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MEMORANDUM FOR: James L. Blaha, Assistant for Operations
Office of the Executive Director for Operations

FROM: Robert M. Bernero, Director
Office of Nuclear Material Safety and Safeguards

SUBJECT: TRANSMITTAL OF LETTER AND ANALYSIS FROM THE CHAIRMAN OF THE NATIONAL RESEARCH COUNCIL RELATIVE TO THE SZYMANSKI REPORT

The enclosed subject information is to be submitted to the Commissioners' Assistants for their use. It is the latest in a series of reviews and responses related to the reports by Jerry Szymanski. As you will recall, Mr. Szymanski (formerly of the U.S. Department of Energy, Yucca Mountain Project Office) proposed a model for groundwater upwelling at Yucca Mountain. The ideas of Mr. Szymanski were the subject of an in-depth review by a panel of scientists sponsored by the National Research Council, and that review resulted in a finding of no evidence to support Mr. Szymanski's ideas. Dr. Press' letter and the accompanying analysis by a member of this staff, Ms. Ina Alterman, are the most recent affirmation of the results of that panel's findings.

If you have any questions, you may contact B. J. Youngblood of my staff at 504-3408.

Original signed by
Guy A. Arlotto

Robert M. Bernero, Director
Office of Nuclear Material Safety
and Safeguards

Enclosure:
As stated

CJP 2/1/93 #2

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NATIONAL RESEARCH COUNCIL

2101 CONSTITUTION AVENUE WASHINGTON, D. C. 20418

OFFICE OF THE CHAIRMAN

January 7, 1993

Charles B. Archambeau
University of Colorado - Boulder
Department of Physics
Theoretical and Applied Geophysics Group
Campus Box 583
Boulder, CO 80309

Dear Arch:

I am writing in response to your letter of 19 November 1992 in which you take issue with the National Research Council's report on the ground water at Yucca Mountain.

I must say that I was surprised not to see any reference in your letter to the United States Geological Survey Open File Report 92-516 by our mutual colleague and friend, Jack Evernden, who, at your request, reviewed your "Minority Report" in which you strongly support J. Szymanski's ideas. Remarkably, I feel, Jack independently corroborated most of the National Research Council's panel's observations, analyses and conclusions, including the geochemical and mineralogical review in the Academy's report that you challenge. He also found no evidence to support the contention that deep thermal waters have risen to the surface periodically over thousands of years in the Yucca Mountain area. In my view, this simply reaffirms that science properly done is reproducible.

I appreciate your interest in the report but, based on the available field and other scientific evidence carefully considered by a properly constituted panel of experts in the appropriate fields of specialization, and reaffirmed by Jack's independent, detailed study, I see no reason to question the NRC report's conclusions. I enclose a longer critique of your letter prepared by the NRC staff in consultation with our panel.

Yours sincerely,

Frank

Frank Press
Chairman

Enclosure

NATIONAL RESEARCH COUNCIL
COMMISSION ON GEOSCIENCES, ENVIRONMENT, AND RESOURCES
2101 Constitution Avenue Washington, D.C. 20418

FILE

BOARD ON
RADIOACTIVE WASTE MANAGEMENT
(202) 334-3066 Fax: 334-3077

Office Location:
Milton Harris Building
Room 456
2001 Wisconsin Avenue, N.W. 20007

January 6, 1993

MEMORANDUM

TO: Frank Press

VIA: Stephen Rattien
Executive Director
Commission on Geosciences, Environment, and Resources

FROM: Ina B. Alterman
Senior Project Officer
Board on Radioactive Waste Management

SUBJECT: Analysis of the Letter and Report from Charles B. Archambeau

Charles Archambeau's letter and report of 19 November, 1992, critiquing the National Research Council's report, "Ground Water at Yucca Mountain: How High Can It Rise?" raises serious charges that question the scientific integrity of the panel that did the study. It is unfortunate because public attacks by one scientist on the personal integrity of scientists who disagree with his theories and interpretations of evidence can only reflect poorly on the scientific community in general and on that scientist, in particular.

In considering the issues raised by Archambeau, several facts should be kept in mind:

1. Archambeau's review of the National Research Council report, Ground Water at Yucca Mountain: How High Can It Rise, attached to his letter and described by him as "a brief synopsis" of a report for the State of Nevada is, in fact, the full report, according to the copy we have obtained elsewhere. The review attached to his letter is missing only the title page which identifies it as a product of TRAC, a consulting firm formed by J. Szymanski and his small group of supporters, under contract to the State of Nevada.

2. There is no reference, in either the letter or accompanying report, to the United States Geological Survey Open File Report 92-516, written by Archambeau's (and your) friend, Jack Evernden, at Archambeau's request to review his "Minority Report" written as a member of the Department of Energy's External Review Panel that evaluated J. Szymanski's ideas on upwelling ground

water at Yucca Mountain. You will recall, Evernden independently corroborated many of our panel's observations, analyses and conclusions, including the geochemical and mineralogical issues Archambeau challenges in the Academy's report. Evernden also found no evidence to support the upwelling of thermal waters in the Yucca Mountain area.

3. Although there are several points of disagreement, the major focus of Archambeau's criticism is geochemical and mineralogical, which are outside his area of expertise. In this era of sub-specialization, it is critical that there be an appropriate match of scientific discipline and professional experience with the issues at hand in complex scientific issues. Archambeau's letter and review reflect a lack of understanding of the relevant scientific knowledge necessary to comprehend the geologic and geochemical evidence, which underscores his lack of formal training or experience in these areas of earth science. An example is his argument about the wide range of zircon ages in the carbonate cements in Trench 14 and Busted Butte, which to most geologists indicates multiple source rocks of varying ages for the zircons. By some leap in logic Archambeau attributes with "very high confidence" (p. 4 of his report) the younger ages of zircons to "heating of this crystal" (p. 4 of his report). His view that rising hot waters from great depth could reset some zircon isotopic "clocks" and not others in the same sample is a clear indication of his lack of understanding of the process required. Moreover, the high temperatures necessary to reset the isotope ratios (the "clock") require great depth and could not be attained at such shallow depths as Trench 14 and Busted Butte.

Parenthetically, considering the range of uncertainty in the isotopic dating of the zircons it is possible for all of the zircons to fall within the time of silicic volcanism in the region. Thus, there is no need to evoke later "thermal waters" to reset the clocks.

4. In violation of sound geologic practice Archambeau ignores direct geologic evidence, cited by both the panel in its report and by Evernden in his, that clearly demonstrates the surface origin of the water and materials that produced the carbonate veins. Such direct evidence as the mineralogic composition of the veins (carbonate with quartz sand, clay and volcanic ash inclusions that could come from no other source than the overlying soil), carbonate mineral grain size (three orders of magnitude smaller than well-known hydrothermally-produced veins known world-wide); structure (veins thinning downward and becoming discontinuous); and relations to surface-parallel carbonate deposits (carbonate

deposits uphill of the veins as thick as those downhill) unequivocally demonstrate the surface origin of the water and materials that produced the veins and surface-parallel carbonates in question. Instead of these clearly observable features, Archambeau invokes indirect, more abstract geochemical data that are highly subject to interpretation and that require the special training and knowledge of geochemistry to understand. Parenthetically, in the meeting of our panel with the DOE 5-member External Review Panel referred to in Archambeau's letter and report, his colleague in producing the "Minority Report" of that panel, Neville Price, wagered that the Trench 14 vein continues downward two hundred feet. Since then, deepening of the trench showed that the vein disappears into thin stringers 15 feet below.

5. Although trained and experienced in theoretical seismology in which he has achieved a high reputation, Archambeau offers no criticism of the panel's views on earthquake models or other areas in which he has expertise. In this connection, however, he accuses the panel of misrepresenting his stated minority position in the meeting of May 31, 1991, with the External Review Panel, relying on his recollection of the discussion reported in his notes. Our panel, however, relied on the transcription of the taped meeting which reaffirms that Neville Price, his "minority" colleague, agreed that the seismic pumping mechanism cannot account for the volume of carbonate in the Yucca Mountain area. Archambeau did not contradict this statement at any time in that meeting and, moreover, stated later during his presentation to our panel that Szymanski must invoke thermal convection to explain the isotopic composition if the carbonates, and that the minority believes that hydrothermal convection is the "only feasible mechanism" to get that volume of carbonate.

They made at least two other changes to their publicly stated positions at that meeting:

(a) They voluntarily announced that Szymanski no longer believed the Trench 14 and Busted Butte veins were formed by the rise of ground water along fractures. They now believe that the veins were formed by downflowing water that came from ground water that rose up from a fracture somewhere "uphill." That no fractures "uphill" have been found seems of little consequence to them. (This is like a scientific shell game: as soon as one idea is proven wrong, they change the argument.)

