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23 December, 1997

To:

United States Nuclear Waste Technical Review Board,
2300 Clarendon Boulevard, Suite 1300, Arlington, VA.

At the request of the Nuclear Waste Technical Review Board, I have read nine of the reports (list attached) submitted to the Board by the Attorney General of the State of Nevada. I also read the NAS 1992 report entitled "Ground Water at Yucca Mountain: How high can it rise?", several additional reports sent to me by the NWTRB and a pre-print of the comments made by J.S. Stuckless and others on the published paper by C.A. Hill et. al. entitled 'Overview of calcite/opal deposits at or near the proposed high-level nuclear waste site, Yucca Mountain, Nevada, USA: pedogenic, hypogene, or both?'

I was asked by the NWTRB in the course of my review to address the following questions:

1. Are there significant new data since the 1992 NAS report?
2. What is the quality of this data?
3. How much credence does it lend to the hypothesis of ongoing, intermittent, hydrothermal activity at Yucca Mountain?
4. If these data significantly affect the conclusions of the NAS report, how can the issues be resolved?

In summary, my views are as follows:

1(a) There are indeed new data in some of these reports. In addition, some data available before 1992 but not referred to in NAS report have been presented in a new way.

1(b) In my opinion most of the data, as presented and used in the reports I reviewed, are not very significant. My reasons for making this judgement are given in item 3 below.

2. The analytical data are likely to be of good quality as they have been provided by reputable laboratories.

3. As you know the claim made or implied in the 9 reports I reviewed is that there have been, and may still be, ongoing, intermittent and possibly violent hydrothermal eruptions at Yucca Mountain. These events have been accompanied by, or are a

consequence of, episodic influxes of thermal waters derived from below the mountain. These waters allegedly ascended and discharged at the surface on occasions. A detailed response to all the points raised in the reports would require much time and mean that this letter would be many times longer than it is. Instead, I address some of the important topics raised in the reports that fall within my field of experience.

(a) Hydrothermal Eruptions

Szymanski and Archambeau (1996) claim that "the potential for the occurrence of hydrothermal eruptions over the future 10-100 Ka is fairly high". The evidence that they offer, so far as I can see, for there having been hydrothermal eruptions at Yucca Mountain in the past comprises:

(i) The textures evident in figure 4 (page 11) of the item by Szymanski (1996).

(ii) The claim on page 17 of the item by Szymanski and Dublyanski (1996) that a breccia body contains an extensive maze of carbonate and opaline silica veins that "gives it the appearance of a hydrothermal eruption breccia, as noted by Nelson and Giles (1985) and Chepizhko et. al. (1996)".

However, in my opinion the textures shown in the photograph are not those of a hydrothermal eruption breccia and, unfortunately, the authors do not provide adequate descriptions of the deposit in the text of their report. Further, the Chepizhko et. al. (1996) report gives no additional evidence. The Nelson and Giles (1985) paper is a good one, but these authors do not describe any breccias at Yucca Mountain and their general description of hydrothermal breccias do not really match those given in any of the reports I reviewed.

It is true that hydrothermal eruptions are very common in active geothermal systems and occur as they evolve. Indeed, hydrothermal eruptions can be regarded as typical events in high enthalpy systems. However, hydrothermal eruptions produce very distinctive breccias characterised by sub-round to subangular, matrix-supported clasts of different lithologies and, usually, hydrothermal alteration mineralogies. The focal depths of these eruptions range from a metre or so down to 300 metres and the largest eruptions produce deposits that extend, at most, 2 km from their vents. There is nothing in any of the reports that mentions that any of these distinctive breccias occur at Yucca Mountain and, therefore, there is no evidence that hydrothermal eruptions have ever occurred there. Indeed, the low thermal rank alteration of the subsurface rocks implies that temperatures were never hot enough for water to flash to the steam needed to provide the lift that ejects rocks during a hydrothermal eruption.

Nor is there any evidence, in any of the reports I have read, that there are breccias at Yucca Mountain that were produced by either phreatic or phreatomagmatic eruptions.

In summary, in my opinion, there is no evidence given in the reports that hydrothermal eruptions have ever occurred at the Yucca Mountain site.

(b) Subsurface alteration

Several of the reports refer to the hydrothermal alteration of the subsurface volcanic rocks at Yucca Mountain. This comprises both replacement and vein alteration.

(i) The occurrence of several zeolites and some other hydrothermal minerals that replace volcanic glass is not in dispute and this is mentioned in the 1992 NAS report. These minerals indicate that the host rocks have been affected by warm to hot water, another point not in dispute. However, the authors of some of the reports I reviewed claim that the incursions of hot water into the Yucca Mountain rocks have been intermittent and the fluids that did this were derived from below. The evidence for this claim includes the presence of, and the compositional variations in, the clinoptilolite - heulandite zeolites that replace volcanic glass in cores recovered from 5 wells.

Livingston and Szymanski (1996) plot chemical data reported in 1986 in an interesting way. Clearly water was added to the rocks at some stage in their history but there is no evidence, based on the reports, that other constituents were introduced also. There is no logical connection made in the Livingston and Szymanski (1996) report between the glass and zeolites whose compositions they show and the statement, made on page 27, that these analyses mean that there have been enormous quantities of constituents added to the rocks i.e. there is just no evidence given in the report that the latter statement is correct and it appears to be a conclusion taken from an earlier report by Livingston that I have not read and therefore cannot judge. There are standard methods to estimate the magnitude of mass balance events that have affected hydrothermally altered rocks but the method described here is not one of them.

The statements that zeolitization requires large concentrations of cations, e.g. "ranging from 10^4 to 10^5 ppm" (p. 38), are simply not correct.

In my opinion, the zeolites and the other hydrothermal minerals described could have formed from either ascending thermal waters or descending ground waters that became heated by conduction. In any event, the evidence about the ages of the replacement hydrothermal minerals is not clearly stated in the reports I reviewed so their youthful age is not demonstrated.

The evidence for there having been intermittent incursions of hot water into Yucca Mountain is not convincing. I would expect such events to be recorded by appropriate textures evident in the host rocks. Some of the reports do mention that petrographic observations imply these occur but no details or descriptions are given.

Geothermal fields change in their hydrology during their lifetimes and episodic incursions of thermal fluids do occur in some. However, in those fields hosted by volcanic rocks where this has happened, the textural relations of the hydrothermal minerals record such events. For example, by having a chronologically deposited sequence of hydrothermal minerals that filled veins and cavities, hydrothermal minerals replacing other hydrothermal minerals and cross-cutting veins of different ages. There is no evidence, given in the reports, that these textural features occur in the rocks at Yucca Mountain.

(ii) One of the reports I reviewed, Chepizhko et. al. (1996), claims that the occurrence of hydrothermal zircons and some other minerals in calcite/silica veins provides "direct and unequivocal evidence for the hydrothermal origin of some breccias". The actual evidence for these minerals being of hydrothermal origin is nil. None of the photographs show zircon growing on a vein wall or fracture and this is the sort of evidence that is needed to demonstrate their hydrothermal origin. Hydrothermal zircons do not occur in any geothermal field that I know of and apatite occurs in only one. However, euhedral zircons are very common trace or accessory minerals in volcanic rocks where they are of primary origin so I suggest it is possible that the zircons described derive from them. In any case, the zircons have not been dated and unless they are, and give very youthful ages, I judge that there are no "significant" data or conclusions contained in this report.

In summary, there is no evidence in the reports that I reviewed that the secondary minerals present, either as replacements or in veins, which implies that there have been intermittent (including recent) incursions of thermal fluids into Yucca Mountain; nor do they indicate hydrothermal eruptions have occurred there.

(iii) The reports of Szymanski (1996) and Hill et. al. (1995) contain sections on the isotopic compositions of subsurface deposits with attention being directed at strontium isotopes in particular. Stuckless et. al. (1997) responded to the Hill et. al. (1995) paper in a detailed way pointing out, for example, that the strontium isotopic data that the latter authors use actually eliminates ground water as being a possible source for carbonates present in veins at, and near, Yucca Mountain. Szymanski (1996) claims (p. 19) that there are at least nine independent lines of evidence that the carbonates present in a set of veins are the products of hydrothermal circulation. A point by point discussion of them would make this letter even longer, however, most do not hold up to scrutiny in my opinion; for example, the claimed similarities between the isotopic compositions of calcite in a set of veins and those of the local epithermal deposits is arguable; the statement (p. 27) that the presence of *in-situ* grown hydrothermal accessory minerals (not demonstrated) in some breccias "provides direct and unequivocal evidence for the hydrothermal-eruptive origin of these breccias" is not true; the statement that the isotopic data reveals (p. 28) that the deposition of vein and alteration minerals occurred intermittently is not supported by necessary textural or any other evidence.

3(c) <u>Surface deposits</u>

Some of the surficial calcite/silica deposits are deemed to have been deposited from cooling thermal waters or else pedogenic deposits affected by them. Evidence for this claim includes the presence in some deposits of vesicles deemed to be gas cavities created by degassing of cooling fluids. Cavities do occur in silica sinter and travertine but it is impossible that these could form from degassing of thermal water; most cavities I have seen elsewhere were produced after plant roots or stems decayed or where detrital minerals have been dissolved by steam condensate. I notice also that there is no mention in the reports that the calcite/silica at Yucca Mountain contains morphological features common around carbonate depositing springs (for example, terraces, flow features). The NAS 1992 report made this point forcefully but it has not been addressed in the reports I reviewed. Dublyanski and Szymanski (1996) claim that waters which deposited the near surface calcite/silica did not actually flow over the ground surface but rather through the surficial colluvium and alluvium. If correct, then, this would explain the absence of morphological features, but it seems very unlikely to me that any thermal waters would flow, in the way described, for distances of 3 km or more without descending into the rocks below.

On the basis of evidence in the reports, however, I do not believe that the authors have proved their claim that the near surface calcite/silica deposits formed from cooling CO_2-rich waters that discharged at the ground surface.

3(d) There are many other points in the reports that could be addressed or answered. They are full of unsubstantiated conclusions, errors of fact and ex cathedra statements not supported by any, or dubious, evidence. There are no discussions of errors or statistical treatment of data.

In summary, there is no evidence in the reports, so far as I can see, that there have been either intermittent or recent thermal events at Yucca Mountain or hydrothermal eruptions there.

4. I do not believe that the data in these reports significantly affect the conclusions reached, and the evidence presented, in the 1992 NAS report. Indeed, I am surprised and disappointed that the authors of the reports that I reviewed made no effort to address seriously the issues and points mentioned in this report. However, there are a few topics that could be addressed when, and if, further work at the Yucca Mountain site is deemed necessary. Please note, however, that I have not read many of the NWTRB reports or visited Yucca Mountain:

(a) There is no mention in either the NAS 1992 report or those that I reviewed about the primary phenocryst phases present in the Yucca Mountain volcanics. Have they been hydrothermally altered or not? If the latter then the products and intensity of this alteration needs to be determined since they will provide a record of the passage of thermal fluids. If these phenocrysts are unaltered then this needs to be reported also (maybe it has been already but there is no mention of it in the reports I read).

(b) A search should be made of the Yucca Mountain area for breccias of possible hydrothermal, phreato- or phreatomagmatic origin. I doubt that any would have been missed during the geological mapping but it would be important to record their absence (I am not referring here to the breccias whose genesis is under dispute). As I mention earlier in this letter, breccias produced by hydrothermal eruptions have distinctive characteristics.

(c) I note also that some of the recommendations of the NAS 1992 report have not been implemented. Perhaps it would be worthwhile considering doing so?

(d) There is a disagreement about the prevailing geothermal gradients. The steep gradients claimed (in excess of 40°/km) by the authors of the reports I reviewed are not in agreement with those actually measured in drill holes. I also note the response to the Hill. et. al. (1995) paper by Stuckless et. al. (1997) which points out that the heat flow at Yucca Mountain is anomalous because it is low there.

I would like to have read more details of the mention made by Szymanski and Archambeau (1996) that there are two independent centres of hydrothermal circulation, including one located below Yucca Mountain itself. This should not be too difficult to demonstrate, if correct, by making careful measurements in some of the drill holes. It would also be worthwhile determining and interpreting the chemical and isotopic analyses of the deep waters and the pore waters in the tunnel (maybe this has already been done).

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REPORTS AND PAPERS REVIEWED OR READ

The thermodynamic evolution and present state of the lithosphere at Yucca Mountain, Nevada. J. Szymanski and C. Archambeau, 1996.

Chemical heterogeneity of the clinoptilolite-heulandite Fraction at Yucca Mountain, Nevada: Evidence for Polygenetic, Hydrothermal Alteration. D. Livingston and J. Szymanski, 1996.

