample from this unit. And you can see it is plotted on two scales and you can see that there is one sample up here which was in fact screened petrographically to have no tuffs xenoliths in it, nevertheless has radiogenic argon in it, correlated with potassium as well. So, it doesn't give the age of the tuff or anything. It gives it an intermediate age.

Here is another plot which focuses on these data and you see again the small difference between zero and 100 kilo-years. Here is another sample which looks like it has a little extra argon in it. And you'll see that in each one of these diagrams if you can draw a horizontal line through most of the data that intersects this axis at about the number of 297 or 298, and I would contend that you could probably interpret these data in terms of the lava being younger than 100,000 years and having a slight amount of relatively evenly distributed radiogenic argon that it had on eruption and didn't get equilibrated with air.

Some of these error bars are incorrectly calculated. This one, for instance, is a little too big. But it doesn't really change the picture.

And, finally, here is another one of this error bar is incorrectly calculated. It should be more like the size of this one. And so basically these are state-of-the-art data. The point there is to make about this is that this is a situation which provides naturally a very difficult task in terms of dating it by K-Ar or $^{40}Ar/^{39}Ar$. And although these data are good as one can normally do, with the best of instrumentation and care, I think that they are consistent with the very young age of the order of 10^{5th} years or less. But the idea that these would be definitive in terms of the age, you know within 50,000 or 100,000 years doesn't seem entirely warranted by me, as far as I am concerned.

So, basically I think that there is--that the conventional approach is to dating these lava flows are difficult--it is difficult to see how they are really going to resolve the age if you want to resolve age differences or the details of the age, because of the probability of a small amount of excess ^{40}Ar and the small proportions of radiogenic ^{40}Ar. It is just very difficult to get resolution.

Now, in fact, Brent has some new data which has some higher resolution and I haven't really seen them plotted in all the appropriate ways yet. And they may help to get around some of these problems.

Step-heating of whole-rock samples in some cases have turned out to be a better approach and in fact, Brent Turrin has now started to measure some whole-rock samples by that method. And, I think that has more hope of resolving the age at the level one would like.

In any case, if the ages are a little off at Lathrop Wells, they are likely to be a little too old. So, they will provide an upper bound of a sort on the age.

I think that in order in the end to provide a convincing case for the age at Lathrop Wells based on K-Ar one will need to do some more detailed work on mineral separates or tuff inclusions. And if the duration of magmatic activity is less than 50,000 years, it is unlikely that the age differences between units could be confidently established by that method.

Now going onto the ^{3}He method, that has got some advantages as I see it, and one of them which I think is a very important one is that it has good age resolution, somewhere in the order of 10,000 to 20,000 years.

Helium loss problems for olivine which have been demonstrated I think for lavas that are a million years old or more, have not been shown to be a very significant problem for ages of 100,000 years or less. I don't think that is going to be a major issue here.

And there is some ability to get a check by doing ^{21}Ne as well as ^{3}He. Now, the main problem with this method at the moment is the production rates. And, this upper diagram comes from a publication by Mark Kurz, in which he has done measurements on Hawaiian lava flows whose age has been independently measured by ^{14}C. And he shows the normalized production rate. This is the production rate it would have, I guess near the equator and near sea level as a function of the ^{14}C age of the lavas. And you can see that it is a number of about 120 or 130 near the present. And then rocks that are 3,000 to 5,000 years old sort of suggest a lower production rate. And then back here there is more scattered, but the numbers are sort of between those two limits.

And each one of these is integrated over the age of the lava. So, this number represents the average production rate since 10,000 years ago, and these average since 5,000 years ago and so on.

Again if you then take where we have data and look at where Lathrop Wells ages may fall, we don't really know what the productions rates are out here. On the other hand, since this is probably a sinusoidal like curve and if one is looking--if one is going to be integrating over that curve, the farther one goes out in time, the less these oscillations are going to be important for determining the age. And consequently, I think with a few calibration points out here, this issue of the production rate of ^{3}He could be dealt with pretty well. The only problem is how to find rocks that you can date well by some other method than ^{3}He that are about 10^5 years old.

So, going to the disadvantages, as I say, the production rates are a bit uncertain. And, presumably these are due to secular variations in seismic-ray shielding by the geomagnetic field.

This second one is getting to be less of a problem everyday. There are more data coming out. By a year from now there will be quite a few applications of this method, and I think the prognosis is good at this point. There is this possibility of sand dunes blowing over the lava flows at Lathrop Wells, but I think, again, some of these things can be probably dealt with in a not unreasonable way.

So, I think the geomagnetic shielding problem probably can be estimated. It will take more data that exists, not just in the neighborhood of Nevada, but by other investigators. But, I think those will be forthcoming in the next year or so.

You have to be careful about calibrations of the method in 0 - 20,000 year range, because that doesn't necessarily apply to the older rocks. Presumably from the geology you can place some limits on burial and erosion corrections. And, in the end, this will give you a lower limit on the age anyway. Most of the corrections will increase the age. So, in combination with argon, one should be able to bracket ages reasonably well.

I think the most important issue at this point is that the method has a possibility of resolving age differences between units. If there is a 10,000 or 20,000 year age difference between two units or between three of the units at Lathrop Wells, for instance, this method probably can get the age difference, can sense the age differences. The absolute values of the ages could be in need of slight modification after that, but nevertheless you could see the differences.

At this point I think that maybe Jeanne has enough data to change this statement, but I am not sure. I don't think there is sufficient data yet to make a strong case for either the absolute age or the age differences, but it is getting close to that point.

Briefly, on the uranium - thorium data, this actually is an attractive method for a couple of reasons. One of which is that the--it does have potentially good age resolution and the basis of the method is pretty straight forward. The decay constants are well known. So, unlike the production rate issue with helium, the decay constants are well known for the system and there is every reason to expect pretty systematic behavior in this system. It should be less susceptible to the types of things, for instance that may disrupt the argon system. This overhead is not in my packet that you have, but you have seen it before and you may see it again today from Mike Morrell.

The one issue that I brought up and it borders on nitpicking is that with regard to this uranium - thorium age of 150,000 years, is that as Mike Morrell is well aware that the range in uranium - thorium ratio which is between about .86 and about .9 is about four percent, four to five percent. And in fact this system is not based on ^{238}U and ^{230}Th, it is based on ^{234}U and ^{230}Th, and one needs to know that the ^{234}U to ^{238}U ratio is constant.

Now, if there were enough spread in these ratios so that the spread out along this line in a much greater distance, I think the probably variability of ^{238}U to ^{234}U would be unlikely to be an issue. It is only because they are so tight together here that it might be an issue. It is well known that during weathering, uranium is fairly mobile, and ground-waters for instance have ^{234}U to ^{238}U ratios which are as much as 20 percent different than the equilibrium value. So, we know that ^{234}U can be released during weathering for minerals at a rate that is different from ^{238}U.

So, this can be checked. Those ratios can be measured. I don't know whether Mike Morrell had mentioned at one point that he was working on that. So, in the best of all worlds there would be more spread on this and one could have quite high confidence in the age determined by that method.

Now with regard to the thermoluminescence data, this has one theoretical advantage in having potentially very good age resolution even down to a few thousands years. The problems that I see with this are in fact the mechanism by which minerals record their age is known, I call it qualitatively. That is that there is an understanding of the basic physics, but it is at a very basic level. Often the discrepancies between ages measured by this method and other methods can be explained only in retrospect and that each sample, it is hard to predict how a particular sample is going to behave and you have to evaluate how it behaved in retrospect.

The bottom line is that it is still in the developmental stage. I personally see it as having applicability in the Holocene more than in the Pleistocene, but there are people who have managed to use this method for what looked like reasonably good ages on volcanic materials that are a hundred thousand years old or more. But, I think the number of cases is still relatively small. I am overall less convinced that this method is going to give very useful data at Lathrop Wells than the other three.

I wanted to make a brief comment on the paleomagnetic data and I just want to in a way play this back to Duane as the way I think I understand this issue. In general, I think it is clear that paleomag data or secular variation data is useful as a stratigraphic tool. It has been demonstrated to be useful. And, I see it as being particularly useful in continuous lava sequences, such as the ones in Hawaii that Duane Champion and others have studied, where small flow-to-flow changes in direction imply relative closeness in time and large changes suggest that there may be hiatuses.

The detailed evaluation of proximity in time for different volcanic units, though, a far as I can tell needs to be treated with care. And, the result that Duane has that two directions separated by about five degrees have been observed, I think, the way I look at this is when I look at the data from Hawaii, if one looks at the typical SV rate, although it is as high as five degrees per century in some areas, it is also as low as one degree per century for more of the time that is covered by the lavas in Hawaii. So, that would change this five degrees from looking like instead of 100 years to 500 years or maybe more. And the typical cinder cone eruptions last less than one year, then anything over 100 years means that you have got two separate events. So, in fact, once you have got two events, then it's a different situation than if you have only one.

And there are, in my, as far as I can tell, few data for comparison on flow sequences where the age is as great as 100,000 years and the flows aren't buried. That is like the ones at Lathrop Wells. And, again, with regard to this basic argument, this is a rather complicated plot, which probably takes about three days of study to figure out. This one is not so complicated. It shows the probability of a particular angle of magnetic direction in a rock differing from the average angle or the distribution and basically tells how the magnetic field wanders around the geographic pole. It typically is close to the pole and sometimes it gets away by 20 degrees, or several degrees, and it drops off in a sort of normal looking distribution as a function of angle.

One can convert these approximations to the field behavior so that one can take a difference in direction between two flows and can assess the probability that either the two flows have sampled the magnetic field in time totally randomly so that they have no relation in time to each other, or in fact they are part of the same flow or the same event so that they sampled it at the same instant in time.   And those two probabilities are the ones easiest to assess. This shows the probability Hr is the random hypothesis and probability depending on how the field behaves is somewhere in this range here and as Duane has pointed out the 4.7 degrees indicates about eleven, plus or minus probability of about 11 percent, which means that there is an 11 percent probability that lavas that have direction of 5 degrees have nothing to do in time with each other.

However, if you assess the probability that they are exactly at the same instant, this depends partly on how precisely you measure the sample, and if it is measured very precisely as Duane has done it, the probability that they are exactly the same in time is also about 10 percent. So the probability of randomness and simultaneity are approximately the same. So, Duane has taken it a step further and said that since we know that they are not the same because we can measure the difference, what is the likely time difference. Now this is--and he has come up with estimates that are in the hundreds of years range. And I just feel like there is a little bit uncertainty as to how far you can go with that and be confident, because you start to have to weight the probability of one statement versus the probability of another statement. There is no established method for doing that. Duane may have one, but I don't know the details of it.

Now, finally I want to make a comment on the geological relationships in the stratigraphy. I think the geologic characterizations of the Lathrop Wells site is one of the most critical aspects of the evaluation and similarly this could be extended to some of the other centers, but it is probably more important for the Lathrop Wells site because of its proximity in time to the present.

The soils data are very useful as an enrichment of the volcanic stratigraphy information. And, as I mentioned before, I think the geochemical data are useful as modifiers regarding likely changes in effusion rate in the future and for assessing the continuity of activity at individual centers. Certainly, these data are important. I mean, statements to the effect it is not clear how they relate to the geochronology, I think are unfounded. And, again, this overhead is not in the batch that I gave you, but it will probably be shown by Frank Perry, and if not you can get a xerox of this.

It shows the relationship between rate of eruption and basalt chemistry in Hawaii for the Mauna Kea volcano and shows that there is a systematic change in chemistry as it is shutting off and Frank has made some comparisons. Again, this is the type of information that can allow one to infer what is happening at the mantle depths where magma is created. If one has some information on whether magma is being created at an increasing rate or decreasing rate, certainly that must be an important observation.

So, in summary, with regard to the credibility issue, I think that there are some discrepancies between data sets, but I think it is likely in the long run and not all that long run they can be rationalized and they can still result in the credible hazard assessment, at least as it relates to the geochronology.

The conflicts between the investigators I think just call into question whether any useful information about age is in hand and that could lead someone to conclude that we are ignorant of the age and implying that the hazard cannot be satisfactorily estimated and I think that would be unfortunate because, I don't that is the case.

And, my assessment up until now, and I hope that it is history, is that public posturing by the investigators has been more of a problem than the geochronological data and the probability calculations.

Where do we stand as I see it? I have actually revised what I think slightly since I made this overhead. I think the oldest activity at Lathrop Wells is probably greater than 65,000 years and it is probably not older than 150,000 years. These are the brackets by helium and argon.

There were at least two eruptive events at the same locality and they are separated in time sufficiently that they need to be considered as two separate events. It would be nice to be able to determine how separated in time they are.

At the moment, there is insufficient data to rule out events younger than 65,000 years. Both the chronostratigraphic unit 2 and the possible cinder cone could be younger than that. We can't therefore, rule out the possibility of more than two events; can't define the time interval between the events.

My assessment is that the systematic investigations that are keyed to the geology will resolve these issues. And I think probably the $^{40}Ar/^{39}Ar$ and helium data are going to still be the most important aspects of assessing the issue in the end. And I am beginning to think or more and more thinking that the ^{3}He data because of its ability to dissolve events is going to be one of the most important individual types of data that will be brought to bear on the problem.

Thank you.

DR. ALLEN: Thank you, Don. Are there questions from the Board of its staff?

(No audible response.)

DR. ALLEN: Are there questions or comments from anyone else? Carl Johnson?

DR. JOHNSON: Carl Johnson.

Earlier in your remarks you made the comment that mantle melting is required in order to have a volcanic event. I agree with that; it sounds very logical. If that is the case, then that leads to a very fundamental question and that is, do we have mantle melting occurring beneath Yucca Mountain? And so, my question to you is from your oversight of the DOE program could you tell me where in the department's program they are going to address that issue?