(b) Queried about a statement attributed to him in The New York Times that "you could blow the top off the (Yucca) mountain", Archambeau claimed not to remember saying it, then called it "a figure of speech", then rephrased it to say that if water rose up into a repository, and "if you got lots of breakage", and if the water were superheated, and if CO_2 rose with the water, then "you could get leakage .... into the environment rather quickly." He then admitted that he is not an expert on these matters but could imagine such scenarios.

6. It is common knowledge that, with but one exception, no independent earth scientists with expertise critical to understanding the evidence in the Yucca Mountain region, i.e., expert geochemists, soil scientists, mineralogists, and volcanologists, have peer reviewed and commented on Szymanski's reports or Archambeau's iterations of them prior to submitting them to DOE, the State of Nevada, or the media. The only exception is Evernden, whose review of Archambeau's "Minority Report", as I stated above, supports the Academy panel's conclusions and refutes Archambeau's. As is well known in the sciences, the peer review process in scientific publication is necessary to evaluate objectively the quality and credibility of scientific studies regardless of author. Archambeau no doubt demands it in his own area of expertise. Does Archambeau believe, one wonders, that areas related to the study of Yucca Mountain are less deserving of proper scientific scrutiny, so that he and his small group of Szymanski supporters choose to ignore this means of objectively assessing the validity of their use of data, the consistency of their conclusions, and the overall basis of their arguments? None of his, or Szymanski's, writings on Yucca Mountain for that matter have ever been published in the scientific literature.

It is recommended that the National Academy of Sciences stand by the scientific validity and integrity of the report on Yucca Mountain ground water. Our confidence is based on the careful internal and external objective scientific scrutiny of our report, independent corroboration by other scientists like Evernden and the majority of the DOE External Review Panel, and on the expertise of the earth scientists on the NAS panel, who reflect the diverse areas of specialization required to understand the complex geologic issues that the report addresses.

The scientific caliber of our panel can be measured by the fact that during the two years of the panel's review of Szymanski's ideas and supporting evidence, five members of the panel were honored by their peers for their contributions to

their respective fields of specialization predating the Academy's study. These include Robert Fournier, awarded the American Geophysical Union's highest Geochemistry/Volcanology award, the Bowen Award, for his contributions to the concepts and understanding of geochemical and hydrochemical aspects of geothermal processes; Robin McGuire, elected president of the Seismological Society of America for his outstanding work in seismic risk assessment; George A. Thompson, elected to membership in the National Academy of Sciences for his contributions in a lifetime of research in geophysics; John Bredehoeft, elected to the Russian Academy of Sciences for his contributions to hydrologic modeling; and Brian Wernicke, awarded the Young Scientist Award by the Geological Society of America for his contributions to the field of geology despite his youth. Under no circumstances would these individuals compromise their scientific integrity for any issue.

November 19, 1992

Dr. Frank Press, President
National Academy of Sciences
2101 Constitution Ave., NW
Washington, DC 20418

Dear Frank:

I expect that this letter may come as a surprise to you since the subject matter is a little unusual. However, I feel obligated to communicate on an important issue affecting both the Academy and, more importantly, the nation. The issue in question involves the suitability of the purposed Yucca Mountain site as a nuclear waste repository. This issue has recently been the subject of an Academy study by the Panel on Coupled Hydrologic/Tectonic/Hydrothermal Systems at Yucca Mountain, culminating in a report entitled Ground Water at Yucca Mountain: How High Can It Rise? I feel strongly, along with a number of my colleagues, that this report is poorly done and misleading and will adversely affect both the Academy and the country's program for nuclear waste disposal.

I expect that you might be interested in understanding the basis for our concerns. In this regard, I hope this letter and attached report will be adequate to allow you to evaluate our concerns, at least to the extent that it may be possible for you to conclude that there may be a serious problem with the Academy report.

By way of introduction I think some background on the subject and on my experience and current activity in this area would be helpful. I have devoted most of the remainder of this letter to those considerations, while the attached short report is directed to a technical review of the Academy report itself.

In regard to the background information, it seems appropriate to mention that I have recently been involved in a DOE-sponsored study of the proposed repository at Yucca Mountain. The study was primarily an evaluation of work by J. S. Szymanski, a former DOE geologist, who has developed a model for the geologic and hydrologic evolution of the site based on hydro-tectonic interaction processes. The model he proposes predicts episodic upwelling of ground water at the site in response to major tectonic events; that is moderate to large earthquakes and/or volcanic activity. His model incorporates both seismic pumping and gas-assisted, fracture-controlled, thermal convection. Ground water upwelling (in large volumes) is a critical issue, since if there is a likelihood of water flooding the site within the first few thousand years after emplacement of the waste, then the site would not be suitable as a repository, at least not if current licensing regulations are applied.

Szymanski's evidence in support of his model consists of a rather large body of geological and geochemical data relating to past hydrothermal alteration of the rocks at

this site and the quite abundant calcretes and opal-rich calcium carbonate veins observed at the surface and at all explored depths within the mountain. In this regard, if this model is relevant then there should be evidence of *recent* hydrothermal alteration at the site. Furthermore, there should be evidence that the calcite-opal veins and the calcretes are primarily products of *upwelling* ground water. However, if there is no evidence of recent hydrothermal activity and if the pervasive occurrences of calcretes and opal-calcite veins can be better explained by some other depositional mechanism, then it would be unlikely that the processes involved in the model are operative at this site. In this regard, the alternate explanation for the occurrence of calcite deposits at the site is that the calcites and opal have been deposited by rain water evaporation, with the calcium in solution originating from windblown dust accumulations at the surface.

Thus, the issue is whether or not there has been recent, episodically occurring, hydrothermal activity younger than 10 million years and continuing into the Quaternary and whether the observed calcite and opal deposits are produced by upwelling ground water or whether the calcites are produced instead by descending rain water containing the necessary amount of calcium derived from wind-blown dust.

In the course of reviewing Szymanski's model and the data he presented in its support, I became convinced that the model, while not very quantitative and while lacking in detail, was nevertheless a credible explanation of the observations and that these observational data indicated recent hydrothermal activity at the site. During the course of this review I spent considerable time in the field looking at the geologic evidence bearing on the issue, both that cited in support of his model and that, as argued by others, said to be against it. In addition, I reviewed many reports and papers from the DOE, the USGS and independent investigators concerned with the suitability of the site.

The five-man DOE review panel on which I served ended up with two reports to the DOE, a minority report by Dr. Neville Price and myself and a majority report by Dr.'s Dennis Powers, John Rudnicki, and Leslie Smith. Our minority report strongly supported Szymanski's ideas while the majority report was critical of them in many respects.

Since our submission of these reports in November of 1991, Szymanski has produced other major reports which provide additional data and arguments in support of his interpretations. We have reviewed these new reports along with many recent papers and reports generated by others. These reviews and related investigations have been conducted with support from a contract with the State of Nevada.

Because of the Nevada contract support we've been able to put a good deal of time and effort into an "in-depth" assessment of the emerging geologic evidence for tectonically triggered upwelling of water at the site. We also have been able to engage in computer modeling of this type of phenomenon. These recent investigations have strengthened my previously held conclusions.

As a part of this continuing effort we have reviewed the National Academy of Sciences' report generated by the Panel on Coupled Hydrologic/Tectonic/Hydrothermal Systems at Yucca Mountain, as previously noted. The Panel report contained a large number of strong conclusions, with the overall conclusion stated as follows (p. 3):

> *"The panel's overall conclusion was that none of the evidence cited as proof of ground-water upwelling in and around Yucca Mountain could be reasonably attributed to that process. The preponderance of features ascribed to ascending water clearly (1) were related to the much older (13-10 million years old (Ma)) volcanic eruptive process that produced the rocks (ash-flow tuffs) in which the features appear, (2) contained contradictions or inconsistencies that made an upwelling ground-water origin geologically impossible or unreasonable, or (3) were classic examples of arid soil characteristics recognized world-wide."*

I, along with my co-investigators, have taken strong issue with the Academy report in general and with this conclusion in particular. In fact, we disagree with most of the conclusions and recommendations made in the report. Therefore, this is not what might be termed a "disagreement about scientific details" but major criticism directed at the Panel for their disregard of critical data that was available and known to them, their misrepresentation of other data and results, and the use of equivocal and often contradictory field "observations" and data to draw very strong conclusions and recommendations.