Hydrothermal Accessory Mineral in Tuffs, Breccias, and Calcite/Opal Veins at Yucca Mountain, Nevada. A Chepizhko, Y. Dublyansky and J. Szymanski, 1996.

Overview of calcite/opal deposits at or near the proposed high-level nuclear waste site, Yucca Mountain, Nevada, USA: Pedogenic, hypogene, or both? C.A. Hill, Y.V. Dublyansky, R.S. Harmon and C.M. Schluter, 1995.

Fluid inclusions in Calcite from the Yucca mountain Exploratory Tunnel.
Y. Dublyansky, V. Reutsky and N. Shugurova.

Stable Isotopes Gradients in Slope Calcretes at Yucca Mountain, Nevada. J. Szymanski and Y. Dublyansky, 1996.

Sr-, C-, and O- isotopic profile from the USW VH-2 borehole, Crater Flat, Nevada.
J. Szymanski, Y. Dublyansky and D. Livingston, 1996.

Epithermal Mineralization, Alteration and Spring Deposits at Yucca Mountain, Nevada-Thermodynamic Evolution of the Geologic System. J. Szymanski, 1996.

Carbonate Deposits at Yucca Mountain (Nevada, USA) and the Problem of High-Level Nuclear Waste Disposal. Y. Dublyanski and J. Szymanski, 1996.

Other references:

National Academy Press (1992): Ground water at Yucca Mountain: How high can it rise?

Department of Energy Report (1993): Report on the origin of calcite-silica deposits at trench 14 and Busted Butte and methodologies used to determine their origin.

C.A. Hill and C.M. Schluter (1993): Petrographic description of calcite/opal samples collected on field trip of December 5-9, 1992.

D.L. Bish and J. Aronson (1993): Paleogeothermal and paleohydrological conditions in silicic tuff from Yucca Mountain, Nevada. Clays and Clay Minerals, 41, 148-161.

D. Vamiman, S.J. Chipera and D.L. Bish (1995): Petrography, Mineralogy and Chemistry of calcite-silica deposits at Exile Hull, Nevada, compared with local spring deposits. Los Alamos National Laboratory Report, LA-13096-ms.

J.F. Whelan and J. Stuckless (1992): Paleohydrological implications of the stable isotope composition of secondary calcite within the Tertiary volcanic rocks of Yucca Mountain, Nevada. International high level radioactive waste management Conference Proceedings. American Nuclear Society, p. 1572-1581.

C.E. Nelson and D.L. Giles (1985): Hydrothermal eruption mechanisms and hot spring gold deposits. Economic Geology, 80.

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June 16, 1998

Dr Leon Reiter
NWTRB
2300 Clarendon Boulevard
Arlington VA
22201-3367
UNITED STATES OF AMERICA

Dear Dr Reiter,

At the request of the NWTRB I have read the documents and responses you sent me, namely:

1. S. Levy and C. Naeser, 1991. Bedrock breccias along fault zones near Yucca Mountain Nevada, Submitted for publication as a US Geological Survey Bulletin.
2. Y.V. Dublyansky and B. Lapin, 1995. Bedrock tuffs, mosaic breccias, and young volcanic rocks at Yucca Mountain: field observations, petrography and chemistry. Report submitted to the Nevada Nuclear Waste Projects Office, February 1996.
3. J. Quade and T. Cerling, 1990. Stable isotope evidence for pedogenic origin of fracture-filling carbonates in Trench 14 near Yucca Mountain Nevada. Science, v. 250, pp. 1549-1552.
4. Y.V. Dublyansky, 1995. Stable isotope composition of carbonates exposed in trenches at the Stagecoach Road fault. Report submitted to the Nevada Nuclear Waste Projects Office, February 1996.

I have reviewed items 2 and 4 in the light of the questions you ask in your accompanying letter. My comments are given in my report following.

Yours sincerely

Patrick Browne
DIRECTOR
GEOTHERMAL INSTITUTE

Encl:

Comments on Document entitled Bedrock Tuffs, Mosaic Breccias and Young Volcanic Rocks at Yucca Mountain: Field Observations, Petrography and Chemistry by Y.V. Dublyansky and B. Lapin

This report has two parts. The longer (Chapter 1) comprises mainly quantitative petrographic descriptions of 95 samples (Nos. HV 22 and 23 are not described in the report) with some brief accounts of their field relations. The samples come from several locations in, or close to, the Yucca Mountain area and some from outside it.

The second part of the report (Chapter 2) gives the results of chemical analyses made of 31 of these samples and interprets them.

So far as I am able to judge the petrographic descriptions are correct and the analyses are good. I would have liked, however, more details about the secondary mineralogy. For example, chlorite is reported as being present in several samples but we do not get much information about its characteristics (no XRD data). Levy and Naeser (1995) report sepiolite in some of the breccia samples they describe but this mineral is not mentioned by Dublyansky and Lapin . There are no details given about how the analyses of the 31 samples were made which I would have expected and it would have been good to determine the amount of CO_2 present directly, rather than to assume it is incorporated within the category Loss On Ignition(LOI). As I have not visited the area a location map would have helped me also. Some of the petrographic descriptions are rather meagre and thus insufficient for me to decide whether or not the conclusions drawn from them are justified. Figures 11 or 18 are missing from my copy and figure 21 on page 34 is labelled as figure 15. There is no stratigraphic column either which would have helped me with respect to the very young rocks reported here, some apparently for the first time.

There is new data in the report as it includes descriptions of more rocks than were considered by the authors of the 1992 NAS report. For example, of the exploratory tunnel and drillpad sites (I think these deserved more detail in Dublyansky and Lapin's report). Much of the new data is not 'significant' in terms of the questions you pose in your letter. However, the exceptions to this, as I see them, are:

1. The claim that there are young rocks in the Yucca Mountain area not previously recognised or described.

2. The comment that the authors did not find any root casts in the AMC breccias they examined.

Chapter 1 describes the bedrock tuffs and the breccias. The authors accept the non-genetic classification used by Levy and Naeser (1991) but reject most of their conclusions and those of the NAS panel (1992). Dublyansky and Lapin's claim (p. 163 for example) that "most of these breccias are cemented by low -temperature hydrothermal (epithermal) minerals" and that the textural relations of the secondary minerals "implies multiple episodes of deposition" (p. 163). I offer the following opinion:

1. Breccias of several types are common in terrains comprised of volcanic rocks, especially pyroclastics, which have no hints of ever having been affected by geothermal activity. The authors agree with Levy and Naeser (1991) and the NAS panel (1992) that breccias at Yucca Mountain and its environs have formed in several different ways. Some were clearly produced by faulting although the relationship between the distribution of some breccias does not everywhere coincide with the locations of the faults that allegedly produced them. The origins of other breccias are not clear, but Dublyansky and Lapin believe some, e.g. those at drillpad WT-7, were produced by a hydrothermal explosion. However, none of the field and petrographic observations describe rocks or outcrops that could have been produced by hydrothermal eruptions of a type known to have occurred in an active geothermal field. Deposits from the latter are almost invariably matrix-supported and characterised by clasts that are both multi-lithological and were hydrothermally altered before being deposited.

2. Some of the AMC breccias described, however, have textures that could be interpreted as having been produced in the subsurface by hydraulic fracturing. For example, figure 86 on page 117 shows a breccia with a such a texture. Hydraulic fracturing is a common process in many geothermal systems and its products occur in many epithermal deposits. The brecciation takes place when pressures within a fracture in a reservoir become locally high enough to shatter the confining rocks, thereby reducing fluid pressure so suddenly that water turns to steam and expands. The clasts that result from the expansion wedge open the fracture and produce a breccia with a " jigsaw-puzzle" texture. However, there is no evidence given in the report that implies any of the brecciation is recent enough to alter the conclusions reached in the NAS (1992) report which considers the genesis of the breccias at the wellpad WT 7 site. Nor do I accept the claim (p. 159) "that the AMC breccias show all the petrographic features typical of low-temperature hydrothermal process[es] form[ed] elsewhere"...

4. The presence of secondary minerals in the breccias is obviously important and there is no dispute that many of the rocks described have undergone some post-depositional changes. The secondary minerals likely include those produced by deuteric alteration, by the devitrification of glass, oxidation, surficial pedogenic processes and hydrothermal alteration. Indeed, it would be very surprising if silica

rich volcanic rocks of Miocene age that contained glass had not undergone some changes. The NAS (1992) report accepts that many of the rocks at Yucca Mountain were, indeed, hydrothermally altered, in the subsurface by thermal fluids. There is no evidence of the age of the thermal alteration in the Dublyansky and Lapin Report (1995) so I see no reason to dispute the judgement of the NAS panel on this point. The occurrence of 'prenite' [prehnite], for example (p. 7) in Trench 14 implies that the altering event was ancient since this mineral is nowhere known to have formed closer to the ground surface than a few hundred metres.

5. The textural relations shown in several photographs are of sequences of silica and carbonate minerals that were deposited episodically. However, this does not prove that they did so as a result of multiple hydrothermal episodes. Other secondary mineral deposition processes can be episodic too and, in any case, there is no solid information in this report about the age of the secondary minerals.

6. It is surprising to me how little replacement-style alteration occurs in the rocks described. Very few samples reportedly show any replacement-style alteration, or even oxidation, so it is clear that thermal fluids have never fully pervaded the rocks at Yucca Mountain.

7. Dublyansky and Lapin (1995) suggest that some of the surficial and shallow silica cemented deposits in the Harper Valley are "most probably" sites where thermal waters discharged on the surface in the past. There is no evidence in the report that I can see for this conclusion, nor for the suggestion that sample HV #20 is geyserite.

Chapter 2 reports the major element chemistry of some of the rocks sampled, including those that are "supposedly young" (p. 150). As mentioned earlier, I expect the analyses are of good quality although there is no description of the methodology used, discussion of possible errors or an adequate statistical treatment of the data. However, the implication stated in the report that the analyses imply large scale alteration "by epigenetic hydrothermal processes (metasomatism)" is not justified in my opinion. This implication in the report is mainly based upon:

1. The differences between the average compositions of 12 older tuffs and three younger ones. The average differences are called and plotted as "enrichments" and "depletions" in the report when they are simply just "differences". That is, the claimed mass transfer is not demonstrated. There is no discussion in the report about vertical and lateral primary variations in the compositions of pyroclastic deposits that surely needs to be considered. Nor is there an adequate discussion of the very high LOI values of some analyses. What are these due to? I expect they

represent the amounts of CO_2 present in the samples (and/or OH in clays) but this is not stated.

2. The comparison between the young tuffs (average of 3 analyses) and the Ammonia Tanks tuff (2 analyses), and plotted on figure 109B, is also similarly misleading and on the basis of the data presented here does not indicate that enrichment or depletion of the younger tuffs occurred - only that there are some compositional differences between them.

3. The large differences in the compositions of the cements in the breccias (Table 3 and figure 111) compared with that in the old tuffs are hardly surprising and the comparisons claimed are meaningless in my opinion. The cements are composed of mixtures of silica and calcite so the chemical compositions depend simply on the proportions of these minerals that were present in the particular samples that were analysed.

4. There is no account taken of density differences upon which a rigorous attempt to estimate mass transfer should be based. In any case, mass transfer through hydrothermal alteration can best be demonstrated and quantified by considering differences between fresh and altered rocks of the same initial compositions.

5. The way that the data are plotted in fig. 109 is misleading. For example, the difference in P_2O_5 contents between the old and young tuffs is 0.02% but this is expressed as a 40% enrichment on the figure because the average P_2O_5 content of the Older Tuffs is only 0.05%. Since we do not know the errors inherent in the anaylses this claim of enrichment has no significance that I can see.

The report is full of non-sequiturs, special pleadings, reliance on dubious conclusions reported in the earlier reports and assertions presented as proofs. There are several mentions, for example, in the report, of so called "hydrothermal accessory minerals such as zircon and apatite" (Chepezhko and Dublyansky 1995) despite this claim not having been proved in the cited report.

Report 3B - Stable Isotopic Composition of Carbonates exposed in Trenches at the Stagecoach Road Fault by Y.V. Dublyansky

This report briefly describes the occurrence of near surface carbonate deposits close to the Stagecoach Road Fault south of Yucca Mountain. It includes the results of 29 carbon and oxygen isotope analyses made of the carbonates including several samples from two 3m or so deep trenches. The author interprets the results to indicate that the carbonate deposits "formed, most probably, due to the action of a system of thermal springs" (p. 22). Although I expect the analyses reported are of good quality I found the report rather frustrating to read because many of the supporting features I expect in a report such as this are absent. These range from minor ones (no location map, inadequate referencing - how am I supposed to check "(Ford, unpubl. data)" on page 12, for example?), through to more serious ones (elevations of sample sites not stated). Other omissions include:

1. An adequate statistical treatment or the data. The Appendix cites isotope values to the third decimal place but we are not told how reliable this last figure is and we surely need some discussion of errors and preferably depiction of error bars on the appropriate figures.