DR. DEPAOLO: Maybe Greg Valentine will be talking about that briefly, but I think, in fact it is not far from the issue that Keith McConnell brought up. And, that is presumably this is related to extension rates and other aspects that one can characterize as the geophysics regionally as well as locally. And one can draw on the results, you know, how much volcanism has been related to how much extension and so on over the past ten million years and come up, I think, with combining it with some of the geochemistry data that Frank Perry has been gathering; make some I think reasonable statements about the likelihood and the extent.

DR. ALLEN: Yes. Cording.

DR. CORDING: Just a question regarding the summary. When you say the data indicate or they are insufficient to give the information there, are you limiting that to the geochronological data or do you refer in any way to some of the qualitative age data?

DR. DEPAOLO: I was mainly talking about the geochronologic data, the numerical data. There is a suggestion from the helium data that the cone, consistent with the stratigraphic data that the cone is younger than any of the flows by a substantial amount. But, I think in number of data that exists now would leave one a little bit unsatisfied that it was demonstrated.

DR. ALLEN: Any questions or comments from the audience?

Are you stepping forward, Jack? Jack Evernden.

MR. EVERNDEN: I am the first attendee at all this.

According to a figure that Crowe showed, the volcanic centers are distributed over a line which fills the time frame from sort of four million years up to four and a half, of some time frame up to nearly the present. What difference does it make what the age of any individual event of that thing is? Why is it any big concern how old Lathrop Wells is? It sits there. It's young. It's in the trend. What difference does it make how old it is?

DR. DEPAOLO: Well, I won't answer the question, but I will say that I did make a comment that relates to that. I think it needs to be better defined what difference it makes. And Bruce can--

DR. CROWE: Let me make just a quick comment. I will be answering that tomorrow and it makes differences in two ways. In how you plug the events into probability model and particularly how you look at whether we have multiple events because that has been factored into the potential of releases.

DR. ALLEN: Leon Reiter.

DR. REITER: Don, I wonder if you could shed some light on a part of the controversy that has appeared in the past that people have placed a lot of importance on, at least both in oral exchange and literature. And, that is the statistical techniques, the use of weighted means versus non-weighted means.

Briefly, if I understand it, Brent in the kind of ages he has come up using weighted means has come up with ages with relatively smaller errors and Bruce and his people have indicated that if you use the data without the weighted means, which is the appropriate way to get you get much larger scatter on the data. Could you sort of put that issue in any relevance or importance to what it is?

DR. DEPAOLO: The way I would answer the question is with this slide, with this overhead.

If you look at the data on the top graph, I would ask Kip Hodges what he thinks also, but I would say that it looks to me like there is something going on that has nothing to do with the age of the lava. And, there is a population of points which averages an argon ratio of around 300 something. And if one calculated the difference, the slope of the line between the average of those points and 0295.5, one would get an age.

If one measured another 100 points from this same population, average them and calculated the mean, and the standard deviation of the mean, as you increase the number of data points, assuming the distribution didn't change, the standard deviation of the mean would shrink and shrink and shrink. You would have a much more precise determination, but it could be absolutely wrong.

So, in a way, the statistical approach is not the only point even. But certainly averaging, you know, making a large number of measurements and calculating the standard deviation to the mean is going to guarantee you have a precise answer, but is not in any way going to guarantee that you have the correct answer or the answer you would like to have.

DR. REITER: Maybe I misunderstood. I thought one of the points that Brent and those people had made was that you didn't just take the data and average it, but you gave more data to that which had smaller variances. And therefore, as a result you narrowed the error bars. Maybe you addressed that, but I am not sure if you have.

DR. DEPAOLO: I didn't address that, because personally I think it is not the main issue.

I mean, let's say that there were no mistakes in this diagram and these were the correct error bars, you would get a lot of weight on this point, this point and this point and not much weight on the others. And they would essentially define the age.

In fact, since this one is close to the origin, this one would define the age. In fact, Brent didn't count this one because he sees that it is far off and must be contaminated. So, this one defines the age. But there is no guarantee that that one point is the right, I mean, it may have some contamination from radiogenic argon just like some of the others do like this one. There just isn't any way to statistically get the answer out of a data set like this. One can get the best estimate based on the data one has, and one can make that precise by making a large number of measurements, but I can't see how one can be certain that one has the answer one would like, which is the age of the event.

DR. ALLEN: Kip, do you have any comments on this?

DR. HODGES: Well, I am going to talk a little bit more about this tomorrow. But, basically, I agree with what Don says.

DR. ALLEN: Other comments or questions?

(No audible response.)

DR. ALLEN: Okay. Thank you, Don.

Jeanne, introduce your next speaker.

DR. COOPER: The next speaker will be Dr. John Geissman speaking about the paleomagnetism studies. John is from the University of New Mexico.

DR. GEISSMAN: I need to preface this discussion with the fact that the illustrations that are being shown as overheads are not identical to those in the package you received at the back of the room when you first entered. A new package is available for all interested at the back of the room and my sincere apology. They are similar, but not identical.

As Bruce Crowe has introduced and as Don DePaolo has reiterated, I feel as if there is a bit of confusion over the interpretation of the very large of paleomagnetic data which had been gathered from the Lathrop Wells locality. And long that line, first of all, regarding confusion, I would like to complement Don DePaolo in his, I think, quite accurate interpretation of the utility of paleomagnetic work in such a setting.

So, allow me to spend just a few moments going over some of the fundamental interpretations of fundamental aspects of paleomagnetic work when we are dealing with young volcanic rocks and the utility of paleomagnetism for relative dating.

First of all, and some of this may be very old hat to many of you, so I apologize, but nonetheless this won't take much bit of time. A fundamental tenet of course is that most geologic materials are capable with a high degree of fidelity of recording the geomagnetic field at some point in their history. And for young lava flows, that point in history so to speak, is when the lavas initially cool over a range of what we describe as magnetization blocking temperatures. This range is below approximately 580 degrees centigrade and you may continue down to much lower temperatures. For young, individual flows, the time period over which magnetization blocking occurs can be very, very short.

When we are dealing within lava flows, we can glean a great deal of information about the geomagnetic field and its past history in terms of first of all the directional changes of the geomagnetic field and also actually relative changes in intensity. But, the information is only partially available; partially deciphered. That is to say the record by virtue of the fact that the lava flows do not record a continuous history of the geomagnetic field on the record is spotty. So there are inherent problems with our interpretation of paleomagnetic data from young volcanic rocks.

Now to launch into the second point, that is to say the geomagnetic behavior, what we know of the geomagnetic field behavior, there is a number of issues which need to be covered. Don DePaolo mentioned very briefly this issue of secular variation. It is an important issue. What we are talking about here are on a geologic time perspective relatively short-term changes in the direction and intensity of the geomagnetic field at any particular locality. Secular variation, we believe, has a number of origins. One, it is controlled by non-dipole components of the geomagnetic field. Another potential source to secular variation is literal wobble or procession of the main dipole of the geomagnetic field, which averaged over time constitutes at least 80 percent of geomagnetic field behavior.

So, what we see at any particular locality in terms of secular variation in direction and intensity of the geomagnetic field may not be identical to what we see at any other locality, nearby perhaps, for that same time period, because, of the simple fact that non-dipolar components of the field are involved in this process.

Some studies, as is emphasized here have shown that secular variation in some localities can be as high as four to six degrees per century. Don has already mentioned this. But, this is based on available data using historically recorded lava flow sequences, and the data admittedly are very spotty. We do not understand how the secular variation of the geomagnetic field has operated over long periods of time in history.

Now, getting to higher amplitude phenomenon, so to speak, very briefly, it should be mentioned that geomagnetic excursions/what have been described as polarity episodes, simply represent high amplitude, short-term break downs of the geomagnetic field. Where the field, for example, may attempt to reverse itself in its polarity, but aborts, so to speak because of some instability in the dynamo generating process and returns to the pre-existing polarity state.

Bona fide geomagnetic field reversals as we all know have occurred on numerous occasions in the geologic past. The most recent being now approximately 780 kilo years ago.

Finally, averaged over long periods of geologic time, a very fundamental tenet in paleomagnetism and geomagnetism is that the geomagnetic field can be taken to be represented as an axial geocentric dipole. Axial meaning parallel to the rotational axis of planet earth; geocentric, meaning of course, centrally located within our planet. And this gives rise to fundamental hypotheses about paleomagnetism in the way in which we can push continents, so to speak, around on the surface of our planet. That is not the issue this afternoon, however.

In my discussion today, brief discussion, I'll emphasize there are ongoing studies at Lathrop Wells, and again to reiterate as Bruce Crowe mentioned, this is truly a progress report. I would like to highlight the following facts. Our sample has been conducted in May of 1991. We sampled only ten paleomagnetic sampling sites where a number of independent samples have been obtained. Four sites in Ql_5; 4 sites in Ql_6; and, two sites in the buried lava flow which Bruce emphasized in earlier discussion.

Progressive demagnetization has been carried out on much of the collection, not all of the collection to date and we will show some examples of progressive demagnetization work and the results of progressive demagnetization. As well, just as an internal check to see whether or not a fabric inherited in the rocks might be contaminating the paleomagnetic results to some degree and anisotropy of magnetic susceptibility measurements have been made on at least one specimen from each of the independent samples from all of the sampling sites prior to demagnetization.

Then in terms of looking at some of the data, I think it is important to bring out right away that for the three independent units sampled, our results are indicating nothing--no indication of an unusual geomagnetic field direction being recorded at the Lathrop Wells locality. And in fact, our results are not dissimilar from those reported by Turrin et al., in their 1991 Science article.

On the basis of the paleomagnetic data alone, however, I feel it is very, very difficult to place any quantitative estimate on the time duration of volcanic activity at Lathrop Wells. There are some problems encountered with dealing with rocks such as these. Lightening strikes for example. Also the problems of sampling intact material. It is not easy to do in a series of deposits like this.

Okay. Those of you who have picked up the new package of overheads, you will find this is the second illustration in this package. This simplified geologic map of the Lathrop Wells locality simply identifies our paleomagnetic sampling sites, 1 through 4 in Ql_6; 5 through 8 in Ql_5; 9 and 10 in the buried lava flow off to the north and west of the main cone.

The sampling strategy here, it should be emphasized was not intended to closely correspond with previous paleomagnetic sampling of the Lathrop Wells locality. It was simply to, let's take a look at what we thought were independent units and see what their paleomagnetic signature is.

Now, just for the sake of completeness in this discussion, I have prepared a very simplified illustration summarizing the paleomagnetic results of Turrin et al., described in their Science 1991 article. The results from Ql_3 shown here with the declination slightly west of north. The result of Qs_5 series of units with a declination very slightly east of north.

I have also plotted in this illustration for the sake of completeness the present geomagnetic field direction for the Lathrop Wells locality, at least present out of the 1985 compilation. And then finally, whether or not this is of utility or not for this issue, the time averaged axial geocentric dipole field direction, which is due north and has a inclination which were slightly steeper than the results observed from Ql_3 and the Qs_5 series. And again as noted by Turrin and colleagues a 4.7 degree angular separation between the Ql_3 and the Qs_5 results.

I don't want to bore you with a great deal of paleomagnetic data. We will look at some examples, first of all simply the natural remnant magnetization results from a few of the localities. We will look at some examples of progressive demagnetization behavior. And then finally look at examples of the magnetization vectors which I feel confidently about in terms of representing a thermal remnant magnetization in some of the units sampled.

The first few plots are simply plots of the direction of natural remnant magnetization in the independent samples from several of the paleomagnetic sampling sites. Now the natural remnant magnetization has been abbreviated NRM as you have often seen in paleomagnetic literature, and please remember that the NRM is simply a composite. It is a vector sum of all magnetizations conceivably present within that independent sample. So here are the individual samples from site LW2 in Ql_6. And if you think back to the previous illustration, these results are not dissimilar. The NRM directions are by no means dissimilar from the results from Ql_3 in the Qs_5 sequence by Turrin and colleagues.

NRM directions from site LW2 are certainly much steeper and there is a case to be made, which I will come back to in a few moments when we show the demagnetized results, the results from this site after progressive demagnetization has been applied. But, again, this is also from Ql_6.

We are finishing up, as we speak, results from Ql_5 and also the buried flow, so I am going to concentrate principally on NRM directions from these results. As Turrin and colleagues noted in their Science article, obtaining paleomagnetic results from flow Ql_5 was difficult at best. And, I fully concur with this. Bruce Crowe and I did the sampling back in May of '91. We are fortunate that I think we have come across a few decent respectable intact sites in Ql_5. One for example, is LW7. Where are the NRM results. This site is in the process of being demagnetized.

And also another site, LW8, NRM directions in flow unit Ql_5. Somewhat respectable results, but again these samples are in the process of demagnetization as we speak.

Now, speaking of demagnetization, allow me to show you two examples of progressive demagnetization treatment carried out on our friends from Lathrop Wells. In these funny looking diagrams, what paleomagnetists attempt to show is both the directional and intensity change of the magnetization present in a sample during progressive demagnetization. And bear with me a few moments so I can explain this diagram, this diagram and the following one, just show you get a complete understanding of what we are doing in progressive demagnetization treatment and exactly how we access the demagnetization results.

The magnetization vector, of course, in a rock is a three dimensional entity. It is a tensure of second rank. And to portray it on a plane, we need to fold that vector onto simultaneously a single plane. The way this is done is we first of all project the magnetization vector onto the horizontal plane. In this case, the north/south axis is here; east/west axis is here; east 90 degrees clockwise from north as it should be, of course; and the horizontal projection of the magnetization vector is shown as solid symbols.

We take the vertical plane which is simple the summation of the vertical value of a magnetization and we plot it with respect to the horizontal plane which is the square root of the sum of the squares of the north/south and east/west components, and plot that as a separate projection, but now we simply fold it down onto this plane of view and that vertical projection is shown as open symbols. What we are seeing in this diagram then is a series of projections of the magnetization present or a sample after treatment at 40 milli Tesla an alternating field demagnetization; 50 milli Tesla; 60 milli Tesla, and so on and so on and so on.

