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**DIALOGS ON THE OSTI
YUCCA MOUNTAIN CONTROVERSY**

SPECIAL REPORT No. 5
CONTRACT No. 92/94.0004

SPECIAL REPORT Submitted to the
Nuclear Waste Project Office
State of Nevada

March, 1993

Assembled by:

**C. B. Archambeau
J. S. Szymanski**

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44
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DIALOGS ON THE YUCCA MOUNTAIN CONTROVERSY

REPORT ORGANIZATION

PART 1

Introduction of Controversy

Dr. C. B. Archambeau

PART 2

Letter to F. Press

Dr. C. B. Archambeau

PART 3

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

Dr. C. B. Archambeau

PART 4

"Critical Review of the National Research Council Report:
Groundwater at Yucca Mountain: How High Can It Rise?"

Dr. M. Somerville et al.

PART 5

Letter to C. B. Archambeau

Dr. F. Press

PART 6

Report to F. Press (Memorandum--January 6, 1993)

NRC Staff

PART 7

Letter of Reply to F. Press

Dr. C. B. Archambeau

PART 8

"Safety of Proposed Yucca Mountain Nuclear Repository as
Regards Geological and Geophysical Factors: Evaluation
of Minority Report by Archambeau and Price."

Dr. J. F. Evernden

PART 9

"Review on USGS OFR 92-516."

Dr. M. Somerville

PART 1

Introduction of Controversy

INTRODUCTION

The recent, 1992, report prepared by the Panel on Coupled Hydrologic/Tectonic/Hydrothermal Systems at Yucca Mountain for the National Research Council of the National Academy of Sciences, entitled *Ground Water at Yucca Mountain: How High Can It Rise?* has generated critical reviews by Somerville et al. (1992) and by Archambeau (1992). These reviews were submitted as reports to the Nuclear Waste Project Office, State of Nevada by Technology and Resource Assessment Corporation under Contract No. 92/94.0004. A copy of the review report by C. B. Archambeau was also sent to Dr. Frank Press, President of the National Academy of Sciences, along with a cover letter from Dr. Archambeau expressing his concerns with the NRC report and his suggestion that the Academy President consider a re-evaluation of the issues covered by the NRC report.

Dr. Press responded in a letter to Dr. Archambeau in February of this year which stated that, based on his staff recommendations and a review report by Dr. J. F. Evernden of the United States Geological Survey, he declined to initiate any further investigations and that, in his view, the NRC report was a valid scientific evaluation which was corroborated by Evernden's report. He also enclosed, with his letter, a copy of the report he received from his staff.

In March of this year Dr. Archambeau replied to the letter and NRC staff report sent by Dr. Press with a detailed point-by-point rebuttal of the NRC staff report to Press. Also, in March, a critical review of Dr. Evernden's report by M. Somerville was submitted to the Nuclear Waste Project Office of the State of Nevada and this report, along with the earlier review of the NRC report by Somerville et al., was included as attachments to the letter sent to Dr. Press.

The reviews of our reports and of the NRC report, as well as the responses to these reviews, are considered to be an important part of the work being supported by the State of Nevada since they focus on the specific data and observations that are at the heart of the controversy over the suitability of the proposed Yucca Mountain

Repository. In principal the opposing interpretations of the geologic data and observations could be directly compared with respect to their plausibility, completeness and overall scientific validity. Therefore, we have included the various reviews and responses in the following sections of this report to provide, in one document, interested readers with material which may enable them to contrast and assess the views of the opposing sides in this controversy. At the least, these documents reveal the character of the debate, which may be enlightening to those unfamiliar with this controversy.

All the reviews and letters are included, but we do not include the NRC report itself since it is available from the National Academy Press.

PART 2

Letter to F. Press from C. B. Archambeau

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 proposed 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,



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cba/rmf
Attachment

PART 3

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

**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

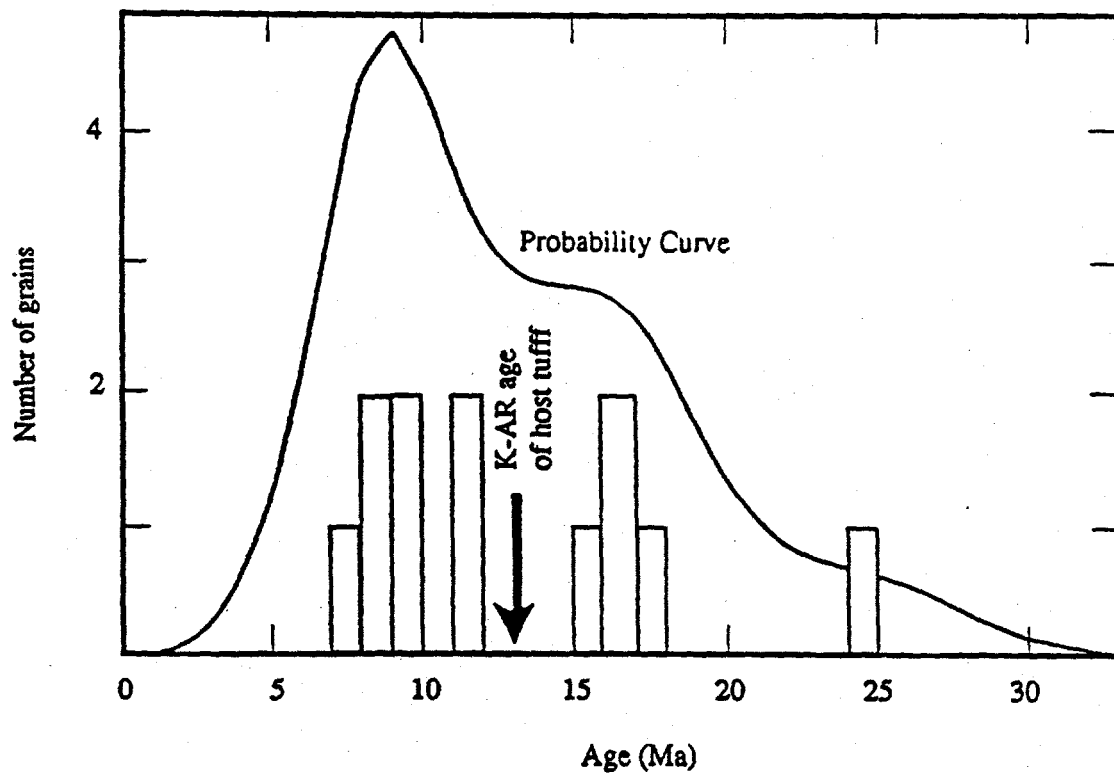
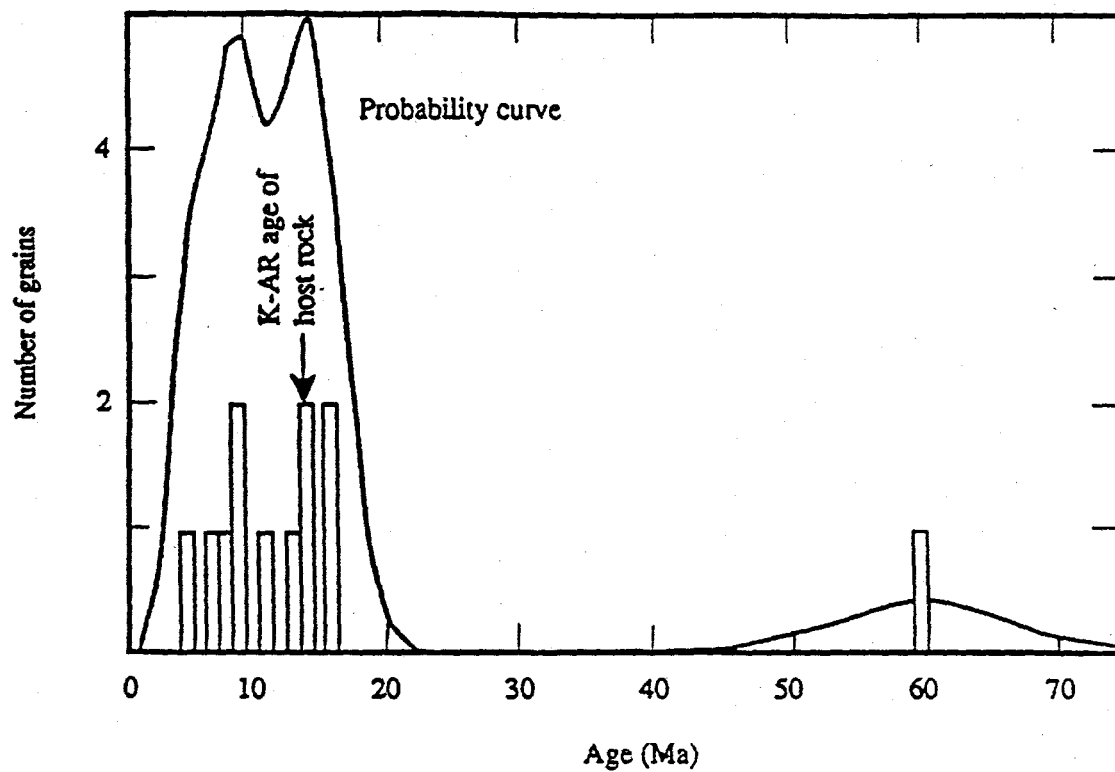


Figure 1. Fission track ages of zircons from breccias at Busted Butte (top) and Trench #14 (bottom). From Levy and Naeser, 1991.

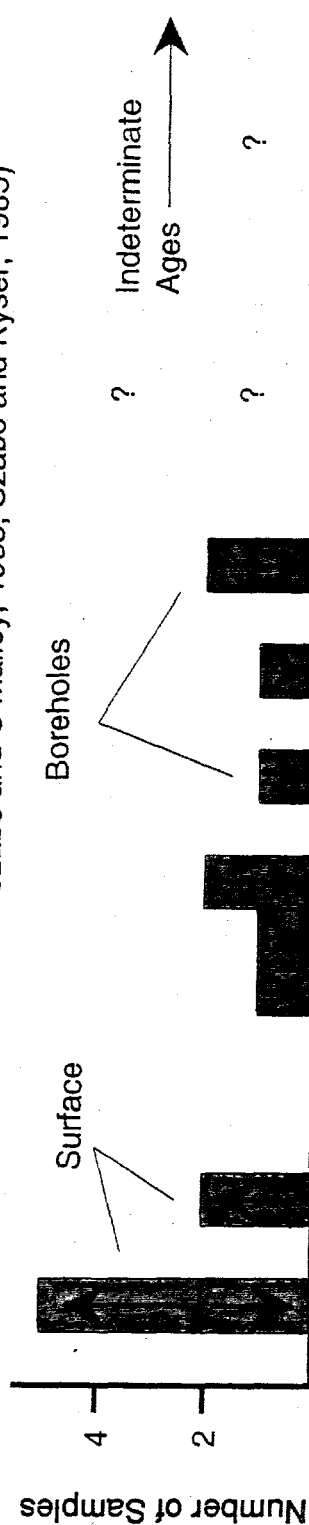
several additional dates near 8.5 Ma. The two sigma interval attached to the youngest age, of 4.8 Ma, is 2.5. Thus, there is very high confidence (over 90%) that the age of heating of this crystal was between 2.3 Ma and 7.3 Ma, with the highest probability for a specific age being 4.8 Ma. The same interpretation 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.

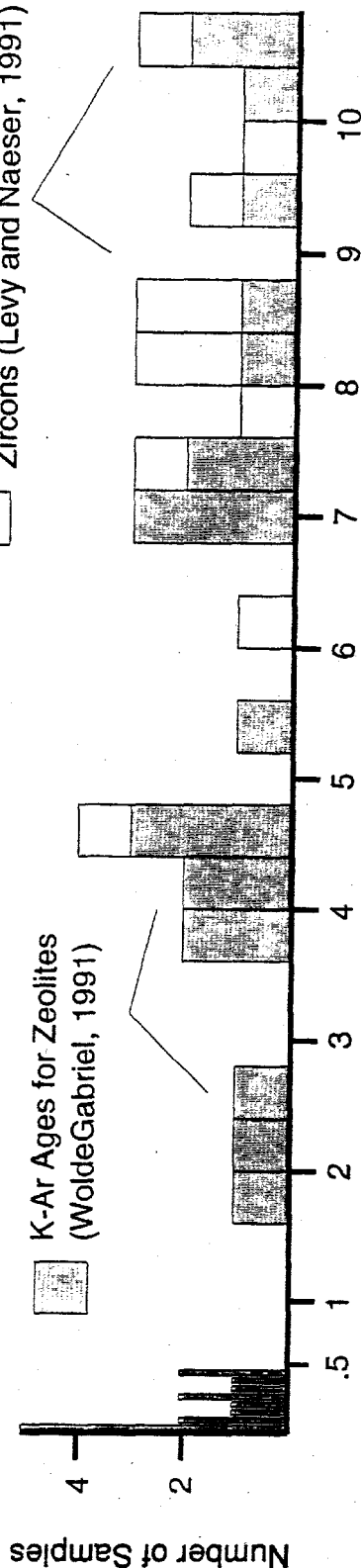
This issue is extremely important in that these are the only age data used in the NAS report to substantiate the claim that the last and final hydrothermal event occurred some 13 to 10 Ma ago. Age data from uranium series dating of calcites from veins at depth as well as potassium-argon dates from zeolites, 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.

■ U-Series Ages for Secondary Calcite (Szabo, et.al., 1981;
Szabo and O'Malley, 1985; Szabo and Kyser, 1985)



□ Fission Track Ages for Heat Annealed
Zircons (Levy and Naeser, 1991)

■ K-Ar Ages for Zeolites
(WoldeGabriel, 1991)



Age of Formation (millions of years)

← LOCALIZED
VOLCANISM

→ LARGE SCALE
VOLCANISM

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

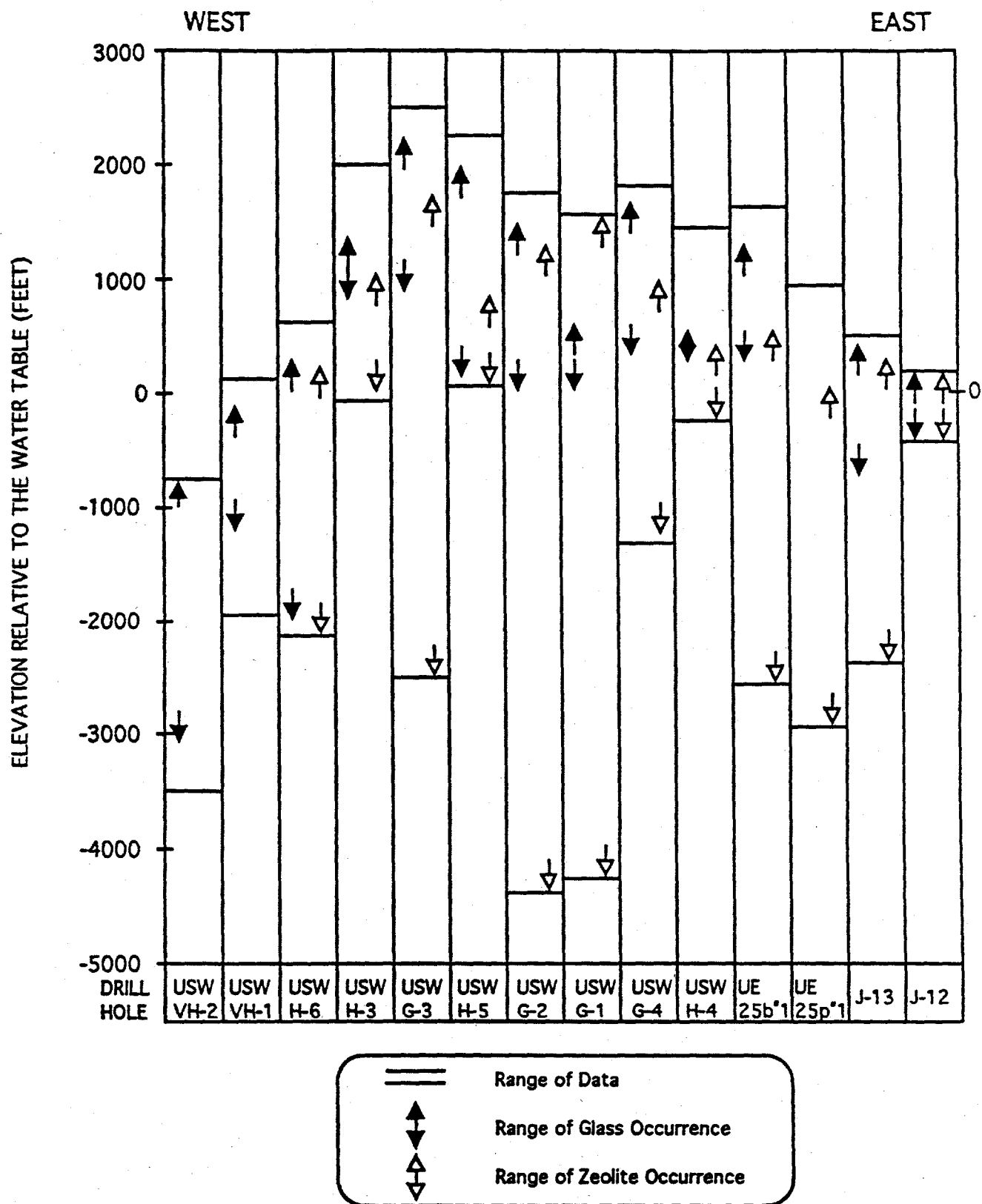


Figure 3. Distributions of glass and zeolite relative to the water table. Drill hole locations are distributed within and very near the proposed repository at Yucca Mountain with the exceptions of VH-2 and VH-1 in Crater Flat to the West and J-13 and J-12 in Jackass Flat to the East.

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	$^{87}\text{Sr}/^{86}\text{Sr}$	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 separates
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 $.5 \text{ km}^3$) 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

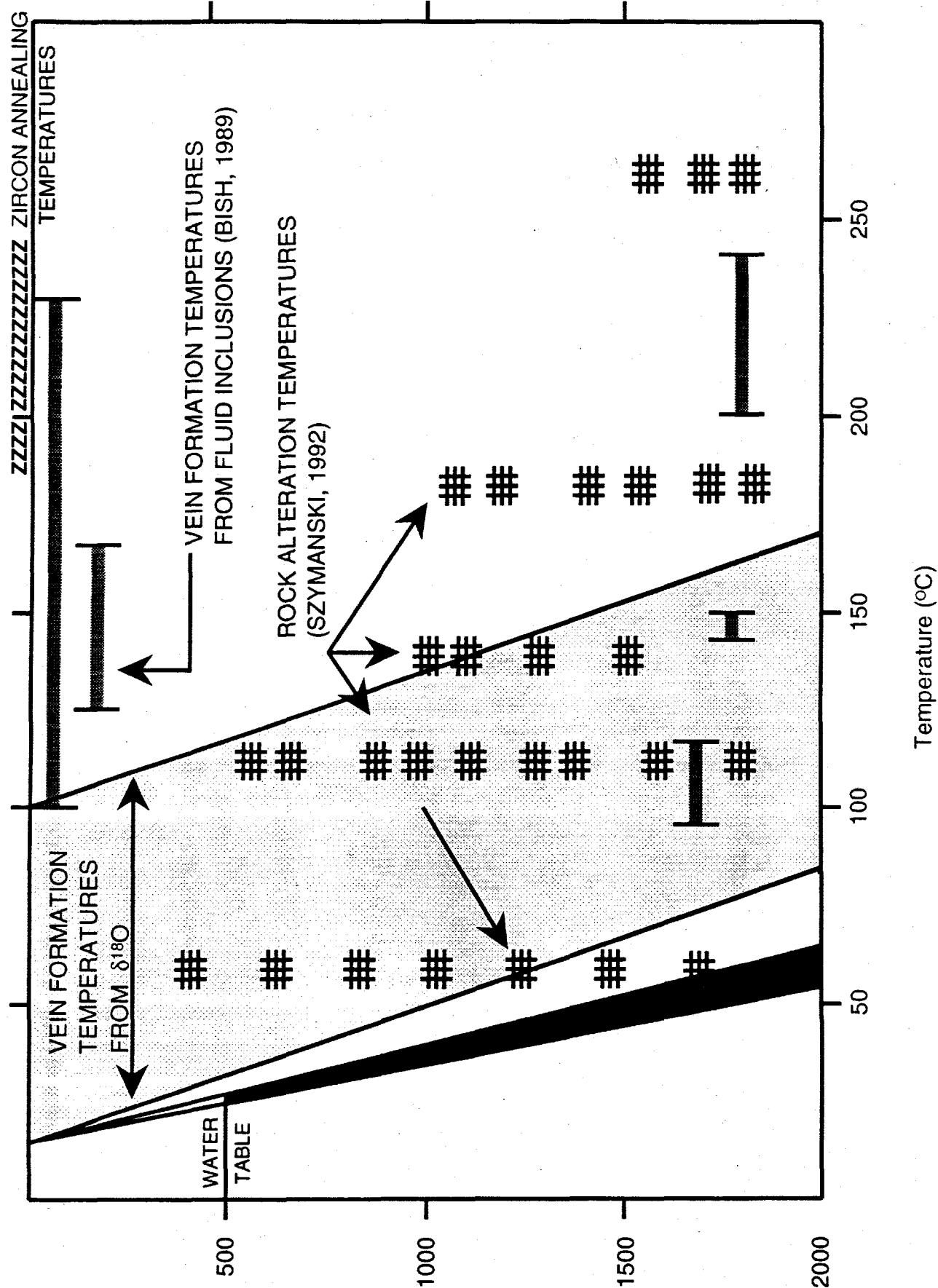


Figure 4. Borehole samples from Yucca Mountain reveal alteration products formed at high temperatures, indicating that the site has been invaded by high temperature fluids.

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.

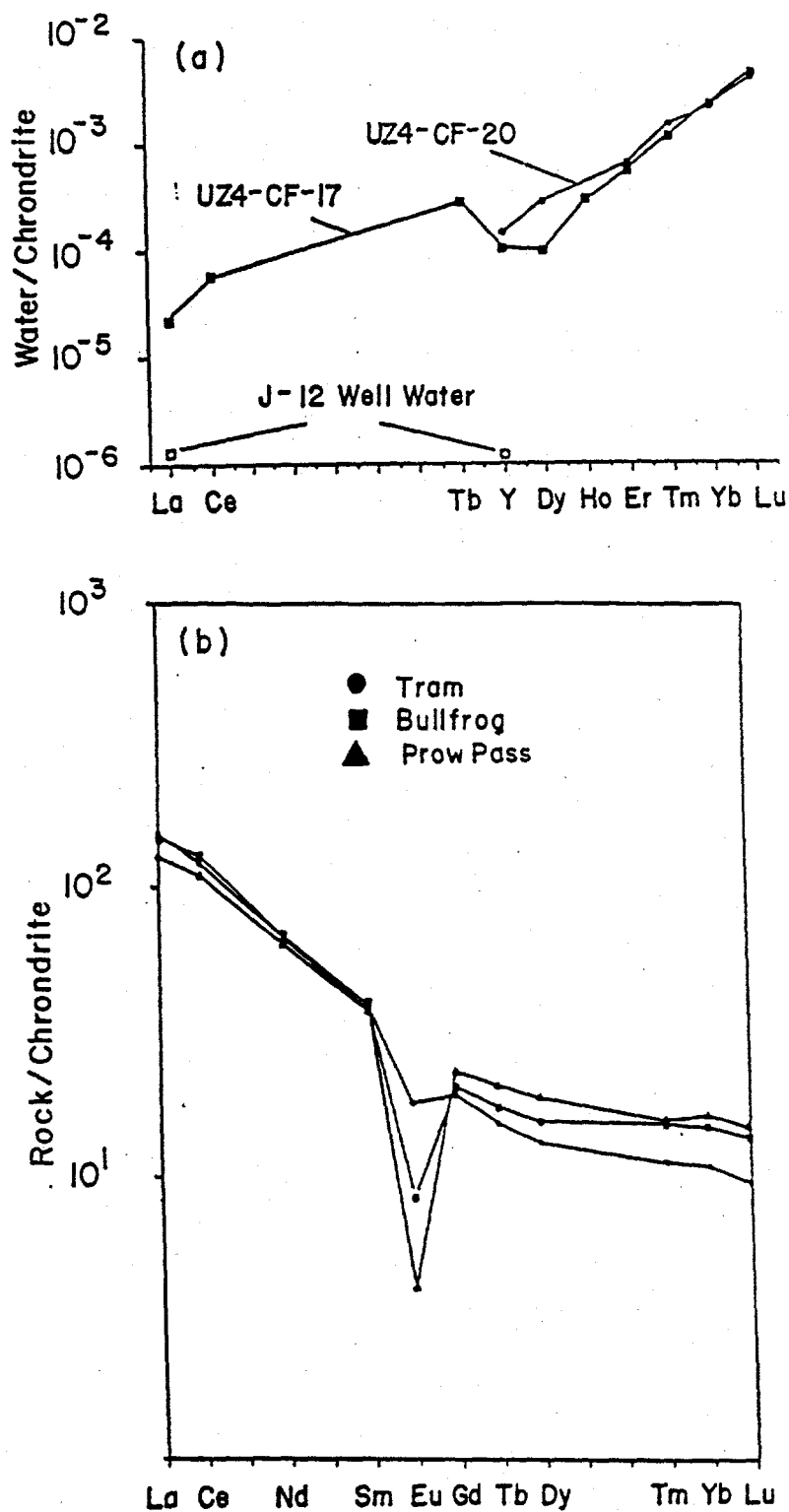


Figure 5. Chondrite-normalized REE abundance patterns. a.) interstitial fluids and well water residing in Ignimbrite fractures: data from Smith (1991). b.) Crater Flat ignimbrites: data from Scott and Castellanos (1984). Heavy REE enrichment for interstitial fluids is due to high CO_2 pressure.

The inference of a high CO₂ 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 CO₂ 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 samples. 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 enrichment 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 hydro-tectonic 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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PART 4

**Critical Review of the National Research Council Report:
Groundwater at Yucca Mountain: How High Can It Rise?**

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**CRITICAL REVIEW OF THE NATIONAL RESEARCH
COUNCIL REPORT:
"GROUNDWATER AT YUCCA MOUNTAIN:
HOW HIGH CAN IT RISE?"**

REPORT No. 2
CONTRACT No. 92/94.0004

QUARTERLY REPORT Submitted to the
Nuclear Waste Project Office
State of Nevada

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**CRITICAL REVIEW OF THE NATIONAL RESEARCH
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TABLE OF CONTENTS

I. Introduction: Background of the Controversy about Hydrotectonic Conditions at Yucca Mountain	1
II. Summary of the Technical Disagreement with the NAS/NRC Report	4
III. Hydrothermal Alteration of the Vadose Zone Ignimbrites	11
IV. Hydrothermal Processes	15
Whole-Rock Alterations	17
Epigenetic Mineralization of the Vadose Zone Rocks	18
Paleothermal Gradients	19
Homogenization Temperatures of Fluid Inclusions	21
Spatial Distribution of the Metamorphic Facies	23
Interstitial Fluids from the Vadose Zone	24
V. Mosaic Breccias	26
Breccias in Paleozoic Carbonates	27
Isotopic Characteristics of Mosaic Breccia Cements	28
Relative Concentrations of Carbon-13	30
Fission Track Ages of the Breccia Enclosed Zircons	30

VI. Calcite Paragenesis and Isotopic Data	32
Isotopic Comparative Analysis	33
Origin of the Calcite Veins Based on the Strontium Isotopic Ratios	35
VII. Geodynamics of the Yucca Mountain Area	38
VIII. Hydrologic Behavior Inferred from Modeling Studies	42
REFERENCES	47
LIST OF FIGURES	52
FIGURES	53

I. Introduction: Background of the Controversy about Hydrotectonic Conditions at Yucca Mountain

Controversy has developed over the issue of whether or not the 500-m thick vadose zone at Yucca Mountain has been recurrently invaded by hypogene fluids. Geologic formations at the site contain abundant signs which indicate that fluids have altered the original ignimbrites and deposited the controversial calcite-opal-sepiolite veins. Radiometric ages from samples of alteration minerals from above the contemporary water table, are nearly uniformly distributed over the past 13 million years (e.g., WoldeGabriel, 1991; Figure 2). In addition, the reported ages from samples of the calcite-opal-sepiolite veins range from about 25,000 to over 400,000 years B.P. (e.g., Szabo et al., 1981; Szabo and O'Malley, 1985; Szabo and Kyser, 1985). Thus, there is abundant evidence that geologic formations comprising the Yucca Mountain vadose zone have been altered by subsurface fluids during the relatively recent geologic past. The disputable scientific issue involves the origin of these fluids. Specifically, the appropriate question is: **Do the observed alteration and mineralization represent supergene/pedogenic processes or, conversely, are they representative of epigenetic/hypogene processes?**

Hazardous conditions at the site would arise from processes that could spread radionuclides into the biosphere, and subsurface fluids represent the most likely means for transport. Upwelling fluids of the type interpreted from the geologic record (Smith, 1991; Szymanski, 1992) could flush through the repository, corrode waste packages

and transport dissolved radionuclides to the biosphere.

Spring deposits were identified at and adjacent to Yucca Mountain in the early stages of site investigations (e.g., Hoover, et al., 1981; Knauss, 1981; Szabo, et al., 1981), but serious technical concerns regarding repository performance were first documented by former DOE scientist, J.S. Szymanski. After repeated attempts from 1984 to 1987 to refocus site investigations, and to resolve critical questions about upwelling fluids, he reported (1987, 1989) interpretations that the local hydrologic system is controlled by tectonic factors and, as a result, recurrently undergoes major changes.

In response to Szymanski's 1989 report, the DOE initiated two external reviews: one to report directly to the DOE and one to report to the National Academy of Science's National Research Council (NAS/NRC). The DOE Panel, composed of five experts, reported two divergent views: three of the experts (Powers, et al, 1991) judged Szymanski's interpretations to be inappropriate; and two of the experts (Archambeau and Price, 1991) judged Szymanski's interpretations and model to be appropriate. In view of the remaining and unresolved controversy, however, investigations remained defocussed and ill-suited for rapid recognition of potential hazardous conditions (e.g., GAO, 1992; SAIC, 1992).

The NAS/NRC Panel has recently released their findings in the report Groundwater at Yucca Mountain: How High Can It Rise? which appears to offer good news for those

advocating suitability of the site to accommodate a high level nuclear waste repository.

In particular, the Panel reported:

The panel's overall conclusion was that none of the evidence cited as proof of groundwater upwelling in and around Yucca Mountain could be reasonably attributed to that process. (p. 3)

Unfortunately the overall conclusion, as well as other interpretations throughout the report, are not supported by currently available facts. In this regard, the Panel has ignored multiple lines of evidence and does not justify the all-inclusive dismissal of hydrotectonic hazards at Yucca Mountain. More importantly, analytical data (e.g., radiometric ages, geothermometry and mineral and isotope abundances) not considered by the Panel provide evidence for recurrent invasions of the Yucca Mountain vadose zone by hydrothermal fluids.

II. Summary of the Technical Disagreement with the NAS/NRC Report

The topic addressed by the Panel arose from interpretations that the hydraulic conductivity structure could be controlled in part by tectonic stress, and that a hydrothermal circulation system is present at Yucca Mountain (Szymanski, 1989). These interpretations, in turn, point to the likelihood of erratic hydrologic disturbances which could explain the presence of youthful calcite-opal-sepiolite veins that are common throughout Yucca Mountain. Concerns about hydrologic stability have thereby exacerbated the controversy surrounding the genesis of the calcite-opal-sepiolite mineral assemblage occurring in the form of breccia cements, veins, calcretes and silcretes. In brief, the question is: **Were these minerals precipitated from supergene fluids (rainwater) or from hypogene fluids (hydrothermal solutions)?**

The NAS/NRC Panel on Coupled Hydrologic/Tectonic/Hydrothermal Systems finds that **there is no unequivocal field evidence for hypogene fluids having risen to the surface over the past 100 thousand years.** In support of their interpretation, the Panel draws a series of deductions which we regard to be at odds with mineralogic, geochemical, isotopic and geochronological data. The foundation of the conclusions reached by the Panel is the belief that the water table has not risen more than 60 meters above its present position over the past 11.6 million years, based on the depth distributions of glass and zeolites. This belief, if appropriate, would eliminate the possibility of any hydrothermal circulation within the vadose zone since the end of the

hydrothermal stages of activity of the Timber Mountain Caldera, more than 10 million years ago. Dismissing the possibility of hydrothermal circulations, the Panel argues that the only fluid, other than infiltrating rainwater, that could have by some means (climate change, volcanic intrusion or earthquake) invaded the vadose zone and precipitated the controversial minerals would be of the same chemistry as the contemporary fluids residing in the ignimbrites. Because the latter fluids are known to be isotopically discordant with the parent fluids of the calcitic-opal-sepiolite veins, the Panel argues that the resulting minerals could not have precipitated from "analyzed" ground water, and therefore concludes that the minerals must have precipitated from infiltrating rainwater. The possibility that the minerals precipitated from the kind of fluids residing in Paleozoic carbonates and discharging at nearby thermal springs was not considered by the Panel. However, in this review we illustrate how the mineralogic, geochemical, isotopic and geochronologic data are much more readily explained in terms of hypogene paragenesis rather than in terms of supergene paragenesis.

The belief that zeolites found above the water table were produced in response to supergene/diagenetic processes is an essential part of the Panel's case. Yet the chemical composition of the zeolites, relative to the parent glasses, is inexplicable within the supergene/diagenetic context. A more straightforward and less problematic interpretation of the genesis of the zeolites is the traditional view that they are products of propylitic alteration. This interpretation is consistent with the fact that the Yucca Mountain ignimbrites, both above and below the water table, contain literally billions of tons of metasomatic elements (calcium, magnesium, and strontium) that were not

present at the time the ignimbrites were metamorphosed during the hydrothermal stages of activity of the Timber Mountain Caldera. Potassium/argon ages of the zeolites obtained to date are as young as 2 million years B. P., and the high ratios of strontium isotopes ($^{87}\text{Sr} / ^{86}\text{Sr}$) are consistent with a deep source, possibly in the Precambrian basement, not rainwater. Samples of fluids enriched in alkali-earth elements have been extracted from pore space in shallow (<100 meters) ignimbrites, and these exhibit substantial enrichment in base and noble metals and rare earth elements (REE), indicative of a hydrothermal origin. Mosaic breccia cements are similarly mineralized in a manner that is not explicable in the context of the supergene/pedogenic hypothesis. On these bases we bring into question many of the conclusions of the NAS/NRC Panel Report.

The only analytical data used in the Panel report to substantiate the claim that the last and final hydrothermal event occurred some 13 to 10 million years B. P. are fission track ages of zircons embedded in the mosaic breccia cements. The Panel characterized these data by stating that:

...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. (p. 44)

However, the authors of the work stated 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. (Levy and Naeser, 1992)

Indeed, examination of the fission track data shows a multiply peaked distribution

of ages in each sample and that the youngest age is 4.8 million years B. P. In addition, the ninety percent confidence interval for this age is from 2.3 to 7.3 million years B. P. Finally, the majority of the twenty-four most probable ages are actually significantly younger than the age of the host ignimbrites, which is given by K/Ar dating as 13 million years B. P. Therefore, contrary to the Panel's description of the fission track data, most of the ages are much younger than the host ignimbrites and provide evidence that hydrothermal activity has occurred at much more recent times than considered by the Panel.

We refrain from discussion of field exposures in this review, because written descriptions do not lend themselves readily to the task. Instead, we focus on analytical results that are more amenable to a written discussion. In particular, we focus on a large quantity of geochemical and mineralogic data that were not considered by the Panel. Their section on "Geochemical and Mineralogic Considerations" is less than a page in length (p. 47), and yet leads the Panel to the crucial conclusion that the water table has been essentially static for the past 11.6 million years. This conclusion was, in turn, used to justify the dismissal of hydrothermal processes as a potential means for raising the water table. The lack of consideration of geochemical and mineralogic evidence by the Panel was a flaw in the execution of their assignment, which is stated as follows:

The panel regarded their task as not only evaluating the staff scientist's thesis, but also assessing the likelihood that the ground water level could rise to the height of the repository by any plausible geological process, or that such a rise had occurred in the past. (p. 2)

By dismissing hydrothermal processes as a factor, the Panel obtains "no" as the answer to the question: "*Has it happened?*" For reasons outlined briefly below and discussed subsequently, the correct answer might well be "yes," in which case the answer to the question "*Can it happen?*" may also be "yes" instead of "no" as the Panel concluded.

A very large set of geochemical and mineralogic data was not considered by the Panel. Specimens of vadose-zone interstitial fluids, fluids residing in ignimbrites and carbonates, glasses and alteration products, and epigenetic veins have been analyzed by project scientists. The results provide a spatial and temporal image of the post-Miocene alteration and mineralization experienced by the Yucca Mountain ignimbrites (Szymanski, 1992).

The earliest hydrothermal episode produced weak alkali-earth metasomatism (Ca + Mg ~10-25 mole % of the exchangeable cations, cf. ~3% for glass) which is pervasive in the lower part of the stratigraphic section. Associated alteration minerals include clinoptilolites with K/Ar ages ranging from 9.5 to 10.5 million years B. P., contemporaneous with the late stages of activity of the Timber Mountain Caldera. Both the strontium isotopic ratio ($^{87}\text{Sr} / ^{86}\text{Sr} \sim .709$) of whole-rock samples and spatially correlative calcites, and the carbon isotopic ratio ($\delta^{13}\text{C}$ from -2 to 4.5 per mil wrt PDB) of the calcites are indicative of parent fluids having resided in the underlying Paleozoic carbonates.

Hydrothermal activity subsequent to the Timber Mountain hydrothermal episode

differed spatially, chemically, isotopically, and in duration. The more recent metasomatism observed higher in the stratigraphic section is less pervasive, and appears to be confined to aureoles typically associated with faults and fractures. The whole-rock Ca + Mg substitution is greater than that associated with the Timber Mountain hydrothermal episode, and may be as high as 50% of the exchangeable cations. Clinoptilolites have (mixed) K/Ar ages ranging from 2 to 8.5 million years B. P. The strontium isotopic ratio ($\sim .712$) of whole-rock samples and spatially correlative calcites, and the carbon isotopic ratio ($\delta^{13}\text{C}$ from -10 to -3) of the calcites are both suggestive of parent fluids from deep-seated sources, specifically from the Precambrian basement and mantle igneous CO_2 , respectively. In contrast to the prolonged (1 million year) Timber Mountain metamorphism, hydrothermal alterations over the past 8.5 million years have been intermittent, have spanned a much greater depth range, and have been primarily associated with faults and fractures.

Remnants of late hydrothermal fluids have been separated from cores in two shallow boreholes (Smith, 1992). Relative to the local ignimbrite-based fluids, these interstitial fluids are strongly enriched not only in alkali-earth elements, but also in transition, base and noble metals and rare earth elements (REE), suggestive of a hydrothermal origin (Szymanski, 1992). In addition to the overall enrichment of REE, there is an unusual enrichment of heavy REE relative to light REE that is not shared by the host ignimbrites. It is significant that relative enrichment of heavy REE is observed elsewhere for hydrothermal solutions that are concentrated and rich in CO_2 , where carbonate anion complexing is the mechanism believed responsible (Michard and

Albarede, 1986; Michard et al., 1987).

The mineral enrichment in the trace elements of the interstitial fluids is comparable with that of the mosaic breccia cements. Similar solutions have evidently caused fragmentation of bedrock associated with the late carbonatization, in addition to late alkali-earth metasomatism and calcic zeolitization.

High temperatures of formation of shallow subsurface veins are documented by fluid inclusion temperatures well in excess of 100°C, and by elevated paleogeotherms determined from oxygen isotopic ratios. Within 30 meters of the fluid inclusion samples there are calcites with uranium series ages younger than 100 thousand years. If, as is expected, the dated calcites are representative of the ages of the nearby samples of calcite used in obtaining the inclusion temperatures, then the high temperatures obtained provide direct evidence for recent hydrothermal activity at shallow depths in Yucca Mountain and clearly contradicts the reported conclusions regarding recency of hydrothermal activity.

Consequently, these observations taken together strongly suggest that, over the last several hundred thousand years, episodes of calcite emplacement contemporaneous with local mafic volcanism have occurred at intervals that are not long in comparison with the isolation time required for a HLRW repository. Yet, evaluations by the Panel fail to even consider the great wealth of geochemical data that reveal distinct patterns of hydrothermal evolution extending throughout the Plio-Quaternary time span.

III. Hydrothermal Alteration of the Vadose Zone Ignimbrites

One of the main conclusions reached by the Panel was that the water table at Yucca Mountain had been essentially static in the post-Timber Mountain time. Citing the results of Levy (1991), the Panel stated:

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. (p. 48)

We could not find support for this conclusion from the relevant data (Figure 1). The data show minimum and maximum elevations of the occurrence of glass and zeolites, relative to the water table, as reported by Bish and Chipera (1989), Carlos et al. (1990), Sheppard et al. (1988), Carr (1982), and Carr and Parrish (1985) at the drill sites shown in Figure 2. Levy noted:

...the downward transition from vitric to zeolitized tuffs is a gross feature common to all Yucca Mountain drill holes. (Levy, 1991)

However, the inference that the water table has not risen more than about 60 m in the past 12.8-11.6 million years B. P. is not self-evident. In some drill holes, glass is present hundreds of meters below the water table. Further, zeolites (clinoptilolite, stellerite, mordenite, analcime) are present hundreds of meters above the water table in some drill holes. In the vadose zone, both vitrophyres of the Topopah Spring Member exhibit fracture-based devitrification. The distributions of zeolites and glass do not bear simple relationships with the water table, i.e. glass above and zeolites below the water table.

This casts doubt on the viewpoint that the vadose zone zeolites were formed in response to supergene/diagenetic processes. A very significant fact that is not considered by the Panel is that the zeolites have radiometric ages considerably younger than the host ignimbrites (Figure 3).

Relationships between the occurrences of zeolites and glass are clarified (Szymanski, 1992) by plotting K/Ar ages of clinoptilolites (WoldeGabriel, 1991) against depth below the deepest occurrence of glass (Figure 4). The results suggest that an upward progression of zeolitization occurred as glass became less abundant at depth. This feature is consistent with a progressive hydrothermal zeolitization.

Significant depth trends are also apparent in the chemical composition (Figure 5) of the alteration products (zeolites and whole-rock ignimbrites) and the strontium content and isotopic ratios (Figure 6) of the whole-rock ignimbrites. The deepest clinoptilolites have ages of about 10 million years B. P. and are essentially alkali (K + Na) rather than alkali-earth elements (Ca + Mg) in chemical composition. Younger, shallower clinoptilolites have alkali-earths ranging above 50% of the exchangeable cations. The strontium content of the whole-rock samples (30 to 700 ppm) is also generally much higher than for glass (19 ± 6 ppm: Peterman et al., 1991). These vast quantities of alkali-earth elements could not possibly be present if the zeolitization were of a diagenetic/supergene origin, as claimed by Levy (1991) and WoldeGabriel (1991). In regard to repository performance, it is particularly disturbing that the most strongly metasomatic zeolites containing high concentrations of total strontium and strontium-87

are also the shallowest and youngest.

A satisfactory alternative to the hypothesis of the diagenetic (supergene) paragenesis is the traditional view that zeolites are alteration products formed in response to hydrothermal metamorphism (Sheppard et al., 1988; Steiner, 1955; Coombs, 1970; Meyer and Hemley, 1961). As stated by Weiss:

The presence of extensive and pervasive propylitic alteration, \pm fluorite, in otherwise fresh tuffs of Miocene age clearly implies the existence of a large fossil hydrothermal system in Yucca Mountain, and supports our earlier contention that zeolitic alteration may not be entirely of a diagenetic or deuteric origin as is commonly believed. (Weiss, 1990)

A hydrothermal origin has been inferred for illite/smectite alteration at depths between approximately 1070 and 1525 meters in drill holes USW G-1 and USW G-2 (Bish and Aronson, 1992). K/Ar dates for these minerals average 10.4 million years B. P., suggesting that they were formed in response to a hydrothermal episode associated with the Timber Mountain Caldera, some 10 km north of the proposed repository. Clinoptilolites below a depth of about 1000 meters are of about the same age as the clay minerals, and were also presumably formed by fluids associated with the Timber Mountain hydrothermal episode. However, the younger clinoptilolites must have been formed by subsequent hydrothermal solutions much richer in alkali-earth elements. Relative to the Timber Mountain hydrothermal fluids, these solutions were also abnormally enriched in strontium-87 and depleted in carbon-13. Taken together, these isotopic characteristics were likely acquired in a deep substratum, probably the Precambrian basement. The following has been observed:

Importantly, at Yucca Mountain, the parent fluids for both the strontium-87 enriched calcites and the carbon-13 depleted calcites were also involved in (1) whole-rock strontium metasomatism, and (2) calcic zeolitization. It follows then that by proposing a supergene-pedogenic origin for the carbon-13 depleted and strontium-87 enriched calcites, the USGS investigators are in fact proposing that the supergene-pedogenic processes, in addition to being capable of (1) producing veins some 300 meters below the water table, and (2) causing a factor of 1.5 increase in the geothermal gradient, are also fully capable of causing the observed space-differential calcium, magnesium, and strontium metasomatism in large volumes of initially homogeneous ignimbrites. Such a proposition, however, is in accord neither with established geological principles nor with common sense. (Szymanski, 1992)

In summary, contrary to the opinions adopted by the Panel, the maximum and minimum elevations respectively of altered and vitric tuffs can not be taken as meaningful indicators of the range of water table changes over the past 11.6 million years. Further, the age trends of the zeolites, and their chemistry and isotopic characteristics, support the view that these are hydrothermal alteration products, rather than diagenetic or deuteric, as suggested by the Panel.

IV. Hydrothermal Processes

The lack of consideration of hydrothermal processes is, perhaps, the major scientific deficiency of the Panel's report, which stated:

Inasmuch as the only deposits associated with hydrothermal processes in close proximity to Yucca Mountain were formed more than 10 Ma during formation of the tuffs, and the only Quaternary evidence for warm springs observed by the panel was more than 55 km from Yucca Mountain, at Travertine Point (from the earliest Quaternary (2 Ma - 700 ka), the panel discounted hydrothermal systems as a potential mechanism for raising the water table level in the Yucca Mountain area. (p. 130)

The first point seems to rely on the Panel's inference regarding the stability of the water table, discussed above, and on the inferences concerning the paragenesis of surficial calcites, which are discussed subsequently. The second point, concerning the proximity of hydrothermal springs visited by the Panel, is factually incorrect. Notably, the majority of the Panel visited active hot springs in Oasis Valley, a few miles north of Beatty, that are located about 25 km from Yucca Mountain.

Thermal springs are not hard to find in the Yucca Mountain region. For example, Hill (1992) cited several examples, among them the following:

There is evidence of hydrothermal activity throughout the area, both past and present. This can be seen at Devil's Hole, the discharge point for much of the watershed of the Yucca Mountain site, where the temperature of the water measures 34°C (Hoffman, 1988).

Oxygen isotope analyses of calcite mammillary crusts in Devil's Hole range between about 14 to 17 (PDB), indicating that such low-temperature hydrothermal activity has been going on at least throughout the last 250,000 years or so (Winograd et al., 1988).

Hydrothermal activity is also indicated by the occurrence of the sepiolite mine near Yucca Mountain. Ehlman et al. (1962) described the occurrence of sepiolite in Utah and Nevada and believed these were formed by low temperature hydrothermal solutions associated with quartz, opal, and sulfide mineralization and with nearby acidic intrusive rocks and magnesium-rich carbonate rocks. (Hill, 1992)

Similarly, referring to the Wahmonie gypsum-sepiolite mound (situated about 15 km northeast of Yucca Mountain), the United States Geological Survey advised the DOE as follows:

Concurrent with drilling at Calico Hills, geophysical studies conducted at Wahmonie indicated that the granite, which occurs at the surface, would be only marginally large enough for a repository at the depth needed. These studies, plus surface mapping, also suggest that the granite within reasonable depth was probably altered by hydrothermal solution. In addition to the altered granite, local surface deposits from recent warm springs indicate upward seepage of groundwater possibly from great depth. (DOE, 1985)

In sharp contrast to the Panel viewpoints, we believe that even a casual researcher of the Yucca Mountain area can be reasonably certain about one important point. This point is: the post-Timber Mountain hydrothermal activity cannot be discounted without ignoring a large quantity of mineralogic, geochemical, isotopic, geochronological and geophysical data.

In this regard, in the following sections we will focus on the large body of quantitative data and results related to the question of post-Timber Mountain hydrothermal activity. These data and results pertain to: a) whole rock alterations, b) epigenetic mineralization of the vadose zone rocks, c) paleogeothermal gradients, d) homogenization temperatures of fluid inclusions, e) spatial distribution of the metamorphic facies, and f) interstitial fluid from the vadose zone.

Whole-Rock Alterations

The view that alteration of the Yucca Mountain ignimbrites is essentially deuteric and/or supergene/diagenetic is at odds with the fact that vast quantities (literally billions of tons) of metasomatic elements now reside in these rocks. Livingston has compiled data from Broxton et al. (1986) on the extent of whole-rock alteration, relative to glass, of ignimbrites from both above and below the water table (Figure 7), and concluded:

1. *A substantial portion of the altered rocks are 4 to 10 percent lower in silica than is glass.*
2. *Most altered rocks are 0.1 to 0.5 percent richer in titania than is glass.*
3. *Most altered rocks are richer in alumina by at least 0.5 percent than is glass, and a substantial portion is richer by 2 to 5 percent.*
4. *Most altered rocks are richer by 0.5 to 4.5 percent in iron oxide than is glass.*
5. *Most altered rocks are richer by 0.25 to 1.75 percent in magnesia than is glass.*
6. *Almost all altered rocks are 0.5 to 5 percent richer in lime than is glass.*
7. *Most altered rocks are 0.5 to 2.5 percent lower in soda than is glass, although a few are enriched by 2.5 percent.*
8. *Most altered rocks are lower in potash by 0.5 to 3.0 percent than is glass, although a few are richer by 2.5 percent. (Livingston, 1992)*

A conclusion that can be drawn from this data is that: Because metasomatic alteration is present well above the water table, and because all age-dated zeolites from the vadose zone postdate hydrothermal stages of activity of the Timber Mountain Caldera, it is inappropriate for the Panel to have discounted hydrothermal systems as a potential mechanism for raising the water table level

in the Yucca Mountain area.

Epigenetic Mineralization of the Vadose Zone Rocks

The results of electron microscope examinations of specimens of the Topopah Spring Member of the Paintbrush Tuff were reported by Carlos et al. (1990). Fracture-coating minerals were also analysed by X-ray diffraction. From these studies both deuteritic mineralization and subsequent epigenetic mineralization are apparent. The deuteritic mineralization, developed in lithophysal cavities and along cooling cracks, is represented by tridymite (sometimes transformed to cristobalite or quartz), hematite, and fine-grained manganese oxides and zeolites. Subsequent epigenetic mineralization is represented by drusy quartz, fluorite, smectite, coarse-grained zeolites and calcite. In borehole USW GU-3, drusy quartz occurs over tridymite and its pseudomorphs, and fluorite is interpreted to have been formed after tridymite and quartz. In several boreholes, coarse-grained zeolites were formed after manganese oxides and fine-grained zeolites. Many of the epigenetic zeolites are euhedral, neither crushed nor slickensided, and in a few instances are developed over slickensided fractures. Calcite appears to be the latest mineral formed, and in borehole USW G-2 calcite was observed to occur as two distinct generations separated by the deposition of heulandite. The latter observation represents strong evidence for recurrent invasion of the vadose zone by hydrothermal fluids, a possibility that was discounted by the Panel.

The epigenetic character of some of the vadose-zone mineral species was also

recognized by Weiss, who reported hand-lens and binocular microscope examinations of some 1850 meters of rock core. Among his observations were:

Fluorite is present as veins and fracture fillings and as irregular drusy coatings lining lithophysal and (or) relict pumice cavities and is not confined to great depths, or the northernmost part of Yucca Mountain, but was observed in fractures at depths as shallow as 318m and 362m in drill hole USW GU-3. (Weiss, 1990)

The presence of extensive and pervasive propylitic alteration, +/- fluorite, in otherwise fresh tuffs of Miocene age clearly implies the existence of a large fossil hydrothermal system in Yucca Mountain, and supports our earlier contention that zeolitic alteration may not be entirely of diagenetic or deuteric origin as is commonly believed. (Weiss, 1990)

These observations are again at odds with the Panel's dismissal of "...hydrothermal systems as a potential mechanism for raising the water table level in the Yucca Mountain area." Moreover, when considered in conjunction with the ages of zeolites, these data indicate post-Timber Mountain hydrothermal activity.

Paleogeothermal Gradients

Paleogeothermal gradients have been reconstructed by Whelan and Stuckless (1992) from the vertical distribution of oxygen isotopic ratios from samples of calcites and opals extracted in drill cores (Figure 8). The paleogeotherms, from 34 to 140° C/km, are much higher than the contemporary geothermal gradients, from 18 to 24° C/km, measured in the corresponding drill holes (Sass et al., 1987). The high paleogeotherms by themselves indicate that the subsurface calcites and opals are of a

hydrothermal origin, a possibility that was not considered by the Panel.

The existence of contemporary geothermal gradients that are low by Great Basin standards is explained as follows:

Heat flow is likely to vary between about 30 and 70 mWm⁻² (.75 to 1.7 HFU) within the very small area. This in turn suggests very shallow (in the range of 2.5 to 5 km) heat sources and sinks as the cause of the variation. The most likely sources and sinks would be hydrologic... From the present series of measurements; it seems clear that various fluids are moving about in the unsaturated zone, that water is moving in a very complicated manner within the saturated zone to depths on the order of 1 km, and that in the Paleozoic rocks beneath the tuffs there is also a complex hydrothermal circulation system. (Sass et al., 1983)

The area mentioned in the first sentence of this quote is shown by the dashed rectangle in Figure 2, and the reference to hydrothermal circulation in the Paleozoic carbonates pertains to well UE 25 p#1.

In an earlier study, it was noted that:

The nearly threefold variation in conductive heat flow over a lateral distance of only 25 km suggests the presence of a more deeply seated hydrothermal convective system with a net upward flow beneath Calico Hills and a net downward flow beneath Yucca Mountain. (Sass et al., 1980)

As far as conductive heat flow (unperturbed by convective circulation) is concerned Sass and Lachenbruch, 1982, conclude:

The regional heat flow from beneath the zone of hydrologic disturbance may be the same as that characteristic of the Great Basin in general (80mW/m² or 2 HFU), or it could be as high as 100 mW/m², or 2.5 HFU. (Sass and Lachenbruch, 1982)

It is evident that, in the Yucca Mountain area, there are strong indications of geothermal circulations of crustal fluids. The evidence for gangue mineralization and the young ages of zeolites are both consistent with recurring post-Timber Mountain flows of this type. Therefore the Panel's conclusion that there is no evidence for post-Timber Mountain hydrothermal activity and that renewed hydrothermal circulations are not likely is not well founded. On the contrary, there is considerable evidence supporting the opposite conclusion.

Homogenization Temperatures of Fluid Inclusions

Bish (1989) has reported homogenization temperatures of fluid inclusions in calcites from drill cores. The shallowest samples, from drill hole USW GU-3, had homogenization temperatures ranging from 101° to 227° C, at a depth of 31 meters, and from 125° to 170° C, at a depth of 131 meters. These temperatures are shown as a function of depth in Figure 9, along with other geothermal data. The ages of the calcites are not known, but U-series ages have been determined by Szabo and Kyser (1985) for nearby calcites from the same drill hole. The ages, in thousands of years B.P., are 227 ± 20 (19 meters), 26 ± 2 (40 meters), >400 (97 meters) and 30 ± 4 (100 meters).

The calcites with high homogenization temperatures were located within 30 meters of samples with ages considerably younger than 100 thousand years B.P. If the ages of the calcite samples (used to obtain fluid inclusion homogenization temperatures) span the same range as those of nearby calcites for which ages are available, and there is

little or no reason to expect that they wouldn't, then the temperatures obtained are a direct indication of very recent hydrothermal activity in the Yucca Mountain vadose zone. This directly contradicts the Panel's conclusion that there is no evidence for post-Timber Mountain hydrothermal activity and that renewed hydrothermal circulation is very unlikely.

Curiously, the Panel makes no mention of the fluid inclusion data, but recommends the acquisition of such data:

In order to avoid circular reasoning, independent estimates of the temperatures of paleo-ground waters should be made. Calcite veins intersected in drill cores should be searched for fluid inclusions. Microthermometry of the fluid inclusions will provide independent estimates of calcite precipitation temperatures. (p. 168)

The subsequent quotation, however, reveals a new complexion of the Panel's recommendation:

Further efforts should refocus away from the descending/ascending water controversy. (p. 134)

In summary, the currently available fluid inclusion data may be regarded as strong evidence for the post-Timber Mountain hydrothermal activity at Yucca Mountain. It seems to us that these data cannot be discounted and ignored and that, at the least, further inquiry is necessary.

Spatial Distribution of the Metamorphic Facies

Szymanski (1992) has constructed paleogeotherms from the observed zonation of zeolitization and illitization. The gradational sequences used for this purpose are the clinoptilolite-analcime-albite facies and the alleverdite-kalkberg clay-illite facies, respectively.

The alteration temperatures are shown in Figure 9, along with fluid inclusion data (Bish, 1989) and present-day temperatures in the drill holes from which calcite samples were analysed for their $\delta^{18}\text{O}$ content to determine paleogeotherms (Whelan and Stuckless, 1992). The position of the minimum paleogeotherm (34°C/km) on the temperature axis is indeterminate, and arbitrary surface temperatures (ambient temperature and 100°C) are used to illustrate the paleogeothermal gradient.

All of this evidence points to invasion of the vadose zone by hydrothermal fluids. Szymanski (1992) has demonstrated that some of this activity is: a) intermittently recurring and b) significantly younger than the hydrothermal stages of activity of the Timber Mountain Caldera. This interpretation directly contradicts the Panel's conclusions regarding past and possible future hydrothermal activity at Yucca Mountain.