In order to be specific about our criticisms, I've attached a brief synopsis of our review of the Academy report to this letter. This synopsis focuses on only the major problems we have with the report. A more expanded and detailed review of the NAS report, contained in a recent report to the State of Nevada, Nuclear Waste Project Office, by Technology and Resource Assessment Corporation, is available should you wish additional information. I believe that the issues raised in this review are of sufficient importance to warrant your personal attention. In fact, I hope that you will agree that they are such that a re-evaluation of the Academy Panel report, by the Academy itself, is appropriate.

Sincerely Yours,

Charles B. Archambeau

Charles B. Archambeau
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Theoretical and Applied Geophysics Group
University of Colorado-Boulder
Campus Box 583
Boulder, CO 80309

cba/rmf
Attachment

Review of the NAS/NRC Report: "Groundwater at Yucca Mountain: How High Can It Rise?"

by

Charles B. Archambeau

There are three basic and serious problems that produce disagreement with the conclusions and recommendations of the Academy report. These are: *First, the report ignores a considerable body of critical data relating to the ages and nature of hydrothermal alterations at the site; second, many of the strong conclusions expressed in the report are not reasonably supported by the evidence presented and, in some cases, are inconsistent with data and results available to the committee but which are not cited or used by them; and finally, there are statements describing field relationships and data that are not consistent with the facts or are made in such a way as to be misleading.*

Zircon Age Data: Evidence for Hydrothermal Activity

An example of what can be regarded as a misleading characterization of data is given on page 44 of the report. The Academy Panel states:

> *"Fission - track dating of eroded fragments of (or detrital) zircons found in carbonate that cements AMC - type fault breccia at Trench 14 and at Busted Butte gives a spread of ages showing heterogeneity of source material, with some zircon ages older and some younger than the age of the bedrock in the immediate region (Levy and Naeser, in press). However, within the analytical uncertainty, most of the ages are about 10-12 Ma, or about the same as those of the dominant volcanic rocks in the region."*

However, the Levy and Naeser reference states (p. 17):

> *"The spread in ages from each sample indicates that there are zircons from multiple sources present. In both samples there are crystals significantly younger and significantly older than the age of the tuff."* (Emphasis added.)

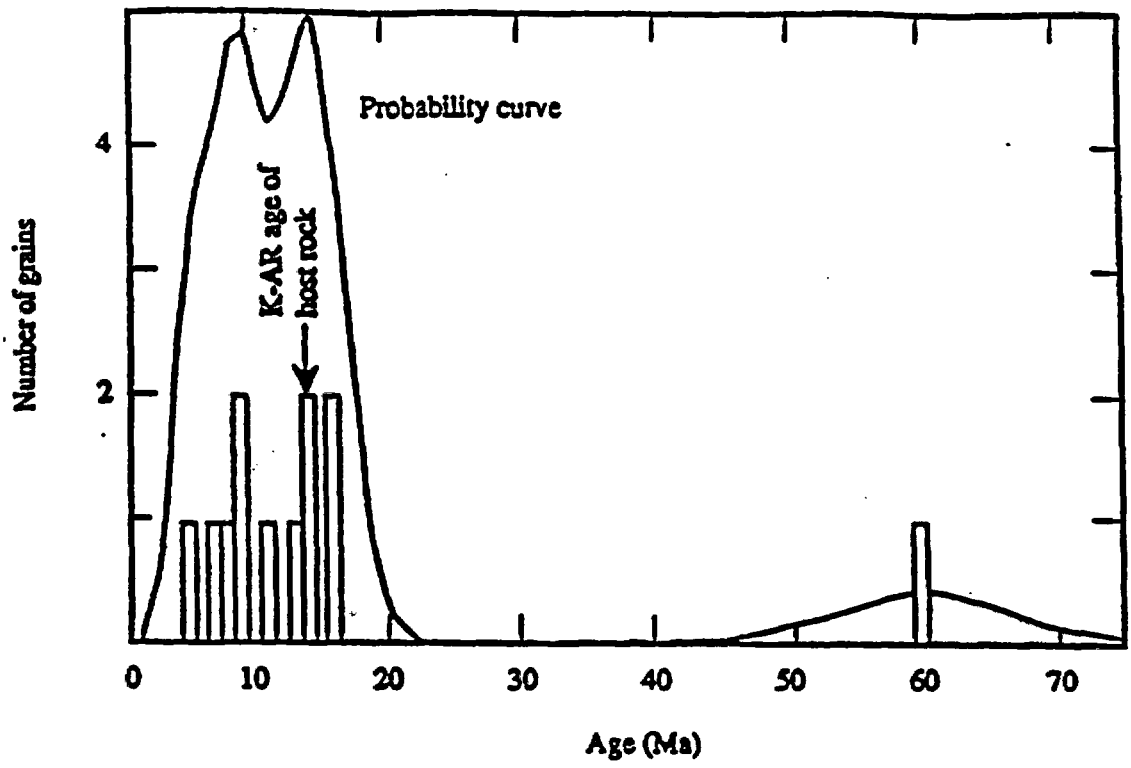
In the following paragraph Levy and Naeser go on to show plots of these data and state the basis for their confidence in the observed spread in zircon ages as follows (references quoted are omitted):

> *"One way to illustrate the spread in the ages is through the use of a probability density distribution plot. The probability density plot sums the normal distribution curves for all the grains in a sample. These curves are calculated from an age and its standard deviation. Figure 6 shows an example of a sample with a single age population; the Fish Canyon Tuff zircons are used as a primary age standard for most fission-track laboratories in the world and the probability curve exhibits a normal distribution. In contrast, samples HD-41-4 and HD-74-2 both show multiple age peaks (Figures 7 and 8). The ages of the individual grains are shown in the histogram beneath the probability curves for all three samples."*

The data shown by Levy and Naeser in their Figures 7 and 8 are reproduced in the attached Figure 1. These data clearly show the multiple peaks identified by Levy and Naeser. *Contrary to what is stated by the Panel*, most of the zircon crystals analyzed from each sample show dates considerably less than the Potassium-Argon ages of the host tuff (13 Ma), rather than greater than the age of the tuff. Further, the Panel implies an age for the host tuff of 10-12 Ma, while it is clearly stated to be 13 Ma.

As seriously misrepresentative is the neglect of the Panel to indicate that the authors clearly use the term 'significant' in a technical sense. In fact, the Panel report does not even mention that the authors themselves attach significance to peaks in the distribution and that they do *not*, in any way, suggest that *"within the analytical uncertainty the ages are about the same as those of the dominant volcanic rocks in the region."* This is the Panel's statement, but they do not distinguish this assertion from the previous sentence referencing the paper by Levy and Naeser. They thereby induce the reader to assume that this statement is consistent with the results of the authors. In this way they do not have to explain why their characterization of these data is different from that given by the authors, or even mention that a difference exists.

An examination of the age data, as given in Figure 1, shows that there are ages 4.8 Ma, 6.2 Ma, 7.5 Ma, and 7.7 Ma among the crystals in these two samples. There are



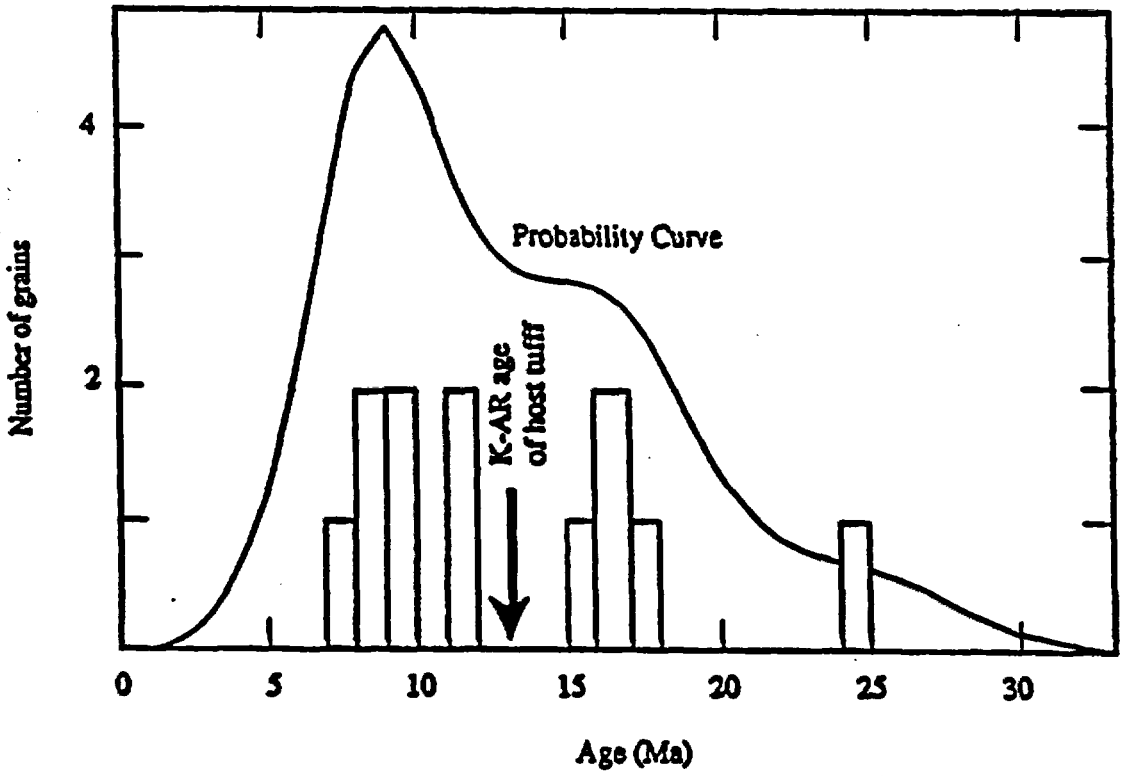


Figure 1. Fission track ages of zircons from breccias at Busted Butte (top) and Trench #14 (bottom). From Levy and Naeser, 1991.