I think you can see quite clearly that the demagnetization trajectory here is quite well defined. In fact if we fit a three dimensional line to these data points we would get a maximum angular deviation of at most a few degrees. And it trends essentially to the origin.

And so, in the ensuing diagrams when you look at the magnetization vectors portrayed, please understand that each one of those vectors represents then the result of a progressive demagnetization and a linear least squares fit of the demagnetization data first of all inspected on such an orthoganal demagnetization diagram.

I show another example, we could go on and on with these, but I think it suffices to say as we would expect with young lavas the paleomagnetic signature of these rocks certainly isn't very internally complicated and demagnetization work suffice it to say does a very adequate job of resolving individual magnetization vectors present within the samples.

So now to some of the results now for a number of different sites at the Lathrop Wells locality. Here is site LW1 in Ql_6. These are the projections of the individual magnetization vectors now from the independent sites. And I also plot two other entities here on this diagram for the sake of completeness. One the present day field, at least 1985; present day field. And then finally the other point, the time averaged, axial geocentric dipole field direction for sake of comparison. The results overall are again as I mentioned not dissimilar. The results from Ql_6 here are not dissimilar from those reported by Turrin and colleagues from Ql_3 and the Qs_5.

Second site in Ql_6, slightly different declination overall; inclination is very similar. We can superimpose these two if you wish, although the diagrams are slightly different in their diameter. I apologize about this. Declination slightly different. And I think this harps on one important point. And that is that it not always easy to sample intact material in an environment such as at Lathrop Wells. And then finally, two other results for the buried lava flow which Bruce spoke of earlier. Again, these are magnetization vectors, site LW9, somewhat dispersed and certainly is slightly shallower inclination than the expected time averaged geomagnetic field direction and also previously results from Ql_3 and Qs_5.

However, if we go to the second siege in the buried lava flow, we see the problem, which I perhaps overly described, but it is a very real problem. And that is the difficulty of its sampling intact material in some of these exposures. For example, one of our samples here may have come from a block which has been rotated very much out of place, and so on and so on. Difficulty dealing with surface exposures in these materials. Much greater distribution of magnetization vectors at this particular site.

Let me just come back to the conclusions here in terms of ongoing studies at Lathrop Wells. There are problems which we have encountered in terms of sampling intact material. Overall, so far at least, our results are not dissimilar from those reported by Turrin and colleagues. However, to come back to the issue of paleomagnetic secular variation in interpretation, I think we need to be very cautious about how we interpret the paleomagnetic data from such young rocks. Our knowledge of secular variation of the geomagnetic field over geologic time is very spotty at best.

Thank you, very much.

DR. ALLEN: Well, thank you, John.

Are there questions from the Board or staff?

(No audible response.)

DR. ALLEN: Questions from anyone else at the front table?

(No audible response.)

DR. ALLEN: Questions or comments from the audience?

Jack Evernden.

MR. EVERNDEN: I am prepared to believe that I am speaking in ignorance. On your picture here on Ql_6 there are a bunch of circles. I understand those are different samples from Ql_6?

DR. GEISSMAN: At one particular sampling site, sir.

MR. EVERNDEN: So those--wait a minute. At one site these are different samples. So, we've got different axis, different directions of magnetism out of the sample?

DR. GEISSMAN: Yes, that is correct.

MR. EVERNDEN: Over the spread. So we go to the figure just before that and we have Ql_3 and Qs_5, and are those plots there averages of fields like a distribution field shown on the adjacent figure?

DR. GEISSMAN: They indeed are, sir. And, as I mentioned at the--

MR. EVERNDEN: Then, the final question is given the spread that is on something like Ql_6, I would like to know the statistical significance of the difference of those two averages Ql_3 and Qs_5. It would suggest there isn't much statistical significance to that.

DR. GEISSMAN: Right.

MR. EVERNDEN: And therefore, this whole discussion about 4.7 degrees "mas/menos" number of years is sort of a pointless discussion.

DR. GEISSMAN: I think you raise a very important issue. Given the amount of information presented in the Science article by Turrin and colleagues, I find it very difficult to assess the statistical significance of these two results. Turrin and colleagues report at a high level of probability that these two observations are statistically distinguishable. And, that is all I can say. But, I think you raise a very important issue.

MR. EVERNDEN: Thank you.

DR. ALLEN: Any other comments? Are Turrin and Champion here? Do you want to comment?

DR. CHAMPION: Duane Champion, US Geological Survey. Jack I can answer your question to some extent. The Ql_3 and the Qs_5 are the means of means. The individual site means have alpha 95 attended to the source of dispersion that John was showing in his diagrams. They are 3 and 4 and 5 degree alpha 95s. But then when you mean on the basis of geologic unit establishment, then you get a new alpha 95 for the different units as you assign them. And then those two alpha 95s are distinct from one another at the 99.98 percent confidence level. You can still quibble that the 4.7 degrees doesn't really mean anything, and I suspect once I send this to peer review for journal review, my reviewers will be harassing me that I dared to identify a difference in those two directions. But it can be statistically stated.

DR. ALLEN: Jack, do you want to come to the mike?

(No audible response.)

DR. ALLEN: Other comments or questions?

(No audible response.)

DR. ALLEN: Thank you, John.

All right. We are potentially on schedule. Jane will be the final speaker before the break.

DR. POTHS: I'll be talking to you today about what we know at the moment about cosmogenic helium. For this talk I will start out by stating what my conclusions are based on what we know so far. I will then attempt to convince you that those conclusions are based on some reasonable data. I'll give you a little bit of background to understand cosmogenic helium data, though Don DePaolo has done a great job on that. I'll mention what our results are and where we are going from now and then a few observations about excess argon in the samples at Lathrop Wells.

Are conclusions so far are that Ql_3, Ql_4, and Ql_5 lavas all erupted about 65,000 years ago. This is on the cosmogenic helium time scale. With in the current resolution that we have of around 10,000 years Ql_3 and Ql_5 are the same age. To put this on an absolute or a numerical age range, we believe that these lavas erupted sometime between about 40,000 and 100,000 years. The cone, we think is at least older than 18,000 years, but we cannot rule out that it is the same age as the lavas.

I'll next go through the background. This is a figure that didn't make it into your packet but it will be by the end of tomorrow. This simply illustrates that the buildup of cosmogenic helium, I have assumed a constant production rate which is one of my assumptions. I'll discuss that in a minute. Basically, there is a small background in the rocks. You have a buildup of cosmogenic helium with time. So, if you know that you have 20 million atoms of cosmogenic helium, the age is around 65,000 years.

That is the ideal case in practice due to effects such as erosion or potential cover of the samples. The exposure age is always less to or equal to the true eruption age. I have signed an uncertainty of about plus or minus 30 percent to the ^{3}He ages. This is based on evaluations of the possible fluctuations in the production rate with time based on Lau's work. We do not work with whole rock samples, but rather separate out olivine from the samples. This is because olivine retains the cosmogenic helium quantitatively and I have a figure with cosmogenic neon to demonstrate that.

I will now move onto what results I have. First a map of Lathrop Wells borrowed from Bruce Crowe a few minutes ago. We have two different surfaces from Ql_3; two surfaces from Ql_4 that may have some problems. We have two different surfaces from Ql_5, and we have three loose bombs from the summit of the cone that are about a kilogram each. There have been a lot of geologists running up and down on that cone, so we can't guarantee that these samples from the cone are in place.

DR. ALLEN: What do you mean in place? A bomb in place means?

DR. POTHS: That it is sitting on the surface of the flat surface of the cone. Someone might have picked it up and moved it for some reason. There are a lot of paleomagnetic poles that have been sampled up there and such. I don't know if someone might have possibly moved something.

DR. CROWE: This is Bruce Crowe. Let me see if I can clarify that a little bit.

In the ideal condition at the summit of the cone, what we like to have is agglutinated bombs where they have been plastic enough when they have landed that they adhere and there is a good coherency to the deposit. One of the problems of the summit of Lathrop Wells is that there is very, very limited agglutination. So, we have to be very concerned about whether there has been downslope wasting or slumping or whether there are just disturbances. I mean there is an active quarry area and a lot of people have been up playing around on that thing and it is a concern we have for the whole spectrum of samples that come from there.

DR. ALLEN: I can visualize people carrying the bombs downhill. I can't visualize them carrying the bombs uphill very well.

DR. POTHS: Having been up there, I agree with you.

These are the results that we have so far. These are all of the individual analyses. Duplicate analyses are shown just analytical precision is shown on the same line. Different surfaces are shown on separate lines. For instance, for Ql_5, we have an analytical duplicate on one surface and a single analysis of another surface.

Let's look at the results for the lava so far. We think that within our analytical uncertainties, Ql_3 and Ql_5 are giving about the same age and reassuringly do indeed seem to be clustering quite well. We think our analytical uncertainties are quite reasonable here as well. So, this is the basis of the statement that we think Ql_3 and Ql_5 may be somewhere around 60,000 to 75,000 years old.

Ql_4 presents a small problem because it is overlain by Ql_3, yet it gives a younger age. We think this may be a surface preservation problem. In this case, we need to go back and resample. That was one of the earlier samples and we may not have been as clued into the surface preservation problems there as we are now.

As far as the cone is concerned, we have three bombs. In this case, they show a fair amount of scatter that we do not understand at this point in time. The most conservative approach is to say that it is at the lower limit of the youngest bomb that we say and that is the basis for saying that it is at least 18,000 years old. It could be as old as the flows. That will require some more sampling and analysis that we have in process.

DR. ALLEN: Can I ask a question here a moment? You say that Ql_4 may be in error because of overlaying by Ql_3. Isn't Ql_5 also overlain by Ql_3?

DR. POTHS: Yes, it is. But there is no difference--

DR. ALLEN: Why do you believe one and not the other?

DR. CROWE: Basically, what we think the problem is is that the Ql_4 lava where we sampled is right in the axis of deposition of sand and there is a pretty high chance we have had sand dunes when we crossed that. We think basically it has been shield by sand.

DR. ALLEN: Well, it is a local--

DR. CROWE: We think it is a local phenomenon, right.

DR. POTHS: Also, if you have had sand movement like that, there is a possibility that there has been more significant erosion of that surface than for the other surfaces.

And the way that other people doing surface exposure dating get around this sort of a question as they sample, not just two surfaces, but multiple surfaces, and look for convergence of the age, and because these tend to be minimum ages they believe the oldest age that they find. So, we are taking the conservative approach to this. We don't fully understand Ql_4 but from stratigraphy, we know that it must be at least as old as Ql_3.

DR. MELSON: Could I just pursue this briefly? This is Bill Melson.

In various your presentations here it would be nice if you could maybe speak briefly to the penetration of the cosmogenic particles? I mean, how rapidly does the production of what is retained drop off with depth? Just, you know roughly.

DR. POTHS: About 50 centimeters. In about 50 centimeters depth the production is down by a factor of 2. So, it is not an extremely large effect and we have gone to great lengths to collect samples where we think we have chilled margins of flows and if possible original preserved surface features, so that we do not have to worry a lot about surface preservation.

One of the questions that always comes up is whether you can be certain that the $_3$He has been retained in the samples. This is an attempt to demonstrate that we believe that that is the case. This is the ratio of cosmogenic $_3$He to $_{21}$Ne. The $_{21}$Ne is produced in much the same process. This is for two different volcanic fields that we worked on. The predictions by Lal et al. And we find that the $_3$He/$_{21}$Ne ratios are in quite reasonable agreement with the predictions and also with an analysis by Marti and Craig of Olivine from Hawaii. We would expect that there had been significant loss of $_3$He, that this ratio would tend to be lower than the predicted value.

We have done, and this is another figure that is just for illustration that is not in your packet. I'll be sure that you get these additional figures by the end of the meeting. This is work that has been done at the Potrillos volcanic field in southern New Mexico in our lab. These are different volcanic centers within the Potrillos volcanic field.

The main purpose of this view graph is simply to illustrate that we are getting quite good, reasonable reproducibility. Where we know it these are in stratigraphic order; where there is overlap and indeed we do agree with stratigraphic order and we are getting reasonable reproducibility. The samples at Black Mountain are actually three sub-centers within that, so that the scatter we see there is not unusual.

Where are we going to go from here? We have collected five duplicate surfaces from each of Ql_3 and Ql_5. We will go back and do the same thing for Ql_4 and try and determine what our true reproducibility from sampling as well as analysis is. We have collected in place bombs from the site of the cone. They are on the order of 100 kilograms, so we know no one has been carrying those around. And since they are on the side rather than on the flat summit of the cone, we believe that since there has been so minimal erosion of the side of the cone, that there is no question of potential cover of those samples with time. And, so we feel that those will represent some of our best chance to determine an age for the cone itself. We are also planning on taking a scoria sample just from the surface of the cone and see if we can separate enough olivine from that to get a reasonable age.

We have identified a small spatter mound near the quarry that looks quite young. And, a potential vent area for the Ql_6 flow that have nice well-preserved surfaces and we are intending to sample those and determine what ages those have. That is more to the point of finding out what the age range is. And if addressing the model of polycyclic volcanism.

Obviously, one of the largest challenges for this is to try and cross calibrate to other chronometers. This whole controversy would not have erupted if this was an easy problem. We have two sites where we are working on this. Both in the Zuni-Bandera volcanic field near Grants, New

Mexico. In one case we collected charcoal from underneath and within a flow. Those give ages of 9,100 and 9,800 years; cosmogenic helium gives 13,000 years. With this plus or minus 30 percent, that is just about at those limits. And that is not too unreasonable agreement.

Another tack to address the problem of calibrating older ages, there is a uranium series disequilibrium age by Ken Simms, of about 80,000 years for the Bluewater flow again near Grants. And we are in the process of measuring the cosmogenic helium in that sample. It is worth noting that that same flow also gave a 2 million year potassium argon age. But in our work that we have done so far, we have found that there is extremely large amounts of excess mantle $_{40}$Ar in those samples. And, so that age is clearly in error.