Interstitial Fluids from the Vadose Zone

Samples of fluids residing in pore space have been separated from ignimbrite cores from two shallow (~100 m) dry-drilled boreholes. Chemical analyses of these fluids, and of fluids from below the water table, have been reported by Smith (1991). Mineral enrichment of the interstitial fluids (relative to fluids from below the water table) is illustrated in Figure 10. The alkali-earth affinity of the interstitial fluids is indicated by a tenfold enrichment of calcium and magnesium and a thirty-fold enrichment of strontium. The enrichment in trace elements, including rare earth elements (REE) and base and noble metals, is consistent with a hydrothermal source. This fluid cannot reasonably be regarded as either deuteric or diagenetic, and its alkali earth character shows a kinship with the late zeolitization and carbonatization (Szymanski, 1992).

In addition to the overall enrichment in REE, there is an unusual enrichment of heavy REE relative to light REE (Figure 11). In contrast, the host ignimbrites have the usual enrichment of light REE. Enrichment of heavy REE is attributable to carbonate anion complexing and is observed elsewhere (France, Bulgaria: Michard et al., 1987; Michard and Albarede, 1986) for hydrothermal solutions that are rich in carbon dioxide.

The chemical data for interstitial fluids are of assistance in interpreting the paragenesis of surficial calcites and breccia cements, which are discussed later.

The chemical data for shallow interstitial fluids at Yucca Mountain indicate a

hydrothermal origin, probably involving fluids high in CO_2 . Because these interstitial fluids are also high in the alkali-earth elements, in distinction to the alkali ignimbrites, they can be reasonably associated with the alkali-earth zeolites which carry radiometric ages that are as young as 2 million years.

In summary, the chemical data from samples of the interstitial fluids, in combination with the radiometric ages, indicate post-Timber Mountain hydrothermal activity occurring in the Yucca Mountain vadose zone. These data provide a strong basis for disagreement with the Panel's conclusion that: *"there is no evidence for post-Timber Mountain hydrothermal activity and that hydrothermal systems have essentially no potential to raise the water table at Yucca Mountain."*

V. Mosaic Breccias

The Panel distinguished four types of breccias at Yucca Mountain and concluded that:

None of these can be attributed unequivocally to upwelling pressurized ground water; on the contrary, evidence strongly supports a surface process origin for some. (p. 49)

This opinion differs from that of an earlier review panel convened by the DOE. Referring to the silica-cemented breccia cut by the calcite-opal-sepiolite veins in Trench #14, this panel stated:

On the basis of field inspection it may reasonably be interpreted as a hydrothermal eruption breccia. (Hanson et al., 1987)

Leaving aside contentious differences of perception of field exposures, mineral assays of Trench #14 breccia reported by Weiss (1990) provide unmistakable evidence of a hydrothermal origin. The results are shown in Figure 12. Enrichment of the rock samples is computed relative to the stratigraphically equivalent background (Castor et al., 1989). Results for lithopysal ignimbrites (in the first column of Figure 12) are indicative of the extent of deuteric enrichment of these rocks. Results in the second column are for the median of seven breccia specimens. Significant enrichment is evident for the most strongly mineralized specimens, as shown in the third column. The variation in degree of mineralization may be regarded as evidence for polygenetic formation of the breccia. Finally, the fourth column shows that the degree of enrichment

of the interstitial fluids (discussed previously) is comparable with that of the more strongly mineralized breccia specimens. The interstitial fluid enrichment, however, is unlike the deuteric mineralization exhibited by the lithophysal tuff.

In summary, the mineralization of the mosaic breccia, although disappointing from the perspective of mineral resources, is nonetheless unmistakably hydrothermal.

This conclusion is further supported by a number of independent lines of evidence. Among these are: a) occurrences of equivalent breccias in the Paleozoic carbonates, b) isotopic characteristics of the breccia cements, c) relative concentration of carbon-13, and d) fission track ages of the breccia enclosed zircons. These lines of evidence were developed by Szymanski (1992) and are summarized below.

Breccias in Paleozoic Carbonates

At localities where Paleozoic carbonates crop out near Yucca Mountain (e.g., Bare Mountain, and just north of Highway 95 some 10 km southeast of Yucca Mountain), authigenic-mineral-cemented (AMC) breccias are commonly found. These resemble the disputed AMC breccias at Yucca Mountain in every way except that the clasts are carbonate, not ignimbrite.

The Panel's postulate of syn-depositional brecciation, or brecciation caused by the deposition of younger ignimbrites, fails to explain the paragenesis of the

carbonate breccias. The postulate fails the most elementary uniqueness test.

Isotopic Characteristics of Mosaic Breccia Cements

The mineral assemblage comprised of calcite, opal A, opal CT, and sepiolite is common to mosaic breccia cements, veins, and calcretes. These three facies share the same $\delta^{18}\text{O}$ vs. $\delta^{13}\text{C}$ field, and are texturally equivalent. Both of these observations may be taken as indicating that all facies were precipitated from common solutions but with a varying degree of topographic exposure. The $\delta^{18}\text{O}$ vs. lithofacies gradient may be regarded as reflecting the combined effects of evaporative enrichment and temperature-dependent fractionation, while the $\delta^{13}\text{C}$ vs. lithofacies gradient may be regarded as reflecting the diffusional enrichment in carbon-13. For an upwelling solution, oxygen-18 enrichment would be lowest for the parent solutions of the breccia cements, higher for the parent solutions of the bedrock veins, and highest for the parent solution of the calcretes. The observed isotopic gradients (Figure 13) are just as would be expected for the precipitates of hydrothermal solutions as they rise, cool, and discharge at the topographic surface. A supergene/pedogenic mode of deposition would not produce the observed isotopic gradients.

Characteristics shared by fluids that precipitated the calcite-opal-sepiolite veins and fluids that produced the late metasomatism of the Topopah Spring Member are alkali-earth bulk composition and abnormal enrichment in strontium-87 (Szymanski, 1992). As noted by the Panel (p. 50), $^{87}\text{Sr}/^{86}\text{Sr}$ ratios for Trench #14 and Busted

Butte calcites are high, in the range 0.7119 to 0.7127. Strontium isotopic ratios for the metasomatically altered ignimbrites are also high, in excess of 0.7119 (Peterman et al., 1991). Because shallow ground waters have lower ratios, from 0.7100 to 0.7115, the Panel reached the conclusion:

It is concluded that vein calcites from Trench 14 and Busted Butte did not precipitate from analyzed ground waters. (p. 165)

What is missing from this statement is that the analyzed fluid samples are exclusively sodic-potassic in bulk composition. The host rock for these fluids consists of ignimbrites. An altogether different picture emerges if one considers the alkali-earth fluids from well (UE 25P#1) that penetrates the underlying Paleozoic carbonates. In this case, the strontium isotopic ratio is 0.7118 (Stuckless, 1990). The Panel's conclusion is therefore only applicable to the shallow sodic-potassic fluids, not to the deeper alkali-earth solutions.

In consideration of the above discussions, we observe that: Using an incomplete process of elimination, the Panel reasons that if the vein calcites could not have precipitated from fluids residing in the ignimbrites, they must have precipitated from infiltrating rainwater. A much more satisfactory alternative is that the parent fluids for the controversial calcites had resided in the Paleozoic carbonates and/or in the underlying Precambrian basement, which is the most plausible source of strontium-87.

Relative Concentrations of Carbon-13

The isotopic signature of carbon in the breccia cement (-7 to -8 per mil with respect to PDB) is similar to that present in fluid inclusions in hydrothermal calcites and that in calcites of unquestioned hydrothermal origin (Faure, 1986; Hoefs, 1987; White et al., 1990). The carbon-13 content is greater than is expected for carbonate solutions produced by known supergene/pedogenic processes (Szymanski, 1992).

We believe that the carbon isotopic ratios from the breccia cements are not consistent with the postulated supergene/pedogenic origin of these cements. We also conclude that these ratios indicate that the carbon content of the breccia cements did not originate from inorganic reservoirs such as Paleozoic carbonates. The carbon isotopic ratios, however, are consistent with the hypothesis that the carbon originated from an igneous source. This conclusion is further supported by the fact that, for the last million years, all five of the locally recognized magmatic events were accompanied by contemporaneous episodes of carbon-13 depleted carbonatization (Szymanski, 1992).

Fission Track Ages of the Breccia Enclosed Zircons

Fission track ages of zircons contained in the AMC breccia cement establish an upper bound on the age of the breccia, which dates the most recent annealing of fission tracks (at temperatures above about 200°C). Levy and Naeser reported ages for twelve zircon crystals in each of two samples, one from Trench #14 and the other from Busted

Butte. They reported as follows:

The spread of 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. (Levy and Naeser, 1991)

The Panel described these results differently:

However, within the analytic uncertainty, most of the ages are about 10-12 Ma, or about the same as those of the dominant volcanic rocks in the region. (p. 44)

This statement contradicts Levy and Naeser (1991), who attach statistical significance to the multiple age peaks exhibited for both specimens (Figure 14). Furthermore, contrary to the statements by the Panel, the K/Ar age of the host tuff is 13 million years B.P. and not 10-12 million years B.P. as implied by the Panel and secondly, most of the ages from the zircons are significantly younger than the host rock, the youngest being 4.8 million years B.P. Like the younger zircons, the metasomatic zeolites in the vadose zone also carry radiometric ages significantly younger than the age of the host ignimbrites (Figure 4).

In summary, the most recent annealing of fission tracks in zircons may have been caused by hydrothermal solutions that produced the alkali-earth zeolitization and were involved in deposition of the carbon-13 depleted and strontium-87 enriched veins. In view of this possibility, it is difficult to understand why the Panel "...discounted hydrothermal systems as a potential mechanism for raising the water table level in the Yucca Mountain area." (p. 130) On the contrary, it seems necessary to invoke post-Timber Mountain hydrothermal activity to explain the observed data.

VI. Calcite Paragenesis and Isotopic Data

Concerning the Panel's interpretations of isotopic data, their report is flawed by inconsistencies and invalid conclusions. According to the Panel:

The hypothesis of rising ground water as the origin of the calcites in the Yucca Mountain area has failed the tests of isotope geochemistry and is, in fact, contradicted by the available data. (p. 167)

This statement is not only incorrect, but also is contradicted by the Panel's own statements. Specifically, the Panel stated that:

In the discussion that follows, it will be demonstrated that known surface calcite deposits at Yucca Mountain did not precipitate from analyzed present-day ground waters. Whether or not the calcites could have precipitated from ancient ground waters cannot be proven because critical data on paleo-ground waters are lacking. (p. 150-151)

That the disputed veins did not precipitate from the present-day sodic-potassic fluids (host rock consists of alkali ignimbrites) is undisputed and is beside the point. The issue is whether these veins could have precipitated from alkali-earth fluids resembling the vadose zone interstitial fluids discussed previously. The latter fluids are enriched in base and noble metals and have REE enrichments, suggestive of a hydrothermal origin. Solutions of this kind most likely have been responsible for the observed post-Timber Mountain metasomatic zeolitization, and have probably formed the trace-element enriched breccia cement in Trench #14. The issue is whether such solutions could have also precipitated the carbon-13 depleted and strontium-87 enriched veins.

Strong evidence for a hydrothermal origin of the disputed veins is provided by the high paleogeothermal gradients obtained from oxygen isotopic ratios and by the high homogenization temperatures obtained from shallow calcites.

Further support for this interpretation is provided by two additional lines of evidence: a) isotopic comparative analysis, and b) strontium isotopic ratios. Both of these lines of evidence are considered below.

Isotopic Comparative Analysis

The NRC/NAS report stated:

Isotopic evidence shows that none of the surficial calcite deposits analyzed to date could have precipitated from known ground waters. (pp. 55-56)

The panel concludes that to date the preponderance of evidence supports the view that the calcretes and other secondary carbonates in veins of the area formed from meteoric water and surface processes. (p. 56)

In view of the fact that, at Yucca Mountain, the isotopic compositions of paleo-ground waters, the conditions of carbonate precipitation, and the post-depositional isotopic modifications are not constrained by available data, it is appropriate to consider alternative avenues of investigation. One such avenue is isotopic comparison of the Yucca Mountain veins with local and regional calcites of unquestioned hydrothermal origin.

Figure 15 shows that, in terms of $^{234}\text{U}/^{238}\text{U}$ isotopic ratio, the Yucca Mountain

calcretes, surficial veins, and subsurface veins are indistinguishable from the Furnace Creek and Amargosa Basin travertines. In contrast to the Devil's Hole veins, which are submerged, these travertines are appropriate analogs because they occur above the water table, where the leaching environment is similar to that of the Yucca Mountain vadose zone (Szymanski, 1992)

Figure 16 shows that the Yucca Mountain calcretes, surficial veins, and subsurface veins have a range of carbon isotopic ratios similar to that of Long Valley Caldera travertines and hydrothermal veins (data from White et al., 1990). An even wider range of carbon isotopic ratios is exhibited by the worldwide data compiled by Hoefs (1987) for carbonate gangue associated with various hydrothermal ore deposits. Hoefs (1987) has explained the wide range of carbon isotopic ratios observed in magmatically active regions as a consequence of dual carbon sources. Hydrothermal fluids deriving their dissolved carbon from marine limestones are relatively enriched in carbon-13 and have $\delta^{13}\text{C}$ ratios of 0 ± 2 per mil PDB. At other times, hydrothermal fluids may acquire carbon through dissolution of igneous CO_2 . Typically, such fluids are depleted in carbon-13 and have values of $\delta^{13}\text{C}$ ranging from -3 to as low as -10 per mil PDB (Faure, 1986).

Figure 17 shows that, in terms of oxygen isotopic ratios, the Yucca Mountain calcites are indistinguishable from hydrothermal carbonates elsewhere in the western Great Basin. Carbonate gangue in the Carlin disseminated gold deposits has a wider

range of isotopic ratios than the Yucca Mountain calcites. The Cortez carbonate gangue and the Amargosa Basin spring-marsh deposits also have wider ranges of oxygen isotopic ratios.

Figure 18 shows that, in terms of strontium isotopic ratios, the Yucca Mountain calcites are indistinguishable from the Devil's Hole veins, which are of undisputed hypogene origin.

Collectively, as noted above and in Figures 15-18, the similarities of the U, C, O, and Sr isotopic ratios from the Yucca Mountain veins to those from known hydrothermal deposits support the notion that the former could likewise be of hydrothermal origin.

In summary, we conclude that the Panel has elected to either dismiss or ignore the broader body of isotopic data reported in the literature. These data support the viewpoint that the disputed veins could have formed from geothermal fluids.

Origin of the Disputed Veins Based on the Strontium Isotopic Ratios

Flawed deduction of paragenesis by the Panel is exemplified readily in the case of strontium isotopic ratios. Strontium isotopic ratios of carbonates are not appreciably altered by fractionation and post-depositional modifications that affect the other isotopic ratios. The Panel correctly observed (p. 50) that the strontium isotopic ratios of calcites

(0.7119 to 0.7127) are discordant with those of analyzed sodic-potassic fluids (0.7100 to 0.7115). However, this does not mean that the disputed veins precipitated from infiltrating rainwater. With isotopic concordance as the criterion, the Panel could have considered the affinity between the Yucca Mountain calcites and present-day thermal fluids discharging at Devil's Hole. These fluids reside in Paleozoic carbonates and exhibit a strontium isotopic ratio of 0.7123 (p. 49). A similar affinity is apparent with fluids from Paleozoic carbonates in drill hole UE 25 p#1. Samples of these fluids yielded the strontium isotopic ratio of 0.7118 (Stuckless, 1990). These strontium isotopic affinities indicate that the disputed veins could be of a hypogene origin.

Similar conclusions can be drawn by asking the following two questions: (1) why are the strontium isotopic ratios so high for both the disputed veins and the metasomatically altered ignimbrites, and (2) why is the strontium content so high for the metasomatically altered ignimbrites? Strontium enrichment is evident both for altered ignimbrite relative to glass (Figure 6) and for vadose-zone interstitial fluid relative to the contemporary sodic-potassic fluids (Figure 10). A clue to the origin of the strontium is provided by its isotopic ratio. Unfortunately, no data has been reported for two of the Yucca Mountain lithostratigraphic complexes (the pristine ignimbrites and the Paleozoic carbonates), and again we have to rely on indirect data (Figure 19). Representative ratios are 0.707 for young (unaltered) ignimbrites, 0.709 for marine limestones of Paleozoic age, and 0.717 for the Precambrian basement. The metasomatic zeolitization and the latest carbonatization are associated with strontium isotopic ratios significantly higher than those expected for fluids which have acquired their strontium content from

either carbonates or ignimbrites. This is also reflected in the relatively high strontium isotopic ratios of present-day ground waters. An obvious inference is that the Precambrian basement is the most plausible source of strontium. This possibility was not considered by the Panel so that, in effect, it was dismissed without argument.

In summary, we observe that the Panel has elected not to consider all of the available strontium isotopic data. These data, in fact, support the contention that the disputed veins could have formed from geothermal fluids.

VII. Geodynamics of the Yucca Mountain Area

Considering the present state of geodynamic instability of the crust and upper mantle at and around Yucca Mountain, the Panel's discounting of *"hydrothermal systems as a potential mechanism for raising the water level"* is particularly difficult to understand. An unstable geodynamic configuration is indicated by several independent lines of evidence. Among these are: a) the results of seismic tomography studies (Monfort and Evans, 1982; Evans and Smith, 1992), b) the results of a seismic reflection survey (Brocker et al., 1989), c) considerations of local magmatic activity during the Plio-Quaternary time span (Noble et al., 1991; Szymanski, 1989 and 1992), and d) the results of in situ stress measurements (Healy et al., 1984; Szymanski, 1989).

With reference to the contemporary geodynamic configuration of the Yucca Mountain region, perhaps the most illuminating are the results of seismic tomography studies. These studies were performed initially by Monfort and Evans (1982), and later by Evans and Smith (1992), and are summarized in Figures 20 through 22. From these figures, it may be inferred that, locally, the lower crust and uppermost mantle are in a state of incipient/partial melt. In this regard, Figure 21 indicates that the upper mantle to the east-southeast of Yucca Mountain has anomalously low velocities. The absolute values of the velocities are not specified and the variation percentages reflect changes relative to the horizontally averaged means. The mean values for P-wave velocities in the upper mantle of the Basin and Range are known to be low relative to stable continental areas (e.g., Archambeau et al., 1969), so that 3% decreases are significant.

Such low velocities have typically been interpreted as being indicative of incipient/partial melting, with the amount of decrease in the velocity being proportional to the degree of melting.

The same considerations regarding the occurrence and manifestation of partial/incipient melt apply to the crust. In this regard, higher velocity variations for the crust are shown in Figure 22. Here, the lower-than-average velocities imply some degree of melting in the lower crust. From the results shown in the figure, it is evident that an anomalously low velocity zone exists beneath both Crater Flat and over the entire width of Yucca Mountain. The most extreme decrease in P-wave velocity is directly beneath Yucca Mountain, while the low velocities beneath Crater Flat are at mid-crustal depths and not as extreme. However, the Panel report commented on these results in the following terms:

Analysis of far-traveled earthquake waves (P-waves) passing nearly vertically through the crust and upper mantle beneath Yucca Mountain and surrounding regions (Evans and Smith, 1992) shows no evidence of a low velocity feature that would suggest a volume of molten rock (or magma chamber) beneath Yucca Mountain. (p. 98)

While evidence of a magma chamber is not evident, this is not by any means the whole issue. For the Yucca Mountain region, partial/incipient melting is the most likely source of recent volcanism. Indeed, it is just such a zone in the upper mantle that appears to be responsible for the recent (basaltic) volcanism in Crater Flat.

The observed distribution of seismic velocities suggests both elevated

temperatures and high lateral temperature gradients in the middle/lower crust. Under these circumstances, convective circulations of intracrustal fluids constitute a thermodynamic necessity. These rather obvious conditions were not addressed by the Panel, instead the focus was entirely on whether or not a magma chamber might be present. This addresses an extreme case scenario and avoids confronting the issue of whether the observed crustal velocities indicate an unstable situation that could result in hydrotectonic disturbances at Yucca Mountain.

Other measurements that are important to an assessment of the geodynamic stability of the site are the in situ stress measurements, such as those obtained by Healy et al., (1984). These observations, while critical to an assessment of suitability of the Yucca Mountain site to accommodate a high level repository, were not considered by the Panel. Contrary to the Panel's assessment that the Yucca Mountain area is not likely to experience a large earthquake in the near future, the stress measurements imply the opposite. The recent earthquake activity near Yucca Mountain appears to indicate that an unstable stress state, rather than a quasi-stable state, actually prevails.

We find that these geodynamic data are of paramount importance in considering the suitability of Yucca Mountain to accommodate a high level nuclear repository. Consideration of these data by Szymanski (1989 and 1992) leads to the overall conclusion that the local hydrologic system is profoundly influenced by tectonic factors. The abnormal geothermal conditions at depth create a situation whereby Rayleigh-Bernard instabilities are intrinsic elements of

the local hydrologic regime. Evidence that the local rocks are deforming leads to another important conclusion, specifically that the hydraulic conductivity structure is controlled by in situ stress and is subject to significant temporal changes. With both of these factors present (i.e., convective boundary conditions and in situ stress dependence of hydraulic conductivity) the Yucca Mountain hydrologic system must be regarded as susceptible to episodic changes. This possibility has not been considered by the Panel, and in our view, by itself invalidates major conclusions reached by the Panel.

VIII. Hydrologic Behavior Inferred from Modeling Studies

The NAS/NRC Panel purports to examine the extent of hydrologic disturbance that might be produced by a local igneous intrusion and/or by a local earthquake without regard for interactive processes affecting crustal fluids. Furthermore, the resulting estimates of hydrotectonic effects are flawed on two counts: (1) observed behavior at other tectonically active regions is either ignored or misinterpreted, and (2) numerical models employed by the Panel fail to account for first-order processes that govern coupled hydrotectonic interactions.

Analyses of the type presented in the Panel report might be useful for some purposes, but are grossly inadequate for use as the basis for the Panel's strong conclusion that:

...stress/strain changes resulting from an earthquake are inadequate to cause more than a few tens of meters rise in the water table based on the convergence of the results of a variety of models and assumptions, especially if the deep carbonate aquifer is as incompressible as the limited data suggest. (p. 116)

The Panel's analysis of the effects of a volcanic intrusion is even less representative, yet the Panel's opinion of benign behavior is more strongly depicted. In this regard, the Panel stated:

...a 25 m rise in water table is clearly a conservative upper bound estimate for the expected form of intrusion in the Yucca Mountain region. (p. 101)

The essential deficiency is that numerical models are applied to predict behavior of the system without first demonstrating some capabilities for simulating actual hydrotectonic behavior. Furthermore, the numerical models used as the basis for Panel interpretations fail to account for even first-order processes.

Some of the more serious problems in their representations are: First, that fluid flow is wrongly assumed to take place exclusively through interstices, even though the Panel acknowledges elsewhere in the report (p. 174) that this form of diffusive flow is unimportant compared with channeled flow through networks of fractures. After concluding that an earthquake can only cause small changes in the water table, the Panel noted that if the fractured system were more accurately modeled:

It may then be possible to determine if physically reasonable conditions consistent with an hypothesis of seismically-driven flooding of the repository horizon would develop at the site. (p. 118)

These are clearly inconsistent statements and the Panel's conclusions regarding small changes are unjustified. Second, that flow properties are erroneously assumed to remain invariant when subjected to tectonically induced changes in stress and strain. Given the importance of fracture flow, such tectonically induced changes can reasonably be expected to fundamentally restructure the flow system. Third, that thermal convection of fluids is inappropriately assumed to occur in isolation from flow that is induced by rock deformation. For the case of an earthquake-induced flow, the effects of thermal convection are simply ignored; whereas, for the case of an igneous intrusion, the two first-order flow-inducing effects are treated as independent

noninteractive processes. Fourth, that the magmatic environment of the Yucca Mountain area has not been considered in evaluations of hydrotectonic processes and interactions. For example, deep seated fluids can absorb large quantities of CO₂ that are introduced in association with local magmatic processes. As fluid pressures reduce in response to local strains or to seismically induced flow, CO₂ can be expected to come out of solution and form gas bubbles. The accompanying reduction in fluid density introduces substantial buoyancy forces which promote fluid migration and further emergence of CO₂ in a positive feedback mode, accelerating the process to potentially explosive proportions. Such a mechanism could account for hydrothermal eruption breccias identified at Yucca Mountain (e.g., Hansen et al., 1987; Szymanski, 1989, 1992; Archambeau and Price, 1991). Fifth, that dissolved minerals also influence flow paths, as evidenced by the abundant networks of veins found throughout Yucca Mountain. The lesson is that fracture conduits, that once provided flow paths for mineralized fluids, have since become plugged. However such phenomena are again not considered in the reported evaluations of hydrotectonic interactions.

Given the inadequate formulations used by the Panel, the modeling results can hardly be expected to accurately predict or bound hydrotectonic interactions at Yucca Mountain. Whereas hydrothermal processes are commonly associated with igneous activity, the Panel report did not consider such associations, and fails to provide a single example in which active volcanism has had benign effects on the hydrologic system of the type interpreted for Yucca Mountain. Hence, conclusions about how a tectonic event might influence the hydrologic system are substantively without merit. Neither the

amplitude of changes in water table nor the dimensions of the zone of influence are based on observational data or representative analysis. This leaves the Panel's conclusions without justification and, hence, are unwarranted.

Some observational data are cited for earthquake-induced effects on ground water at other sites where conditions differ in important respects from those at Yucca Mountain. For example, most of the cited earthquakes did not occur in response to tectonic extension where normal faulting tends to relieve extensional strain and generally compresses the effected rock. In contrast, earthquakes responding to tectonic compression (e.g., reverse faulting earthquakes) tend to release subhorizontal compression and dilate the effected rock, so that a rise in the water would not be expected

Two earthquakes studied carefully for post-seismic changes in surface outflow of water, namely the 1959 Hebgan Lake ($M=7.3$) and the 1983 Borah Peak ($M=7.0$) earthquakes, however, occurred under conditions of crustal extension similar to those at Yucca Mountain. For these two events little is known about resulting long term changes in the water table. In particular, most of the observed hydrologic effects pertain to volumes of mobilized groundwater. In this regard, however, increased stream flows within several tens of kilometers from the surface ruptures indicate that large quantities of water, ranging from .2 to .8 km^3 (Wood and King, 1991), were mobilized during a period of about one year after the respective earthquakes. By modeling amplitudes and spatial distributions of recorded outflows, Wood and King deduced that fluids appear to have been mobilized from depths of at least 5 km in both cases. If comparable volumes

(i.e., a significant fraction of a km^3) were to be mobilized in response to an earthquake at Yucca Mountain, there would be ample fluid volume to flood the vadose zone over an extended area and still produce large volumes of surface runoff. To illustrate by example, we note that 0.3 km^3 of mobilized fluids would suffice to fill fractures that occupy 10^{-4} of the total volume of the medium. In a 0.5 km thick vadose zone extending over an area the size of the Nevada Test Site (about $3,000 \text{ km}^2$), half of the mobilized fluid (0.15 km^3) would still overflow to and discharge at the land surface. Furthermore, the resulting increase in hydraulic head that would accompany a 0.5 km rise in the water table elevation is within the range of average changes in stress (i.e., tens of bars) interpreted from historical earthquakes (e.g., Kanamori and Anderson, 1978).

In summary, while observational data are not available for direct interpretation of earthquake effects on a deep water table of the type found at Yucca Mountain, reasonable extrapolations of available data, by Wood and King (1992) in particular, strongly imply that the deep water table could well rise hundreds of meters in response to a local earthquake. The Panel failed to consider relevant evidence. Clearly the answer to their question "*Can it happen?*" would have to be, on the basis of these observations alone: "Very likely."

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LIST OF FIGURES

- Figure 1: Distributions of glass and zeolite relative to the water table.
- Figure 2: Location of drill holes discussed in text.
- Figure 3: Relationship between the K/Ar ages of clinoptilolites and ages of host rocks.
- Figure 4: K/Ar ages of Yucca Mountain clinoptilolites as a function of availability of vitric material.
- Figure 5: K/Ar ages of the Yucca Mountain clinoptilolites, as a function of depth for individual boreholes, and mol % of exchangeable cations.
- Figure 6: Depth comparison $^{87}\text{Sr}/^{86}\text{Sr}$ ratios, Sr concentrations, and major cation concentrations of alteration products..
- Figure 7: Representative alteration in concentration of components of whole-rock drill core samples from Yucca Mountain.
- Figure 8: Paleo-geothermal gradients determined by Whelan and Stuckless.
- Figure 9: Comparison of contemporary and paleo-temperatures.
- Figure 10: Mineral enrichment of vadose zone interstitial fluids.
- Figure 11: Chondrite-normalized REE abundance patterns.
- Figure 12: Mineral enrichment of breccia cement.
- Figure 13: Comparison of the isotopic signatures of carbon and oxygen.
- Figure 14: Fission track ages of zircons from breccias at Busted Butte and Trench #14.
- Figure 15: Comparison of uranium isotopic ratios.
- Figure 16: Comparison of carbon isotopic ratios.
- Figure 17: Comparison of oxygen isotopic ratios.
- Figure 18: Comparison of strontium isotopic ratios.
- Figure 19: Strontium isotopic ratios of unaltered ignimbrites, paleozoic carbonates and Pre-Cambrian rocks of the western Basin and Range Province.
- Figure 20: Maps of the seismic station distribution and principal topographic features in the Yucca Mountain and Nevada Test Site areas.
- Figure 21: Crust and upper mantle compressional velocity variations near Yucca Mountain.
- Figure 22: Crustal compressional velocity variations at Yucca Mountain.

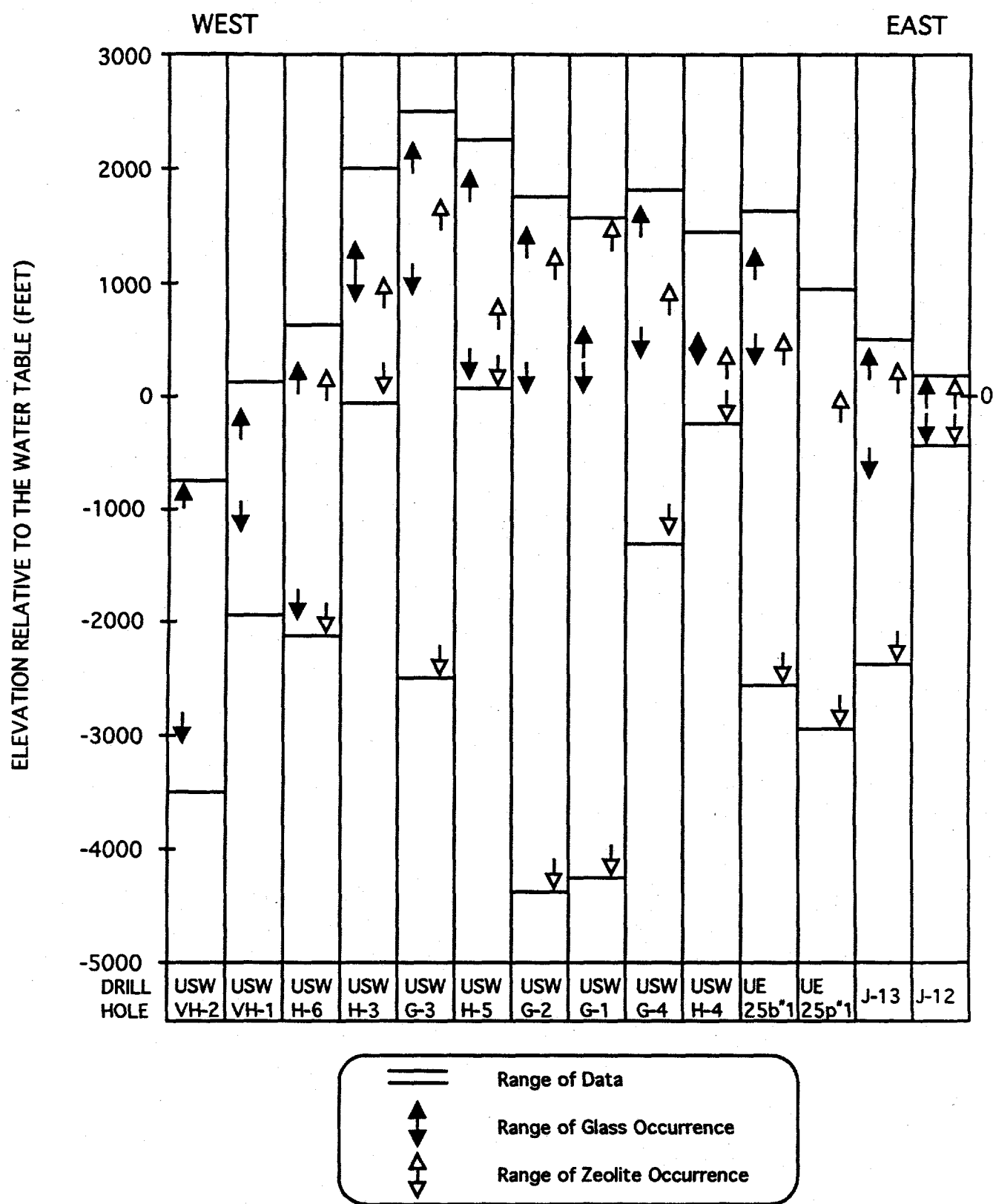


Figure 1. Distributions of glass and zeolite relative to the water table. Drill hole locations are illustrated in Figure 2.

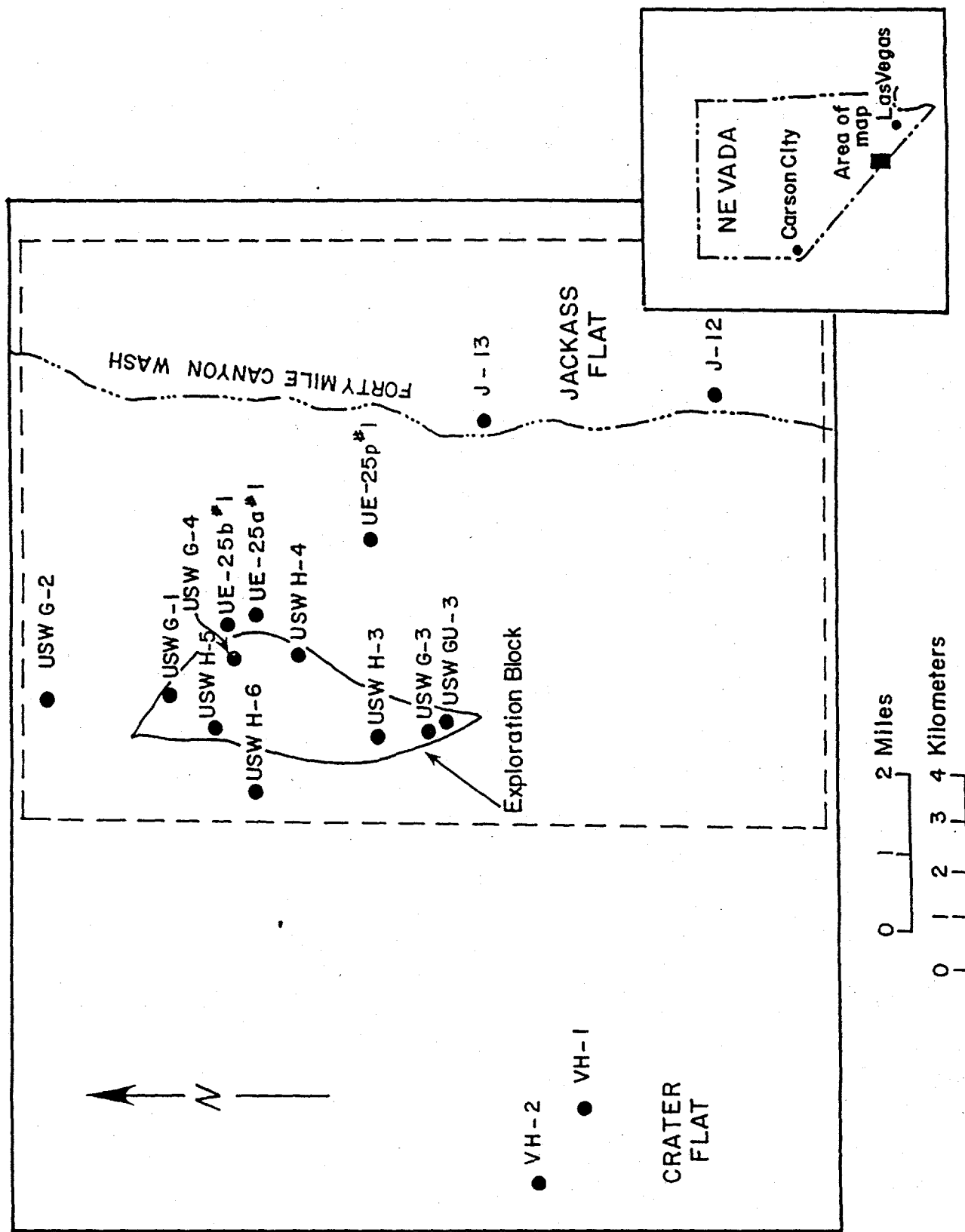


Figure 2. Location of drill holes discussed in text. Modified from Levy (1991) and Sass et al. (1981).

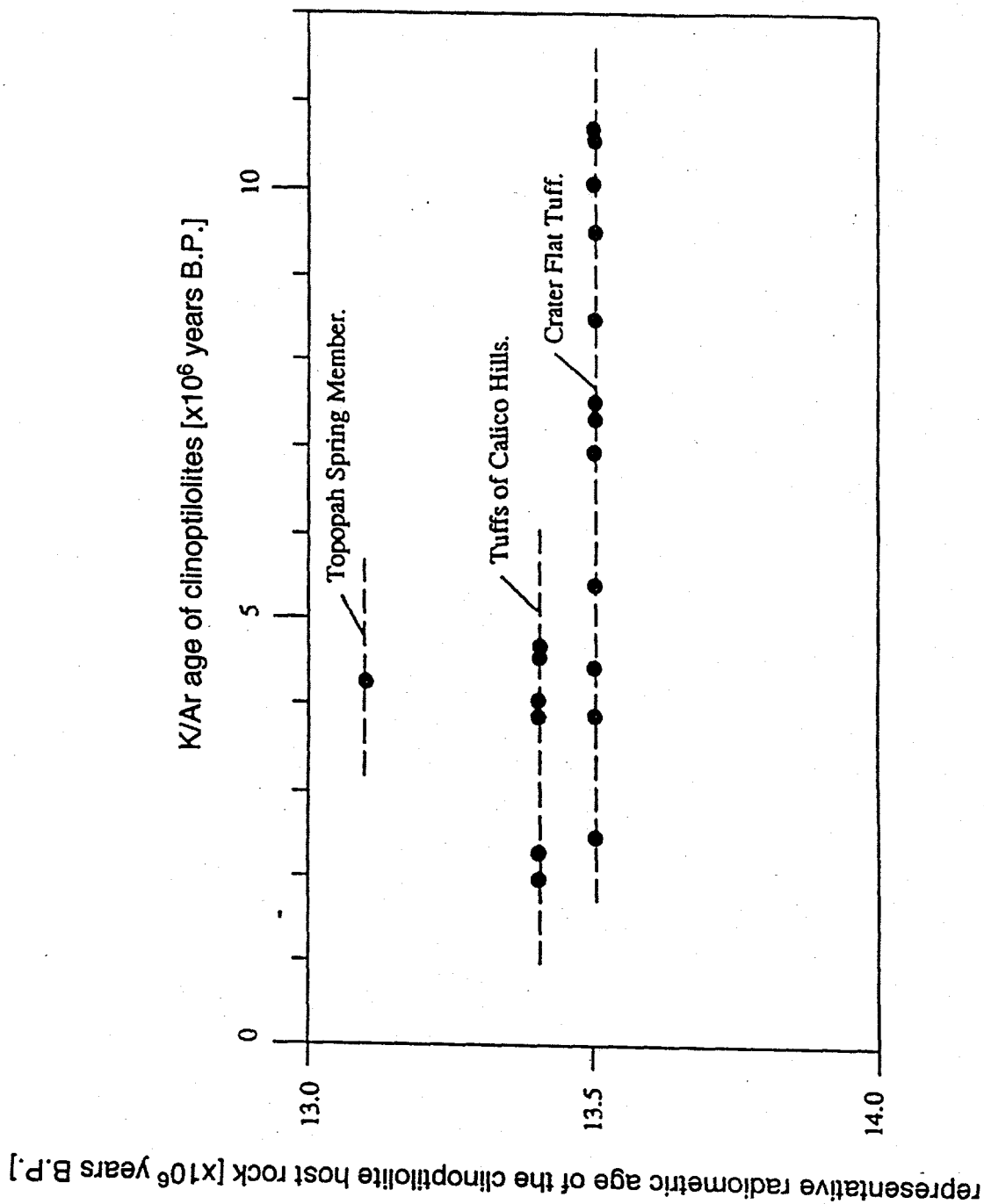
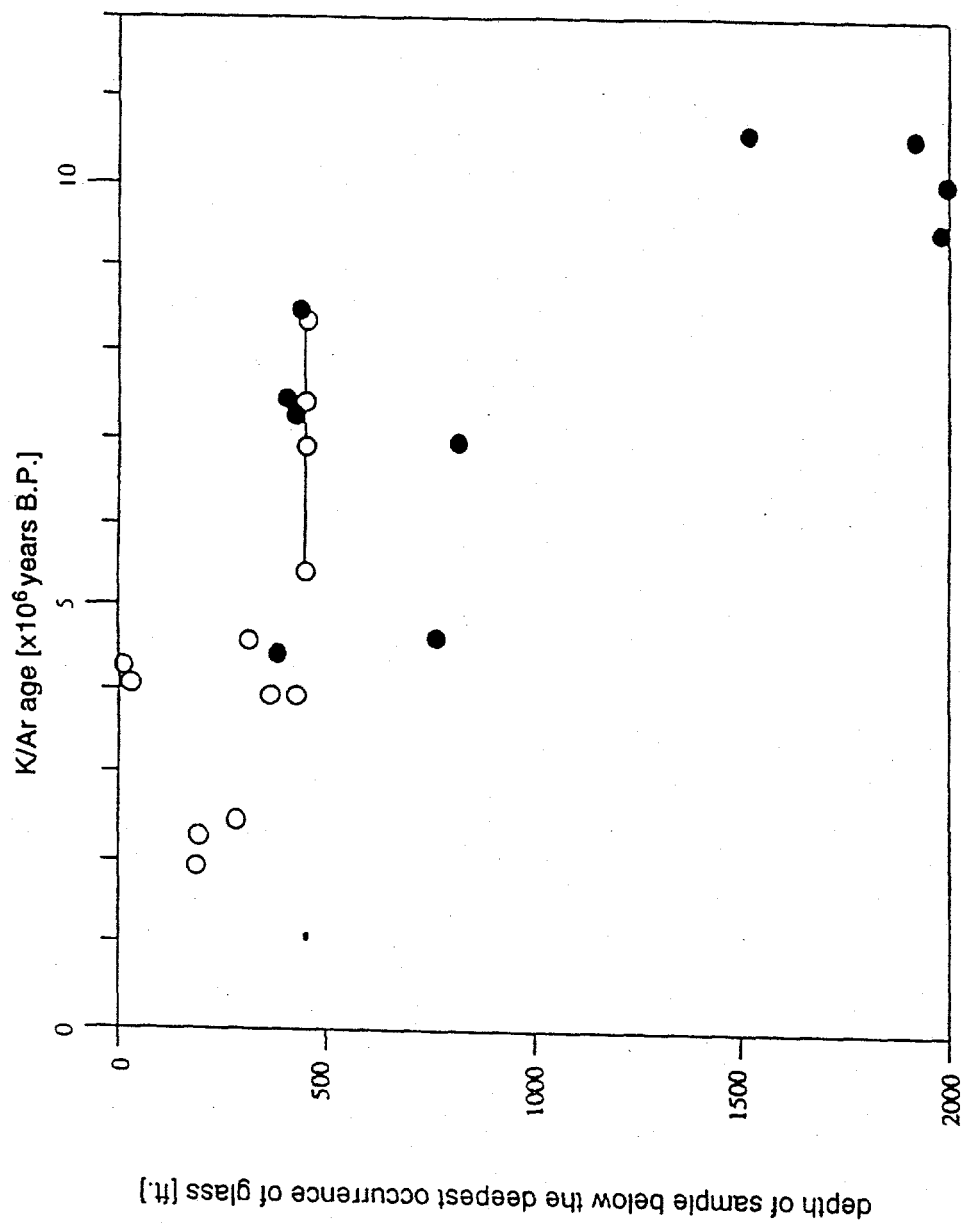


Figure 3. Relationship between the K/Ar ages of clinoptilolites and ages of host rocks. From Szymanski (1992); data are from WoldeGabriel (1991).

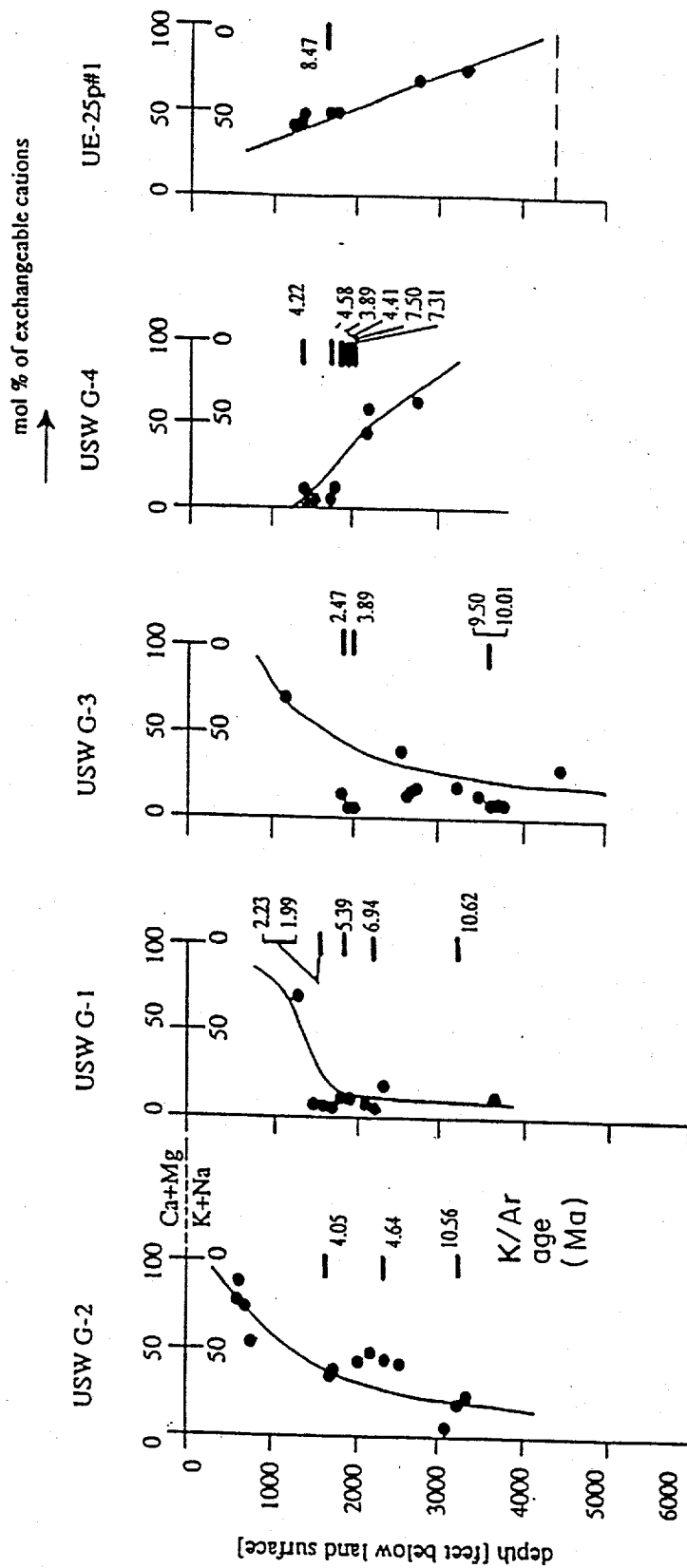


Key:

O - clinoptilolite sample from the vadose zone

● - clinoptilolite sample from below the water table

Figure 4. K/Ar ages of Yucca Mountain clinoptilolites as a function of availability of vitric material (Szymanski, 1992).

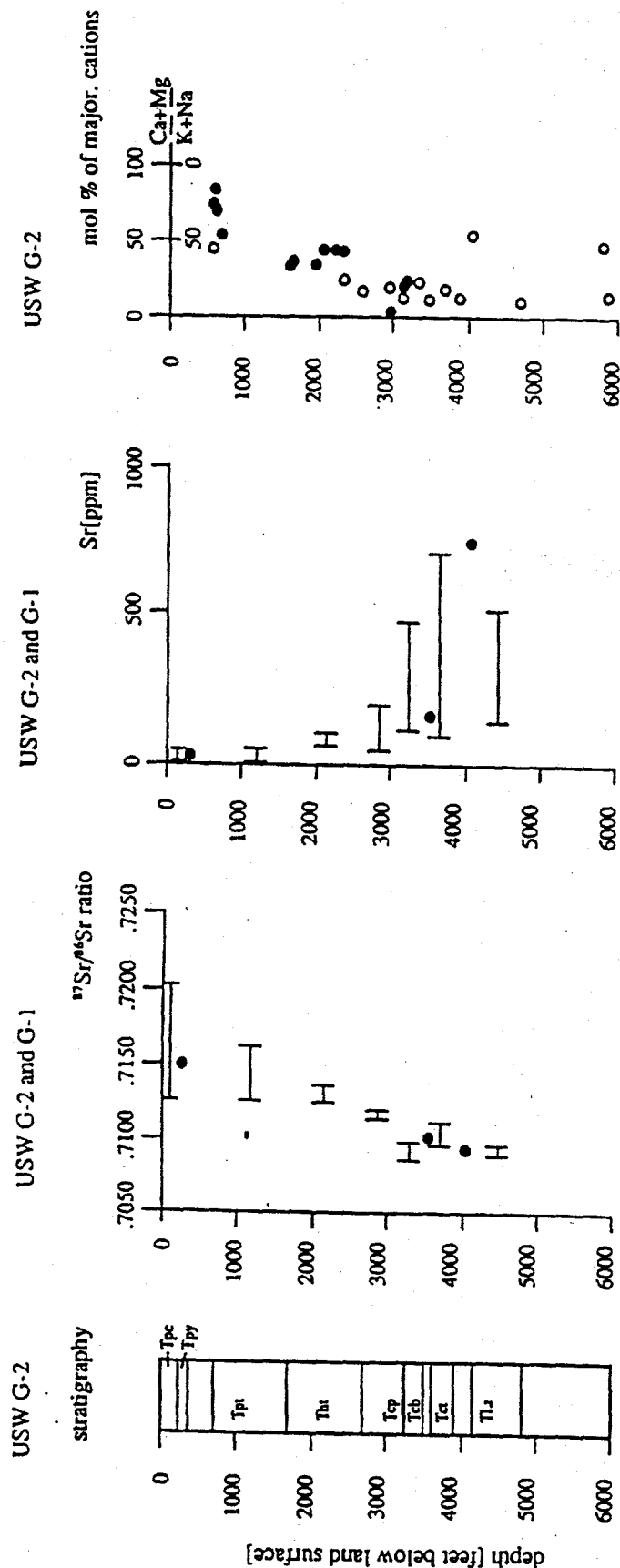


Note:

a) mol % of exchangeable cations are from Broxton et. al., (1986)

b) K/Ar ages are from WoldeGabriel (1990)

Figure 5. K/Ar ages of the Yucca Mountain clinoptilolites, as a function of depth for individual boreholes, and mol % of exchangeable cations. From Szymanski (1992).



Explanation:

- - major cation content of clinoptilolite samples
- - major cation content of whole-rock samples
- bar indicates range of values for either $^{87}\text{Sr}/^{86}\text{Sr}$ ratio or concentrations of Sr (whole-rock samples)
- Tpc - the Tiva Canyon Member, Tpy - the Yucca Mountain Member, Tpt - the Topopah Spring Member, Tht - the Tufts of Calico Hills, Ttp - the Prow Pass Member, Tcb - the Bullfrog member, Tct - the Tram Member, and Tlt - the Lithic Ridge Tuff.

Figure 6. Depth comparison - $^{87}\text{Sr}/^{86}\text{Sr}$ ratios, Sr concentrations, and major cation concentrations of alteration products (whole-rock cores and clinoptilolites).

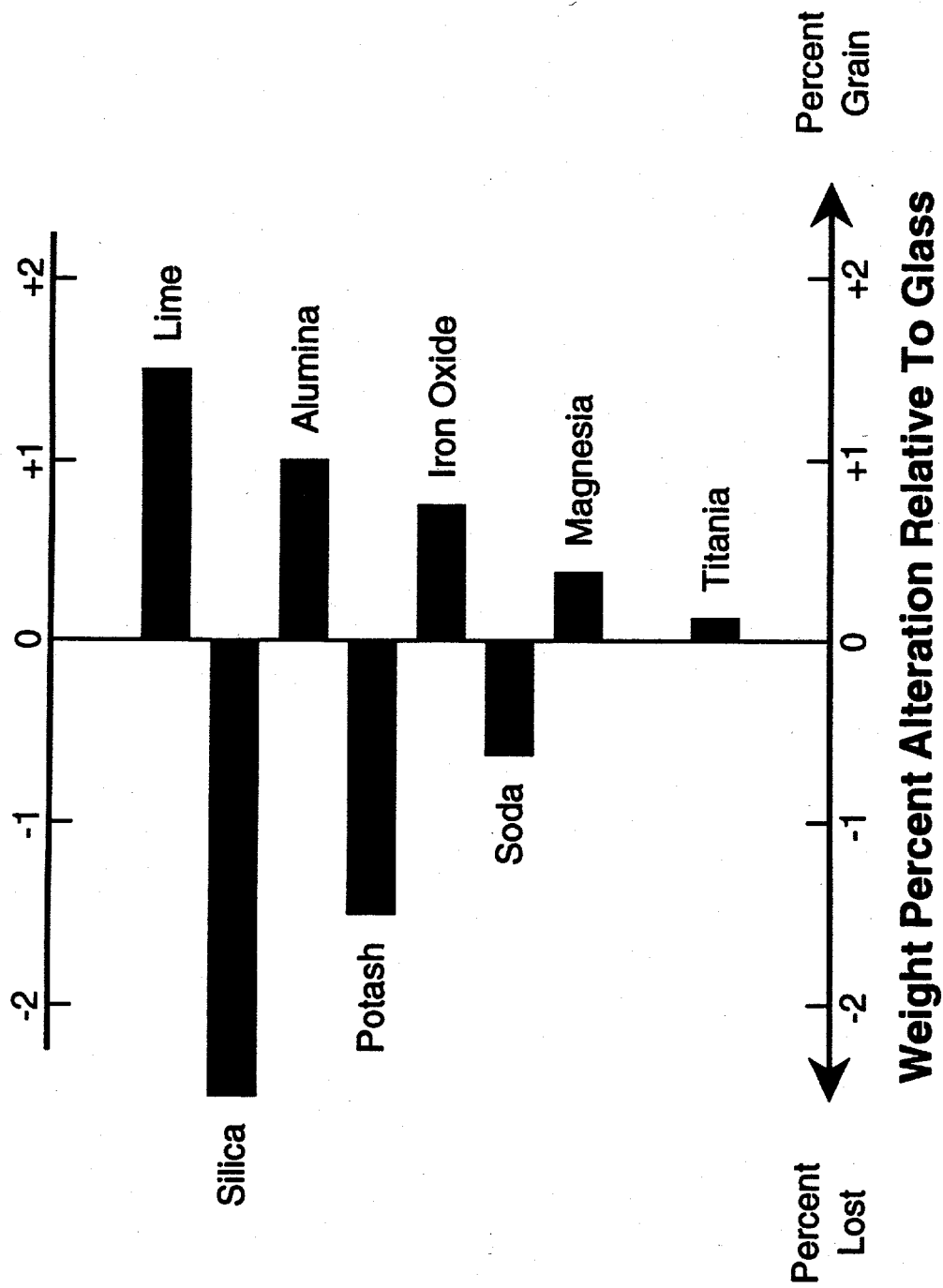


Figure 7. Representative alteration on concentration of components of whole-rock drill core samples from Yucca Mountain. Data from Livingston (1992).

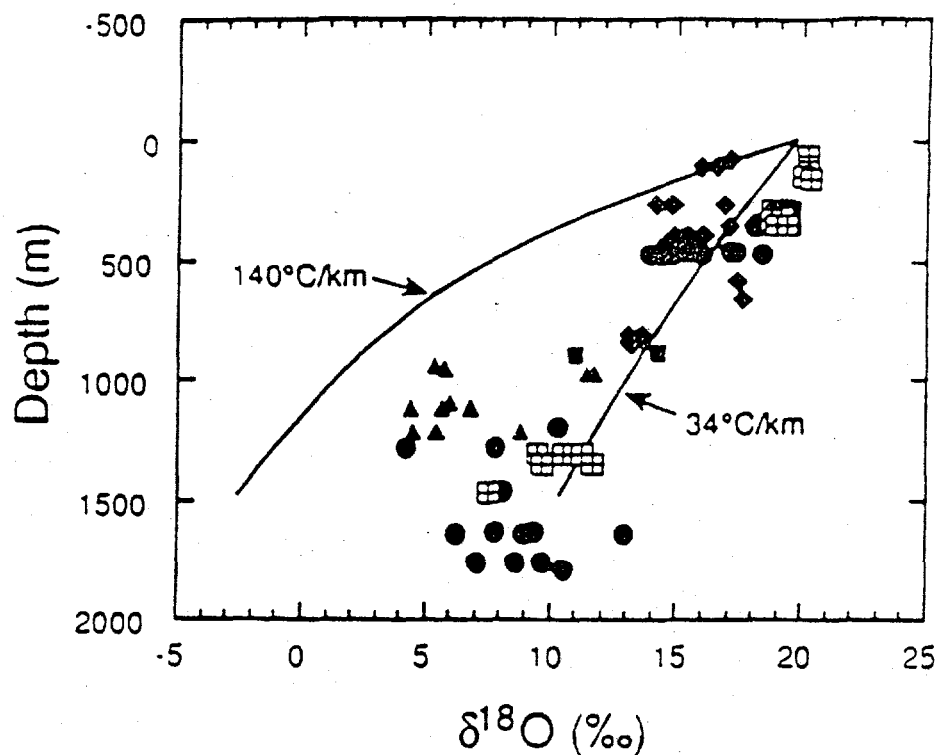


Figure 8. Paleo-geothermal gradients determined by Whelan and Stuckless (1992) from the depth-distribution of isotopic ratios of oxygen-18 and -16 in boreholes USW G-1, G-2, G-3, G-4 and UE-25b#1. Contemporary geothermal gradients measured in these boreholes are 18, 24, 22, 24, and 20° C/km respectively (Sass et al., 1987). Paleo-gradients during crystallization of the calcitic veins were at least 50% greater than the contemporary gradient.

