June 2, 1995

MEMORANDUM TO: Michael J. Bell, Chief
ENGB/DWM/NMSS

FROM: John S. Trapp, Sr. Geologist
ENGB/DWM/NMSS

Stephen McDuffie, Geologist
ENGB/DWM/NMSS

SUBJECT: TRIP REPORT ON SITE VISIT WITH DOE AND CNWRA EXPERT PANEL AT LATHROP WELLS CONE, NV

On April 2-3, 1995, John Trapp and Stephen McDuffie of the NRC, participated in a site visit to the Lathrop Wells cone with DOE, Los Alamos National Laboratory (LANL) volcanologists, CNWRA volcanologists, and members of the CNWRA Volcanism Expert Panel (Panel). The primary purpose of this site visit was to allow the LANL volcanologists an opportunity to present their interpretations of the geologic history of Lathrop Wells cone. The results of this site visit are provided in the attachments to this memo.

The primary conclusion that can be drawn from this site visit is that the staff, the CNWRA, and the members of the Panel either disagree with the majority of the interpretations presented by LANL or find the interpretations unsupported by the data presented. This reinforces the need for review of the data requested from DOE in the April 21, 1995, letter from Michael J. Bell (NRC) to Stephen J. Brocoum (DOE), and the selection of volcanism as an area for the vertical slice audit approach. The staff considers that there are serious unresolved concerns in the area of volcanism which must be resolved for DOE to present a complete license application.

If there are any questions regarding this trip report, please contact either John Trapp at 415-8063 or Stephen McDuffie at 415-6684.

Attachments w/Trip Report text:
1. Ltr fm B.Crowe/F.Perry (LANL) to members of CNWRA Expt Panel
2. Agenda for site visit
3. Ltr fm P.Delany(USGS) to B.Hill(CNWRA) re doc of interpret. of info present on site by P.Delaney and S.Self(U Haw)
4. E-Mail Msg fm P.Lipman(USGS) to B.Crowe (LANL) re providing comments on observ fm Lathrop Wells site visit

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DATE	6/2/95		6/2/95		6/5/95		6/7/95	

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TRIP REPORT ON SITE VISIT WITH DOE AND CNWRA EXPERT PANEL AT LATHROP WELLS

1.0 INTRODUCTION

On October 3 and 4, 1994, the Center for Nuclear Waste Regulatory Analyses (CNWRA) hosted a meeting of five experts in the field of basaltic volcanism to review the program of research being conducted by the CNWRA in basaltic volcanism. The experts were: Paul Delaney of the United States Geological Survey (USGS); Peter Lipman of the USGS; Alex McBirney of the University of Oregon; and Stephen Self and George Walker of the University of Hawaii. This meeting was followed up by a field trip on October 5-7 to basaltic volcanoes of the Yucca Mountain region. Following the field trip, each expert produced a report which was sent to the CNWRA and was incorporated in the report Expert-Panel Review of CNWRA Volcanism Research Programs (CWNRA 95-002). This report describes the entire process which was used in the review of the program, as well as containing recommendations and responses to suggestions by the Panel to improve the program. In addition, while the majority of the individual comments by the Panel were directed at the CNWRA program, many comments make reference to concerns with the interpretations being made by the Los Alamos National Laboratory (LANL) scientists performing the volcanism site characterization program for the Department of Energy (DOE).

On January 24, 1994, prior to publication of the formal report by CNWRA, Bruce Crowe and Frank Perry of LANL sent a letter to the five members of the Panel objecting to the comments made regarding the DOE program and requesting that they conduct a field trip for the Panel to present the LANL information. (See letter from Crowe and Perry, January 24, 1995, Attachment 1.) As a result of this letter, a site visit with DOE and NRC was conducted on April 2 and 3, 1995.

The purpose of this site visit was to allow LANL scientists the opportunity to present their field data which supports their interpretations to the NRC and the Panel. The objective was to determine the opinion of the Panel on the validity of the field work and to determine the range in interpretations which the data allowed.

This report provides the opinions of those members of the Panel, who were able to attend the site visit (Self, Lipman, Delaney), on the validity of the interpretations being made by LANL and NRC observations on the suitability of the data base and interpretation for supporting licensing findings in the field of volcanism.

2.0 SITE VISIT PARTICIPANTS

2.1 NRC/CNWRA

John S. Trapp, NRC
Chad Glenn, NRC
Steve McDuffie, NRC
Brittain Hill, CNWRA
Charles Connor, CNWRA

Attachment

2.2 DOE/LANL/YMSCO

Jeanne Nesbit, YNSCO
Bruce Crowe, LANL
Frank Perry, LANL
Chris Einberg, DOE/HQ
Steve Nelson, M&O/WCFS
Greg Valentine, LANL
Les McFadden, UNM/LANL
Stephen Wells, UC-Riverside/LANL
Thomas Bierstead, YMCSO
Terry Crump, M&O/TRW
Kean Kinnegan, LANL
Jim York, Weston

2.3 PANEL MEMBERS

Stephen Self, U of Hawaii
Peter Lipman, USGS
Paul Delaney, USGS

2.4 OTHERS

Carl Johnson, State of Nevada
David Tillson, State of Nevada - Consultant
Gene Yogodzinski, UNLV
E. von Tiesenhausen, Clark County
John J. Perry, Nye County
Kurt Roggensack, Arizona State University
Stan Williams, Arizona State University
Bill Melson, NWTRB/Smithsonian

3.0 RESULTS OF THE SITE VISIT

This site visit was conducted in accordance with "Open Meeting Statement of NRC Staff Policy", 43FR28058, 06/28/78 which details the open meeting policy for applicants and licensees.