Now, that leads into the observations that we have on olivine samples from Lathrop Wells itself in terms of excess argon. Part of our procedure for analyzing the samples is to crush the samples to try and release trapped mantle gases in vesicles in the olivine grains. In doing this we measure the amount in isotopic composition of argon in a few cases. And for both the Ql_3 and Q_5 lavas we find $_{40}$Ar/$_{36}$Ar ratios considerably in excess of air.

Our interpretation is that this is excess argon that is coming from the mantle and not from in situ decay of

$_{40}$K, because release on crushing should never release a radiogenic component. The concentrations are about as equivalent to what you would get in whole rock from 100,000 years of potassium decay. However, I want you to note that olivine only makes up a few percent of these samples and so if that was the only source of excess argon, it would not affect potassium argon ages. However, it has been commonly found that if you have excess argon in olivine, you also have it in fine grained groundmass and glass. And we feel that before we can state whether we believe or disbelieve the potassium argon in $_{40}$Ar/$_{39}$Ar ages, that this distribution of this excess argon component needs to be better documented.

To emphasize that point, this is a viewgraph that plotted the Turrin et al., 1991 Science data for Ql$_5$. They are very close to the, again the increased potassium is in this direction and this is the $_{40}$Ar/$_{36}$Ar ratio. They plot very close to the air line. And the gas release during crushing shows extreme excess of the argon compared to their samples. And this adds emphasis to the fact that you need to understand the distribution of this component before you can interpret the potassium argon data.

With that I will stop and ask for questions.

DR. ALLEN: Thank you.

Any questions? Bill Melson.

DR. MELSON: I just have a quick one. What is retentivity like for the feldspar plagioclase for example and -- is it very poor for $_{3}$He?

DR. POTHS: Extremely poor. In our measurements we have found that based on the cosmogenic $_{21}$Ne less than 5 percent of the helium is retained in those samples. In addition to the work that I have done, there have been diffusion rate studies on actual separated olivine separates that have cosmogenic helium in them. And they have shown diffusion coefficients that suggest that the cosmogenic helium should be retained in olivine on an order of a billion years. And, so there is a real limit that says that it is highly unlikely that there are diffusion loss problems in olivine.

DR. MELSON: What is the retentivity of magnetite? What I am getting at is olivine doesn't occur in a lot of very important lavas and your method seems so promising. I wondering are we restricted simply to olivine bearing lavas or can this be used on other minerals?

DR. POTHS: The work so far by Poreda and Craig and other people has shown that clinopyroxene retains the cosmogenic helium and has an almost identical production rate to the olivine. So, that offers some spread in the materials that can be dated. Also, we have a very good idea that the neon is retained in quartz. So, it is a more difficult measurement, but that can be measured in quartz and also allows calibration to other cosmogenic nuclides such as Chlorine 36 or Beryllium 10 that are often measured in quartz.

DR. MELSON: Just one other question. How does hornblende turn out in regard to retentivity?

DR. POTHS: I don't believe that has been measured as yet. There may be problems in that we need to rely on very low uranium phases and there are a number of issues that have to be addressed in terms of whether there could be other components introduced by different kinds of minerals. We know that olivine is a very simple system and that our corrections are very small.

DR. ALLEN: Kip Hodges.

DR. HODGES: I have three questions. The first one is simple. At what level of confidence do you described your errors? When you say plus or minus 30 percent is that one sigma, two sigma?

DR. POTHS: I would say that was about 1.5 sigma.

DR. HODGES: Okay. And the second question is you were concerned about the amount of the release or the retentivity of the helium in the various samples and you showed a diagram, I believe, of helium versus neon some isotopic ratio. What do you know about the relative diffusivity of helium and neon? I mean if they left the system in effectively equal proportions then you wouldn't be able to make the test you are trying to test that way. I don't know. I am just wondering.

DR. POTHS: There have been many cases where it has been seen that there is helium loss from rocks and not significant neon in quite a few phases. I know of no examples and it has been studied quite a bit, particularly in meteorite work in step wise heating and such. Helium is always lost preferentially.

DR. HODGES: Okay, and then the third question, do you have any idea where the argon is sitting in the olivine that you are looking at? And the reason I ask that is that there is increasing evidence, certainly from metamorphic minerals that a lot of the excess argon is actually sitting in fluid inclusions and not within the intact structure. Do you have any evidence to that fact?

DR. POTHS: The evidence that I have is that certainly from analogy to the magmatic helium that is released, we have done extreme studies on that. And, the helium to argon ratio is about constant. So, you can draw conclusions based on the helium. It is very grain-sized dependent. And below about 200 microns you have very little left upon crushing. And, so it must be in glass inclusions or small late crystallizing inclusions within the olivine.

DR. ALLEN: Bob Luce.

DR. LUCE: I guess my question was pretty much answered there. I was wondering what the size was relative to other atoms of the helium there. I just don't have a feel for it where it would actually fit in these structures?

DR. POTHS: Because it is a nuclear product it is basically randomly distributed within the structure, because it is not--oh you mean the magmatic.

DR. LUCE: Within the silicate structure, let's say. What size openings does it have to have to stay in there? Do you also look at fairly unweathered, I guess you must look at unweathered samples.

DR. POTHS: I think basically it is in inclusions of sorts within the minerals. It is not particularly included within the structure, except perhaps within glass is the other classic case that people look at. But, size-wise, I don't know off the top of my head.

DR. ALLEN: Other quick questions or comments?

Yeah, Don DePaolo.

DR. DEPAOLO: Jane, what is the equivalent sea level low latitude production rate of helium that you are using for these calculations?

DR. POTHS: It is around 100 atoms per gram per year.

DR. DEPAOLO: 100. Okay.

DR. POTHS: It is based on Thure Cerling's calibration.

DR. ALLEN: We have got to cut it off here quickly, but Duane Champion.

DR. CHAMPION: In a presentation that you made this spring, you presented a small data table of the production rates and the age results that you had received or calculated for the different Lathrop Wells products. And you indicated that they were derived from Thure Cerling's 432 rate appropriate to Utah and Utah's elevation. In that table you used a production rate of 288 for the bomb products and 257 for the lava products. And, I understand that a more southerly latitude and a lower elevation production rate would be lower, but can you explain why your productions rates were ten percent different from each other?

DR. POTHS: Certainly. The cone that reflects the altitude dependence, the top of the cone is some 400 feet higher than the lava flows and that ten percent difference simply reflects the correction for elevation.

DR. CHAMPION: So, 100 meters is ten percent? I mean the attenuation is that great over the 100 meter of air column?

DR. POTHS: 130, yes.

DR. ALLEN: Okay. I think we ought to call it quits here and start our break.

Let's break for 15 minutes and then we will return and continue.

(Whereupon, a recess was had.)

DR. ALLEN: Okay, Jeanne, would you introduce the next DOE speaker, please?

DR. COOPER: The next speaker will be Dr. Les McFadden from the University of New Mexico and he will be speaking about soils and geomorphic studies, part I.

DR. MCFADDEN: In 1986 Steve Wells and I submitted a open file report to the US Geological Survey that described the results of our soils and geomorphic work in the Lathrop Wells/Crater Flat volcanic field. The results of our work, number one, suggested that on the basis of soil and geomorphic evidence that the most recent volcanism had occurred in the last 20,000 to 30,000 years and then second, on the basis of stratigraphy of soils and volcanic deposits exposed in a quarry, shown on the right, that the eruptive activity had been episodic, that is at least enough time had passed between eruptions to allow soils to develop.

Little did we realize that this would lead to a great deal of controversy and contention, yet we continued our work and in 1990 published a paper describing these results compared with the results of our more extensive work in the Cima Volcanic Field in the Journal of Geology.

Basically today I would like to talk about the soils and geomorphic evidence. There has been a lot of controversy over this. The essence of much of the criticism of the soils in geomorphalogical studies concerns basically some of our interpretations of the soils and stratigraphy exposed in the now infamous cinder quarry in the southern flank of Lathrop Wells Cinder Cone, as we now call it. The principal areas of concern shown on the right concern the heterolithic lapilli-rich, quartzo-feldspathic deposits exposed in the Lathrop Wells Quarry, as to whether these are pedogenically modified primary fall-out deposits or sediments emplaced by some mass movement process. And, also as important, there is some concern about the age estimates for soils and geomorphic features at Lathrop Wells and how that evidence for age estimate and basis for calibration, correlation.

So, the talk again, as I mentioned previously will concern both the discussion of ongoing studies but also focus on some of these questions concerning the interpretation of the work. The first part of the talk which will concern soils, this is an outline for that part of the talk. The second half of the talk will be presented by Steve Wells who will discuss the geomorphological studies.

My intention with respect to soils is to, number one, talk about the basis for much of your work on soils in arid regions which basically has been done in the Cima Volcanic Field in Mohave Desert. That will give you a good framework or good basis for at least looking at some of the more recent work on scoria soils that we see preserved in the Lathrop Wells Cone. We will also talk about some of the differences and interpretation of the parent materials for the soils at Lathrop Wells, what I regard as soils and other peoples' interpretation of those materials.

I would also like to talk about age estimates and try to clarify some of the problems involving arguments concerning age estimates of soils. And finally talk a little bit about some of the future studies I have planned for soils.

Again, I would like to start with the Cima Volcanic Field. It is here that we have done our most extensive work on the development of soils on volcanic land forms in an arid and semi-arid climate. The Cima Volcanic Field located in the central Mojave Desert of southern California, consists of over 60 cinder cones and associated volcanic flows which range in age from Pleistocene or latest Miocene in age to latest Quaternary. My work has centered on looking at the soils that have formed in a variety of land forms in this volcanic field.

On the right-hand side you see a pit excavated on a late Quaternary flow. And to make a long story short, one of our major discoveries is that soil development does not occur by virtue of altering the basalt to subsequent secondary clays and the like. Instead, soil development consists primarily of development of soils in the dust that is incorporated on these flows and the development of pedogenic horizons in the dust below, and I repeat below, a stone pavement which is maintained at the surface, as soil development takes place, not only just a few thousand years, but over literally hundreds of thousands of years.

As you see on the right, soil development on the latest Quaternary flows in the Cima Volcanic Field is rather limited. In fact, in this soil we see a thin horizon at the top of the soil that is referred to as vesicular A horizon, sort of a pale horizon composed of the most recently incorporated silt, fine sand and clay. Below that is a weakly developed horizon called the B horizon by pedologists or more correctly a cambic B horizon. And there are minor accumulations of calcium carbonates and salts that occur largely below the B horizon, also accumulated on coarse material that consists of rubblized material presumably derived from mechanical weather of the flow.

UNIDENTIFIED SPEAKER: Would you point those out?

DR. MCFADDEN: Sure, why not? I guess there is a pointer somewhere, or I could just walk up to it.

The Av horizon is this horizon right at the surface you can see breaking out with angular blocking structure to secondary porous prismatic. This slightly redder zone in here is called the cambic B. This light material here consists of calcium carbonate that has accumulated largely on the bottoms and sides of the larger and coarser class in the profile. Is that enough? Believe me, I can go on and on.

Now one of the most interesting things we noted here is that on increasingly older flows, we observed increasingly more systematic increases in soil development. On the left-hand side of the slide for example, you see the soil that has formed on a 500,000 year old flow dated by Brent Turrin from the US Geological Survey. Here you see the ubiquitous vesicular A horizon. That is again this pale, brownish, yellow horizon at the surface, but below that instead of a weakly developed cambic B, then below the Av is a much more well-developed horizon referred to by pedologists as an argillic horizon, again developed completely, almost completely in accreted eolian material, and here a class of rubble material and on the bottoms of those, we find moderately thick, continuous accumulation of calcium carbonate and occasionally more soluble material such as gypsum and soluble salts. So, with increasing time we see more well developed soils.

This led us to propose a sequence of soil development and as Dr. Wells also determined, a sequence of land form evolution with which the soil development is associated. And, what we recognized is that one of the most critical aspects of soil development in this area was the development of this well-developed Av horizon.

What we have concluded is is that these soils, the development of these soils over tens of thousands to hundreds of thousands of years reflects cumulic soil development. That is soil development was initiated at the flow surface with progress and incorporation and accretion of fines below the evolving pavement. Eventually, we build up these thick soil profiles developed below a stable volcanic stone pavement.

On your left, what you see is a schematic diagram that represents for example the weakest phase of soil development which we find only on the youngest volcanic flows in the Cima Volcanic Field. At the surface of the soil, we see the volcanic pavement. Below that we see the vesicular A. We presume that the vesicular A horizon represents the primary horizon that is accreting and actually growing upward with time. It represents the accumulation of clay and silts that ultimately consolidate and form what we refer to as a pedogenic B horizon.

Below this horizon, more soluble materials are carried by solution transport such as calcium carbonate, gypsum and more soluble salts. They can be carried to the base of the accretionary eolian material, rubble contact, or even deeper as some exposures of flows would suggest.

On the right-hand side, you see in the Cima volcanic field what soil development looks like in scoria. Now most of our early studies, in fact all of our early studies of soils in this volcanic field, concentrated on soils developed on the volcanic flows. However, in the past few years, we have had to transfer our attention to soil development and scoria, because at Lathrop Wells the soils are developed in scoria. Soils on flow are obscured by the accumulation of large dunes.   So, we have had to worry about what soils in scoria look like.

On the right-hand side, you see a scoria in the Cima Volcanic Field. This scoria was erupted from Black Tank Cone, a cone that Steve Wells will talk about in greater detail later. This cone, we believe is the youngest cinder cone and eruption in the Cima Volcanic Field. Perhaps, as young as 15,000 to 10,000 years old.

What is important to me from a pedological point of view is the soil that is formed in this, you can see that it has very minimal soil development. The most soil development that is as characterized by the accumulation by what pedologists call plasma which are all materials capable of pedogenic transport within the solum. It consists of a very thick vesicular A horizon; the horizon that we call the transitional AB horizon, and then below that we have basically a framework scoria characterized by merely the accumulation of salts, carbonates, below class and the oxidation of the tops of the class, principally characterized by the accumulation of some pedogenic iron oxides and small silt caps.