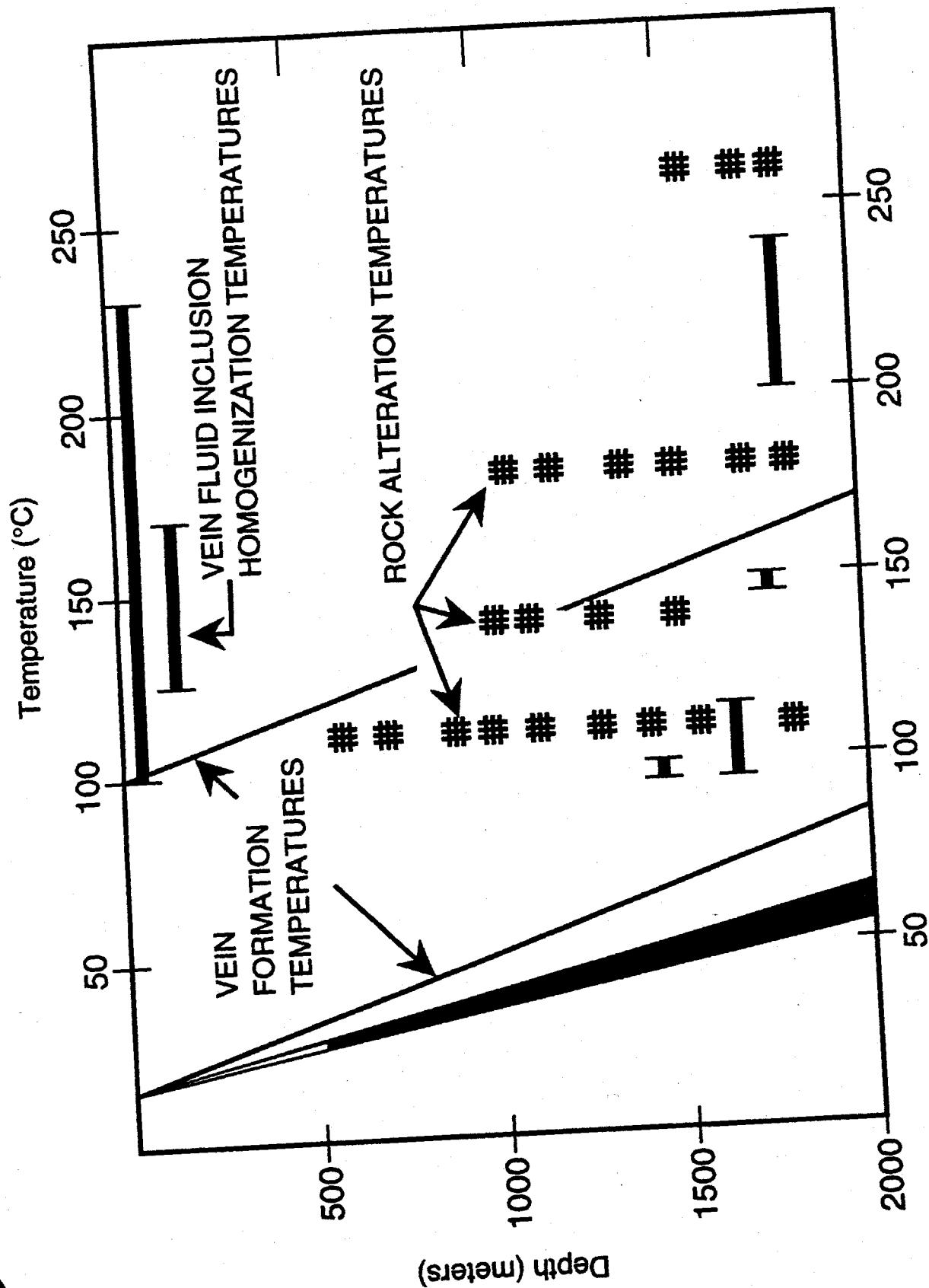


Figure 9. Comparison of contemporary and paleo-temperatures. The minimum paleo-gradient (34/km) determined from oxygen isotopic ratios is illustrated for two arbitrary surface temperatures (ambient and 100° C).

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.

Figure 10. Mineral enrichment of vadose-zone interstitial fluids relative to well waters residing in ignimbrite fractures.

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); enrichment relative to average concentrations for the Yucca Mountain area (Castor et al., 1989).

 **Data from Smith (1991); enrichment relative to well water (See Figure 10).

Figure 12. Mineral enrichment of breccia cement: results for lithophysal tuff and interstitial fluids are shown for comparison.

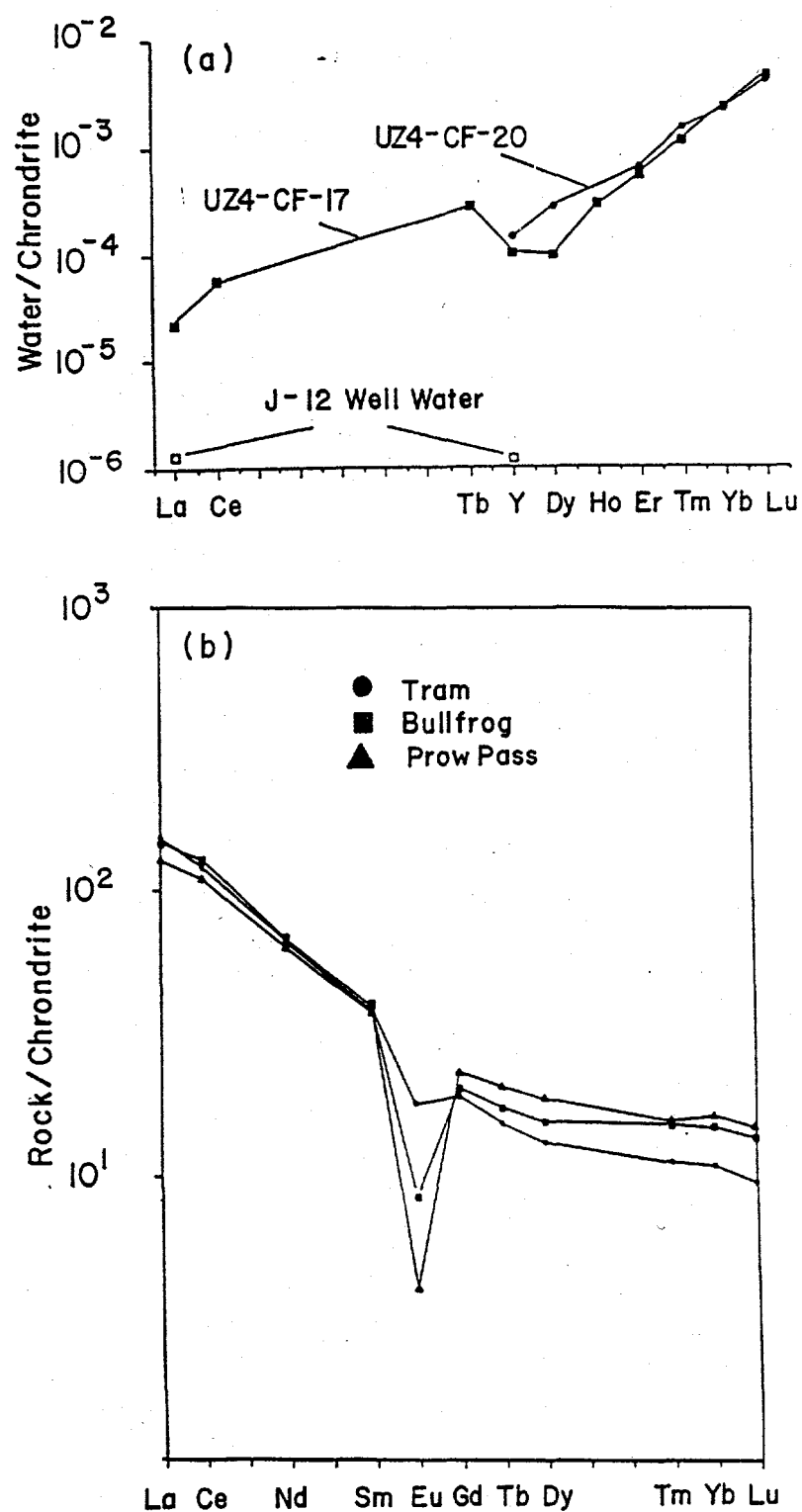
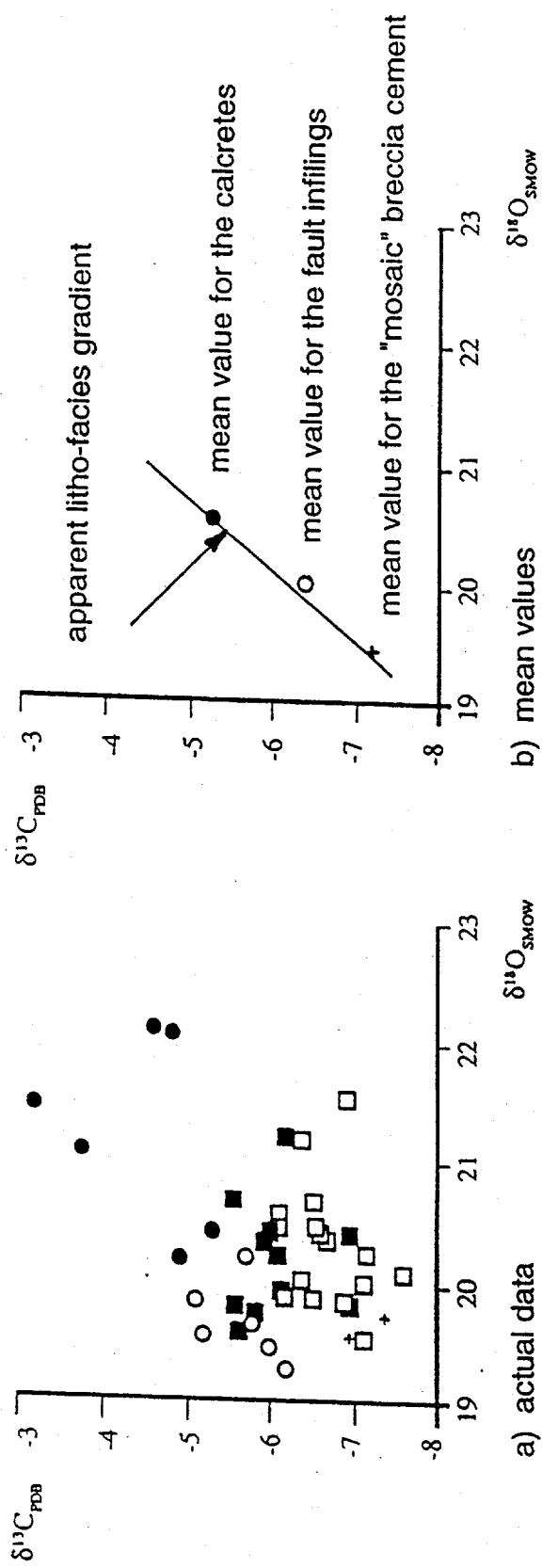


Figure 11. Chondrite-normalized REE abundance patterns. a.) interstitial fluids and well water residing in ignimbrite fractures: data from Smith (1991). b.) Crater Flat ignimbrites: data from Scott and Castellanos (1984). Heavy REE enrichment for interstitial fluids is due to high CO_2 pressure.



Explanation:

- - Trench 14 calcretes
- - Busted Butte calcretes
- - Trench 14 fault infilling
- - Busted Butte fault infilling
- + - Trench 14 and Busted Butte "mosaic" breccia

Figure 13. Comparison of the isotopic signatures of carbon and oxygen in samples of "mosaic" breccia cement, surficial veins, and local calcretes (Whelan and Stuckless, 1990). The isotopic gradients are consistent with precipitation from rising, cooling solutions.

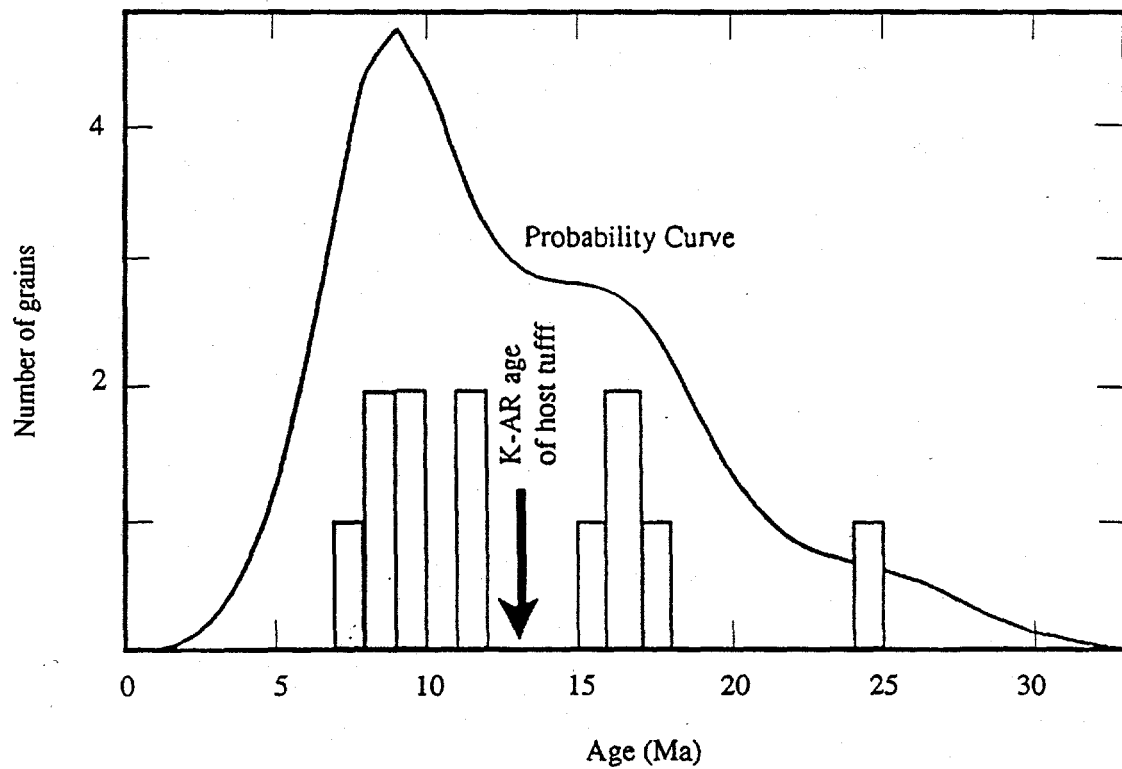
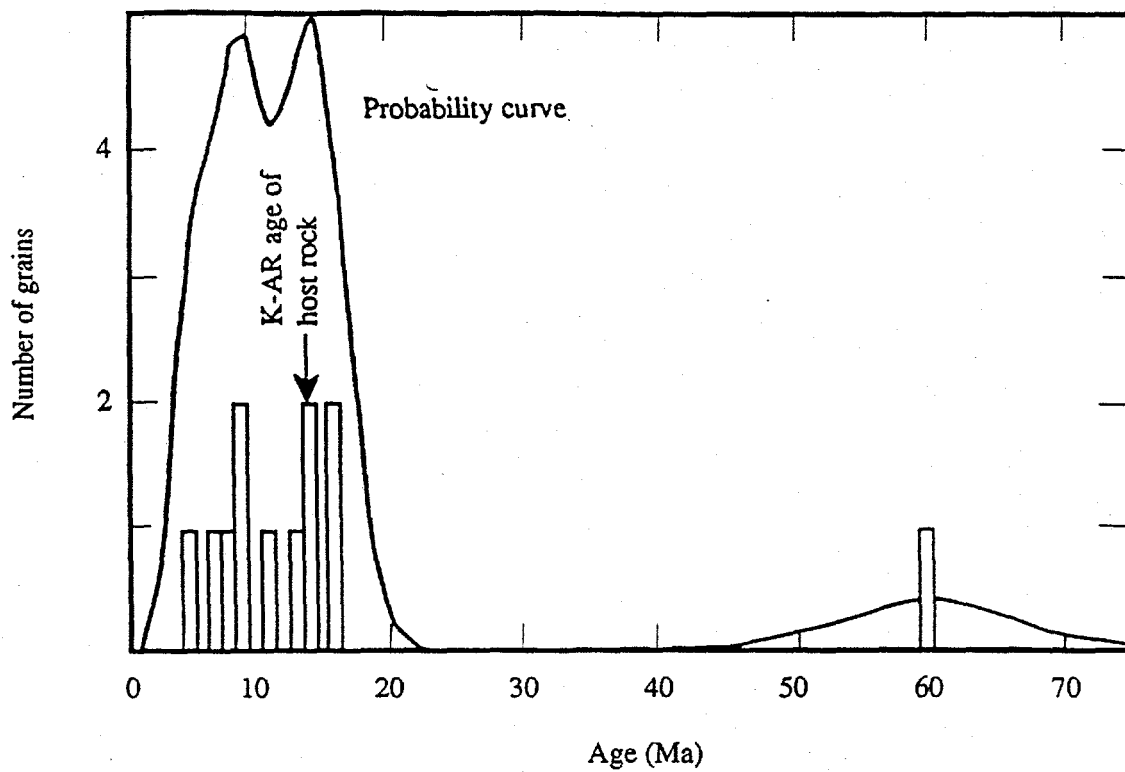


Figure 14. Fission track ages of zircons from breccias at Busted Butte (top) and Trench #14 (bottom). From Levy and Naeser, 1991.

Amargosa Basin travertine veins and calcitic deposits.

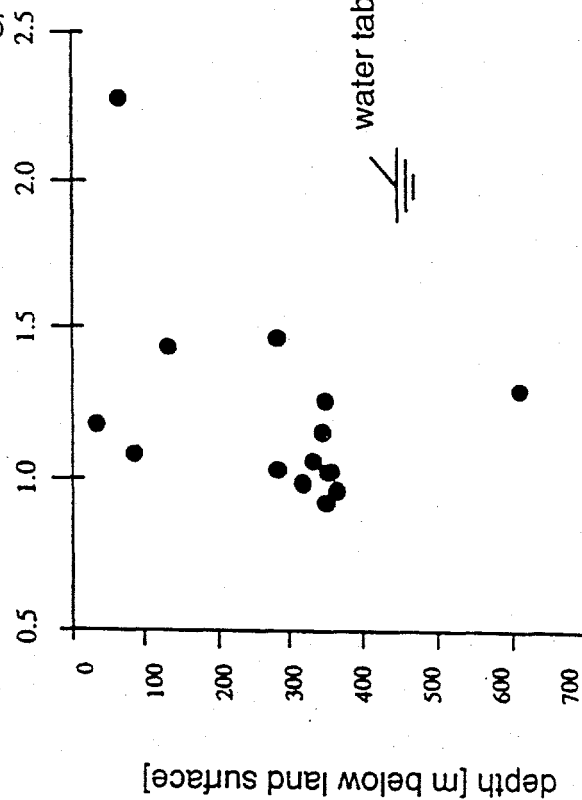


Furnace Creek travertine veins.

Yucca Mountain calcretes and surficial calcite veins.



$^{234}\text{U}/^{238}\text{U}$ ratio.



water table in borehole UE-25a#1.



Figure 15. Comparison of uranium isotopic ratios for regional thermal analogs (Szabo and O'Malley, 1985), Yucca Mountain calcretes and surficial veins (Szabo et al., 1981; Szabo and O'Malley, 1985) and subsurface veins (Szabo and Kyser, 1985). From Szymanski (1992).

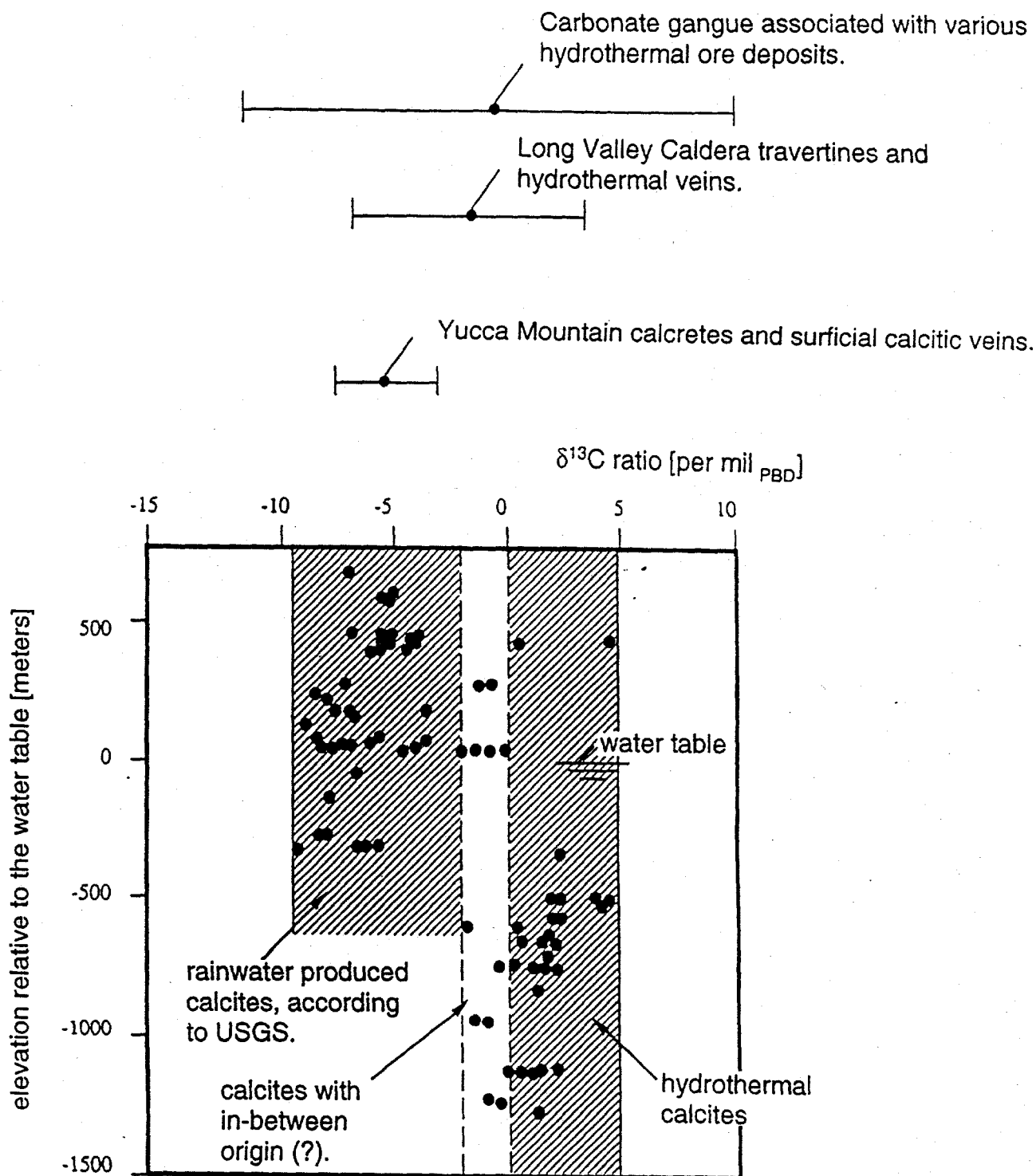


Figure 16. Comparison of carbon isotopic ratios for thermal analogs (Hoefs, 1987; White et al., 1990), Yucca Mountain calcretes and surficial calcitic veins, and subsurface veins (Whelan and Stuckless, 1991). From Szymanski (1992).

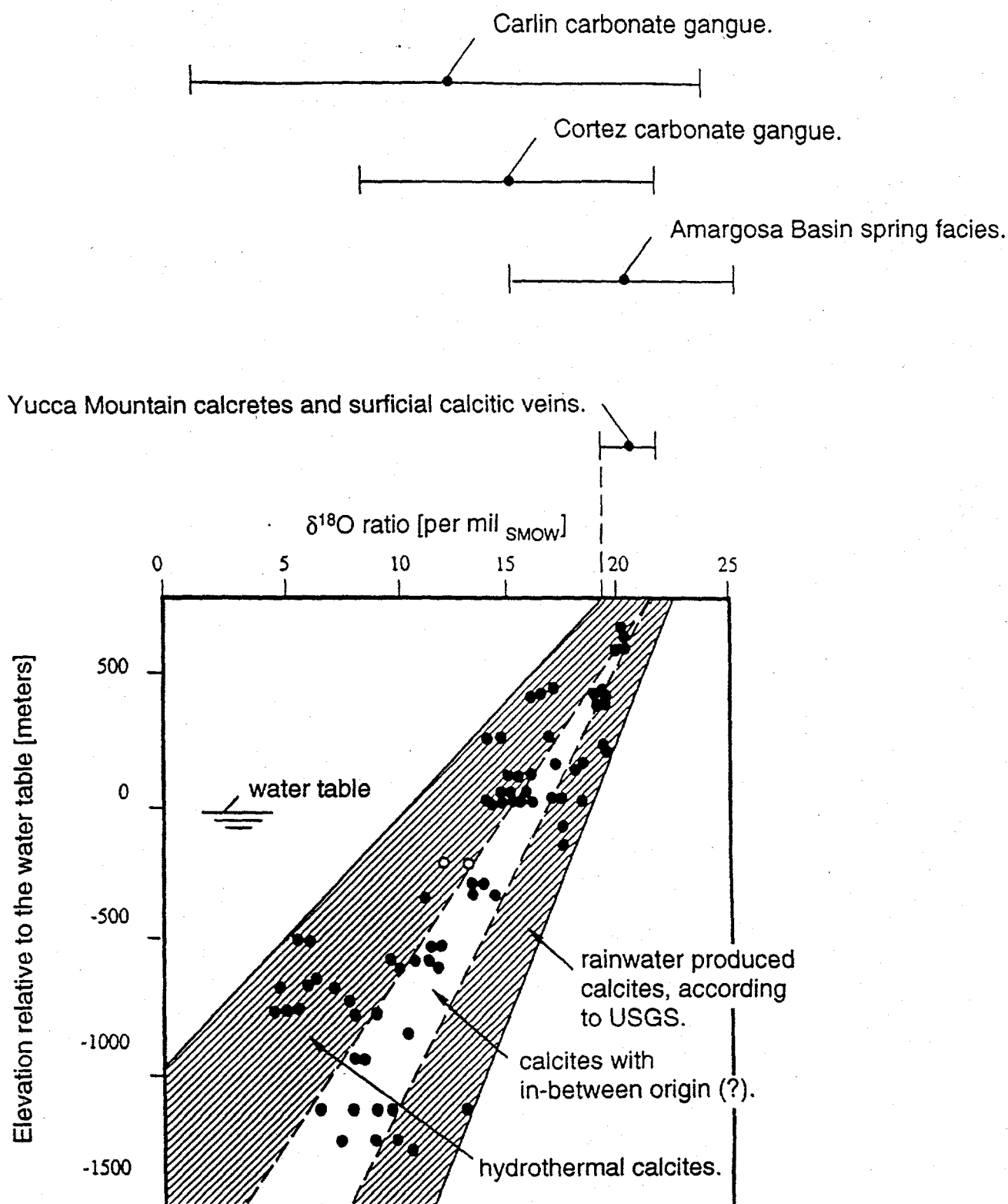


Figure 17. Comparison of oxygen isotopic ratios for thermal analogs (Rye, 1985; Hay et al., 1986), Yucca Mountain calcretes and surficial calcitic veins, and subsurface veins (Whelan and Stuckless, 1991; Broxton et al., 1986). From Szymanski (1992).

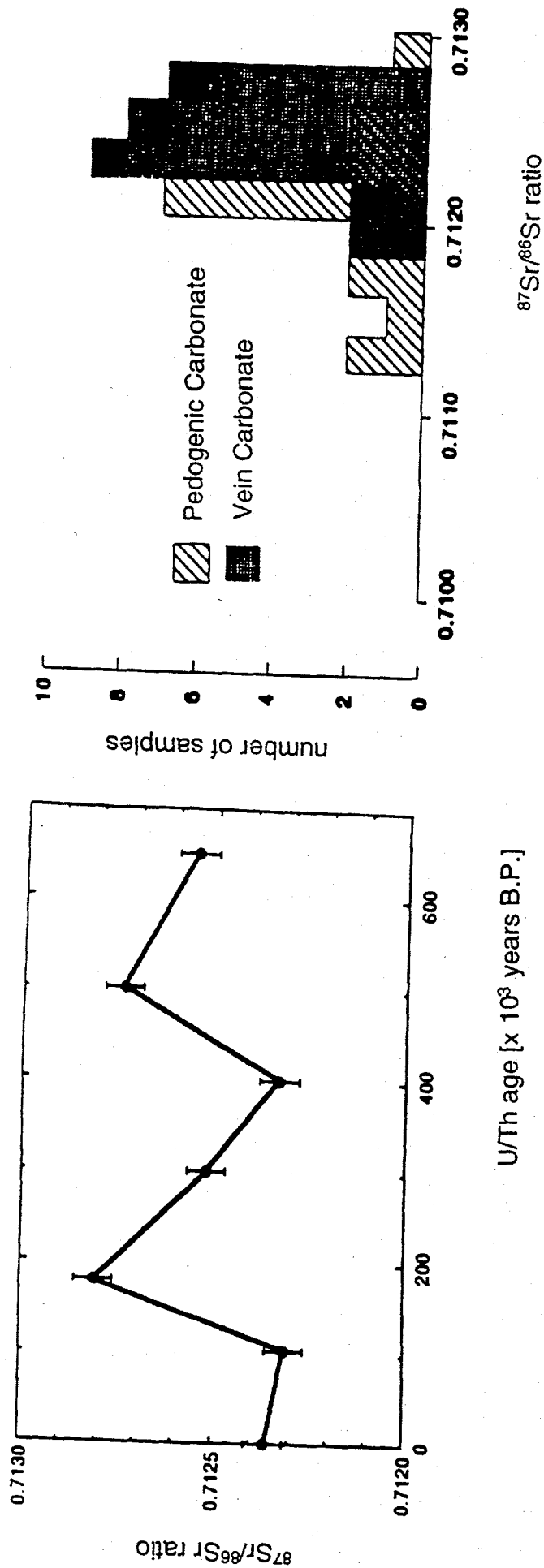


Figure 18. Comparison of strontium isotopic ratios for a nearby thermal analog (left, Devil's Hole; Marshall et al., 1990) and Yucca Mountain (right; Stuckless, 1990). Calcitic veins from Devil's Hole and Yucca Mountain are indistinguishable in terms of strontium isotopic ratio.

Location	Rock Type	$^{87}\text{Sr}/^{86}\text{Sr}$	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 separates
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				

Figure 19. 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.

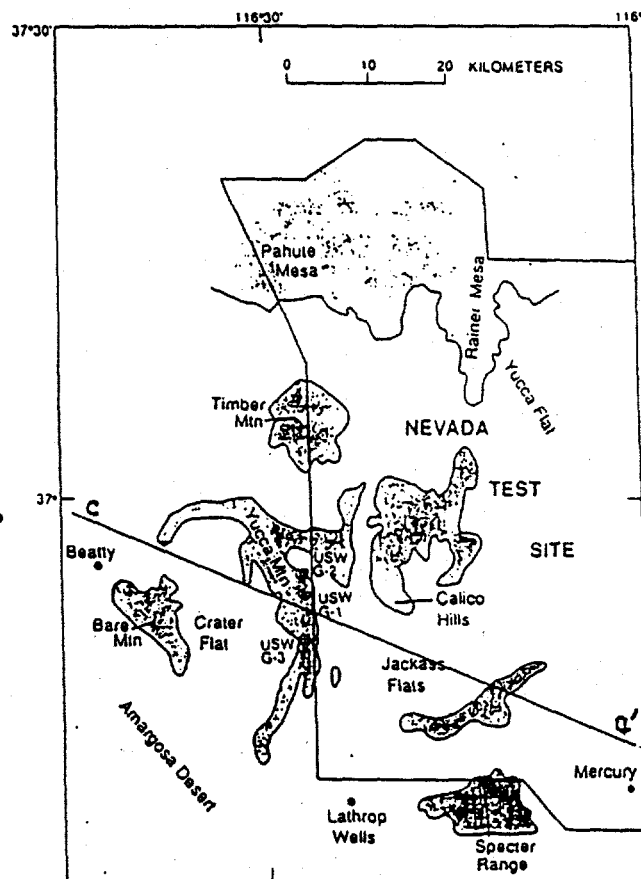
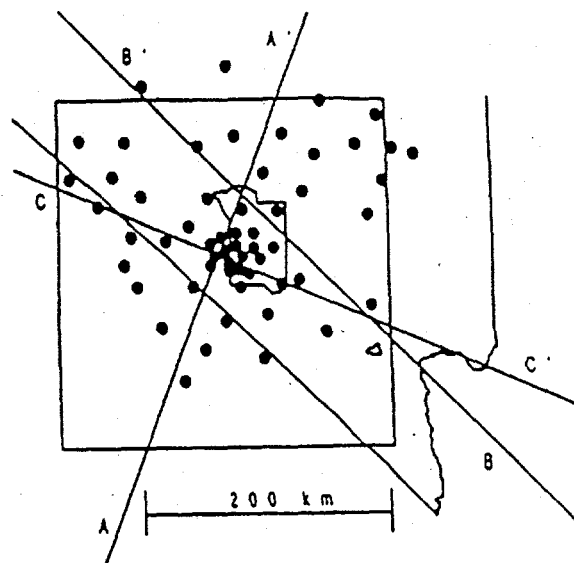


Figure 20. Maps of the seismic station distribution and principal topographic features in the Yucca Mountain and Nevada Test Site areas. Tomographic sections showing the structure at depth along the profiles AA', BB' and CC' were obtained by Evans and Smith, 1992.

Crust and Upper Mantle Compressional Velocity Variations Near Yucca Mountain

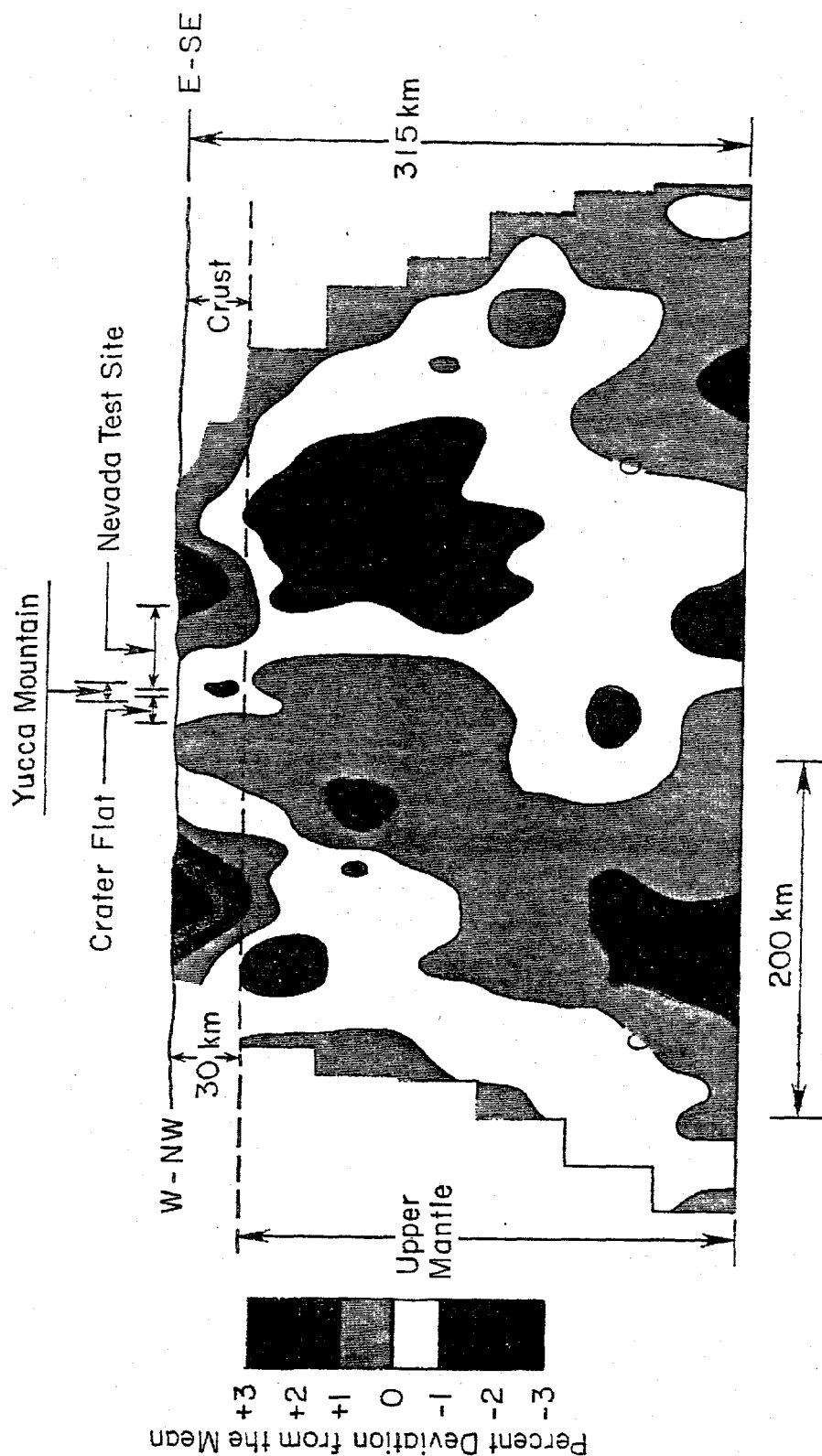


Figure 21. Tomographic depth section (profile CC) showing seismic velocity variations (in percent of deviation from the horizontally averaged mean) in the crust and upper mantle beneath and near Yucca Mountain. Details of velocity variations in the crust are not well resolved. The low velocity zone in the Upper Mantle most probably represents partial melting and a source of volcanism. Lower crustal zone heating and possible partial melting may be indicated by the Low Velocity Zone (LVZ) directly beneath Yucca Mountain. (From: Evans and Smith, USGS, 1992)

Crustal Compressional Velocity Variations at Yucca Mountain

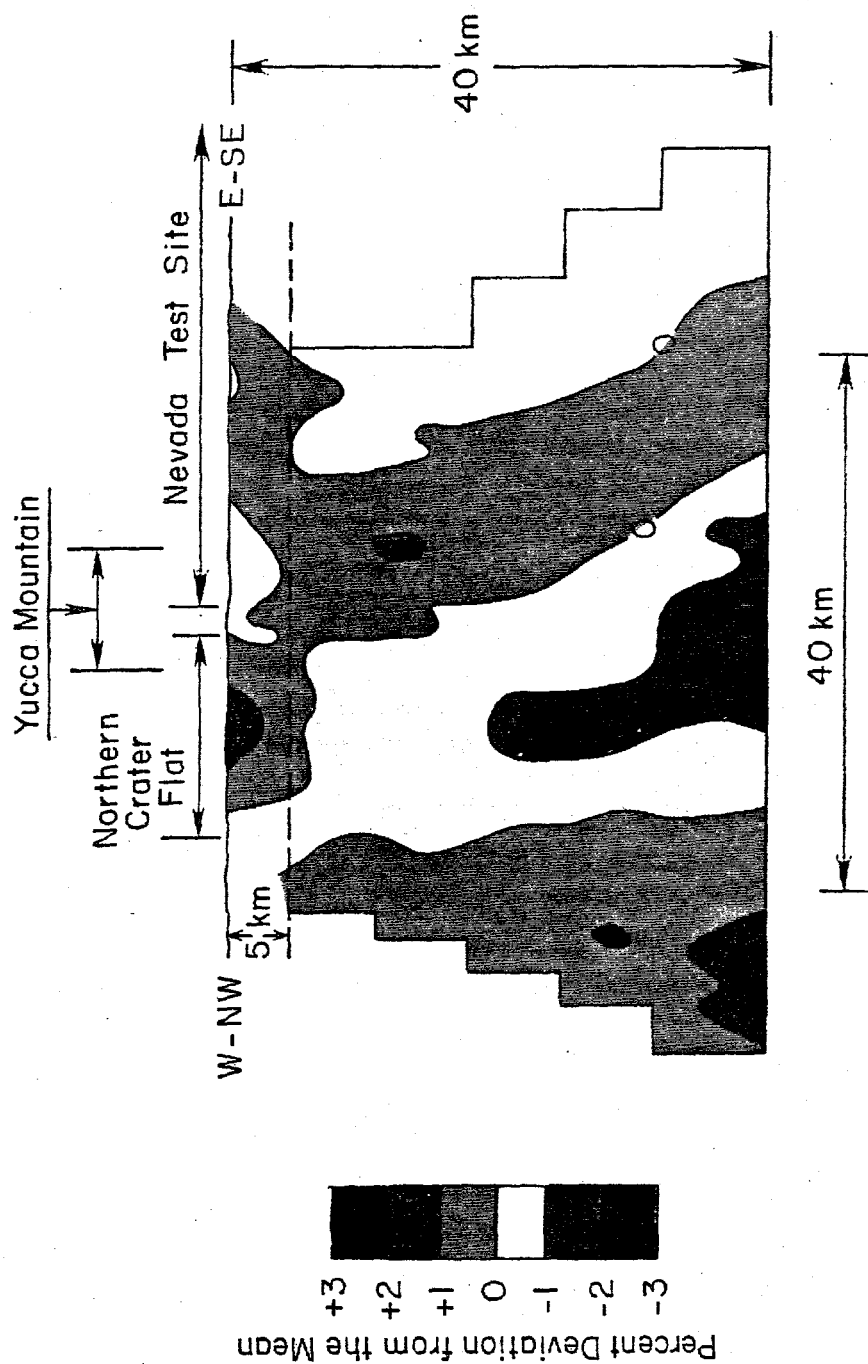


Figure 22. Tomographic depth section (profile CC) showing seismic velocity variations (in percent of deviation from the horizontally averaged mean) in the crust beneath and near Yucca Mountain. Velocity variations in the top 5 km are more uncertain than those at larger depths. The low velocity zone beneath Crater Flat and Yucca Mountain may represent crustal heating and a source of new volcanism. (From: Evans and Smith, USGS, 1992).

PART 5

Letter to C. B. Archambeau from F. Press

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 Press
Chairman

Enclosure

PART 6

**Report to F. Press (Memorandum--January 6, 1993)
from NRC Staff**

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

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 of 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 CO₂ 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.

PART 7

Letter of Reply to F. Press from C. B. Archambeau

Department of Physics
Joint Seismic Program Center

Campus Box 390
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(303) 492-5243

March 17, 1993

Dr. Frank Press
National Academy of Sciences
2101 Constitution Ave., N.W.
Washington, D.C. 20418

Dear Frank,

I am more than surprised by your reply to my letter of mid-November on the NRC Yucca Mountain report. Your appeal to Evernden's report instead of addressing the serious issues that I (and others) have raised regarding the NRC report is, I believe, evasive and inappropriate. After all, the issue is the NRC's report, not Evernden's report. His is just another in a long list of reports, some pro some con, on the issue. I doubt if any fair minded person would expect me to discuss the details of these reports in the context of a review of the NRC report. Again, the issue is the NRC report and its scientific validity and merit, not some other study, however it came into being.

Lest you think I wish to avoid addressing Evernden's report and its content, I am enclosing our review of his USGS report. I think you will find that there is much to question in his report and that the essential (assertive) arguments he presents are at variance with a wide variety of well documented evidence which, by the way, has largely been produced by the USGS.

Consistent with your letter, I find the report by your staff, which you attached, to be evasive of the specific scientific issues I raised in my review of the NRC report. Further, it is disturbing to find that your staff has seen fit to couch their commentary in terms of questions of scientific integrity. I think you know me well enough to understand that neither my interest nor my intent is to

question the panel members' integrity. However, I do certainly question the conclusions they reach and the scientific basis they give for these conclusions. In addition, I believe that there is a strong scientific bias exhibited in the panel's report which is manifested by their neglect of important data and their distortion of some of the data and results they chose to quote.

If your staff believes that this constitutes questioning the integrity of the panel, then so be it, but I think they fail to grasp what is involved here; namely a criticism of a narrow and scientifically biased report. As has been demonstrated many times in the past, ordinarily honest people, when in a controversy, have been selective in their use of information and have also pushed the interpretation of data to the (probablistic) limit in order to make their point. However such people are not usually engaged in what can be described as "fair and unbiased scientific inquiry". Clearly, the accepted standards are higher here than they are in ordinary debate, including those applied in a political debate.

In reading over the details of your staff's "analysis" of my report I find most of it to be a series of assertions that parrot the NRC's and Evernden's reports, combined with appeals to irrelevant issues that have nothing to do with any of the essential scientific arguments set forth in my review. Further, they report incidents and statements attributed to me and others that are inaccurate and distorted. In some cases their description of past statements and events are complete fiction. Lastly, their "analysis" is laced with a kind of foolish sarcasm that is inappropriate and certainly does not reflect well on their maturity as scientists, or as unbiased reviewers from the National Academy of Sciences. Nowhere do they directly address any of the specific scientific points raised in my review and I conclude that they are unwilling (and unable) to do so. Instead they have tried to attack the reviewer rather than defend the report.

To be specific in my reply to your staff's report, and the origins of the above conclusions, I will address their comments in order, as they enumerated them. (A copy of their commentary is attached for reference.)

- (1.) The staff report asserts that: "Archambeau's review of the National Research Council report (title given) 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". This assertion is incorrect and is suggestive of the superficiality of the NAS staff report. The longer report in question is Report No. 2, to the State of Nevada, Nuclear Waste Project Office dated August 1992, submitted by Technology and Resource Assessment Corporation, Boulder, Colorado. The report is entitled Critical Review of the National Research Council Report: "Groundwater at Yucca Mountain: How High Can it Rise?" by M.R. Somerville, J.S. Szymanski, G.A. Frazier, C.B. Archambeau, C.M. Schluter, D.E. Livingston. I have enclosed a copy of this report in case your staff again doubts its existence; or for your reference in case you should decide to further pursue an evaluation of the NRC report.

(2.) Your staff refers to the USGS report by Evernden and the fact that I did not refer to this report in my review of the NRC report. I have already expressed my opinion on this point. However, I would add that I did not ask Evernden to review the DOE Panel "Minority Report", by myself and Neville Price, but instead suggested that he might want to become involved in a broad evaluation of the Yucca Mountain site as a repository and in the question of up-welling water in particular. How he actually ended up doing some work on this problem is an interesting story, but suffice it to say that he made two short visits to the field, only one of which involved time at Yucca Mountain. On these occasions he was either alone or in the company of USGS and/or DOE personnel. I therefore take no credit (or responsibility) for the outcome of his work. His choice of title, involving an evaluation of our "Minority Report", is his alone and quite arbitrary, particularly when the content of his report is compared to ours. Evidently, however, the U.S. Geological Survey is willing to allow his report to be issued under their banner. Nevertheless, I find it difficult to believe that it could have been reviewed by USGS scientists in that it contains so much that is purely assertive and unsupported by, or is directly contradicted by, observational evidence. An example, discussed in our attached review, is Evernden's assertion that there has been no evidence for large scale seismic activity in

the area of Yucca Mountain during the Holocene and even the Pleistocene and therefore that the seismic hazard is negligible. As documented in our review and as is now widely known, this is certainly not correct. There is abundant and well documented evidence in numerous papers and reports published in the last five years, including the DOE Site Characterization Plan for Yucca Mountain (1988), that there has, in fact, been major offsets along fault scarps in the area during the Pleistocene and Holocene. The contrary assertion made by Evernden is the major cornerstone of his argument that upwelling of water cannot be expected to occur in the next 10,000 years. His conclusion, based on this asserted behavior, is therefore not valid. As a consequence of this denial of established results, among others noted in our review, we find it difficult to take his report seriously. It therefore seems to me that it would have been wise for your staff to have reviewed Evernden's report carefully before quoting it in defense of the NRC report. Indeed, in addition to major contradictions with existing data and results from many observational studies, his report is not even in agreement with essential parts of the NRC report.

(3.) Your staff complains that I am not a specialist in geological and geochemical studies and therefore cannot possibly understand the "complex scientific issues" involved in these disciplines. I would like to point out, however, that the issues I raise in my review involve common sense and simple recognitions of results misquoted, data ignored and lapses of elementary logic, rather than considerable specialized knowledge. None of these observations from my review are addressed by the staff commentary. Instead they raise this red herring to avoid confronting them.

As a matter of fact I have had training in geology, as you should know as my graduate advisor, and have worked closely with geologists and geochemists throughout my career. In particular, the DOE panel Minority Report was co-authored with Neville Price, a well known and highly respected geologist, and the enclosed longer review of the NRC report, which was used as a basis for my review, was co-authored by, among others, a very experienced field geologist and by three individuals trained in

geochemistry, including D.E. Livingston a former professor of geochemistry and past employee of the DOE who worked on the Yucca Mountain project in Nevada as a geochemist. Thus, while not a specialist in an area of geology or geochemistry, my associations and background certainly enable me to engage in a scientific evaluation of this problem, particularly when the physical and geological processes involved in this debate are so strongly related to tectonic processes and geophysics generally. In any case, I think it's obvious that my review draws on expertise from a number of people, including myself, that have, taken together, formal training and considerable experience in all the areas covered in the review, so that your staff's criticisms are unfounded in addition to being evasive of the specific issues raised in the review.

The example cited in your staff's commentary which is supposed to illustrate my lack of "formal training or experience in these areas of earth science" involves the wide range of zircon ages in the cemented breccia in Trench 14 and Busted Butte. The staff commentary states that, to most geologists, this wide range of ages would indicate multiple source rocks of varying ages for the zircons. The report then goes on to elaborate on this statement by disputing the idea (attributed to me) that this wide range of zircon ages is due to heating of some of the zircons (thereby resetting their ages based on fission tracks) by high temperature water from great depth below the sample sites. The concept being promoted by the commentary is that the observed zircon age range is due to transport of zircons into the sample area from other (distant) rocks by some (unspecified) mechanism and that the zircon ages reflect the ages of these rocks not the time of the last heating occasioned by hot water influx.

It appears, however, that the author(s) of these statements have forgotten that the NRC panel used the hydrothermal heating argument for the zircons to justify their conclusion that no hydrothermal activity had occurred more recently than 10 to 13 million years ago. Therefore, the NAS staff report assertion that most would interpret the observed age range differently apparently does not apply to the 17 members of the NRC panel. Or the contrary, it would appear that most scientists (all in this case) have

interpreted the zircon fission track age data as age markers for hydrothermal heating.

The contested issue actually involved in my discussion of the zircons is also apparently misunderstood by the authors of the staff commentary. In particular, the objection I raised in the review of the NRC report is that they misrepresented the age data of the zircons from the samples at the two sites in question. In this regard I pointed out (on p.1 of my review) that the ages of the zircon crystals were stated by the authors of the study to be spread into groups "significantly younger and significantly older than the age of the tuff"; which is 13 million years old. I then went on to note that the authors used the term significant in a technical sense, in that it meant a probability of over 90% that the ages were different than that of the 13 million year old tuff. The NRC panel had, however, represented the results of this study by stating that "within the analytical uncertainty, most ages are about 10-12 Ma, or about the same as those of the dominant volcanic rocks in the region", which misrepresents the results of the study.

In the course of my discussion of the specific age data given by the authors of the zircon study I note (on p. 4 of the review) that the youngest age is 4.8 Ma and that, based on the statistical evidence given by the authors, "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." This statement was made to illustrate why the authors had stated that there were *significantly younger* zircons present in the sample. However the staff commentary refers to this statement in the following terms: "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)." This is clearly not what was said, the high confidence phrase I use obviously refers to the confidence in the age not to confidence in heating and the context of my statement is completely different than that implied by the NAS staff statement.

Worse yet the staff commentary then goes on to talk about *isotopic ratios* in the samples and that it takes high temperatures to reset them.

Apparently your staff does not understand that the zircon ages are derived from fission track counts, not isotopic ratios. Further, these ages, based as they are on fission track counts, can be changed by annealing ("healing of the tracks") at high temperature, but isotopic ratios are not changed simply by heating. Therefore, your staff seems to be laboring under several misconceptions that suggests a totally confused misunderstanding of the science involved here; namely they imply by their statements that the dates given for the zircons are obtained from isotopic ratios and that the ratios can be changed at higher temperatures. Both of these notions are false and one can only wonder how it is that these individuals are in a position to comment on either my qualifications in this area or on the subject matter under discussion.

Nevertheless, the staff commentary goes on to even more fully demonstrate their superficial and confused thinking. In particular, they state: "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 isotopic ratios (the "clock") require great depth and could not be attained at such shallow depths as Trench 14 and Busted Butte". In addition to the basic misconceptions that isotopic ratios are used to obtain the ages and can be reset at high temperatures, these comments display a remarkable rigidity of thought and narrowness of perception.

That is, if we were to ignore the improper references to isotopic ratios and replace them by references to fission track annealing at high temperatures, then these statements might be applied to the zircons. Doing this however does not change the fact that these statements do not reveal what the authors think the "process required" is, nor do the statements indicate a realization that the episodic water flows that are likely would move through fracture systems that are localized and narrow, so that only the rock immediately surrounding the fractures would be subjected to major (transient) heating. In this case, for episodic flows over a long period and with deposition of calcite and silica filling some fractures and new ones opening under continuing deformation, it could easily happen that even a rather small sample

volume, especially one near the surface where fracture densities are high, would show different zircon annealing ages reflecting the different epochs of water intrusion along narrow fractures. Thus, each intrusion of water would normally have its own narrow pathway through the sample so that many annealing ages could be recorded, each to first order, unaffected by the others.

On the other hand, it would also be expected that upwelling water moving rapidly through fractures could carry zircons from depth to the surface and that this is how they get there in the first place. Obviously transport of the relatively heavy zircons by upwelling water is, at the least, plausible. Furthermore, in the process the hot water at depth could cause the reset of ages by annealing which occurs in zircons at about 220°C. None of this seems to have occurred to the author(s) of the staff report, but perhaps it is not so surprising that they lack insight given their basic misunderstanding of the nature and meaning of the age data under discussion.

In a final statement on this subject the staff commentary reverts back to an insistence that the data might still be interpreted as being in the age range from 10 to 13 Ma. This after I had quoted the contrary interpretation of the ages by the authors of the original article in my review and even after having explained the meaning of confidence intervals for such observational data. In particular, their statement is: "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". As authors of the original article and I pointed out, the overwhelmingly most probable ages of one age group is *much* younger than the 13 Ma. age of the silicic volcanism in question. For your staff to continue to argue for what is, at best, a highly improbable interpretation makes a farce of scientific discourse and, in my view, is only designed to try to escape responsibility for the NRC's earlier distortion of the study results in their report. Furthermore, after seemingly having admitted that there is a "a wide range of zircon ages in the carbonate cements in Trench 14 and Busted Butte", they now say the opposite. One wonders what they do in fact believe or know, if anything.

(4.) The staff commentary alleges that I have ignored direct geologic evidence in my assessment of the situation at Yucca Mountain. My review focused on the NRC report and some of the most obvious and flagrant errors in that report. I therefore heavily constrained my discussion and did not cover some subject areas. The enclosed longer review covers more of them in greater detail and in it we address geologic field relationships. I expect, however, that the staff commentary is indirectly quoting from Evernden's report and is actually referring to the DOE panel report I wrote with Neville Price. In this report we cover the geologic evidence in great detail and address each of the relationships and observations mentioned in the panel commentary, along with many others. We do not find the "evidence" quoted in the commentary as unequivocal to say the least. In particular the assertion that the geologic "evidence" cited "unequivocally demonstrates the surface origin of the water and materials that produced the veins and surface parallel carbonates in question", as is stated by Evernden and parroted in the commentary (top, p.3), is unfounded and unsupportable under any rational test of uniqueness as to a causal mechanism.

There simply is no single line of geologic evidence that, by itself, can be used to *unequivocally* demonstrate the origin of the opal-calcite veins and surface calcretes. This has been recognized by eminent geologists with far greater experience at the site (and elsewhere) than Evernden and the NAS staff. To make such a statement on the basis of such a narrow range of observational data is simply ridiculous.

In this regard, consider that none of the "evidence" cited includes the observational data recovered from drill holes at Yucca Mountain that show young calcites and low grade metamorphic alteration of the rocks throughout the mountain, both above and below the water table, that could not be produced by downward moving surface water (*ie.* rain water) nor, like the zircons, could they be a result of the silicic volcanism that took place 13 million years ago because of their measured young ages. To ignore all this information, along with much more, only allows you to make a (weak) case based on fragments of geologic data that might be interpreted in your favored way.

Beyond selectively, consider also that none of the observational data cited need be interpreted in a manner resulting in the "evidence" given in the NAS staff commentary. For example, the mineralogic composition of the veins at the surface should contain fragments of local soil material, whether the water moved up from depth or percolated down from rainfall. Of course, we have contended that both occur, but in any case the presence of such material in the surface exposure of a vein is not, in itself, definitive or unusual. However, it is noteworthy that uncemented volcanic ash is present, which seems an unlikely occurrence if the veins are formed by deposition of calcite from constantly evaporating rain water that should have cemented the exposed ash soon after its air borne deposition. Likewise, the small grain size of the carbonates means rapid crystallization and if (CO₂) degassing of the upwelling water occurred, as would be expected, the result would be small grain size. In fact, gradation of grain size to fine grained near the surface is observed in a known hydrothermal vein exposure in Death Valley, which is less than 40 miles from Yucca Mountain, and is probably due to degassing. Thus, small grain size does not automatically mean that the minerals were deposited by evaporation of rain water.