2. An adequate discussion of the work of other authors who reached quite different conclusions, for example the important paper by Quade and Cerling (1990).

Figure 7 is absent from my copy of the report and the scale on figure 4 appears to be wrong since it does not match the depths of the sample sites given in the Appendix. The conclusion that the carbonates described are travertines that are the products of thermal springs is not established in the report. Specifically:

1. The relationship between the calcite-opal veinlets and the carbonate layers is not demonstrated in my opinion. The author writes about spring "orifices" and "feeders" and "vents" but they are nowhere described nor shown to be so. The reader is expected to just accept the author's assertion, yet this is a very important and controversial point that needs to be proved or, at least, discussed. Figure 8, it is true, is described in the text as being "a feeder" inferred by the presence of "vents" (p. 12) but this is not apparent to me in the photograph.

2. We do not see on a map the inferred flow directions of the supposed thermal waters nor their vent locations. Nor is there any mention in the text of the morphological features I would expect to be preserved in travertines deposited by degassing and cooling of CO_2 - rich waters, e.g. terracing, flow features, micro-biological signatures.

3. The interpretation of the data itself is not convincing. I fail to see how the author can claim (p. 6) that the carbonates are heavier in the isotopes of carbon and oxygen further away from the suspected 'orifice' or 'feeder'. Figure 3, for example does not show this for the 4 samples plotted. Elsewhere (p. 10), we read that the "constant values" of six samples for their carbon and oxygen isotopes are "most compatible with the deposition of calcite brought from depth (Palaeozoic limestones) and deposited on or at the topographic surface by up welling hypogene fluids". The basis for this assertion is not evident to me from the data presented here.

4. I am not convinced by inferences that the author makes from the calculated isotope gradients both for some of the reasons given above, and because there is a 75% gap in the sample spacing over 100 metres (fig. 9 on page 17). There also seems to be a spread of data points along both vertical axes that nearly coincide, making the gradient values spurious. The author then compares the lateral gradients with those estimated for "different travertine - forming systems at Yucca Mountain". (Similarly, not proven in the paper cited either in my opinion - see my earlier comments on this).

Please note, in summary, that I am not claiming that the carbonates were deposited by pedogenic processes only that the interpretation and data given in this report do not justify the author's conclusion or inference that they formed from degassing thermal waters. There <u>may</u> be something important in the data here but the poor quality of the science described in the report, in my opinion, stops any such inferences being made with confidence.

Summary

In response to the questions you posed, my review of these two documents leads me to answer as follows:

1. **<u>Are there significant new data since the 1992 NAS Report?</u>**
   There is new petrographic, geochemical and isotope data. I do not believe the petrographic or geochemical data are 'significant' but some of the isotope data could be if the accompanying science was more rigorous.

2. **<u>What is the quality of this data?</u>**
   I judge the isotope and chemical analyses to be of good quality although the analytical methods are not described properly. Most of the petrographic descriptions are adequate, if brief, and I believe the mineral identifications are correct within the limits of the petrographic methods used.

3. **<u>How much credence do these data lead to the hypothesis of ongoing, intermittent hydrothermal activity at Yucca Mountain?</u>**

I do not believe that the data, observations and interpretations given in these two reports demonstrate that there is <u>ongoing</u> hydrothermal activity at Yucca Mountain. There is no convincing evidence that I have read in any of the reports that indicates that thermal fluids have moved through the rocks in the past million years or so.

4. **<u>If these data significantly affect the conclusions of the NAS 1992 report, how can the issue be resolved?</u>**

I do not believe that the data significantly affect the conclusions in the 1992 report but the two reports I reviewed here raise some issues that can be tested or examined more closely.

1. A geologist experienced in studying breccias in geothermal areas and/or epithermal ore deposits should examine the controversial breccias and judge whether or not they could have formed by hydraulic fracturing or some other hydrothermal process (there are well-qualified geologists at the USGS who can do this, for example).

2. The disagreement about whether or not root casts occur in the AMC breccias and how common they are should be resolved. This should not be difficult to do.

3. The younger volcanic rocks reported by Dublyansky and Lapin should be mapped, described and dated. These authors claim such rocks have not been recognised previously.

4. Careful sampling and isotopic micro-analyses should be made of the carbon and oxygen isotopes present in the carbonates. This was recommended earlier by Professor John Valley and will undoubtedly be very revealing.

5. The deposits of slope carbonates should be examined carefully to see if they contain any morphological features that indicate they could be the products of the degassing of CO_2 - rich thermal waters. I recommend also that they be sampled and examined to see if they contain any evidence of thermal micro-biological activity (e.g. fossils of thermophyllic algae, filamentous moulds, clotted fabrics, string fabrics and/or tube fabrics). Microfossils and pollen are common in many surficial hot or warm water deposits and their presence or absence in the carbonate deposits could help resolve the issue of the genesis of the carbonates.

P.R.L. BROWNE
/5 June 1998

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January 2, 1998

Dr. Leon Reiter
Nuclear Waste Technical Review Board
2300 Clarendon Blvd., Suite 1300
Arlington, VA 22201-3367

Dear Dr. Reiter:

I have completed my evaluation of scientific data concerning the problem of ongoing, intermittent hydrothermal activity in the vicinity of Yucca Mountain, Nevada. In my review of information related to the question of whether or not there has been recent hydrothermal activity at Yucca Mountain, I have focused my attention mainly on the fluid inclusion data. My assessment is based only on that information that was provided to me by your office (the nine documents originally provided plus additional reports which I requested during the review), and I realize that there may be other information, as well as other ongoing evaluations of this problem, that I may not be aware of. I have organized my report according to the guidelines laid out in the original instructions, and I have concentrated on the evidence for or against hydrothermal activity, rather than on the theoretical model for hydrothermal activity.

Are there significant new data since the 1992 NAS Report?

The 1992 NAS Report (NAS, 1992) makes little mention of fluid inclusions, except to recommend that fluid inclusion studies be undertaken (c.f., pp. 57, 101, 133, 134, 168). Apparently the only pre-1992 fluid inclusion data are those obtained by Bish (1989) on drill holes USW G-1, G-2 and G-3. Bish (1989) gives only a brief mention of the fluid inclusion results, and the data cannot be used to assess the hydrothermal model because the details of how the inclusions occur, techniques used to collect data, and assumptions involved in data interpretation are lacking. Based on a review of the Bish (1989) and Bish and Aronson (1993) data (Bish and Aronson apparently did not obtain any new fluid inclusion data, but rather used the data from Bish, 1989), Dublyansky and Reutsky (1995) believe that most of the fluid inclusion data reported by Bish (1989) represent the earlier and deeper hydrothermal system associated with the 11 to 9 Ma Timber Mountain volcanic event. Given the available data, I see no reason to question this interpretation.

Since publication of the NAS report (NAS, 1992), numerous publications have reported fluid inclusion data, including Bish and Aronson (1993); Harmon (1993); YMP (1993); Dublyansky (1994); Roedder et al. (1994, 1995); Dublyansky and Reutsky (1995) Dublyansky et al. (1996). As noted above, the Bish and Aronson (1993) data are apparently the same data reported earlier by Bish (1989). The data of Harmon (1993; reported in Hill et al., 1995 and in Dublyansky and Reutsky, 1995), are for fluid inclusions in quartz from the Pull Apart Fault, and its relationship to the calcite/opal deposits being considered here is unknown. YMP (1993) reports 27 (?) homogenization temperatures for fluid inclusions, 7 of which were used by Hill et al. (1995) and Dublyansky and Reutsky (1995) to calculate a recent geothermal gradient at Yucca Mountain. I have not seen the document referred to as YMP, 1993, but many of the tables in other documents (c.f., Table 2 in Dublyansky and Reutsky, 1995) refer to YMP, 1993, as the source of the information. The source (i.e., researcher or laboratory) of the data in YMP (1993) is not obvious from the documents provided to me. The only data collected since 1992 appear to be data collected by Dublyansky and Reutsky (1995) (see also Dublyansky, 1994, and Dublyansky et al., 1996), and qualitative data collected by Roedder et al. (1994, 1995). The Dublyansky and Reutsky (1995) data are from 6 calcite samples collected from the exploratory tunnel at Yucca Mountain. The data of Roedder et al. (1994, 1995) are from 4 samples from drill hole USW G1.

What is the quality of these data?

In terms of fluid inclusions, it is not so much the "quality of the data" that is the issue but, rather, the interpretation of those data. Based on my evaluation of the data available, I am convinced that the "numbers" presented are probably of high quality, in the sense that the numbers (temperatures) reported probably do accurately reflect the homogenization temperatures of the inclusions. With today's high-magnification microscopes and easy-to-use and highly accurate heating/cooling stages, obtaining a precise (and accurate) homogenization temperature is the least challenging aspect of a fluid inclusion study. The quality of the fluid inclusion data, then, is best considered in terms of how the data are collected, and the interpretation of those data. In order to provide a meaningful assessment of the data collection and interpretation techniques used by Dublyansky and his co-workers, it is first necessary to briefly describe the correct protocol one should follow.

In order to use fluid inclusions to determine paleo-temperatures associated with past geological events, the fluid inclusions must trap a single, homogeneous phase at formation conditions, the inclusion volume must remain constant following formation, and nothing may be added to or lost from the inclusion following entrapment. Additionally, and most importantly, the petrogenesis (origin) of the inclusions and the host phase relative to the event being studied must be known (Bodnar, 1994). The procedures for testing the three assumptions above, and for determining the origin of fluid inclusions, have been clearly described by Roedder (1984) and Goldstein and Reynolds (1994).

The first step in a fluid inclusion study is to determine the origin of the fluid inclusions. Inclusion origins are classified as either primary, secondary, or pseudosecondary, depending on when they were trapped relative to formation of the host mineral. *Primary inclusions* are inclusions trapped during growth of the host mineral as a result of growth irregularities or imperfections in the growing crystal surface. *Secondary inclusions* are trapped along fractures some time after formation of the host crystal. Secondary inclusions may form relatively soon after formation of the host crystal, or may form many 10s or 100s of millions of years later. *Pseudosecondary inclusions* form when the host crystal fractures during growth and traps some of the fluid along fractures in the already-formed part of the crystal. Pseudosecondary inclusions are recognized based on their occurrence along fractures that start in the interior of the crystal and terminate at an internal growth surface (i.e., the fracture does not extend all the way to the edge of the host crystal.

It should be noted that, in terms of the questions being addressed at Yucca Mountain, knowing the temporal classification (primary, secondary, pseudosecondary) of the inclusions being studied is not as important as knowing the absolute age of the host mineral. For example, if the fluid inclusions being studied are secondary and indicate "high" temperatures, this means that the mineral was exposed to high temperature (hydrothermal) fluids at some time *after* its formation. If this same host mineral has a very young age, this indicates that high temperature fluids were present at the sample depth at some time in the geologically recent past (since the time of formation of the mineral). Thus, knowing the absolute age of the host mineral is much more important than knowing the temporal relationship of the inclusions to the host mineral, in terms of assessing the probability that high temperature, hydrothermal fluids have entered the near-surface environment at Yucca Mountain within the past few tens to hundreds of thousands of years.

Once the origin of inclusions relative to formation of the host has been determined, the next step is to test the fluid inclusions to determine if they trapped a single fluid, and have not changed volume or gained or lost material since trapping. This can be accomplished by grouping fluid inclusions into *fluid inclusion assemblages* (FIAs) and then testing the three assumptions described earlier. An FIA is defined as a group of fluid inclusions which, based on petrography, were all formed at the same time (Goldstein and Reynolds, 1994). An FIA can consist of a single fluid inclusion but, the larger the number of inclusions comprising the FIA, the more reliable are tests of the three assumptions listed above. Petrographic evidence that a group of inclusions represents an FIA would include (1) a group of fluid inclusions all occurring along a growth surface in the host crystal (primary inclusions); (2) a group of inclusions all occurring along a single fracture that cuts partly (pseudosecondary) or completely (secondary) through the host crystal; (3) a group of inclusions that occurs in a random, three-dimensional distribution, usually near the core of the host crystal (primary inclusions).