3.1 SCOPE OF THE SITE VISIT

The site visit consisted of an overview of volcanological research at LANL on the evening of April 2, 1995, and a visit to various field locations at the Lathrop Wells cone where discussions were led either by Bruce Crowe, Frank Perry, Stephen Wells or Les McFadden on the nature of work performed and the interpretations which have been drawn by the various LANL workers and contractors. As outlined in Attachment 2, the actual site visit consisted of nine scheduled stops. In addition, there was one unscheduled stop at the end of the day to view remains of presumedly young tephras.

3.2 STOP 1

Stop 1 is an outcrop of supposed Qs4 (Holocene?) tephra which no longer exists due to quarry activity, although pictures and samples were taken of the outcrop, as described in the LANL literature. In addition, CNWRA volcanologists have independently obtained samples of these outcrops. They had several questions regarding this outcrop, as described in the literature. It is generally agreed that the Qs4 tephra is separated from underlying deposits by a soil horizon. However, the Qs4 unit contains both oxidized and nonoxidized grains and, thus, does not appear to be a primary fallout deposit. Another alternative interpretation, which was put forward, is that this could be an explosion breccia from a hydrovolcanic eruption. From what was presented on the field trip by LANL volcanologists, it appears that the interpretation of this non-existent outcrop has changed from that presented in the literature. As it was agreed that it was a waste of time to discuss what was not present when there was so much cone still available for examination, discussions at this location were cut short. The NRC needs to determine where the latest interpretation of this outcrop is documented and determine if the CNWRA concerns still exist.

3.3 STOP 2

This location has been mapped by LANL volcanologists as a separate vent from which old deposits were draped by younger fall sheets. The oxidized nature of the overlying material would suggest that overlying material was deposited when the vent was hot. Alternatively, the material could have been emplaced through the overlying fall deposit. From observations at the outcrop, it does not appear possible to determine if this feature is an in-place vent, a bocca, a rafted vent, or some other feature. However, the lack of symmetry suggests this is not in place. While paleomagnetic data suggest an in-place origin, this could also have been due to the material having been rafted while the temperature was above the magnetic blocking point of about 500°C. Another troublesome aspect of the LANL interpretations is that a source location is required for the overlying material which has not been identified.

3.4 STOP 3

This stop has been interpreted by LANL as a Qs2 fall sheet on Miocene bedrock with a good in-filling soil development on the top of the deposit. The interpretation of the soil unit formation appears readily acceptable, however, at this location and at other locations which were visited, the soils were all suggested to be in the 10,000 years or less age category. The lack of older soil units suggests only that volcanic processes were pre-10,000 years. In addition, this unit lacks a readily identifiable vent location.

3.5 STOP 4

This stop has been interpreted by LANL as showing a flow overlying an older scoria mound/vent. An alternative interpretation, which appeared favored by most people at the outcrop, is that this outcrop represents a blocky lava flow

overriding its flow-front breccia. Therefore, the oxidized rubble at the front of basaltic lavas should have the same composition as the overriding flow.

3.6 STOP 5

This stop was to demonstrate geomorphic relationships suggesting a long period of time has elapsed following deposition of the two older lava flow and scoria units and prior to deposition of the main cone and other inferred younger units. LANL workers stressed compositional differences between the scoria fall and main cone deposits, and obvious differences in geomorphic modification. They maintain that similar degrees of dissection should exist on similar age features, regardless of deposit type. Coarse angular alluvial deposits overlie the fall deposits on the western proximal sector to the cone, whereas the cone is not modified by alluvial processes except for minor rilling on the south side. Neither proponents or opponents of this interpretation could prove their points convincingly. Obvious differences in the degree of eolian infiltration may influence erosive processes between cone and fall, as will the presence of established bedrock drainages above the fall deposit. However, gravity-induced slumping may not be sufficient to obscure small rills that might have developed on the cone, assuming such rills were ever developed. Sources of testable ambiguity include the influence of eolian infiltration on slope stabilization, comparisons with other cinder cones of similar age and genesis (e.g., cones at Cima are more agglutinated, which affects erosion rates significantly), and overall preservation and weathering of the fall sheet.

The units in this area were deposited prior to the end of the Pleistocene to develop observed geomorphic relationships. This would suggest that the older units at least are greater than 10,000 years old, but observed geomorphic relationships provide no information on the age of the younger interpreted units or their relationship to the older units. It is argued, by LANL personnel, that because of the lack of geomorphic modification on the main cone that the main cone should be significantly younger than the Qs1 and Qs2 units. As the end of the Pleistocene marked a period of eolian influx, hence soil development, more information would be needed to support this argument, as no evidence was presented as to the soil development which would have been present prior to the end of the Pleistocene. Without this "soil" development, the infiltration potential of the cone material could have been extremely high which would hinder geomorphic development of the cone. Instead of the geomorphic argument, similar shoulders between the main cone and the flow sheets have been observed as constructional features by CNWRA volcanologists at both Cerro Negro and Tolbachik, and it does not appear that LANL personnel have tested this interpretation. In summation, it would appear that the only definitive conclusion which can be drawn from the LANL work is that most soil and geomorphic development occurred during the Holocene, therefore, all volcanological material except the unconfirmed Qs4 tephra material had to be deposited prior to this time. LANL personnel are planning more work in this region which may provide more definitive information, however, in conducting this work a constructional, non-geomorphic origin of this material should be considered.

3.7 STOP 6

At this stop, the older rilled fallout deposits are interpreted to project under the younger cone deposits. However, no trench to date has demonstrated an unconformity or soil horizon between these two units, hence the interpretation appears unsupported. Surge deposits have also been interpreted to be present in this area by LANL. The well-sorted nature of the deposits, internal bed forms, and the large amount of quartz in the deposits, may indicate the unit formed through eolian rather than volcanic processes.