Other "geologic evidence" that is asserted to occur (veins thinning downward with depth below the surface and becoming discontinuous and calcretes as thick above (upslope) Trench 14 as below it) are similarly improperly characterized as evidence for surface water deposition of the opal-calcite deposits. The actual observations are that the calcretes are *thinner* above this cut in the hillside than downslope from the cut and that the veins in Trench 14 do *not* disappear into thin stringers in 15 feet from the surface. In fact, the principal vein is best exposed on the North wall of the trench and it continues below the floor of the trench where it is about 0.5 meters thick. In this consideration of observations of veins and calcretes at Trench 14, it seems not to have occurred to the NAS staff (nor to Evernden) that the veins are within the Bow Ridge fault zone which trends northerly. Consequently, if upwelling of water along faults were to occur one could expect calcretes intermittently along this fault in areas both upslope and downslope from Trench 14. Thus, the presence of calcretes

upslope as well as downslope from the veins exposed in Trench 14 is not evidence against upwelling nor is it evidence for rain deposited calcites.

(5.) Your staff seems to take the view that it's not necessary to forthrightly respond to comment or criticism if you can think of some series of outrageous comments, assertions and distortions that might be distracting to the reviewer and confusing to everyone else.

First of all, in reference to my lack of comment in my review on the NRC panels' "views on earthquake models;" I made no comment because the NRC panel does not discuss earthquake models. They do refer to modeling of water flows due to earthquake induced strain changes (by others) and do make some primitive estimates of water level changes that they suggest could/might be expected from earthquake strain changes, but then add that all these estimates are crude. I might have commented on what they did (or didn't do) in this area, but thought that the worst of their conclusions should receive priority in a short review. In my view, however, what work was done on "hydro-tectonic modeling" by the NRC panel was pathetically superficial and contributed little, one way or the other. In short, their quantitative work was virtually nonexistent and there was little to evaluate, or worth evaluating.

The staff commentary goes on to make a whole series of outrageously inaccurate statements about what occurred at the May 31, 1991 joint meeting between the DOE External Review Panel and the NRC Panel. These NAS staff statements, in fact, seem purposely designed to mislead readers of their report and, as well, designed to discredit me, the reviewer of the NRC report.

Normally I would ignore such nonsense, but given that it comes from your office and that it has been distributed by your staff to at least several reporters (three or four of whom then called me), I feel it is necessary to respond.

First of all, the commentary says that in my review I accused the NRC Panel of misrepresenting my position in the meeting with the NRC panel.

They also state that I was "relying on his recollection of the discussion reported in his notes". First of all it is *not* true that I relied on my recollection of the discussion from my notes. As I state (p. 23) in my report, my objections are based on my written summary submitted to the NRC Panel at the meeting, which I adhered to in my presentation. It is true that I disputed a NRC Panel statement in their report as misrepresentative, but my objection had to do with *when* information concerning Szymanski's model for cyclic invasion of the Mountain by water was available to them and whether they had an opportunity to evaluate this model. In this regard the NRC panel asserted (see exact quote on p. 22 of my review) that they had first heard of the hydrothermal aspect of this model at the May meeting and because of time constraints did not have time evaluate it and that, besides, they had already made up their minds. They also said (incorrectly) that "Szymanski no longer believed that 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".

In my review of the NRC report I point out, on p. 23, that: "If the Panel had actually read Szymanski's report (of 1989) they would have found that this latter concept is discussed in considerable detail and was thought to be the principle mechanism for deposition of calcite throughout the mountain". Therefore it was obvious that the Panel had either not read the very report they were charged to evaluate or had misrepresented the true situation in order to avoid dealing with a difficult problem. Accordingly I objected to all of this in my review.

In our presentation, both written and oral, we simply described what Szymanski's model entailed, including the thermally driven aspect of it, and stated that both seismic pumping and thermally driven flows would contribute to calcite and opal deposition. In our opinion, and in the opinion of Szymanski, we stated that there was agreement that the longer lasting thermally driven flows would be expected to deposit most of the volume of minerals seen at Yucca Mountain.

By some perversion of all of the above, your staff seems to be trying to make the case that there was some change in the representation of the model and that there was some change in our "publicly stated positions" involving the importance of seismic pumping versus thermally driven convection. First of all there was no change in Szymanski's position as can simply be verified by reading his 1989 report. In our case we made no public statement on this particular issue prior to the meeting and in any case have always held that thermally driven convection could be initiated after a tectonic event (seismic or volcanic) and that it could be long lasting and produce upwardly moving water flows through fractures that could deposit large amounts of calcite and other minerals. (See our DOE Panel Minority Report for elaboration.) It would appear from all of this that your staff cannot get the questions right, let alone the answers.

The staff commentary also asserts (in 5a.) that we voluntarily announced that: "Szymanski no longer believed the Trench 14 and Busted Butte veins were formed by the rise of ground water along fractures". This statement is false, as are the statements made following it. What was said was that the surface *calcretes* upslope from Trench 14 could be the result of upwelling along fractures, upslope from the Trench 14 veins located within the Bow Ridge fault zone area, where Trench 14 is also located. Since this fault zone area is large and active, the presence of fracture conduits in the past, and future, is certainly plausible, if not to be expected.

Lastly, in paragraph 5b. of their commentary the NAS staff produces a totally garbled and outrageously distorted rendition of what I said concerning a New York Times quote attributed to me. The quote in question (only partly reproduced in the commentary) is: "You flood that thing and you could blow the top off the mountain". When asked if that was what I said, I said that I couldn't remember my exact words since I had spent several hours at different times talking to the reporter who wrote the story and said a great many things, including much that was background, but that if he quoted me, then most likely it was accurate since I knew him to be careful in such matters.

I then went on to explain the origins of my concern with water intrusion into the repository in order to make clear why I had made a statement like that in the first place. In particular, I explained that in a "hot repository" of the type planned the cannisters would reach temperatures of considerably more than two hundred degrees centigrade (or higher, depending on details of design) and the surrounding rock between cannisters and throughout the repository would be at average temperatures well above 100°C. (This condition is part of the documented design of the repository, and would reach this state relatively rapidly, on the order of a hundred years, and stay near these temperature for hundreds to thousands of years.) If ground water were to begin flooding the repository, due to seismic pumping or convection stimulated by volcanism or a local seismic event, then it would release CO₂ as it moved upward and it would also turn to steam as it came in contact with the hot rock and the cannisters. Consequently the repository volume would quickly contain a mix of superheated water, steam and released CO₂ that would certainly vent into the atmosphere through fractures above the repository and this could, in fact, be quite forceful.

In addition, I noted that with superheated water in contact with the cannisters one could expect highly accelerated corrosion of the container surfaces due to activated chemical process and also due to the production vapor bubbles at the surface which would move upward, away from the cannister, but quickly collapse as they cooled in the surrounding mass of water sending a jet of high velocity water back towards the cannister. Such jet action would cause mechanical damage at the cannister surface, flaking off bits of metal or cement and producing pits in the surface so that a larger surface area would be in contact with the water. I explained that this phenomena had been studied in laboratory experiments and could turn into a "run-away" process, due to the rapidly increasing exposed surface area producing more and more vapor bubbles and that in some cases could result in steam explosions. I noted that this explosive process didn't seem likely for the repository, but that the general mechanical process described would most assuredly result in very rapid corrosion and relatively rapid release of radionuclides from the cannisters. I was asked about the rate of corrosion

and release and it was at this point that I said that I was certainly not an expert in the matter of corrosion rates or on these processes, but guessed that they could produce cannister breakdown in a matter of weeks or even days. (A more complete exposition of the consequences of water flooding, including that just described, is contained in the DOE Review Panel Minority Report.)

To characterize all of this in the manner attempted by your staff is truly an outrage and it cannot but harm everyone involved.

(6.) This final item in the NAS staff commentary is a complaint that Szymanski's reports (and mine) have not been peer reviewed prior to submission to DOE, the state of Nevada or the media. (No reports are, of course, submitted to the media.) Contrary to what your staff says, Evernden's so called "review" of my "Minority Report" is no exception, since he did not even see my report prior to submission to DOE. (As I noted earlier, Evernden's report can hardly be called a review in any case.) Further, it is not true that Szymanski's reports were not reviewed before submission at DOE, his 1989 report was thoroughly reviewed by DOE scientists, just as USGS reports are normally reviewed by USGS scientists. [Since the preponderance of the material on Yucca Mountain is in the form of DOE, USGS (and other DOE contractors), and State of Nevada reports, it hardly seems justified for your staff to accept some (such as Evernden's) and not others based on review standards that are, in fact, highly variable at best.] In any case, we will submit material for publication to science journals in the future, when we're ready to do so.

I hope that I have conveyed the level of my dissatisfaction with your staff's commentary without being offensive in the process. However, as is apparent I expect, I think the issue deserves better treatment than that given to it by the NRC panel and certainly better than that contained in your staff's commentary. I can only hope that there might be improvement, from some source, in the future.

Sincerely Yours,

A handwritten signature in cursive script, reading "Charles Archambeau". The signature is fluid and elegant, with a large initial 'C' and a long, sweeping tail.

Charles Archambeau
Department of Physics
Theoretical and Applied Geophysics Group
University of Colorado
Boulder, CO. 80309

PART 8

**Safety of Proposed Yucca Mountain Nuclear Repository as
Regards Geological and Geophysical Factors: Evaluation
of Minority Report by Archambeau and Price.**

U. S. DEPARTMENT OF INTERIOR

U.S. GEOLOGICAL SURVEY

Safety of Proposed Yucca Mountain Nuclear Repository as Regards

Geological and Geophysical Factors:

Evaluation of Minority Report by Archambeau and Price

by

Jack F. Evernden¹

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Safety of Proposed Yucca Mountain Nuclear Repository as Regards

Geological and Geophysical Factors:

Evaluation of Minority Report by Archambeau and Price

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Introduction

The Department of Energy, via its Las Vegas Office and the Yucca Mountain project, organized a five-man panel to evaluate G. Szymanski's assertions relative to the safety as regards geological factors of the proposed nuclear waste repository at Yucca Mountain. Two reports resulted from the work of the panel, a three-man Majority Report and two-man Minority Report, the latter report being authored by Charles B. Archambeau and Deville Price ('An Assessment of J. S. Szymanski's Conceptual Hydro-Tectonic Model and its Relevance to Hydro-logic and Geologic Processes at the Proposed Yucca Mountain Nuclear Waste Repository', Minority Report of the Special DOE Review Panel, undated, but submitted to DOE in late 1991).

Charles Archambeau requested that I evaluate the credibility of the Minority report. The following document is my response to that request.

My mode of response has been to take their fundamental conclusions and/or model parameters and to evaluate their credibility against available data. I began by thinking I would make a detailed critique of their report. I soon realized that to be a hopeless approach. I concluded that the only approach for me was to make my own evaluation of the problem of the Yucca Mountain repository based upon available data and several field excursions, and to then compare my conclusions with those of the Minority Report. I have not addressed their argumentation in detail as I concluded that the entire report was so misguided as to not warrant such an approach.

I must state at the start that I did indeed feel it to be presumptuous and possibly beyond my competence to attempt to evaluate in a period of a very few weeks the supposedly complicated and certainly many dimensional work of numerous competent people over years of devoted scientific effort. I have found, to my surprise, that (a) field relationships of various types of carbonate-silica deposits at and in the regions of southern Nevada surrounding Yucca Mountain are so clear as to leave no room for doubt as to the mode of origin of the Trench 14 deposits (i. e., there was no need whatever for isotopic data to establish the nature and mode of genesis of the Trench 14 carbonates), and (b) the borehole data of all types (isotopic, stress, chemical, water productivity and permeability) lead to a simple model totally consistent with the field data of (a). It may be presumptuous of me to write this report but I no longer feel it to be beyond my competence as I have concluded the evaluation of all available data to be so straightforward as to be easily perceived by one with my composite of geological and geophysical expertise.

The term "Yucca Mountain", though used on all USGS topographic maps, potentially carries an unwarranted connotation of scale and magnitude for this feature. The maximum elevation on Yucca Mountain is 4950', about 1500' above flanking Flats. Much of the crest of Yucca Mountain is less than 1000' above flanking terrain. This is a trivial topographic feature relative to nearby mountain ranges.

The assertion within the Minority Report that it alone provides a conceptual model for geologic processes at Yucca Mountain is false. It apparently considers that a 'model' simply derived from the vast amount of available data is not a 'model'. I assert that (a) I herein provide a 'model' that explains the geologic relationships at Yucca Mountain and (b) the 'model' of the Minority Report is no model at all but only a collection of unsupported and unsupportable hypotheses.

I found it convenient to implement my thought processes and overall evaluation via an outline format, and so this report has a somewhat unusual structure. I believe the reader will find it convenient, and so I have not changed it.

A few comments about the content of this report. The report is over 60 pages in length when single-spaced but, even at this length, it is an abstract of the relevant data and a useful elaborative discussion. All discussions are carried only to the points necessary, in my opinion, to establish firm bases for interpretation. Other authors would have contracted where I expanded and expanded where I contracted. I hope my selection of emphasis will not confuse any readers.

In addition, I should remind the reader that one role of this document is to evaluate the plethora of arguments contained in the Minority Report. In the arena where this report may enter, it is not adequate to demonstrate "the truth" if counter arguments of whatever quality exist which are not effectively addressed and evaluated. Thus, after a long series of arguments which to me seem irrefutable, I still felt it necessary to evaluate the several deposits named by the Minority Report to be of hydrothermal origin.

I do not feel bound by previously published interpretations of any aspect of this report. As is my way, I evaluate everything within my own capabilities and let the interpretation be what it may be. I found no bases for disagreement with most published analysis and conclusions. However, in a critical few cases, I did disagree and I present my arguments for so doing. Of course, I am not including the Minority Report as a "published interpretation". As you shall see, I disagree with nearly everything in that report.

I have attempted to give a reference for all data presented. I have not searched the literature to ascertain what all have said and published on the topics discussed. In fact, I have striven to reach my own conclusions independently of any previously published. So, citations of opinions expressed by others may well be deficient. In addition, I may have missed some important data. However, the several bodies of data here cited and used do tell such a compelling and mutually supporting story that I have no doubts about the validity of the general conclusions.

Finally, I thankfully acknowledge the great amount of assistance given me by several people, most particularly Emily Taylor, Zell Peterman, Isaac Winograd, Richard Pengler, Dwight Schmidt and James Paces. Most of these people permitted inclusion within this report of as yet unpublished data, data vital to some of the arguments developed.

Index

Spring-deposited calcite versus soil carbonates - Morphologic Contrasts	page 5
1. Spring deposits visited	page 5
2. Pedogenic calcite-opal deposits visited	page 7
3. Per ascensum versus per descensum source for soil carbonates	page 10
4. Conclusion	page 14
 Trench 14 Deposits	page 14
1. Field data and ages	page 14
2. Rates of accumulation of atmosphere-derived soil carbonate at Trench 14	page 15
3. New data from deepening of Trench 14 to 22 feet	page 17
 "Veins" at Trench 14 and their mode of origin	page 17
 Conclusions relative to mode of origin of Trench 14 deposits based on data discussed in A through C	page 18
 Bore-hole stress measurements in Yucca Mountain	page 19
 History of faulting in area and potentiality for significant faulting under or within a few tens of miles of Yucca Mountain	page 20
1. Muddy River deposits, Mormon Mesa and environs	page 20
2. Kyle and Lee Canyons, Spring Mountains	page 21
3. Plio-Pleistocene and Holocene Faulting in SW Nevada	page 21
4. Faulting at Yucca Mountain	page 21
5. Significance of the Little Skull Mountain earthquake of June 29, 1992 to the analysis given above	page 23
6. Furnace Creek Fault and shaking at Yucca Mountain	page 26
7. Potential faulting at Yucca Mountain in my perception	page 27
 Data from boreholes on and near Yucca Mountain - Part 1	page 27
1. Diagenetic changes of volcanic rocks vs. depth	page 27
2. Static head and flow-rates in boreholes as a function of depth and stratigraphy	page 29
3. Distribution of vein calcite as a function of depth	page 30
4. Interpretation/conclusions of all above	page 30
5. Potential analogs for Trench 14 "vein" deposits	page 31
6. Biologic-derived content of Trench 14 carbonates versus that of the Ash Meadows and Crater Flat spring deposits	page 32
 Data from boreholes on and near Yucca Mountain - Part 2	page 34
1. Strontium isotopic ratios as a function of depth in vein calcite, wall-rock and water under Yucca Mountain	page 34
2. Carbon and Oxygen isotopic ratios	page 35
3. Uranium-Thorium systematics	page 40
4. Lead	page 42
5. Ca^{++} , Na^+ , HCO_3^- , $P(CO_2)$, SiO_2 , etc.	page 42
 Evidence against Szymanski's convective model	page 47
1. Springs at Ash Meadows	page 47
2. Springs in the Spring Mountains	page 48
3. Lack of spring deposits on faulted west face of Yucca Mountain	page 48
4. Temperature gradient under Yucca Mountain	page 48
5. Character of Trench 14 deposits	page 48
6. Chemistry of aquifer waters under Yucca Mountain	page 48

7. "Pre-Cambrian aquifer"	page 49
8. Spring deposits at Crater Flat	page 49
Discussion of sites deemed by the Minority Report to be indicative of hot hydrothermal solutions	page 50
1. Harper's Valley	page 50
2. WT-7	page 53
3. Wailing Wall Fault	page 54
4. Sites 106, 106F, Red Cliff Gulch	page 58
5. Conclusions	page 63
Remaining problems	page 63
1. Source of carbonate dust	page 63
2. High water table in Yucca Mountain north of proposed repository	page 65
Overall Conclusions	page 65
Appendix - General criteria for distinguishing non-pedogenic from pedogenic calcium carbonate and opaline silica	page 66
of Tables and Tables	page 70
of Figures	page 75
References	page 95

Report

.. Spring-deposited limestone versus calcite-rich deposits - Morphologic Contrasts

1. Spring deposits visited

a) Furnace Creek entrance to Death Valley - immediately north of welcome sign to DV National Monument

- 1) Multiple veins in coarse Tertiary conglomerate, coarsely sparry calcite at base, finer grained at top, emplaced in an alluvial fan sequence (Funeral Formation of Pliocene age).
- 2) Veins connect with a small (!) area of surficial tufa. Tufa has microsparite texture (you can see calcite crystal and cleavage faces throughout a fresh broken surface of the dense tufa). Numerous vuggy calcite-lined cavities occur within the tufa.
- 3) Both veins and tufa are essentially pure calcite, i. e., no detrital component in either. Though veins are banded, there is crystal continuity throughout most of the vein, i. e., slight changes in solution but continuing deposition in crystallographic continuity with previous deposition (crystal growth perpendicular to vein boundary). The vein pattern is nearly identical on both walls.

b) Devil's Hole. I sampled spring-deposited vein as well as pedogenically formed calcite on surrounding outcropping Paleozoic limestones at ground surface.

- 1) Spring-deposited calcite as in a) above.
- 2) Pedogenically-deposited carbonate coats sides and bottoms of fractured Paleozoic limestone with micritic dirty "stalactitic" carbonate. The surfaces of the Paleozoic limestone are pitted and etched by soil solutions, indicating solution of carbonate in the surface or near-surface environment. No such phenomenon is associated with the spring deposits of 1).
- 3) The two types of deposits in 1) and 2) above are totally distinct.

c) Spring mound (tufa) deposits on west side of California Wash.

- 1) Reached via gravel road going west from Ute exit (Exit 80) on I-15 to base of mountains on west side of valley.
- 2) Two mounds about 50' high, 150' wide, 600' long, rise above the surrounding calcrete surface.
- 3) Note that these are local "point mass" accumulations in contrast to the soil-related calcretes which cover many square miles and are usually much thinner (a very few feet).
- 4) Mounds are essentially pure carbonate. No evident detrital component, no bands of opal identified during my short perusal.
- 5) Layered, each layer basically massive with typical tufa structures within it. Large lined vugs, some never filled. Vertical columnar structure. Roots and root casts.
- 6) On a second visit, numerous travertine veins, up to a foot or so in width and apparently feeders for the main mass, were seen on the SE slope of the southern of the two mounds. Other such veins may well exist on both mounds.
- 7) No platy layering as in soil carbonate deposits.
- 8) D. L. Schmidt says it is obvious by study of nearby mounds of the same age that these mounds are part of the Muddy Creek sediments (> 5 Ma), have been exhumed by later erosion (probably about 4 Ma ago), and are surrounded by a later soil calcrete. For later reference, note that the history

and present configuration of these mounds attest to their great resistance to erosion.

- 9) The mounds can be compared with local soil calcrete by going eastward along the narrow road that follows the line of wooden power poles about half a mile, where the road drops down across the outcropping eroded edge of the calcrete.
 - 10) D. L. Schmidt reports (p. c.) that the isotopic $\delta^{13}\text{C}$ versus $\delta^{18}\text{O}$ composition of the calcite of the tufa mounds is markedly different from that of the carbonate in calcretes of California Wash, the composition of the latter falling within the calcrete population of other authors (Benson and Klieforth, 1989; Quade and Cerling, 1990). Thus, here we have in juxtaposition carbonates resulting from issuance at the surface of the ground of waters from a Paleozoic aquifer and carbonates resulting from surficial soil processes. The isotopic differences of these deposits is as predicted and expected (see discussion of isotopic measurements in Yucca Mountain).
 - 11) These mounds, with an age of > 5 Ma, occur along an old fracture. However, there is no discernible evidence of motion on this fracture, either associated with the mound formation or since that time (D. L. Schmidt, p. c.).
- d) Deposits at Ash Meadows on floor of Amargosa Valley resulting from flowing line of springs. This is a palustrine deposit, consisting of much uniform fine-grained silt and fine sand within the carbonate precipitating from the out-welling spring-line. Carbonate-cemented eolian silt and fine sand is a major element of the deposits at Ash Meadows.
- e) Along US 95 east of highway 18 miles south of Beatty
- 1) Easily seen to east of highway
 - 2) Like f) 2) below. Radius of deposit is a few hundred feet.
 - 3) A mammoth tusk was actually found protruding from an eroded remnant of this deposit.
 - 4) All the aspects of a palustrine spring-supplied deposit.
 - 5) Formed at surface of ground (lies upon the local pediment and seems to be only a very few feet thick), not within the soil.
- f) Deposits at south end of Crater Flat along main drainage. Reached via gate at US 95 NY 40.2.
- 1) Inside Crater Flat. Ditto below.
 - 2) Just outside Crater Flat in Amargosa Desert. This deposit is visible from US 95 (white mass of approx. 1000' diameter).
 - a> Composed of silt and carbonate, relative amounts not clear to me though silt content obviously high.
 - b> Most of deposit as seen is very friable but I do not know the deposit's character at depth. All deposits of this type are very soft in outcrop, but harden appreciably at a depth of a few inches. How they behave at a depth of a foot or so is unknown to me.
 - c> An existing hole a foot or two deep in the deposit seemed to show no change in sediment character with increasing depth.
 - d> Locally, hard zones at the surface display an incredible development of root casts and silicified (?) roots (\geq quarter of an inch in diameter, not diameter of roots of desert plants. The softer calcareous material surrounding the silicified (?) roots is eroding leaving the harder root forms as lag on the surface of the outcrop. With this guide, one can easily follow such casts into the mass of the rock (via inspection of broken surfaces).
 - e> This deposit contains layers composed entirely of diatoms, unequivocal evidence of standing water (E. Taylor, p. c.).

- f> In contrast to all of the sheet-like calcretes extending over many square miles on fan and valley floor surfaces, these small spring deposits are invariably white in outcrop.
 - g> The rootcasts, total lack of sand and gravel, etc. suggest a palustrine environment supplied by underground water, the resultant pond or marsh filling with wind-blown fines (E. Taylor, p. c.).
- g) Palustrine spring deposits near Ute along and just west of Interstate 15. Some are actually cut through by I-15.
- 1) Very friable on surface but they get harder within a very few inches. I don't know how they are at a depth of a foot or more.
 - 2) Clearly, there is a large silt component as in f) above. The deposits are distributed over about a mile of low area of valley floor. All of these are Muddy Creek deposits, the white carbonate component of these beds having derived from regional ground water discharge during Muddy Creek deposition (D. L. Schmidt, p. c.).
 - 3) Palustrine as at Ash Meadows. Much like silty member at f) above.
 - 4) Very fine grained, very white in outcrop. No gravel or sand component.
 - 5) In places, series of small vugs, most empty but occasional minor bridging of vug.
 - 6) Clear large ($\geq .25$ in) root cavities. On weathered surfaces, very clear, can be followed into interior on broken surface.
 - 7) Overlain by gravel calcrete which ranges across the valley and along its axis for miles. The calcrete is deeply cemented, gravel clasts float in fine-grained matrix in lower portions, case-hardening on steeply inclined surfaces (so much as to mask gravel texture and to give a false appearance of a thick fine-grained layer), limestone clasts at surface and in upper part of deposit show extreme solution of upper surface of clasts (concave upward or bowl-shaped on many clasts) and thick deposition on underside with development of very small "stalactites".

2. Sites of Pedogenic calcite-opal deposits

a) Several surfaces in neighborhood of Moapa - some data from Gardner, 1972

- 1) Mormon Mesa (Highest Surface) - (milepost 95 on Interstate 15 in Nevada)
 - a> This is the major surface that carries the name "Mormon Mesa" in the literature.
 - b> In most places, the present surface is the hard dense carbonate originally formed at a depth of some tens of centimeters, or, as the result of the great age of this deposit (certainly greater than 2 Ma), the present surface resulted from long-continued deposition atop the impermeable layer originally developed some tens of centimeters below the surface (D. L. Schmidt, p. c.).
 - c> This carbonate is micritic (not microsparitic as tufa under 1. a) above.
 - d> There are sand grains throughout it, in some places these being very numerous and always matrix-supported. Limited number of matrix-supported pebbles (nothing like surface at mile 86.2, see below)
 - e> Below hard dense horizon, carbonate deposition decreases rapidly with depth, there being only wispy films of carbonate in the silty sandy material. The hard dense horizon totally prevented transport of carbonate below itself.
 - f> Incipient nodules occasionally found below the calcrete. The surface of the ground is locally strewn with relic nodules lagging from erosion of the upper soil horizons. These nodules are always dominantly fine sand (and silt?) cemented by carbonate.

- g> From milepost 95, one can see miles of the eroded edge of this upper surface. The calcrete is somewhat wavy at its base but is nearly flat at the top. It goes on for miles like this with tens to hundreds of feet of deposit exposed below the well-cemented calcrete. Nowhere in all the exposures of the eroded edge of this surface is there even a hint of veins extending to depth and acting as sources of the carbonate.
- h> So, here is a layer of carbonate (and silica ?) that is today unbroken over many square miles (extends northward for at least 15 miles from here and east-to-west for at least that much), was originally more extensive as evidenced by the outcrop pattern of its remnants, and has not a single detectable feeder from depth. In total contrast to the deposit in 1. a) above.
- i> The subaerial exposure of this sedimentary surface occurred about 5 million years ago when the Colorado River captured the local drainage (Virgin River etc.) and drained the shallow lake (approx. 100' depth) in which the sediments (Muddy Creek Formation) had been accumulating for 5 MY or more (D. Schmidt, p. c.).
- j> In nearly all places, there is no faulting of this 5 Ma old surface. Locally, there is graben development with cumulative vertical displacement of about 200 feet.

2) Lower surfaces.

- a> At mile 86.2 on I-15 Nevada. Large gully coming in from east. Bridges on I-15 cross it.
 - 1> North bank of gully.
 - a: The upper surface at this site has a well-developed desert pavement composed of Tertiary volcanic fragments displaying desert varnish, indicating age of at least 150 Ka.
 - b: Within inches of surface, strong cementation of conglomerate. Floating gravel obvious.
 - c: Many cobbles extensively etched and pitted on upper surfaces.
 - d: Much case-hardening on vertical to near-vertical cut bank surfaces within the gravelly layer. These vertical layers of case-hardened calcite are very hard.
 - e: Below the layer of strongly cemented gravel, less and less carbonate introduced, the lowest exposed strata being nearly carbonate-free friable sand or sand-silt mixture.
 - f: This surface is probably 300 - 500 Ka in age, and at a single locality displays vertical fault displacement of 6 feet. Other faulting of this surface is insignificant (D. L. Schmidt, p. c.).
 - 2> South bank of gully.
 - a: Younger, lower surface. Poor development of pavement.
 - b: No conglomerate as under north-side surface, just scattered pebbles.
 - c: Well-developed platy-K horizon with sediment still between them.
 - d: Below the platy-K horizon, friable sandy silty stuff with small and scattered nodules (composed mostly of sand & silt, cemented with carbonate).
- b> Just north of Moapa turnoff from I-15, bluffs to west of road.
 - 1> Probably higher than last two surfaces discussed but uncertain. See Gardner, 1972.
 - 2> Strongly cemented, micritic, with sand grains in it.
 - 3> Upper levels of soil eroded away. Any original pavement is not present.
 - 4> Are things like this younger than higher levels or exhumed? Younger (Gardner, 1972 and D. L. Schmidt, p. c.).

- 5> Below the micritic carbonate-enriched zone (1 to 1.5 feet), the deposit is soft and friable.
- c> Follow Nevada Highway 168 west through Moapa to and beyond Warm Springs (Muddy River Springs).
 - 1> Note that the Muddy River originates as a group of springs issuing at present within the Warm Springs area, the water coming from the Paleozoic aquifer which transports water from as far north as Ely to these springs.
 - 2> One of the reasons for taking this route is to be surrounded by multiple calcrete soil surfaces at various elevations, from the Mormon Mesa itself to lower levels. Each must be associated with a still-stand or aggradation of the valley floor as it was being incised during the past 5 Ma or so. Some of the layers are so thick and impregnated by secondary carbonate that they must have taken over 1 Ma to develop.
 - 3> None of these surfaces are associated with feeders from depth, all are areally extensive or clearly were at one time.
 - 4> All zones cemented by secondary carbonate and/or silica were clearly generated within the soil environment.
 - 5> Continue to mileage 17.45 (1.45 miles beyond milepost "SR 168 CL 16"; you must use milepost 16 as reference because milepost 17 is missing):
 - a: The reason for driving to this outcrop is to see the extreme development of the platy-K structure typical of pedogenic carbonate deposits.
 - b: Go into the gully on the north side of the road and inspect the east face.
 - c: The interbedding of plates and sedimentary layers is well shown. This layering structure was clearly developed within the soil environment.
 - d: No such horizontal platy structure is ever found in tufa mounds.
- d> At and west of Ute, there are at least three pedimented calcretes that are crossed sequentially as one follows the gravel road to the west side of the valley.
- b) Just north of Las Vegas along I-15, there is a young surface that I never visited. I was going to do it on a field trip with Archambeau but he has been unable to find the requisite days for a field trip.
- c) On large fan extending from US 95 up Kyle and Lee Canyons into Spring Mountains (Nevada Hwys. 156 and 157).
 - 1) When driving up the lower portion of this surface, one is upon the broad alluvial fan formed from the Kyle Canyon drainage.
 - 2) As soon as the surface is gullied, the calcrete layering can be seen.
 - 3) The first gully I noticed was just before reaching the fan level where Paleozoic limestone ridges began south of the road. I don't know how far eastward the calcretes can be directly observed in the gullies.
 - 4) I sampled and inspected the calcrete in Kyle Canyon about .1 to .2 miles west of where the road leaves the fan surface and goes into the major gully of Kyle Canyon. I inspected the north side of the gully wall.
 - 5) Multiple plugged calcrete horizons over depth of 30 feet (?) with soft gravel between and below carbonate layers
 - 6) Thin platy-K horizons separating gravel with much carbonate distributed under and between pebbles (bridging).
 - 7) Feet of massively cemented gravel showing floating pebbles, extreme solution effects on upper surfaces of limestone pebbles.
 - 8) All pebbles are Paleozoic limestone.

- 9) The calcrete surfaces on this fan extend to at least 8000' elevation, can be observed at around 4000' and probably extends to the valley floor at less than 3000'. Thus, deposits a very few feet thick at most mantle a surface that changes in elevation by several thousand feet. Clearly, the deposits have developed in the soil environment, their distribution conforming to that environment. Also, their uniform thickness and character over several thousand feet of elevation makes it obvious that the process of formation was via a mechanism which was insensitive to elevation or depth of water table. There is no credible means by which ground water could have served as a significant element in formation of the calcretes of Kyle and Lee Canyons and the associated widely distributed fan.
 - 10) Where datable, horizons like this one take hundreds of thousands (to a couple of million ?) of years to develop.
 - 11) They extends north and south for many miles.
 - 12) This fan was completely developed by Plio/Pleistocene (late Miocene, D. Wiede, p. c.). There is no faulting of the surface of this massive fan which flanks the eastern side of the Spring Mtns. The area has been tectonically "dead" since the Pliocene if not earlier, to quote D. Wiede.
- d) US 95 in Nye County. Roadcut 4.5 miles east of Hwy. 373 turnoff (near Lathrop in Amargosa Valley).
- 1) 30' ? roadcut to north of highway. Nothing like it anywhere around.
 - 2) Well developed platy-K zone.
 - 3) Much detrital content in all zones.
 - 4) Looks like some opal layers.
 - 5) Soft horizons below with some coating on pebbles and a few carbonate "wisps" in fine grained material.
 - 6) May be lower platy K horizon, i. e., multiple development of calcrete.
 - 7) Clearly part of valley and valley-side surface that extends for many miles.
- e) US 95 in Nye County. Gate at 39.55 miles on US 95 NY (0.2 miles south of gate to spring deposits).
- 1) Pit dug just to left of road inside fence.
 - 2) Surface appears to me to be very young gravelly surface.
 - 3) Many pebbles have thin coatings of calcite on bottom.
 - 4) Thin platy carbonate horizons scattered throughout 1 to 2 feet of gravel.
 - 5) Below lowest such horizon, sandy gravel with little evidence of carbonate deposition.
- f) E. Taylor's pits on approach to Trench 14.
- 1) They illustrate early stages of development of soil carbonate, rather than fully developed calcretes as in examples above.
 - 2) Stream bank under Holocene surface -- wisps, very poor or minimal coatings on pebbles.
 - 3) First pit -- just calcite on bottoms of pebbles with no bridging. Under surface of about ?? age.
 - 4) Second pit -- Bridging of carbonate from one pebble to another, starting to form sheet of carbonate. Under surface of about 150 Ka (??) age.
 - 5) Third pit -- not visited due to lack of time.
 - 6) Fourth pit -- not visited due to lack of time.

3. Per ascensum versus per descensum source for soil carbonate.

- a) Arguments against per ascensum model (capillary rise from CaCO_3 -rich groundwater level (Figure 4.4, Goudie, 1983))

- 1) Blake (1902) proposed that secondary calcite accumulated in desert soils by "upward capillary flow of calcareous water, induced by constant and rapid evaporation at the surface in a comparatively rainless region." However, this process has been demonstrated as inoperative in most areas of the southwest where Blake proposed its operation. Reasons for its inapplicability in these areas are summarized (Machette, 1985):
 - a> Such a mechanism certainly cannot operate in a region of entrenched Pleistocene drainage (low drainage channels to the Colorado River also entrenched during much of the Pliocene (D. L. Schmidt, p. c.))
 - b> In essentially all areas of calcretes in the southwestern USA, the ground water from which the calcite might be derived has remained well below the surface since deposition of the soil parent material or shortly thereafter, i. e., too far below the surface for capillary rise to be a surficial process.
 - c> Concentration of Ca^{++} is usually low in groundwater, thereby limiting the potential amount of carbonate that could be precipitated if ground water were to reach the surface and evaporate.
 - d> Many calcic soils in the Southwest develop in medium- to coarse-grained sediments that have little potential for capillary rise (Mormon Mesa as an example, the calcrete having developed on the sandy Muddy River deposits observable at milepost 95 on I-15).
 - e> There are several situations where calcic soils and caliches in the USA SW have developed upon impermeable shales, there thus being no possibility for rising waters to have provided the deposited calcite.
 - f> Areas such as the Llano Estacado, Texas, are of rolling topography with relief of 80' or more. Caliche covers the entire surface. If ground water from the aquifer rose by capillary rise and deposited CaCO_3 , it would do so largely in the low areas and not on the high ones.
 - g> In the Llano Estacado, calcic soil development is distinctly less on the windward sides of rises than on the leeward sides or in the playas. Such a relationship seems unexplainable via a per ascensum model.
- 2) Additional arguments relative to southwest Nevada
 - a> It will be argued below that the depth of the water table under Yucca Mountain has always been greater than 1000'.
 - b> On each of the fan surfaces of Kyle Canyon, the thickness of the developed calcic horizons is independent of elevation on the surface (Surface 1 varies in elevation from 1300 to 2600 meters, Surface 2 from 1400 to 2100 meters, Surface 3 from 1200 meters to 2400 meters) and independent of slope of the fan surface. The shapes of these surfaces are constructional, not acquired by later deformation (Surface 3 is the modern surface). No credible conformation of a ground water surface, combined with the limited height of capillary rise in these fan deposits, could explain the generation of these surface relationships.
 - c> As will be discussed in detail in a later section, all available evidence in SW Nevada indicates the water table in most areas of calcrete development to have been at a depth of several to many hundreds of feet throughout the entire Pliocene, Pleistocene and Holocene, thus eliminating any possibility of the operation of the per ascensum model.
 - d> An argument relative to Yucca Mountain not implied in other discussions goes as follows (I. Winograd, p. c.):
 - 1> To begin with, consider the white calcitic veins in Pliocene fanglomerate (veins from 1 m to a few mm in thickness) discussed above near Death Valley. The fanglomerate is densely jointed, probably as the result of movement on the nearby transcurrent Furnace Creek Fault. Everyone agrees that these veins are the product of upwelling low temperature (i. e., ground water) solutions.

The point here is the pervasive emplacement of calcite into the myriad of available fractures in the fanglomerate. Several other tufas and associated vein-filled fractures can be seen by walking away from the road. Similar vein deposits with pervasive filling of all available fractures occur throughout Death Valley and the Amargosa Valley.

- 2> Now to Yucca Mountain. The east side of Yucca Mountain is a dip slope and the site of the various calcite and calcite/silica deposits (Trench 14, etc.). The faulted west face of Yucca Mountain is a series of alternating cliffs and shelves, conditioned by alternating hard ignimbrites and softer interbeds. The hard ignimbrites are characterized by extensive fracturing, there being 20 to 40 fractures per cubic meter. As is well known, fluid movement in such dense rocks as ignimbrites and limestones is primarily through fractures, not intergrain porosity.
 - 3> If, as proposed in the Minority Report, the surficial deposits on the eastern slope of the mountain are the product of upwelling hydrothermal solutions as in Death Valley and Amargosa Valley, dozens (even hundreds?) of the vertically extensive fractures in the ignimbrites on the west face of Yucca Mountain should be filled with calcitic veinlets as the result of the repetitive upwelling hypothesized in the Minority Report. Numerous tufas might be expected also. See below.
 - 4> The fact of the matter, of course, is that there are no vertically extensive fracture fillings in any of the fractured ignimbrites on the west face of Yucca Mtn and no tufas.
 - 5> Lack of such deposits certainly seems strong evidence against the hypothesis of upwelling of hydrothermal carbonate-bearing solutions into the mass of Yucca Mountain.
 - 6> When the lack of such deposits was pointed out to Szymanski during the 1988 field trip, his response was that he was simply a bureaucrat trying to help out on a problem and it was up to USGS geologists to explain the apparent anomaly. (Of course, when they did, he would not accept the answer).
- e> Maybe it is worthwhile to point out specifically that all of the hydrothermal deposits of the general area described above and referenced in the Minority Report are clearly the product of the cool waters of normal underground aquifers emanating at points where the topography dips below the local water table (the Muddy River north east of Las Vegas rises from just such an aquifer-supplied spring which drains a Paleozoic aquifer extending as far north as Ely). They are not the product of deep hot hydrothermal sources rising vertically many thousands of feet along fissures. Their existence is a logical correlate of all the facts of geologic history sketched in this document.
- f> I am surprised at the apparent way that the Minority Report discusses the movement of the water table under Yucca Mountain. Maybe I'm missing something but it seems to be discussed in terms of a puddle under the mountain unconnected with the gross aquifer-flow patterns of the area. They seem to talk about the water table going up and down in response to decrease or increase of the fracture porosity under the mountain. The report seems to ignore the simple fact that the deep ground water under Yucca Mountain is a small element of a widely extensive slightly dipping Tertiary aquifer, sloping towards its outlet or base level in Death Valley.

I totally fail to comprehend how they model hypothetical stress changes in the mountain causing major changes in water level within the mountain. If their conjectural rises are hypothesized as slow enough or as permanent enough to allow precipitation of the Trench 14 deposits, the mountain would never be filled with water as the rising water would flow down the surface of the upward-bulging water table to the surrounding very large drainage area. If the bulge was even a few miles broad, a high water table in Yucca Mountain would imply a lake or lakes in surrounding valleys, but such have never existed. If the hypothesis is rather of rapid transitory rise associated with quick collapse, thus not leaving evidence of its existence, deposition of the Trench 14 deposits becomes impossible as a lot of time and water would be required to deliver the requisite amount of carbonate from aquifers of such very low calcium content (see discussion of chemistry of the Yucca Mountain aquifers, while noting that spring carbonate veins where actually forming grow at rates of from about 5 cm/Ma to about 50 cm/Ma!! , or .05 to 0.5 cm/10,000 years!! , D. L. Schmidt, p. c.). If the final resort is to a whole series of quick rises limited in dimension to the mountain, the report will have arrived at a model so linked to special pleading and so insensitive to actual data from both the surface and boreholes as to create doubt about the sincerity of the proposers of the model.

b) Arguments in favor of a per descensum source.

- 1) All relationships listed above under a) are consistent with a per descensum model (Figure 4.5, Goudie, 1983). In addition,
- 2) The pattern of decreasing age towards the top of the Trench 14 calcretes, in conjunction with the clear plugging of the horizon at the base of the calcrete, indicates upward development of the most developed platy-K horizon and a surficial source for the carbonate.
- 3) Correlation of stage of development of calcic horizons and calcrete development on non-carbonate fans with direction of winds in Las Vegas Valley (Latham, 1973) establishes a dust source for the carbonate and thus a per descensum source for the calcretes in the valley. To quote from Latham (p. 3022):

"The most strongly developed cementation on fans not composed of carbonate detritus always occurs downwind of playas on whose upwind side are carbonate ranges and fans."

Also (p. 3023),

"The prevailing winds in western Nevada are from the southwest, west, and northwest. There are no extensive carbonate bedrock outcrop areas west of 118° 30' W and nowhere west (upwind) of this line was there found any carbonate cementation beyond light pebble coatings or local, very weakly developed calcic horizons. In eastern Nevada, extensive carbonate bedrock outcrops occur and noncarbonate fans downwind of these outcrops commonly show well-developed cementation including plugged horizons."

- 4) Extensive solution of upper surfaces of carbonate cobbles (halves, two-thirds, etc.) in some calcretes with thick deposition of carbonate on lower sides, with development of stalactitic structures is clear evidence of surface solution and redeposition.
- 5) Platy structure is always at the tops of hard calcretes if such calcretes have developed. Such structures probably result from movement of downward moving calcium bearing solutions along the top of plugged horizons or

horizons impermeable to downward flow. Thus, such platy structures are indicative of per descensum sources.

- 6) The always observed decrease of carbonate deposition with depth below a zone of maximum development, to the level of no addition of carbonate to the detrital material, is hard to explain via a per ascensum model but is a logical development in a per descensum model (originally shallow deposition due to evaporation in the soil, or as the result of water extraction by plant life, followed ultimately by plugging and upward growth of the deposit).
- c) The per ascensum model has been abandoned by investigators all over the world for good and sound reasons. I agree with them and I consider the resort of the Minority Report to this out-dated concept as in total error.
4. Conclusion. The reality of massive pedogenically derived meter-thick or thicker calcretes covering tens to hundreds of square miles throughout the US SW (and even into Oregon and Montana) is thoroughly established, and is unquestioned by anyone who chooses to go look and investigate them. In addition, their derivation in large part via calcium-bearing rain and dust has been established beyond any doubt.

For our purposes, the remaining question is whether the surficial deposits at Trench 14 are of pedogenic origin also and whether the so-called "veins" at Trench 14 are of pedogenic or hydrothermal origin.

urficial or soil carbonate-bearing layers at Trench 14 (see Taylor and Huckins, 1992, for more details of stratigraphy and for definitions of Stages)

1. Field data and ages

a) Unit 1

- 1) Pale brown, soft, gravelly silty sand.
- 2) Secondary carbonate forms thin coatings on the undersides of pebbles. Stage 1.
- 3) Basal contact is abrupt and wavy.
- 4) Estimated age -- latest Pleistocene or early Holocene.

b) Unit 2

- 1) Light yellowish brown to yellowish brown, compact, silty sand. Moderately sorted, subangular to subrounded sand, and 5-20% angular to subangular pebble-cobble gravel.
- 2) Toward the base of Unit 2, indurated platelets cemented by secondary calcite and opaline silica of unit 3 have been reworked into a fine-grained matrix.
- 3) Dated at 39 ± 10 and 55 ± 20 Ka.
- 4) Pinches out downslope.
- 5) Two discrete soil horizons, each with clear wavy lower boundaries, the lower one (2B+K) containing platelets cemented by carbonate (Stage IV) and opaline silica (Stage 4) that have been moved up from and (or) downslope from the horizon immediately stratigraphically below (3Kmq1).

c) Unit 3

- 1) Correlatable via physical and chemical characteristics to unit Q2e of Hoover et al. (1981), thus placing a maximum constraint on the age of Unit 3 at 720 Ka ("Unit 3 younger than Bishop Tuff", see below).

- 2) Unit 3 has yielded progressively older ages with increasing depth of 88 ± 5 , 270 ± 90 , 420 ± 50 , and 480 ± 90 Ka. Because the oldest age is near the maximum age determination possible by the method used (Uranium trend), the base of Unit 3 may be significantly older than 490 Ka.
- 3) An opaline silica band above the main fault zone, in the maximally developed K horizon that continues downslope into the slope-wash alluvium has been dated by the same technique as >350 , >400 , >400 and >550 Ka, i. e., it is possibly as old as the base of Unit 3.
- 4) Horizons
 - a> 3Kmq1 -- indurated by secondary carbonate and opaline silica, well sorted silty sand with 20% pebble-cobble gravel, clasts up to 20 cm across. Horizon characterized by carbonate and opaline cemented plates (Stage IV). Up to 10% of this horizon is composed of discrete opaline silica stringers that form "sandwich" like zones within the platy carbonate. This horizon is continuous, though fractured, over the main fault zone and bedrock on the upthrown block. Horizon contains lenses with up to 80% ooidic carbonate.
 - b> 3Kmq2 -- Very similar to 3Kmq1. See Taylor and Huckins, 1992 for further details.
 - c> Silty sand and gravel (5-40%). Cemented by disseminated carbonate (Stage III) and contains thin stringers of opaline silica (Stages 3 and 4) aggregating $<5\%$ of the horizon. In places, up to 50% of the horizon is composed of ooidic carbonate. Contains filled animal burrows.
 - d> 3Bkq1 -- Soft except for stringers of carbonate-cemented gravel, non-bedded and poorly sorted, silty sand with 15-20% pebble-cobble gravel. Stringers of dense continuous carbonate (Stage 3). Between stringers, carbonate forms continuous coats on the underside of gravel clasts with some matrix bridging (Stage 2). Lenses within the stringers are locally entirely ooidic carbonate.
 - e> 3Kbq2 -- soft, sand to silty sand with 10-15% pebbles and cobbles. Contains lenses of ooidic carbonate. Very little carbonate deposition (Stage I).
- 5) So, as in all soil carbonates visited, and as is typical of such deposits throughout the world, carbonate accumulation in the soil ceases below a depth of a few feet, no doubt as the result of deposition having sealed ("plugged") the zone against further downward transport of carbonate.

2. Rate of accumulation of atmosphere-derived soil carbonate at Trench 14.

- a) It is argued in the Minority Report that the rate of accumulation of soil carbonate via dust accumulation cannot begin to approach that required to explain the Trench 14 deposits.
- b) A few pertinent data:
 - 1) Taylor and Huckins, 1992, have measured detrital and carbonate contents by weight of the several horizons of Units 1 through 3 (their Table 2). Using their data and beginning at a depth of 50 cm. (very little carbonate above that level), one estimates there to be 110 gm per sq cm of carbonate in a column of soil and carbonate weighing 318 gm per sq cm, i. e., the two meters of deposit are only one third carbonate. Such a value is typical of soil carbonate deposits, not of tufa deposits which are nearly pure carbonate. Using a Unit 2 age of 40 Ka and a basal Unit 3 age of 490 Ka, one calculates a rate of carbonate addition to the Unit 2-3 column as $(110 \text{ gm/sq cm}) / (450 \text{ thousand years}) = 0.24 \text{ gm/sq cm/thousand years}$.
 - 2) The present rainfall in the area of Trench 14 is around eight inches per year (Winograd and Thordarson, 1975).

Minority Report that the very existence of those veins proves upward movement of the aquifers.

- e) As suggested by I. Winograd, the argument given earlier (pages 11-12) relative to absence of vein deposits on the scarp face of Yucca Mountain applies here also. If the calcite deposits in the vadose zone are the result of upward movement of hydrothermal solutions, why are no such veins seen on the exposed scarp where there is a plethora of fractures?

3. New data from deepening of Trench 14 to depth of 22 feet.

- a) The carbonate-silica veins terminate with depth! They are not extensions of veins extending to depth.
- b) The horizon at the base of the deepened trench described as a spring mound by Szymanski is clearly a volcanic vitric tuff. A hammer and a hand lens are adequate this fact.
- c) It seems obvious to me that the "veins" are filled from above, the fissures filled by these veins being the result of the fractured head-wall of the fault face slumping downhill as sediments were accumulating via dust accumulation, soil creep, etc. on the downthrown downhill side of the fault.
- d) A favorite argument of the Minority Report is that the veins found at depth within the vadose zone (approx. 1500' thick) in Yucca Mountain boreholes must by their very presence establish upward vertical movement of the water table to or near the surface of the ground. The isotopic data from these veins will be discussed below. At this point, it is only relevant to note that their basic assumption, i. e., that it is impossible for surficial water to carry depositable carbonate downward to the base of the vadose zone, is demonstrably false. Just because the base of the vadose zone here is 1500 feet or so below the ground surface does not *ipso facto* deny the possibility of a surficial source for the vein carbonates in these 1500 feet of vadose zone. Other data must argue the actuality of the carbonate source, and the isotopic data neatly do just that.

"Veins" at Trench 14 and their mode of origin.

1. Some horizons of surface-parallel deposits continue into veins, indicating the same mode of origin for both.
2. "Veins" disappear as a function of depth, the veins being, to my mind, fillings of slump features developed at the fault face (see above).
3. Chemistry of the veins is indistinguishable from that of the surface-parallel layers with consequent interpretation (see below) of similar origin.
4. To the original trench depth of 11' (only depth investigated by R. Forester), the "veins" are permeated by rootcasts and filled root cavities characteristic of plants of arid terrain (i. e., C3 and C4 vegetation), not of plants in water-saturated ground as is typical of spring deposits. There are even calcium oxalates filling rootcasts, a sure indicator of surface biologic activity (R. Forester, p. c.).

5. All evidences of biologic life, including $\delta^{13}\text{C}$ values of the carbonate mass itself, are typical of C3 or C4 vegetation, not of pond or marsh vegetation associated with outpouring springs (R. Forester, p. c.).
6. If one insists as does the Minority Report on proposing the untenable argument (see above) that the deposits at Trench 14 are the result of upwellings of carbonate-laden water with these upwellings occurring every 10,000 years or so, why are tufa deposits (like those cited in the Minority Report, for example) entirely lacking from the mountain (the nearest being 15 km away)? According to Szymanski, it is because they all have been completely eroded, the present "veins" at Trench 14 being the "throats" of such eroded tufas.
 - a) This seems highly unlikely in light of the
 - 1) extreme hardness of tufa deposits, they being dense fine-grained carbonate that resists erosion as effectively as marine limestone (for example, exhumed spring mounds west of Ute).
 - 2) existence elsewhere in the general area of tufas many many thousands of years in age. In Nevada, within 60-100 km of Yucca Mountain are tufas of up to several million years age surviving whatever erosional processes have operated. For example, the mounds west of Ute discussed above were exposed to erosion 4 to 5 Ma ago!! (D. L. Schmidt, p. c.).
 - 3) If the tufas of Yucca Mountain are so easily eroded, so must be the tufas in surrounding areas as well as other surficially developed deposits. However, throughout the USA Southwest including Yucca Mountain are Pleistocene pack-rat middens. These middens are composed of bits of vegetation and bone cemented by pack-rat urine. They are preserved beneath rock ledges and crevices in arid and semi-arid climates and are very delicate. Erosional processes (certainly largely chemical) capable of removing dense spring-deposited carbonate should have obliterated pack-rat middens in the same environment. Yet, dated middens have ages ranging from 1,000 to >40,000 years (I. Winograd, p. c.).
 - b) Therefore, the untenable hypothesis is doubly untenable.

conclusions relative to mode of origin of Trench 14 deposits based on data discussed in A. through C.

1. Trench 14 deposits do not have morphologic characteristics of either type of spring deposit observed in the general region.
2. Trench 14 deposits do have morphologic characteristics consistent with soil carbonate deposits, i. e., pedogenic deposits.
3. To my mind, these deposits are the result of carbonate and silica deposition within the soil environment. Truly, I cannot not see how anyone even casually familiar with the many many square miles of similar deposits mantling alluvial, coluvial and even bed-rock surfaces in this general region, and with the correspondence of these deposits in morphologic character with calcrete terraces throughout the American West (they extend from south Texas to south Montana, from eastern California through east central Colorado southward into New Mexico) can have any doubts as to the mode of origin of the Trench 14 deposits. See Machette (1985) Figure 2 for map of US calcic soils and calcretes.

4. The position of the Minority Report that the Trench 14 deposits could be (or most probably are) the product of hydrothermal solutions, or even per ascensum soil processes, is certainly totally false (remember the discussion above of per ascensum versus per descensum origin of the Trench 14 deposits).
5. To my mind, the morphologic arguments for a pedogenic source for the carbonates are so compelling that all of the work on isotopes, though of great interest and confirmatory character, has been totally unnecessary.
6. Taylor and Huckins (1992) give a much more elaborate and competent development of the morphologic evidence characterizing spring and pedogenic deposits than is given here. Their paper should be carefully read, in conjunction with Bachman and Machette (1977).

ignificance of bore-hole stress measurements.

1. I agree with the conclusions of Swolfs, Savage and Ellis (1988) that the stresses measured in boreholes in Yucca Mountain are those to be expected under the extant gravitational load in a mountain made markedly asymmetric by pervasive highly directional Miocene faulting.
2. As noted by them, there is no evidence in the measurements of stored stress resulting from tectonic process at shallow depths within and just below the mountain (an essential feature of their proposed stress release model), a not surprising result in light of the tectonic history sketched above and in light of the total lack of evidence of Holocene, or even Pleistocene, fault scarps in the area. (Note that stress relaxation of several tens of bars at the focal depth of local earthquakes (15 km. or so) of even magnitude 7 would not lead to the physical phenomena they engender within their model, their model requiring high deviatoric stress at very shallow depth. I have discussed this point with Archambeau and I am sure he agrees that their model fails if the only stress at shallow depths is that resulting from gravitational load. See comments later about the recent Little Skull Mountain Earthquake.
3. I think the emphasis given by the Minority Report to the potential for failure in tension of the rocks of the mountain if the water table rises to the surface is misplaced.
 - a) I will argue below why I conclude that the water table in Yucca Mountain has never been significantly higher than it is today, so concern with what might happen if it does rise are of no consequence. It has not risen by either of their proposed processes because there has been no tectonic process (see above) or heat flow convective process (see below) to cause occurrence of such.
 - b) I think their fundamental argument is questionable as the rising water table they hypothesize would, while increasing the gravitational load, also increase the horizontal confining stress on all blocks in the mountain, thus largely if not entirely negating the effect of increased gravitational associated with a high water table.
 - c) The discussion of water production of Yucca Mountain wells makes it clear that their have long been impenetrable barriers to vertical flow of water to the surface at Yucca Mountain, so that the proposed rise of the WT has not been possible for millions of years.

- d) In addition, investigation of the water production of the Yucca Mountain wells as a function of depth demonstrates that the Tertiary rocks under the mountain are typified more by lack of fracture porosity than by its presence, i. e., the hypothetical rise of water and stress change could not have the proposed effect on most of the Tertiary section.
 - e) Finally, the Minority Report proposes frequent rise of the WT during the Pleistocene as an essential element in their total "model". Why, if such has happened, has the mountain not "collapsed" long ago? One cannot seriously argue that the stress in the rocks of the mountain has changed significantly in the last few million years, most particularly not in the last several tens of thousands of years. If there ever was a threat of collapse of the type they propose, operation of their model would have eliminated it long ago.
4. The model used in the Minority Report of a 30 bar drop in tectonically induced stress throughout the mountain and depths immediately below as the result of a local earthquake of magnitude 6 or thereabouts is totally denied by the bore-hole data. Yucca Mountain and the depths immediately below are not at measurable tectonic stress, the releasable stress.
 5. Though earthquakes with rupture lengths of a few tens of kilometers appear on occasion to result in significant modification in spring and stream flow rates in the immediate area above the rupture, this fact has no bearing on expected events at Yucca Mountain there is no prospect of such an earthquake within the next 10,000 years. The argument used by Szymanski to support the case for such an earthquake within 100 years has no merit. See discussion below of the Little Skull Mountain Earthquake of June 29, 1992 and its significance (none) relative to the conclusions above.

history of faulting in the area and potential for a significant earthquake under or within a few tens of miles of the site.

1. Muddy River Deposits, Mormon Mesa and environs

- a) The Muddy River sedimentary sequence contains datable volcanic ashes low in the sequence which have ages as old as 10-12 ma (D. L. Schmidt, p. c.). These deposits post-date essentially all major faulting in the area. The present mountain/valley conformation was established that long ago. All who have studied the tectonics of southwest Nevada agree, via various data and arguments, that the area is essentially tectonically "dead", much more so than to the north.
- b) The Mormon Mesa surface was developed on the top of the Muddy River sedimentary series. The calcitic soil deposition of the Mormon Mesa surface began about 5 ma ago, at the time of capture of the Virgin River drainage by the Colorado River (resulting in drainage of the Muddy Creek Lake). Minor faulting breaks this surface in a localized small area developing there cumulative graben-like displacements of about 200 feet. The tufa mounds near Ute described above, though located along a fault zone, were not associated with any actual faulting nor has there been any significant faulting along this line in the last several Ma (D. L. Schmidt, p. c.).