Compared to the soils developed on the flows associated with this cone, I would argue that this represents phase I soil development. For your interest this scoria fall sheet was emplaced over a much more developed soil that we call a phase II soil. This soil formed over hundreds of thousands of years in a cone apron deposit derived from a much older cone that erupted in the almost precise position of the current or the youngest Quaternary cone. And you can see how soils at least gives you a feeling for the relative age difference in these deposits in this volcanic field.

Now I would like to turn your attention to our ongoing studies of soils formed in similar scoria and pyroclastic surge deposits associated with Lathrop Wells Cone.

On the left-hand side is the map that Frank Perry gave me that turned out to be missing a big lava flow in here, but no big deal. Typical IBM screw-up, compared to my nice MacIntosh slides. At any rate, you see here that these three points are the points that indicate some of the soils we will be discussing. This of course, is the infamous quarry site stratigraphy. That point up there is a soil in what is unequivocally scoria which I will be showing you some data for. And the highest black point, which I could have jumped to when I was a lot younger, is this particular soil right here which is a soil developed in a pyroclastic surge.

Now briefly, to make a long story short, the soils we see developed in these types of deposits in Lathrop Wells are almost completely identical to the same type of soils we see developed in scoria associated with the youngest cone in the Cima Volcanic Field.

The upper most horizon is a thin vesicular A horizon. Below that is a very thin BW horizon and then these light accumulations here are accumulations of calcium carbonate, fine silts and clays which have accumulated in the very well-bedded pyroclastic surge materials. Again, we would consider this a phase I soil similar to those that occur in the Cima Volcanic Field. And, I might mention that the climates of these two regions are essentially the same; an arid climate characterized by around 15 to 20 centimeters of annual rain, depending on where you are. And, accordingly a vegetation primarily consisting of such as larrea tridentata. In other words, we believe we can compare soils in these areas by virtue of forming the same types of parent materials and under the same type of climate.

Now, what I am going to show you here is some really detailed, tediously detailed soil information, but it is interesting to me, so you get it too.

This on the right is a column that shows the typical type of soil that is developed in scoria in the Lathrop Wells area. First of all, the upper most unit would be the pavement that is formed in the scoria itself. Underlying that is the vesicular A horizon which I have told you about and which I believe is the most critical soil horizon ultimately responsible for the development of the cumulic soil that forms the B horizon over long enough period of time.

Its thickness varies between one to as much as five centimeters. I won't read all those things other than to say those are the typical types of field properties that characterize the critical soil horizons. Those are the types of properties that you describe in the field in order to recognize what we call genetic soil horizon.

Beneath the vesicular A, is the ABvk or the Bwk. This is just a fancy way of saying that we have a transitional horizon that exhibits characteristics of both an A and a B horizon sometimes vesicular pores that characterize A horizon actually continue well down into the slightly redder and angular block B horizon. B and K are just subordinate modifiers that distinguish characteristic properties of the diagnostic horizons here. That means vesicular and this stands for pedogenic calcium carbonate. If you don't see vesicles then we just identify it as a B. This means weak development, again that means carbonate. This horizon can be as much as 15 centimeters thick.

The critical aspect of both the Av and the Bwk horizons or the AB horizons, are that in these cases that the matrix or the fines which consist almost of entirely incorporated eolian material completely fill the pores and in some cases we would argue have actually resulted in expanse of soil development or dilatant soil development which is critical to soils as I will point out in a few minutes.

Now, below that, we get into more of a framework scoria where the pedogenesis is characterized primarily by the accumulation of calcium carbonate and more soluble salts on the basis of class as well as the alteration and formation of iron oxides, which we see in the tops of scoria materials.

Below that, in the scoria, the horizon can be several meters thick. We have seen some exposures in pits that would suggest that salts are being leached to depths that exceed two meters. That is not too surprising, given the solubility of sodium carbonates and other similar types of soluble salts.

Now, on the right-hand side, you see some data for this particular soil which is formed on a scoria unit identified by Bruce Crowe and others as Qs4. This scoria unit is present again on the north side or the north flank of the cone. And what is critical about this particular soil is clearly developed in scoria. Nobody would argue that this is a cone apron deposit.

Let's look at what we call these types of deposits, to back up, or these type of graphs are depth functions. And depth functions are what we use for certain properties to determine the extent to which we have pedogenesis influencing the original parent material.

Silt plus clay for example, identifies pedogenesis because there is no silt in clay in the original framework dominated scoria. And you can see again that the diagram would indicate that most of the soil development is concentrated, at least by virtue of looking at silt and clay in the upper two decimeters of the soil profile. Again, this would suggest pedogenesis limited to primarily accumulation of silt, clay and other plasma in these upper two most horizons. Minimal amounts of silt and clay have accumulated in the lower horizons, but importantly a great deal of silt and clay have been translocated over a depth that probably exceeds two meters. All we could get down was one meter because of safety precautions owing to quality assurance regulations.

Here are some more depth functions for the same soil profile. On the left-hand side what you can see is that the coarse fraction or the gravel content is increasing with depth as you might expect. No big deal. The more and more fines you accumulate the less and less gravel contents you have. What is critical, is that, what I shall point out later, you are not just accumulating fines in primary pore space, you are actually getting what we call dilatant soil development. That is critical to recognizing soil development in arid climates.

On the right-hand side you see a calcium carbonate content. Calcium carbonate accumulates in soils as CO_2 dissolved in soil moisture produces carbonic acid which dissolves calcite. So calcite movement in soils is by solution transport, rather than colloid transport which characterizes clay movement.

On the right-hand side, what you can see is that most of the calcium carbonate is accumulated in the upper two decimeters of the soil. The reason it hasn't been leached deeper is because in this dry climate with limited moisture and with limited CO_2 production by the very sparse vegetation, calcium carbonate cannot be transported deeper than, and with the increasing available water holding capacity of the upper two horizons and the accordingly lower permeability of these horizons, that is where most of the calcium carbonate accumulates, much of it in the matrix as disseminated carbonate. Although, coatings of calcium carbonate also occur around some of the coarser class preserved in these horizons. Again, in the lower part of the solum, the calcium carbonate is accumulated primarily as thin coatings on the basis of some of the class.

In contrast to calcium carbonate, salts and gypsum owing to their much greater solubility can be transported to much greater depths. The significance of looking at electroconductivity which is a proxy for soluble salts and gypsum is that here is where you see that soil development extends to much greater depths than 20 centimeters. In fact it extends to depths that clearly exceed one meter.

Also, it is important to point out that it would be impossible to accumulate gypsum of other sulfates or chlorides in these soils by alteration of scoria. Clearly, these components in addition to the quartz and feldspar materials in the clay/silt fraction must be from dust. And this emphasizes one of the most critical findings concerning research in desert soils is that soil development is primarily characterized by an accumulation of eolian materials and the development of horizons from those materials. It is not characterized primarily by the alteration of scoria as might be the case in much wetter climates such as Hawaii.

So, on the left-hand side, I would like to very quickly summarize the primary processes which we believe influence soil development in scoria.

Number one, we entrap calcareous, salt-bearing eolian dust.

Secondly, infiltrating soil water carries or translocates its materials to depths in the soil depending on whether the transport is colloidal, mechanical infiltration or solution transport. And, also, of course, this material accumulates to form the matrix in both the vesicular A and subjacent B horizon.

Soil development directly associated with scoria framework grains includes the limited chemical alternation to secondary iron oxides and other materials we see in the bottoms of fragments and the preferential accumulation of salts and carbonates on the bottoms. The iron oxides on the top; the salts on the bottom of the class.

And finally, the increasing clay content favors dilatant, cumulic soil development above the framework-supported scoria parent material. Again, this is critical to recognize that this is in fact going on in soils because it explains how stone pavements remain at the surface for literally hundreds of thousands of years.

Now, I would like to talk about some of the complications that we have encountered in looking at how scoria is modified in the near surface environment. Because, it is critical to recognize these complications when one is trying to use soil development as a basis for looking at volcanic history and estimating age from soils and geomorphic evidence.

On the left-hand side, what you see is a photo of a soil developed in the scoria exposed north of the cinder cone, the Lathrop Wells cinder cone. What you see is a typical Av, thin Av and very thin Bw horizon, but here you see a feature which is redder and includes matrix that extends to a depth much larger than the rest of the soil horizons which parallel the surface.

This is what is called a krotovina. That is a burrow, probably made by some small mammal that has become subsequently infilled with matrix. I would argue that its increased reddening is due to the fact that given the higher available water holding capacity of the soil and its accordingly higher matrix potential that water tends to move down along the matrix filled krotovina more slowly and has more time to alter that. In addition, water moving down the sides of this probably moves into that by virtue of much greater matrix potential. These kinds of features must be characterized in order to effectively understand soil development in these areas, because, one of the things we have observed in scoria is that it is highly bioturbated in many areas complicating the description of soils in these types of deposits.

On the right-hand side, you see the scoria fall at the Black Tank Cone in the Cima Volcanic Field. You see a similar type of soil here. What you see here is that this particular blanket has been dissected as result of incision of the wash that serves as a local base level for the scoria blanket. As that wash is cut down small ephemeral streams have dissected the scoria. This shows the ease or erodability of a scoria that we believe is probably less than 20,000 years old. But, in addition, we see something else that is very important. We see that although you would expect the scoria to be highly permeable to produce little runoff, that the accumulation of soils and the development of this plasma filled framework, that this is probably what allows the system to produce runoff. So, these kinds of features are also important in looking at soil land form relationships in volcanic areas characterized by scoria falls.

Another interesting complication, at least it is interesting to me, that involves alteration or modification of scoria deposits in the near surface environment is this feature. In our initial studies, we had thought that perhaps that these kinds of features when we only had this much material exposed, when we had no access to trenching, we thought perhaps that these could be buried soils.

However, as we entrenched these exposing the entire deposit, what we see is that these things are clearly different from the overlying features which are unequivocally soils. In fact what these are is we have the wash to the right-hand side of this trench exposure and as the wash becomes stabilized at given base level, infiltrating water during runoff events carries silts and some of this stuff laterally infiltrates into this single scoria package. And with time, what happens over long enough periods of time, that is, the lateral infiltration this stuff creates materials which look a lot like soils. But now in describing these in detail we can clearly characterize the difference between these types of things which conform by virtue of the very porosity of the deposits as well as the abundance, almost the ubiquity of silt in the region and from true soils.

Now, I would like to comment on some recent criticisms of some of our work in soils that we feel have formed in scoria, but according to other observers have formed in what they feel are not scoria, but are perhaps some type of apron deposit.

In this particular diagram, it was argued that this represents the characteristic grain size frequency for true scoria with the solid lines and dots. Whereas this data, which comes from the basal parts and I am not sure which basal parts, but at least the basal parts of the soils that Steve and I described at Lathrop Wells.

The argument was made that most of the material here and on up consists of quartzo feldspathic, fine-grained matrix and accordingly it was argued that that material could only have been transported from an overlying sand blanket by our method. Instead, they argued that it couldn't have been a pedogenic phenomenon, but would have to represent the original parent material of some kind of cone apron deposit. And it was argued that we did not present this material which according to these authors would indicate that this was non-pedogenic but instead an artifact or attribute of the original cone apron environment. That is, an original quartzo feldspathic matrix.

I have plotted here data from three soil horizons that have formed in what I believe anybody would regard clearly as a primary scoria. And what you can see here is that our parent material, almost precisely overlaps the data from the scoria determined by or analyzed by Turrin and Champion. But if you look at the lower horizons the AB horizon and the vesicular A horizon, what you see is, the data for those horizons totally envelops the data for the deposit that they argued could not be pedogenic. I would argue that this unequivocally shows that they can be pedogenic and that there are some really good reasons for why they are pedogenic and not part of the primary depositional environment.

Number one, I just concluded that I don't think if you looked at my data for the soils on scoria, that you could use the data published by Brent and Duane to preclude their being pedogenic.

Second, I believe the pedogenic origin of Lathrop Wells Quarry units can be shown by the kinds of things we have recognized in soils developed in volcanic land forms both in the Lathrop Wells area and the Cima area. The presence of systematically spatially oriented, pedogenically accumulated coatings on scoria; depth functions of less than two millimeter materials; and, the presence of the vesicular A horizons above the Bwk or Bk horizons exposed in the Lathrop Wells Quarry.

Three, I would argue that the question of the large quartzo feldspathic component can be explained as being an result of cumulic, dilatant soil development which enables continued accumulation of a fine-grain matrix that can ultimately greatly exceed the depositional primary porosity.

Second, I would argue that the stratigraphic character of the deposits in the quarry which are only a few decimeters thick, and are also bounded by vesicular A horizons, precludes accumulation of translocated fines over large depths, but instead favors accumulation of a large degree of matrix in the basal part of the units.

Finally, I would argue that appropriate consideration was not taken by Turrin and Champion regarding volume-weight percent-bulk density relations and particle size data.

DR. ALLEN: Les, you are running out of time here.

DR. MCFADDEN: I'm sorry.

DR. ALLEN: In concluding, can you sort of explain to us why this makes any difference in terms of site suitability?

DR. MCFADDEN: Well, my work is soils and geomorphic studies, but perhaps I can get into that.

These are the types of data that you can collect from--I had half an hour, have I gone way over? I didn't think I had.

DR. ALLEN: No, you have already gone over half an hour.

DR. MCFADDEN: Okay, sorry. I am in the last few slides, Clarence.

What you see here is the types of ages that we believe you can collect from soils data based on the table published by Coleman, et al. And, the argument is that you can collect primarily relative age data, but if you have the appropriate types of calibration, that you can collect calibrated data.