3.8 STOP 7

At this stop, there is a large trench with heavy carbonate coatings on the blocky lava at the base of the trench. Overlying fall deposits reportedly have little carbonate development. This has been interpreted as indicating a large time lapse between units. However, there is no evidence of a soil development between the units, the only difference being the degree of carbonate development. LANL supposedly has photographs of this exposure which may clarify the interpretations. To the south of this unit LANL has interpreted an aa flow on top of a scoria mound, however, this appears to be similar to other exposures which can be interpreted as an aa flow top which has been rolled over by the progressing flow (flow-front breccia).

3.9 STOP 8

At this stop, LANL has interpreted scoria mounds and vents with a feeder dike suggesting a rooted eruption. However, the feature interpreted as a dike is very irregular and blocky, having textures consistent with formation as part of a blocky lava flow. The NRC would consider this as an internal feature typical of aa flows, not a dike.

3.10 STOP 9

This stop, at the top of the cone, was to point out alignment of what LANL has interpreted to be the scoria mounds, which represent fissure eruptions from dikes. This interpretation is quite questionable, however, as these are supposedly the oldest units present and erosion has not exposed any unequivocal dikes related to the interpreted vents. LANL has rejected the possibility of rafting of material due to the viscous nature of the lavas. However, Panel members who have worked in Iceland have seen rafting in viscous lavas at volcanos, such as Heimaey. The LANL volcanologists argued for the linear nature of the "vents", however, the mapped distribution of these interpreted features and the air photos of this area do not appear to strongly support any linear alignment.

3.11 STOP 10

This unscheduled stop has been interpreted as a remnant of unit Qs4 based on geochemistry. All that can be observed at this location is some volcanic material which appears to be some sort of lag on top of eolian sand. There is

no structural or stratigraphic relationship which can be seen. The LANL interpretation is considered to be unsupported.

4.0 GENERAL COMMENTS

1. While LANL has mapped what it interprets as a tephra unit as the youngest unit in the development of Lathrop Wells, there are no associated flow units or vent deposits/vent locations. In addition, there are no unmodified exposures of this interpreted unit left for examination, as the NRC does not consider it reasonable to interpret Stop 10 as an exposure of any recognizable geologic unit. The LANL interpretation is, therefore, highly questionable.

2. For the LANL interpretation to be correct, vents for unit Qs4 must be present in the area. As this is the youngest unit and, therefore, must have the most recent and unmodified geomorphic expression of any unit, the lack of such vents makes the interpretation of Qs4 unsupported.

3. The geochemical data which were presented show no compositional gaps between Units 1-3 but instead show a gradual change as could be expected during a chemically zoned eruption. For the geochemical data to support different eruptions with long-time gaps without having some type of recognizable geochemical break between the various units, it would be necessary to hypothesize a long-lived magma system which somehow was not changed by replenishment or wall-rock assimilation, or it would require multiple magma chambers which fortuitously had magma chemistry almost identical to the previous magma which then changed very slightly. The next magma batch generated would again require almost the same resulting magma as the end of the last eruption. The LANL interpretation may be an over-interpretation of the data. It is recognized that the majority of the LANL data has not been presented, therefore, this LANL interpretation needs to be further evaluated upon receipt of the geochemical data requested from DOE.

4. For the LANL interpretation to be correct, there should be an unconformity separating the older units from the Qs3 unit. LANL can not conclusively demonstrate such an unconformity.

5. For the LANL interpretation to be correct, there should be a fallout deposit associated with formation of the main cone, however, none has been identified. The interpretation of a main cone with no fallout deposit is unprecedented in the volcanological literature and from extensive studies of cinder cone forming processes.

6. For the LANL interpretation to be correct, it should be possible to locate outcrops where a soil can be demonstrated to be present both above and below temporally distinct units. No such evidence has been found despite the numerous trenches and extensive mapping performed.

7. Lathrop Wells is located at the intersection of the generally south trending Windy Wash/Solitario Canyon Fault and the generally northeast trending Stagecoach Road Fault. While there would appear to be some

structural relationship between these faults and the cone itself, there appears to have been no effort by LANL to map the adjacent structure, or do any type of geophysical investigation to better define the relationships of these structures to the cone. LANL has also inferred that various fissures should exist, however, it has presented no evidence to document such features, but rather has based this interpretation on vent alignments. As there are concerns about the interpretation of these features as vents, the inferred subsurface relationships can not be supported, and there appears to be no program planned to gather the data necessary to support the LANL interpretations.

8. The geochronological data, which relies extensively on exposure-dating methods, can at best be described as inconclusive for supporting any interpretation.

5.0 CONCLUSIONS

Based on the observations of the NRC and CNWRA personnel, and on the observations of the Panel, as documented in the attachments to this report, the NRC has the following conclusions:

1. Many of the primary interpretations presented by LANL (features mapped as dikes, flow sheets overriding fall sheets, plugs overlain by younger fall sheets) have alternative explanations which appear more consistent with field observations at Lathrop Wells and at other observed active volcanic fields.

2. Alternative explanations of the geochemical and geocronological data presented are possible. At best, the information can be considered inconclusive in supporting the LANL interpretations.

3. The geomorphological and soils information can only support a pre-Holocene development of the Lathrop Wells features. No conclusive evidence has been presented for Holocene volcanic activity. In addition, certain features which have been interpreted as geomorphic in origin could be primary features developed during the construction of the cone.

4. Many of the features necessary to support the LANL interpretations (unconformities, dikes feeding vents, vent locations for various units) have not been documented in published reports, and the NRC has not yet received access to the unpublished data.

5. The presence of structures and fissures controlling many of the interpreted features have not been documented.

6. Alternative explanations for many of the physical processes assumed by LANL at Lathrop Wells are possible, based on recorded observations of actively erupting basaltic centers.

7. The interpretive constructional and geomorphic history of Lathrop Wells can not be supported by evidence from any active or recently active analog volcano.

8. The interpretation of a polycyclic origin for the Lathrop Wells center can not be disproven conclusively by what was presented in the field, however, the NRC considers that a more orthodox interpretation is better supported by the data.