- c) Younger calcrete surfaces were developed on lower erosional terraces below Mormon Mesa at lower elevations inside the valley eroded in the Mormon Mesa. As noted above, some of these have thick micritic, carbonate soil zones, implying more than half a million years to generate them. These younger surfaces show less and less effects of localized faulting the younger they are (see above) (D. L. Schmidt, p. c.) and nowhere show any significant faulting.

2. Kyle and Lee Canyons, Spring Mountains.

The massively developed alluvial fans rising on both sides of the Las Vegas Valley (specifically, the large fans extending up Kyle and Lee Canyons towards Mt. Charleston) in the Spring Mountains, are in their higher portions 5 to 6 Ma old (D. L. Schmidt, p. c.). The coalescing fans coming down Kyle and Lee Canyons and reaching into the valley are actually composed of three surfaces. From bedrock outcrops to the center of the valley along US 95 there is no evidence of faulting of any of these surfaces.

3. History of Faulting in SW Nevada.

- a) Everybody who writes about the geological history of SW Nevada agrees that large scale tectonic activity essentially ceased 10-12 ma ago. It doesn't matter whether it is Carr or Hamilton with their different scenarios of faulting.
- b) There are, of course, two zones of Pleistocene faulting in the area, the Furnace Creek Fault along the east side of Death Valley and the faults along the east face of Bare Mountain (Reheis, 1988). There is no detected evidence of faulting east of the latter.
- c) The presence of only 100 feet or so of Pleistocene or Pliocene movement on range front faults between Beatty and Mesquite (D. L. Schmidt, p. c.) and the absence further south of detected movement along some range front faults such as that fronting the Spring Mountains (maximum elevation of 12,000 feet) implies very low probability of movement along minor faults associated with such trivial features as Yucca Mountain.
- d) Summaries of mapped evidence of Holocene and Pleistocene faulting in southern Nevada (Wallace, 1981 and 1984) indicate there to be no reported evidence of Holocene faulting in this area and probably no evidence of faulting for the past 500,000 years or more east of Bare Mountain ("Late Pleistocene" in Wallace, 1984 is intended to denote the last half or third of the Pleistocene, i. e., 1 Ma or so, R. Wallace, p. c.)

4. Yucca Mountain

- a) In Trench 14, the base of Unit 3 in the hanging wall block of the Bow Spring Fault may be as old as or much older than 500,000 years. It is interesting that Unit 3 seems to be much thinner on the hanging wall block. Unit 3 crosses the fault and shows no displacement within it. Thus, there has been no displacement along the Bow Spring Fault for at least the last 0.4 - 0.5 Ma.
- b) Faulting along west side of Yucca Mountain.
 - 1) There is no evidence of active faulting.
 - 2) The implication within the Minority Report of the potentiality for a magnitude 6 or even 7.7 earthquake in the immediate vicinity of Yucca Mountain

rupturing the surface and releasing significant tectonic energy at shallow depth has no basis whatever in demonstrable fact or even suggestive relationship. See later discussion of recent Little Skull Mountain earthquake.

- 3) There is no reason I know of for hypothesizing the potentiality of even minor surface rupturing along this zone within the next 10,000 years. See later discussion of recent Little Skull Mountain earthquake.
- 4) In northern and central Nevada, there are clear fault scarps cutting Quaternary alluvial fans and pediments. Return times on faulting along these scarp-lines is 7 - 10 thousand years (R. Wallace, 1977). Detailed analysis by a variety of investigators and techniques demonstrate that a one meter displacement along these scarps persists as a detectable surface for 100,000 years (T. Hanks, et al., 1984). Lack of any evidence of scarps along the west side of Yucca Mtn., in an erosional environment that is probably less severe than in northern Nevada, demonstrates to me that the faulting/earthquake threat along the west side of Yucca Mountain imagined in the Minority Report is nonexistent.
- 5) Another implication of the Minority Report is that lengths of rupture versus magnitude that are typical of western California are also typical of this area. Such an implication is false (Evernden, 1981; Evernden and Thomson, 1988).

	Western California			
Magnitude	6.0	7.0	7.5	8.0
Length of rupture (km)	10	50	100	250

	Nevada			
Magnitude	6.0	7.0	7.5	8.0
Length of rupture (km)	2	10	25	50

Thus, the physical phenomena associated with a magnitude 7 event in Nevada are those associated with a magnitude 6 event in western California, an inadequate event as regards the phenomena of importance to the Minority Report.

- 6) Thus, the hypothesized event of vanishing probability would not create the phenomena proposed in the Minority Report.
- 7) I have not discussed theoretical models of the possibility of water flow from depths of thousands of feet associated with a large earthquake for two obvious reasons:
 - a> I believe the earthquake required to make such deeply derived flow credible in any minds has no possibility of occurrence within the next 10,000 years. "No" means that there has been such event in the last million years (probably several ma), so why should there be one in the next 10,000?
 - b> Conclusions drawn from such models are dependent upon setting conditions and parameters that no one knows with certainty. Construction of such models is useful for guidance of thought but not for drawing firm conclusions. I thought it pointless to spend time evaluating studies that I deem irrelevant to Yucca Mountain, and I do not wish to appear to give credence to the potentiality of such an earthquake by discussing such models in connection with Yucca Mountain.

5. Significance of the Little Skull Mountain Earthquake of June 29, 1992 to the analysis given above.

a) In brief, the answer is that, rather than being symptomatic of seismic risk for the proposed repository, the earthquake refutes nothing given above, while providing strong evidence itself of the lack of seismic risk at Yucca Mountain..

b) In more detail:

1) In the first place, the occurrence of a small earthquake anywhere in southern Nevada cannot be considered as a basis for refutation of several million years of geologic history. I include just for fun the observation which could be documented that it is highly probable that the City of Chicago, Illinois, will have to endure significantly higher earthquake-induced ground accelerations within the next 1000 years than will be experienced at depth under Yucca Mountain in the next 100,000 years.

2) That recent Little Skull Mountain earthquake occurred at a depth of 15 or so kilometers, a normal depth for Nevada earthquakes. It had a rupture length of about 1 km (Evernden and Thomson, 1988). The Loma Prieta earthquake, an earthquake with a rupture length of about 50 kilometers and a stress change at the failure surface of about 100 bars caused a stress change (relaxation, not increase) on the Haywards Fault, at a distance of 30 kilometers, of about one bar. So, we use as a rule of thumb for this discussion that there is about a 100-fold drop in stress change from the failure surface to a perpendicular distance from the fault of one-half the rupture length. Thus stress changes for the Little Skull Mountain earthquake were less than a bar at a distance of half a kilometer from the rupture. Such small stress changes at such small distances from short ruptures is why there can be several earthquakes of small rupture length from the "identical" (in so far as seismologists can determine) point.

3) If we scale up the Little Skull Mountain earthquake to magnitude 7 (a certainly very infrequent event), the rupture of 10 kilometer length probably will not reach the surface, even if the point of initial rupture was at the base of the failure zone. Stress change at five kilometers from the middle of the rupture will be less than a bar as will the stress change at 5 or so kilometers off the end of the rupture even if original stress at such sites was similar to that at the point of rupture.

4) It is a generally observed seismological fact that there is little stress release in the upper few kilometers of the earth's crust even when the fault rupture reaches the surface. For even the earthquakes of greatest rupture length and extensive surface rupturing, the amplitudes of short period phases behave at short distances from the fault as if all arriving seismic energy derived from depths of several or more kilometers (amplitude of ground motion ceases to increase several kilometers from the fault as the fault-line is approached from distance), establishing unequivocally that the rocks at shallow depths in the earth are at very low states of tectonically-derived stress. The Dixie Valley, Nevada earthquake of December 16, 1954, an earthquake which ruptured to the surface for numerous kilometers, did not generate high amplitude waves in the near-field. In fact, near-field shaking was less than expected at comparable distances from a California strike-slip fault, possibly related to the fact that there was a large component of normal fault motion. Evernden and Archambeau (1986) pointed out that energy release for even large earthquakes is nearly invariably (always, as far as our data could tell) at

significant depth (10 kilometers or more) below a near-surface low velocity zone while the energy release for explosions is at the surface with a resultant amplification of surface wave amplitudes, these relationships markedly complicating the problem of distinguishing the seismic waves of earthquakes and explosions. An unpublished study by myself using Archambeau's programs determined that an explanation of the Rayleigh wave amplitudes of the Imperial Valley earthquake of 1979 required essentially all energy release to have occurred below the near-surface low velocity zone of several kilometers thickness, even though this earthquake ruptured to the surface for several tens of kilometers. The surface wave amplitudes of the Loma Prieta earthquake were low for California earthquakes, probably linked to the greater than normal depth of that earthquake.

- 5) Obvious conclusion: Since large earthquakes in California rupture to the surface but release all of their seismic energy at depths of several kilometers or more, it is established that there is generally very little tectonic energy to be released in the upper several kilometers of the earth's crust! The evidence from the boreholes on and near Yucca Mountain and the evidence of the Dixie Valley earthquake indicate that the same situation applies in Nevada. It actually applies throughout the world, as evidenced by the M_s/m_b relationships for world-wide earthquakes (Evernden and Archambeau, 1986) and the amplitudes of ground motion associated with large earthquakes throughout the world (Evernden, 1983).
- 6) All of this suggests strongly that any typical Nevada earthquake of magnitude 7 at normal depths, as was the Little Skull Mountain earthquake, will be associated with insignificant stress changes at or within a few kilometers of the surface.
- 7) Archambeau might well respond that the arguments above, though generated with his participation, must be wrong in specific cases and most particularly in SW Nevada because of the "tectonic release" (long period energy release in a pattern consistent with the stress fields related to earthquakes of the area) observed at the time of shallow nuclear explosions in hard rocks at Nevada Test Site (NTS). In conversations with Archambeau, he has expressed the view that this "tectonic release" implies tens of bars of tectonic stress at shallow depths.
 - a> In this regard, I point out that:
 - 1> When Barry Raleigh measured stress in the hard tuffs under Yucca Flat at 5000' depth via strain rosettes, he found only load stress at a value consistent with the depth and rock density (i. e., zero tectonic stress), even though a subsequent explosion in that hole showed significant "tectonic release".
 - 2> The effective strength of the hard rocks of NTS, as determined via empirical insertion of a strength parameter selected to explain observations into theoretical codes, is about 100 bars, not the multi-kilobar strength of small laboratory samples.
 - 3> As Archambeau's relaxation theory makes clear, relaxation is quantitatively significant to a wavelength or two. Thus the long period ($T = 20$ second) waves of relevance in measurement of "tectonic release" are developed via quantitatively significant relaxation out to distances and depths of over 100 kilometers. They are not the result of relaxation within a few kilometers from the epicenter.

- 4> Relaxation theory, combined with observations of 1 second P waves and 20 second surface (Rayleigh) waves, clearly shows that the effective relaxation volume for 20-second waves is the same for small earthquakes (rupture lengths of a kilometer or less) as that for large earthquakes (rupture lengths of many kilometers). Even if the tectonic stress environment around a small rupture were several tens of bars, there would be very low 20 second Rayleigh waves unless relaxation extended to many tens of kilometers.
- 5> Thus, it is clear that the relaxation leading to significant "tectonic release" is relaxation below several kilometers depth, no matter what the state of stress in the upper few kilometers.
- 6> The quantitative explanation of the "tectonic release" phenomenon (observed at some level for explosions throughout the world) is not yet secure, but it must incorporate low but non-zero near-surface tectonic stress (must be some decrease in stress to which the rest of the world can respond via relaxation), low fundamental strength of the near-surface hard rocks, and consequent large volume of the de-stressed sphere surrounding the explosion.

b> I conclude that the as yet not fully understood phenomenon of "tectonic release", observed at NTS and elsewhere in the world, does not refute the massive data of world-wide earthquakes as regards the low to vanishing level of tectonic stress in the upper few kilometers of the earth's crust.

- 8) So, let us calculate the expected MM Intensity and peak acceleration expected at the surface of Yucca Mountain as the result of a magnitude 7 earthquake (10 kilometer rupture -- see table immediately above) under Yucca Mountain. Using the same programs as used immediately below, an MMI of 6.3 and an expected peak acceleration of 0.15 g are calculated at the surface for the materials of Yucca Mountain. There would be almost certainly a factor of two or greater amplification at the top of the ridge as the result of topographic effects, giving expected values of 0.3 g or greater. However, such amplification would not occur at the depth of the proposed repository. In fact, underground amplitudes of ground motion are markedly less than surface amplitudes. Thus, an underground installation in Yucca Mountain would experience a peak expected horizontal acceleration of about 0.1 g (or a possible (2 x expected) peak value of about 0.2 g) at a frequency of several Hertz (lower accelerations at lower frequencies) as the result of a magnitude 7 earthquake immediately under the installation.

The probability within the next 10,000 years of an earthquake under Yucca Mountain of sufficient size to rupture to the surface is so miniscule (by the arguments given above based on faulting in SW Nevada during the Pliocene and Pleistocene) that I do not consider it relevant to give estimated ground motions for such an earthquake.

- 9) Inspection of rockfalls associated with the Little Skull Mountain earthquake and their interpretation are in progress with resultant preliminary interpretations (Brune, 1992). To quote from that report:

paragraph 7:

"The Little Skull Mountain earthquake dislodged numerous large boulders along the crest of Little Skull Mountain. This was to be expected as a consequence of the high ground accelerations likely in the immediate vicinity of the earthquake (on ridge crests, JFE)."

"Near the proposed repository site in Solitario Canyon a large number of precariously balanced rocks have been documented. A technique is being developed to use such rocks to place upper limits on the ground motion for the last several thousand years Although the technique requires further quantification, it does suggest that the region of Solitario Canyon near the proposed repository site has not been subjected to large ground accelerations (greater than about 0.2 g) in the last few thousand years No precarious rocks of the type found in Solitario Canyon have been observed in any of the regions of strong shaking around historical earthquakes in Nevada and California."

Comment by JFE: Remembering the values of acceleration predicted at the ridge crest (expected 0.3 g or greater, possible (twice expected) 0.6 or greater)) for a magnitude 7 earthquake under Yucca Mountain, the presence of large numbers of precariously balanced rocks in Solitario Canyon (estimated value of acceleration required to tumble these being about 0.2 g) implies that nothing approaching a magnitude 7 earthquake has occurred under Yucca Mountain in the last few thousand years.

- 10) The arguments given above force anyone who wants to take seriously the Minority Report's model of the earthquake-related risk to any Yucca Mountain installation to deny reality for they must hypothesize an earthquake which cannot occur, a large earthquake rupturing to the surface and releasing several tens of bars of tectonic stress at shallow depth, with consequent accelerations well in excess of 1 g.
 - 11) Another point worth mentioning is that damage from earthquakes is usually the result of shaking or shaking-induced ground failure. The free field peak ground motion (the value predicted above) is magnified within a structure as it vibrates like a pendulum or seismometer. An underground structure is constrained by its enclosed environment from significant vibration and thus from amplification of free field motion. Thus, a 0.1 or 0.2 g free field maximum acceleration will mean much less to an underground structure than to a surface structure.
 - 12) So, the Little Skull Mountain earthquake, rather than being symptomatic of seismic risk to the proposed repository, provides strong evidence that there is little or no seismic risk to the proposed repository.
6. Faulting along Furnace Creek Fault and potential shaking at surface and within Yucca Mountain.
- a) This entire area lies within the K=6 area of the attenuation map I have developed, based on actual earthquakes, for the USA coterminous 48 (Evernden, 1981; Evernden and Thomson, 1988).
 - b) The maximum potential rupture length in such regions is about 80 kilometers (not 400 kilometers as in K=7 areas, i. e., western California).
 - c) Putting a fault of 80 km. length on the Furnace Creek Fault immediately opposite Yucca Mountain, and using a ground condition factor of -2.2 (see Evernden and Thomson, 1988, for details of the model and other references to this type of analysis), the model predicts expected mean values of peak acceleration and velocity of 0.11 cm/sec/sec and 12.6 cm/sec, expected maximum values of 0.22 cm/sec/sec and 25.2 cm/sec, respectively, at the surface at Yucca Mountain.

These values can be divided by two or more for predicting motion at depth within the mountain.

- d) In other words, maximum faulting along the Furnace Creek Fault, an event of unknown probability in the next 10,000 years, should pose no threat to the proposed repository.

7. Potential faulting at Yucca Mountain in my perspective.

Of course, I cannot guarantee that the tectonic history of the past 10 Ma as regards Yucca Mountain will not be countermanded within the next 10,000 years, in the same way as I cannot guarantee that the earth will not be impacted by a large meteorite within the next 10,000 years. To put things in perspective, I consider the probability of the latter event as greater than that of the former.

Data from boreholes at Yucca Mountain.

1. Diagenetic changes of volcanic rocks as a function of depth. (Broxton, et al., 1987)

a) Diagenetic zones

1) Zone I

- a> Thickness -- 170-584 m.
- b> Zone I occurs above the modern water table.
- c> Fresh volcanic glass, smectite, opal, cristobalite
- d> Widespread preservation of glass in vitric tuffs; smectite and opal are the primary alteration minerals. Ca-clinoptilolite and/or heulandite are confined to local zones of alteration.

2) Zone II

- a> Thickness -- 480-700 m.
- b> Clinoptilolite, mordenite, opal, cristobalite, authigenic K-feldspar, smectite.
- c> Original volcanic glass is replaced by clinoptilolite, mordenite and silica phases. Smectite and authigenic feldspar are minor diagenetic minerals.
- d> Top of Zone II is about 950 m above SL in USW G-3 in southern part of Yucca Mountain and >1650 m above SL at north end of Yucca Mtn, where the zeolitic tuff of Calico Hills crops out at Prow Pass. Between USW G-3 and USW G-1, the contact between the zeolitic rocks of Zone II and the vitric rocks of Zone I is about 225 m above present SL on west side and 120 m above present SL on east side of Yucca Mountain.

3) Zone III

- a> Thickness -- 98-400 m.
- b> Analcime, authigenic K-feldspar, quartz, smectite, calcite
- c> Analcime, quartz, and authigenic K-feldspar replace clinoptilolite, mordenite, opal and cristobalite. Cores of some plagioclase phenocrysts are replaced by calcite.

4) Zone IV

- a> Thickness -- >750 m
- b> Authigenic albite, authigenic K-feldspar, quartz, smectite, calcite
- c> Authigenic albite replaces analcime. Feldspar phenocrysts locally altered to calcite, authigenic albite, and K-feldspar. Mafic phenocrysts are altered to chlorite, epidote and iron oxides. Diagenetic processes may affect devitrified rocks as well as those rocks that were formally vitric.

b) Open versus closed system diagenesis.

- 1) The variable compositions of zeolitic tuffs, as well as the contrast in chemical composition of bulk rock samples of unaltered vitric and devitrified tuffs and the equivalent rocks subsequent to zeolitization (Zone II) (Figure 11 a of Broxton, et al., 1987), suggest that the formation of zeolites from volcanic glass in Zones I and II occurred in an open chemical system at Yucca Mountain.
- 2) The identical bulk composition of Zone II, III and IV rocks suggest that the rocks of Zones III and IV, formed as they were from previously zeolitized rocks (Figure 11 b)), suggest formation in closed chemical systems. The chemical differences east to west in the zeolitized tuffs are preserved in Zones III and IV, indicating restricted chemical migration, probably because of the low permeability of the zeolitic tuffs during the mineralogic transformations.

c) Temperature of diagenesis.

- 1) The reported present day geothermal gradient at Yucca Mountain ranges from 20° to 40°C/km; the higher gradients are in the northern part of the mountain. Based on these gradients, ground water would have had to be saline brines containing 10⁵ ppm Na⁺ to form analcime and authigenic albite at such low temperatures. No evidence exists for subsurface brines in the Yucca Mountain area, now or in the past, and modern ground water in the area generally contains <100 ppm Na⁺ (see later discussion of chemistry of Yucca Mountain aquifers).
- 2) Thus, the present diagenetic zone boundaries were established during an earlier period of higher geothermal gradient probably associated with emplacement of upper crustal magma chambers of the Timber Mountain-Oasis Valley caldera complex to the north (see d) below).
- 3) The upward displacement and thinning of diagenetic zones is likely due to a higher geothermal gradient in northern Yucca Mountain which was closer to the locus of silicic volcanism.

d) Time of diagenesis.

- 1) The boundary between vitric Zone I and zeolitic Zone II (parallel and at or near water table of the time) is a planar surface dipping gently eastward, cutting across stratigraphic contacts of volcanic units which also dip eastward but at a slightly greater angle. These relationships suggest that zeolitization ended before uplift and rotation were completed.
- 2) Time of tilting of the stratigraphic units is constrained to have occurred between 11.3 and 12.5 Ma ago. The Tuff of Lithic Ridge, one of the oldest volcanic units affected by diagenetic alteration, has an age of between 13.7 and 13.9 Ma. Thus, most of the zeolitic deposits were formed between 11.3 and 13.9 Ma ago and were contemporaneous with the most active period of silicic volcanism within the southwest Nevada volcanic field.
- 3) Authigenic illites from Zones III and IV in drill holes USW G-1 and USW G-2 have K-Ar ages of 10.9 ± 0.6 Ma (there are now ten such dates), indicating that this deeper more intense alteration was contemporaneous with Timber Mountain volcanism.
- 4) No available data suggest a later period of elevated temperature and associated diagenesis.

e) Conclusions

- 1) Vertically zoned diagenetic mineral assemblages formed in response to mineralogic transformations as temperatures rose during burial of the tuffs.
- 2) Diagenetic zones rise in elevation and thin northward, reflecting higher temperatures in that direction.

- 3) The present diagenetic zone boundaries did not form in response to the modern geothermal gradient, but developed in response to emplacement of the Timber Mountain-Oasis Valley caldera complex to the north.
- 4) These diagenetic changes were complete 10-11 Ma ago, with there being no evidence of later significant diagenetic alteration of the tuffs.
- 5) Water table elevations today are only slightly different than they were at the end of diagenesis.

2. Static head and flow-rates in boreholes as a function of depth and stratigraphy.

a) Well UE-25#p1

1) Water yield as a function of depth

- a> The fact that the static head of the Paleozoic aquifer is 20 meters higher than is the present WT (see below) establishes the effective impermeability of the lowest part of the Tertiary rocks, and is consistent with their essentially zero water yield.
- b> A small proportion of the production occurred from older tuffs (unnamed) and the Lithic Ridge Tuff (873-1137 meters depth). Exactly how little is unknown because of a leaking cement plug at the time of the test.
- c> No measureable yield from the Tram Member (690-873)
- d> Very little yield from the Bullfrog Member (558-683)
- e> Lower part of the Prow Pass Member yielded no water.
- f> An interval > 30 m thick in the upper part of the Prow Pass Member yielded 58% of the flow.
- g> Calico Hills tuffaceous yielded less than 2 % (381-422), although almost the entire unit was saturated (WT very near the top of this unit).

h> In Paleozoic section, only about 5% of the yield came from below about 1550 m (well to 1805 m), suggesting that

- 1> water movement in the Paleozoic limestones under Yucca Mountain may be either largely the result of weathering and resultant porosity/permeability at the old now-buried erosion surface or, as favored by I. Winograd based on data presented in Winograd and Thordarson (1975), a zone of fracture porosity fortuitously at that old erosion surface.
- 2> As far as yet penetrated, deeper Paleozoic limestones under Yucca Mountain are presently impermeable and not characterized by high fracture-engendered porosity and permeability.
- 3> I. Winograd has interpreted the presence of fractured intervals at different depths in different wells in the area of Yucca Mountain (always intermixed with thicker sections of impermeable strata) as evidence that the entire column of Tertiary volcanics is open to vertical flow through a highly contorted but interconnected fracture system. As I discussed with him, I find the long-sustained differences in static head (see immediately below) as compelling evidence that no such interconnection exists.

2) Static head as a function of depth

- a> Depth WT to 834 m below ground surface – Static head 729.9 to 730.8 m ASL (ASL = above sea level).
- b> Depth 1044-1114 m – Static head at 734.5 m ASL. This increase of static head with depth exists today even during times when the recharge rate of the Tertiary aquifer is less than required to maintain the level of the water table at its pluvial levels, and less than discharge (I. Winograd, p. c., Benson and Klieforth). The progressive rise of static head with increasing depth in this interval indicates an effective impermeability to

upward vertical flow, the impermeable unit being the zeolitized Calico Hills tuffs.

- c> Depth 1297-1805 m -- Interval within Paleozoic limestone. Static head 750.8 to 751.9 m ASL, i. e., 20 meters above the present water table and 15 meters above the static head within the Tertiary section at a depth of 1114 meters. Thus it is clear that the basal Tertiary section is effectively impermeable to upward vertical flow.

b) Well USGS GW-1

- 1) Static head as a function of depth

- a> WT at 730 m ASL

- b> At depth of 1800 m ("in Crater Flat Tuff"), head at 784 m ASL, i. e., 50 meters above the WT. This producing horizon may be the same as that producing 58% of the flow in UE-25#p1.

- c> Well did not reach the Paleozoic.

- c) Based upon the data of these two wells,, both the Tertiary section beneath the present vadose zone and the Paleozoic section cannot contribute to upward vertical flow within the vadose zone due to permeability barriers. However, I. Winograd, in response to the interpretation above, has responded: "Clearly, the Tertiary strata below the Tonopah Springs Formation are generally aquitards; but they contain permeable fractured intervals, that can be pumped at moderate discharge (i. e., they contain interconnected fractures). The absence of clear-cut stratigraphic relationships of fracture zones in adjacent holes, plus the near-vertical attitude of the fractures leads me to favor vertical hydraulic connections within the upper half of the tuff sequence."

- d) The sometime-expressed view that the Tertiary section under Yucca Mountain is essentially everywhere permeable via an omnipresent fine-scale open fracture system, with resulting incredible weakness and potential for deep collapse, is denied by the data given above. Most horizons below the water table do not contain any open fractures today (and probably have not for the last 10 Ma, see several places in text). Long ago, the Tertiary section may have been permeable throughout via extensive fracturing, but metamorphism sealed any such zones in most of the Tertiary section.

3. Distribution of vein calcite as a function of depth (Z. Peterman, p. c.)

- a) Present in fractures in Zones I, III and IV

- b) Absent from Zone II, i. e., from the zeolitized tuffs of the Calico Hills, the exact unit that, though saturated, produces very little water as well as being the unit separating zones of differing water pressure. This absence of veins may be the result of the impermeability developed as an inherent element of the processes of zeolitization. If so, it suggests a zone essentially impermeable to upper movement of ground water from depth. It is possible that this impermeability is inherited from the time of diagenesis (10 to 11 Ma ago).

- c) Whether the calcite in Zones III and IV was or could be a concomitant of diagenesis or a later process will be discussed below.

4. Interpretation/Conclusions of all above

- a) The data suggest barriers to vertical flow at the base of the Tertiary section, within the Calico Hills member and probably at other depths. It appears that these volcanics are only capable of significant transport of water in narrow intervals

while much of the Tertiary volcanic rock underlying Yucca Mountain is today effectively impermeable to upward vertical flow of water, most particularly the basal portion of the pile as well as the zeolitized Calico Hills tuffs (I. Winograd agrees with this overview (Winograd, p. c.)).

- b) The discussion of diagenesis of the volcanic rocks of Yucca Mountain established that the diagenesis was completed 10-11 Ma ago.
- c) If one wishes to propose that these rocks have been fractured repeatedly via tectonic activity in the last 10^6 years, thus permitting upward vertical flow, one must then suppose them to have been just as repeatedly totally resealed by processes unknown and unsupported by data.
- d) Credulity is stretched far less by simply accepting what appears to be the obvious interpretation as well as an interpretation consistent with data of several distinct disciplines, i. e., the mountain has been essentially sealed to upward vertical transport of water for the past 10 Ma.

5. Potential analogs for Trench 14 "vein" deposits (Vaniman, et al., 1988)

- a) Major depositional features of Trench 14 "vein" deposits
 - 1) Abundant calcite and opal-CT, generally intergrown but with some relatively pure silica laminae.
 - 2) Clay minerals, including smectites and chain-structure clays such as sepiolite.
 - 3) Opal-A is present where organic structures are preserved.
 - 4) Thin layers of black volcanic ash.
 - 5) Presence of ooids.
 - 6) In the deposit as a whole, cross-cutting laminae are frequent.
 - 7) Fine scale ("fractal" character) root casts and root fillings
 - 8) Cyclic co-precipitation of calcite and opal.
 - 9) The connected surface-parallel deposits
 - a> do not extend to the surface;
 - b> are only 25-55% calcite, most of the remainder being detrital fine silt to cobbles;
 - c> change in carbonate content with depth, being very low at the surface, reaching a maximum and then decreasing to nearly zero.
- b) Potential analogs
 - 1) Hydrothermal veins of area
 - a> typically associated with sulfur-bearing minerals
 - b> some near-surface hydrothermal veins in the Calico Hills, though containing no sulfur, contain no calcite while containing opal-C (rather than opal-A or opal-CT) plus quartz and abundant manganese mineralization.
 - c> no detrital component such as clay or ash.
 - d> Conclusion: No hydrothermal veins with the mineralogy of Trench 14 are known.
 - 2) Warm-spring deposits of area
 - a> all sites contain abundant sulfur minerals
 - b> if opal occurs, it is always opal-A, not opal-CT (appears that opal-A in such deposits goes to cristobalite, chalcedony and quartz without going through an opal-CT stage)
 - c> calcite is rare
 - d> no detrital component such as clay and ash
 - e> Conclusion: No warm-spring deposits with the mineralogy of Trench 14 deposits are known.

- 3) Cold-spring deposits
 - a> composed mostly of calcite (>99%), generally euhedral microspar.
 - b> essentially no silica.
 - c> ooids present in tufa, none in veins.
 - d> no cross-cutting laminae.
 - e> At vein level (not surface tufa), finely laminated, coarsely crystalline, contains fluid inclusions, has perfectly matched laminations on facing sides of fissures.
 - f> at tufa level, flow tube structure common.
 - g> triangular casts of bullrushes, etc.
 - h> the flat lying (tufa) portion of the spring deposit
 - 1> lies upon the surface
 - 2> is essentially pure calcite
 - 3> is uniformly pure calcite throughout the thickness of the tufa, the base of the tufa being an abrupt transition from tufa to the material upon which the tufa was deposited.
 - i> Conclusion: No cold-spring deposits with characteristics of the Trench 14 deposits
 - 4) Pedogenic calcretes -- many soils in region with such deposits
 - a> calcite
 - b> opal-A and opal-CT
 - c> smectite and chain-structure clays
 - d> Conclusion: The Trench 14 veins are similar to pedogenic calcretes.
 - 5) Clearly, the Trench 14 deposits have as their analogue the pedogenic calcretes of surrounding areas.
6. Biologic-derived content of Trench 14 carbonates vs. those of the spring deposits at Ash Meadows and Crater Flat (the entire following section is based upon a discussion with R. Forester)
- a) Present climate is atypical of climates of the region for the past 800,000 years, the typical climates having been much more pluvial to glacial in character than today (more water, cooler, greater and different biomass).
 - b) Potential regimes (to be remembered that, given an environment of appreciable biologic activity, the carbon in precipitated carbonates will be strongly isotopically altered by that biologic activity - too long a story for here):
 - 1) extreme dryness as today -- $\delta^{13}\text{C}$ values controlled by inorganic processes, so expected to be low (near 0)
 - 2) a bit wetter -- so some microbial activity, limited C3/C4 biomass, giving $\delta^{13}\text{C}$ values of -3 or thereabouts.
 - 3) increasing levels of wetness and cooler, but always with evaporation exceeding precipitation -- dominantly C4 biomass and $\delta^{13}\text{C}$ values of -6 to -9.
 - 4 wetness so great that precipitation exceeds evaporation -- no carbonate precipitation.
 - c) Relevant field data
 - 1) Trench 14 veins -- upper 11 feet of trench (only interval available at time of collecting samples by Forester):
 - a> the near vertical so-called vein deposits are in large part a mass of calcareous root casts, infilled with biogenic carbonate and opal C-T (demonstrated by careful disaggregation of samples in the laboratory).
 - b> the root cast structure is pervasive and is of "fractal" character, i. e., present at all size scales, including microscopic and SEM scales.

- c> Calcium oxalates have been found in these root casts (D. Vaniman, cited by R. Forester), such oxalates being unquestionable indicators of biologic processes.
 - d> Associated biota are extremely limited, consisting only of algae and diatoms associated with a damp soil environment. There are no ostracods, no mollusks, no aquatic plants.
- 2) Crater Flat spring palustrine deposits
- a> the outer edges of the deposits are dominantly root casts with little or no associated fresh water biota.
 - b> centrally, however, the calcareous mass is a detrital deposit (water-laid?) with many ostracod testes, these ostracod taxa indicating cool ($< 20^{\circ}\text{C}$) and shallow fresh water. [Ostracod taxa are sensitive to temperature, dissolved major ions (Ca, Mg, Na, K), as well as to sulphate, carbonate and chloride content of the water, thus providing a powerful means for establishing environment at the site of their growth and accumulation]
 - c> Other discussions in this document about the probable mode of origin of these deposits of Crater Flat reach similar conclusions to those based on the biota.
- 3) Ash Meadows (Devil's Hole)
- a> ostracods swimming happily in the standing water and are ultimately incorporated into the mass of precipitated carbonate.
- 4) Death Valley tufas or spring mounds
- a> high content of ostracods and other biota, indicative of a spring environment.
- 5) More biologic data can be provided for all sites discussed if desired.
- d) How root casts are formed in the soil (R. Forester, p. c.).
- 1) The soil water reaching the bounding root membrane contains many ions, both simple and complex, that are inimical or even lethal to the plant. The root membrane filters out these ions, permitting essentially only phosphorous and potassium to cross the membrane into the root system along with the water.
 - 2) In an evaporative environment, high concentrations of calcium develop around the roots because of systematic movement of soil solutions to the root membranes and the extraction of the water by the root system.
 - 3) High microbial activity in such soils leads to high $\text{P}(\text{CO}_2)$ values.
 - 4) The high $\text{P}(\text{CO}_2)$ values from microbial activity and the high Ca^{++} from the action of the membrane barrier lead to precipitation of calcium carbonate root casts.
 - 5) This is a common, widely observed and well understood process operating in evaporative soil environments.
- e) Conclusion -- There seems no doubt that the biologic data for Trench 14 and the spring deposits of this general area provide compelling arguments against a spring or hydrothermal origin for the Trench 14 carbonates.

Data from boreholes on and near Yucca Mountain - Part 2

1. Sr ratio ($\delta^{87}\text{Sr}/^{86}\text{Sr}$) as a function of depth in vein calcite, wallrock and water in Yucca Mountain (Peterman, et al, 1992, and Z. Peterman, p. c.)).

a) Data

Depth	Deposit or Aquifer	Ratios
		Wall-rock
Surficial - "pedogenic"	.71233 \pm 0.00028 [75]	---
Vadose zone (d \leq 400 m)	.71215 \pm 0.00034 [12]	.716
Vadose zone (48/84 m ab. WT)	.71098 [4]	.713
[Tertiary aquifer]	.7107 \pm 0.0004	
WT to 247 m below WT	NO VEINS ENCOUNTERED IN ANY HOLES	
250 to 500 m below WT	.7092 - .7098	.7096-.7098
500 to 1000 m below WT	.7088 - .7092	.7091-.7093
> 1000 m below WT	.7086 - .7089	.7089-.7095

b) Comments

- 1) There is a very marked separation of surficial and near-surface values from all deeper values, as well as marked disagreement between these shallow vein calcite values and those of the enclosing wall-rock. The agreement of the surficial and shallow vein values is consistent with a common source of strontium for both sets of deposits. Their disagreement with the value of the enclosing wall-rock assures that they were not formed in equilibrium with wall-rock chemistry and their generation probably had nothing to do with wall-rock chemistry.
- 2) It is interesting to note here that the single investigated calcite precipitate on the underside of a Paleozoic limestone cobble shows a Sr ratio value of .712x, even though the limestone cobble itself had a value of .707-.709, apparently establishing that the process of generation of the basal precipitates is one which incorporates non-cobble Sr. What other sources than blown-in dust?
- 3) The near-agreement between the values within 85 meters of the WT with the value in the Tertiary aquifer is suggestive of the origin of these calcites by deposition from the Tertiary aquifer, thus suggesting occasional rise of that water table by 250' or so. Such a rise is consistent with the explanation offered elsewhere in this document for the palustrine deposits near the mouth of Crater Flat.
- 4) The absence of detected calcite veins from the WT to 250 meters below it, (from the zeolitized Calico Hills unit) is suggestive of the absence of open fissures within this unit at any time. As suggested elsewhere, this may imply that this zone has been impermeable to upward vertical movement of water since its zeolitization, i. e., 10-11 Ma ago.
- 5) At greater depths, two clear relationships emerge
 - a> the Sr isotopic composition of vein and wall-rock is essentially identical at all depths, there being a slight decrease in both values with increasing depth.
 - b> the vein isotope values are lower than in the Tertiary aquifer.
- 6) The only samples of the Tertiary aquifer that have been analyzed by Peterman are from near the WT. Therefore, either the precipitation of vein calcites more than 250 meters below the present WT had nothing to do with waters of the present aquifer within those rocks, or the waters in the Tertiary volcanics are vertically zoned isotopically. If one accepts the latter as a possibility, one

must accept the fact of no vertical flow within the Tertiary section.

- 7) The Sr values in the two aquifers are indistinguishable.
- 8) A seemingly reasonable model that explains the Sr isotopic values in these deep veins is that they were formed in equilibrium with waters other than those now present in the rocks that enclose the veins. Such interaction of depositing vein and wall-rock probably requires (1) elevated temperature to increase reaction rates, as well as (2) a closed chemical system so that isotopic equilibrium is established between vein and wall-rock. Both of these conditions are consistent with the vein calcites more than 250 meters below the WT having been formed at the time of diagenesis of these rocks, i. e., 10-12 Ma ago. See G. 1. above.
- 9) It may be useful to point out here that at Devil's Hole, a modern cold-water spring and a spring whose depositional history for the past 600,000 years has been investigated, the Sr isotopic compositions of vein deposits over this time range (actually, 60,000 to 600,000 years ago) are identical to that in the present Paleozoic aquifer feeding the spring. These relationships indicate great stability of the sources and flow channels of this aquifer (totally reasonable in light of the tectonic history of the region sketched elsewhere in this discussion), as well as the expected reproduction within the derived vein carbonate of the Sr composition of the spring waters.
- 10) The Yucca Mountain Sr isotopic data are certainly inconsistent with the concept of repetitive flooding of the mountain from depth, as such a concept would require the deeply derived water to have the Sr isotopic composition of the present surficial deposits. The near constancy of the Sr ratio at Devil's Hole for the past 600,000 years (.7123 to .7128) demonstrates that whatever tectonic events occurred in the past 600,000 years were inadequate to appreciably alter the Sr ratio of waters flowing to the Devil's Hole outlet. The source waters for the Devil's Hole spring derive from the terrain immediately east of the terrain providing drainage under Yucca Mountain (Winograd and Thordarson, 1975). The constancy of one under whatever tectonic activity of the last 600,000 years occurred argues strongly for the constancy of the other.

2. Carbon and Oxygen Isotopic Ratios

a) Carbon in boreholes of Yucca Mountain.

- 1) Data are from wells USW G1, G2, G3, G4 and UE25 b#1 and p#1 (Whelan and Stuckless, 1992; Quade and Cerling, 1991)
- 2) If all their data are used in a simple interpretation, it would appear that the WT may have experienced upward excursion(s) of 500 meters and downward excursion(s) of 300 meters or so, this interpretation depending upon several heavy carbon values above the water table and several soil carbonate-like values down to 300 meters below the present WT (see Figure 1A).
- 3) However, it is pointed out by Whelan and Stuckless that all of the heavy carbon values above the WT come from vugs in the same vein samples that gave lighter carbon values. It is much easier to imagine that these values from vugs develop for special reasons, as is discussed by Whelan and Stuckless, than to assume vein and vug deposits to have derived from different aquifer fluids. The effect of removing the vug data from their Figure 5, using only data for veins and cement can be easily seen on Figure 1A of this document. On Figure 1A, all except one $\delta^{13}\text{C}$ values above the present WT narrowly surround the range of the surficial carbonates (-4 to -9 ‰, most at -7 ‰) and distinctly out of the range of values from > 500 meters below WT (-2 to +2 ‰).
- 4) The explanation of these two ranges is apparently straight forward.
 - a> The values from the deep cores are those expected of marine limestone, suggesting that these deep carbonates acquired their carbonate from

limestone aquifers, not from Tertiary hydrothermal sources.

- b> The shallow values are those expected of marine carbonate values modified by soil and air interactions with local plant-life. It is pointed out by Quade and Cerling (1990) that $\delta^{13}\text{C}$ values of -7 ‰ are expected if C4 vegetation was present around the site of Trench 14 during most of the period of precipitation of the soil carbonates. This would require a downward movement of about 750 meters of C4 vegetation relative to present sites of C4 vegetation in southwest Nevada. There is no problem with this lower topographic position of C4 vegetation during much of the Pleistocene.

Note that the fact that the $\delta^{13}\text{C}$ values of the Trench 14 soil calcretes are consistent with C4 vegetation at the site during the formation of the calcretes indicates that the local climate to correlate with their formation was wetter (though still arid or semi-arid) and probably windier than today. Thus estimates given below of the potential rates of modern-day calcium accumulation at the Trench 14 site via rain and dust are minimum estimates of the actual potential rates.

- c> The occurrence of soil-conditioned $\delta^{13}\text{C}$ values down to the present water table, with no admixed heavier values, indicates:
 - 1> The veins within the vadose zone did not acquire their carbonate from a limestone aquifer nor from any other upward-flowing aquifer fluid of deep origin (i. e., from below the Paleozoic aquifer).
 - 2> The veins within the vadose zone apparently acquired their carbonate by downward percolation of soil-modified carbonate.
 - 3> The water table under Trench 14 has been nearly unmovable (but not quite, see below) during the entire Pleistocene, this interpretation being in agreement with other evidence discussed within this document.
 - 4> The supposedly intuitively obvious argument that surficial waters cannot descend to a few hundred meters has no basis science or fact. Surficial waters can and do descend to the water table all over the world, the distance of the descent depending upon the distance to the water table.
 - 5> The set of soil-carbonate carbon values at a depth of 70 meters or so below the water table are from USW G4. From whence cometh such values at such a depth is not certain. A variety of explanations can be proposed, one of the simplest being transitory lowering of the WT.
- d> $\delta^{13}\text{C}$ values in the present Tertiary aquifer are -6 to -9 ‰, values in great disagreement with the $\delta^{13}\text{C}$ values of vein carbonates at the same depth. Therefore, the vein carbonates below the water table are not in equilibrium with the Tertiary aquifer.
- e> In this regard, it should be remembered (see elsewhere in this document) that only very small intervals of the Tertiary volcanic rocks in the boreholes yield significant water. Thus, most of the vein carbonates and cements may well not be (never have been ?) in significant contact with the "Tertiary aquifer".

b) Oxygen in boreholes of Yucca Mountain.

- 1) The source of the data used is as for the carbon data.
- 2) Discussion will be in terms of Figures 1B, i. e., data of Whelan and Stuckless with data of vugs removed.

- 3) The veins (and cements) of the vadose zone are interpreted by Whelan and Stuckless as derived from soil-conditioned carbonate precipitating at depth with a temperature gradient of 34° C per km and a surface temperature of about 13° C. They seem to suggest that this is probably the minimum gradient appropriate to the water-saturated zone also. This gradient is much higher than that typical of the Basin and Range Province (about 20° C per km.) and has been used in the Minority Report as support for a presently higher than normal gradient induced by deeply buried abnormally hot volcanic materials, attesting to the active danger of local volcanic activity under Yucca Mountain or at least to the upwelling via Szymanski's convective hypothesis of hot hydrothermal fluids. Thus, it is important to investigate by all data available the reality of the proposed value of the temperature gradient in and below Yucca Mountain.
- 4) Note first, that this is a gradient deemed appropriate to time of formation of the vadose zone carbonate veins, i. e., times other than the present moment (see a) 4) b> above) and a gradient appropriate to much of the last 500,000 years (age of lowest part of Trench 14 calcretes). So, even if the figure of 34° C/km is an accurate interpretation of the data, it is clear that it has never induced extraordinary events at Trench 14, and cannot be used as an indicator of a developing disaster.
- 5) The persistence of such a high gradient seems doubtful as the normal gradient in the Basin and Range area is only 20° C/km.
- 6) The possibility of having such a high gradient, at times of high rainfall with the associated high water table and active flow of meteoric-supplied water through both aquifers under Yucca Mountain, seems highly doubtful. Such aquifer flow would in all probability totally obscure the actual geothermal gradient and might yield an actual pattern of temperature with depth having nearly a step-wise character.
- 7) So what is one to make of the Whelan and Stuckless argument?
 - a> Note first that the arguments given earlier suggest that the calcite veins within the saturated zone (except possibly for a few tens of feet near the top of that zone) were formed at the time of metasomatic alteration of the volcanic rocks 10.5 Ma or more years ago. Whelan has indicated his acceptance of this interpretation. Therefore, it is illogical to try to interpret these deposits, formed millions of years ago under certainly a different temperature regime than applies to formation of the calcites of the present vadose zone, in a coherent pattern with the vadose zone carbonates which, by analyses given above (and totally supported by the carbon data of Whelan and Stuckless), have formed in the last .5 to 1 Ma by downward percolating fluids, fluids which may well have never in that time penetrated below the present water table (permeability barrier, see discussion of static head and water productivity vs. depth of Yucca Mountain boreholes). Thus, the overall suggested interpretation given by Whelan and Stuckless must almost certainly be wrong.
 - b> It is pertinent to note that
 - 1> $\delta^{18}\text{O}$ values in both the Tertiary aquifer and Paleozoic aquifers are -13 to -14 ‰, and that
 - 2> $\delta^{18}\text{O}$ values in modern rainwater are -13 to -14 ‰. (Figure 17-2 in Drever, 1988, "The Geochemistry of Natural Waters"), i. e., the same as in the two underground aquifers.

- 3> Isotopic partition is high and positive from water to precipitated carbonate ($\delta^{18}\text{O}$ of +23 ‰ @ 50° C, +28 ‰ @ 25°, +30 ‰ @ 15° and +32 ‰ @ 5°, thus giving the values of Figure 1B. Quade and Cerling use $\delta^{18}\text{O}$ values re PDB while Whelan and Stuckless use values re SMOW. The approximate conversion formula is

$$\delta^{18}\text{O}_{\text{SMOW}} = \delta^{18}\text{O}_{\text{PDB}} + 31)$$

- 4> All of these waters would give carbonate precipitates with the same $\delta^{18}\text{O}$ values at the same temperatures. Thus the oxygen values by themselves do not distinguish the source waters for the veins and cements in Yucca Mountain boreholes.
- c> Whelan and Stuckless consider that a satisfactory explanation of the details of the oxygen values in the deep cores is not yet available. The problem seems to be that these data do not support the concept of a high temperature gradient with depth. In fact, inspection of Figure 1B makes it clear that a very low gradient is at least as consistent with the data of the vadose zone as is a high gradient. Consider those data without reference to the data from below -200 meters. The G-4 data suggest a near-zero gradient to and just below the present water table, the G-3 data suggest 17° C/km or anything one pleases, while the G-2 data say nothing. The data of the different wells do not seem to come from the same population. If one considers the data from below -200 meters, they form such a roundish mass that one could have no confidence in any gradient calculated.
- d> To my mind, the 34° C/km interpretation is highly questionable. For illustration, I have added a line with 17° C/km gradient to Figure 1B. It seems as credible an interpretation of data of the vadose zone as does a 34° C gradient.
- e> Fortunately, there are other data, not investigated by Whelan and Stuckless, that have a bearing on estimation of the present temperature gradient (the gradient critical to the argument of the Minority Report) below Yucca Mountain. These are the water temperature values obtained when the Yucca Mountain wells were pumped for obtaining both temperature and chemical composition of the aquifers. All of the data I use are available in Kerrisk (1987), the pertinent data being reproduced here as Table 1. Note that intervals of various lengths ("ELEV RANGE" column of Table 1) were open for pumping.
- f> The statistical technique used was that which minimized the perpendicular distances of the data from the best-fitting straight line.
- g> I have analyzed these data in various ways.
- 1> First, in Figure 2, I use only the data for the eight intervals with lengths of 200 meters or less (it is known that only the top of the Paleozoic section produces water in UE-25p#1, so I considered the production from the Paleozoic aquifer in this well to be from an interval of less than 200 meters length). I have included a surface value of 20° C at an elevation of 1300 meters ASL as an additional point. Using only these nine data constitutes giving "infinite weight" to them. The gradients found when using only these data were 20° to 23° C per km., depending upon the exact set of data used. Such a

gradient is indistinguishable from the normal Basin and Range gradient. This is the steepest gradient found by any mode of analysis and I will argue that it is probably too high for both the vadose and volcanic sections and probably slightly high as an estimate of the mean gradient.

2> Figures 3, 4, and 5 use sub-sets of the total data while applying various relative weights to individual datum values depending upon the elevation range open to pumping. Intervals in 200 meter increments were weighted equally (i. e., all intervals of less than 200 meters always got a weighting factor of 1 if used, all intervals between 200 and 400 meters got the same value (the actual value depending upon the weighting function used), etc. Table 2 gives all of the relevant calculated quantities, as well as the data for a maximum weight of 25 (not included on the figures). Table 2 and the pertinent figure should be read simultaneously.

a: Figure 3 uses all data with various relative weights. At a maximum relative weight (ratio of relative weights of shortest and longest open intervals, MAXRWT of the figures) of 50 (all data other than the nine of weight 1 add only 0.7 to the total weight and intervals of 200-400 meters have a weight of 0.09, see Table 2), the calculated gradient is 20.4°C per km. One must effectively exclude all except the nine data of weight 1 in order to get a gradient as high as 21°C per km. To me, this seems unrealistic, so I conclude that the best estimate of the overall mean gradient is essentially 20°C per km.

b: Figure 4 uses all data from the Tertiary volcanics, as well as the surface value. At a MAXRWT of 50, the calculated gradient is 15.5°C per km. Only by effective exclusion of all except the eight points of weight 1 can a gradient as high as 20°C be calculated.

c: Figure 5, in addition to excluding the Paleozoic point, excludes the three shallowest data. The logic is as follows:

1: The surface point was added to the data set by me. Let us remove it for this analysis.

2: The two other points removed for this analysis were obtained from well UE-29a#2, i. e., the northerly well on Yucca Mountain with a water table at 1184 meters, rather than 730 meters as in the other wells used. Removal of these data allows an estimate of the gradient within the saturated volcanics for the wells with the 730 meter WT.

d: It is seen on Figure 5 that a MAXRWT of 50 gives a gradient of 6°C per km., while a MAXRWT of 200 (total weight of 5.2, 200/400 weight of 0.02, Table 2) predicts a gradient of only 12°C per km.

8) I conclude that a credible model for temperature as a function of depth under Yucca Mountain at the depths relevant to this investigation (depths at which temperature is controlled by that of the flowing aquifers rather than by a uniform static gradient) is a discontinuous temperature function:

a> The water temperature at the 730 meter WT is 33°-34° C

b> The aquifer temperature near the base of the Tertiary volcanics (these fluids are isolated from the Paleozoic aquifer by the permeability barrier discussed elsewhere) is 39°-43° C.

c> Thus, there is a significant jump in temperature between the Tertiary aquifer temperature at around sea level and the Paleozoic aquifer temperature at 200 meters or so below sea level.

d> Little can be said about the gradient in the vadose zone. The maximum possible gradient would be about $(33-20)/600$ or 21°C per km. I would suggest that the gradient may be less with a near-jump in temperature at the water table.

9) I conclude that the present temperatures of the Tertiary and Paleozoic aquifers deny the existence of an abnormally high temperature gradient under Yucca Mountain today, and thus of the hypothesis of the Minority Report relative to an abnormally high gradient associated with the existence of hot magma below Yucca Mountain and environs. The simplest interpretation (Figure 2 and a MAXRWT of 50) indicates a uniform gradient of 20°C per km. The more complicated interpretation suggested above arrives at the same temperature at depth but argues that the thick vadose zone in conjunction with active flow within the Tertiary aquifer gives a gradient within those rocks that is well below that of the normal regional gradient at depth.

c) Conclusions

- 1) The stable isotope data (C and O together) are consistent with derivation of vadose zone carbonates from downward percolating soil-conditioned surface waters. There is nothing in the data from the vadose zone requiring further explanation. Most particularly, there is no suggestion of incursion of warm carbonate-bearing solutions into the present vadose zone as there are no carbonates with $\delta^{18}\text{O}$ values comparable to those only 500 meters below the WT.
- 2) Might warm fluids have risen, cooling as they rose, thus giving the observed $\delta^{18}\text{O}$ values? In principle, yes. However, while ignoring the implications discussed elsewhere of slow rise of the water table under Yucca Mountain on the water table in surrounding areas, I simply point out that such solutions as they exist today in the Paleozoic aquifer would yield incorrect $\delta^{13}\text{C}$ values. Waters of the present Tertiary aquifer would if cooled to 15°C give $\delta^{13}\text{O}$ values very similar to those of the present soil carbonates. Of course, such a rise is not necessary for interpretation of the data and the Sr data discussed above deny this possibility.
- 3) For our purposes, it is sufficient to establish that the fluids that deposited the vein and cement carbonates below the water table cannot have been the fluids that deposited carbonates in the present vadose zone and at the surface, and that no fluids with their $\delta^{13}\text{C}$ values conditioned by long passage thru marine carbonate rocks could precipitate carbonates with the C and O compositions of the soil carbonates of Trench 14.
- 4) The temperatures of the Tertiary and Paleozoic aquifers seem to deny the existence of an abnormal temperature gradient under Yucca Mountain today, and thus of the hypothesis of the Minority Report relative to an abnormally high gradient associated with the possible existence of hot magma below Yucca Mountain and environs.

3. ^{238}U - ^{234}U - ^{230}Th Systematics (Muhs, et al., 1990; Stuckless, 1991)

a) Model:

- 1) Precipitate uranium from solution at time of formation of carbonate. No thorium is precipitated because it is insoluble and there is none in natural waters.
- 2) Assume closed system evolution of uranium and ^{230}Th , the behavior of analyzed data establishing whether the assumption is valid.
- 3) In closed systems, the initial values of the ^{234}U - ^{238}U and ^{230}Th - ^{234}U activities (whatever it was and zero, respectively) will both change to 1 with time following well-defined and calculatable paths.

- 4) It may appear both remarkable and puzzling that isotopic fractionation can easily occur in an element as heavy as uranium. The explanation seems to be as follows:

- a> ^{234}U is a short-lived (half-life = 2.48×10^5 years) daughter product of ^{238}U (half-life = 4.5×10^9 years). In a system that has been closed for an adequate length of time (few hundred thousand years), the $^{234}\text{U}/^{238}\text{U}$ nuclear activity ratio will become one, i. e., there will be one disintegration of ^{234}U for each disintegration of ^{238}U . In a volcanic melt, this ratio will be one, so that a volcanic rock upon solidification will have a ratio of one (the average uranium activity ratio of 30 young volcanic rock samples from two boreholes at Yellowstone Park is 1.023 (Sturchio et al., 1987)).
- b> As time passes in such a rock, the ratio stays at one. However, this is a dynamic process. Thus, many ^{234}U atoms ultimately reside at sites which were originally sites of ^{238}U . This nuclear transformation is associated with ejection of an alpha particle and recoil of the uranium atom within the crystal lattice, these processes resulting in weakening of the crystal lattice with consequent easier penetration of the lattice by any circulating waters. Since all ^{238}U atoms are in unweakened sites, there will be preferential solution of ^{234}U atoms, resulting in uranium activity ratios of greater than one in the solution. The details of the leaching process and the age and character of the rocks will lead to variable resultant values of the $^{234}\text{U}/^{238}\text{U}$ activity ratio in the solutions.
- c> Thus, the isotopic fractionation of uranium has nothing to do with the processes achieving isotopic fractionation in light elements such as hydrogen, carbon, oxygen and sulfur, but is a fortuitous result of the disintegration process.
- d> The uranium follows the calcium into the precipitated carbonates without any isotopic fractionation of the uranium.

- 5) So, sample several levels in the carbonates at Trench 14 (and Busted Butte), assuming such samples to be in all probability of differing age.
- 6) Calculate the $^{234}\text{U}/^{238}\text{U}$ and $^{230}\text{Th}/^{234}\text{U}$ nuclear activities of all samples and plot them on a figure with the two activity values as coordinates.
- 7) Compare the implied initial $^{234}\text{U}/^{238}\text{U}$ value with that of natural waters from various sources.

b) Data:

- 1) Most natural surface waters have $^{234}\text{U}/^{238}\text{U}$ activities of 1.00 to 2.00.

All soils and surficial sediments at Yucca Mountain have $^{234}\text{U}/^{238}\text{U}$ activities of 2.0 or less (most < 1.4).

The Paleozoic aquifer as sampled has a $^{234}\text{U}/^{238}\text{U}$ activity of: 2.6 - 2.8 at Ash Meadows; 2.7 at Yucca Mountain; 3.6 - 3.7 at Yucca Flat; 4.9 at Jackass Flats.

The Tertiary aquifer as sampled has a $^{234}\text{U}/^{238}\text{U}$ activity value of: 3.3 - 3.4 at Yucca Flat; > 5 at Yucca Mountain.

- 2) The data from the soil calcretes of Trench 14 and Busted Butte when plotted on a $^{234}\text{U}/^{238}\text{U}$ vs. $^{230}\text{Th}/^{234}\text{U}$ figure follow an evolution curve with an initial $^{234}\text{U}/^{238}\text{U}$ activity of 1.4 - 1.5, i. e., within the range of natural soil waters and definitely not approaching the $^{234}\text{U}/^{238}\text{U}$ values of either of the deep aquifers under Yucca Mountain (Figure 6). On Figure 6, I have included data from Devil's Hole (Stuckless), these data following a markedly different

evolution curve than those of Yucca Mountain and approaching an original $^{234}\text{U}/^{238}\text{U}$ value indistinguishable from that of the present aquifer waters at Devil's Hole (2.75).