One point that I would like to make clear is that when we look at holocenes, when we look at soils that form in the Holocene we are calibrating using radiocarbon ages. Okay, we are not calibrating using potassium argon ages. And it has been asserted that we have mis-calibrated these soils because we have based it all along a potassium argon age. But, that is false for looking at the younger soils. So, our estimates of the age of the soils at Lathrop Wells are based where we have calibrated ages from radiocarbon dates in the Mohave Desert for soils, not in potassium argon ages.

For example, what you see here are two soils, one developed in less than 10,000 year old materials for which we have numerous radiocarbon ages. And on the right on a fan deposit of Holocene age for which we have radiocarbon data that it exhibits characteristics demonstrative of Holocene pedogenesis which we feel are similar to those soils at Lathrop Wells. And that is what is critical. We say they are Holocene; we say this is young. And, they say it is 140,000 years old.

That is what a soil looks like that is developed in deposits that are over 100,000 years old. You have a very strongly developed reddened horizon enriched in clay that exists over thicknesses much greater than 50 centimeters to a meter. So, the essence of our argument is, is that if those deposits and land forms were that old, we would expect to see soils that look like that.

Finally, these are the types of studies which we hope to pursue in the future to more accurately characterize soil development in scoria. One of the most exciting things we are trying to do is direct numerical aged dating from soils by radiocarbon dating calcite that is formed pedogenically in these soils. We have some preliminary exciting new dates for soils in the Silver Lake area, and we are now going to try that in the Lathrop Wells area.

That's my talk.

DR. ALLEN: Thank you, Les.

We are running short of time, but do members of the Board or consultants have any quick questions?

(No audible response.)

DR. ALLEN: Any other questions or comments.

I don't mean to press you on this issue of what does it all mean, but I hate to go through a day and half and only at the very end do we get to any discussion of whether or not this has any impact on the evaluation of the site. I assume it does.

DR. MELSON: Let me comment on that. I think what Les is doing is looking at a resolution of a very real time dependant phenomena, that of soil formation.

And, it is resolving things at a level where isotopically we are having lots of trouble. So, I think instead of contradicting each other, the soil formation study that Les is doing so very carefully, are complimenting and are going to be integratable with the isotopic methods eventually.

DR. ALLEN: I don't question that. I guess my concern is how much difference does it make in terms of site suitability?

Bruce you have time for a quick comment.

DR. CROWE: Let me make it as quick as I can.

DR. ALLEN: You are going to be talking on this issue tomorrow.

DR. CROWE: I will, but the basic issue comes down to if these are simple monogenetic centers as a number of people have asserted, then the hazard is greatly simplified. If these have multiple events, we have to look at those multiple event processes in terms of what would happen to repository, would not only be penetrated once, but it probably would be penetrated twice. And so the hazard becomes greater when you have multiple events. So the real issue here is the question of is there single or multiple events, and then what is the age of the events. But, probably the greatest impact on hazard is the single versus multiple events.

DR. ALLEN: Thank you.

Jeanne.

DR. COOPER: The next speaker is Dr. Stephen Wells to talk about Part II of Soils and Geomorphic studies.

DR. WELLS: I can follow up a little bit with that because the geomorphology is very much linked to the soil development. And the uses of geomorphology in soils are fundamental for one, defining volcanic stratigraphic units which then plays right back to what Bruce Crowe was saying whether we have multiple units erupted from a single vent, which then goes back into the hazard situation.

So, what I want to do is talk about geomorphic processes that are operative on the hill slopes, the scoria slopes themselves, talking about a regional survey of what we see going on, putting that into a review of the criteria for distinguishing between sedimentologic and volcanic deposits, because there has been a question raised about our ability to recognize things that are due mass wasting and erosion of hill slopes versus those primarily due to volcanic processes. And then run through a sequence of geomorphic processes operating on cone slopes so that we can use that to say something about the relative ages of these cones and the volcanic units associated with them. This is based primarily on work that was established by John Dohrenwend, myself and Brent Turrin, as well as other people in the mid-80s.

And, then I will turn and use the results of Black Tank volcanic center in the Cima Field as an analog for the Lathrop Wells where we are going to present new mapping and trenching results; some age estimates for those units; and, what this means for polycyclic volcanism. Because that has been raised as a question, whether that is a valid model for these volcanic centers. And then, conclude with the significance of comparing Lathrop Wells with Black Tank and hopefully that will come back in to the significance.

As we pointed out early, a lot of this is being done in response to helping us understand what kinds of features we see along these flanks, and whether, maybe this will help Clarence, we are looking at one sequence of volcanic units with mass wasting interbedded, or whether we are looking at one eruptive sequence followed by another one with a hiatus represented by a soil bounded unconformity. So we are trying to use the soils and geomorphology together. And again, in the Turrin, et al., Science article, that quarry section has been questioned in terms of the types of processes that we see operative there.

And so, a survey of several volcanic fields was done on air photos as well as visiting them. And these are the primary type of geomorphic features that we see. Garlands, which are shown here, perhaps faintly on this diagram, which are these types of features that are produced according to McGetchin et al., 1974, as gravity driven materials during the very last stages of the eruption where the scoria comes to the angle of repose. So, that is one of the primary feature that we see.

We see agglutinate mounds and proto-agglutinate mounds, that is concentration of bombs. And then we see fairly pristine slopes, such as the one here on Lathrop Wells that have angles greater typically than 27 degrees.

What do we see in terms of the features that modify this? We see debris flow channels and deposits filling those, such as the one over here on the right-hand side. This is the debris flow here and there is the rill behind it. We see debris flow and channel filled rills and gullies such as the one shown over here. Again, a secondary erosional feature. And, these rills can be really highlighted here. This is the El Nino year of 1983 with the vegetation highlighting the rills at the base of the Black Tank or A Cone in Cima Volcanic Field.

And all these processes, erosional that we see, leads to the production of a cone apron, which is illustrated on this 500,000 year-old flow, according to potassium argon date by Brent Turrin. We have these large cone shaped materials surrounding the flanks of these due to the erosion of this. So, those are the types of features that we see.

What we don't see, and this is significant, is that we don't see large scale rotational slumps, block slides, block glides, earthflows, debris avalanches or debris slides that are typically with andesitic volcanism that you might see in an arc. These type of sector collapse features are just not very common in terms of the morphologic expression in these volcanoes.

And, significantly, we can measure these. These are not things that we just think we see. We can go out and measure these in terms of the area of the apron, fluvial incision, garland or pristine slopes and agglutinate areas. The significance is, is if you go to a single cone, such as the one here Black Tank, or the U Volcanic Center in the Cima Field, then side by side, presumably from the same source have significantly differences in apron area, smaller and larger, amount of incision and garland area. And we think this is good evidence, geomorphic evidence there is polycyclic volcanism; that these aren't simply volcanic eruptions at all these cones, not necessarily monogenetic. We can't presume that model; we need to test it.

So what do we do when we are out here looking at stratigraphic sections such as the ones shown on the right-hand side here in our ability to determine what kinds of processes produce these stratigraphic sequences.

Well, essentially we look at three major things: clast characteristics; texture stratification; and depositional morphology. And these are the criteria and I am not going to go through them in detail, but these are the criteria we look for when we are trying to separate things such as volcanic tephra flow/fall versus a viscous slurry or debris flow. And some pictures of those just to give you an idea that they are recognizable we will show over here.

One of the things that we look for is the relationship between bedding and bounding surfaces in here whether they are parallel. We also have to be able to recognize as Les very nicely pointed out the pedogenic overprinting that goes on in these deposits, such as illustrated by the soil development at the top of this unit. We have to be able to separate that. That may not show up but what you would see here is an open framed scoria unit from the Black Tank Cone. Here you can see parallel bedding to the cone slope, and that those things are clearly recognizable from things such as this which is an apron of debris flow material off of one of the cones, which is also distinguishable from this section, and this is fluvial cone apron deposit above tephra here and this is all re-worked material. So we use that criteria that I just presented here to separate these types of features.

Let's put a temporal framework on this to see how things change over time since we are trying to therefore understand the amount of time between volcanic units and their stratigraphy. And, the work that John Dohrenwend, Brent Turrin and I did in the mid-60s produced this type of sequence using potassium argon dates that are published in Isochron West, Brent's work, where we looked at a variety of cones that have a resemblance to eggs in this picture. But essentially what we are looking at is the crater, the constructional cone slope and the apron. And you can see how they change, and let's not worry about the exact time or if whether we are using raw data or weighted means, but how they change as you go back in time from there to here with the development of rills, the integration of those rills and the incision of this material and eventually the removal of the apron at the base as this material cuts down through it. That is the temporal sequence.

This sequence is described over here where we look at cessation of the eruption, the development of something like the angle or repose like 30 to 30 degrees. Then we start trapping the fines. I'll come back to the significance to this in driving the geomorphology and evolution of these cones. We start developing small discontinuance debris flows and rills and this primarily happens in response to runoff where we have decreasing permeability because of soil development, or we have agglutinate or proto-agglutinate showing up.

Then, the debris flows continue to develop. We get major incision and the development of the apron. And eventually the apron becomes dissected and we have multiple apron and eventually excavation of that apron. This is all based on the assumption that John and Brent and I used, is that we could use substitution of space for time; cones of different age and different places to model temporal changes. And those diagrams are illustrated here where you can see, perhaps faintly, the development of rills on the Black Tank Cone that you can see here, the breached cone evolving. And then on the G cone which has a couple of ages on it, the development of these aprons, the rills and then you can see the complicated dissection of that process.

What is important with this is that we can use these features to say something about the age of the deposits even in stratigraphic context, not only morphologically. And, so to touch up on this before I move into the Cima data, the significance of Les's work is that to get runoff developed on these slopes, to modify them by mass wasting and fluvial processes, we have to develop a plasma or a soil development. And we can test that hypothesis by looking at some work that Aaron Yair from Hebrew University and I have done, where Aaron has done rainfall simulations on hill slopes in the Sde Boker area of Israel looking at bedrock areas versus colluvial areas and the response, i.e., permeable versus non-permeable units. Rocky non-permeable units produce a tremendous amount of runoff as you would expect. This isn't an intellectual major exercise, but it is an interesting documentation. The less permeable units soak it up.

The significance of this is is to get runoff on volcanic cones, you must put in the soil plasma, reduce the permeability and start generating runoff or have agglutinate or proto-agglutinate in that area. So, that is the way that we apply it to the volcanic units. So, the soil and geomorphology work together in concert to modify these land forms.

What I would like to do now is switch to the Cima Volcanic Field, whereas we did in the geology paper in 1990, we used the Black Tank or A Cone located right here, and illustrated right here as an analog for our work at the Lathrop Wells. We have done considerable trenching in this area to understand whether we have age differences between primary scoria cone slopes and features such as this, which show more rilling and modification than these do. And the results of that, of our work, comes from as Bruce Crowe pointed out earlier, nine backhoe trenches. And what we find is that there are three vents here, all side-by-side, they have their associated flows, tephras and apron deposits. We have stratigraphic evidence that they are there and we can see that there are hiatuses between these units and I'll give you some of the data on that in a minute here.

In addition, working with the geochronologists, we have some age estimates for these units. Our youngest one is the Volcanic Center III, Lava Flow A, there are two lava flows associated with it; a TL date by Steve Forman of about 8,500 to 9,000 years and then a stone from Hal Stone's work when he was doing past work there produced a date of around 12,000 of cosmogenic helium.

That separated from an underlying unit, as I will show in a minute by a bounding unconformity of buried pavement and of buried soil. And that unit has a date a cosmogenic helium date, age estimate of about 22,000 by Chad Olinger working with Jane Poths as Las Alamos.

That in turn, and Les showed a picture of this earlier separated by a bounding unconformity, a buried stone pavement and buried soil, which then is our oldest one which is deeply eroded and we are waiting for dates on that now.

The map of this, these units look as follows: Here is our vent with the breach. This is this cone and flow shown here. We have a second vent here which I believe this is the one that Duane and Brent sampled. We went up on this vent here and then we have been down here and looked at this truncated and buried vent here which is really the erosionally modified oldest unit Qv1.

Here are our trench locations. And just to show you what one of those looks like, this is a trench log at the base of the cone and almost exactly right here to be specific. And what we see is a buried stone pavement of our volcanic unit II with the scoria and a little bit of apron deposit of unit C here sitting on top of that. And that projects right underneath the main cone. And so we believe that we can now trace, and we have put other trenches, we can trace this around laterally to show that Black Tank, as well as Lathrop Wells has polycyclic volcanism and therefore significant into the volcanic history.

And just to briefly show you, these are some of the morphological differences in the slopes. This is our youngest one right here, which is this slope here. This is our second volcanic unit, which is primarily local scoria mounds and tephra falls and this is the apron on a lava flow of the oldest unit. So we can see morphological distinctions between those.

Finally, what does this mean so that we can bring this back into perspective the Black Tank with Lathrop Wells is that both centers display geomorphic processes which are consistent with what we have seen in our regional survey. The slopes do not show any aberrations; they are similar. Both volcanic centers display unconformities or buried soils and stone pavements, indicating a complex volcanic history. We believe that these soil pavements, soil and stone pavements represent hiatuses between eruptions. That is a significant part of our interpretation so that there may be polycyclic as well as monogenetic volcanism.

At the Black Tank area the experimental numerical ages the TL and cosmogenic ^{3}He are relatively compatible but different significantly from ^{40}Ar/^{39}Ar and K-Ar results which Brent and Duane have published about 100,000 years on that, I think are their estimates.

So, on conclusion, Hillslope processes are dominated by debris flow and stream flow modification, not by large scale mass wasting processes such as andesitic volcanism. Field criteria for distinguishing these features was applied to the Lathrop Wells center that you see over here, and we believe that this quarry site, represents volcanic units bounded by soils and not mass wasting deposits. We stand by our interpretation and believe that the problem with their interpretation is because they failed to incorporate an understanding of soil stratigraphic pedogenic features, standard particle size analysis and understanding of the geomorphic and sedimentologic processes on the vents and they lack any geochemical testing of those units.