9. Field activities, such as borings, geophysics, and mapping of structural features, have not been conducted to support LANL interpretations. Furthermore, trenching activities, while numerous, have not been conducted in areas, such as the basal contact of the Qs3 unit, to provide the information needed to resolve many concerns.

Attachment 1

Los Alamos
Los Alamos National Laboratory
101 Convention Center Drive, Suite 820
Las Vegas, NV 89109

WBS 1.2.3.2.5
"QA:N/A"

LA-EES-13-LV-01-95-041

January 24, 1995

Paul Delaney, U.S. Geological Survey
Peter Lipman, U.S. Geological Survey
Alexander McBirney, University of Oregon
Stephen Self, University of Hawaii
George Walker, University of Hawaii

SUBJECT: RESPONSE TO THE CENTER FOR NUCLEAR WASTE REGULATORY ANALYSES COMMITTEE'S REVIEW OF THE YUCCA MOUNTAIN PROJECT VOLCANISM PROGRAM (SCPB: 8.3.1.8.1.1, 8.3.1.8.5.1)

It is our understanding that each addressee of this letter participated in an external review of the volcanism studies of the Center for Nuclear Waste Regulatory Analyses, a contractor of the Nuclear Regulatory Commission (NRC). While we have not received any formal notification or record of the results of that review, it is our understanding that some observations from the review reflect negatively on the quality of volcanism studies conducted by Los Alamos National Laboratory (LANL) for the Yucca Mountain Site Characterization Project (YMP). LANL has been the lead organization since the early 1980's charged by the U.S. Department of Energy (DOE) to gather scientific data to assess the risk of future volcanism with respect to potential permanent storage of high-level radioactive waste at the Yucca Mountain site. Normally, we would not respond to secondary information. However, we have verified these informal observations during telephone conversations with Peter Lipman, chairperson of the review committee. We have also verified that concerns about the quality of volcanism studies by LANL participants were passed on to members of the Advisory Committee for Nuclear Waste (an NRC committee), the Nuclear Waste Technical Review Board (NWTRB), management staff of the DOE involved in the YMP, and our colleagues and management at LANL. Accordingly, we feel some response is required.

We would like to point out that our volcanism studies for the YMP have been reviewed many times over the last decade by numerous scientific groups composed of nationally and internationally recognized scientists and volcanologists. Our work has been reviewed on an almost yearly basis by a subcommittee of the NWTRB. For the most part, we take considerable pride in the positive results of the numerous reviews of our work. We recognize that aspects of our work have been controversial and almost certainly will continue to be controversial. Controversy is a nearly universal aspect of the complex hydrologic and geologic studies being conducted for the DOE's program for disposal of high-level radioactive waste. Studies are politically sensitive at both the State and National levels, and the programmatic focus of much of the work results in somewhat different perspectives than traditional scientific or academic research. We are not surprised by controversy, and we do not object to assessing the scientific impact of differences of opinion. In fact, the approach in our volcanism studies has been to continually encourage the development and assessment of alternative interpretative models in an attempt to ensure completeness of probabilistic volcanic risk assessment for the YMP.

It is unfortunate that we were not directly involved in the presentation of information concerning our work during your review. There is extensive literature developed around our work and the data we have gathered is lengthy, complicated, and in some cases, difficult to interpret. Since we were not present at your field trip, we cannot comment on how adequately our work was presented. However, we can make several observations. It is very unlikely that a short visit to the Lathrop Wells volcanic center would be sufficient to fully appreciate the complex field relations, the scope of studies conducted there, and the justification for the development of alternative eruptive models for the center. For example, the critical outcrops for viewing the field evidence of possible Holocene tephra units separated by soils were destroyed in the last year by commercial quarrying activities (the data have been described and documented with photographs). Alternative interpretations of key field relations have been facilitated by construction of numerous shallow trenches. Most of those trenches have either been buried or are no longer accessible. We continue to be surprised by the difficulty of obtaining conclusive results using conventional field and analytical tools including geochronology, paleomagnetic, and geochemical studies. We have brought in well-recognized University consultants from the fields of geomorphology and soils studies of arid regions who have contributed unique perspectives to the interpretation of field and geochronology data that are not used normally in volcanology studies. We feel we have broken new ground in several technical areas and, by virtue of the newness of our methods, it is expected that some results are viewed as controversial. But the full range of our data and the history of our studies must be examined to appreciate our work.

We recognize the controversial nature of the interpretation that the Lathrop Wells center may have formed during multiple, time-separate eruptive events. Our original interpretation of the center and other centers in the region were that they are simple monogenetic volcanoes. Each person involved in the studies initially viewed with skepticism the concept that the centers could have complex eruptive histories, and we are still somewhat uncomfortable with this interpretation from the perspective of magmatic processes. We have agonized over this problem, discussed it with many other scientists and volcanologists, and have conducted many field trips to the center. You are not the first to question these interpretations and will not be the last. Assessing volcanic risk for 10,000 years is a unique problem and we have taken a different approach than traditional volcanology studies. We are attempting to assess the risk of volcanism where risk is defined as a combination of the occurrence probability and consequences of future volcanic events. The limited record of past volcanic events in the Yucca Mountain region (7 Quaternary centers) means that, by definition, there will be considerable uncertainty in this assessment. Because of this uncertainty, we make several initial assumptions: (a) it is difficult to prove or disprove alternative geologic and tectonic models with limited data; (b) completeness in risk assessment is best achieved by consideration of multiple alternative models; and (c) the test of the significance of alternative models is their impact on probabilistic risk assessment–if risk implications are similar there may be no need to discriminate alternative models. We have been very careful to guard against falsely rejecting a hypothesis (type II statistical error) that could lead to an unacceptable *underestimation* of volcanic risk. The approach we use in studying polygenetic models is to attempt to disprove the model. We now have a virtually unprecedented amount of field, geochronology, paleomagnetic and particularly geochemical data that *do not allow rejection* of the polygenetic model despite numerous tests designed to disprove. We do not accept the polygenetic model as proven, but it remains a viable alternative model. We can also state with a fair amount of confidence from the perspective of abundant multidisciplinary data, that the Lathrop Wells center *cannot* be a simple monogenetic center. We continue to use both monogenetic and polygenetic models in volcanic risk assessment.