- 3) The data from Yucca Mountain imply origin of the soil carbonates of Trench 14 from near-surface soil processes (i. e, rain water initially dissolved near-surface carbonate and uranium, transported it downward and precipitated it in the calcretes in a system that remained closed subsequently).

c) Conclusion:

Thus, as for other modes of evaluation, the U-Th evidence imply that neither of the deep aquifers has ever contributed uranium, and thus calcium, to the calcretes of Trench 14.

4. Pb

To my mind, available analyses of Pb isotope data (Zartman and Kwak, in press) from Trench 14 calcretes, etc. shows only that the data appear consistent with a surficial origin of the veins and sub-horizontal calcretes of Trench 14.

Since, as far as I know, there is nothing in these data that suggests interaction with deep sources of lead, I do not deem it important to discuss these data further.

5. Ca^{++} , Na^{+} , HCO_3^{-} , $\text{P}(\text{CO}_2)$, SiO_2 , etc. (Kerrisk, 1987)

A discussion of the compositions of the several samples taken from the Tertiary aquifer (and the single sample from the Paleozoic aquifer) in wells on and near Yucca Mountain is included with the intent of helping the reader to understand the potential of these aquifers for precipitating CaCO_3 if they were to reach the surface by some process of upwelling. The data used in the following discussion and figures is included as Table 3 and were obtained from Appendix A of Kerrisk, 1987.

- a) To begin with, Figures 7 and 8 show the overall percentage compositions of the cation and anion content of the aquifers sampled in the wells in the environs of Yucca Mountain. These figures show relative mmol/l per cent, the figures including data for all measured cations and anions. Each coordinate has a value of 100 at its corner and a value of zero at the opposite side, all points within the triangles defining a composition with components totalling 100. The figures illustrate the close relationships between the several samples of the Tertiary aquifers and their distinct difference from the Paleozoic aquifer. As is clear, these are dominantly sodium bicarbonate aquifers, this composition having great impact on the potential concentration of Ca^{++} in the Tertiary aquifers.
- b) Note on Figure 7 that the concentration of SiO_2 in the samples of the Tertiary aquifer is three times that of potential CaCO_3 (SiO_2 averages 0.824 mmol/l, while Ca^{++} (potentially CaCO_3) in these samples averages 0.249 mmol/l, see Table 3). A greater concentration of SiO_2 than CaCO_3 occurs in many aquifers transiting non-carbonate rocks, this being the case for the Tertiary aquifer waters of Yucca Mountain which have passed through valley fills and volcanic rocks. Though the Ca^{++} value in the Paleozoic aquifer is four times higher than the silica (2.495 mmol/l vs. 0.682 mmol/l, respectively), the silica concentration is still nearly that in the Tertiary aquifer. The reason for including this paragraph is to address the argument of the Minority Report proponents that "silica is not soluble in underground waters except at high temperatures." Of course, an elaborate discussion of silica, its types, and its solubilities and precipitation could be included, but I will leave that subject in the textbooks where it belongs.

c) A comment about the chemical compositions of the aquifers given by Kerrisk (1987) is required. For my present purposes, I have used averaged concentrations when Kerrisk presented multiple integral samples from the same wells and intervals. The data relative to concentrations are presented by Kerrisk as mmol/l to 5 or 6 significant figures. However, when one converts these values to ppm, it becomes clear that the original analytical data for all species except HCO_3^- were obtained in ppm at 2 significant figures, occasionally 1 (for illustration, calculated Na^+ values in ppm are 38.00, 42.00, 48.32*, 56.00, 54.00, 54.99, 44.00, 56.99, 50.99, 120.00, 72.99, 60.00, 86.37*, 79.50*, the three asterisked values coming from mmol/l values obtained by averaging in Kerrisk's table as noted above). All ion imbalances calculated from the values in the tables of Kerrisk are within the bounds explainable by the quality of the original data, even that for the Paleozoic aquifer where the calculated imbalance is +1.3 mmol/l with positive ion strength being +15.0 mmol/l (reported Na^+ and Ca^{++} ppm values were 150 and 100, respectively, for the sample). The average ion imbalance value calculated for the samples of the Tertiary aquifer is -0.126 mmol/l while the average calculated positive ion concentration is +3.29 mmol/l, the calculated imbalance being such a small fraction of the total of positive and negative ion concentrations that ion balance in these solutions can be assumed to be confirmed. The reason for this paragraph is that some have opined that these aquifers are out of ion balance with consequent aspects of uncertainty in normal modes of interpretation such as I will follow.

d) My intent is to investigate the potential behavior of these aquifers as regards the precipitation of CaCO_3 if they were to reach the surface via upwelling.

1) It is important in analysis of such waters to have HCO_3^- , $\text{P}(\text{CO}_2)$, temperature and PH values appropriate to the samples when at depth. The procedure followed in most if not all samples was to make measurements at the well head of HCO_3^- , CO_3^{--} , PH and temperature, thus achieving determination of $\text{P}(\text{CO}_2)$ by formulas in Drever, 1988. The lack of any evidence of effervescence in the Tertiary samples and the speed of determination hopefully imply the acquisition of accurate data.

2) A comment about use of $\text{P}(\text{CO}_2)$ as the X coordinate on several figures to follow (Figures 11, 14 and 15) may be useful. It is the convention to use this coordinate in such figures even though there is no gas phase associated with the sample localities. To quote from Drever, 1988, (p. 48): "It is convenient to adopt the convention that dissolved carbon dioxide is all H_2CO_3 , and to use equilibrium constants with this convention." One thus has

$$a(\text{H}_2\text{CO}_3) = K(\text{CO}_2) \times \text{P}(\text{CO}_2).$$

where "a" refers to chemical activity.

"Thus for every $\text{P}(\text{CO}_2)$ there is a corresponding $a(\text{H}_2\text{CO}_3)$ and for every $a(\text{H}_2\text{CO}_3)$ there is a corresponding $\text{P}(\text{CO}_2)$. In the literature, it is quite common to report $a(\text{H}_2\text{CO}_3)$ as the corresponding $\text{P}(\text{CO}_2)$ even when no gas phase is present." Figure 9 gives this relationship for values pertinent to the Tertiary aquifer samples, and one can consider the X axes on Figures 11, 14 and 15 as in $a(\text{H}_2\text{CO}_3)$ by a simple change in scale. It is obvious from Table 3 and Figure 10 that most of the carbonate ions in the Tertiary aquifers are HCO_3^- .

3) Several of the following figures display a statistical fit to the data. As there is uncertainty about exactly what each datum represents (probable mixing of

multiple source fluids with resultant somewhat randomized chemical relationships), it seemed correct to analyse the data as if there were effective uncertainty or error in both components. In such a situation, it is inappropriate to use least squares, this procedure assuming all error in the data to exist in one component, none in the other. A useful procedure when comparable error assumed to exist in both elements of the data is one which minimizes the sum of the perpendicular distances of the data from the best fitting relationship. I used a linear MPD formulation but applied it relative to the data or to the logarithm(s) of the data. Thus a statement on a figure such as "Stat: Linear MPD/Log. vs. Log" means that the logs of the data were calculated, the MPD fit was applied to these numbers, and the resultant relationship was drawn in whatever units seemed appropriate for the figure.

- 4) Figures 11, 12 and 13 show some of the significant relationships in the Tertiary aquifers, i. e., the expected linear correlation between PH and $\log(P(\text{CO}_2))$, the very low Ca^{++} values associated with high HCO_3^- values, and the low Ca^{++} values with high PH values.
- 5) I now follow the mode of analysis presented in Drever, generating figures similar to his Figures 4-6 and 4-7 combined as a single Figure (Figures 14 and 15 of this report). To make such figures, one must determine a term labeled K^* on those two figures. This quantity is defined as

$$K^* = K_1 \times K_{\text{cal}} \times K_{\text{CO}_2} / (K_2 \times \tau(\text{Ca}^{++}) \times \tau^2(\text{HCO}_3^-)),$$

all of these quantities being defined in Drever, the K values being equilibrium constants and the τ values being activity coefficients.

The four K values on the righthand side of the equation are obtained from Table 4.1 of page 49 of Drever, while the τ values are calculated from the Yucca Mountain data and the equations and table (Table 2.1) of pages 24 and 25 of Drever. I used K values for 25° C, calculated ionic strength (I) (page 24 of Drever), τCa^{++} , τHCO_3^- (equation (2-8), page 25 of Drever) and K^* for each Tertiary sample and averaged these fourteen values, the calculations yielding an average ionic strength value (mmol/l) of $.00377 \pm .00080$ and K^* value ((mmol/l)³/atm.) of 1.657 ± 0.330 .

Use of several specified M values (see definition on page 64 of Drever, and at top of Figures 14 and 15 where chemical symbol is concentration in mmol/l), the mean K^* value and the equation of page 64 of Drever

$$m_{\text{Ca}^{++}}(2m_{\text{Ca}^{++}} + M)^2 = P(\text{CO}_2) \times K^*$$

yields Figure 14 for the Tertiary aquifers.

Similar calculations for the single datum for the Paleozoic aquifer yielded ionic strength and K^* values of .02013 and 2.903, respectively, and Figure 15.

Thus, Figures 14 and 15 are for fixed ionic strengths of .00377 and .02013 and fixed K^* values of 1.657 and 2.903, respectively, for a range of values of Ca^{++} and $P(\text{CO}_2)$, and specified values of M (+.01, +.005, +.001, 0, -.001, -.005, and -.01) in mol/l. The drawn curves for specific M values express the conditions for saturation in Ca^{++} at the specified K^* value and temperature of the figure when the aquifer is in contact with solid CaCO_3 and total pressure is one atmosphere.

We can now plot the calculated values for each aquifer sample, plotting observed Ca^{++} versus observed $\text{P}(\text{CO}_2)$ as solid squares, and observed M versus $\text{P}(\text{CO}_2)$ as solid circles, the latter values expressing the M values consistent with Ca^{++} saturation in the presence of solid CaCO_3 at the calculated $\text{P}(\text{CO}_2)$ (see above) and ionic strength values. On Figure 14, it is seen that all except the aquifer sample with lowest Ca^{++} (the two plotted values for this sample overlies each other) have Ca^{++} values below that appropriate for saturation, i. e., all thirteen of these are undersaturated in Ca^{++} at 25°C when in contact with CaCO_3 at the indicated $\text{P}(\text{CO}_2)$ values. By reference to Figure 15, it can be seen that these aquifers are saturated at 35°C , i. e., at their underground condition. A regional rise of the water table would result in lowering of the aquifer temperature so I used 25°C on the figure. The undersaturation would be even greater if the aquifer was at a temperature of 15°C (see Figure 15). This is essentially the same result calculated by Kerrisk via somewhat different procedures.

Of course, when considering the calculated $\text{P}(\text{CO}_2)$ values as related to actual pressures forcing retention of H_2CO_3 in solution, it might be expected that $\text{P}(\text{CO}_2)$ (or $a(\text{H}_2\text{CO}_3)$) values would fall as these aquifers were raised to the surface. As a matter of fact, there was no evidence of effervescing as these samples were raised and analyzed.

Note the terms that enter the M calculation, i. e., only anions and cations that at low concentrations (as in these aquifers) are insensitive to changes in temperature, $\text{P}(\text{CO}_2)$ and PH . In the nomenclature of Drever, they are conservative. Therefore, if we hypothesize that there might ultimately be loss of CO_2 by degassing (leading to marked increases in PH (Figure 11)), the M values for each sample would not change (i. e., the solid circle for a sample would move to the left along a curve of fixed M value), while the solid square would move horizontally to the left at the same Ca^{++} value until precipitation occurred. If we assume that $\text{P}(\text{CO}_2)$ falls to that appropriate to water in contact with the atmosphere (the dashed vertical line at $10^{-3.5} \text{P}(\text{CO}_2)$), most samples would become "supersaturated" by a factor of 2 or 3. However, it is a common observation that CaCO_3 will not precipitate from such waters at when "supersaturation" as calculated via such figures reaches such values. Thus, precipitation from these aquifers, even in the presence of solid CaCO_3 , requires high evaporation in order to increase calcium concentrations to those adequate for precipitation. A cooling to 15°C (60°F) would eliminate this tendency to precipitate.

Thus a Tertiary aquifer rising to the surface in Yucca Mountain would not be expected to precipitate CaCO_3 at depth (no evaporation), nor would an actively flowing spring be expected to create a local concentration of precipitated carbonate (calculate the volume of evaporated Tertiary aquifer required to precipitate the so-called "vein" deposit of Trench 14 and you will see why an actively flowing spring on the top or side of a hill could not generate a significant carbonate deposit). What might well result in carbonate precipitation would be a rise of the water table that intersected the surface at a site of potential ponding of aquifer waters with associated high evaporation rates from the pond in a semi-arid to arid environment, i. e., the conditions suggested elsewhere in this report for the palustrine deposits scattered along the side of the Amargosa Desert between Mercury and Beatty.

Now, to consider the Paleozoic aquifer under Yucca Mountain. Figure 15, for this aquifer, is somewhat different from Figure 14. Though the basic equations are the same, all curves are for the same M value (that found from

the data from UE-25P#1) but for different temperatures (also for different K^* values of K^* as it is a function of temperature). At 50° C, the approximate temperature at depth, the data suggest the sample was over-saturated in Ca^{++} (the plotted data point is above the 50° curve). Whether this is correct or whether the data are in slight error is uncertain. What is certain is that, if this aquifer were released to the surface at 50° C, the $\text{P}(\text{CO}_2)$ would drop to something like .005 atmosphere and oversaturation values would reach 20 or so, guaranteeing deposition of CaCO_3 . In fact, when a sample from this aquifer was raised to the surface in a sealed bailer and subsequently opened, there was strong effervescence and the liquid poured from the bailer was turbid, strongly strongly precipitation of CaCO_3 . The suggestion in the Minority Report that the Trench 14 calcretes are formed by the above process is, of course, denied by the character of those calcretes, by the absence of surface tufas and by the character of the vein material sampled at depth within Yucca Mountain (no travertines encountered).

Figure 15 also can be used to understand what would happen if the Paleozoic aquifer were to rise slowly to the surface on a regional basis. Ignoring for the moment the potential for dilution by Tertiary aquifers on such a journey, Figure 15 shows that at the same $\text{P}(\text{CO}_2)$ but at 15° C, the aquifer would be strongly undersaturated. If it degassed to a $\text{P}(\text{CO}_2)$ value of approx. .005 atm., the supersaturation value would reach about 4, possibly implying some precipitation. However, data from Yucca Mountain indicate that such levels of supersaturation in CaCO_3 do not lead to precipitation at < 25-30° C.

If dilution by Tertiary aquifers is included in the journey of a Paleozoic aquifer to the surface, precipitation may be impossible. Thus there are numerous "Paleozoic aquifers" discharging at the surface in SW Nevada, none of which are precipitating carbonate. Chemical properties and temperature for several of these are included as Table 4, data for these being shown on Figures 7, 8 and 16. Collectively, these data suggest either that the Paleozoic springs are supplied by aquifers that are not as hot as that under Yucca Mountain or that they have been diluted by less concentrated aquifers, though it does appear that the diluting agent or agents did not have the composition of Yucca Mountain Tertiary aquifers. The Ca^{++} concentrations are one-half to about one-quarter that of the Paleozoic aquifer under Yucca Mountain (.274 mmol/l), while the issuing temperatures are 17° to 35° C. Both of these factors result in non-precipitating solutions. Figure 17, its parameters set by the two wells near Muddy Springs and the Big Muddy Spring, indicate this effect. If the temperature were raised to 30° C (actual issuing temperatures varying from 27° to 33.5° C), these solutions would be just saturated at a $\text{P}(\text{CO}_2)$ of a little over .01 atm., a value equivalent to that of several of the Yucca Mountain Tertiary aquifers. None of these Tertiary aquifers effervesced when brought to the surface and neither do the springs and wells at Muddy Springs. Even though this pressure is well above the usually quoted equilibrium $\text{P}(\text{CO}_2)$ when in contact with the atmosphere, the evidence is that a calculated $\text{P}(\text{CO}_2)$ pressure of 0.01 atm. does not lead to precipitation (lower pressure via degassing would lead to supersaturation). Assuming the Paleozoic aquifers sample aquifers from depths of several thousand feet, I suggest that the reason these springs do not precipitate CaCO_3 is that they have been diluted and cooled by mixing with shallower aquifers. Regional rise of the Paleozoic aquifer under Yucca Mountain would be associated with dilution and cooling by the shallower aquifers.

- 6) I suggest that regional rise of the present Paleozoic aquifer to the surface could not deposit the observed carbonate at the sites of the Trench 14 deposits. To

continue promulgation of the idea of aquifer-derived carbonates at Trench 14, one must, in addition to ignoring or somehow circumventing numerous arguments given above, propose a source of fluids with characteristics markedly different from any now present under Yucca Mountain, while hypothesizing that these different fluids did not in any way effect the Sr isotopic composition of the fluids issuing at Ash Meadows and Devil's Hole.

Evidence against Szymanski's convective model.

1. Springs at Ash Meadows

Within Szymanski's model, the line of springs at Ash Meadows is considered to be a site of present convective upflow. Devil's Hole is an element of this line of springs.

- a) The water table at Devil's Hole presently is 15 m below ground surface.
- b) The entire observed length of open vent at Devil's Hole (120± meters) is lined with vein calcite deposited at 30° - 40° C.
- c) Detailed uranium series dating of the vein calcite from a depth of 30 m below the present WT at Devil's Hole, with associated petrographic analysis, indicates continuous calcite deposition from 60,000 YBP to 600,000 YBP or earlier (I. Winograd et al., work in progress). For presently obscure reasons (slight change in chemistry of issuing aquifer fluids (?), see discussion of chemistry of Yucca Mountain aquifers), deposition of calcite at Devil's Hole apparently ceased about 60,000 YBP.
- d) Winograd and Thordarson (1975) cited evidence that the WT fluctuation at Devil's Hole has not exceeded 9 m. in the last 40,000 years.
- e) For at least the last 600,000 years, there have been no surficial tufa deposits at this site.
- f) So, there was continuous calcite deposition for > 500,000 years, while the WT never rose from its present location by as much as 15 m. and never fell by as much as 30 m.
- g) Thus, this fracture with optimum characteristics for detecting and recording stress changes (fracture oriented at right angles to the primary extension direction, thus being perfectly oriented to open and close under the changes in near-surface stress hypothesized by Szymanski) during the last 600,000 years, has as far as can be determined behaved in an unaltered mode for the entire time (I. Winograd, p. c.).
- h) Regional study of the Paleozoic aquifer of the area (Winograd and Thordarson, 1975) shows the line of springs in Ash Meadows to be supplied by that aquifer. Continuity of deposition of calcite as well as lack of movement of the water table suggest a nearly fixed flow and fixed aquifer source. This is certainly the simplest model that explains all observations.

- i) If rather than resorting to an hypothesis of stress change, one resorts to high heat flow and moving patterns of upward convective flow (an element of Szymanski's model), the stability of flow at Devil's Hole indicates that such hypothetical events certainly have not affected flow there beyond the limits discussed above, and thus not significantly effected the performance (i. e., water table) of the Paleozoic rocks supplying water to those springs.
- j) Though the Paleozoic aquifer under Yucca Mountain appears to be separated from that supplying the Ash Meadows springs, it is immediately adjacent to that aquifer and would be expected to have experienced a comparable pattern of stress change and WT stability (Winograd and Thordarson, 1975).

2. Springs in Spring Mountains

Szymanski's use of the springs high in the Spring Mountains as evidence of a convective cell under that range is denied by long available and published facts.

- a) These are springs developed from local perching of modern day meteoric water. See Winograd and Thordarson, 1975 (actually, the pertinent data were published in an OFR in 1963). Discharge rates of several high-yield springs vary seasonally by an order of magnitude, and summer water temperatures range from 6° to 21° C, varying inversely with altitude. Both of these characteristics are inconsistent with flow from depth, and are consistent with a meteoric source of the water.
- b) As regards the Paleozoic aquifer, inspection of Plate 1 of USGS PP 712-C (Winograd and Thordarson, 1975) shows that
 - 1) Six Mile Spring in Pahrump Valley (SW side of Spring Range) taps the Paleozoic aquifer at about 2600' ASL.
 - 2) Three wells at the northwest end of the range which reach the Paleozoic aquifer had static water levels when opened to this aquifer of 2361', 2370' and 2415' ASL.
 - 3) Two wells east of the two above and east of Cactus Springs which reach the Paleozoic aquifer had static water levels when opened to this aquifer of 2730' and 2742' ASL.
 - 4) The Spring Mountains rise to 12000' ASL with springs at nearly all elevations and major springs at 8000 - 9000' ASL, i. e., the static head in the Paleozoic aquifer surrounding the mountain is thousands of feet below that required for this aquifer to be the source supplying water to the springs.
 - 5) It is not a credible hypothesis to propose a convective cell so narrow that it supplies springs high in the mountains but is not present at the sites listed above.

c) The identical argument can be developed for the Tertiary aquifer.

- 3. Lack of spring deposits on the faulted west face of Yucca Mountain (see discussion under "per ascensum vs. per descumsum")
- 4. Character of actual near-surface deposits at Trench 14 deny any contribution to these deposits from water supplied from the Tertiary or Paleozoic aquifers.
- 5. Multiple isotope arguments (vein material, wall-rock and extant aquifers) already discussed above deny significant movement of the Yucca Mountain water table.
- 6. The discussion of the chemistry of the aquifer waters indicates that these waters would not deposit carbonate in the Trench 14 environment even if they did rise to the surface.

7. What about the suggestion that there could be something called a "Precambrian aquifer" that, under either heatflow or tectonic impulse, supplied a water mass that simulated in some characteristics the isotopic composition of the surficial carbonates at Trench 14?
- a) Though the Precambrian rocks are indeed saturated in many places, they are highly impermeable and nowhere is there a significant spring flowing from such rocks. Their behavior causes them to be described by Winograd and Thordarson, 1975, as the Lower Aquitard.
 - b) Underground in the area of interest, they actually act as barriers to water motion, not as avenues of water movement, thus behaving underground as they do in outcrop, i. e., as an aquitard (Winograd and Thordarson, 1975).
 - c) It seems unreasonable to me to assume that all of such deposits are behaving as aquitards today but, that for unspecified reasons, they would suddenly become avenues of high permeability, all such avenues being subsequently resealed.
 - 1) Szymanski's convective model is not based on a changing pattern of heat flow, but rather on an unstable pattern of convective cells triggered by a steady high heat flow from depth. There is absolutely nothing in such a model which could trigger the conversion of the rocks of the "Lower Aquitard" to a "Lower Aquifer". Szymanski would have to assume high heat flow simply as an aspect of deep and extensive fracturing rendering Precambrian, Paleozoic and Tertiary presently impermeable horizons permeable to vertical transport of water. Thus, accelerated heat flow is incidental and not fundamental in his model.
 - 2) Szymanski's tectonic model might conceptually introduce pervasive fracture porosity and permeability into these rocks. Of course, the trouble with this model is that all evidence re tectonic activity in the area is unequivocal in establishing the lack of any adequate tectonic activity over the last few million years.
8. The argument that spring deposits at and near the mouth of Crater Flat require some level of vertical flow driven by convection, is certainly false.
- a) The relevant spring deposits have no developed tufa mounds. They display un-mounded flat and thin (few feet) deposits of calcareous silt, i. e., these are palustrine deposits at sites where water outflow was never more than enough to develop small areas of swampy environment (E. Taylor, p. c., J. Quade, p. c.). The rate of accumulation of dust into these small areas was fast enough relative to rate of deposition of carbonate that the dust had a major impact on the accumulated deposit. In addition, the included biota establish these deposits as having been associated with cold springs (R. M. Forrester, p. c.).
 - b) Today, the WT at these sites is 250' below the surface.
 - c) Is it credible that in the normal course of glacial and pluvial climates and resultant increased rainfall that the aquifer in Crater Flat rose sufficiently to just overflow the ground surface at these sites?
- In this regard, it is pertinent to consider Winograd's discussion relative to the movement of the water table in Yucca Flat (Winograd and Thordarson, 1975). They suggest and marshal data and analysis to support the hypothesis of a higher water table at Yucca Flat during pluvial times, followed by continuous lowering during times of low rainfall such as the present.

Therefore, the hypothesis of a somewhat higher water table in Crater Flat during pluvials, followed by lowering during times of high aridity such as the present, is not an unreasonable explanation of the palustrine deposits at the south end of Crater Flat. Note that even during pluvials and glacial epochs, evaporation exceeded precipitation so that evaporative phenomena would still operate within the surface environment. This explanation of the Crater Flat palustrine deposits is consistent with the isotopic data from the veins of Yucca Mountain, which suggest an occasional rise of the WT of about 85 meters, and with the chemical data from the Tertiary aquifer (see above for discussions of both of these sets of data).

discussion of sites deemed by the Minority Report to be indicative of hot hydrothermal solutions.

Pages 35 through 44 of the Minority Report discuss several sites which are described as unequivocal evidence of the action of high temperature hydrothermal solutions. These sites are thus intended to provide data adequate to refute all of the previous arguments of this document. I, in the company of Zell Peterman, Richard Spengler and James Paces, have visited those sites and will now proceed to demonstrate the misinterpretations of these sites contained in the Minority Report. The relevant sites are named "Stop 106", "106F", "Red Cliff Gulch", Wailing Wall Fault", "WT-7", and "Harper's Valley", and are shown on Figure 18 (Figure 7 of the Minority Report). These will be discussed in an order which hopefully facilitates the reader's understanding.

1. Harper's Valley .

- a) To quote from the Minority Report (the numbering inside these quotes is mine and indicates the order of discussion of the quotes):

pages 40-41:

"....."Harper's Valley".....is characterized by the exposure of (3) numerous silica dikes and plugs intruded into formations with ages from just over 10 million years. The (4) abundance of these intrusives and the (1) strong deformation associated with them requires a very energetic mass transport source from depth. Because there is no isotopic age data available here, nor detailed mapping of which we are aware, (5) it can only be concluded that these features are younger than the rocks in which they were emplaced; that is younger than about 10 million years. However, there is no doubt that they were emplaced after the last recognized major volcanic activity in the area. (6) Thus, the possibility exists that they could have occurred during the early Quaternary when cones were active within Crater Flat, a few miles to the west of this site, or as recently as the last eruption at the Lathrop Wells Cone, a few miles to the south, which is estimated to have occurred only about 100 thousand years ago."

page 36:

(Though not in the paragraph discussing Harper Valley, this sentence is relevant to that site as the following statement is intended to interpret the red staining at all of these sites and there is much red staining at Harper Valley):(2) "Here staining is almost certainly associated with hydrothermal alteration from up-welling warm or hot water along the fault".

b) Comments

(1) and (2) The amount of deformation and the distribution of staining at Harper's Valley is extremely easy to see and understand. The terrain at this site is the right-hand (looking up valley) side-wall of the upper steep end of the small Harper's Valley. It is a comparatively steep side-wall, there thus being excellent exposures of bedrock.

I do not know what the phrase "strong deformation" is intended to mean in the context of this site. In normal geological parlance, such phrasing implies strong folding and/or complex faulting. Neither of these interpretations of the phrase apply to this site. The beds have a uniform gentle dip and the only apparent faulting is simple normal faulting that has duplicated small pieces of the section. Much of the exposed outcrop is unfaulted (the reddened section described below is one of these).

Much of the middle of the outcrop is composed of three acidic highly pumacious ash falls, one lying upon the other and each a few meters thick. All of these are well exposed and their contacts are not masked in any way. The base of each ashfall is its original bone-white color with numerous quite large pumice fragments, all being unaltered glass. The color of each of these ashfall deposits changes progressively upwards from white through pinks to reddish at the top, this red top being immediately overlain by the white base of the next succeeding ashfall. It is obvious that no hydrothermal process could have given the actual color pattern of these deposits. The ashfalls are physically indistinguishable (and, presumably chemically), there thus being no character within the deposits that would lead to selective coloration of the tops of each of the ashfalls. In addition, it is a widely observed phenomenon in other areas of the world that the tops of ashfalls are colored red by low to warm temperature processes operating within the recently fallen ash deposit. The on-site definitive proof that the reddening of these tuffs is not the result of a warm to hot hydrothermal process is that all pumice fragments within the reddened portions of the tuffs are today unaltered glass. For those who may not know what pumice it is, I note that it is the result of release of pressure on gas-laden highly acidic hot magma. Such material is highly viscous so that gas release is achieved by blowing the magma fragments into a glass froth, sizes of fragments being from dust to a centimeter or two in diameter, with the effective density of the froth fragments being less than that of water. This mass of comminuted glass and larger fragments (the "pumice" fragments) is blown into the air where it cools and falls upon the ground. The strands of glass within the pumice fragments have thicknesses measured in microns and are highly susceptible to alteration by warm to hot solutions. For example, the Calico Hills Tuff, where outcropping northwest of Yucca Mountain, has been hydrothermally altered throughout with all original glass having been eliminated.

In addition to elimination of the glass, hydrothermal solutions result in generation of a new and characteristic mineral assemblage. No such hydrothermally induced minerals are present in the ashfall deposits in Harper's Valley.

(3) and (4): These assertions about "numerous silica dikes and plugs" are very difficult to relate to what is observable in Harper's Valley.

There is extensive development of calcite deposits in fractures throughout the mass of the exposed ignimbrites, the calcite being on surfaces of all orientations (none of the ashfalls show this development). This is the relation between bedrock and calcite found for many miles around this area. It is certainly the complimentary deposit in bedrock to the calcretes in alluvial or other.

sedimentary and deep soil deposits. This point will be discussed in more detail when discussing WT-7.

The number of siliceous "dikes and plugs" is trivial, both in number and size, to the calcite deposits. I do not understand why the focus on the silica rather than the calcite. The silica veins I saw were of three types: (a) thin (1 cm or so) botryoidal sheets of opal in the same fractures as calcite, such opal layers being a low temperature phenomenon; (b) thin .5 cm veins of silica attached to surfaces of large loose blocks of ignimbrite, such coatings apparently having developed when these blocks were in place in the ignimbritic mass and of the same origin as the veins in (a); (c) a few (I saw less than 5) "dikes" 7 to 10 cm in thickness which where seen were vertical and cutting through the ignimbrites but not the ashfalls. These "dikes" are composed of fragments of the ignimbrites thoroughly and tightly cemented by silica, probably opal. Szymanski has asserted that these are the results of hot siliceous fluids rising forcefully and carrying somehow-created small breccia fragments upward. It is important to note that the edges of these "dikes" are clearly exposed and there is not even the suggestion of alteration of the wall-rock (fine-grained ignimbrites) where it is contact with the "dikes". In other words, the emplacement of these "dikes" was not a hot process. As regards the source of the "breccia" fragments, their character is consistent with their having been derived from above the "dike" locale. Whatever the detailed mode of origin of these "dikes", that origin was not a hot process and the rock fragments most probably came from above.

Outcrops of all of these silica deposits are quite unique for the area of Yucca Mountain. What is also unique is the outcrop of a thick deposit of highly acidic ashfalls, deposits which readily provide silica into solution. It is my conclusion that the silica veins and "dikes" (I saw nothing I would characterize as a "plug") are the product of silica derived from the ashfalls and transported downwards. Such a source is consistent with their being composed of low temperature silica (opal) and showing no alteration of wall-rocks. In this connection, a discussion in Drever (1988), p.197-203 ("Soil Solutions in Volcanic Ash") is pertinent. He presents data to show that soil waters in acidic pumice ashes (as at Harper's Valley) have SiO_2 concentrations of 60 to 120 ppm while having Ca^{+} concentrations of 10 ppm or less. Even in laboratory experiments, where the ash was placed in distilled water at soil water temperatures with an atmospheric $\text{P}(\text{CO}_2)$, SiO_2 concentrations of 100-120 ppm were reached in 100-140 days. If the tuffs were originally somewhat warmer, higher SiO_2 concentrations would have reached. Subsequent evaporation of such water in open fractures could generate the observed botryoidal veins of opal.

(5) and (6): What data support the conclusions so forcefully given in these sentences? The unnoted (by them) calcite deposits within the ignimbrite are certainly the product of surficial process and thus subsequent to development of present topography. The botryoidal opal veinlets may also be so derived, the reason being their occurrence in the same fractures as calcite. However, I know of no way to be certain about the silica "dikes". However, since it is demonstrable that all of these vein and "dike" deposits are the result of cold processes, the major "possibility" suggested in (6) is certainly false.

Another interesting bit of data is that a clear and well-displayed fault cuts the ignimbrite which lies upon the upper outcrop of ashfalls. This fault shows no evidence of mineralization. It seems to me that the post-volcanism forcefully rising hot hydrothermal solutions proposed in the Minority Report to explain non-fault related silica deposits lower on the ridge should have left some trace of their action on this fault surface.

2. Site WT-7

- a) This locale is the site of a well drilled by the USGS (WT-7) for determination of depth to the water table. The site is 50 feet or so up the side of a valley, requiring excavation of the hill slope in order to develop a flat site large enough for drill rig, etc. The back wall of the resultant cut provides the evidence used by Szymanski and friends to support the case for "aggressive" water rising from depth and depositing calcite veins throughout a mass supposedly fractured by this forcefully rising carbonate-bearing water.

b) Comments

The most obvious facts first. This site is bedrock, i. e., there is little or no soil development on the hard dense volcanic rocks of the area. Below the surface, and extending essentially to the surface, are a large set of calcite veins penetrating at various angles throughout the rocks, some nearly horizontal. These calcites extend so close to the surface (open cracks in which there is calcite at a slight depth extend to the surface) that it is inconceivable that hot carbonate solutions that had forced their way upward for several thousands of feet at least would not have gone the last few inches through open fractures and deposited large tufa spring deposits atop the bedrock. Such tufa deposits are dense tough calcite and would certainly still be present if ever generated. There is not a scrap of such a deposit. It may also be noted that all known tufa mounds of the area can be seen to be supplied by travertine veins. Such mounds are never associated with incoherent non-travertine-bearing fracture systems at the surface.

If upward moving solutions did not provide the calcite in these fractures, what did?

To answer this question, a bit of far-ranging data is required. All well-developed sheet-like soil calcretes are on detrital deposits of one type or the other (fans, valley floor sediments, thick soils, etc.), never on bedrock. This relationship can be seen along any valley or fan in southwest Nevada. The loose deposit may have a well developed calcrete deposit extending to contact with the adjacent bed-rock hill, while the bedrock hill seems at first glance to be devoid of any calcite deposit.

However, if one rips off the top of the bedrock (roadcut, drilling site, etc.) one always find the subsurface fractures filled with fine-grained non-travertine calcite. It doesn't matter what the bedrock is, just so long as it is fractured. Thus, it is clear that the fundamental processes leading to calcretes in loose materials are operating also in bedrock areas, again transporting carbonate obtained from the surface downward and precipitating it in available fractures. Though the Minority Report does not like the process of carbonate crystallization pressure being important in any soil or bedrock calcite deposits, it most certainly is and is instrumental in opening these near surface fractures beyond that of the original fractured mass.

A beautiful area to see this development is in the neighborhood of the tufa mounds described earlier west of Ute along I-15. Here are the tufa mounds (pure calcite with no detrital component) with their associated travertine feeders, a flanking and surrounding calcrete extending for miles into and along the valley (no travertine feeders, no extension to particular depth and largely composed of detrital material though sometimes giving the appearance at first glance of pure calcite) and terminating against the bedrock hill side of Paleozoic limestone. A road has been cut into the Paleozoic limestone, and the fractures in the limestone

are seen to be filled with calcite of a totally different texture and mode of deposition than the Paleozoic limestone. Bottom surfaces of cobbles or blocks can have a well-developed micro-stalactite development as noted earlier, sure evidence of a near-surface process.

This development of calcite-filled fractures in bedrock outcrops of the greater Las Vegas area is pervasive. No model of locally upwelling hot solutions can hope to explain the actual facts of occurrence of this type of relationship. The only reasonable explanation is also the obvious one, i. e., the calcretes in the loose materials and the calcite veins in the immediately adjacent bedrock are two aspects of the same depositional process.

Thus, the calcite veins at WT-7, veins that cannot be explained via the process proposed in the Minority Report, are simply an example of a surficial process whose operation can be found over hundreds of square miles in the general area. All data at the site are consistent with such an explanation, the same data denying the possibility of the process proposed in the Minority Report.

Finally, another characteristic of such shallow bedrock veins is that, where analyzed on Yucca Crest, they have insoluble fractions of 20 to 60 per cent, a characteristic always observed in pedogenic surficially-derived calcretes and never observed in hydrothermal limestone deposits. A sample to be discussed below, taken at Site 106 within the alluvial fan and deemed to be a hydrothermal limestone deposit by the Minority Report, had 70% insolubles (not soluble in an acetic acid leach adequate to extract all carbonates). In cases, such as at Trench 14, where the insoluble residue has been analyzed, it is anything from gravel to fine silt to clay in size and is fragments of local rock.

3. Wailing Wall Fault.

a) To quote from the Minority Report:

pages 39-40:

"Figure 14 (not included, JFE) shows a large fault scarp (the "Wailing Wall" fault) at the south end of Yucca Mountain just northeast of Stop 106. (2) This dramatic example of faulting is accompanied by calcite-silica cementing of the sand along the foot-wall of the fault (actually the hanging wall, JFE). (1) We infer that the development of slickensiding and polishing on a fault surface is evidence that, at the time these features developed, the fault surface was dry and was heated to high temperatures. However, it takes, at most, only a few seconds for the fault to move sufficiently for frictional heating to melt the rock and a correspondingly short time for the melt material to cool to form these thin features on the fault surface. (3) "Subsequent to the phase of polishing, ground-water, as the result of seismic pumping, is quite capable of reaching the surface along the erstwhile dry fault zone. Further, later upwelling associated with thermal convection moving upward along the fracture zone could occur. Indeed, holes dug in the sand adjacent to the fault line, as shown in Fig. 15 (not included, JFE) reveal that, close to the fault, the sand has been cemented by carbonates. (4) Other excavations in the downthrown block, at a short distance perpendicularly away from the trend of the fault trace, show that the sand is uncemented. Thus, only along the base of the scarp is the sand cemented. (5) "One can infer from the disposition of the cemented sands and also from the topography of the site, as shown in Figs. 14 and 15 (not included, JFE), that the most feasible source of water bearing the cementing material is that which may well up and be transported along the fault zone. (6) "While this fault could be relatively old, as is suggested by its limited exposure, it can serve as a conduit for up-welling water at times much later than

its origin; so the cementation in the sands along the footwall could be quite recent. Indeed, the fact that loose sands are cemented at the surface would certainly imply a young age. (7) In any case, whatever the age of the fault and the footwall cementing, in our view up-welling water along the fault is by far the most likely process and would indicate that mechanisms like those proposed by Szymanski may have been recently active and produced flows along available fault zone conduits." page 42:(8) "...cementation of the sand only at the footwall of the "Wailing Wall Fault" is very peculiar if a rain depositional process is all that is involved since more wide-spread cementation could hardly be avoided, yet there is no evidence of it."

b) Comments:

As is obvious to any reader, very few data are given in everything above. To aid in understanding this locality so that the few data presented by the authors can be put in context, a somewhat elaborate word description of the area is included.

Imagine yourself standing a short distance (300' or so) downstream from the fault facing the fault. What you would see on your left is an alluvial fan rising towards Yucca Mountain. The entire fan surface has a well developed calcrete upon and within it, this calcrete extending far up the fan beyond the small fault feature and well below it. Towards the right, this fan surface intersects a rounded distinctly higher bedrock ignimbritic hill that extends a quarter mile or more away from the intersection with the fan surface and parallels the fan/bedrock intersection towards Yucca Mountain. A modern actively eroding gully now separates the fan from the ignimbrite in the general region of the fault. However, upstream from the fault, there are numerous residual fragments of the calcrete clinging to the ignimbrite slope at the elevation of the alluvial fan to the left, attesting to the fact that the fan did at one time extend to the ignimbrite, gully in the fan having then been elsewhere. The fault location, about 100 feet long, is along the right hand side of this gully, modern sands in this active gully extending to the base of the fault.

In conformance with the processes described under the discussion of WT-7, the ignimbritic hill has no soil development upon it and is another of the numberless sites of bedrock outcrops showing no calcrete upon the surface but showing calcrete development in fractures just below the surface. Towards the top of the faulted face of the ignimbrite is a horizontal fracture filled with calcrete, i. e., the widely observed and expected relationship between calcrete and bedrock fractures in southern Nevada.

At the base of the fault, just inches above the modern sand of the gully, there are several inches of calcite-cemented gravel and cobbles. Is this deposit related to fluids rising along the fault? If one walks along the present gully, at sites where there is certainly no faulting but only the ignimbrite slope extending into the gully, one finds numerous patches of identical calcite-cemented gravel and cobbles. The presence of deposits identical to those at the base of the fault along the gully where there is no faulting certainly implies a mode of origin independent of the fault. In places, the entire bottom of the gully is a surface of calcite-cemented cobbles. Calcite cementation of gully and valley stream channels is a common phenomenon in arid terrains. Some rains occur, there is consequent stream flow, the water picking up some dissolved carbonate in its course into the stream bed. Downstream, in such places, flow rate decreases, the water sinks into the sand and gravel lining the floor of the gully and evaporates, leaving calcite coatings and fillings. Subsequent gully erosion can leave remnants

of such deposits along the sides of the gully. In addition to the similarity of the deposit at the base of the fault to several other deposits along the gully, it must be stressed that this small deposit looks like no tufa deposit I have ever seen. Thus, inspection of the gully and gully walls show clearly that the mode of origin of the deposit at the base of the fault is not as suggested in the Minority Report, but is a normal product of stream flow in arid terrains.

Note that the calcrete-covered alluvial surface just across the gully from the fault is higher in elevation than the base of the fault. Flow from that fault could not contribute to the calcrete. Of course, the authors of the Minority Report always propose hidden faults at higher elevation to explain calcretes at higher elevations than observed faults. They also either do not know about or fail to mention the many hundreds of square miles of similar deposits present on alluvial surfaces throughout the USA Southwest, deposits most certainly having nothing to do with faulting and having everything to do with surficial processes. See earlier portions of this report for a small fraction of the evidence proving that deposits such as the calcretes under discussion here are pervasive and are formed by well understood surficial processes.

With this background, brief comments on the numbered sections of the quotes given above will suffice:

- 1) This entire comment about slickensliding being associated with strong heating and melting of rock is both incorrect and irrelevant to the following discussion of the quote. For many years, Neville Price has pushed this concept of slickensliding implying melting, his opinion being opposed by all other geologists as well as by the facts of slickenslide occurrences and by the fact such surfaces are characterized by the absence of melted rock.
- 2) Already discussed. Their interpretation of this layer is incorrect.
- 3) These two sentences are astonishing. As one of the authors well knows, fault zones at focal depths are characterized by nearly lithostatic load pressure in the waters saturating such zones and by exceedingly low permeabilities. These facts are unequivocal. Fault zones are characterized by their great impermeability, not by their being easy avenues for movement of water. As this author knows, his own mathematical model of the earthquake process demands very narrow failure zones (measured in millimeters) and nearly lithostatic load pressure in the water, with very low permeability in the fault zone being required to keep the water heated by friction from escaping the fault zone. The water must not escape or the failure mechanism proposed cannot operate (heat the water in a narrow previously generated failure zone 4°C , thus increasing fluid pressure 80 bars or so and bringing fluid pressure to lithostatic load and totally releasing the failure surface, it being unequivocal that fault failure is a very low energy process and water pressure underground in nearly all places at focal depths of even shallow earthquakes is at or very near lithostatic load). The authors of the Minority Report seem to believe that what they see in a 100' by 15' outcrop of a fault face exposed at the surface characterizes that fault face at focal depths or at even a few thousand feet underground. Such a view is not supported by any facts I know.

Their appeals to seismic pumping and convection are totally unsupported by anything but their conjectures. I have discussed elsewhere their purported evidence for convection and shown them to be in error in their interpretation (the sites which they say prove active convection today nicely

demonstrate the lack of convection, while their dedication to the idea of an abnormally high temperature gradient under Yucca Mountain as a driving force for convection is shown to be inappropriate as the temperature gradient there is normal for the Basin and Range Province, approx 20° C per km).

Their appeal to seismic pumping is an appeal only. I know of no evidence to support the idea of a fault of the dimensions of that under discussion (even if extended in an unobserved mode to a length of a kilometer or so) being associated with seismic pumping. It is unequivocal that the earthquake process is dominantly a stress relaxation process, not a stress concentration process. Thus, in most cases there will be no driving force for seismic pumping. In most cases, the increased water flow after earthquakes is the result of stress relaxation and the resultant opening of fractures with resultant increased facility of drainage of underground water to adjacent stream valleys (it is very shallow meteoric water). Archambeau has one example of a large California earthquake (rupture length of several tens of kilometers) that may have displayed seismic pumping. Fine. But to extend that observation into a generality both as regards frequency of occurrence (a general phenomenon), and size of earthquake that may display the phenomenon (even small ones) is to deny available observations. I pointed out earlier that the size (physical dimensions and regional stress change) of a Nevada earthquake of the same magnitude as the California earthquake which may have displayed seismic pumping is several times smaller than the size of that California earthquake, thus putting the Nevada earthquake in a size range where no data of which I know indicate there to be changes in water flow in local streams at the time of the earthquake related to any process. If the authors wish to raise the potential threat of seismic pumping along such a trivial feature as the "Wailing Wall" Fault, they must give a discussion of seismic pumping that would pass peer review in a scientific journal. They do not do that in this document.

- 4) These are the calcite-cemented gravel and cobbles discussed above, i. e., the product of normal and widely observed stream processes in arid and semi-arid terrains.
- 5) Here, the authors are comparing the calcite-cemented gravel and cobbles of a now partially eroded former stream bottom with sands now actively moving down the gully. Since they do not understand the origin of what they see, they arrive at a totally incorrect interpretation.
- 6) As far as I am concerned, one can interpret no such thing. The calcrete mantled fan flanking the fault exposure is at a higher elevation than the base of the fault. Thus, in the region of the fault, the calcretes clearly did not derive from waters issuing from the fault. In addition, this calcrete extends far up the fan and has the same physical characteristics as that mantling hundreds of square miles of fan and valley surfaces in immediately surrounding areas where there is no possibility of subterranean source for the fluids. The only (not "most feasible") source of water that can explain what they see in the bottom of the gully as it passes the fault is rain water.

Of course, they appeal throughout their Report to unexposed faults or fault extensions to explain calcrete deposits at elevations above that possible from exposed minor faults, again "explaining" deposits which are certainly derived by pedogenic processes and not by their proposed mechanism.

7) Conjecture upon conjecture and continued misinterpretation. I must stress that these authors are basing their entire interpretation of geologic processes at this site and throughout the associated alluvial fan on two scraps of misunderstood data while totally ignoring the great amount of data available within 200' of the site which deny their conclusions and provide the bases for understanding what they see at the site.

8) Pure conjecture and point of view, all of which is denied by available data.

9) See the beginning of this discussion.

4. "106", "106F", and "Red Cliff Gulch"

a) Selected quotes from the Minority Report (I am selecting primarily quotes which purport to relate facts of observation, their conjectures and geologic generalizations being largely ignored until my comments):

page 35:

"The calcrete material at "Stop 106" was dated at 78 Ka and is very thick, with about two to three meters of its thickness exposed by erosion at some points along the wash which extends south from the fault."

page 36:

"Brecciated material and vein development (are) exposed at the south end of Yucca Mountain....These veinsat Yucca Mountain are often injected to form extension features or may be associated with faults. The close relationship of staining and faulting at Yucca Mountain is indicatedHere staining is almost certainly associated with hydrothermal alteration from up-welling warm or hot water along the fault.

"In both of the sites, extensive calcretes are exposed in gullies down-slope from the faults..... While the fault scarp in Figure (9) (not included here, JFE) is only exposed locally over about a 30 foot extent, with a steep walled gully extending downslope from it, the fault scarp in Figure (10) (not included here, JFE) is exposed over a considerable distance along the side of a canyon, with numerous small gullies downslope and extending to the bottom of the canyon. The red staining of the tuff in Figure 10 is also present in the tuffs a few feet down-slope and are exposed in the gullies below, with the color shading from red to orange-yellow."

page 37:

"The exposed breccias at the canyon base appear to us to indicate very energetic flows, probably involving CO₂ gas along with hot water. The breccia veins, along with considerable amounts of calcrete, are well exposed along about the half mile extent of the canyon floor indicating a large volume of flow."

b) Comments:

I think these quotes give the full flavor of their text. It is as follows: All reddening of tuffs is the result of hot hydrothermal solutions, all reddening is associated with faulting, calcretes thicken downstream from faults, and large volumes of fluids issuing along these faults, seen or unseen, deposited all of the massive amounts of calcrete observable in gully walls and on fan surfaces. The 78 Ka age proves all calcretes are very young and that rain-and-dust processes could not have formed such thick deposits in so short a time.

- 1) First, what about the brecciation of bedrock tough volcanic rocks by forceful injection of fluids from below?

It should be obvious to a reader what this is all about. Again, we are dealing with calcrete deposits in bedrock as at WT-7, in the area west of Ute discussed under WT-7 above, Harper Valley, Wailing Wall Fault and thousands of other outcrops, roadcuts, etc. Nothing in the rocks in the area under discussion indicate hydrothermal solutions. The "veins" of calcrete are at nearly any orientation, totally unlike in physical form and fabric from anything ever seen in a hydrothermal spring deposit. There are no travertines, there are no tufa deposits, all of these calcretes in bedrock being subsurface and having texture and appearance similar to conventional calcretes rather than tufas. The under-surfaces of some blocks show mini-stalactites which are not a feature of tufa deposits but are seen in numberless places on surface cobbles and near-surface fractures over many square miles in southwest Nevada. There is not a single mound of tufa-like hard limestone lying upon the surface, this fact not barring the authors from proposing forceful intrusion of hot hydrothermal solutions to within inches or less of the surface, along fractures that are wide open to the surface, without eruption of hydrothermal solutions and their resultant tufas onto the surface. Their explanation of these deposits is not credible. There is nothing about these calcretes which suggest hydrothermal origin. The aspect of such deposits that always overawes these authors (and which they invariably call "brecciation" with its concomitant ideas of forceful water) is the width of the calcrete veins with the consequent separation of once adjacent blocks (no rotation, no real brecciation, just separation of once contiguous blocks fractured by normal near-surface processes). How they imagine that forceful waters drove blocks apart without forming travertines while failing to reach the surface a very few inches away along fractures that they must assume were also opened by that forceful water is beyond my comprehension. They refuse to accept the obvious (obvious after numerous field trips to comparable sites) that the opening of such veins must be the result of a surficially operating process. When, in conversation, it is indicated that data from the field and laboratory indicate crystallization pressure of calcite to be an operable and adequate mechanism, they assert with vehemance that it is obvious to any sane mind that such a process cannot operate, much less explain field relationships anywhere.

- 2) Second, does the reddening of tuffs have a one-to-one link with exposed faults and is such reddening the product of hot hydrothermal solutions rising along faults and altering the volcanic rocks?.

Those who have read the discussion of Harper's Valley will not be surprised to learn that all of the reddened beds at Site 106 and environs are ashfalls and even the reddest contains undivitrified pumice fragments and shards. At the small fault crossing Red Cliff Gulch, only the reddened top of the ashfall can be seen. However, at the long outcrop in the gully at the side of the wash or fan, the complete thickness of an ashfall (Rainier Mesa Member of Timber Mountain Tuff sequence) is displayed. As at Harper's Valley, the base is coarse and white. Upwards, colors gradually change through oranges and pinks to red. Everywhere, pumice fragments are still undivitrified glass. In this thick ashfall, the matrix remains reddish to the top of the ashfall, but the large pumice fragments at the top are bone-white. Everything about this ashfall declares that it has not been effected by warm or hot hydrothermal solutions. The reddening of these ashfalls happened at or shortly after their time of deposition and has nothing whatever to do with much later minor faulting and hypothetical hydrothermal solutions. As far as

I could make out, there is no relationship between the distribution of reddening in this ashfall and faulting of any dimension.

The four of us on our excursion searched carefully for evidences of hydrothermal alteration of the volcanic rocks in contact with the ashfalls. Though the authors of the Minority Report declare there to be such alteration, we saw none.

- 3) Third, is there downstream thickening of calcretes below faults, the thickening being induced by extruding hydrothermal fluids?

Concisely, no.

As one leaves the dirt road and starts walking across the alluvial fan or up the gullies towards these sites, one immediately sees that the deeper the gully the thicker the exposed calcrete. These deposits are well up gully walls. It is not credible that fluids flowing down the gullies deposited these deposits nor is it credible that waters emanating far up the fan from a supposed faultline would follow a near horizontal flow course down the fan surface depositing surface-parallel layers of carbonate below the surface. Where the gullies are deepest and the exposed calcretes thickest, there is no evidence whatever of faulting.

At the Red Cliff site (a site where the total exposed fan thickness is much less than near the road), a face of pumice ashfall tuff a few feet high crosses the gully and is quite certainly a fault face. The upstream block was raised relative to the downstream block, thus raising bedrock and resulting in the accumulated fan deposit being several feet thinner upstream of the fault (bringing fan thickness down to a very few feet) and the modern downcutting gully being deeper below the faultline than above it as it has had a much lower base level. What the authors of the Minority Report report as thickening resulting from hydrothermal fluids exiting at the fault is simply the result of normal deposition of pedogenic carbonate in fan material of varying thickness, associated with modern erosion of the gully.

- 4) Fourth, is there evidence that proves the Site 106 calcretes to have an age of 78 Ka, and is this a totally incredible age for these calcretes if their formation is presumed to be by pedogenic processes, such an age thus implying a hydrothermal source for the calcretes?

In 1981, B. Szabo et al. of the U.S.G.S. published a uranium series age for a calcrete sample from this area of 78 Ka (Szabo, et al., 1981). This date is used by the authors of the Minority Report as compelling evidence of the impossibility of pedogenic origin of the observed calcrete deposits (too short a time for such thick calcretes). Therefore, they resort to an hypothesis of numerous unseen and unknown faults issuing unknown but certainly vast amounts of hydrothermal fluids over a short time period (there is certainly no hydrothermal activity today or in the recent past and none older than 78 Ka within their interpretation), all of this without a scrap of carbonate tufa typical of world-wide spring deposits and without a bit of quantitative analysis of feasibility.

Arguments that refute such an interpretation:

i> hydrothermal waters are concomitant with super-normal temperature gradients, often and as proposed in the Minority Report related to volcanism or buried hot intrusive volcanic rocks. It has been shown earlier that the temperature gradient at Yucca Mountain today is only that characteristic of the Basin and Range Province. There are no available data that support the case for high temperature gradients under Yucca Mountain in the last few million years.

ii> hydrothermal waters (the type of source fluid proposed in the Minority Report) are meteoric water (i. e., rainwater) that has reached hot rocks at depth and been put into convective motion.

It should be noted, that though most of this text follows the Minority Report's usage of the term "hydrothermal" fluids, their usage is not that used in the geologic literature. In that literature, "hydrothermal" fluids are ore-bearing or ore-depositing solutions, while the much cooler fluids issuing at the surface are called "hot" or "thermal" springs.

True hydrothermal fluids are hotter than any temperatures measured in Yucca Mountain boreholes and their chemistry is dominantly chlorides of sodium, potassium and calcium, with total concentrations of these ions at several hundred thousand parts per million, low concentrations of other anions and low but significant concentrations of numerous metallic ions (data in this paragraph, unless otherwise noted) are from Skinner and Barton, 1973). Occasionally, such fluids reach the surface at temperatures of 80° C or so, though the character of these solutions generally prevents their reaching the surface because of having to pass through the normal near-surface zone of meteoric water. Normally, as these hot brines rise, they react to equilibrium with the wall-rocks through which they pass. If these hydrothermal fluids rise through rocks such as limestones, they will react strongly and will cool rapidly when mixing with the unavoidable meteoric waters.

Most hydrothermal ore deposits are deposited at depths of a very few kilometers, say 2 to 3, with deposition being the result of boiling. The residual cool solutions of changed chemistry (the carbonate-precipitating hot springs at Yellowstone Park - rising through rhyolitic volcanic rocks, rocks not markedly different from those under Yucca Mountain - have compositions close to 51 ppm of SiO₂, 117 ppm of Na⁺, 55 ppm of K⁺, 72 ppm of Mg⁺⁺, 351 ppm of Ca⁺⁺, <.01 ppm of Fe, 412 ppm of HCO₃⁻, 744 ppm of SO₄⁻, 153 ppm of Cl⁻, and are at temperatures of about 70° C (Sturchio, 1992)) that reach the surface have little or no ore-making potential.

It seems clear that if the calcretes of Site 106 are the product of true hydrothermal fluids degraded to thermal springs by reactions and mixing at depth, there should be associated easily detectable reaction products within both the present vadose and saturated zones under Yucca Mountain. No such products have been detected, thus providing additional compelling evidence against the Site 106 calcretes having formed from hot upward-moving solutions.

iii> In addition, the observed $^{234}\text{U}/^{238}\text{U}$ activity ratio of Szabo's samples was 1.26, a value consistent with these deposits having acquired their uranium and calcium from natural soil waters ($^{234}\text{U}/^{238}\text{U}$ activity ratio of 1 to 2, see earlier section), while being inconsistent with these deposits having acquired their uranium and calcium from underground aquifers (determined ^{234}U - ^{238}U activities for the Tertiary aquifers in and around Yucca Mountain range from 3.3 to > 5 , while those for the Paleozoic aquifer range from 2.6 to 4.9, see earlier section). If there can be anything termed a "Precambrian aquifer" as hypothesized by the Minority Report, it certainly would not have a uranium activity ratio of 1 to 2.

However, the authors might be prone to say that their warm (certainly not hot, see above) "hydrothermal" solutions, rising through several thousand feet of Tertiary volcanic rocks, would strongly leach these rocks, penetrating well into the dense fabric of the rocks and thus acquiring uranium with a $^{234}\text{U}/^{238}\text{U}$ activity close to 1.