And finally, we believe that a geomorphic response model for the evolution of these slopes is dependent upon the formation of soil to drive runoff and cause this modification and give you the matrix for debris flows. And that the recognition of soil and stone pavement bounded unconformities indicate that there is polycyclic volcanism or that that is a fair interpretation that many of these previously considered monogenetic centers.

Thank you.

DR. ALLEN: Thank you, Steve.

Are there questions from the Board or staff?

(No audible response.)

DR. ALLEN: Duane, do you want to make any response now or wait until you talk tomorrow?

DR. CHAMPION: I guess I just have one quick question for you, Steve. You have indicated that you feel that our size analysis and Les made comments in a similar vein is unsound. The indicated position in the DOE program at the present time is no such activities were intended to be carried forward, that they weren't thought to be viable procedures. Are you going to now do size analysis of these tephra deposits?

DR. WELLS: Well, size analysis is being done with respect to the soils. But, we don't think and if you look at the criteria, that grain size and if most textbooks and experts would say that grain size analysis is not a viable or a major technique for distinguishing different types of units. I think most people would agree with that. You may want to address that, Bruce, but we don't think that granumetric analysis is really that significant. If you separate the pedogenic from the primary constructional features such as cleaner bedding, flattened bombs, all those things that were listed up here.

DR. CHAMPION: In my reading of a recent bulletin of volcanology articles by Schmenke and Bruce Houghton and such, size analysis is a critical portion of any work on fragmental volcanic deposits. I was surprised that it has not been part of the DOE program.

DR. WELLS: As I understand you are talking about the distinguishing of the different types of sedimentary versus volcanic units and I stand by what I say. I don't think it is that important for distinguishing those. I think there is enough field evidence which we will get to look at Wednesday that you don't need to go to that.

DR. CHAMPION: To even characterize the deposits.

DR. WELLS: Pardon?

DR. CHAMPION: To even characterize the deposits.

DR. WELLS: Oh, yeah, perhaps if you want to characterize that. Yeah. But not for separating whether it is debris flow, rotational slide versus volcanic.

DR. ALLEN: Other comments or questions?

Bill Melson.

DR. MELSON: Steve, it almost sounds like you backed down a little bit from the clear cut evidence that you have of polycyclic volcanism. I mean, you are actually saying that the evidence looks like or is consistent with. And to me it is pretty clear you had polycyclic volcanism. I mean your evidence is almost irrefutable. And how that reconciles with the potassium argon dates may yet to be resolved. But, I think you made a very strong and clear case that through a number of years your team has made that case. I see no reason based on what I have seen to back off of this at this point.

DR. WELLS: I didn't mean to give the evidence that I was backing off. If anything I would venture to say that we have got more data now, actual numerical age estimates as well as documentation at other sites. So, I may have been a little conservative in the word use but I still feel very strongly that we are seeing these features. And you know, there is evidence at many other places we have looked at other centers and I just don't have that quantified like we do here.

But, what I was trying to do was to show you the kinds of evidence that we have built at the Cima Field over the past year to even document that more.

So, no, I don't mean to suggest that I am backing down and I think you'll see in the next presentation even more evidence to support polycyclic volcanism.

DR. ALLEN: Further comments or questions?

(No audible response.)

DR. ALLEN: Okay. Thank you, Steve.

Jeanne.

DR. COOPER: Our last talk this afternoon will be Dr. Frank Perry. He will be speaking about petrology studies. Frank is from the University of New Mexico.

DR. PERRY: The geochemical evidence that I am going to present today it also largely addresses this question of a monogenetic model of volcanism versus a polycyclic model. And my conclusion will be that the geochemical data is inconsistent with a monogenetic model but is entirely consistent with this polycyclic model that has been presented by Steve and Les in the last couple of talks.

The geochemistry studies address there areas. One, petrogenetic models. Now we are mostly concerned with monogenetic versus polycyclic volcanism. Constraints on physical models. I presented some of this at the last TRB meeting dealing with the issue of waning volcanism. I won't touch on that much today, except a little bit on some evidence that Lathrop Wells itself, the magmatic activity appears to be waning through time.

Then third, we found recently that the geochemistry is becoming very useful for constraining the stratigraphy and putting together the overall evolution of the volcanic field; how it developed through time with different eruptive units.

The monogenetic model of volcanism is that eruptions is--there is either a single eruption or eruptions closely spaced through time and most usually in a period of several years. An important part of that is that it generally involves a single batch of magma. Magma is generated in the mantle, it extends through the crust and is erupted.

Now there can be variations in chemistry at monogenetic centers, but the chemical changes can be understood in terms of a single magma batch evolving. So those are predictable and we understand those.

The polycyclic volcanic model, as we understand it now involves multiple eruption phases, separated by thousands or even tens of thousands of years. Because of this it would require a multiple magma batches. Because these small volume magma batches have a limited time in the lithosphere. And so for the volcano to be active for thousands or tens of thousands of years, you would have to have more than one magma batch. So, the geochemical data can distinguish between a single and multiple magma batch. That is what I will show you today.

DR. ALLEN: Let me ask you a question, surely out of ignorance. On a volcano like St. Helens that has been active many times in the last few thousand years, are you saying that because of the extent of that period of time, it must have several different magma sources?

DR. PERRY: Yes. And that is a polygenetic volcano active for hundreds of thousand of years. So, it requires multiple batches of magma.

The accepted interpretation of these very small volume basalt center is that it is only one batch of magma and the eruption is very limited in time.

DR. ALLEN: Does St. Helens does have a petrologic characteristics independent of the timing that would suggest polycyclic behavior.

DR. PERRY: Yes.

DR. ALLEN: I see. Thank you.

DR. PERRY: I am just showing how we use the geochemical data to distinguish between single or multiple batches. This is an example of variations within a single lava flow on the Taos Plateau in New Mexico, data from Laura Crossey in a master's thesis.

These are two incompatible elements; samarium and thorium. They are excluded from the crystallizing phases, so as a basalt evolves, you would expect these to increase in the residual liquid. So these variations in this flow are caused by differential crystal sorting within the flow, and you see that they form a positive slope. As samarium increases thorium also increases because they are both excluded from the crystals as the flow crystallizes.

Also note that I have one sample indicated, 44-14 circled. That would be the most evolved residual liquid on this plot because it has the highest samarium and thorium. So to be consistent with a single batch magma on any other variation diagram using different elements, that particular sample should also occupy the most evolved position. That is what is shown on this next plot.

This is a compatible element, cobalt which goes into olivine as olivine crystallizes, versus lanthanum, another incompatible element. So here you get a negative slope. As cobalt decreases during fractionation of olivine, lanthanum is excluded and increases. And you see the same sample in the corner there occupies the same relative position. So these variations are entirely consistent with a single batch of magma, which is what you would expect from at a monogenetic center.

And what I will show you is that both Black Cone and Lathrop Wells violate these relationships and I interpret that as indicating more than one batch of magma.

Now if you extend this to an entire volcanic field, this is data that was just published from the Portrillo field. Again these are two incompatible elements, niobium and zirconium. These are six different vents within the Potrillo Field separated by several kilometers. What you see again at each individual center, you see positive slopes of niobium and zirconium. The only way to get negative slopes on this type of diagram is if you were comparing two different centers. In this case, say Aden Crater and the Potrillo Mar. These are related by negative slope and you would interpret that as being two different magma batches. That is the only way they can be related by a negative slope. And that would be the correct interpretation because these two vents are separated by about 30 kilometers. So, clearly they are two batches. But this is the same relationship we see at a single center at Black Cone and Lathrop Wells. So, we are seeing at a single center what you see, you know, between different vents at a monogenetic volcanic field.

Okay, this is quarry sample at Black Cone. There are two major flows, the northern flow, which is the youngest and the southern flow which is older. There is a third flow which we haven't sampled yet, but based on the geomorphology, it is probably the oldest flow. So, we have samples from an older flow and a younger flow and also from the lava lake from the summit.

What we see now in terms of chemical variation-- okay, I put this up on the left to show again what you expect in a single magma for two incompatible elements. These are two different incompatible elements for Black Cone. And you see just the opposite relationship. You see a negative slope relating the northern flow and the lava lake to the southern flows. So, there is no way that this can represent a single magma. These variations require two separate magma batches. I have shown a vector on the right of what you would expect for fractionation of one magma.

The same, here a plot of a compatible element versus an incompatible. The same element is on the right for Black Cone. Again you see the two flows can be distinguished, the northern flow, the lava lake, and the southern flows. Here they have a positive slope that is again inconsistent with a single magma and requires two separate magmas. And the vector again shows what you would expect with one magma.

So the conclusion at Black Cone is that at least two separate magma batches were involved.

Now Lathrop Wells, this is the old map. It's been slightly revised. But, what we see from the chemistry is that there is a minimum of four or five separate magma batches. Each major flow $Q1_6$, Ql_5, Ql_3, and Ql_4, have a distinct chemistry as well as some of the units within the quarry that are exposed. And the relationships and the chemistry between these rule our derivation from a single magma.

In all of the slides that follow, I am going to talk about Ql_6 quite a bit. We think this is the oldest, the first flow out of this center and subsequent flows flowed around the Ql_6 flow. We found the vent for Ql_6 is directly under the most recent cone. I'll show the evidence for that.

First of all, this is the sample site. We now have over 100 samples so we are pretty confident and we understand the variations among different eruptive units. We have very little major element data at this point because of QA restrictions, so most of the data is traced element isotopic data, which I'll present. I'll show a little bit of data based on major elements from some very early analyses that we did.

The evidence for multiple magma batches--

DR. ALLEN: Excuse me. Hold it, what do you mean by QA restrictions?

DR. PERRY: Well, the QA software program like running XRF analyses hasn't been approved yet, so we can't get XRF.

DR. ALLEN: I see, not a field problem, it is a software problem?

DR. PERRY: Yeah. In the software, the runs, the XRF has not been approved, so we have not be able to use it.

Evidence for multiple magma batches at Lathrop Wells includes petrography. There is distinct phenocryst assemblages in different flows which are difficult to reconcile with one magma batch. And, there are geochemical differences, which also preclude a single magma batch.

I'll so the petrography evidence first. This is a bomb from the main cinder cone and it is typical of most of the flows because the only phenocryst phase is olivine. Plagioclase occurs only as a micro-phenocryst.

This is from the largest flow, the Ql_3 flow. You see the same thing. Olivine is the only phenocryst. This is much better crystallized now, so it has a well crystallized ground mass instead of glass, but again plagioclase occurs only as a micro-phenocryst.

In Ql_6, the oldest flow, you see a very different phenocryst assemblage. Olivine again, but not plagioclase is also a very significant phenocryst phase. It is difficult to imagine how you can make a major change in phenocryst assemblage without major changes in the chemistry of the magma. That is what the next slide points out.

Where it has been documented that there are changes within a monogenetic sequence of phenocryst assemblages, there are also significant changes in chemistry. An example is Cerro Negro in Nicaragua which has erupted over about a 150 years. There are chemical differences and phenocryst differences in the successive lava flows, but all of them can be modeled back to one single magma. So, it is considered monogenetic up in this range as olivine and pyroxene fractionate from a magma. The Mg number decreases regularly. So, these are, for Lathrop Wells, these are down here. They are fairly evolved basalt magma. And the important thing is that they cluster. That part of the histogram represents all of the major flow units Ql_6 through Ql_4. This is a unit in the quarry.

The important thing is that these cluster at one point. It says that all the magmas are evolved about the same amount. If it was a single magma, then you wouldn't really expect any trace element difference between the different magmas. But it also suggests that there is a strong physical process that is controlling the ascendance of these magmas. As Mg number decreases, density also decreases. So it would suggest that density may be controlling when these things erupt or are able to ascend.

The important thing is this limits how much trace element variation there can be between the different lava flows.

Now this is a pretty complicated plot. I showed a variation of this at the last TRB. The top is lanthanum/samarium versus lanthanum. The importance of that is that you don't expect much change in lanthanum/samarium ratios as the basalt evolves because they're both incompatible. Lanthanum a little bit more than samarium. The only thing that can significantly change them during basalt fractionation is significant fractionation of clinopyroxene. And on both figures the vectors show what type of chemical changes you would expect for 3 percent fractionation for clinopyroxene, olivine and plagioclase. So pyroxene can change the lanthanum/samarium ratio.

The top figure requires at least three separate magmas. The quarry units have high lanthanum/samarium at low lanthanum content. And you can't increase that lanthanum/samarium ratio without increasing the lanthanum content. So, having the same lanthanum content at higher lanthanum/samarium, means it can't be related to the other magmas.

Also in this diagram, if pyroxene fractionation is causing the change in lanthanum/samarium, then Ql_6 circled in red would occupy the least evolved position. It would be the parental magma. But on this diagram below the same units plotted Ql_6 occupies the most evolved position if clinopyroxene is the major fractionating phase. So, on two different plots, I showed earlier that the same samples have to occupy the same relative positions. Ql_6 does not occupy the same relative position. So, again it precludes derivation from the same magma.

On the top plot, Ql_6, the quarry units and all the units up to the right that would represent three different magmas on this plot.

Another point of this, you can increase the lanthanum/samarium ratio by partial melting. And this would be consistent. The Ql_6 unit would represent--okay, lanthanum/samarium increases as you decrease the amount of partial melting. So on this plot Ql_6 would represent a larger degree of partial melting. The Ql_5 units in the middle would represent slightly smaller degrees of partial melting in the mantle and the Ql_3 and the Ql_4 units would occupy the smallest degree of partial melting. So this is consistent with a decrease in the amount of partial melting through time as you go through the oldest lavas to the youngest. And that is some evidence that the Lathrop Wells system is waning, going to decreased amounts of partial melting.

An analogous plot is Mike Murrell has done some mass spec measurements of thorium and uranium of concentration. And this is basically the same plot as lanthanum/samarium versus lanthanum, but this is thorium/uranium versus thorium. And again it shows the same relationships that the oldest, as you go from the oldest to the youngest lava flows you increase the thorium/uranium ratio and that is consistent with separate magma batches that are related by smaller and smaller degrees of partial melting through time as the Lathrop Wells center formed.