Some of the issues that we have been made aware of concerning your review criticisms have had a long history of past discussion, contention and more discussion; the depth of discussion can be fully appreciated only by those having a long period of involvement in volcanism studies for the YMP (minimum of 5-6 years). The issue of monogenetic versus polygenetic eruption models has been debated extensively since 1988 and published in papers and comments in *Geology* and *Science*. It has been the topic of at least two rousing and memorable NWTRB meetings. The issue of the interpretation of results of different geochronology methods has been hashed and rehashed possibly to the point of boredom. The interpretation of paleomagnetic data continues to be controversial and centers on realistic assessment of measurement reproducibility and interpretations of permissive versus definitive data. The issue of satellite vents versus rafting origin of scoria deposits has received much outcrop discussion, and is one area that trenching, geochemical and paleomagnetic data have resolved in favor of a satellite origin. Again, unless the history of these studies was discussed and the full range of data evaluated, it is unlikely that our studies could be fully assessed in your review.

Finally, the point of this letter is that we would like to invite committee members on a return field trip that we would organize. We think it is important to resolve your concerns. We welcome alternative interpretations and appreciate the considerable expertise offered by the committee. In our view, the best way to work toward clarification or resolution is to schedule another field trip to the Lathrop Wells center and/or to any other Pliocene or Quaternary Basalt centers of concern in the Yucca Mountain region. This is of sufficient importance both from our perspective and the perspective of the YMP that we are willing to offer to pay for all travel expenses if this would facilitate the field trip. Accordingly, we would like to try to schedule a field trip to the Yucca Mountain region sometime during the months of next April or May. Pending scheduling of firm dates for a field trip, we will issue formal invitations to each of you under visiting scientists agreements through the Los Alamos National Laboratory. This would allow you to make individual travel arrangements and be reimbursed for expenses. We recognize that with busy schedules and travel it may be difficult or even impossible to schedule a single trip and accommodate all committee members. If conflicts develop, we will consider scheduling multiple trips. Additionally, some members of the review committee are also participants in a volcanism-expert judgment panel funded by the YMP through Geomatrix Consultants. Workshops and field trips are scheduled as part of committee participation and the field trips may be adequate to address review concerns. In either case, we want to offer maximum flexibility in accommodating future review activities. The only requirement of participation would be that each member write a short summary report (one to several pages) of informal observations from the review trip. The report would be submitted to Dr. Jeanne Nesbit, manager of volcanism studies for the DOE.

We look forward to hearing from each of you. We also look forward to interactions in the field and in discussions, and we are confident that the program will benefit from your insights and comments. We doubt that everyone will agree with all interpretations but we feel strongly that you will be impressed by the perceptions we have gained from years of very detailed work at a small volume, continental alkali basalt center. Please respond and provide scheduling information or any questions

to Frank Perry (505 667-1033, fperry@lanl.gov) or Bruce Crowe (702 794-7096, croweb@ymv5.ymp.gov). Again, we look forward to presenting our volcanism data and its applications for probabilistic volcanic risk assessment for the YMP.

Sincerely,

Bruce Crowe, LANL

Bruce Crowe

Frank Perry, LANL

BMC for Frank Perry

BMC:FP:jp02

Cy: R. Dyer, DOE/YMSCO, MS 523
S. Jones, DOE/YMSCO, MS 523
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W. Myers, LANL, DD-EES, MS D446
G. Valentine, LANL, EES-5, MS F665
EES-13/LV, LANL, MS 527
RPC/LV, MS 527

Attachment 2

LATHROP WELLS VOLCANIC CENTER FIELD TRIP
APRIL 3, 1995

Meet in front of the Stardust at 7:30 a.m. Bring your own lunches with liquids. Scheduled field itinerary subject to change.

Stop 1. East Quarry Site of Qs3 and Qs4 tephras, separated by soil horizons.

Stop 2. South quarry. Qsl vent deposits draped by Qs2 fall sheet deposits.

Stop 3. South of Qllb flow. Qs2 fall sheet remnant on top of Miocene tuff bedrock.

Stop 4. Qlld flow. Flow/vent relationships.

Stop 5. West main cone. Qs2 fall sheet, alluvial fan, main cone geomorphic relationships.

Stop 6. North of main cone. Overview of alluvial fan, main cone geomorphic relationships.

Stop 7. Trench exposure of Qsl flow relationships.

Stop 8. Qsl vents east of main cone.

Stop 9. Main cone summit, overview.

Discussion/Comments on field observations.

Attachment 3

eroded away. We conclude that eolian processes seem to be very good at maintaining beautiful aerodynamic shapes of the features they modify.

Finally, we walked up to the top of the cone. This was useful because we determined that the cone surface is not presently at its angle of repose, because fill from the roads doesn't spread down the entire slope, and that sand sheets blanket much of the cone surface. We didn't spend much time discussing the crater, which we should have. Getting back to *Qs4*, there is an experiment that could be done: drill the inside of the scoria cone. There isn't a vent for *Qs4* and its the youngest deposit. We predict that the cone surface has been stripped of it's last-erupted scoria and that the vent in the scoria cone has lots of infilling.

From the top of the cone, we did notice that the northern margin of what is mapped as *Ql1* and *Ql2* seems to have flow levees. We looked long and hard at the eastern margin of the flow, where Crowe and workers, want to place some vents and saw nothing suggestive. We looked at the many scoria mounds and saw no vent structures. Also, we saw no indications that mounds mapped as *Qs1* differed from *Qs2*. Although we are not sure, this distinction is apparently geochemical and not based upon physical characteristics of the deposits.