The problem with this hypothesis is that the data from Yellowstone do not support it. Sturchio et al. (1989) and Sturchio (1991) provide data on $^{234}\text{U}/^{238}\text{U}$ activity ratio for both wall-rock and fluids associated with Springs Y-7 and Y-8 at Yellowstone. The mean of 30 determinations from cores is 1.023 (0.91 to 1.15), while the mean of 7 fluid samples is 1.96 (1.52 to 2.46). These data indicate the limited leaching capability of such thermal springs. As discussed earlier, the $^{234}\text{U}/^{238}\text{U}$ activity ratio in a closed system should, after being closed for a few hundred thousand years, be 1 (i. e., one ^{234}U disintegration for each ^{238}U disintegration, ^{234}U being a daughter product of ^{238}U and having a half-life $1/18,000$ that of ^{238}U). When these volcanic rocks solidified, the uranium activity ratio was essentially one and has not changed since. However, many of the original ^{234}U atoms have disintegrated while they have been replaced by new ones resulting from disintegration of ^{238}U atoms. The new ^{234}U atoms are in sites damaged by the disintegration process (a widely observed phenomenon), sites from which uranium can be more easily leached than from original sites of either ^{234}U or ^{238}U .

Therefore, even if warm solutions had risen through the volcanic rocks of Yucca Mountain, they would have carried uranium activities greater than soil values at Yucca Mountain (approx. 1.5).

iv> Another important datum is that the sample dated by Szabo had insolubles (insoluble under an acid treatment designed to dissolve the carbonate component and little else) totalling 70 per cent by weight of the sample! The deposits formed by the Yellowstone springs are at least 98% chemical precipitate with generally $< 1.5\%$ detrital component (Sturchio, 1992). As described at the beginning of this document, carbonate deposits of thermal springs are always characterized by very low levels of insolubles, while massive dense pedogenic calcretes have large insoluble components, this component having the composition of surficial materials at the site.

v> All of the above suggests that something is wrong. The age of 78 Ka is certainly inconsistent with the thickness of the calcretes in some gullies (much greater than a meter) while the calcretes cannot have been formed by hydrothermal solutions from depth.

I note first that Szabo, et al. did not imply that the age of 78 Ka obtained on a "seep-deposited tufa or calcrete intercalated in Q2 alluvium" (p. 20, Table 3) dated the thick calcretes of the fan. In fact, they said (ibid.): "(The age) gives approximate minimum (emphasis added, JFE) age of Q2 alluvium." Thus, the interpretation given to this age in the Minority Report is incorrect.

The U.S.G.S. is now determining uranium-series ages for several samples from calcretes of the Site 106 area. Initial results suggest minimum ages of about 250 Ka for the base of the upper meter of calcrete as sampled at a locality of thick calcrete just north of the road. Assuming that calcite forms only 30% by weight of these recently analyzed samples as in Szabo's sample and as found at Trench 14 (see earlier), and assuming that the density of the calcrete mass is about 2.5 gm/cm³, the rate of accumulation of carbonate at this site would be (30 x 2.5 / 250) or 0.30 gm/cm²/1000 years, 75 gm/cm²/250 Ka, or 135 gm/cm²/450 Ka. This is 25% greater than the rate observed at Trench 14, and about one-eighth the rate of carbonate accumulation via dust at Carrera. Lying as this site does in a topographically low position, the surprise is that the calculated rate of accumulation of carbonate is not more than 1¼ times that at Trench 14.

5. Conclusion: I conclude that all arguments presented in the Minority Report which purport to prove the operation of hydrothermal processes at the several sites discussed above do not withstand serious scrutiny.

Remaining problems

1. Source of dust

- a) The problem is probably only apparent, resulting from lack of completion of studies now underway.
- b) Though the Sr data in surficial deposits demonstrate that the Sr did not derive from deep aquifers (see above), no firmly documented source for that Sr has been established. However, (Z. Peterman, p. c.):
 - 1) The ⁸⁷Sr/⁸⁶Sr values of the pedogenic carbonates of Trench 14 are 0.712x.
 - 2) The large outcrop areas of eroding Paleozoic limestone have values of 0.7078 to 0.7094 in the Spring Mountains and .7107 to .7119 at Black Marble Hill, thus implying that some other source(s) with higher ⁸⁷Sr/⁸⁶Sr values must be found.
 - 3) The Precambrian rocks have Sr isotope ratio values of 0.713 or higher.
 - 4) Several of the Tertiary volcanics outcropping in the area of Yucca Mountain have high Sr isotope ratios. Values measured are as follows:

Formation	$^{87}\text{Sr}/^{86}\text{Sr}$
Topopah Springs (mean of 12 samples)	0.71603
Calico Hills (mean of 4 samples)	0.71328
Prow Pass (mean of 2 samples)	0.71172
Main Rhyolite Yucca Crest Caprock (5)	0.71348

5) To date, a program of determining $^{87}\text{Sr}/^{86}\text{Sr}$ values in the soluble fractions of playa clay samples has yielded the following 5 values:

Site	$^{87}\text{Sr}/^{86}\text{Sr}$
Stewart Valley	0.71100
Bonnie Claire	0.71024
Sarcobatus Flat	0.71022
Mesquite Flat	0.71472
Alkali Flat	0.71283

- c) Thus, one need only imagine mixing of dust of Paleozoic limestone, Tertiary volcanics and/or playa dust, with maybe some Precambrian dust thrown in for good measure, to explain the observed $^{87}\text{Sr}/^{86}\text{Sr}$ ratio in the pedogenic limestones of Trench 14.
- d) It has been suggested that calcretes on limestone fan deposits are not evidence of atmospheric processes of carbonate accumulation, the limestone being the probable source of the redeposited carbonate. To demonstrate the fact that calcretes associated with Paleozoic limestone have higher $^{87}\text{Sr}/^{86}\text{Sr}$ values than the associated limestone, thus attesting to introduction of high isotopic ratio Sr by atmospheric processes, consider the following data (paired $^{87}\text{Sr}/^{86}\text{Sr}$ values from limestone and attached calcrete):

Formation	$^{87}\text{Sr}/^{86}\text{Sr}$	
	Limestone	Calcrete
Roberts Mountain Fm.	0.70982	0.71122
Roberts Mountain Fm.	0.70883	0.71093
Nevada Fm., Striped Hills	0.70887	0.71229
Goodwin L/S, Striped Hills	0.70930	0.71244
Bonanza King Fm., Devils Hole	0.70981	0.71222

Thus, it appears that the formation of these attached calcretes is not a simple process of solution and reprecipitation of the Paleozoic limestone but does involve introduction of dissolved carbonate from rainwater and/or soluble carbonate from dust.

Note that, in order to achieve such marked increases in ratio from the materials apparently available (see b) 4) and 5) above), the dominant component in the calcrete must be the added component, not the Paleozoic limestone.

2. High water table in Yucca Mountain north of proposed repository.

I can contribute nothing to this problem at this time. Hopefully, the drilling campaign now underway will resolve the problem.

Overall conclusions as regards credibility of the Minority Report

Anyone who has labored through these many pages already knows my conclusions. I find little that I can accept in the interpretations of extant data given in the Minority Report and even less in their proposed model of past or potential future events.

The Minority Report asserts that several lines of evidence refute the idea of a pedogenic source for the Trench 14 calcretes. I have shown that, in every case, they are in error in their interpretations and conclusions. Their errors appear to arise out of an inadequate background in the requisite geologic disciplines.

Their appeals to such exotic processes as seismic pumping associated with repetitive major earthquakes along the faulted west side of Yucca Mountain and convection of hot ground water induced by abnormal rates of heatflow under Yucca Mountain and environs are appeals unsupported by data, in fact denied by data.

Contrary to their assertion, their model is not a model at all, simply a set of unsupported and unsupportable assertions. Those who have read this document or have kept abreast of the literature dealing with these issues know that available data are internally consistent with the simple model implied by the data and analyses given in this document.

Appendix

Appendix Supplied by E. Taylor -- General Criteria for Distinguishing Non-Pedogenic from Pedogenic Calcium Carbonate and Opaline Silica

----- Table on following three pages -----

General Criteria for Distinguishing Non-pedogenic from Pedogenic Calcium Carbonate and Opaline Silica
Compiled by E. Taylor

Factor	Non-Pedogenic	Pedogenic	Observed in Trench 14
Geomorphology, spatial arrangements	Isolated points at/near springs downslope of fractures or faults in bedrock or surficial deposits	Follow topography & geomorphic surfaces, laterally persistent	Laterally persistent in slope-wash alluvium
Location of the initial CaCO_3 & opaline SiO_2 deposition in a gravelly deposit	Random orientation, gravel remains in contact (clast supported); bedding features may be preserved	Deposition on the underside of clasts; gravel does not remain in clast contact (matrix supported); bedding features lost or poorly preserved in advanced stages	Initial deposition on the underside of gravel, matrix supported; bedding features lost or poorly preserved
Physical characteristics of max developed CaCO_3	Discrete stratiform; mounded, or draped strata; commonly displaying vegetative molds or vugs	Continuous layers underlain by bedrock or a plugged horizon	Continuous laminar layers that have formed plates
Change in concentration of CaCO_3 & opaline SiO_2 with depth	No systematic change, uniform deposition	Decreases with depth below a near-surface maximum	Decreases with depth below a near-surface maximum
General distinguishing petrographic & mineralogic characters	High temp -- no ooids; 1 opal-C Low temp -- few ooids; 1 opal-A Both are poorly stratified and have common sulfide, sulphate & manganese minerals	Ooids common, usually opal-CT; well stratified; common smectitic and illitic clay minerals	Ooids common, primarily opal-CT; well stratified; common smectitic and illitic clay minerals
Ca:Mg ratio of clay minerals	No systematic depletion of Mg^{++} over time when compared to CaCO_3 precipitation	Progressive depletion of Mg^{++} in comparison to the accumulation of secondary CaCO_3 ; formation of magnesium-rich clays	Formation of Mg-rich clays including sepiolite and palygorskite

crystallinity & percent	microsparite, and sparite; crystals > 99.5 % pure	crystalline b-fabric; commonly clay, MgCO_3 , and opaline SiO_2 present; << 99.5% pure	b-fabric with clay and opaline SiO_2 ; < 70% pure
Opaline SiO_2 % & crystallinity	Silcrete, > 85% opaline SiO_2 , amorphous->coarsely crystalline	Duripan, << 85% opaline SiO_2 , amorphous, dense, hard	Duripan, > 85% amorphous opaline SiO_2
$\delta^{13}\text{C}$ vs $\delta^{18}\text{O}$ in CaCO_3 (see note below) (Note: $\delta^{13}\text{C}$ is vegetation dependent and $\delta^{18}\text{O}$ is dependent on mineralization of CaCO_3 and fluid source)	Expected range within concentrations reported for spring deposited CaCO_3	Expected range within concentrations reported for pedogenic CaCO_3	Range with concentrations reported for pedogenic CaCO_3
δD vs $\delta^{18}\text{O}$ in CaCO_3	Shift in $\delta^{18}\text{O}$ concentrations away from the concentrations of meteoric water	No shift in $\delta^{18}\text{O}$ concentrations away from the concentrations of meteoric water	Concentrations are equal to those of meteoric water
Pb isotopes (see note below)	Dominated by isotopic concentrations different from that of the soil parent material or, in the veins, the adjacent bedrock	Dominated by isotopic concentrations of the soil parent material or, in the veins, the adjacent bedrock	Pb isotopic composition very similar to bedrock from which the slope-wash alluvium is derived and through which the veins penetrate

∞ (Note: reflects the isotopic composition of the rocks with which the water that precipitated the CaCO_3 was in contact)

Sr isotopes (see note below)

Expected $^{87}\text{Sr}/^{86}\text{Sr}$ ratios within range of independently obtained samples from ground water, spring water, spring deposits, limestone or volcanic tuffs, or both

(Note: geochemical analogue of Ca^{++} ; indicates the isotopic composition of the rocks with which the water that precipitated the CaCO_3 was in contact)

Expected $^{87}\text{Sr}/^{86}\text{Sr}$ ratios within the range of independently obtained samples from soils developed on stable alluvial surfaces or from eolian samples

$^{87}\text{Sr}/^{86}\text{Sr}$ concentrations in the slope-wash alluvium and veins are similar to independently obtained soil and eolian samples

Factor	Non-Pedogenic	Pedogenic	Observed in Trench 14
U-series isotopes (see note below)	Dominated by isotopic concentrations similar to samples independently obtained from ground water and spring water	Dominated by isotopic concentrations similar to samples independently obtained from soil and eolian samples	U-series concentrations in the slope wash alluvium and veins are similar to independently collected soil and eolian samples
(Note: indicate the isotopic composition of the rocks with which the water that precipitated in contact)			
Ostracodes (see note below)	Almost always in spring deposits	Not present, or if present in a soil environment, are part of the eolian component, and external surfaces must have evidence of wind abrasion	No ostracodes are present
(Note: a calcareous microfossil that requires a saturated and oxygenated environment. Species are dependent on temperature and chemistry)			

This table is a modification of one developed by E. Taylor and is used here with her permission.

Tables

1. Water Temperatures in Yucca Mountain Boreholes - Data
2. Station Sets versus Weights versus Gradient
3. Chemical Data from Aquifers in Yucca Mountain Boreholes
4. Chemical Compositions of "Paleozoic" Aquifer Springs and Wells

----- Tables 1 through 4 on following three pages -----

TABLE 1

WELL	WELLHEAD ELEV	WT DEPTH	BOTTOM DEPTH	WT ELEV	BOTTOM ELEV	MID ELEV	TEMP °C	ELEV RANGE
J-12	954	225	347	729	607	668	27	122
J-13	1011	282	1063	729	-52	338	31.5	781
UE-258#1	1200	470	1220	730	-20	355	36	750
UE-258#1	1200	470	1220	730	-20	355	36	750
UE-258#1	1200	470	1220	730	-20	355	37.2	12
UE-250#1	1131	400	914	731	217	474	41.5	514
UE-250#2	1132	401	913	731	219	475	40.5	512
UE-250#3	1132	402	913	730	219	474	40.8	511
UE-259#1	1114	381	1805	733	-691	-283	56	200
UE-299#2	1215	29	422	1186	793	915	25.1	107
UE-999#2	1215	29	422	1186	793	1065	22.7	126
USW G-4	1270	541	915	729	355	542	35.6	374
USW H-1	1302	572	1829	730	-527	672	33	115
USW H-1	1302	572	1829	730	-527	44	34.7	1142
USW H-3	1483		1220	730	263	463	26.5	400
USW H-4	1249	519	1220	730	29	380	34.8	701
USW H-5	1478	704	1220	774	258	516	36.5	516
USW H-5	1478	704	1220	774	258	516	35.3	516
USW H-6	1302	526	1220	776	82	429	37.8	694
USW H-6	1302	526	1220	776	82	508	41.6	82
USW H-6	1302	526	1220	776	82	675	37.2	38
USW VH-1	955	184	762	771	193	482	35.2	578
USW VH-1	955	184	762	771	193	482	35.5	578
USW VH-1	955	184	762	771	193	482	35.5	578
SURFACE	1300					1300	20	

TABLE 2

STATION SET VERSUS WEIGHTS VERSUS CALCULATED GRADIENT

SET	MAXPWT	WT (200-400)	TOTAL WT	GRADIENT °C/km.
ALL	1	1	28	19.0
9 of WT 1 (Figure 3)	25	0.17	10.5	18.6
	50	0.09	9.7	20.4
	200	0.02	9.2	22.6
	1000	0.005	9.04	23.3
ALL TERT.	1	1	27	15.6
8 of WT 1 (Figure 4)	25	0.17	9.5	12.8
	50	0.09	8.7	15.5
	200	0.02	8.2	19.9
	1000	0.005	8.04	21.9
ALL BUT 4	1	1	24	2.1
5 of WT 1 (Figure 5)	25	0.17	6.5	3.8
	50	0.09	5.7	5.9
	200	0.02	5.2	11.9
	1000	0.005	5.04	17.7

MAXPWT = Maximum Relative Weights (Ratio of weight of 0-200 meter interval (1) to weight of 1000-1200 meter interval)

WT (200-400) = Weight given to data from 200-400 meter intervals.

TOTAL WT = Total of weights used for all intervals.

GRADIENT = Calculated temperature gradient.

TABLE 3

WELL	SAMP INT	WELL HEAD elev (m)	WT/DEPTH (m)	WT elev (m)	TEMP °C	Ca mmol/l	SiO2 mmol/l	HCO3 mmol/l	PH	PCO2 atm	M mmol/l	I mmol/l	K _w mmol/l ^{1/3} /a
J-12	INT	953	225/347	728	27	0.349	0.899	1.95	7.1	0.00845	0.929	3.55	1.629
J-13	INT	1011	282/1063	729	31	0.299	0.949	2.032	7.2	0.00766	1.284	3.36	1.61
UE-25B#1	INT	1200	470/1220	730	36.5	0.449	0.865	2.431	7.2	0.00917	1.399	3.92	1.663
UE-25C#1	INT	1131	400/914	731	41.5	0.274	0.932	2.475	7.6	0.00468	1.717	3.7	1.642
UE-25C#2	INT	1132	401/913	731	40.5	0.299	0.899	2.278	7.7	0.00538	1.667	3.58	1.632
UE-25C#3	INT	1132	402/913	730	40.8	0.274	0.882	2.245	7.7	0.00335	1.707	3.54	1.627
UE-25#P1	381-1197*	1114	381/1805	733	44.3								
UE-29A#2	87-354	1215	29/422	1186	23.5	0.24	0.732	1.754	7.1	0.00722	0.886	3.13	1.588
U5A-64	INT	1270	541/915	729	35.6	0.324	0.75	2.278	7.7	0.00313	1.767	3.66	1.639
U5A-H1	572-687	1302	572/1829	730	33	0.133	0.729	1.942	7.6	0.00328	1.672	2.88	1.562
U5A-H1	687-1829				34.7								
U5A-H3	822-1220	1483	../1220	(730)	26.5	0.02	0.716	4.491	9.2	0.00015	4.147	5.79	1.828
U5A-H4	INT	1249	519/1220	730	34.8	0.424	0.766	2.835	7.4	0.00739	2.211	4.71	1.735
U5A-H5	INT	1478	704/1220	774	35.6	0.049	0.799	2.073	7.9	0.00206	1.949	2.99	1.574
U5A-H6	INT	1302	526/1220	776	37.8	0.088	0.799	3.458	8.2	0.00148	2.671	4.67	1.731
U5A-H6	753-835				41.6								
U5A-H6	606-646				37.2								
U5A-UH1	INT	955	184/762	771	35.4	0.258	0.827	2.699	7.6	0.00424	2.195	4.8	1.743
AVERAGE					34.9							3.73	1.657
STD DEV												0.77	0.319
MIN					23.5								
MAX					44.3								
UE-25P#1	1237-1805	1114	381/1805	733	56	2.495	0.682	9.325	6.6	0.16047	5.665	0.13	2.903

TABLE 4

COMPOSITIONS OF "PALEOZOIC AQUIFER SPRINGS AND WELLS"

Site	T° C	Ca	SiO ₂	HCO ₃	PH	Mg	Na	K	Cl	SO ₄	F
MSW1	33.5	1.422	0.499	4.418	7.2	1.029	3.838	0.281	1.495	1.666	0.111
MSW2	27.0	1.498	0.499	4.582	7.4	1.110	4.350	0.256	1.720	1.666	0.121
190	32.5	1.647	0.483	4.426	7.2	1.069	4.176	0.256	1.720	1.978	0.111
119	31.0	1.173	0.433	5.114	7.4	0.864	3.219	0.240	0.733	0.926	0.089
240	35.3	1.148	0.499	6.462	7.2	0.617	1.261	0.197	0.226	0.343	0.047
247	27.5	1.098	0.399	4.230	7.3	0.905	0.957	0.136	0.245	0.343	0.016
248	17.0	0.948	0.899	2.459	7.7	0.218	0.913	0.179	0.479	0.104	0.016
249	26.0	1.223	0.499	4.623	7.4	0.946	1.130	0.189	0.310	0.385	0.032

SITE LOCATIONS

	Latitude	Longitude	
MSW1	--	ERTEC MX-6 WELL	36 46 04
MSW2	--	CSV-2 WELL	36 46 50
190	--	BIG MUDDY SPRING	36 43 20
119	--	CRYSTAL POOL SPRING	36 25 14
240	--	ASH SPRING	37 27 49
247	--	CRYSTAL SPRING	37 31 53
248	--	ACOMA WELL	37 32 55
249	--	HIKO SPRING	37 36 34
			114 14 47
			114 43 20
			114 42 48
			116 19 21
			115 11 34
			115 13 58
			114 10 23
			115 12 51
			MUDDY SPRINGS AREA
			MUDDY SPRINGS AREA
			MUDDY SPRINGS AREA
			ASH MEADOWS AREA

Figures

----- (Figures on following pages) -----

1. A. $\delta^{13}\text{C}$ Carbon - Yucca Mountain Boreholes - All Data
B. $\delta^{18}\text{O}$ Oxygen - Yucca Mountain Boreholes - Vug Data Removed
2. Calculated Temperature Gradient - Intervals ≤ 200 Meters
3. Calculated Temperature Gradient versus Weights -- All Data
4. Calculated Temperature Gradient versus Weights -- All Tertiary Data
5. Calculated Temperature Gradient versus Weights -- All Tertiary Data below 730 meters above sea level (ASL)
6. $^{234}\text{U}/^{238}\text{U}$ versus $^{230}\text{Th}/^{234}\text{U}$ -- Samples and Aquifers -- Yucca Mountain, Yucca Flat and Ash Meadows
7. Cation Composition of Aquifers Expressed in Mmol/l Per Cent
8. Anion Composition of Aquifers Expressed in Mmol/l Per Cent
9. H_2CO_3 Activity versus CO_2 Pressure (Total Pressure = 1 Atmosphere)
0. HCO_3^- Concentration versus H_2CO_3 Activity -- Tertiary Aquifers
 1. CO_2 Pressure versus PH -- Tertiary Aquifers
 2. HCO_3^- Concentration versus Ca^{++} Concentration -- Tertiary Aquifers
 3. Ca^{++} Concentration versus PH -- Tertiary Aquifers
 4. Ca^{++} Concentration versus CO_2 Pressure versus M -- Tertiary Aquifers
 5. Ca^{++} Concentration versus CO_2 Pressure versus Temperature -- Paleozoic Aquifer
6. Cation Composition of Aquifers Expressed in Mmol/l
7. Ca^{++} Concentration versus CO_2 Pressure versus M -- Muddy Springs
8. Location Map for Sites Presumably Displaying Hydrothermal Characteristics (Figure 7 of Minority Report)

Figure 1A

Figure 1A

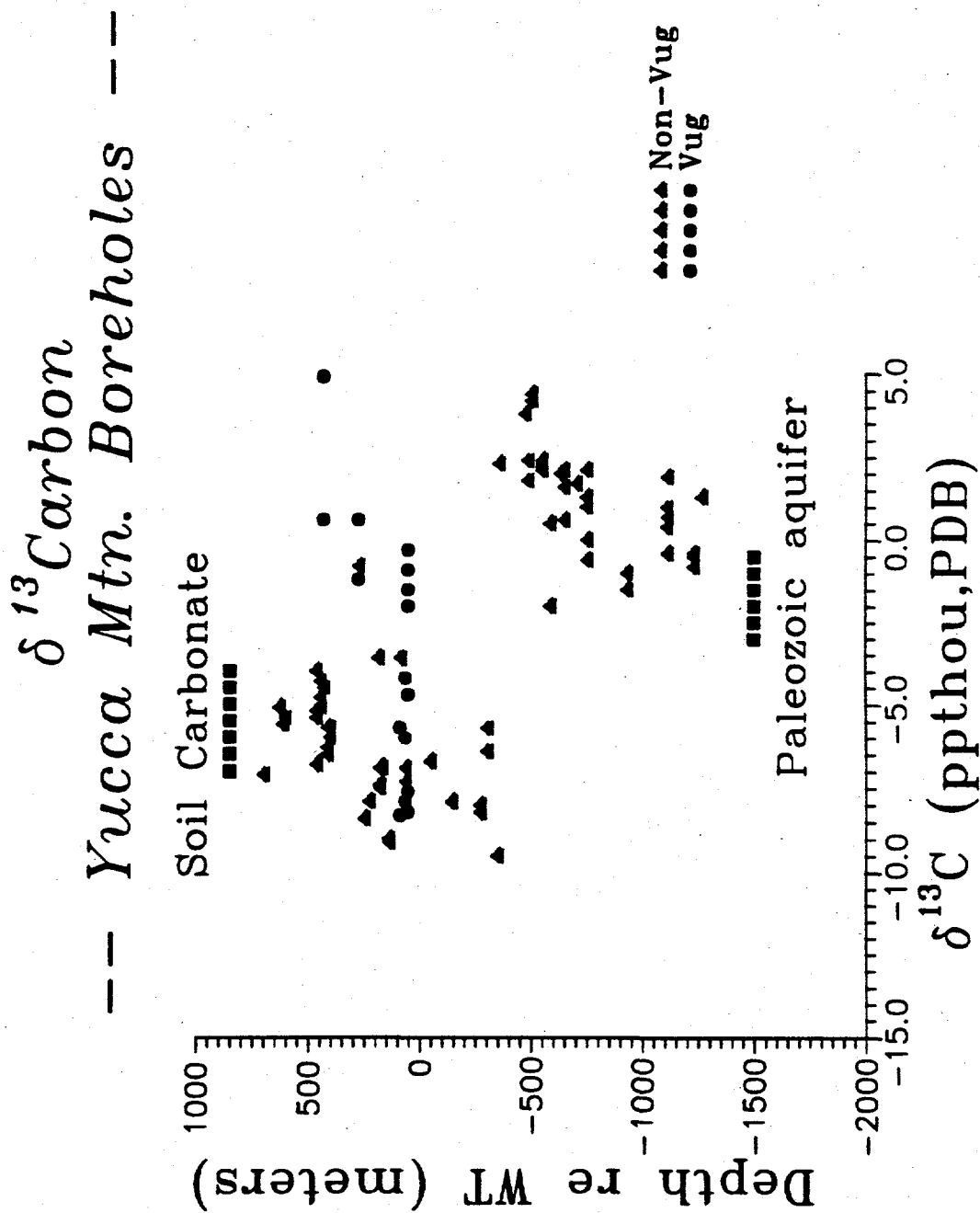


Figure 1B

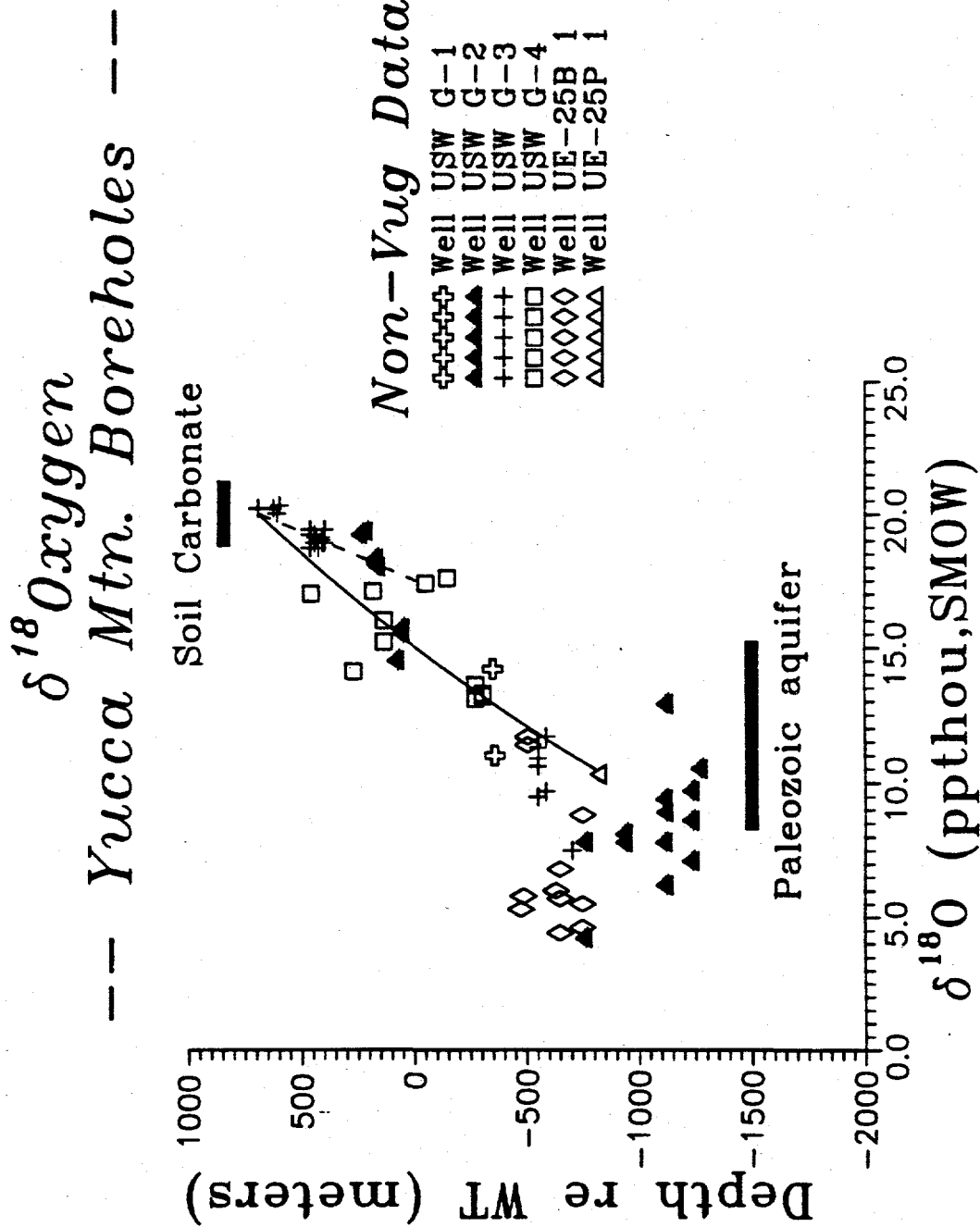


Figure 2

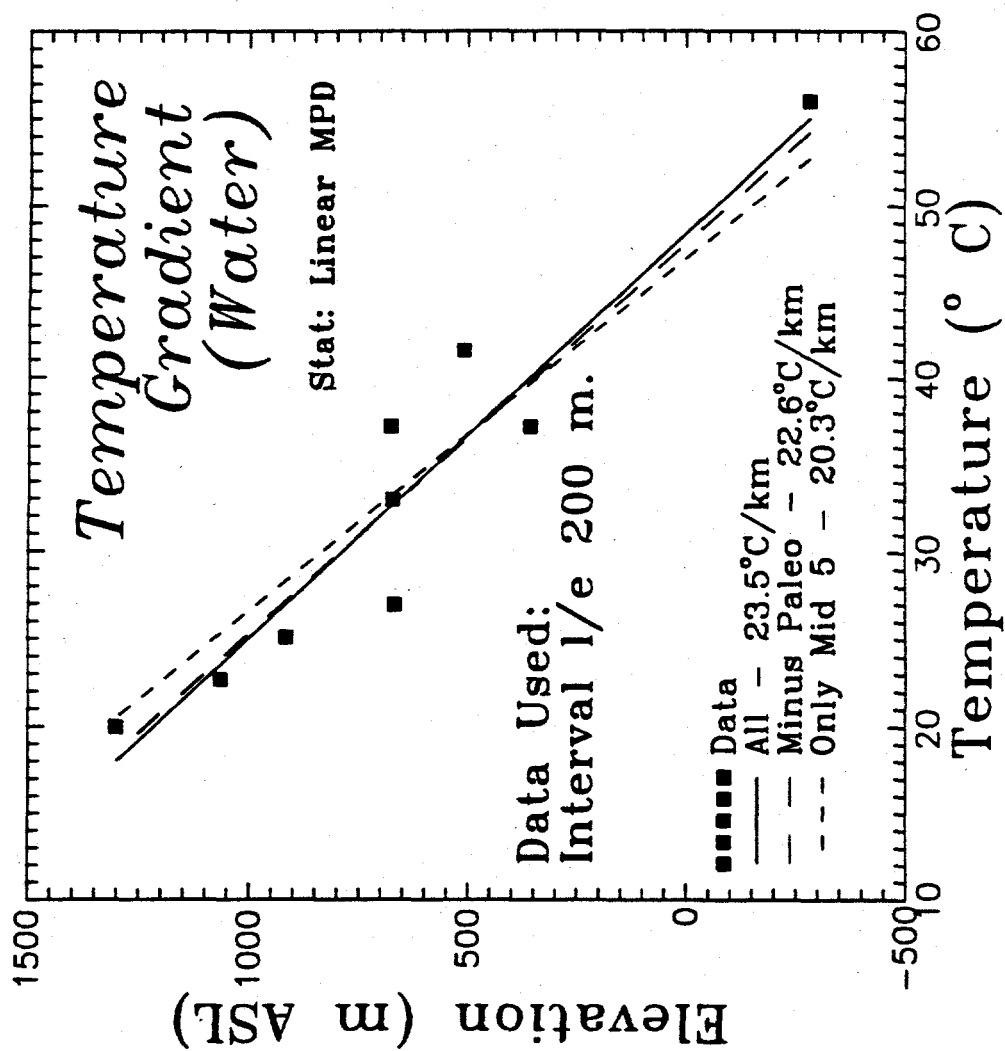


Figure 3

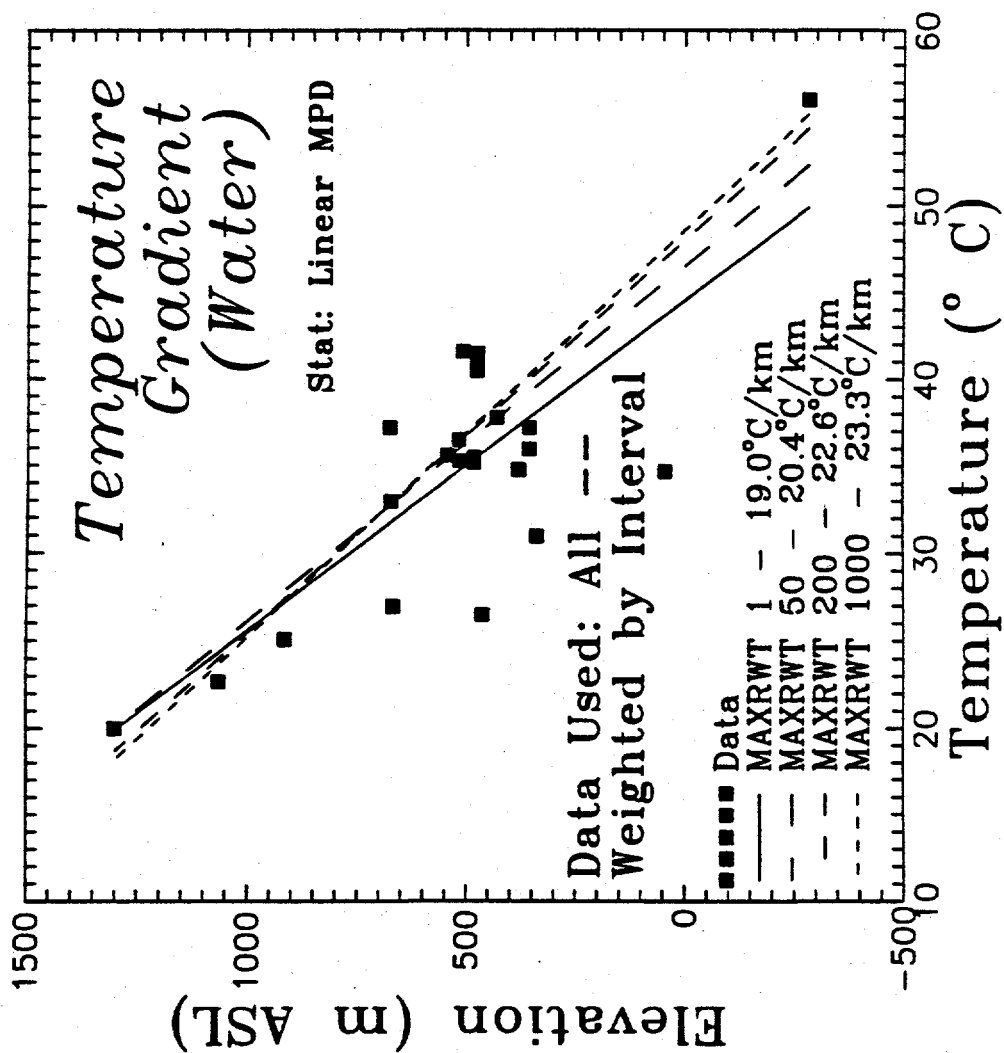


Figure 4

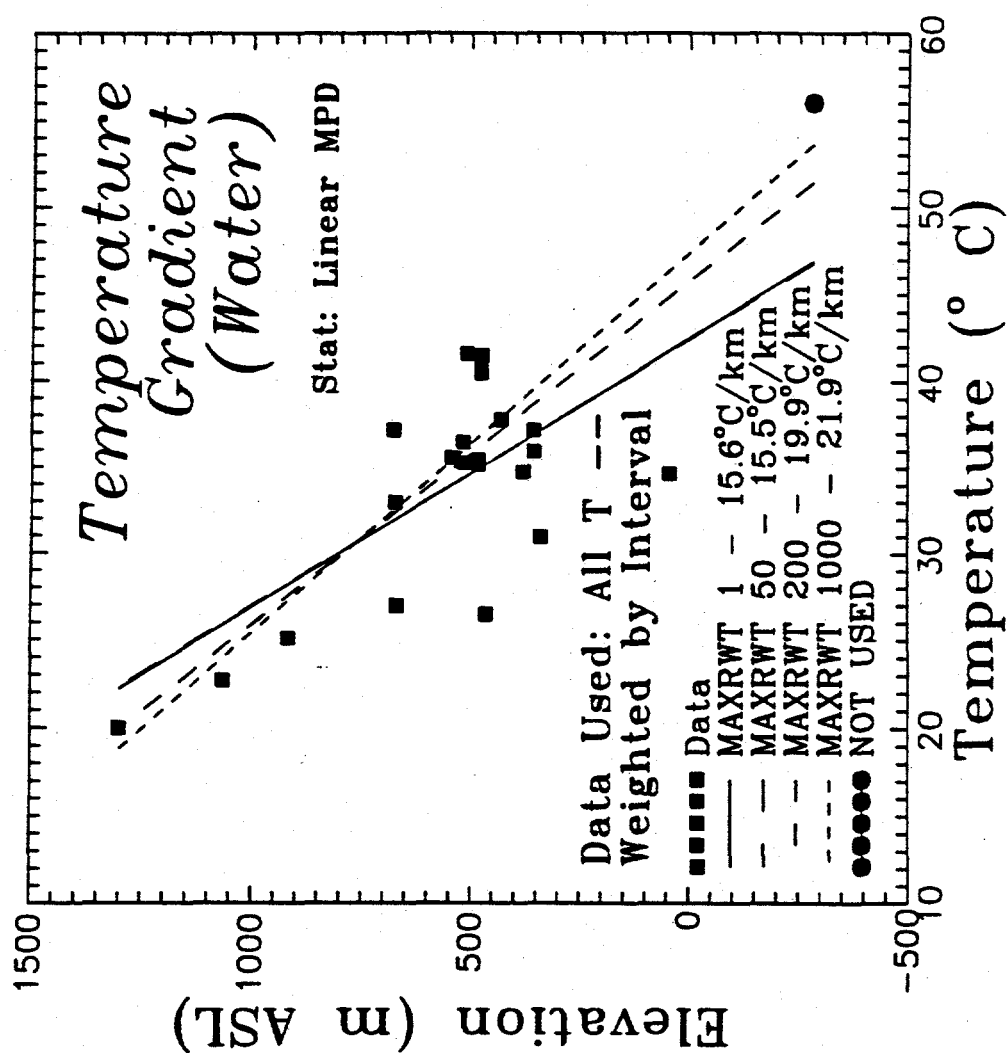


Figure 5

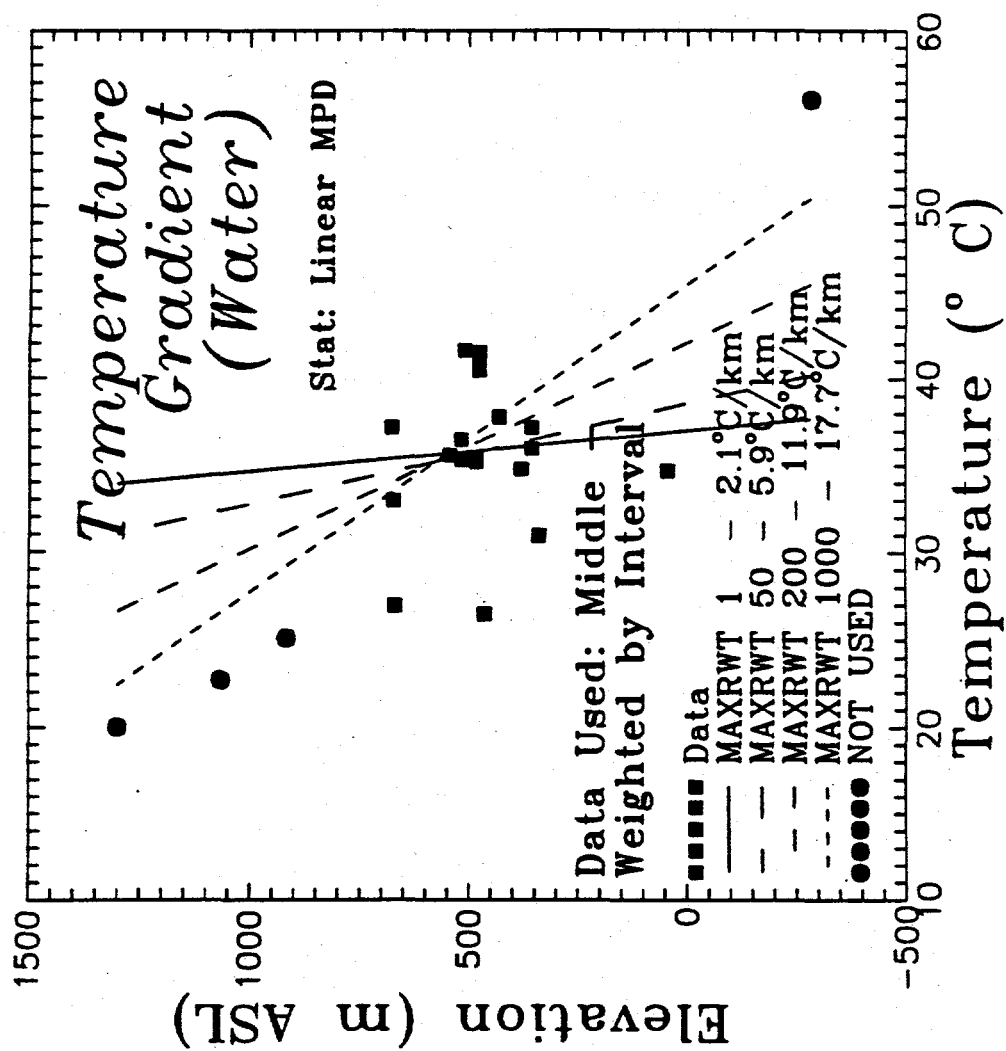


Figure 6

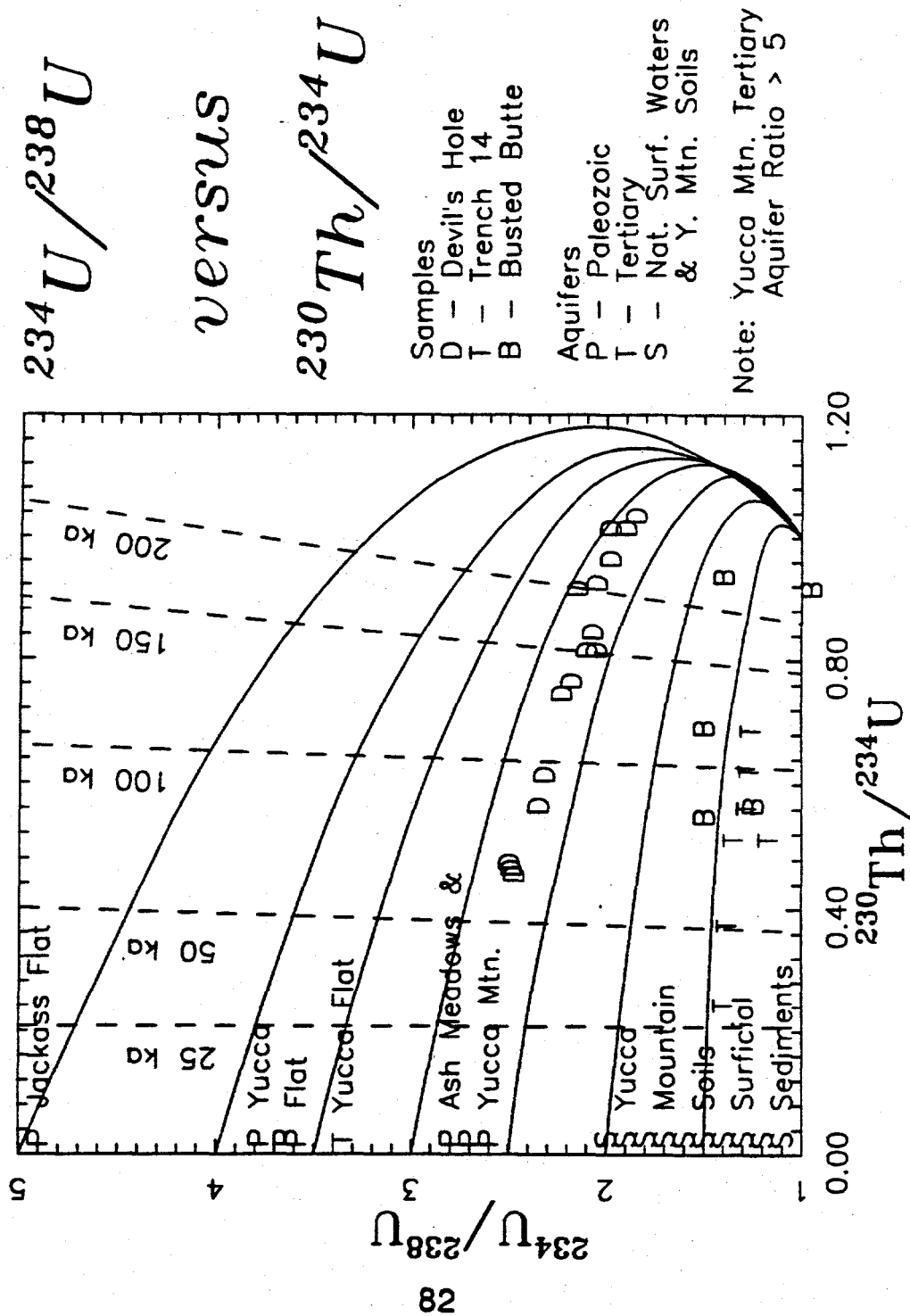


Figure 7

Cation Composition of Aquifers (Expressed in mmol/l Per Cent)

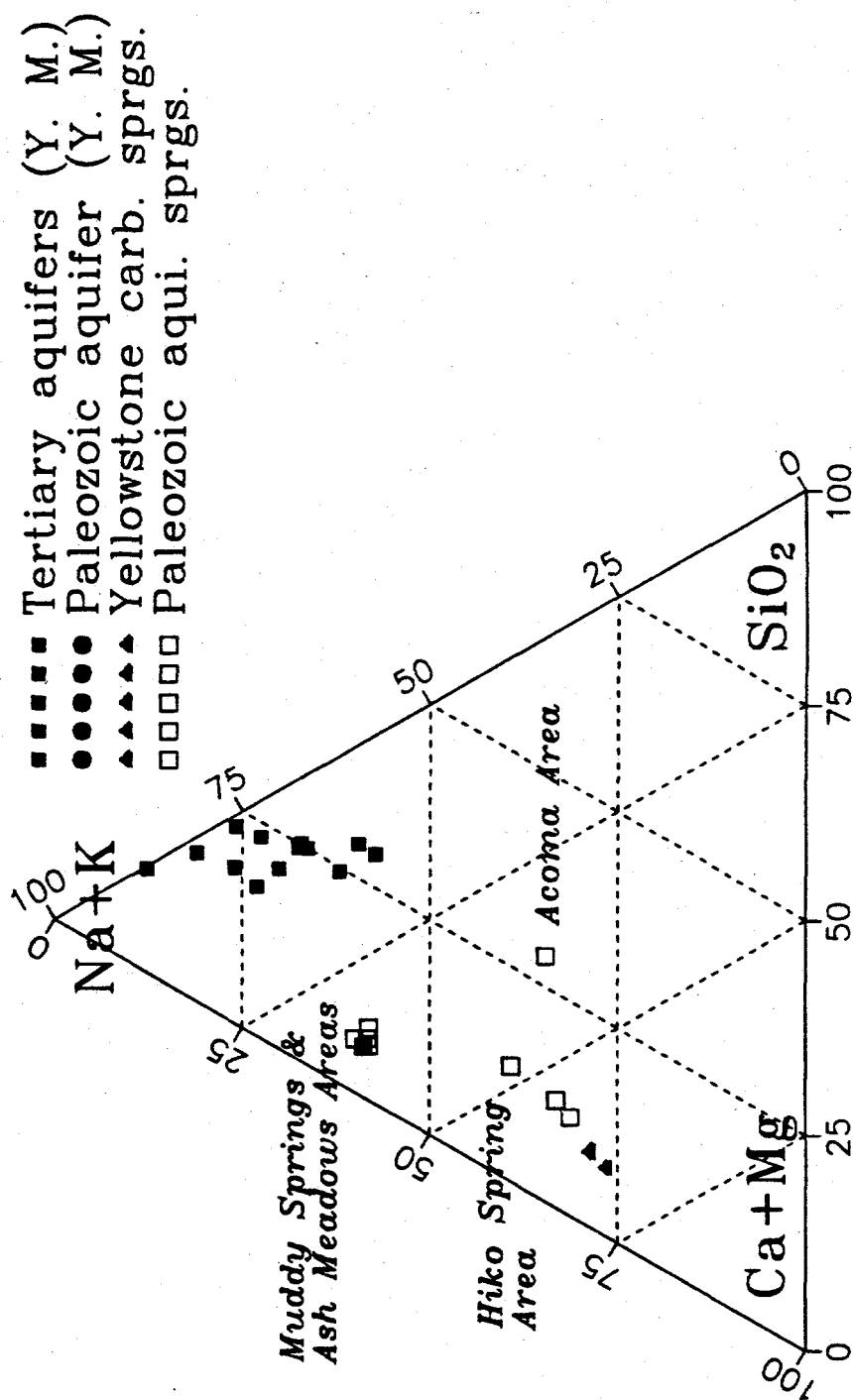


Figure 8
Anion Composition of Aquifers
 (Expressed in mmol/l Per Cent)