several additional dates near 8.5 Ma. The two sigma interval attached to the youngest age, of 4.8 Ma, is 2.5. Thus, there is very high confidence (over 90%) that the age of heating of this crystal was between 2.3 Ma and 7.3 Ma, with the highest probability for a specific age being 4.8 Ma. The same interpretation of confidence intervals applies to the other ages given. Clearly, characterizing these age data as being within the age range 10-12 Ma, given "analytical uncertainty," is incorrect. It is on this inaccurate basis that the Panel states that (p. 3):

> *"The preponderance of features ascribed to ascending water clearly (1) were related to the much older (13-10 million years old (Ma)) volcanic eruptive process that produced the rocks (ash-flow tufts) in which the features appear,..."*

This conclusion is actually directly contradicted by the age data cited.

This issue is extremely important in that these are the only age data used in the NAS report to substantiate the claim that the last and final hydrothermal event occurred some 13 to 10 Ma ago. Age data from uranium series dating of calcites from veins at depth as well as potassium-argon dates from zeolites, which are commonly produced by hydrothermal alteration of volcanic glasses, were ignored by the Panel. However, as shown in Figure 2, many young ages are present in these data as well, some as young as 30 ka. In view of the preceding description of what is actually represented in the zircon age data, and in view of the zeolite and calcite vein age data, it is evident that high temperature annealing of fission tracks occurred at times much more recently than 10 Ma and that related hydrothermal alteration produced the observed young zeolites along with the recent calcite and opal veins throughout the mountain. Indeed, it is likely that analysis of additional zircon samples would show more recent ages, like the age data from the zeolites and calcites. Therefore, contrary to the Panel's statements, the age data actually support the occurrence of recent (post-Timber Mountain) hydrothermal activity rather than providing evidence against it.

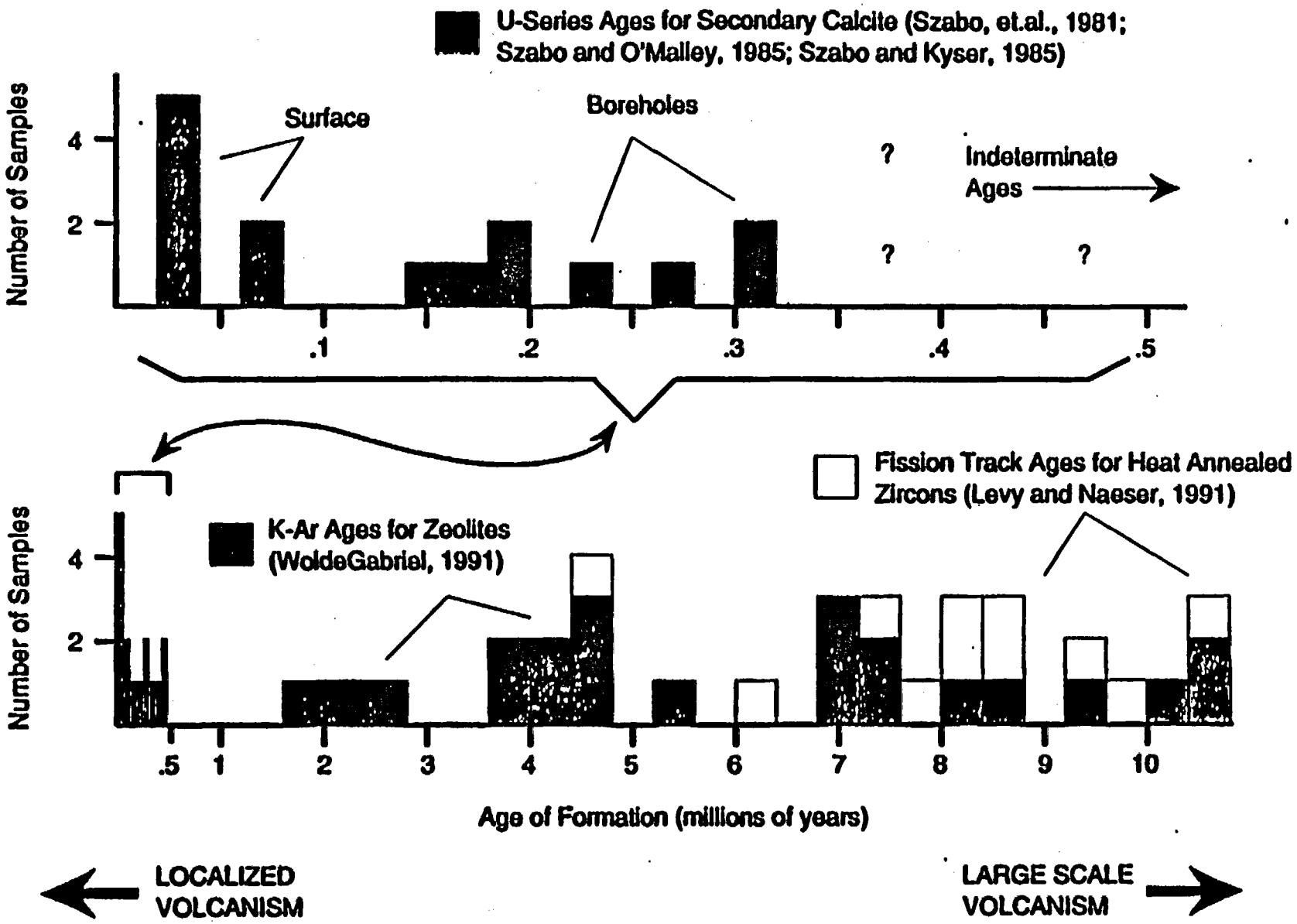


Figure 2. Ages of Fluid Alterations at Yucca Mountain

Field Observations: Spring Mounds, Faults and Surface Calcretes, Zeolites and Glass

Besides these misleading characterizations of important age data, the Panel has also characterized field observations inaccurately. One example is their statement that the Quaternary hydrothermal spring closest to Yucca Mountain is at Travertine Point, some 55 km away (p. 130). This statement is not correct: the hot springs at Oasis Valley just north of Beatty, Nevada, which were visited by the Panel, are only 25 km from the site. Further, they use the Travertine Point mound deposits to make the argument that springs at Yucca Mountain would also have to produce mounds, implying that all springs should produce mounds regardless of their topographic location or the chemical content of the water. However, the nearby springs at Oasis Valley do not now appear to be forming mounds. Likewise other springs in the region, at Boulder Dam and Dixie Valley, are not producing mounds. On the other hand, some of the many hot springs at Tecopa, CA (which is in the general area) are producing mounds, but others in this same area are not.

Consequently, the Panel has generalized from one example to establish a necessary criterion for ancient spring activity (the presence of mounds) and apparently presumed that the near proximity of the example to Yucca Mountain would provide the necessary justification. However, they are wrong on all counts: the example used is not the closest to Yucca Mountain, and mounds are sufficient but not necessary to establish spring activity. Indeed, water emerging from fault zones on a steep slope would not be expected to produce mineral mounds, but instead should produce slope parallel deposits, such as the calcrete deposits at Trench 14 and around Busted Butte.

Yet another example of importance is the Panels' statement (p. 33) in response to the idea that the observed calcretes at Busted Butte are produced by water flowing from up-slope fault zones. Here the Panel report rejects the idea on the basis of their own observation that there are no faults up-slope from these deposits. However, available geologic maps show at least one major fault zone at higher elevations at Busted Butte,

contrary to this statement.