As geologists, we are never dealt a full deck of cards. So, it's to be expected that not all data and observations fit anybody's story at Lathrop. In fact, if everything did fit, we wouldn't believe it! That said, we suggest that the physical volcanology at Lathrop is entirely consistent with a single eruptive sequence. We don't observe the processes that produce magmas, we don't observe geomorphic degradation of landforms, we do observe volcanic eruptions. The safest and most conservative story, in the end, is the one that is consistent with what has been seen.

Best wishes,

Paul Delaney

Paul T. Delaney

Paul Delaney

/or/ Steve Self

Enclosures

cc: Linda Kovach, NRC

From delaney Fri Apr 28 15:01:57 1995
Date: Fri, 28 Apr 1995 15:00:39 -0700
From: Paul Delaney (GD.Flagstaff) (520)556-7270 <delaney>
To: croweb@ymv5.ymp.gov, fperry@lanl.gov
Subject: Lathrop Wells
Cc: self@soest.hawaii.edu, plipman@mojave.wr.usgs.gov,
mcbirney@darkwing.uoregon.edu, lkovach@nrc.gov, bhill@swri.edu,
cconnor@swri.edu, delaney
Content-Type: X-sun-attachment
Content-Length: 16732

Bruce & Frank,

Steve Self and I have put the following together so that you have a summary of our impressions of the trip out to Lathrop Wells.

Thanks again for taking the trouble.

Bruce and Frank:

Now that you've lead yet another group out to Lathrop Wells, it's definitely past time for Steve and I to send along some impressions of our day spent there with your entourage. We thank you for organizing the trip; we want to honor your request for a brief write-up.

There is only one UNDISPUTED vent at Lathrop Wells, the scoria cone. There was certainly an initial fissure, but this is now buried and subsequent effusion localized to the present site of the cone. We believe there is no solid evidence for other vents and this imposes severe constraints: (1) Unless Lathrop is substantially younger than 100 ka, which seems possible, then the story put together by Steve Wells must be wrong in one way or another; (2) The general "younging" trend of the array of isotopic ages must also be misleading in one way or another; and (3) Interpretation of Th/La data cannot require time-separate eruptions.

Let's run through some highlights of the field trip.

The first stop was the quarry. The Qs4 tephra deposits come from no known vent despite the fact that the youngest vent should be the most apparent. Most likely, the Qs4 deposits that lay above the soil horizon were a lag and, most likely, came from one of the final eruptive phases of the Lathrop cone. To get ahead of the story, the single most prominent impression gained during the field trip was for the substantial volume of scoria fall deposits and rubble now missing from the tops of the remaining tephra deposits and lavas. We think you would agree that lots of tephra and lots of spatter, clinker, rubble, etc. commonly scattered over very young rubbly pahoehoe and aa flows just isn't to be found at Lathrop. Our guess is that the scoria cone has also been stripped, possibly removing evidence for the source of Qs4. Extensive stripping of unconsolidated deposits at Lathrop is important, of course, no matter what the origin of Qs4.

We then looked at a Ql1 flow-top squeeze-up, hornito, or something rafted on the Ql1 flow. It must have been formed near a vent, but we doubt it's now located on the original fissure because that outcrop stands about 30 m or more above the pre-eruption surface. We noticed that the bulldozed pad of tephra just south of that site had basalt sticking up through it, so the feature we looked at probably isn't unusual. All of the tephra around there and all the way out to the south end of the Ql1 flow was oxidized and, seemingly, only several meters thick. The Qs2 deposits south of the quarry on your map must be time-correlative with Qs1; they were deposited on still-hot Ql1 lava.

We looked at a flow margin on the west side of the Quarry and saw good

evidence that the flow overran rubble and, possibly, coarse spatter fallout carried on the flow top and dropped off its front. This is consistent with the steep, and high (30 m?) flow margin. As you remember, we walked around on the flow top and it is now pretty "clean." Again, we take this as evidence for stripping of rubble and spatter from the top of the Lathrop flows.

We examined the base surge deposits, and saw no bomb sags or any other evidence of the explosive origin expected so close to the source. Also it appears that the area we examined consists of a single large dune structure bevelled down close to the surface. This would be very large for a surge dune unless it was part of a Taal-type eruptive sequence. But a Lathrop base surge could only be a small, minor-volume early burst from the pre-cone vent/fissure. The clasts look to be broken scoria clasts, maybe in some cases somewhat rounded, and often admixed with quartz sand. Hyrdomagmatic clasts typical of most base-surge sequences are absent, but there is apparently slight palagonitization or alteration. On the basis of the field evidence we've seen, this deposit is as likely to be an eolian scoria sand-dune as a surge deposit.

We looked at the northern flow margin and saw similar relations. The flow bulldozed forward and over-topped the rubble. The topmost flow lobe appears to have not been able to completely override an earlier flow margin before stopping. We looked at the rootless Ql3 flow on the far north and saw no vent; this is most likely a late break-out from the Ql2 flow and therefore time correlative.

We then walked back toward the beveled surface of Qs1 tephra and looked at a feature that, to us, is not a dike, but a ramp-like structure or squeeze-up from the Ql1 flow just beneath. Interestingly, one side of this feature had a lot more tephra in place than the other, as though the flow was locally breaking through its blanket of scoria. On the one side, there was a generic "scoria mound" and on the other, the bevelled surface. As alluded to above, we suspect that most everything identified on your map as Qs1, Qs2, Qs2fs has been indurated sufficiently that it is resistent to stripping. Judging from the height of the scoria mound next to the "dike" outcrop, at least 2 m of tephra was stripped from the Qs1 surface there.