Now, again we go back to two incompatible elements which should produce a positive slope. I have shown up here the vectors for pyroxene, olivine, plag fractionation and I won't dwell on this, but again you get a negative slope that relates to different units. This is the highest precision data we have, isotope dilution. We have done two samples from each unit and you get clustering Ql_6, Ql_5, Ql_4, Ql_3, cone unit and the quarry. So, all the units that we identify from field studies are geochemically distinct and they are not related in a way that can be explained as fractionation of one magma batch.

This, we have played a little bit with this melting model on this diagram, the same as the last one. I have modeled what type of variation you would get with an incremental batch melting model, where basically you melt--take a two percent melt of the mantle, take that melt away, erupt it and then you have depleted the source of incompatible elements and then you melt that source again, take that away and melt the source again.

As I have shown in the open circle, three increments. And you can get a negative slope using that model which matches, you know qualitatively what you see at Lathrop. But to do that it requires rubidium to be a compatible phase in the mantle. And the only way you can do that is with really excessive amounts of phlogopite as a residual phase. And right now, basically we don't think that is a viable model. We are just playing with these different models to see what works and what is reasonable.

What it points to, possibly is that the separate magma batches at Lathrop Wells, don't represent the evolution of a single source that is related through time, but may instead represent separate sources that are not in communication. It's those sources that have slightly different rubidium neodymium ratios in this case. And that fits a physical model of really the type of mantle we are dealing with which from isotopic data is a lithospheric mantle that is fairly cold and is not vigorous and does not have large amounts of partial melt.

We have got some new isotopic data. It doesn't say a whole lot at this point, except that again Ql_6 seems to be distinct from the rest of the eruptive units and possibly the quarry unit also. So these different eruptive units also have distinct isotopic compositions, which is consistent with several magma batches.

I'll show a little bit how we use the chemistry to constrain some of the stratigraphic relationships. This is a shot of the main quarry. This feature here was exposed sometime in the last couple of three years. It has the morphology of one of the low angle scoria mounds that we see around the main cinder cone. So, we are curious about what this was. So, I analyzed a bomb from that and it turns out that it is almost certainly the vent for the Ql_6 lavas. So the oldest lava flow, the vent for that is directly underneath the cinder cone, which we think is one of the youngest features.

This again a plot of lanthanum/samarium versus lanthanum, just to compare that buried vent to the main cinder cone. This is the field for the main cone, about 15 samples of scoria and bombs. This is the chemistry of the Ql_6 lava and this is the composition of the bomb in that buried vent. So, it matches the Ql_6 and is different from the rest of the cinder cone.

Scandium versus strontium, again the field for the main cinder cone and the field for Ql_6 showing that that buried vent has the same composition of the Ql_6 lavas and does not match the composition of the cone.

And finally, this is a photomicrograph of the bomb from that vent, and again you see that it has plageoclase as a phenocryst phase. So it matches the petrography of the lava flow.

We have also used the chemistry to try to constrain the sources of these tephras that are in the quarry section. This is taken from the Wells et al., paper. In black are shown the soils that were described by Les McFadden. What we did was sample the cinder units that are bounded by the soils. And we took four above that unit we felt there was some uncertainty as to whether it represented re-worked units or primary airfall. This one we couldn't analyze because there was too much carbonate enclosing the scoria. So, what we really have are these samples below the first soil and then this sample within the airfall above these soils. What I will show is that they represent two distinct magmas also.

Okay, this is a plot of thorium and rubidium, two incompatible elements. This is the field for the cinder cone. And within that field are the scoria from the lowest unit, the two lowest units within the scoria section or within the quarry section. They match the chemistry of the cone and they may well be a part of the cone. In fact that is our feeling now that they represent the distal edge of the cone. In that unit 78 which lies above the soil has a different rubidium and thorium concentration and has a different rubidium and thorium ratio. It is 4 as opposed to 3 for the cone. So, this indicates a completely different batch of magmas separated by the soil.

Again scandium versus strontium showing the same thing that here is the field for the cone and the lowermost tephras and then that upper unit about the soil has a distinct chemistry.

So, one further implication of this is that this unit that lies above the soil was not derived from the cone, because it has completely different chemistry. These are two photomicrographs of the scoria. This is sample 78, the scoria above the soil in the quarry unit and this is the scoria from the main cone. This is representative of scoria from the cone at least to a depth of about 40 or 50 meters. We took a transect all the way into the center of the quarry.

The scoria from the cone is a very low density, frothy basaltic pumice. The scoria that is exposed in the quarry section above the soil which has been interpreted as a mass flow deposit, is completely different in its physical properties. It is a much more dense pumice. So, it differs in its physical characteristics as well as its chemistry. So this scoria unit is clearly not derived from the cone either by an eruptive process or by a mass flow process.

So the conclusions from this part of the work are, we have two separate magmas represented by these units here and this unit here separated by soils. So if the soil interpretations are correct, we have two magmas separated by a significant period of time, thousands of years. So this is the probably the simplest evidence for this being a polycyclic volcano.

Second conclusion, this scoria is clearly different from anything on the main cinder cone, so it wasn't derived from the cone either by mass flow or by eruption. It has a different source. We are thinking now that the source may be some of the small scoria mounds in the southern end of the quarry, which we haven't sampled yet. We will be doing that next.

In the third implication of this is, this soil has been dated by thermoluminesence at about 10,000 years. So if that is anywhere in the ball park, then there is an eruptive event that was younger than 10,000 years.

So some of the questions raised by this, the polycyclic model, is are multiple magma batches in a short period of time reasonable for a region of low magma flux? Basically, would you expect four or five magma batches to come up in a very short time consistent with a monogenetic eruption. And actually, it is an open question because there may be clustering of events, magma batch, you know, where you get magma batches coming up, where they cluster in certain areas. So, that is an open question.

But it is unlikely that given multiple magma batches that they can ascend at the same time and place without evidence of mixing or homogenization. We don't see that at Lathrop Wells. In cases where you have a monogenetic center that did involve multiple magma batches, like Kilauea or in western Saudi Arabia, it is in a high flux environment where it is not too surprising that you do have multiple batches, but when you do have it there is clear evidence that there was mixing or homogenization.

And the third one is really part of the conclusion. Conclusions are one, that eruptive units at both Black Cone and Lathrop Wells represent multiple discrete magma batches. There is an alternative interpretation. Turrin et al., state that these variations are consistent with a monogenetic center. We don't understand that interpretation.

Second, soil-bounded scoria units at Lathrop Wells represent discrete magma batches which are erupted many thousands of years apart if the soil interpretation is correct. These units are not derived from the main cone, either by eruption or mass flow mechanisms.

So, from the geochemistry, my conclusion is that the most reasonable model for both Lathrop Wells and Black Cone is that they are polycyclic centers formed over many thousands of years.

So, we have looked at two centers now, Black Cone and Lathrop. Both have evidence of multiple batches and conform to a polycyclic model. So this may be the characteristic behavior of volcanism in the Yucca Mountain region, so it has to be considered in any assessment of volcanic risk.

That's all.

DR. ALLEN: Okay. Thank you.

Questions from the Board? Bill Melson.

DR. MELSON: Frank, let me see if I can articulate this. You have one center erupting at least three or four times.

And in terms of the repository the concern is will it be disrupted by an eruption.

Now, it seems to me what you are saying, what the polycyclic model says is that where the eruption, the next eruption is most likely at one of the centers, because they are separated quite a long time in space.

DR. PERRY: Right.

DR. MELSON: Is that what you are getting at?

DR. PERRY: I think so. What has to be considered in terms of volcanic risk is one, what would be the effects of another eruption at Lathrop Wells? Because if there was an eruption in less than 10,000 years, there is a real possibility that it might erupt again if you have from 10,000 to 65,000 longer duration.

Then if there was a shift to the repository site there could be more than one event. What we would like to understand more is what causes these shifts from once being polycyclic to another center and what the duration is at each center and is there any evidence you can find to predict when a center is going to shut off and then there might be a shift to another center. Does that answer it?

DR. ALLEN: What further work do you expect to carry out to shed further light on this?

DR. PERRY: Well, we will test the model at Lathrop

Wells. We need to do microprobe data on these different phenocryst assemblages to make sure that they do represent separate magma batches. Acquire major data which is important for modeling, you know quantitative modeling of how these things are related. We will look at the other centers, Red Cone, Sleeping Butte centers to see how characteristic that behavior is. And, we want to use this chemical data, to constrain the physical models of how you generate these melts in the mantle, what their history is and how you these different centers evolve. So, I think there is quite a bit more work to do.

DR. ALLEN: Are there other questions or comments?

DR. MELSON: You mentioned various analogies for Lathrop Wells and Black Cone, and you didn't mention Paricutin which is, now people are saying this is the extension of the Basin & Range province and therefore very relevant tectonically to the setting you are talking about here. Now, what does it show in terms of a homogenous or is it a multiple batches quite different?

DR. PERRY: It has, throughout it's eleven year history or whatever it was, there are significant chemical variations through time but they can be modeled as relating to one magma batch that is being contaminated in some cases by wall rot. So, that appears to be just a monogenetic center.

DR. ALLEN: Kip Hodges.

DR. HODGES: Yeah. I was wondering what you used to make a distinction between microphenocryst and phenocrysts in the samples? What is the difference, the size between them?

DR. PERRY: Yeah, it is a little bit subjective what the cut off is. The phenocrysts in the Ql_6 lava are about 600 microns, a little over half a millimeter. The ground mass microphenocrysts are something on the order of 50 microns. But there is no firm cutoff. There is a little bit of gradation. So, it is somewhat subjective.

DR. HODGES: Thanks.

DR. ALLEN: Any comments or questions from the audience? Leon Reiter.

DR. REITER: Frank, I want you to follow through on the first question that Bill had asked. I am still not quite sure and that is whether or not the polycyclic represents a decreased or increased hazard for the repository. And I guess the most recent exchange of letters in Science there was some discussion of that. And I guess to me one of the critical issues is did you say that if it is polycyclic and does occur at Lathrop Wells that it could migrate?

DR. PERRY: Well there are two aspects. If the polycyclic model is correct at Lathrop and the most events were young, you might well expect another eruption at Lathrop

Wells. Then it is a question of how--would that affect the repository at all. That is an effects questions.

Then the other aspect is if the thing did migrate, you know, the volcanism migrated and there was an eruption through a repository, depending upon what the time between eruptions is and we don't have a good handle on that yet, but if it is less than 10,000 years, then you would expect more than one eruption through the repository. So, that would be--that would have to be taken into account for affects.

Bruce wants to answer some of that too.

DR. CROWE: I am going to be talking about this a bit tomorrow, but in essence, what we are arguing, and this is an important point that I think you brought out. If a polycyclic event occurs and occurs at Lathrop Wells, we really don't care. We have looked into scenarios of what it means. We are not even doing a risk assessment on that particular model. What we are focusing on and what represents a finite risk to the repository if a new volcano forms. So, tomorrow when I go through our definitions of E-1, E-2, and E-3, I make them very specific to the model that we are using for risk assessment. So, yes, we will be laboring this polycyclic point, because, as Frank pointed out if a new volcano forms and the polycyclic model is correct, it says not only must you worry about it hitting once, but it is probably going to hit several more times, the time frame of which we don't yet know, but we have to relate that to the 10,000 isolation period.

Now what is an underlying assumption that we haven't settled out here is that we really don't care. Another polycyclic event could probably occur tomorrow at Lathrop Wells and other than the public concern, I mean if they are concerned over a 5.6 magnitude earthquake, they are probably going to go crazy over a volcano, obviously. But, it is 20 kilometers away and the other site that could be polycyclic is the hidden cone that is 47 kilometers away. Realistically, we can't come up with any scenario that affects the repository. So, if I can put that to bed right now, I am going to say it again tomorrow, because I couldn't resist putting it in my view graphs, the issue of polycyclic does not decrease the risk in anyway. It says that once one event occurs multiple events can occur.

But we think the risk of another eruption at Lathrop Wells is so small that we are not even studying it in or probability models.

DR. HODGES: But, Bruce are you saying that the likelihood of a new center occurring is the same for monogenetic polycyclic models?

DR. CROWE: Let me see, let me try that a different way.

In one sense the model of polycyclicity is interesting because if these were all simple monogenetics and we had multiple pulses, you would probably form in a new spot. So, there is a certain element of spatial predictability that polycyclic model gives you. Okay, that is a positive thing in some respects.

But what still have to be concerned about is could a new volcano form. That in essence is the risk represented by volcanism.

DR. REITER: But does the polycyclic model, are you taking advantage of the fact in some way that the polycyclic model tells you that the risk of a new volcano was less because a polycyclic model exists?

DR. CROWE: I would like to be able to. I have no way of factoring that into my calculations right now. In fact, what we are arguing is that because we haven't factored it into our calculations they are somewhat conservative. There is a reduction in the risk because of that model. I can't quantify it so I am willing to just define the risk without that reduction. And what I will argue tomorrow is that we can probably live with that risk without that reduction. So, in a sense it is a moot point or a mute point, as we sometimes say.

DR. ALLEN: Any other comments before we close for the day?

DR. COOPER: Clarence could I make one announcement if we are all done with the questions?

DR. ALLEN: Please.

DR. COOPER: I just would like all the people involved in organizing the field trip or leading the field trip if we could meet up here at this corner of the table for a few minutes, just so we have our logistics straight for Wednesday, I would appreciate it.

DR. ALLEN: Okay, let me thank, Jeanne, you for organizing the presentation today, and thank all the speakers. And as I understand it we'll be meeting at 8:30 tomorrow morning.

Thank you.

(Whereupon, the meeting was concluded at 5:25 p.m., September 14, 1992, to reconvene at 8:30 a.m., September 15, 1992.)