These two examples are important in that the Panel uses lines of argument built upon these statements to assert, in their overall conclusion statement, that:

> *"The preponderance of features ascribed to ascending water clearly... (2) contained contradictions or inconsistencies that made an upwelling ground - water origin geologically impossible or unreasonable,..."*

Another line of "evidence," considered by the Panel as contradictory or inconsistent with an upwelling water origin, is the zeolite and glass distribution with depth. Specifically citing the depth distribution of zeolites and glass as its evidence, the Panel states (p. 48):

> *"The boundary between the altered and vitric tuffs indicated that the water reached its highest levels and receded downward from 12.8-11.6 Ma, and that since that time the water level at central Yucca Mountain has probably not risen more than 60 m above its present position."*

However, it is not possible to find the support cited for this conclusion from the actual data, which are shown in Figure 3. In particular, the observations show that, in some drill holes, glass is present hundreds of meters below the present water table. Further, zeolites are also present hundreds of meters above the water table. Thus, the distributions of zeolite and glass do not produce a simple relationship with the water table, that is both glass and zeolite occur above and below the water table making it impossible to establish a boundary and an ancient receding level for the water table based on these data.

In regard to the latter, it is important to point out that the Panel did not mention that the K-Ar dates of the zeolites in question range from 2 to about 10 Ma, as shown in Figure 2, and are much younger than the host ignimbrites. Further, the youngest zeolites are near the surface and the oldest are at depths below the water table. If the water table reached its highest level at 12.8 - 11.6 Ma and receded downward from that

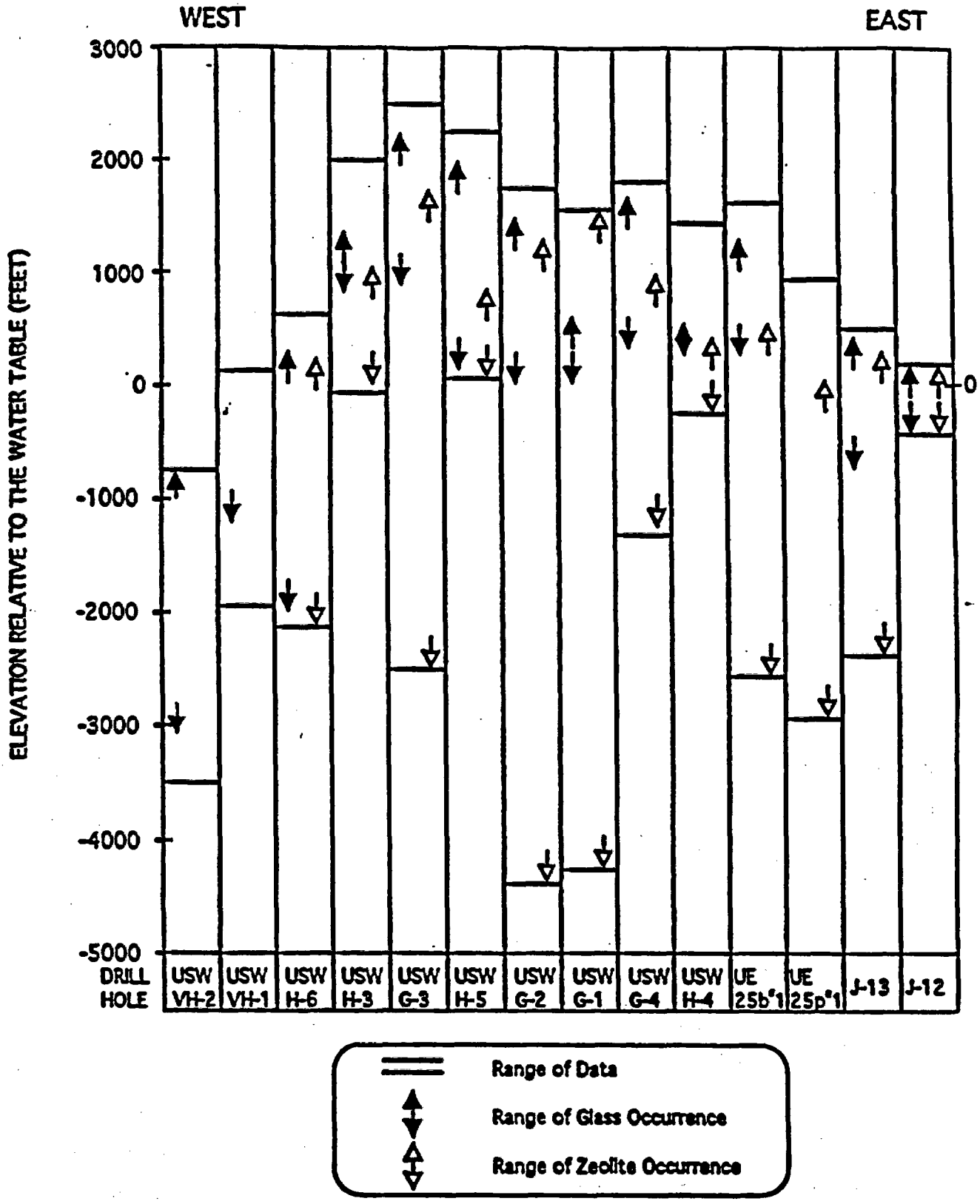


Figure 3. Distributions of glass and zeolite relative to the water table. Drill hole locations are distributed within and very near the proposed repository at Yucca Mountain with the exceptions of VH-2 and VH-1 in Crater Flat to the West and J-13 and J-12 in Jackass Flat to the East.

time to its present level, the opposite depth-age relationship for the post-10 Ma zeolites would be expected. Indeed, this depth-age relationship is what would be expected for an upwelling hydrothermal origin of the zeolites. Furthermore, this is the process generally accepted as being responsible for zeolitization in any case.

Isotopic Data: Comparisons Between Vein Calcites and Ground Water

A second major problem with the Panel report is that the strong conclusions produced by the Panel are either not reasonably supported by the evidence presented or are inconsistent with data and analysis results not cited in the report. This represents a class of problems differing from the previous cases, where the data cited are at least consistent with what is reported in the literature (though insufficient to support the conclusions drawn). However, the data cited are, nevertheless, not sufficient to support the conclusions drawn.

An example of this situation arises from the Panel's statements (*e.g.*, p. 52 & p. 148) that the isotopic ratios for strontium, uranium and thorium for the near-surface vein calcites at Trench 14 and Busted Butte do not match the measured ground water values and therefore that ground water cannot have been responsible for their deposition. Here they compare the isotopic ratios in the calcites to those characteristic of meteoric water at shallow depths below the water table level. At these depths the water resides in volcanic tuffs and does indeed have discordant isotope ratios relative to the surface calcites. However, what the Panel fails to mention is that the isotopic characteristics of the water change with depth, since its isotopic character depends on the host rock properties. Specifically, a strontium isotope ratio measurement from the only well that penetrated the Paleozoic limestones at Yucca Mountain gives a value significantly higher than those from the shallower water in the tuffs, and close to the moderately high values observed in the surface veins in question. Further, while values from yet deeper water, including that in the Precambrian below the limestones, have not yet been

obtained at the site, the samples from older rocks at other sites, particularly in Precambrian rocks and Paleozoic shales, show very high strontium isotopic ratios in the range and higher than those observed in the Yucca Mountain and Busted Butte calcite veins, which average around .7125. The relationships of strontium ratios to rock types are illustrated by the data compiled in Table 1, where rhyolites and tuffs have low ratios around .707, limestones have ratios near .709 while Precambrian rocks have high ratios near .717.

Consequently, it is very likely that if water were convected upward from depths of the order of 3 km or deeper at Yucca Mountain it would have high strontium isotopic ratios and when mixed with the shallower water, which has lower strontium ratios, would produce the moderately high strontium isotopic ratio values observed in the near surface vein calcites. A similar argument applies to the other isotopes, although in the case of uranium series isotopes it is more complex (Archambeau and Price, 1991).

It is significant that the Panel offered no discussion of why the strontium ratios at Trench 14 and elsewhere at Yucca Mountain are so high, relative to observed limestone values. Certainly if these vein and associated calcrete deposits are simply due to the evaporation of rainwater carrying calcium and strontium picked up in solution from wind blown dust from (rather distant) limestone outcrops, as is asserted by the Panel, then one would expect to see strontium ratios near the limestone values of .709 rather than the much higher values that average .7125. Surely one could make the argument that there is no apparent support for such a pedogenic origin based on the isotopic data. Indeed there is every reason to doubt this hypothesis in view of the very discordant values observed in the strontium ratios of the surface calcites at Yucca Mountain relative to the values to be expected from the available sources of wind-transported calcite near Yucca Mountain.