There were no open pits to see the internal structure of the "scoria mounds." We take your word for it that at least a few of them have low-angle radial dips and are indeed scoria. Your observations indicate that some or even most of them cannot be rafted bits of the scoria cone. According to your map, however, if the mounds are vents, then Lathrop has not one, but something like 25. (An embarrassment of riches!) These vents fall along three or four ill-defined trends, nowhere sufficiently linear to place along a single fissure. None of these trends corresponds with the presumed initial fissure and present scoria cone. At least a few of these mounds should preserve vent structures: fissures, welded spatter, spatter ramparts, agglutinate, alteration halos, drain-back, and so forth. We saw none of this. Lots of stuff gets rafted around on lavas, shoved into nooks and crannies, left high and dry, and so forth during an eruption. Small rootless vents can grow where magma degasses. The mounds are probably erosional remnants of these kinds of processes. The easternmost flow, Ql2 on your map, should expose vent structures particularly well since there is little tephra out there. We didn't visit that locality, but there didn't look to be much there from a distance.

We also accept that the Ql1 surface runs beneath the Qs3 cone deposits. Yet, what else would one expect of early-deposited scoria and/or tephra? The cone had not grown and aggraded that far early in the eruption, so there ought to be an unconformable relation.

Moreover, it makes no sense that the scoria cone looks so modern when we know that lots of material has been stripped from around its base. How can this be? In our experience, cones erode downward while keeping their cone-like form. How can the Qs4 deposits have no identifiable vent or other source if everything is so well preserved? (The lack of a vent or some other outcrop of equivalent material is crucial because sparse Qs4 is the only stuff that that

defines an outlier in Frank's Th/La data set.) Some of the cone scoria, as well as the flanking tephra and other unconsolidated deposits, are gone. Eolian processes seem to be very good at maintaining beautiful aerodynamic shapes of the features they modify.

As geologists, we are never dealt a full deck of cards. So, it's to be expected that not all data and observations fit anybody's story at Lathrop. In fact, if everything did fit, we wouldn't believe it! That said, we suggest that the physical volcanology at Lathrop is entirely consistent with a single eruptive sequence. We don't observe the processes that produce magmas, we don't observe geomorphic degradation of landforms, we do observe volcanic eruptions. The safest and most conservative story, in the end, is the one that is consistent with what has been seen.

Finally, we don't have the wherewithal to send this mailing around to everybody in the DOE/YMP organizational structure who may be interested. Please feel free to distribute our comments as you see fit.

Paul Delaney, USGS
Steve Self, Univ. Hawaii

From CROWEB@YMV5.YMP.GOV Wed May 3 12:38:19 1995
From: CROWEB@YMV5.YMP.GOV
Date: Wed, 03 May 1995 12:36:13 -0800 (PST)
Subject: Re: Lathrop Wells
To: delaney@aa.wr.usgs.gov
X-VMS-To: IN%"delaney@aa.wr.usgs.gov"
MIME-version: 1.0
Content-Type: TEXT/PLAIN; CHARSET=US-ASCII
Content-transfer-encoding: 7BIT
Content-Length: 1626

Thanks for sending your response and taking the time to write your thoughts. I forwarded your comments to the appropriate people with DOE and the M&O. I'm disappointed that you disagree with most of our work but you certainly gave us the time and opportunity to try to prove our case. In my mind, since we are proposing a new eruption model, the burden of proof is on us and I accept your position that we failed to convince.

I just have two quick thoughts. First, do you feel we are properly serving the program by carrying both monogenetic and polygenetic models into risk assessment? That is can you think of any way we might be underestimating risk through our approach? Second, I obviously disagree with some of your observations and conclusions but only want to record some frustration that you chose to simply disagree of dismiss some evidence without really listing the basis for that dismissal. For example, you regard Frank's geochemistry data is unconvincing but provide no alternative explanations for the trends. You argue that Well's work is interesting but invalid but have not offered alternatives for the geomorphic differences in surfaces.

Again, I accept that you regard our work as unconvincing and all I can offer is hope that you will remain open minded about alternative models and perhaps my see features in other volcanoes that might lead to thinking more about alternatives to simple monogenetic models.

I just realized as I was getting ready to send this that I didn't include Steve on the distribution. Could you forward this to him.

Thanks again for spending the time at Lathrop.

b. crowe

From delaney Fri May 5 12:44:50 1995
Date: Fri, 5 May 1995 12:44:41 -0700
From: Paul Delaney (GD.Flagstaff) (520)556-7270 <delaney>
To: CROWEB@YMV5.YMP.GOV, fperry@lanl.gov
Subject: Re: Lathrop Wells
Cc: delaney, self@soest.hawaii.edu
Content-Length: 3338

Hi Bruce.

You're dissappointments with our letter concerning our dismissal of Frank's and Steve's work is appropriate. Remember though, that Self and I are paid to fry other fish; we are not full-timers on Lathrop and cannot bring ourselves up to speed in specialties where we have little experience. That's why we chose to limit our comments to the physical volcanology. I would readily acknowledge that our focus isn't a luxury your group can afford. On the other hand, Lathrop is a volcano first and foremost. The physical volcanology is the basis for producing a map and reconstructing an eruption chronology.

Concerning Frank's Th/La data, what I have to say about it is fairly simple. The only data that appear anomalous are from Qs4, the unit which is of quite uncertain origin and for which there is no identified vent. The data do not fit any petrologic model for partial melting, fractionation, contamination, or whatever, of a single batch of magma. Yet, in geophysics, I am continually made aware of just how poor and primative all of our models are: (1) When measurement errors are well constrained, virtually all models yield residuals that far exceed those due to measurement; I usually accept these models as "good" even though I know damn well, in an absolute sense, that they are "bad." (2) When independent observations or data of a type not directly useable by models are also available, they usually mess things up, need to be addressed in a qualitative, or impose a constraint in the form of inadmissible models. My point here is: Why should geochemistry be different? I take Frank's data as clear evidence that there are some interesting things to be learned. I do not conclude that we have to torque the volcanology around to do it.