To test that the assumption that inclusions in a given FIA represent conditions of formation, the inclusions must be subjected to heating and cooling experiments to determine the temperatures of homogenization and ice-melting. Homogenization temperatures (T_h) provide an approximation of the formation temperature, and the ice-melting temperature (T_m ice) may be used to estimate the inclusion composition in terms of an NaCl-equivalent salinity. If all of the fluid inclusions in an FIA have the same composition, then the inclusions have almost certainly trapped a single homogeneous fluid and have not gained or lost material following formation. Numerous studies (c.f., Bodnar et al., 1985a, b; Vityk and Bodnar, 1995) have shown that if the inclusions trap mixtures of fluids (such as might happen in a boiling or immiscible fluid system), or if the inclusions leak after formation, the compositions of inclusions in a given FIA will show a broad range. Similarly, heterogeneous entrapment or leakage will also result in a broad range of homogenization temperatures within an FIA. However, in some cases, fluid inclusions within an FIA show uniform composition but a wide range in homogenization temperature. This indicates that the inclusion volumes have changed following entrapment, without loss of fluid, to generate a wide range in T_h. This type of fluid inclusion reequilibration is referred to as "stretching" in the fluid inclusion literature (Bodnar and Bethke, 1984; Ulrich and Bodnar, 1988). The magnitude of permissible ranges in T_h and composition vary depending on the geological environment, but T_h ranges of less than $\pm 5°C$ for an FIA that contains a reasonably large number of inclusions (>10) are generally considered to be strong evidence in support of the assumption of constant volume following entrapment. Similarly, compositions that vary by less than about ±0.2 wt.% NaCl equivalent are strong evidence that the inclusions trapped a single homogeneous fluid and have not leaked.

With this background information on how a fluid inclusion study *should* be conducted and how the data *should* be tested for accuracy, let me now consider the fluid inclusion data presented by Dublyansky and his co-workers, and whether or not these data support a hydrothermal origin for near-surface calcite/opal deposits at Yucca Mountain. Dublyansky and his co-workers refer to data from two fluid inclusion studies to support a hydrothermal origin. The first consists of seven fluid inclusion homogenization temperatures taken from a larger data set of 27 fluid inclusion measurements from calcite from drill holes USW G-1 and USW G-2 (YMP, 1993; reported in Dublyansky, 1994; Table 3-2). Dublyansky (1994) states that "seven out of twenty seven datapoints represent young "shallow" calcite", and these data were used to calculate a "recent" geothermal gradient at Yucca Mountain. (Note, however, that only two of the four calcite samples in which the seven inclusions occur have been dated (Dublyansky, 1994; Table 3-2. Also note that the classification of the host calcites as being "young" is based on the observation that "old" calcite [presumably related to the 9-11 Ma Timber Mountain volcanic event] does not occur above a depth of about 900 meters, and that "young" calcite does not occur below a depth of 500 meters, at Yucca Mountain [Dublyansky and Reutsky, 1995, p. 4]). Although there is no indication that the inclusions measured in each sample are all part of the same fluid inclusion assemblage, I will assume that they are. This being the case, do the data accurately reflect the inclusion formation conditions? For three of the samples there are only two inclusions, and only a single inclusion was measured in the fourth sample. However, the homogenization temperatures are sufficiently similar (57° and 59°C; 81° and 72°C; 103° and 104°C) within each of the three samples with two inclusions to suggest that the inclusions have trapped a single homogeneous phase and have not leaked or changed volume after formation. Thus, the measured homogenization temperatures represent the temperature in the sample at some time either during or after formation of the host calcite, and suggest that temperatures as high as 104°C existed within 386 meters of the *present* surface at Yucca Mountain at some time in the geologic past. There is no statement that any of the inclusions are primary, so the temperatures may represent a temperature some time after mineral formation, as noted above.

Using the seven temperatures obtained from fluid inclusions, Dublyansky (1994), Dublyansky et al. (1996) and Hill et al. (1995) calculated a geothermal gradient of 170°C/km at Yucca Mountain. The calculated paleogeothermal gradient must be viewed with skepticism because (1) of the small number of data points; (2) the data from different samples show wide variability as a function of depth; (3) the depth range represented by the samples (less than 200 m) is insufficient to adequately define a geothermal gradient in a natural hydrothermal system; (4) there is no evidence to indicate that all fluid inclusions were trapped at the same time. The lack of contemporeneity of the inclusions is by far the most critical of these four concerns. The interpretation that these relatively high temperature inclusions formed recently is based on young carbon-14 ages (20.9 and 45.26 Ka) obtained on the calcites (Dublyansky and Reutsky, 1995; table 2). If the reported ages represent the ages of the fluid inclusions in the calcite (which may or may not be true), then the inclusions used to determine the paleo-geothermal gradient did not all form at the same time, thus invalidating the use of these inclusions to calculate a geothermal gradient at Yucca Mountain. Determining the ages of the host minerals, as well as the ages of the inclusions in those minerals, is one of the most critical pieces of information needed to evaluate the recent hydrothermal activity hypothesis.

The second set of fluid inclusion data presented in support of a hydrothermal origin for the calcite at Yucca Mountain was obtained by Dublyansky and co-workers (Dublyansky and Reutsky, 1995; Dublyansky et al.; 1996) from samples obtained from the exploratory tunnel. These data are described in detail in Dublyansky and Reutsky (1995). Fluid inclusions in all 6 samples show a very wide range in homogenization temperature, from 30°C to as high as 130°C. All six samples show some inclusions in the lowest temperature bracket (30-35°C), but all samples also show scattering of temperatures to higher values. Most of the measured homogenization temperatures are in the 30-40°C range, with progressively smaller numbers of inclusions in the higher temperature intervals, producing a skewed, unimodal histogram (Figure 1 in Dublyansky et al, 1994). The homogenization temperature pattern defined by the inclusions from the exploratory tunnel is characteristic of that for inclusions which have either re-equilibrated through leakage or volume change

(Bodnar an Bethke, 1984), or which have trapped mixtures of liquid and vapor in an immiscible fluid system (Bodnar et al., 1985a, b). In either case, results of numerous experimental studies show conclusively that the homogenization temperature that most closely approximates the actual formation temperature is the *lowest* temperature on the histogram. Thus, based on the data presented by Dublyansky et al. (1996) the correct homogenization temperature of the inclusions is about 30-40°C. Dublyansky and Reutsky (1995) acknowledge that the higher homogenization temperatures are probably the result of leakage or trapping of mixtures of liquid and vapor, and conclude that the calcite formed from epithermal fluids at 30-50°C. The authors further indicate that this temperature range is consistent with the high geothermal gradient determined (incorrectly) from previously published data as described above.

It is clear that some (or most) of the high homogenization temperatures discussed above (mostly from YMP, 1993) are correct, indicating that fluids with temperatures well in excess of the current temperatures flowed through these rocks at some earlier time. Dublyansky and co-workers interpret this to mean that high temperature fluids existed very close to the present surface of Yucca Mountain at some time in the recent past. However, in volcanic environments, it is common to find samples at or near the surface that contain fluid inclusions with homogenization temperatures well above current near-surface temperatures (sometimes over 300°C!). This does not mean, however, that fluids with a temperature of 300°C existed at the earth's surface at the location being studied. Rather, the inclusions represent fluids, and a hydrothermal system, that was operating well below the earth's surface when the inclusions were trapped. The natural process of erosion has exposed the hydrothermal system at the earth's surface and brought minerals containing high-temperature inclusions to the surface. It is possible (likely?) that this has occurred at Yucca Mountain, and that the samples with high temperature inclusions formed at depth in a hydrothermal system at some time in the geologic past. In hydrothermal ore deposits, we commonly see evidence for the repeated re-opening of earlier veins to allow the passage of later hydrothermal fluids having temperatures and compositions significantly different from those associated with the earlier vein material (Reynolds and Beane, 1985). It is possible that the near-surface veins at Yucca Mountain contain some calcite that formed in an earlier, higher temperature hydrothermal system at some considerable depth beneath the surface, and that these inclusions were mistakenly interpreted to have been associated with the later calcite formation in these same veins. This relates to my earlier comment that it is not whether the fluid inclusions are primary or secondary that is important but, rather, the age of the host mineral. It is not obvious that sufficient care has been taken to determine the age of the calcite hosting the fluid inclusions that were measured. (It should also be noted that there have been significant advances in attempts to date the fluid contained in fluid inclusions during the past few years, and this may be feasible for inclusions from Yucca Mountain. If such information could be obtained, much of the ambiguity concerning the origin of the inclusions could be eliminated)

Fluid inclusions trapped at some depth and then brought to the surface as a result of erosion provide one of the best tools available for determining the depth of formation of the samples and, thus, the amount of erosion that has occurred since formation of the mineral. In mountainous regions with high precipitation, erosion rates range from 95-740 mm/ka, whereas in dry, mountainous regions that rate is 45-370 mm/ka (Summerfield, 1991). Assuming that an average erosion rate of 0.1 mm/year operated at Yucca Mountain during the recent geologic past, 100 meters of material would have been eroded from the surface of Yucca Mountain every 1 million years. During the time since the latest episode of rhyolitic volcanism in the vicinity of Yucca Mountain (8-11 Ma), 900 to 1,100 meters of erosion would have occurred. Using a higher erosion rate (1 mm/year) corresponding to higher rainfall, at least several hundred meters of material could have been eroded from Yucca Mountain during the recent period during which the "young" calcite/opal deposits were formed. It more likely that the lower erosion rate is more appropriate, because Yucca Mountain has been a topographically high region only during the recent geologic past.

Dublyansky and Reutsky (1995) also present limited information on compositions of the inclusions from the exploratory tunnel. They note that the gas inclusions contain mostly methane and hydrocarbons, and state that *"the chemistry of the gases entrapped in these inclusions is not compatible with the unsaturated zone environment"* (p. 50). However, it should also be noted that the compositions of the gases are also not consistent with formation from hydrothermal fluids flowing through silicic volcanic rocks. This environment is one of the most studied hydrothermal environments on earth, owing to the common occurrence of epithermal gold and silver deposits in young felsic volcanic rocks, and the occurrence of methane in such fluids is rare. The gas phase in hydrothermal fluids in felsic volcanic environments is almost always dominated by carbon dioxide, with rarely detectable methane or other hydrocarbons. Note also that the compositions are not consistent with fluid compositions in basaltic magmas, where the fluids are dominated by carbon dioxide and water (Roedder, 1984).

Roedder et al. (1994, 1995) also report gas compositions for fluid inclusions from Yucca Mountain. These workers found gas-filled inclusions in calcite from above the water table in drill hole USW G-1. Crushing studies indicated the presence of major methane and lesser amounts of carbon dioxide and "air" in the inclusions, and interpret the results to indicate that the calcite crystals grew from a flowing film of water on the walls of fractures open to the atmosphere. Again, the presence of methane argues against a hydrothermal origin for the inclusions, as methane is uncommon in hydrothermal fluids in silicic volcanic environments, and is never present as the major gas component in such environments. I should note that a second piece of evidence offered by Roedder et al. (1995) in support of a near surface origin is incorrect. These workers state that "*the presence of gases at essentially one atm pressure in the vapor inclusions requires that the veins were open to the surface at the time of trapping*". This is one possible interpretation but, fluid inclusions trapped at very high temperatures and pressures can have one atmosphere of gas pressure at room temperature for certain compositions. The assumption that the one atmosphere of pressure now observed in the fluid inclusions is correct only if the inclusions were trapped at essentially ambient surface temperatures.

Dublyansky and Reutsky (1995) and Dublyansky et al. (1996) also report the occurrence of inclusions containing 11 wt% $MgCl_2$ in calcites from the exploratory tunnel (as well as other inclusions with $MgSO_4$). Inclusions with such compositions are not typical of the epithermal environment in felsic volcanic rocks. Thus, the compositions of the fluids in the inclusions are not consistent with an ascending hydrothermal fluid origin.

In summary, the quality of the data collected since the 1992 NAS report is probably quite good, in the sense that the numbers are probably accurate. However, these data do not bring us any closer to a conclusion to the debate concerning recent hydrothermal activity at Yucca Mountain because the timing of formation of those inclusions is poorly constrained. The inclusion compositions (both gases and solutes) argue against a hydrothermal origin, although the temperatures are somewhat higher that might be expected for an origin from downflowing surface waters. The geothermal gradient of 170°C/km calculated from fluid inclusions is questionable owing to the lack of documentation that all inclusions used to calculate the gradient were formed at the same time. The major issue that still must be resolved is the age of the fluid inclusions being studied.

How much credence does it lend to the hypothesis of ongoing, intermittent, hydrothermal activity at Yucca Mountain?

Fluid inclusion data from YMP (1993) lend credence to the hypothesis of ongoing, intermittent hydrothermal activity at Yucca Mountain *only if the young ages reported for the host calcite can be confirmed.* The main question is the timing of formation of the seven fluid inclusions reported in Dublyansky and Reutsky (1995) and Hill et al. (1995). If the inclusions are in calcite that was formed during the 11-9 Ma Timber Mountain volcanic event, then the data do not support the hypothesis that there has been intermittent, recent hydrothermal activity at Yucca Mountain. However, if the calcite host minerals are young (i.e., less than a few hundred thousand years), then the data would support recent hydrothermal activity at Yucca Mountain.