Thus, the Panel has ignored important consequences of a "pedogenic origin" for the calcites and have also ignored the possibility of upwelling from greater crustal depths, where it is known that the isotopic ratios of the water would be different from those

Location	Rock	$^{87}Sr/^{86}Sr$	Source	Note
Unaltered Ignimbrites				
Long Valley Caldera	Inyo Domes Rhyolites	0.70630	Goff et al. (1990)	mean of 3 samples
do	do	0.70606	do	mean of 7 samples
do	Mafic and Intermediate	0.70630	do	mean of 3 samples
do	Moat Rhyolites	0.70601	do	mean of 6 samples
do	Early Rhyolites	0.70665	do	mean of 2 samples
do	do	0.70716	do	hydrothermally alt
do	do	0.70742	do	do
do	Bishop Tuff	0.7070	do	mean of 2 samples
do	do	0.70713	do	mean of 6 samples
do	do	0.70645	do	sanidine seperates
do	do	0.70745	do	hydrothermally alt
do	Pre-caldera Volcanic	0.70610	do	mean of 3 samples
representative mean value: 0.70667				
Paleozoic Carbonates				
Spring Mountains	Limestone	0.70913	Peterman (1990)	outcrop
do	do	0.70823	do	do
do	do	0.70837	do	do
Ash Meadows	do	0.70990	do	do
Rock Valley	do	0.70934	do	do
representative mean value: 0.70899				
The Precambrian Basement				
Round Vly. Peak, CA	Schist	0.71656	Goff et al. (1990)	PC-derivative
do	Hornfels	0.72201	do	do
do	Sandstone	0.71126	do	do
Dish Hill, CA	Granodiorite	0.7177	Peterman et al (1970)	xenolith
representative mean value: 0.71688				

Table 1. Strontium isotopic ratios of unaltered ignimbrites, paleozoic carbonates and Precambrian rocks of the western Basin and Range Province. The high strontium isotopic ratio (> 0.71) of Yucca Mountain alteration products and calcite veins is indicative of a deep crustal source.

in the shallow water. Further, it is known, or can be inferred, that the ratios from the deep sources of water would be close to those observed in the vein calcites. Instead, they have implicitly assumed that either convection from such large depths does not happen or simply ignored the evidence of the changing isotopic character of the water with depth and formed the conclusion that ground water in general cannot be responsible for the calcite vein deposits at the site. Since Wood and King (1992) show that the volumes of outflow at the surface (approximately .5 km^3) in the vicinity of the Borah Peak (Idaho) and Hebgen Lake (Montana) earthquakes can be explained as upward water flow ("seismic pumping") along fracture zones from depths at least as great as 5 km, it is clear that the possibility of upwelling of water from the Paleozoic and Precambrian should have been addressed by the Panel. Since they neither take note of the upwelling evidence given by Wood and King nor consider the changing isotopic ratios in the water with depth, their conclusion appears inappropriate and, in fact, might clearly be reversed when all the pertinent data are considered.

Indeed, even the limited data used by the Panel to support their conclusions can be interpreted quite differently. Specifically, the shallow water near the top of the water table should be representative of infiltrating rain water in areas at and near Yucca Mountain where there is no upwelling of convected water from depth. Such "sink areas" are extensive at Yucca Mountain and the water at depth should be representative of infiltrating rain water. If this water does not have isotopic characteristics matching the vein calcites, which it does not since the strontium ratio for such water is .7105, then the logical conclusion is that infiltrating meteoric water (which would have taken any available calcium and strontium from wind-blown dust into solution) does not have isotopic characteristics that are compatible with the observed vein calcites. This observation, as well as those given previously, contradict the Panel's general conclusion that these vein calcites are *"classic examples of arid soil characteristics recognized world-wide."* Further, rather than showing that the isotopic character of the vein minerals versus that of the shallow ground water rules out upwelling ground water as a source of the calcite-opal veins observed, the lack of agreement between the isotopic

characteristics of the vein calcites and the shallow water at Yucca Mountain can be interpreted to mean that pedogenic hypothesis advanced is not supported by the pertinent isotopic data.

Water Level Changes at Devils Hole

Another example of a conclusion that is not reasonably supported by the evidence and data cited is the water level data at nearby Devils Hole. The Panel cites evidence (pp. 35, 55) that the ground water level exposed in the open cavern at this location has not fluctuated by more than 10 meters in the last 45 ka. In addition the Panel cites evidence from other studies that imply that the water level has been below the land level, which is 16 meters above the ground water level, for the last several hundred thousand years. However, the Panel fails to mention, or take account of the fact, that the Devils Hole Cavern occurs in an isolated outcrop with its opening elevated above the surrounding area and that within this nearby area there are many active springs. Thus, any rise in the water table would result in greater surface outflow from the active springs and so prevent any rise in the Devils Hole water level above about 10 meters. Consequently, the water level data in the Devils Hole Cavern does not reflect upward rises in the water table, although declines in the level should be correlated with declines in the water table in the area. In this regard, there is some evidence that the water level in the cavern may have been lower in the past than at present. In any case however, the Panel's argument that the water table has probably been stable for a long period of time, based on lack of evidence for any rise in the water level at Devils Hole greater than 10 meters, is not correct.

Age Data, Low Grade Metamorphic Alteration and Temperature Data

The final area of major concern with the Panel's report is the neglect of the very large body of data relating to the ages and character of hydrothermal alterations at the

site. The Panel uses very limited data, and principally the zircon age data previously discussed, to argue that the last hydrothermal event occurred about 10-12 Ma ago. However, in addition to the zircon age data, which actually implies much more recent activity, there is an additional body of data that also indicates that there has been on-going hydrothermal activity.

This data involves the age data shown in Figure 2 in combination with paleogeotherm estimates inferred from oxygen isotopes, rock alteration temperatures from zeolitization and illitization processes in rocks at Yucca Mountain, vein formation temperatures from fluid inclusions, and finally, zircon annealing temperatures from the samples at Trench 14 and Busted Butte. All of this inferred temperature data, shown in Figure 4, indicate high temperatures and high geothermal gradients existent at Yucca Mountain in the past. Since the age data shown in Figure 2 are from samples in close proximity to the locations sampled for the temperature estimates, and in the case of the zircons are the same samples used to estimate annealing temperatures, there is little doubt that the high temperatures and gradients are associated with very recent hydrothermal activity at Yucca Mountain. In particular, the K-Ar and uranium-series dates for zeolites and calcium carbonate vein material, respectively, indicate episodic and moderate to high temperature hydrothermal activity that has continued from 13 Ma to essentially the present. In addition, the zircon ages and annealing temperatures also indicate post-Timber Mountain hydrothermal activity involving quite high temperatures for the fluids involved. Finally, all the geothermal gradients inferred from heat flow and oxygen isotope data are sufficient to produce convection and are therefore consistent with a history of hydrothermal activity.

The fact that the Panel did not consider any of the data pertaining to paleo-temperatures and ignored all the age data, except that for the zircon ages which they misrepresented, has resulted in a description of the recent geologic and hydrologic history of the site that is almost certainly incorrect. Indeed, the only uncertainty that might still be entertained is whether the youngest ages, of less than 500 ka, are correlated with the high temperatures indicated in Figure 4. This can be cleared up