On Wells' work, I have less to say. I readily admit that I don't know where the flaws are. I suspect that the analogy from Cima to Lathrop doesn't stand up, that scoria-cone degradation is not a single-path, constant-rate process. On the other hand, I like what he's done and don't feel terrific about not accepting his conclusions. It's just as simple as that.

Getting back to Qs4, there is an experiment that could be done: drill the inside of the scoria cone. You don't have a vent for Qs4 and its the youngest deposit in your scheme (and in mine as well, actually). I predict that the cone surface has been stripped of it's last-erupted scoria and that the vent in the scoria cone has lots of infilling. A drill hole there could well yield your source of QS4.

Your concerns about the effect on hazards models of the single vs. multiple eruption scenarios is difficult for me to answer. I just don't know. I suppose that, by adding to the number of eruptions at Lathrop, your model couldn't be underestimating the volcanic hazard. My concern, again, is much more focused than yours: if a fair sampling of volcanologists can't agree on what happened at a small, well exposed eruptive center like Lathrop, who among the interested public is going to believe ANY hazard assessment from members of that same crowd?

That question isn't intended to invite an answer so much as to offer my perspective. There's no question in my mind but that the next task for you guys is to move your work into publication. I hope the various DOE shackles don't make that too difficult.

Paul

From CROWEB@YMV5.YMP.GOV Mon May 8 00:36:32 1995
From: CROWEB@YMV5.YMP.GOV
Date: Sun, 07 May 1995 21:59:02 -0800 (PST)
Subject: Re: Lathrop Wells
To: delaney@aa.wr.usgs.gov
X-VMS-To: IN%"delaney@aa.wr.usgs.gov"
MIME-version: 1.0
Content-Type: TEXT/PLAIN; CHARSET=US-ASCII
Content-transfer-encoding: 7BIT
Content-Length: 480

thanks again for your reply. I couldn't agree more that it is down right embarassing that volcanologists can't agree on so many details of what should be a simple, and certainly is a small volume volcano. Believe me it is no fun to be supporting a model that no one else believes but I am also feel strongly that you have to at least present what you think the rocks are telling you. I would also like to get on with other work but Lathrop seems to always pull us back.

bruce

Attachment 4

[1] From: plipman@mojave.wr.usgs.gov (Peter W. Lipman) at Internet 4/13/95 5:14PM (4468 bytes: 82 ln)
To: croweb@ymv5.ymp.gov at Internet
cc: delaney@aa.wr.usgs.gov at Internet, self@soest.hawaii.edu at Internet, melson@si.edun. at Internet, bhill@swri.edu at Internet
bcc: Brittain Hill
Subject: Lathrop Wells trip
---------- Message Contents ----------

Bruce,

A rather belated thanks (I came down with a week-long "flu" last Wednesday in Albuquerque) for inviting some foxes to see the Lathrop Wells "chicken coop." I very much appreciated the opportunity to learn more about the detailed work by you and your associates, and also your williness to respond to the CNWRA panel's desire for a field volcanology workshop at Lathrop Wells.

My main impressions as an outcome of the fieldtrip involve a much improved understanding of the amount of detailed work that has been carried out, and the complexity of volcanic and geomorphic processes that have been active at the site.

How much time was involved remains an elusive parameter to constrain; I was indelibly impressed by my involvement with the 19809 MSH eruption that an incredibly complex geologic record can be generated in seconds to minutes during volcanic eruptions. The geochron work at Lathrop is a important effort to push the limiteds of methodology, but I remain unconvinced that any of the age methods are yielding reliable results upon which we can rely to document passage of major time between events at Lathrop Wells.

Frank Perry's detailed geochemistry documents that complex magmatic processes were active during evolution of the cone and associated flows. Without seeing his data tabulated and interpreted, it remains unclear to me how much of the variation can be accounted for by linked crystal fractionation and assimilation processes, versus tapping and or mixing magmas from different sources. The potential chemical leverage of additing small amounts of hi-silica rhyolite (welded tuff xenoliths) is great because of the compositional gradients, especially for incompatible elements. Certainly no other small basaltic center has been studied in comparable detail, so it is difficult to know how typical/atypical Lathop is. A more narrowly focused concern: I was unclear, at the end of the day, just what kind of material was sampled as Qsa4 tephra at the southern site, because the locality seemed consist of lag material at a site of current eolian reworking; hopefully, the type of material sampled, and how it was purified will be documented carefully in a report by Frank.

It remains bothersome to have to appeal to processes (polyclcylic volcanism) that have never been recorded historically. Perhaps more could be learned by comparisons with other young prehistoric cones in varied climatic settings. I also remain concerned that more could be done with tephra studies, in an attempt to determine dispersal patterns, vent locations, and eruptions energetics.

I was especially pleased to meet Steve Wells and Les McFadden, whose careful work and logic impressed me greatly. Yet doubts remain about the role of intense eolian processes in removing material from the Lathrop cone and adjacent flows, and also possibly inhibiting or stablizing rill erosion in such a totally unconsolidated cone, which nevertheless has been

degraded by some processes such that it is no longer at angle of repose. I also hope Bill Melson will check his samples to see whether the non-basaltic sand in the bedded ash at stop #6 north of the cone represents eolian material, vs plausible lithic components of a volcanic surge. Eolian processes just might be key in resolving some current controversies.

I do hope that the important geochem, geochron, and paleomagnetic data will be presented soon in formal publications, rather than just disappearing in the megamountain of gray DOE literature.

In conclusion, Lathrop Wells is a remarkably interesting place, certainly far beyond anything I could have imagined 30 years ago while working on the ash-flow stratigraphy!

Thanks for putting on a fine show!

Peter

cc: Paul Delaney
Steve Self
Bill Melson
Britt Hill

Peter Lipman, MS 910, U.S. Geological Survey, 345 Middlefield Road, Menlo Park, CA 94025: ph. 415-329-5295, fax 415-329-5203