The more recent fluid inclusion data (Dublyansky et al., 1996; Dublyansky and Reutsky, 1995) are consistent with formation from low temperature (30-40°C), possibly gas-charged fluids of unknown origin. However, in similar volcanic environments, the gas phase is dominantly carbon dioxide, and methane is rare and usually not detected. Fluids in near-surface organic-bearing sediments, on the other hand, are more often associated with reduced gas species such as methane. Similarly, the observation that at least some of these inclusions contain significant amounts of magnesium chloride is also inconsistent with compositions of hydrothermal fluids in felsic volcanic rocks.

An additional concern which lessens the credibility of much of the data in the group of nine papers is the apparent selective use of information that supports a hydrothermal origin for calcite, with non-supporting data being ignored. This is evidenced by wording in the paper by Hill et al. (1995) such as "after eliminating the measurements from deep-seated CVD as well as anomously (sic) high temperature inclusions that might have been caused by stretching,....page 84) and "After eliminating the data yielding geologically unreasonably temperatures..... p. 85). Similar selective use of heat flow data by Szymanski and Archambeau (1996) to support an anomalously high heat flow at Yucca Mountain was noted by Stuckless et al. (in press).

If these data significantly affect the conclusions of the 1992 NAS report, how can the issues be resolved?

The new fluid inclusion data are sufficiently equivocal that they do not help to resolve the issue of whether or not there has been ongoing, intermittent, hydrothermal activity at Yucca Mountain. The main shortcoming of the fluid inclusion studies is that the absolute ages of the calcites hosting the fluid inclusions are poorly constrained.

As noted above, the major limitation to interpreting the fluid inclusion data, as well as stable isotope and other geochemical data, is that the age of the calcite (or other epigenetic minerals) being studied is not known. Geochronology and stable isotope analyses should be conducted on calcite immediately adjacent to fluid inclusions using current state-of-the-art microanalytical techniques. This requires close collaboration between those workers conducting fluid inclusion analyses, and those conducting stable and radiogenic isotope analyses. These groups should meet to examine the samples together and decide how best to obtain the maximum amount of high-quality information from each sample.

A renewed effort should be undertaken to establish the paleo-topography (depth) of Yucca Mountain over the time interval from the end of Timber Mountain volcanism to the present time. Based on studies of other Miocene and younger silicic volcanic systems with elevated topography, a considerable amount of erosion could have occurred in a relatively short period of time at Yucca Mountain. It is likely that the rocks currently exposed at and near the surface of Yucca Mountain were buried to some considerable depth in the not too distant geologic past. If there has been 1 kilometer of erosion from the current top of Yucca Mountain since the time that the fluid inclusions shown in Figure 19 of Hill et al. (1995) formed, then the temperatures given by the inclusions are less problematical - especially if the minerals hosting those inclusions have ages of 9-11 Ma.

Based on my limited review of the literature related to Yucca Mountain, I am unable to determine whether a researcher with experience working in recent and modern hydrothermal systems in silicic volcanic rocks has been involved in the research effort. Over the past 2 decades, there has been much high-quality research into the physical and chemical aspects of near-surface hydrothermal systems in silicic volcanic rocks, owing to the common occurrence of gold and silver deposits in this environment. An examination of the near surface veins at Yucca Mountain by someone who has observed and studied veins that are clearly of hydrothermal origin may be helpful in deciphering their origin.

The compositions of the fluid inclusions argue against an origin from ascending hydrothermal fluids. The major volatile component (other than water) in fluids associated with both felsic and basaltic magmatism is carbon dioxide, and methane-rich compositions are rare in any type of magmatic system. The presence of magnesium-rich compositions is also inconsistent with an ascending fluid source, either associated with a felsic or basaltic magmatic system. The presence of methane is more consistent with a fluid source involving surface waters flowing downward through organic-rich sediments and precipitating calcite at depth.

Final Comment

The fluid inclusion data suggest that temperatures in excess of those currently measured at Yucca Mountain were present at some time in the past. The two major questions that must be answered in order to use these data to assess the probability that there has been recent ongoing, intermittent, hydrothermal activity at Yucca Mountain are:

(1) What is the age of the fluid in the inclusion or, alternatively, what is the age of the calcite immediately adjacent to the fluid inclusion?

(2) Where was the surface of Yucca Mountain at the time that an individual fluid inclusion was formed? That is, is the measured temperature anomalous (in terms of the geothermal gradient) or does it represent the ambient temperature at that depth at the time of formation?

I would be happy to provide clarification or further documentation on any of the issues raised above. On the attached page I have listed the references to the various publications cited above.

Sincerely,

Robert J. Bodnar

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July 8, 1998

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Dear Dr. Reiter:

This letter is a follow-up to my original letter-report dated January 2, 1998. The information presented below is an update on the validity of the Yucca Mountain fluid inclusion data, and is based on conversations and meetings I have had with Dr. Yuri Dublyansky in recent months. I wish to emphasize that my recent interactions with Dr. Dublyansky have not altered my general conclusions expressed in the January 2, 1998 letter-report.

In early June, 1998, Dr. Dublyansky attended the Pan American Conference on Research on Fluid Inclusions (PACROFI) in Las Vegas. I was also at that conference and Yuri and I engaged in several frank, open-minded discussions concerning the fluid inclusion data and their interpretation. One aspect of his interpretation that concerned me was his assertion that the fluid inclusions from Yucca Mountain contained high concentrations of magnesium chloride and magnesium sulfate. After discussing the basis for this interpretation with Yuri, he agreed that his conclusion was not correct and that there was no evidence to support his earlier claim that the inclusions contained either magnesium chloride or magnesium sulfate. Thus, my statement in the January 2, 1998, letter-report that the compositions of the fluids argue against an origin from ascending hydrothermal fluids is now moot because the composition reported earlier by Dublyansky was not correct.

Following our discussions at PACROFI, Yuri Dublyansky visited the Fluids Research Laboratory in the Department of Geological Sciences at Virginia Tech during the period June 15-19, 1998. During that time, Bodnar and Dublyansky examined some of the same samples studied previously by Dublyansky. The purpose of this work was to test the hypothesis that the high temperatures reported earlier by Dublyansky may have been the result of improper sample preparation and/or data collection techniques.

A new doubly polished section was prepared from sample SS#85-86. The sample contains calcite coating fragments of Tiva Canyon tuff, and was studied previously by Dublyansky and Reutsky (1995). The sample was prepared using a low-speed water-cooled saw, and polished by hand on glass. Numerous 2-phase (liquid + vapor) inclusions were observed in the sample, and many of the inclusions were along growth surfaces in the calcite and thus primary in origin. A group of 10 coeval fluid inclusions in the sample was selected, and the inclusions were heated in one-degree Celsius increments starting at 30°C to determine the homogenization temperatures. Eight of the inclusions homogenized in the range 72-75°C, and the other two homogenized at 80° and 82°C. The consistent homogenization temperatures provide strong evidence that the calcite in sample SS#85-86 was precipitated at a temperature of at least 72°C.

Crushing tests were conducted on several all-gas inclusions in sample SS#85-86. The purpose of this test was to determine the internal pressure in the inclusions at room temperature. If the inclusions were trapped at ambient temperatures (≈20-30°C) in the unsaturated zone, the internal pressures in the inclusions should be one atmosphere. However, pressures less than (or greater than) one atmosphere suggest formation at some (unknown) elevated temperature. Crushing tests on 4 all-gas inclusions indicated that all 4 inclusions had internal pressures less than one atmosphere, suggesting that the inclusions do not contain air that was trapped at one-atmosphere in the unsaturated zone. This indicates that the inclusions could not have been trapped at one atmosphere in the vadose zone.

As noted in my letter dated January 2, 1998, the presence of methane and other hydrocarbons is NOT characteristic of hydrothermal fluids in silicic volcanic environments. However, if the volcanic rocks at Yucca Mountain are underlain by organic-rich sediments or sedimentary rocks, hydrothermal fluids containing hydrocarbons could be generated as heated water circulates through the sedimentary rocks and up into the volcanic rocks. Also, in his presentation at the Pan American Conference on Research on Fluid Inclusions (PACROFI) in Las Vegas in early June, Dublyansky described the presence of a droplet of petroleum found in one fluid inclusion in sample SS#85-86. In order to test for the presence of oil in this sample, which could suggest that the fluids are of deep-seated (hydrothermal) origin, we examined sample SS#85-86 under ultraviolet illumination. Hydrocarbons containing aromatic (benzene-ring) components fluoresce under UV light. When we examined several gas-filled inclusions in sample SS#85-86 under UV light, we saw a faint yellowish ring along the walls of the inclusions, consistent with the presence of small amounts of oil in the inclusions. This conclusion was strengthened by Raman analyses which produced a spectrum with a broad featureless "hump" that is characteristic of fluorescence from petroleum-bearing inclusions. When areas of the calcite away from gas-filled inclusions were analyzed, the broad spectral feature was not observed. Neither the fluorescence observed under UV light, nor the Raman spectrum, are considered to be conclusive. However, these characteristics are nevertheless consistent with the presence of small amounts of hydrocarbons in the gas inclusions.

The most important result of the work conducted in the Fluids Research Laboratory during the week of June 15-19, 1998, is that the high temperatures reported earlier by Dublyansky were confirmed to be real and not an artifact of sample preparation or data collection. There is little doubt that the calcite in sample SS#85-86 either formed at or was later exposed to aqueous fluids with temperatures of at least 72°C. The important question, then, that must be answered is "What is the age of the calcite being studied?" Dublyansky and Reutsky (1995) have identified this calcite as being young, based mainly on its stratigraphic location. According to Dublyansky and Reutsky (1995), old calcite (associated with the Timber Mountain Caldera volcanism) occurs at a depth of 900-1200 m and deeper, whereas young calcite occurs from the surface to a depth of 400-500 m. Sample SS#85-86 was collected from the tunnel at Yucca Mountain and, if the relationship between depth and calcite age reported by Dublyansky and Reutsky (1995) is correct, sample SS#85-86 would contain only young calcite.

Recommendations:

There are presently two competing interpretations for formation of shallow calcite at Yucca Mountain. The group arguing against recent hydrothermal activity (hereafter referred to as the USGS) believes that shallow calcite at Yucca Mountain was formed from cold descending surface waters. The group arguing in favor of recent hydrothermal activity (hereafter referred to as TRAC) believes that the shallow calcite at Yucca Mountain formed from upwelling hydrothermal solutions. Based on published and unpublished reports, as well as exchanges at recent scientific conferences, it is clear that a resolution of this controversy is unlikely to be reached with the USGS and TRAC groups working on samples collected independently and using differing analytical techniques. A possible resolution is for the USGS and TRAC groups, along with a third neutral scientist, to jointly visit Yucca Mountain to collect a small number (5-10) of additional samples that could be prepared and analyzed by the two groups jointly. This approach is likely to eliminate concerns about sample locations and their geologic significance, as well as concerns about damage to samples during collection and sample preparation. After collection, samples of the calcite should be sent to a lab or labs agreed upon by the USGS and TRAC groups to determine the absolute ages of the calcites. If the age-dating suggests that the calcites are young, and if both groups agree with these age determinations, samples of that same calcite should be prepared for fluid inclusion analysis using a mutually acceptable laboratory. Finally, homogenization temperatures of inclusions in young calcite should be determined. It is recommended that the two groups be present and participate in fluid inclusion data collection at a lab not associated with either the USGS or TRAC.

Respectfully submitted,

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6/15/98

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Dear Dr. Reiter:

I have examined in detail the two manuscripts you requested that I read:

1) Geohydrological models and earthquake effects at Yucca Mountain, Nevada by Davies & Archambeau (hereinafter referred to as D&A/EG);
2) Analysis of high-pressure fluid flow in fractures with application to Yucca Mountain, Nevada, slug test data (hereinafter referred to as D&A/T).

You asked me to address four main questions concerning these papers. Before I discuss these papers in detail, let me summarize my answers to these four questions. I will follow this summary with an in-depth analysis of these two papers and the feasibility of the model contained in these two papers that suggests that the water table at Yucca Mountain can rise significantly in response to tectonic events.

Summary Answers to Four Questions Examined

1) Are there significant new data and interpretations since the 1992 National Academy of Sciences (NAS) report on Yucca Mountain?

These papers present new and likely improper interpretations of existing data. They do not present new data.

2) What is the quality of this data and their interpretation?