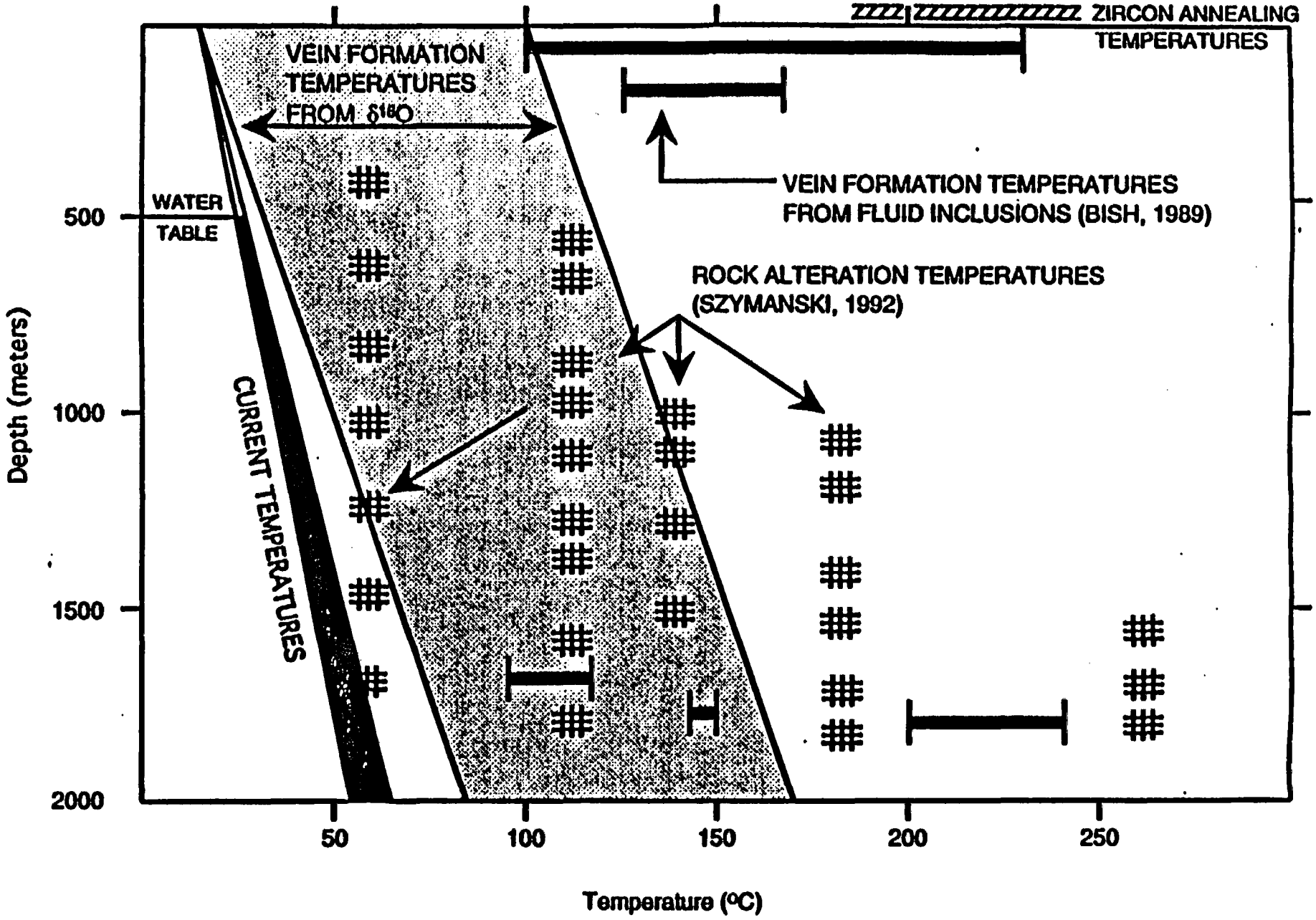


Figure 4. Borehole samples from Yucca Mountain reveal alteration products formed at high temperatures, indicating that the site has been invaded by high temperature fluids.

by additional sampling of course, but in any case there is no reasonable doubt that hydrothermal alteration and deposition occurred well after the time of 10 to 12 million years ago claimed by the Panel. Once this Panel conclusion is recognized as unsupportable in the face of the available quantitative age and paleo-temperature data, it only becomes a question of how frequently and how recently the episodic hydrothermal activity has occurred. The available data shown in Figures 2 and 4 clearly suggest that it has been frequent enough and recent enough to justify the belief that it will most likely continue and that it could occur at any time in the future.

In addition to ignoring age and paleo-temperature data, the Panel did not address the significance of the reported mineral enrichment of interstitial fluids extracted from pores within the tuffs above the water table (Smith, 1991). Relative to local fluids within fractures in the tuffs, the interstitial fluids are strongly enriched not only in alkali-earth elements, but also in transition, base and noble metals and rare earth elements (REE) which at least suggest, if not require, a hydrothermal origin. Table 2 indicates the observed enrichment of several elements found in this trapped water, expressed as a ratio of abundances relative to the element content in nearby well water. Clearly, the presence of noble and base metals is indicative of a hydrothermal fluid. Further, in addition to an overall enrichment of REE, there is an unusual enrichment of heavy REE relative to light REE that is not shared by the host ignimbrites. This enrichment is illustrated in Figure 5 where the normalized REE abundances versus increasing REE atomic weight are shown for the interstitial fluids (a) and local ignimbrites (b). Clearly the abundance trend versus atomic weight is quite different for the ignimbrites compared to the interstitial water. Specifically, the relative enrichment of heavy REE in the interstitial water is conspicuous and since it is also observed elsewhere for hydrothermal solutions that are concentrated in CO_2 (Michard and Albarede, 1986; Michard et al., 1987), it is certainly likely that these fluids are remnants of late hydrothermal fluids.

Table 2

Mineral Enrichment of Vadose-Zone Interstitial Fluids

ELEMENT	ENRICHMENT Ratio *
Magnesium	10
Calcium	8
Nickel	1000
Copper	50
Zinc	45
Rubidium	2
Strontium	30
Yttrium	100
Molybdenum	300
Iodine	20
Tungsten	300
Platinum	**
Gold	**
Titanium	20

*Data are from borehole UZ#4 (interstitial fluids) normalized by J-12 and J-13 well waters (Smith, 1991).

**Well waters contained no measured gold and platinum. Interstitial fluids contained .2 ppb for both metals.

Table 2. Mineral enrichment of vadose-zone interstitial fluids relative to well waters residing in ignimbrite fractures.

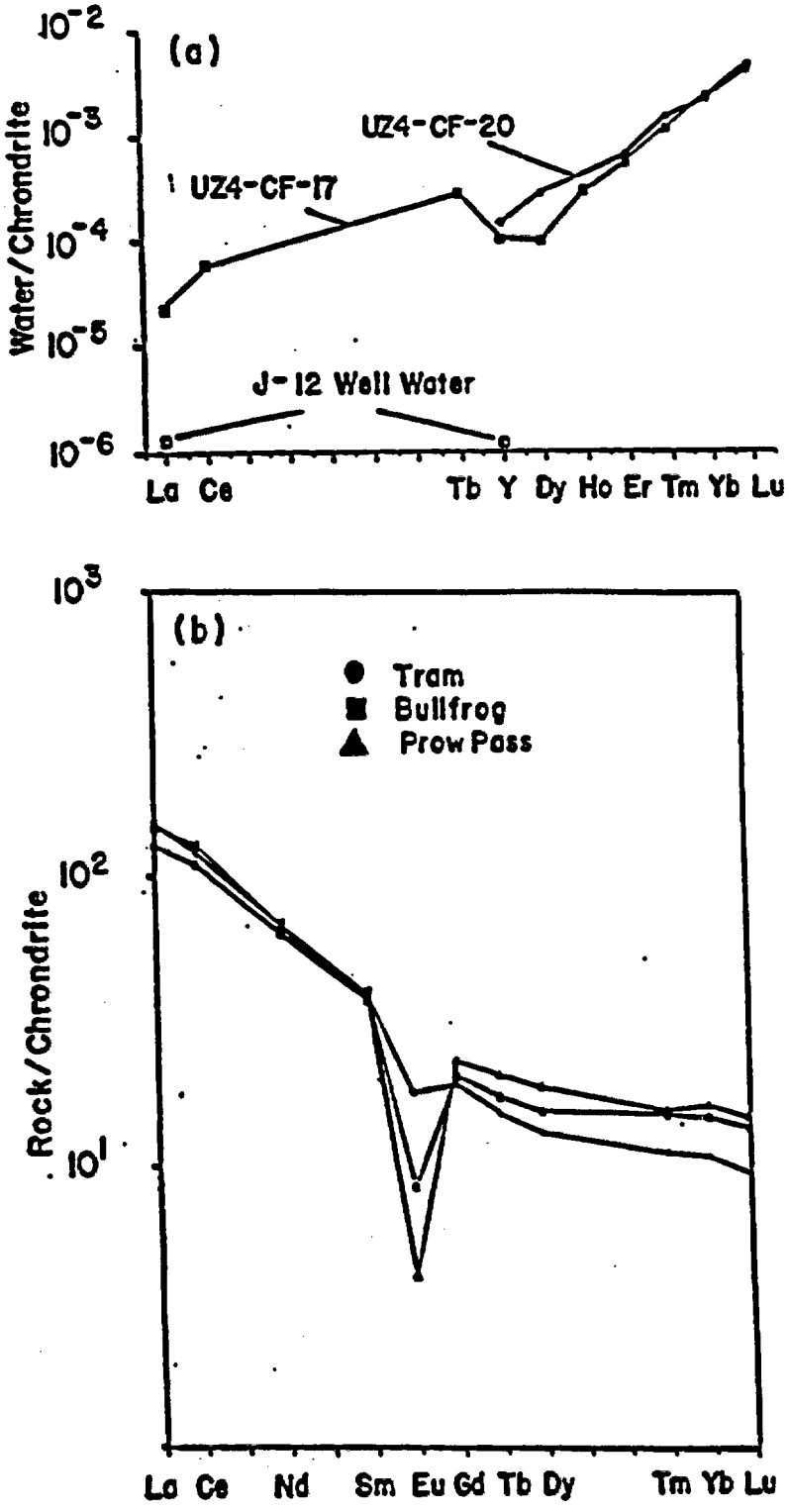


Figure 5. Chondrite-normalized REE abundance patterns. a.) Interstitial fluids and well water residing in ignimbrite fractures: data from Smith (1991). b.) Crater Flat ignimbrites: data from Scott and Castellanos (1984). Heavy REE enrichment for interstitial fluids is due to high CO_2 pressure.