The new interpretations have been published in the refereed literature (D&A/EG and D&A/T were published in Environmental Geology and Tectonophysics, respectively), but the work is, in my judgement, poor in quality.

3) How much credence do these data and interpretations lend to the hypothesis of large earthquake-induced changes in the water table at Yucca Mountain?

The interpretations depend significantly upon theoretical models that have never been tested or previously used and run counter to observations in nature and in the laboratory. Much of the data important to the interpretations is unsuitable. Hence the interpretations that pertain to large earthquake-induced changes in the water table at Yucca Mountain are unsubstantiated. They are not completely falsifiable given the available data, but it is highly unlikely that these data and interpretations lend any credence to the hypothesis of large earthquake-induced changes in the water table at Yucca Mountain.

4) If these data and their interpretations significantly affect the conclusions of the 1992 NAS Report, how can the issue be resolved?

It is very likely that the NAS panel would have rejected the interpretations contained in D&A/EG and D&A/T because they depend upon theoretical models that run counter to observations observed in nature elsewhere and they depend upon unsuitable use of data. Additional field work and data collection at Yucca Mountain would help in fully determining whether or not the interpretations and models employed have potential validity. However, the likelihood of the interpretations being correct is extremely remote.

Sincerely,

Stuart Rojstaczer
Associate Professor of Hydrology

Review of the Results of:

Davies, J.B., and C.B. Archambeau, 1997, Geohydrological models and earthquake effects at Yucca Mountain, *Environmental Geology*, 32, 23-35.

The principal themes of D&A/EG are two-fold: 1) spatial variations in the elevation of the water table in the region containing Yucca Mountain are controlled by spatial variations in the state of tectonic stress; 2) alteration of the state of stress by moderate earthquakes can cause changes in water table height of a few hundred meters.

First let us examine the first theme. For the D&A/EG mechanism to raise the water table significantly, the current and future state of the water table and permeability in the vicinity of Yucca Mountain needs to be strongly controlled by the state of stress. A major component of the D&A/EG model is that the degree to which cracks are open (and hence permeability is high) is dependent on the degree to which the rocks are undergoing extension. Regions of large extension have low water table elevations because of their large crack openings and associated high permeability. Regions of small extension either have a high water table or steep water table gradient because of their small crack openings and associated low permeability. In the D&A/EG model the region around Yucca Mountain behaves like an accordion. Cracks freely open and close in response to changes in deformation.

There are at least four major problems with the idea that the water table and permeability are stress controlled. First, the data upon which the correlation between state of stress and water table variations is made are of poor quality. The preponderance of measurements used to infer the state of stress (derived from D&A/T) are, as is discussed in the review of D&A/T, inappropriate and error filled estimates of state of stress. Second the two points, USW-G-1 and USW-G-2, where reliable extensive estimates of state of stress were made (derived from Stock et al., 1985, and included in the analysis by D&A/EG) differ in water table elevation by 275 m (Kohl and Liang, 1995). Yet, the minimum state of stress is virtually the same at both locations. Stock and Healy (1988) present two additional sites where reliable state of stress measurements were made in the region. While these two additional measuring points, USW-G-3 and UE-25P-1, are not tested as thoroughly as USW-G-1 and USW-G-2, these tests also do not indicate that state of stress is related to spatial trends in the elevation of the water table. The measurements shown in Stock et al. (1985) and Stock and Healy (1988) belie the assertion made by D&A/EG that the spatial trends in water table elevation in the vicinity of Yucca Mountain are stress controlled. Third, the parameter used by D&A/EG as an indicator of state of stress, the average crack opening pressure in a borehole, is not an independent measure of state of stress and variability in the average crack opening pressure can simply be the result of variability in test intervals used to compile that average. Fourth, even if one assumes that the inferred average crack opening pressures are meaningful, spatial trends in crack opening pressure run counter to the assertion made in the paper that the water table is stress controlled. For example both USW-G-1 and USW-H-1 have high average crack opening pressures yet are in regions of a low water table. The argument made by D&A/EG that the high crack opening pressures at these locations are due to their proximity to the steep hydraulic gradient is pure handwaving. In summary, there is no reasonable evidence that spatial variations in the water table are controlled by spatial variations in state of stress. Without this evidence, there is also no reasonable evidence that variations in permeability in the region are controlled by state of stress. Existing reliable data runs counter to the assertion that spatial variations in water table elevation are stress controlled.

Now let us examine the second theme of this paper: earthquakes can cause the water table in the vicinity of Yucca Mountain to rise a few hundred meters. For this to occur we must first assume (evidence to the contrary) that the current configuration of the water table is stress controlled. However, let us make this improper assumption and see where it leads. The theoretical model to raise the water table is as follows:

> An earthquake leads to stress redistribution. The stress redistribution causes crack closure southeast of Yucca Mountain and crack opening in the zone northwest of Yucca Mountain (the current location of a steep water table gradient). Permeability is dramatically reduced southeast of Yucca Mountain due to crack closure and dramatically increased northwest of Yucca Mountain. As a result, the zone southeast of Yucca Mountain temporarily retards groundwater flow. This leads to a water table build-up of as much as a few hundred meters in the vicinity of Yucca Mountain.

This theoretical model of the Yucca Mountain region's hydrologic response to earthquakes has never been observed in nature or the laboratory. The two most recent models that attempt to explain observed hydrologic responses to earthquakes are 1) earthquakes cause increases in permeability that alter rates and directions of groundwater flow (Rojstaczer and Wolf, 1992, Rojstaczer et al., 1995); 2) earthquakes cause poro-elastic changes that can pressurize and depressurize groundwater and hence alter groundwater flow (Muir-Wood and King, 1993). These models are not mutually exclusive. Both processes - permeability enhancement and poro-elastic effects - can operate in response to earthquakes although there is a debate as to whether or not one process is dominant.

The D&A/EG model is a modification of the Muir-Wood and King (1993) model. As in Muir-Wood and King (1993), poro-elastic effects alter groundwater pressures and state of stress. Unlike Muir-Wood and King (1993), those changes in state of stress cause both dramatic increases and decreases in permeability. Areas undergoing compression in response to moderate earthquakes undergo two order of magnitude reductions in permeability. Areas undergoing extension in response to moderate earthquakes undergo two order of magnitude increases in permeability.

It is not possible to reconcile the D&A/EG model with observations in nature. The Muir-Wood and King (1993) model requires permeability to remain high in regions undergoing compression to explain the hydrologic response to major and moderate earthquakes observed throughout the world. It is useful to note that the Muir-Wood and King (1993) model does not predict hydrologic changes in response to earthquakes of the magnitude of D&A/EG. In their compendium of hydrologic changes associated with earthquakes, Muir-Wood and King (1993) find increases in discharge of at most tens of millimeters of equivalent recharge. The water table rise associated with such changes would be:

Water table rise = Equivalent recharge/Porosity

Given porosity on the order of 1 to 10 % the expected water table rise in response to magntiude M=7 earthquakes and smaller would be less than 10 m and typically less than 1 m.

The are other problems with the D&A/EG paper that while not directly related to the D&A/EG mechanism for raising the water table at Yucca Mountain, indicate that the work is of poor quality.

1) Alternative hypotheses for earthquake induced hydrologic effects are never explored.
2) Collaborative data (the hydrologic response to Skull Mountain) cannot be explained well by the D&A/EG mechanism (Why do the largest changes in water table elevation occur several rupture lengths away from the earthquake where as is noted by D&A/EG the expected magnitude of deformation is on the order of 10^{-8}? There is no evidence anywhere in the world for such small strains to cause large reductions in permeability.).
3) The paper contains significant amounts of "science by assertion". Statements are made without any significant data. For example, on p. 29 it is stated that "the fact that the minimum principle (sic) stress measured increases as the hydraulic gradient is approached from the south". The inferred average crack opening pressure in the boreholes, a parameter that cannot be used as a surrogate for minimum state of stress increases from south to north. As is noted in the discussion of D&A/T below, estimation of variability in minimum principal stress cannot be made from estimates of variability of crack opening pressure alone. Also as is noted in the discussion of D&A/T given below, even if crack opening pressure variability could be used as a surrogate for stress variability, the method developed in D&A/T to infer crack opening pressure is clearly seriously flawed. Finally, determining an average state of stress or crack opening pressure for a borehole is largely meaningless since the average can be expected to vary significantly as a function of the test interval chosen. Another example of science by assertion found on p. 29 is that "we can expect a difference between conductivity values for the open-fracture region and the closed-fracture region of from (sic) one to three orders of magnitude. This statement is made on the basis of only two measuring points. It is not valid to identify trends on the basis of two data points. Also, whether there exist two distinct regions in the vicinity of Yucca Mountain, one with closed fractures and one with open fractures, is pure speculation.

Overall, I would rate the quality of the D&A/EG paper as poor. It is an example of the many articles in the refereed literature that are published even though they are of dubious quality.

Review of the Results of:

Davies, J.B., and C.B. Archambeau, 1997, Analysis of high-pressure fluid flow in freactures with applications to Yucca Mountain, Nevada, slug test data, *Tectonophysics*, 277, 83-98.

D&A/T attempts to derive meaningful estimates of flow and stress parameters from slug tests. With regard to whether or not the water table at Yucca Mountain can be expected to rise a few hundred meters in response to a moderate earthquake, it is the estimates of stress parameters that are most important. However, the slug tests were not designed with determination of stress in mind. There is every indication that the attempt by D&A/T to infer stress parameters from slug tests is a failure. Specifically, D&A/T attempt to infer crack opening pressures from the slug tests. We can get an idea of the quality of their estimates by direct comparison with standard estimates of stress inferred from hydraulic fracturing tests. Two standard state of stress tests from Stock et al. (1985) are included in D&A/T (they do not state that these data come from Stock et al. (1985), but they apparently do as is indicated in Kohl and Liang (1995)). These data from Stock et al. (1985) are partially misused since D&A/T derive an average fracture opening pressure from measurements in USW G-2 even though Stock et al. (1985) state explicitly that three of the measurements are not precisely determined and are only upper bounds. Ignoring this problem, the fracture opening pressures obtained from the Stock et al. (1985) tests are almost all 10 to 50 bars higher than those inferred from the slug tests (Kohl and Liang, 1995). The most direct test of the method used by D&A/T with standard hydrofracture techniques is one measurement in UE-25P-1. Comparison of the method used by D&A/T in the interval 1554-1600 m (shown in Kohl and Liang, 1995) with the measurement of state of stress at a depth of 1573 m by Stock and Healy (1988) indicates a difference in fracture opening pressure of 87 bars. These discrepancies indicate that the use of the model derived in D&A/T to infer fracture opening pressures from slug tests contains a great deal of error. Any trends in fracture opening pressure inferred from slug tests are almost certainly meaningless.

It is not surprising that the inferred fracture opening pressures are incorrect. Slug tests are not designed for state of stress measurements. Also, the model used to obtain fracture opening pressures is a very simple one. For example, it assumes that there is no storage in the aquifer and that crack propagation and matrix flow operate independently. Both assumptions are likely invalid. If crack opening is occurring during these tests, the model used is too simple to accurately determine physically meaningful parameters. Finally, alternative models that can likely produce equally good fits to the observed slug test data are not examined at all. The model fit to the data shown as the "USGS fit" in Figure 6 does not, as D&A/T note, fit the observations well. But alternative models that do not require crack opening, such as a dual porosity model, would likely fit the observed data just as well as the model of D&A/T. It would appear that the model fits to the data are simply numerical artifacts with little, if any, physical meaning.

Even if the estimates of fracture opening pressure are correct (and they aren't), one cannot infer anything about state of stress from fracture opening pressure alone. The relationship of fracture opening pressure to the state of minimum horizontal stress requires knowledge of the ambient pressure of the interval. For an existing fracture the relationship is:

Minimum horizontal stress = Fracture opening pressure + Ambient fluid pressure

Without knowledge of the ambient fluid pressure, it is not possible to know the value of the state of stress. It is impossible on the basis of knowledge of variability of fracture opening pressure alone to say anything about variability of minimum horizontal stress unless the ambient fluid pressure is spatially constant. At Yucca Mountain, the ambient fluid pressure varies greatly as is indicated by the high variability in water table elevation. Therefore it is not possible to determine minimum horizontal stress variations over space based on variations in fracture opening pressure.

Similar to D&A/EG, the paper D&A/T is one of poor quality. Its major short coming is that it develops a method to infer fracture opening pressure that in comparison with conventional methods is clearly in error. Its second short coming is that it assumes inappropriately that variations in fracture opening pressure can be used to directly infer variations in minimum stress. Tectonophysics is, unlike Environmental Geology, a highly regarded journal. But even highly regarded journals publish poor manuscripts every now and then. This paper is clearly an example of a poor paper that made it through the cracks in the system.

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December 18, 1997

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Dear Dr. Reiter:

This letter summarizes my findings in regard to the question of hydrothermal activity in the vicinity of Yucca Mountain, Nevada. I have read the 9 documents referenced in your letter of Oct. 31, 1997: Hill et al. (1995); Dublyansky and Szymanski (1996); Dublyansky et al. (1996); and Parts 2, 3, 5, 6, 7, and 8 of the 1996 TRAC-NA Final Report. These documents are authored by Jerry Szymanski or his colleagues, Yuri Dublyansky and others, and they promote an epigenic model for on-going hydrothermal activity at Yucca Mountain that involves fluids upwelling at the surface from sources below the water table. For convenience, when referring collectively to these nine documents , I will call them S+D. I have also read the 1992 NAS Report on Groundwater at Yucca Mountain and the comment by Stuckless et al. (1997) discussing the Hill et al. (1995) paper. These authors dispute the S+D model. While I have also read a number of other related articles, concentrating on those written since 1992, there are many unpublished reports cited by the above manuscripts that are not in the University of Wisconsin Library System and which I have not had the opportunity to examine.

The S+D documents are difficult to critically evaluate. Even in the unpublished TRAC reports, where space is not as precious as in peer-reviewed literature, there is a heavy reliance on citing data and paraphrasing discussion from other unpublished reports. Furthermore, the TRAC reports do not include full citations to sources, making library work more difficult.

Initially, I found the task of assimilating and critically evaluating the extensive Yucca Mountain literature daunting. Accordingly, I have concentrated my efforts on evaluating stable isotope evidence that is presented that would favor a model for the precipitation of carbonate cements and opal either: from rising groundwaters (epigenetic) or from descending rain water (pedogenic). To my surprise, I have found that the issues surrounding these cements are quite clear.

The existence of carbonate cements at all levels in drill core is proof that a quantity of water-rich fluid has moved through certain zones in the Yucca Mountain area. The controversy pertains to the timing, source, temperature and chemistry of these fluids. These cements are complex, likely representing the superimposed effects of multiple events and possibly representing a long time period. Geochronology indicates that at least some of these cements are younger than 100Ka. A full understanding of the processes of carbonate cementation should be sought, regardless of whether the ultimate source of water is recent precipitation or ancient groundwater. While the quantity, chemistry, and temperature of upwelling (epigenic) fluids might provide the most severe challenge to a nuclear waste repository, the presence of cements from descending fluids, pedogenic or otherwise, would also be a concern. Such cements document fast pathways of fluid movement, as have recently been suggested based on ^{36}Cl compositions within the tunnel at Yucca. It is possible that better understanding of carbonate cements would aid in understanding the occurrence of these bomb-blast nuclides at depth.

There are three lines of stable isotope evidence presented by S+D that I consider potentially significant for the genesis of carbonate cements: 1. oxygen isotope thermometry based on micritic carbonates and opal, 2. trends in $\delta^{18}O$ with depth, and 3. trends in $\delta^{18}O$ and $\delta^{13}C$ down slope from faults. An additional important line of evidence is described in the NAS 1992 Report, 4. the isotopic composition of groundwater.

All of the stable isotope data that I have been able to evaluate for Yucca Mountain has been measured using conventional bulk analytical procedures developed almost 50 years ago. These data can only yield accurate estimates of temperature or fluid composition if the carbonate is isotopically homogeneous throughout the volume of material analyzed. Analysis of the homogenized powder from a heterogeneous sample can yield spurious results. S+D powdered material weighing many milligrams and performed bulk dissolutions in phosphoric acid. If there was any zoning or heterogeneity, the information was destroyed. A common precaution in such a study is to drill each sample with a dental burr to evaluate homogeneity at the sub-mg-scale, however this requires multiple analyses from a single sample and I didn't find evidence that this was done.

The best way to study carbonate cements at Yucca Mountain would be to perform micro-analysis of the oxygen and carbon isotope ratios by either fine scale mechanical sampling or ion microprobe (secondary ion mass-spectrometry). Recent advances in technology permit analysis of samples a million times smaller than has been done by S+D (see SEG Reviews in Econ. Geol., 1997). Samples weighing as little as 5ng can now be analyzed insitu from a microscope slide with a small trade-off of precision and accuracy (±0.5‰) and samples of 5µg can be analyzed with no loss in accuracy or precision. Small amounts of sample variability when measured conventionally may be a red flag indicating a heterogeneous sample with zoning many times greater than detected by the bulk analyses. Isotopic zonation would be predicted for the dynamic conditions of either the pedogenic or the epigenic models. It has now been proven in numerous geologic environments, including diagenesis and hydrothermal alteration, that the isotopic variability within a single conventional-size bulk sample can be as great as that found within the entire district. The measurement of gradients and zoning profiles within single crystals by microanalysis would provide information about the evolving character of a hydrothermal system that is not now available.

1. **Oxygen isotope thermometry.** The partitioning of oxygen isotopes between coexisting phases is temperature dependent and is commonly applied as a geothermometer. Hill et al. (1995) report values of $\delta^{18}O$ for calcite and opal in 12 surface and drill core samples, and estimate temperatures of -24 to +238°C. The accuracy of calibration for calcite-quartz thermometry has recently been challenged by Sharp and Kirschner (GCA, 1994), though this will only affect the magnitude of temperature estimates and not their relative values. Another concern comes from the use of opal which contains oxygen in at least two forms, tetrahedrally bonded to silicon and as OH. Different types of opal may fractionate oxygen isotopes differently. The presence of OH affects the calibration of the thermometer and makes opal subject to post-crystallization exchange.

The primary data for calcite-opal thermometry are referenced to two unpublished reports that I have not been able to obtain. Hill et al. dismiss three of their 12 samples as yielding geologically unreasonable results, ie <2°C, and the other 9 are plotted vs. depth (their fig. 21). They state that "this method infers a paleothermal gradient (~180°C/km) much steeper than the current one" (p 85). The scatter in figure 21 is so great that I would question the accuracy of any gradient based on this relatively small data set. Furthermore, as mentioned by Hill et al., oxygen isotope ratios can only be used for thermometry if the two phases of interest have attained isotopic equilibrium. Clearly, the three lowest temperature samples were not equilibrated and no evidence is presented to evaluate the other 9 samples. It is common for fine grained cements to form in a sequence as a hydrothermal system evolves. If calcite precipitated at a different time from the opal, then no meaningful temperature information can be obtained from the technique applied by Hill et al. In fine grained cements of this sort, a careful thermometric study should attempt to evaluate if the calcite and opal co-precipitated by detailed microscopic examination of textures and repeat analyses from a single sample. Numerous forms of imaging are available and might help in this examination. Considering that temperatures of 26 and 238° were estimated at 258-280m depth, it would be prudent to evaluate the reproducibility of these estimates. Such extreme disagreement is far beyond the analytical uncertainties and must either reflect real differences in temperature or an undiscussed problem in the thermometry. An easy way to evaluate this question is to separate small pieces of each sample and repeat the analysis. If the temperature estimates are real, then the temperature estimates should be reproducible for each piece of the sample.

Based on the relatively small size of the data set, the poor reproducibility of temperature estimates, the high percentage of "geologically unrealistic temperature" estimates, and the absence of any supporting evidence to favor equilibration of calcite and opal, I believe that it is premature to make any thermometric interpretation of the calcite-opal data.

2. **Trends in $\delta^{18}O$ with depth.** Hill et al. report paleothermal gradients based on carbonate cements in shallow samples from 4 drill holes. They conclude that "three of the four holes can be seen to display apparent paleothermal gradients higher or much higher than the current gradients" (p 84-85). The points in their fig. 20 plot closely along "linear-fit approximations" giving the impression that measured $\delta^{18}O$ values tightly define these gradients. However, fig. 5 of Dublyansky and Szymanski (1996) shows the actual data from these four holes and there is so much scatter that one can't tell which sub-set of data is fit by each line. Data from a fifth locality, Busted Butte, is shown in fig. 6

of Dublyansky and Szymanski (1996). A gradient of -1.97‰/100m is estimated for $\delta^{18}O$ vs. depth, but the profile is less than 30m long and the data deviate significantly from the linear-fit line. These gradients are not precisely restricted by the data, and given the uncertainties discussed above relating to possible heterogeneity of cements, the gradients may not be accurate either. It is impossible to confidently say that these gradients are different from what would result if temperature varied according to the modern thermal gradient and thus these measured profiles do not distinguish between the epigenic and pedogenic models.

3. **Trends in $\delta^{18}O$ and $\delta^{13}C$ down slope from faults.** Dublyansky and Szymanski (1996) report lateral gradients of $\delta^{18}O$ (0.03 to 0.90‰/100m) and $\delta^{13}C$ (0.06 to 0.82‰/100m) for calcite in surface samples along four profiles down slope from faults at Bare Mountain, the drill site for hole WT-7, and Stagecoach Road (their Table 2). When compared to the elevation effect of Quade et al. (1989), the Bare Mt. and WT-7 gradients are from 1.4 to 21 times greater (Table 3).

These trends deserve close examination. If such trends are reproducible and are in fact different from local elevation effects, this would be strong evidence favoring progressive evaporation and CO_2 out-gassing (and perhaps cooling) as fluids move down slope. The pedogenic model does not account for such effects. If the cements are only found down slope from faults this would favor precipitation of carbonate from water issuing from the faults. I will recommend that this question be studied by further sampling and analysis.

Dublyansky and Szymanski (1996) show maps of the Bare Mt and WT-7 profiles (their figs. 7 and 9, I have not found documentation of the Stagecoach Road profile) and they plot $\delta^{18}O$ and $\delta^{13}C$ vs. lateral distance (figs. 8 and 10). The data for figs. 8 and 10 are tabulated in Tables 1 and 2 of Szymanski and Dublyansky (TRAC-Pt 6) and more detailed maps are shown as figs. 9 and 10 of that report. Tables 1 and 2 contain data for 20 and 11 samples respectively, while the corresponding figures in D+S contain 10 points each. The slopes of linear-fit lines to these data are not well constrained by the small data sets. Furthermore, I obtained different gradients upon plotting the full data sets.

Of more concern is the gradient at WT-7. This is interpreted as two profiles based on the observation of a tributary flow system. Because of the tributary, two lines are fit to the 10 data points, one to the first 5 points and one to points 6 through 10. The resulting gradients are all large and positive in slope. However, if all 10 $\delta^{18}O$ values from WT-7 are fit by a single line, the gradient is small and negative in slope. Thus, the interpretation of this gradient depends critically on the significance of the tributary flow system. The isotope data for WT-7 show the largest change between 870 and 990m and this corresponds to the comment for sample #8, "Possible input of material from another feeder 200m east" (Table 2, TRAC-Pt 6). The evidence for this tributary feeder is described on p 29 of TRAC-Pt6, "about 800m away from the WT-7 vent.... it was apparent in the field that there was a "tributary" input from another "feeder".... located about 200 m east". The field evidence was a change to more densely cemented clasts in contrast to looser cementation higher in the profile. There is no indication that the lateral extent of this region of denser cementation was determined and there appear to be errors in the description and location of this tributary. Figure 10 of TRAC-Pt6 shows the individual sample localities along the WT-7 profile and sample #8 is shown to be approximately 1200m from the WT-7 vent, not 800 m. Furthermore, the second feeder is shown to be approximately 400 m to the east, not 200 m. If the arrow designating flow from the second feeder is accurately shown in fig. 10, then it appears that surface fluids would have to flow across the slope in order to intersect the main profile at a point 800-1000 m from the WT-7 vent. The second feeder is shown to be directly up hill from a small wash that parallels rather than intersects the wash of the main profile. It appears that fluids issuing from this fault might not flow into the main profile.

At best, the profiles described here can be considered preliminary. These trends are suggested by the data, but they may be fortuitous. It would not be difficult to evaluate this. I recommend that carbonate textures and the percentage of carbonate be mapped. Samples should be carefully collected along each profile and laterally away from the profiles. Carbonates from up slope of the faults should be included. Distances and topography must be accurately recorded. Samples should be collected at known distances below the surface. Quade et al. (1989) report a significant decrease in both $\delta^{18}O$ and $\delta^{13}C$ within 50 cm of the surface and this effect should be evaluated at each profile. Samples should be evaluated for isotope heterogeneity.