The inference of a high CO_2 content for these remnant hydrothermal fluids is important in that a high gas content would be consistent with an interpretation of gas assisted fragmentation and brecciation during hydrothermal fluid intrusion and account for observed intense brecciations of the country rock associated with late carbonatization at many sites at Yucca Mountain. This inference, while not conclusive in itself, does certainly bring into question the Panel's conclusion that (p. 46):

> *"...there is no need for, or good evidence in support of, upwelling of deep hot waters to account for the brecciation (of near-surface country rocks) or silica - carbonate cementation."*

If the Panel had presented the fluid inclusion data along with the temperature and age data in their report, it seems unlikely that they could have made such a statement or, if made, have made it sound plausible in the face of the evidence.

A related Panel statement involves the fault breccia cement at Trench 14. The Panel conclusion states (p. 44):

> *"...that the fault breccia cement at Trench 14 and Busted Butte is of pedogenic or surficial origin, based on the presence of older detrital zircons, grain size and structure characteristics, and is not of hydrothermal origin."*

As noted earlier, the zircons are not as old as indicated by the Panel and in any case do not provide an age estimate for low to moderate temperature hydrothermal deposition (see the temperature range for zircon fission track annealing indicated in Figure 4), while the small grain size of the calcite cement could be expected to occur as a consequence of rapid release of CO_2 from a hydrothermal fluid near or at the surface (Archambeau and Price, 1991). Further, the "structure characteristics" referred to by the Panel are precisely those interpreted by others, such as Hansen et al. (1987), as being characteristic of hydrothermal brecciation.

Thus, the strong conclusion drawn by the Panel is certainly not warranted by the observations they cite, in that other interpretations are at least as plausible if not preferable. But beyond these alternative interpretations, it is once again evident that the

Panel should have used additional available data to infer the origins of the silica-carbonate breccia cements and veins at Yucca Mountain. In this regard Table 3 provides a clear indication of the unusual enrichment of the breccia cement in base and noble metals relative to the stratigraphically equivalent background values for the tuffs at Trench 14. The results in the third column are the median values for 25 analyses of nine breccia samples while the fourth column indicates the significant enrichment of the most strongly mineralized specimen. The fifth column shows that the degree of enrichment of the interstitial fluids (discussed earlier) is comparable with that of the more strongly mineralized breccia sampl . Such enrichment contradicts the hypothesis of a pedogenic origin for the breccia cements and combined with the previously mentioned age and temperature data is strong evidence for a hydrothermal origin of the breccia, which is of post-Timber Mountain age.

Beyond the omissions of the data and results already mentioned, the Panel does not address several other topics and related data of considerable importance. In this regard, in situ stress measurements, such as those by Healy et. al. (1984) and Stock et. al. (1984, 1986), are clearly critical to an assessment of geodynamic stability of the site. These observations were not considered by the Panel. However, contrary to the Panel's assessment that the Yucca Mountain area is not likely to experience a large earthquake in the near future, the results from Healy et al. and Stock et. al. imply the opposite. Indeed, the recent 5.6 magnitude earthquake at Little Skull Mountain, 15 km southeast of Yucca Mountain, also indicates that an unstable stress state, rather than a quasi-stable state, actually prevails.

Consequently, at least in part because of their lack of consideration of a large body of the most quantitative and unequivocal data, the Panel reached many conclusions that are not supported by the complete body of data that exists.

Table 3
Mineral Enrichment of Breccia Cement

ELEMENT	ENRICHMENT			
	TIVA CANYON LITHOPHYSAL TUFF FROM EXILE HILL *	MEDIAN, TRENCH #14 BRECCIA CEMENT *	MAXIMUM, TRENCH #14 BRECCIA CEMENT *	INTERSTITIAL FLUIDS **
Ag	2	2	16	
As	1	3.6	36	
Au	<1	2	5	
Cu	.25	1	4	50
Mo	7	18	650	300
Pb	14	65	610	1-5
Sb	<1	25	100	
Zn	44	90	33	45
Bi	<1	<1	<1	

*Data from Weiss (1990); the maximum values of enrichment are for a single sample (3SW195B) with the highest overall mineral evrichment relative to average concentrations for the Yucca Mountain area (Castor et al., 1989).

**Data from Smith (1991); enrichment relative to well water.

Table 3. Mineral enrichment of breccia cement: results for lithophysal tuff and interstitial fluids are shown for comparison.

General Comments on the Panel Report

In addition to a general disregard of important quantitative data and a rather cavalier approach to elementary logic, the Panel not only distorted some of the data and interpretations reported in the literature (such as the zircon age data) but also misrepresented the concepts described by Szymanski in his 1989 report on hydrotectonic activity at the Yucca Mountain site. To make matters worse, the Panel also misrepresented the information given to them during a presentation by the minority members of the DOE External Review Panel (Archambeau and Price). Specifically, the NAS/NRC Panel states, on page 129 of their report:

> *"It should be noted that the charge to the panel included an evaluation of the particular concepts described in the report by Szymanski (1989). Those concepts involved seismic pumping as the primary mechanism for driving the deep ground water to the surface in a cyclic progression of crustal stress changes. The panel evaluated the geologic evidence presented for this process and found both the evidence and the seismic pumping model inadequate to support the consequences attributed to them. As the panel was concluding its studies, the "minority" members of the 5 member external review panel selected by DOE and Szymanski to review his report informed the NAS panel that both the interpretation of some of the evidence and the model itself had changed: that Szymanski no longer believed that seismic pumping alone could drive the water up as high as he had stated in his report, and that he now had a new concept involving a thermally driven hydrotectonic cycle. This information was presented at the NAS panel's last meeting. Although there was no time left for the NAS panel to give consideration to a new thesis, nor was there a written document that could be evaluated, the cyclical concept as presented to the NAS panel appeared to have little validity, given that the panel is convinced that the geologic evidence refutes the assertion that ground water has risen repeatedly 100 meters or more in the recent geologic past. Because an essential part of the "cycle" has not yet happened, there is no basis for postulating a cyclical process whatever the proposed mechanisms involved."*

In referring to the minority members' report, the Panel alleges that they were informed that "both the interpretation of some of the evidence and the model itself had changed" and then go on to elaborate that Szymanski now "had a *new* concept involving a thermally driven hydrotectonic cycle." Both of these statements are false.

Specifically, these statements were not made by the minority members. Indeed the material distributed to the NAS Panel by the minority members describes, in very specific terms, the full concept advanced by Szymanski in his 1989 report which includes the concept of a hydrotectonic cycle involving *both* seismic pumping and thermally driven convection of the ground water following a tectonic event, such as an earthquake. This combined response to changes in the hydrologic system was considered to be the cause of upwelling water and associated mineral deposition at Yucca Mountain. Only if the minority members had contradicted their own written summary of Szymanski's 1989 report could they have made the statements attributed to them and that is simply not what occurred, nor realistically is it credible. Furthermore, the minority members presented a summary of their report to the NAS Panel in May of 1991 and submitted their complete report to the DOE in November of 1991. This final report reproduces the material made available to the NAS Panel. Therefore, it is a matter of record that the Panel had ample time to refer to the relevant material, long before they submitted their report in July of 1992, and in addition shows that they misquoted the minority members.

Beyond this distortion of the facts, the Panel misrepresented the content of Szymanski's 1989 report since they assert that he had changed his original concept of seismic pumping as the primary cause of water level changes and introduced a new concept involving thermally driven processes at a time well after writing his report. If the Panel had actually read Szymanski's report they would have found that this latter concept is discussed in considerable detail and was thought to be the principal mechanism for deposition of calcite throughout the mountain.

Therefore, one can only conclude that the Panel did not actually read Szymanski's report, or if they did read it they chose to misrepresent it. In either case this is hardly what would be expected from a NAS panel that is charged with the responsibility of evaluating a report. On this basis alone there would be reasonable grounds to seriously question the Panel's findings as it suggests an inclination to distort and misrepresent the record.

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