4. **The isotopic composition of groundwater.** Benson and Klieforth (1989) report O, C, and H isotope ratios, and ^{14}C ages of groundwater from 12 drill holes beneath or within 10km of Yucca Mt. and at depths as great as 1800m. The stable isotope data are plotted in Appendix A of the 1992 NAS Report (figs. 1, 2, 5, 6, p.152-158). Figs. 1 and 2 (NAS, 1992) show that modern precipitation averages about 2‰ higher in $\delta^{18}O$ than groundwater. This difference can be explained by selective recharge of the aquifer with lower $\delta^{18}O$ winter precipitation and by the ages of the groundwater, 4-18Ka, that suggest a major component of the groundwater dates from the last glacial period when precipitation was isotopically lighter (see figs. 11, 17, 18 of Benson and Klieforth). Figs. 5 and 6 (NAS, 1992) plot the $\delta^{18}O$ and $\delta^{13}C$ compositions of groundwater and the calculated compositions of calcite that would be precipitated by this groundwater under conditions of equilibrium at 25°C. Figures 6 and 7 (NAS 1992) show the isotopic compositions of calcites from localities at or near Yucca Mt. including Trench 14, soil carbonates and drill core. The newer carbonate data from profiles at Bare Mt. and WT-7 would also plot in this field. All of these Yucca Mt. carbonates plot at higher $\delta^{18}O$ values than could be precipitated from measured groundwaters. Most of the Yucca Mt. carbonates are 4-6‰ too high, but some are over 10‰ too high. This discrepancy cannot be explained by variable temperature; if carbonates precipitated at higher temperatures (above 25°C) the difference becomes greater and even at 15°C, there is no overlap between calculated and measured compositions. In the absence of any suggestion of disequilibrium, I agree with the conclusion of the NAS 1992 Report, that carbonates at Yucca Mt. were not precipitated from the analyzed modern groundwaters.

The question remains, could the carbonates at Yucca Mt. have been precipitated by fluids of different composition from the modern groundwater? Climate change is shown to be insufficient to explain the discrepancy between measured and calculated $\delta^{18}O$ for carbonate. If groundwaters were dominated by modern precipitation, or precipitation from some earlier warm period, values of $\delta^{18}O$ might be 2‰ higher. Likewise, if waters have undergone extremes of evaporation, the $\delta^{18}O$ will be higher, though this should create a diagnostic trend in $\delta^{18}O$ vs. δD. If degassing of CO_2 is important, a trend will also be seen in $\delta^{18}O$ vs. $\delta^{13}C$ (as shown by Hill et al. 1995, fig 13B, for Site 199). A reasonable interpretation of the formation of high $\delta^{18}O$ values of pedogenic carbonate at Yucca Mt.

is a combination of low temperatures for mineral growth (<25°C) and evaporation of water before mineral growth. This model does not explain cements precipitated by warm upwelling waters in an epigenic process.

The epigenic model requires waters of higher $\delta^{18}O$ due to some other process. One possibility would be from a hydrothermal system that has interacted with wallrock at high temperatures such that isotope exchange occurred. Fig. 3 (NAS 1992, p.154) shows high $\delta^{18}O$ from Salton Sea geothermal fluids for example. Such values only form if the geothermal system is "rock-dominated", ie. the fluid to rock ratio is low enough that the higher $\delta^{18}O$ values of the rock dilute the $\delta^{18}O$ values of the fluids. If a significant amount of hydrothermal fluids were formed by such a process at Yucca Mt. and subsequently precipitated the calcites, then there must be a large volume of hydrothermally altered rocks at depth with low $\delta^{18}O$. Such altered low $\delta^{18}O$ rocks are easily recognized and commonly described for areas of hydrothermal activity. I am not aware of data testing this possibility in silicate minerals at Yucca Mt and I support the recommendation of the 1992 NAS Report (p. 56-57) that additional studies be made to document the composition of *paleo* ground-water.

Summary. You have asked four questions in summary of my findings.

1. **Are there significant new data since the 1992 NAS Report?** As discussed above, there is a significant quantity of new stable isotope data since 1992. However, the geological significance of these data is limited by certain critical interpretations regarding the timing of carbonate precipitation, the homogeneity of carbonate cements, the uncertainty of linear-fit lines to small data sets, and the composition of paleo-groundwaters.

2. **What is the quality of these data?** The accuracy and precision of stable isotope data appears to be high. Analyses were made in well known and respected labs. However, the failure to report accurate or detailed field data, or petrographic descriptions compromises interpretation of the data.

3. **How much credence do these data lend to the hypothesis of ongoing, intermittent hydrothermal activity at Yucca Mt.?** I do not see any new data in the nine S+D documents that adds credence to the hypothesis of on-going hydrothermal activity. These documents were frustrating and confusing to review. They rely heavily on unpublished documents, which are difficult to obtain and which are loosely interpreted, sometimes with a misleading effect. Important dissenting information, including much of the 1992 NAS Report, is not mentioned or discussed. In many instances, these documents make conclusions that are so strong as to seem divorced from the preceding data and discussion. In the end, I conclude that the three most important lines of stable isotope evidence from the S+D documents (#1-3, above) yield ambiguous results. However, the carbonate cements in drill core were precipitated by fluids that are not well understood. The only stable isotope evidence that argues *against* on-going hydrothermal activity is the oxygen isotope composition of modern groundwater (#4, above). It is presently uncertain if groundwaters of different composition existed in the past 100Ka which could have precipitated the cements formed in drill core.

4. **If these data significantly affect the conclusions of the 1992 NAS Report, how can the issue be resolved?** I have three recommendations, two of which were made in the NAS Report. While I do not think that the data reported in the 9 S+D documents can be shown to significantly affect the conclusions of the 1992 NAS Report, there are two major uncertainties remaining that are *permissive* of interpretations that would significantly affect those conclusions. 1. If the poorly constrained gradients measured at Bare Mt. and WT-7 can be reproducted, I believe that would strongly argue against a pedogenic model. 2. If carbonate cements can be found in drill core that have appropriate values of $\delta^{18}O$, temperature of formation and age, this could indicate the former existence of groundwater higher in $\delta^{18}O$ than those measured today. Such a discovery could seriously contradict a major conclusion of the 1992 NAS Report, that no known groundwater has the correct $\delta^{18}O$ to precipatate the carbonates at Yucca Mt.

1. There should be an independent Science Coordinator for the Yucca Mt. project (see p. 142 NAS Report). This is necessary both to curate and disseminate the vast amount of research that has already been completed, and to coordinate any new research. Many excellent studies are contained in unpublished DOE, USGS, and other reports. A Coordinator familiar with this literature would be in the position to work with the on-going site characterization in the tunnel, and to recognize and promote related studies.

2. There should be a coordinated study of carbonate cements from surface and drill hole samples involving analysis of fluid inclusions, stable isotope ratios and U-Th ages in the same carefully described samples (see p. 56-57 in NAS Report). Such a study would resolve the ambiguities that remain at sites such as Trench 14, Busted Butte, Bare Mt., and WT-7. The goals of these studies would include evaluating the proposed profiles in near-surface cements both up- and down-slope from faults, and evaluating the compositions of paleo-groundwaters.

3. Some of the samples chosen for stable isotope analysis should be tested for heterogeneity and isotope zoning. These tests should involve multiple analyses from different domains of single hand samples (1-5mg samples), micro-drilling 5-10μg samples), and ion microprobe analysis (5ng samples). The insitu microanalysis should be guided by imaging such as cathodoluminescence, UV optics, scanning electron microscopy, back scatter electron, and X-ray mapping of chemical composition.

Respectfully submitted,

John W. Valley
Professor and Chairman

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TRAC-Pt6 Symanski, J.S. and Dublyansky, Y.V. (1996) Stable isotope gradients in slope calcretes at Yucca Mountain, Nevada. Report submitted to the Nevada Nuclear Waste Projects Office (NWPO).

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May 21, 1998

Dr. Leon Reiter
Nuclear Waste Technical Review Board
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Arlington, VA 22201-3367

Dear Dr. Reiter:

This letter supplements my letter of Dec. 18, 1997 regarding the possibility of hydrothermal activity at Yucca Mountain. I have read two additional unpublished manuscripts that were not available to me at that time.

The manuscripts are:
1) Dublyansky (1995) Stable Isotopic Composition of Carbonates Exposed in Trenches at the Stagecoach Road Fault, 26 p.
2) Dublyansky and Lapin (1995) Bedrock Tuffs, Mosaic Breccias, and Young Volcanic Rocks at Yucca Mountain: Field Observations, Petrography, and Chemistry, 174 p.

Dublyansky (1995) presents stable isotope data from the Stagecoach Road locality. In 1997, I commented that I had not seen these data (p. 4).

In Fig. 3, Dublyansky presents analyses of oxygen and carbon isotope ratios for 4 samples in a traverse away from a "feeder". No other evidence is presented for the existence of this feeder. On p. 6, it is stated that "both carbon and oxygen become heavier with distance away from the suspected orifice". Examination of the Appendix (p. 25) shows that this statement is not supported by the data. These results do not change my previous interpretations. As discussed in my previous letter, the data set is too small, the scatter is too large, and homogeneity and sample locations are too poorly documented to interpret this figure as supporting fluid expulsion from a "feeder". The gradients based on these data (reported in Table 1 and in other reports) are not sufficiently well constrained to be meaningful in this context.

Fig. 5 shows analyses of 6 samples in the 3 meter depth profile pictured in Fig. 4. It is argued that pedogenic cements precipitate so slowly that a 3m thickness would require 0.4 to 8.5 Ma to form, representing periods of variable climate, and would thus be more variable in isotope ratio. The data in Fig. 5 are from 4 macroscopically distinct units (Fig. 4) and do, in fact, show significant variability. As discussed before, without petrography at the appropriate scale for the exact samples that were analyzed, these data cannot be interpreted with certainty.

Figs. 6B, 9B and 10 show a positive correlation of $d^{18}O$ and $d^{13}C$. Fig. 7 is blank in my copy of this manuscript. The positive correlations are interpreted to support the epigenic model favored by the author. However Fig. 5B shows a weak negative correlation and there are processes not discussed by the author. Quade and Cerling (1990, Science) show positive correlations for pedogenic cements. This argument is not convincing.

Dublyansky and Lapin (1995) present many pages of photo and text description of samples from Yucca Mountain and vicinity. Most of these data are for hand specimens that are not located in any detail. Some samples have outcrop-scale locations and some are from thin section examination. Few samples have any supporting geochemical data.

It is concluded in many places throughout the descriptions that hydrothermal fluids were involved in the genesis of some samples. However, data are not presented to document the time of fluid interaction. For most of the samples, the fluid event could have been shortly after igneous crystallization. Hydrothermal alteration is common at all stages of cooling volcanic rocks. Thus, in the absence of information on timing, these observations are not relevant to the epigenic vs. pedogenic controversy.

On p. 158-160, the conclusions of Levy and Naeser (1991) are challenged regarding the genesis of breccias. Levy and Naeser interpret the CTM breccias as formed largely by settling of pyroclastic units shortly after deposition. In contrast, Dublyansky and Lapin state that in most instances, CTM breccias are associated with fault movements. Dublyansky and Lapin support their conclusion with only two sentences, while Levy and Naeser provide detailed text. For AMC breccias, Levy and Naeser report abundant root casts, while Dublyansky and Lapin report: "We did not find any root cast in breccia cement". Considering the direct criticism implied by Dublyansky and Lapin's statements, I am surprised that they do not discuss the pictures interpreted as root casts that are shown by Levy and Naeser. I think it is unfortunate to make such a criticism without examining the samples of Levy and Naeser. It is not clear if they challenge the interpretation of Levy and Naeser, if their samples are simply different from those of Levy and Naeser, or if they failed to recognize root casts in their samples. I do not find the statements on p. 158-160 to be sufficiently detailed to support these criticisms.

Ten conclusions are made by Dublyansky and Lapin on p. 163-165. Hydrothermal activity is proposed, but again without reference to timing. Complex episodes of hydrothermal activity and chemical alteration are recognized as would be expected for a volcanic terrane. "Pipe-like shape" breccias believed to be of "hydrothermal explosion" genesis are mentioned, but not described, and again, without information on timing.

Summary: In answer to your four specific questions:

1) **Are there significant new data since the 1992 NAS Report?** I find these data less significant than those discussed in my previous letter and subject to the same limitations.

2) **What is the quality of these data?** My opinion is the same as before.

3) **How much credence do these data lend to the hypothesis of ongoing, intermittent hydrothermal activity at Yucca Mountain?** As before, I do not see any data in these two documents that adds credence and I am disappointed by the scholarly quality of the reports.

4) **If these data significantly affect the conclusions of the 1992 NAS Report, how can the issue be resolved?** In my Dec. 18, 1997 letter, I made three recommendations that I consider important for further resolving the issues surrounding fluids at Yucca Mountain. My recommendations are not changed.

Respectfully submitted,

John W. Valley

John W. Valley
Professor and Chairman