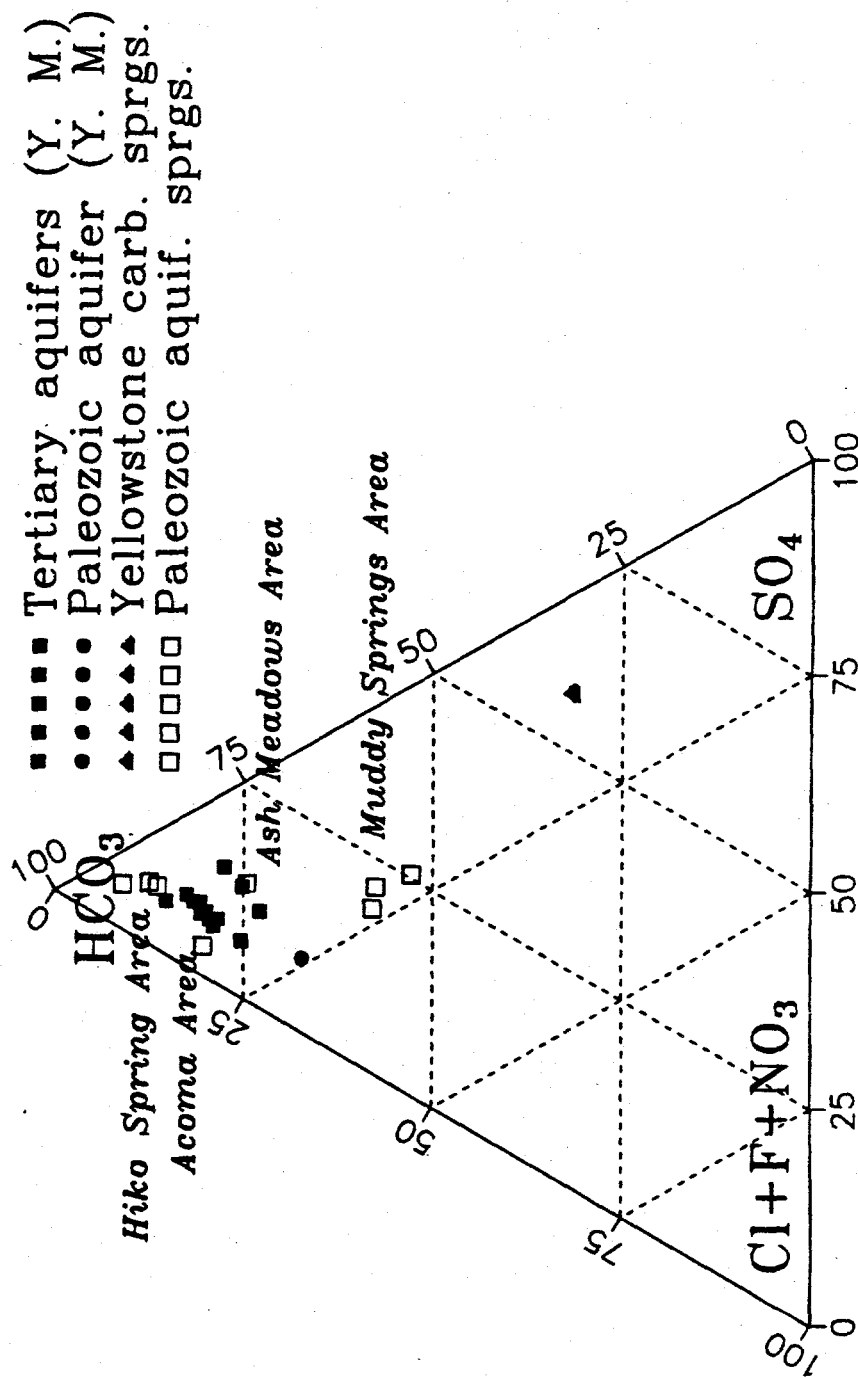


Figure 9

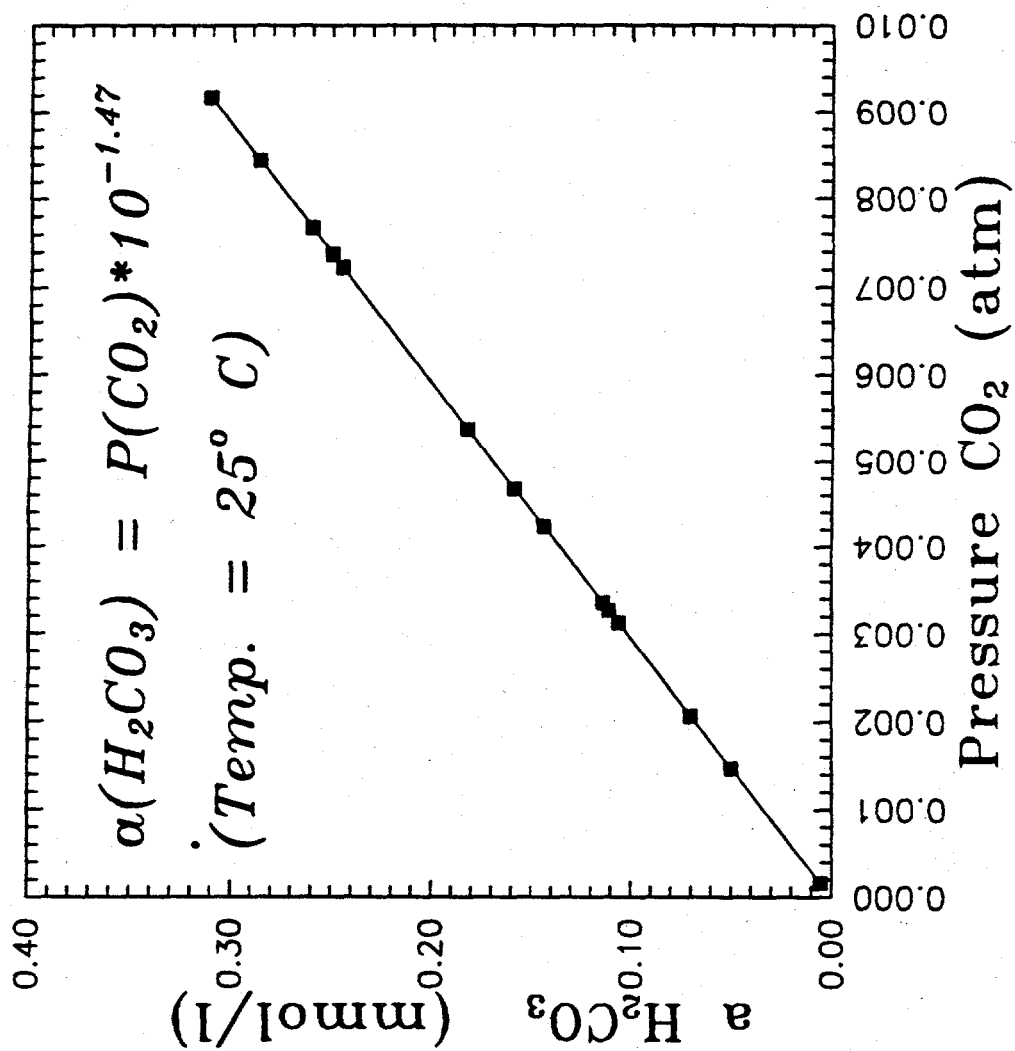


Figure 10

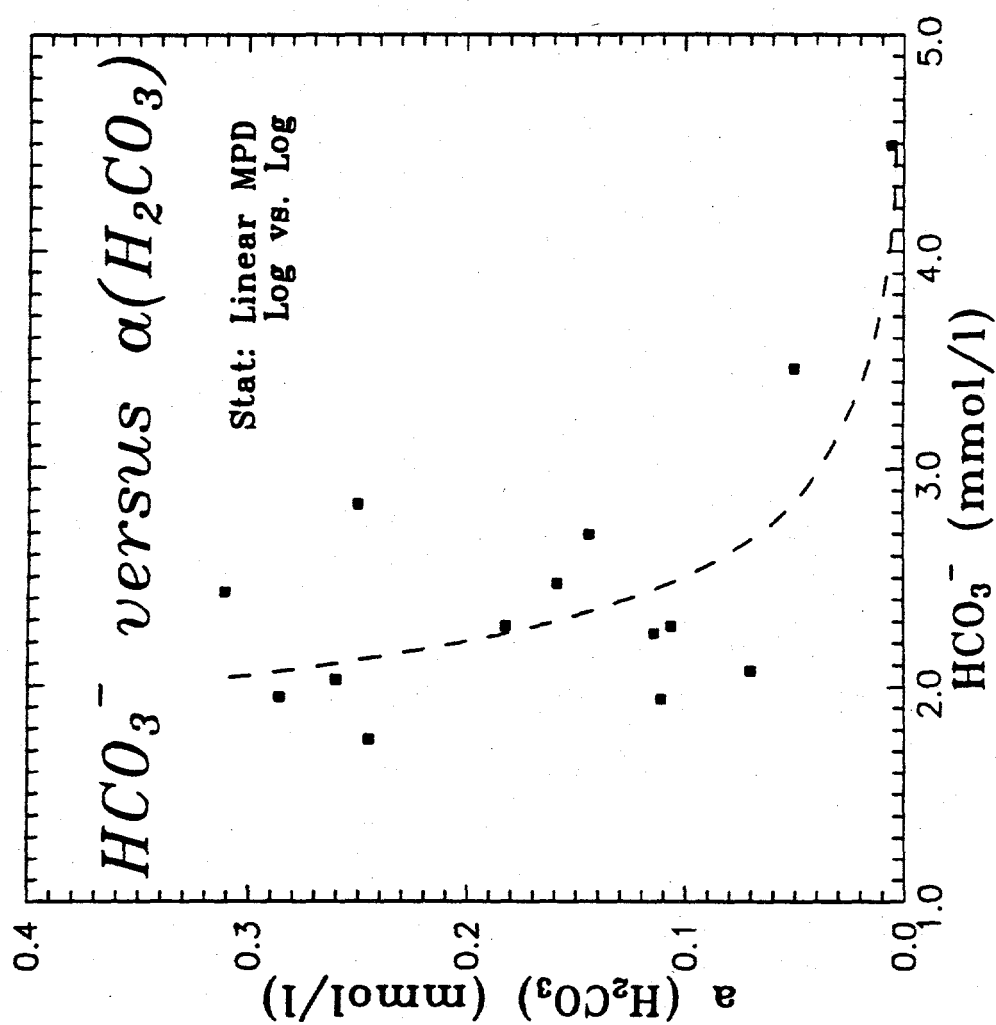


Figure 11

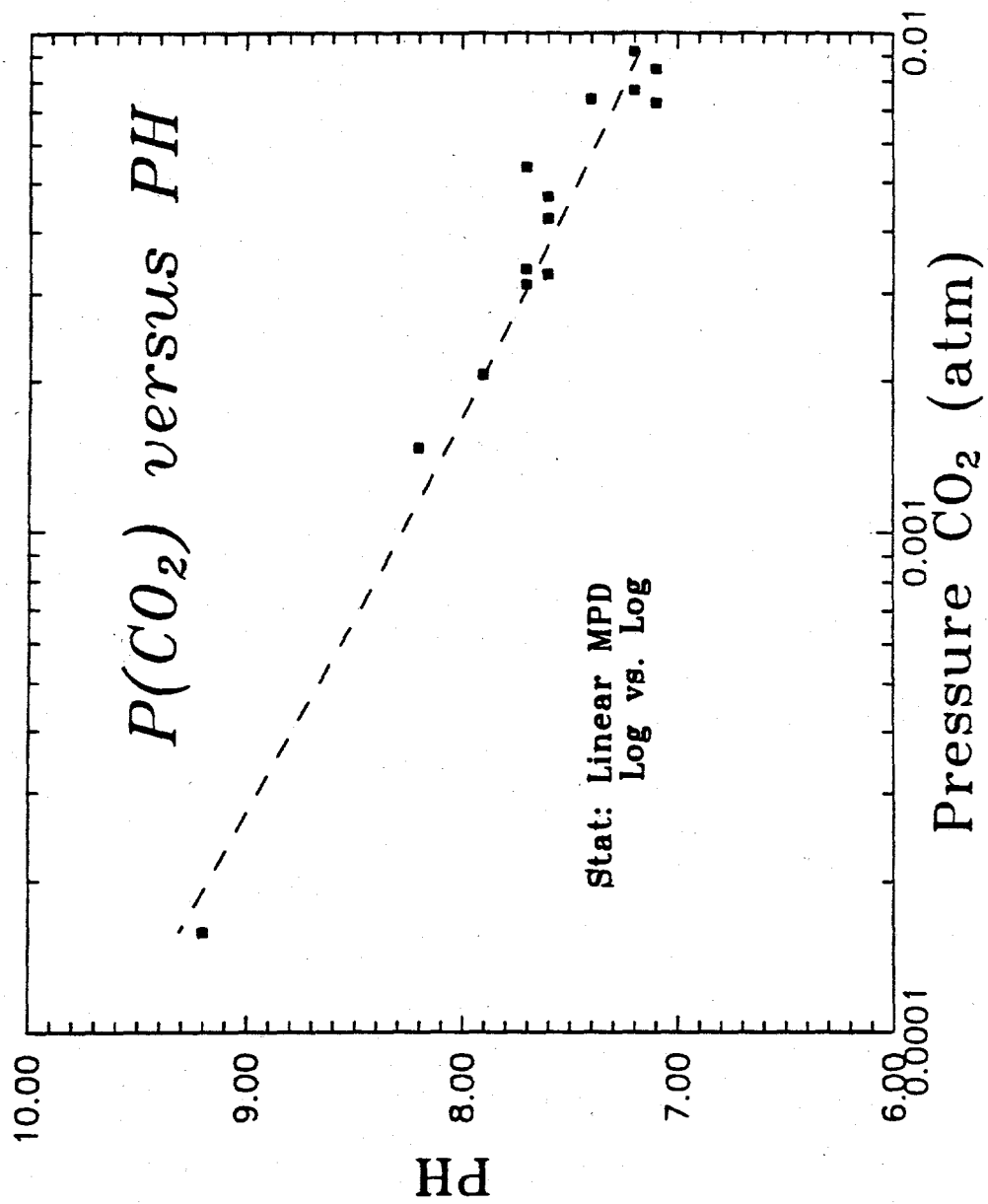


Figure 12

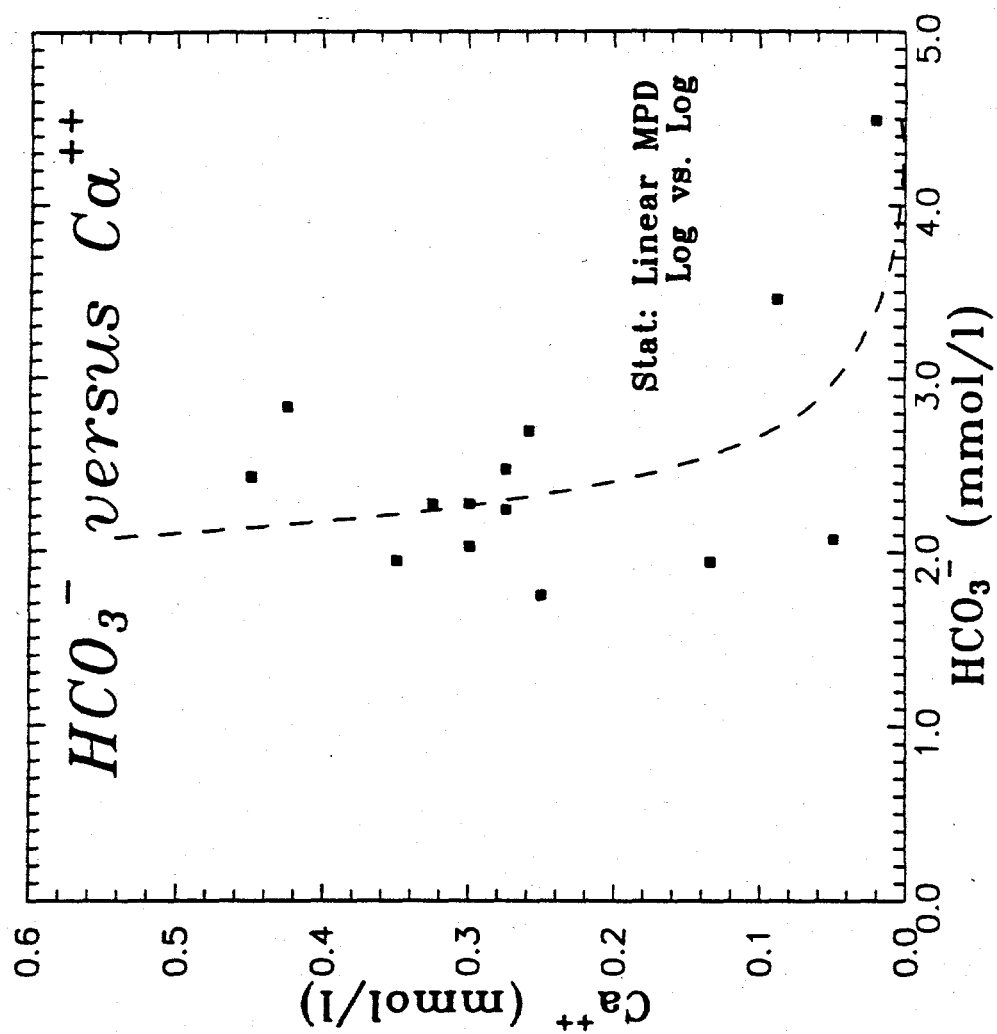


Figure 13

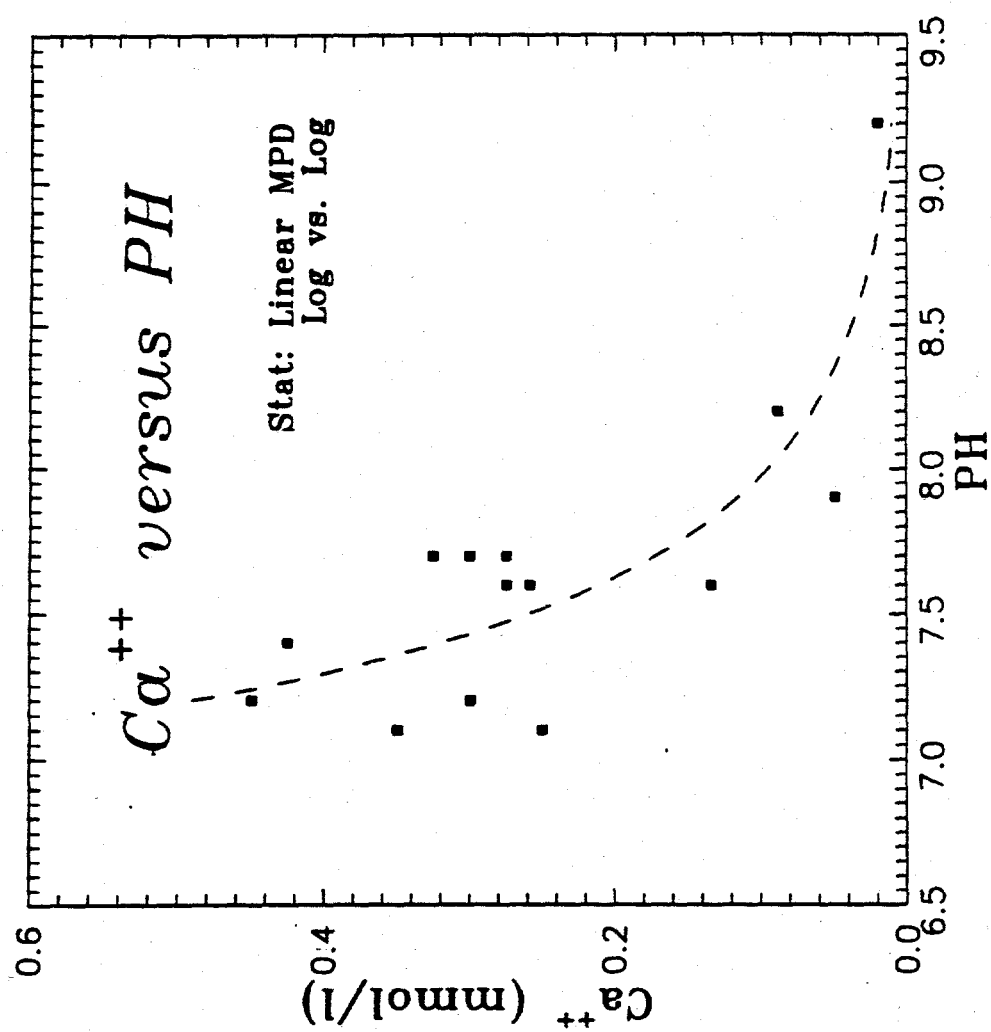


Figure 14

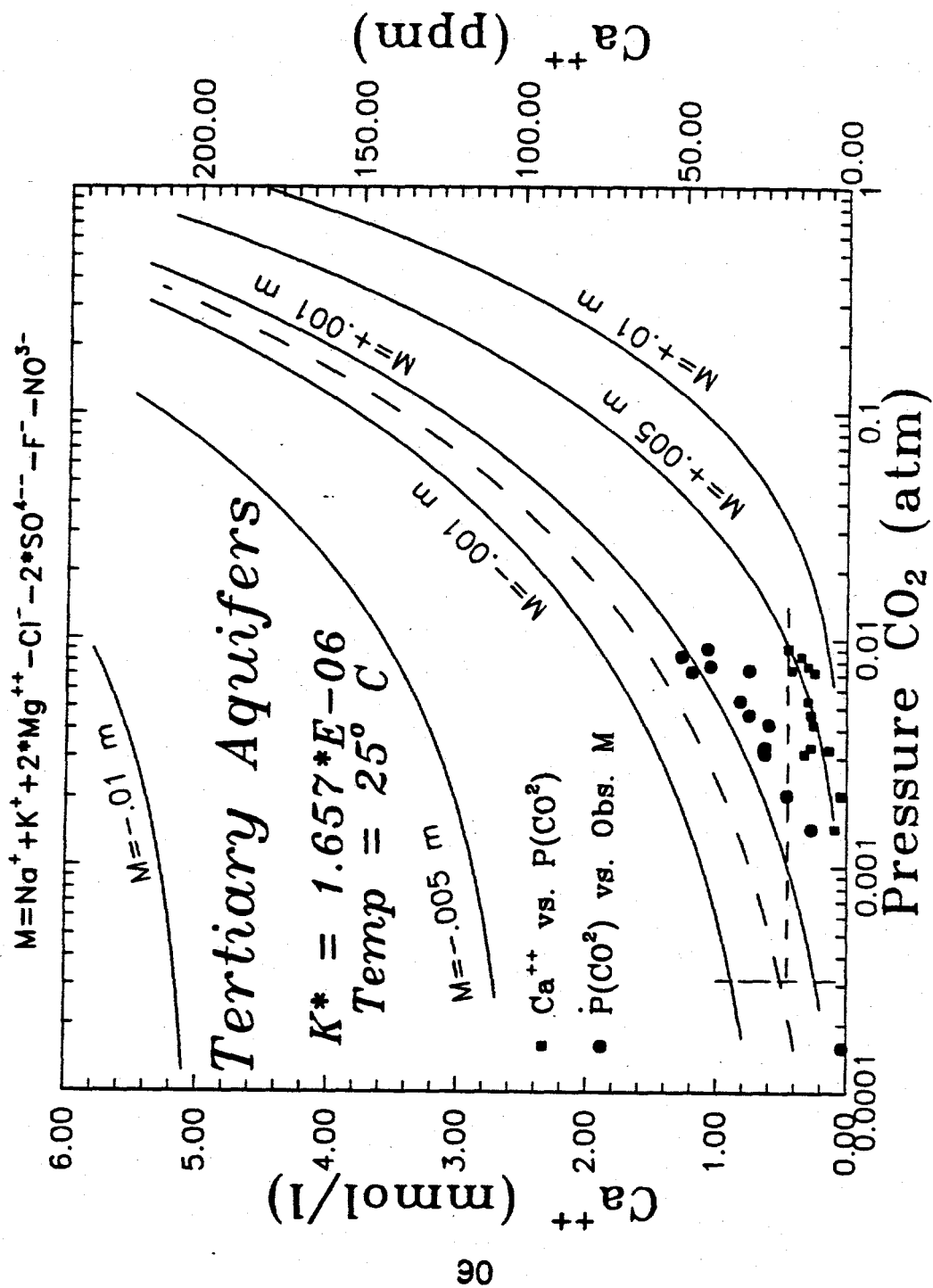
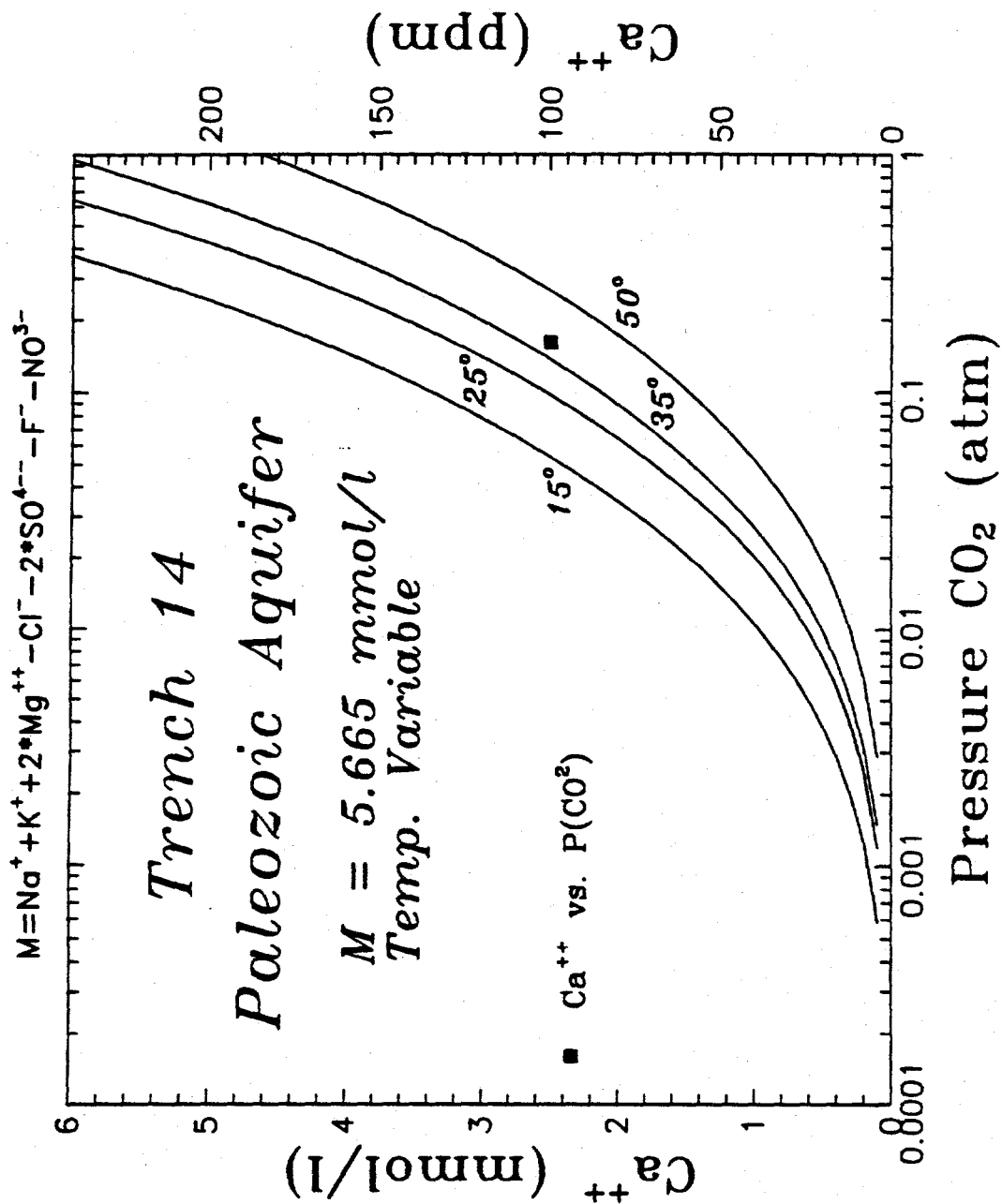


Figure 13



Cation Composition of Aquifers (Expressed in mmol/l)

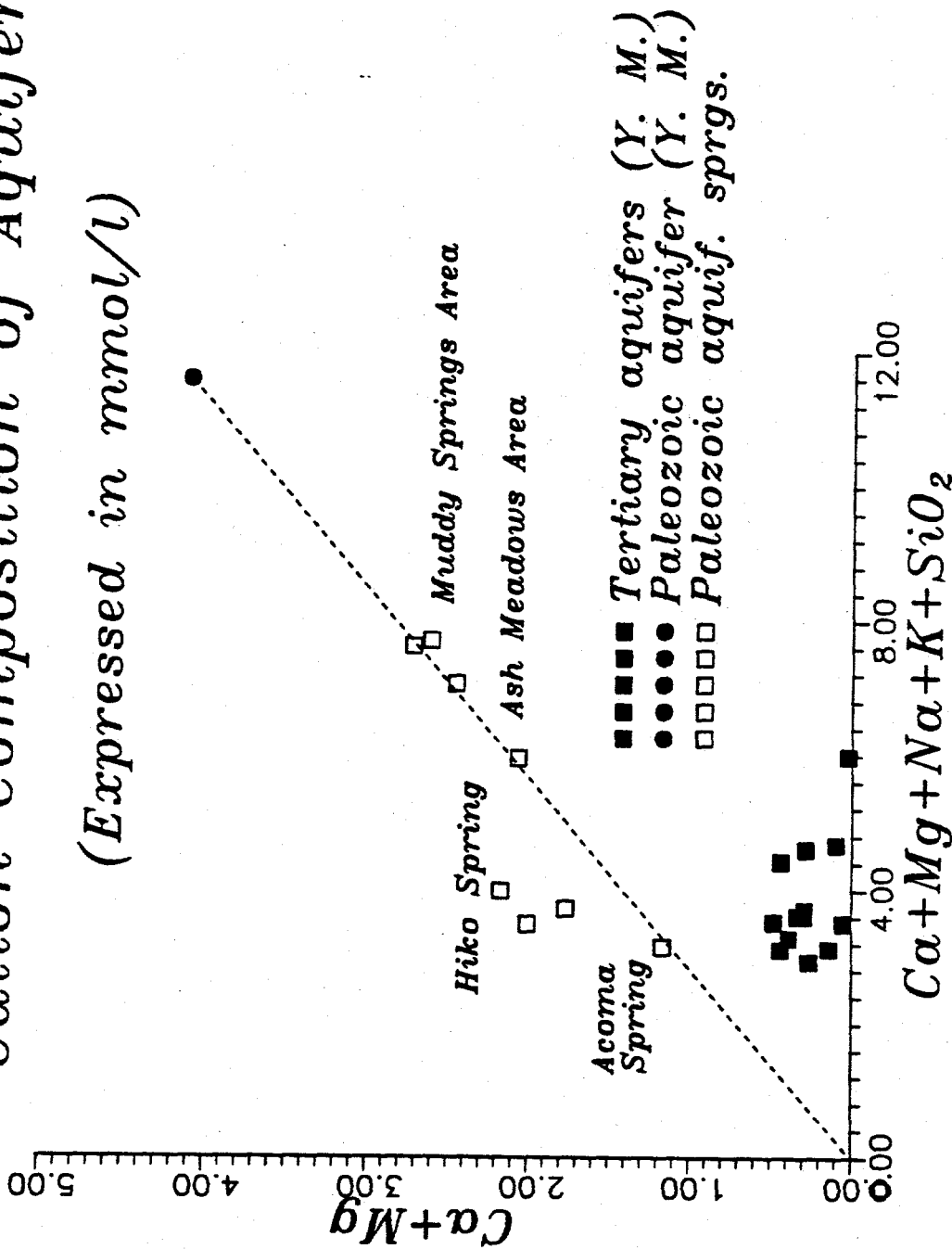
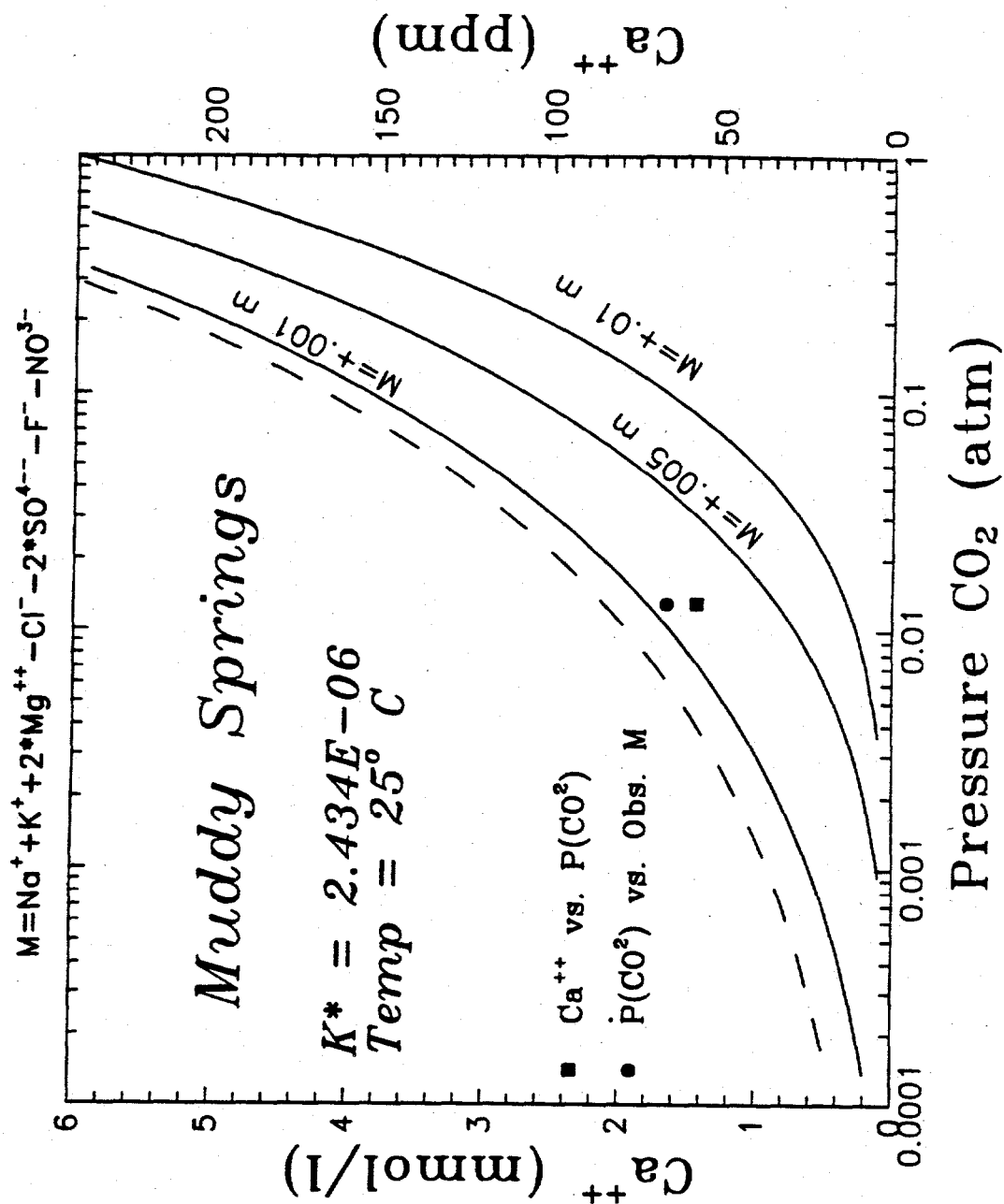
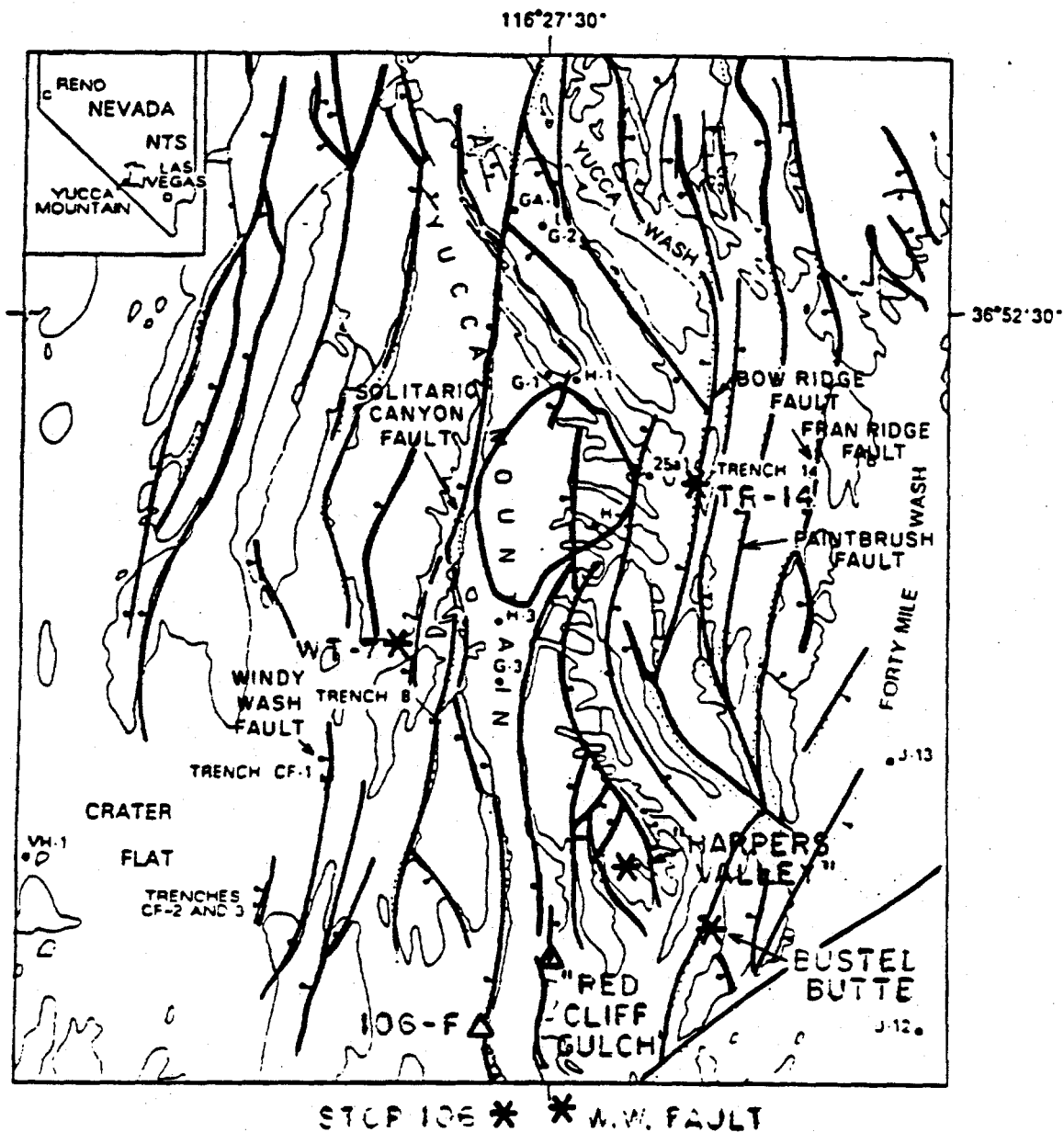


Figure 17





EXPLANATION

• G-2 DRILL HOLE

— TRENCH

— NORMAL FAULT--BAR AND BALL
ON DOWNTOWN SIDE

— PERIMETER DRIFT BOUNDARY

Figure 18

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PART 9

Review on USGS OFR 92-516.

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REVIEW OF USGS OFR 92-516

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REVIEW OF USGS OFR 92-516

CONTENTS

INTRODUCTION	1
SAFETY AS EVALUATED BY OFR 92-516	4
SEISMIC AND FAULT RUPTURE POTENTIAL	5
POTENTIAL GEOHYDROLOGIC EFFECTS OF CREDIBLE SEISMIC EVENTS	8
PROPERTIES OF THE CALICO HILLS TUFFS	12
PALEOGEOTHERMOMETRY AND MINERALIZATION OF THE VADOSE ZONE	16
STRONTIUM ISOTOPIC RATIOS	21
GRADIENT OF HYDRAULIC POTENTIAL	22
CONCLUSION .	22
REFERENCES	24

INTRODUCTION

Our subject is a report entitled: *Safety of Proposed Yucca Mountain Nuclear Repository as Regards Geological and Geophysical Factors: Evaluation of Minority Report by Archambeau and Price*, by Jack F. Evernden of the United States Geological Survey.

Controversy was inevitable when the DOE selected a seismically and volcanically active terrain as a candidate repository site for the nation's high-level radioactive waste. At the time, in the late seventies, earth scientists were only dimly aware of the geologic hazards. OFR 92-516 takes us back to a time when it was possible to believe that "the area is essentially tectonically dead" (p. 20), if one could possibly regard the young volcanic cones flanking Yucca Mountain to the west and south as dying gasps.

By the time the Nuclear Waste Policy Act was amended (1987), leaving Yucca Mountain as the only candidate for the repository, site investigations had produced diverse evidence conflicting with the notion of tectonic and hydrologic quiescence. Faults and travertine veins in the faults were found to be youthful, wholesale metasomatic alteration of the ignimbrite rocks of Yucca Mountain was documented, aberrant gradients in subsurface temperature and hydraulic potential were discovered, and rocks were found to fracture under the load of drilling fluids.

The scientist at DOE responsible for formulation and early resolution of potential licensing issues, J. S. Szymanski, grew apprehensive and challenged the notion of a quiescent steady-state description of the site. He proposed a conceptual model wherein

the hydrogeologic system, responding to a variety of tectonic processes and disturbances, can exhibit diverse behavior including discharge of upwelling fluids at the ground surface and innundation of the proposed repository horizon. Neither an internal peer group nor an external review panel convened by DOE resolved the controversy. The external review panel issued majority and minority reports, the latter being the subject of USGS OFR 92-516.

At the request of DOE, the National Academy of Sciences' National Research Council (NAS/NRC) established a Panel on Coupled Hydrologic/Tectonic/Hydrothermal Systems at Yucca Mountain. The Panel issued a report in 1992 that again left the controversy unresolved. While committed to assessing the likelihood that the ground water level could rise to the level of the repository by any plausible geological process, the Panel did not consider a gamut of available geochemical data, and inappropriately discounted hydrothermal systems as a potential mechanism for raising the water table level in the Yucca Mountain area. Among the issues not considered by the Panel are: (a.) the origin of surficial breccia cements that exhibit hydrothermal mineralization and contain zircons with young fission-track annealing ages; (b.) the origin of young zeolites within and below the unsaturated zone; (c.) the occurrence in the vadose zone of calcite veins with high fluid inclusion temperatures and of interstitial fluids with hydrothermal mineralization signatures; (d.) the presence and origin of aberrant strontium isotopic ratios of surficial and subsurface calcites; and (e.) high paleogeothermal gradients reconstructed from oxygen isotopic data for calcites.

In response to a report detailing these issues, the President of NAS declined to question the NRC report's conclusions, based in part on their corroboration by USGS OFR 92-516. However, only two of the issues are addressed in OFR 92-516 (1.) the high paleogeothermal gradient reconstructed from calcite oxygen isotopic data and (2.) the aberrant strontium isotopic ratios of calcites. The oxygen isotopic data are dismissed, and the aberrant strontium isotopic ratios are listed as a remaining problem.

Under the misapprehension that Yucca Mountain is "essentially tectonically dead" (p. 20), the author of OFR 92-516 discounts the hydro-tectonic model by Szymanski (1989) in the following terms:

Of course, the trouble with this model is that all evidence re tectonic activity in the area is unequivocal in establishing the lack of any adequate tectonic activity over the past few million years. (p. 49)

The facts demonstrate otherwise, for example:

Extension in the Yucca Mountain area has been episodic, at times probably involving approximately concurrent slip on several faults and coeval volcanic eruptions in or near Crater Flat. Calcium carbonate and silica were locally deposited from ground water or meteoric water moving into and through fissures previously formed during faulting. Extensional episodes at Yucca Mountain during the late Pleistocene and Holocene apparently had an average period of not greater than 75,000 years. (Fox and Carr, 1989)

Reliance on outdated information would be less objectionable if the author of OFR 92-516 did not presume to represent the views of all scientists, for example:

Everybody who writes about the geological history of SW Nevada agrees that large scale tectonic activity essentially ceased 10-12 Ma ago. (p. 21)

The purpose of this review is to discuss significant data missing from OFR 92-516, so that misinformation does not cloud the controversy and does not contribute to elimination of the legitimate licensing issues and concerns from further consideration.

SAFETY AS EVALUATED BY OFR 92-516

OFR 92-516 represents Yucca Mountain as a site untrammelled by tectonic processes over the past several million years. Essential elements of this viewpoint include the following propositions.

1. Hazards posed by fault activity are negligible, given the "total lack of evidence of Holocene, or even Pleistocene scarps in the area" (p. 19).
2. Even in the event of a magnitude 7 earthquake the rupture "probably will not reach the surface" (p. 23) and disturb the vadose zone. Such an event could not cause significant hydrologic effects because "the physical phenomena associated with a magnitude 7 event in Nevada are those associated with a magnitude 6 event in western California" (p. 22), "putting the Nevada earthquake in a size range where no data of which I know indicate there to be changes in water flow in local streams at the time of the earthquake related to any process." (p. 57)
3. In any case, hydrologic disturbance of the vadose zone from below is prevented by the presence of an impermeable stratum of unfractured rocks, the Calico Hills tuffs, underlying the proposed repository horizon, so that "the mountain has been essentially sealed to upward vertical transport of water for the past 10 Ma." (p. 31)
4. Thermal disturbance of the geohydrologic regime at Yucca Mountain is out of the question because hydrothermal activity was "complete 10-11 Ma ago, with there being no evidence of later significant diagenetic alteration of the tuffs" (p. 29). "There are no available data that support the case for high temperature gradients

under Yucca Mountain in the last few million years." (p. 61)

This evaluation is wrong on all counts. Much of the missing data is documented in DOE reports, including the Site Characterization Plan. The following discussion of the relevant data is based mainly on these reports..

SEISMIC AND FAULT RUPTURE POTENTIAL

DOE (1988) documents Quaternary offsets along four faults in the immediate site vicinity (within 3 km): the Paintbrush Canyon, Bow Ridge (misnamed as Bow Spring in OFR 92-516, p. 21), Solitario Canyon, and Windy Wash faults. Of these faults, the Windy Wash has the most detailed displacement chronology. The apparent vertical offset of Pliocene basalts dated at 2.5-3.7 Ma is 40 meters (Ramelli et al., 1991). Four episodes of displacement over the past 270 ka have been documented, the youngest displaced stratum being a Holocene eolian silt dated by thermo-luminescence method at 3.0-6.5 ka (Whitney et al., 1986). Holocene offset has also been documented on the Solitario Canyon and Black Cone faults (Ramelli et al., 1991) using the radiocarbon accelerator mass spectrometry method.

In light of this data, there is only one obvious inference that can be drawn from these OFR 92-516 remarks:

There is no reason I know of for hypothesizing the potentiality of even minor surface rupturing along this zone within the next 10,000 years. (p. 22)

...the faulting/earthquake threat along the west side of Yucca Mountain imagined in the Minority Report is nonexistent. (p. 22)

Though earthquakes with rupture lengths of a few tens of kilometers appear on

occasion to result in significant modification in spring and stream flow rates in the immediate area above the rupture, this fact has no bearing on expected events at Yucca Mountain there is no prospect of such an earthquake within the next 10,000 years. The argument used by Szymanski to support the case for such an earthquake within 100 years has no merit. (p. 20)

This inference is that, the author of OFR 92-516 is unfamiliar with the site-specific data which have direct relevance to evaluations of the seismic and fault rupture potentials. Having been familiar with such data, he may have concluded that; far from excluding the potentiality of minor surface rupturing, the evidence is, instead, indicative of distributive faulting characteristic of major earthquakes (m-7). In that regard, for example, Ramelli et al. (1991) state:

Multiple lines of evidence suggest that recent surface faulting has involved concurrent rupture of multiple faults, and that surface faulting may accompany local basaltic volcanism. Evidence supporting this hypothesis include the high degree of fault interconnection, similarities in scarp morphology, similarities in ages and amounts of recent offset along multiple faults, and presence of basaltic ash within vertical fractures formed in fault-filling carbonate exposed in trenches across four faults.

The presence of basaltic ash within fault zone fractures is one of the stronger lines of evidence of concurrent rupture of multiple faults (Swadley et al., 1984; Fox and Carr, 1989; Shroba et al., 1990).

Because the ash-filled fissure in the Windy Wash fault cuts all surficial strata except the youngest, a Holocene silt, the ash probably came from an eruption of the Lathrop Wells volcanic center. The Lathrop Wells center, situated 15 km south of the proposed repository site, is the youngest basaltic cone in the area. Two separate Lathrop Wells basalt lavas have been dated by Turrin and Champion (1991) at 144 ± 35 and 183 ± 21 ka (arithmetic means of $^{40}\text{Ar}/^{39}\text{Ar}$ ages), and 119 ± 11 and 141 ± 10 ka (combined weighted means of K/Ar and Ar/Ar ages). Wells et al. (1990) infer that three additional

events have occurred within the past 20 ka, based on soil stratigraphy of eolian sand and silt containing tephra.

The fact that the volcanic ash found in fault-zone fissures is uncemented bears on the origin of carbonate-silica veins that are ubiquitous in the fault zones. It would appear that the supergene process involving rainwater, advocated in OFR 92-516, has not been operational at Yucca Mountain for many thousands of years. Likewise it would appear that the seismic event or events that produced the fissures did not cause groundwater discharge at the four trench sites sufficient to cement the volcanic ash. However, it is possible that groundwater was injected into the vadose zone at Yucca Mountain and precipitated vein calcites that have been dated by Szabo and Kyser (1985) using the uranium series method. Calcites with U-series ages in the range 100-200 ka were obtained from drill hole GU-3 at a depth of 63 meters and from drill hole G-2 at depths of 346.7, 348.7, 348.8 and 359 meters. Adjusted downward by 47% (Schlesinger, 1985) to take into account the alpha-recoil related effect, the ages are respectively 155, 129, 97, 116, and 126 ka, with standard errors of about 20 ka. Younger calcites with adjusted ages of 17 and 20 ka, and standard errors of a few ka, were obtained from drill hole GU-3 and depths of 131 and 331 meters. Thus, hydrologic disturbance of the vadose zone by the event or events associated with the fracture-fillings can not be discounted as being irrelevant to evaluating safety over the next 10 ka.

The distributive faulting evidenced at Yucca Mountain in the late Quaternary has a significant implication as to the maximum earthquake potential. dePolo et al. (1991)

documented the distribution of surface ruptures caused by 11 historic earthquakes in the Basin and Range. All nine events that ruptured multiple geometric or structural segments had magnitudes 6.8 and greater. In another study directed toward determining the highest magnitude of earthquakes not producing surface rupture, dePolo (1991) studied 21 historic Basin and Range earthquakes of magnitude 6 to 7 since 1920. The largest event that did not rupture the surface had magnitude 6.6. The historic record of Basin and Range earthquakes therefore indicates that (1.) at Yucca Mountain, the magnitude of largest late Quaternary earthquake was 6.8 or greater and (2.) all earthquakes of magnitude greater than 6.6 can be expected to produce surface rupture.

POTENTIAL GEOHYDROLOGIC EFFECTS OF CREDIBLE SEISMIC EVENTS

OFR 92-516 misconceives the hydrologic effects of earthquakes, principally in the belief that water can be discharged only from the very shallow crust. This misconception accounts for repeated assertions as to the absence of tectonic stress, and invulnerability to stress change, of the shallow crust. For example, "Yucca Mountain and the depths immediately below are not at measurable tectonic stress" (p. 20) and "any typical Nevada earthquake of magnitude 7 at normal depths will be associated with insignificant stress changes at or within a few kilometers of the surface" (p. 24). These assertions, aside from being false, are misdirected in terms of the phenomena involved in producing water outflows from the source zones of large normal-faulting earthquakes in the Basin and Range.

As stated in the Minority Report, a large part of the volumetric strain associated with a normal-faulting event involves the closure of fractures to considerable depth, at least 5 km, and expulsion of water. The results reported by Wood and King (1992) for the 1959 Hebgen Lake and 1983 Borah Peak earthquakes have no alternative explanation. The events, of magnitude 7.3 and 7.0 respectively, produced within a year outflows of 0.5 and 0.3 km³. These outflow volumes are commensurate with crustal volume strains causing water to be expelled from depths of at least 5 km, and are comparable with the storage capacity of a large water impoundment reservoir.

Outflows of similar scale are inferred from qualitative information (Wood & King, 1992) to have occurred following other major normal-faulting earthquakes in the Basin and Range (the 1982 Sonora, 1915 Pleasant Valley, and 1954 Dixie Valley events) and in the Apennine Chain in Italy. Normal fault earthquakes of lesser magnitude in the Basin and Range have also produced widespread increases in groundwater elevations and outflows. Among these are the 1901 Milford, UT, 1925 Helena, MT, 1933 southern Utah, 1934 Hansel Valley, UT, and 1935 Helena, MT earthquakes (Wood & King, 1992). These events, with magnitudes in the range 6 to 6.6, did not produce primary surface rupture, and only one, the magnitude 6.6 Hansel Valley earthquake, produced secondary rupture (de Polo, 1991).

Outflow of water caused by release of upper crustal strain, as documented for the Hebgen Lake and Borah Peak events, appears to be characteristic of normal-faulting earthquakes. This phenomenon is very likely responsible for the extraordinarily

prolonged and prolific aftershock sequences that are characteristic of the Basin and Range, for example, the aftershock sequence of the 1915 Pleasant Valley earthquake is still in progress. Similarly, earthquake-induced effects on groundwater are likely responsible for the prolific aftershock sequence of the magnitude 5.6 earthquake of June 29, 1992 at Little Skull Mountain, located 20 km southeast of the proposed repository.

In terms of quantitative evaluation of hydrologic effects based on empirical data, the evidence is unequivocal in placing recent paleoseismicity at Yucca Mountain in the same category as the Hebgen Lake and Borah Peak earthquakes, i.e. events producing distributive normal faulting at the surface. For both of the historic events, the outflow was equivalent to the earthquake-induced volumetric strain accumulated to a depth of 5 km. There is no reason to believe that this would not be the case at Yucca Mountain. For Yucca Mountain, where the vadose zone is 0.5 km deep and the proposed repository is 0.3 km deep, the issue is the effect of the outflow into the vadose zone.

OFR 92-516 offers three lines of defense against the possibility of repository flooding. First, the presence of an impermeable, unfractured stratum of rocks, the Calico Hills tuffs, underlying the proposed repository horizon, would divert upwelling water away from the vadose zone. The plausibility of this notion is evaluated later. Second, "the earthquake required to make such deeply derived flow credible in any minds has no possibility of occurrence within the next 10,000 years" (p. 22), a statement that is in sharp conflict with the paleoseismicity of the site. Third, even if a magnitude 7

earthquake were to occur, the rupture "probably will not reach the surface" (p. 23), and "the physical phenomena associated with a magnitude 7 event in Nevada are those associated with a magnitude 6 event in western California, an inadequate event as regards the phenomena of importance to the Minority Report" (p. 22). This third line of defense betrays a remarkable misunderstanding of the problem.

To begin with, absence of surface rupture associated with a magnitude 7 event in the Basin and Range would be unprecedented. As noted above, all historic events of magnitude greater than 6.6 have produced surface rupture, and all nine events of magnitude 6.8 and greater have caused rupture of multiple faults. However, these observations are not considered by the author of OFR 92-516, who chooses to estimate rupture length by working backward from the seismic intensity distributions of historic earthquakes, a scheme ill-suited to a sparsely-populated area with plentiful scarps of past earthquake ruptures. Using this scheme, the author obtains Modified Mercalli Intensity 6.3 (threshold of architectural damage) at Yucca Mountain for a magnitude 7 earthquake directly below (p. 25). This result is discordant with the fact that the magnitude 5.6 Little Skull Mountain earthquake of June 29, 1992 produced structural damage (Intensity VIII) to DOE's Field Operations Center of the Yucca Mountain Project, a building on NTS located a few km from the earthquake epicenter.

In regard to documented hydrologic effects, the author's comparison of a magnitude 7 event in Nevada with a magnitude 6 event in western California would be appropriate if the events were interchanged. As noted by Wood and King (1992) "Strike-slip, and

oblique-slip fault movements are associated with a mixture of responses but appear to release no more than 10% the water-volume of the same sized normal fault event." Evidence supporting this conclusion includes the observation of a discharge of only 0.025 km³ of water following the magnitude 7.5 oblique thrust earthquake in Kern County in 1952, as compared with 0.5 and 0.3 km³ recorded for the normal-faulting events at Hebgen Lake and Borah Peak. The observed mechanism-dependence of post-seismic hydrologic effects confirms the volumetric-strain model of Wood and King (1992).

If it were true, as claimed by OFR 92-516 (p. 22), that rupture lengths of Nevada earthquakes are five times shorter than those of western California events of the same magnitude, this would make matters even worse for the proposed repository because the outflow of water would have much greater spatial concentration. Allowing that down-dip widths of normal ruptures in the Basin and Range are about twice the rupture widths of western California earthquakes, the fault displacement of the Nevada event would be 2.5 times greater than for a western California earthquake of the same moment, and the volumetric strain, and hence, water outflow from the source zone would be correspondingly more concentrated.

PROPERTIES OF THE CALICO HILLS TUFFS

OFR 92-516 calls upon the Calico Hills tuffs to seal the vadose zone against upward flow of groundwater resulting from a tectonic event (seismic and/or thermal). The sealing quality of the Calico Hills tuffs is deduced from water yield as a function of depth

in well UE-25p#1 (p. 29), and the statement that calcite is absent from the zeolitized tuffs of Calico Hills (p. 30). The latter statement does not appear to be true, as calcite is reported to occur in the Calico Hills, for example at a depth of 468.6 meters in drillhole USW G-3 (Bish and Chipera, 1989).

Fracture frequencies have been measured for the Calico Hills tuffs both at the surface, where they outcrop at Prow Pass, and in drillholes USW-G-1, USW G-3, USW G-4, and UE-25a#1 (DOE, 1988, P. 1-36). Fracture frequencies were found to be 1.2 per cubic meter at Prow Pass, and from 0.2 to 2 per cubic meter in the drillholes. While these fracture frequencies are several times lower than in other tuffs, the Calico Hills tuffs can not be described accurately as fracture-free.

Stock et al. (1985) show televiewer images of examples of fractures in the Calico Hills tuffs in drillhole USW G-2, both steeply dipping subparallel fractures in the interval 594.4-602.0 meters and shallowly dipping fractures in the interval 609.3-617.2 meters. Images of drilling-induced hydrofractures are shown for the interval 660-668 meters: these are very high angle, nonthroughgoing fractures that are prominent in the depth interval 526-678 meters. Stock et al. describe these as "an echelon subparallel fractures merging into one another along a strike of N25°E to N30°E" and infer that the axis of minimum compressive stress is oriented N60°W. These are interpreted as drilling-induced hydrofractures because some of the corresponding core sections are unfractured. While stress measurements in this section were not conducted because of the complete loss of circulation, results obtained nearby show that "the measured stresses at Yucca Mountain are near the limit of those required to cause slip on

favorably oriented pre-existing faults" (Stock et al., 1985). The loss of drilling fluid into opening fractures in the Calico Hills tuffs is inauspicious for a formation regarded as an impermeable seal.

From the characterization of the Calico Hills tuffs as "a zone essentially impermeable to upward movement of groundwater from depth" (p. 30), one might expect temperature gradients in the Calico Hills tuffs to be free of the aberrations afflicting geotherms in other ignimbrite formations, however, this is not the case. Eight geotherms measured by Sass et al. (1988) over a 2.5 year interval in drillhole USW G-2 show that the lower Calico Hill tuffs, in the depth range 620 to 800 meters, are essentially isothermal at 32°C. Above the Calico Hills tuffs there is a modest geothermal gradient and the conductive heat flow is 44 mW/m². Below the Calico Hills tuffs the geothermal gradient is much steeper, and the conductive heat flow is 71 mW/m².

No unique interpretation of the USW G-2 geotherm is offered by Sass et al. (1988). In one case, the difference in heat flow between the unsaturated and saturated zones is attributed to downward percolation of groundwater at 20 mm/year. In another case, the high geothermal gradient at depth is explained as a transient chilling effect of water moving vertically downward through the isothermal lower part of the Calico Hills tuffs, or moving laterally with a downward component of velocity. In any case, water is moving through the Calico Hills tuffs.

The direction of water flow is important to the case in OFR 92-516 because the author

claims that the Calico Hills tuffs are impermeable only to upward vertical flow of water (p. 30-31). A novel current-rectifying quality is attributed to the permeability. Downward vertical flow of water is needed to explain the crystallization of "pedogenic" carbonates, those with low concentrations of carbon-13, found 300 meters below the present water table in drillhole USW G-4. According to OFR 92-516, during a "transient lowering of the WT" (p. 36), rainwater with the right dissolved constituents percolated vertically downward through the Calico Hills tuffs and crystallized calcites in veins in the underlying Crater Flat tuffs.

Fortunately, there is an alternative and much less demanding explanation for the carbon isotopic characteristics of calcite-silica veins present hundreds of meters below the present water table, namely the source of the carbon isotopically "light" is igneous CO_2 . This explanation can also account for the carbon-13 depleted calcites in the vadose zone, the high homogenization temperatures of fluid inclusions contained therein, and the texture and mineralization of breccias, described by Hansen et al. (1987) as "hydrothermal eruption breccias" found in fault zones at Yucca Mountain.

Within the Calico Hills tuffs there are zeolites whose presence is inexplicable in the framework of the notion that "the mountain has been essentially sealed to upward vertical transport of water for the past 10 Ma" (OFR 92-516, p. 31). Potassium-argon ages have been obtained for six clinoptilolites from the Calico Hills tuffs, and these range from 1.99 to 4.64 Ma (WoldeGabriel, 1991). The clinoptilolites bear evidence of recent flow of hot (50-100°C) alkali-earth solutions through the Calico Hills tuffs. Five of

the dated samples are from the vadose zone in drillholes USW G-1, USW G-2, and USW G-4, and the sixth was obtained more than 200 meters below the water table in drillhole USW G-2. It is perplexing that OFR 92-516 makes mention of K/Ar ages of illites (10.9 ± 0.6 Ma) from drillholes USW G-1 and USW G-2 without mentioning the clinoptilolite ages, particularly because WoldeGabriel (1991) reports ages both for illites and clinoptilolites from USW G-2. After citing the illite ages, OFR 92-516 claims that "No available data suggest a later period of elevated temperature and associated diagenesis" (p. 28) referring to the Timber Mountain hydrothermal episode 10 Ma ago. This statement can not be supported in terms of the rules of evidence traditionally applied in the licensing of nuclear facilities in the United States. Further, the term "diagenesis" does not properly characterize the post-Timber Mountain alkali-earth metasomatic alteration of Yucca Mountain.

PALEOGEOTHERMOMETRY AND MINERALIZATION OF THE VADOSE ZONE

OFR 92-516 treats paleogeothermal data on Yucca Mountain in much the same way as it treats the paleoseismic data, by flat denial:

There are no available data that support the case for high temperature gradients under Yucca Mountain in the last few million years. (p. 61)

The presence of young zeolites within and below the vadose zone is just one of several lines of evidence for high temperature gradients in the last few million years. Unlike the alkali zeolitization of the Timber Mountain hydrothermal episode, the younger zeolitization represents alkali earth metasomatic alteration. This can be ascertained by comparing zeolite and whole-rock chemistry with that of the original glass. The data, in

Table 1, from Broxton et al. (1986), are for the sample containing the clinoptilolite dated at 4.64 Ma, discussed previously, from the Calico Hills tuffs at a depth of 740 meters in drillhole USW G-2.

Table 1

		<u>Weight Percent</u>			
		MgO	CaO	Na₂O	K₂O
Glass, Calico Hills (25 samples)		< .01	0.48	3.29	5.14
			±.05	±.13	±.23
Whole-Rock, USW G-2 2430		0.41	1.98	1.68	3.95
Clinoptilolite, USW G-2 2430		0.36	2.77	0.64	2.81
		0.31	3.08	0.72	2.71

The high degree of exchange of alkali earth for alkali elements shows that the alteration is metasomatic, not diagenetic. Another important difference between the young alkali-earth zeolitization and the alkali Timber Mountain zeolitization, which is pervasive throughout the lower part of the stratigraphic section, is that it occurs in aureoles around fractures in the upper part of the stratigraphic section. The alkali-earth zeolitization is not uniform spatially: it is shallower to the northwest and deeper to the southeast of Yucca Mountain. Szymanski (1992) has pointed out the conceptual difficulties involved in explaining this space-differential alkali-earth metasomatism in terms of the downward percolation of rainwater. Instead, he explains this fracture-based alteration in terms of tectonic events on the Stagecoach Road fault, whose trace lies a few kilometers

southeast of the proposed repository, and which is regarded as the master fault of a system that includes the Paintbrush Canyon, Bow Ridge, Ghost Dance, Solitario Canyon, and Windy Wash faults. Contrary to the claims of OFR 92-516, there is overwhelming evidence of episodic post-Timber Mountain metasomatic alteration of tuffs within and below the vadose zone. The evidence for post-Timber Mountain alkali-earth metasomatism can not be dismissed lightly as supergene in origin because it comprises countless millions of tons of rocks at Yucca Mountain.

Further evidence of elevated temperatures subsequent to the Timber Mountain hydrothermal episode has been obtained from fission-track ages of zircons found in breccia cements at Trench #14 and at Busted Butte, within the Bow Ridge and Paintbrush Canyon faults, respectively. The zircon ages date the most recent annealing of fission tracks, at temperatures above about 200°C, and establish upper bounds on the breccia ages. Levy and Naeser (1991) reported ages for twelve zircon crystals in samples from each of the two fault zones, and concluded:

The spread of ages from each samples 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.

The most recent annealing of fission tracks in zircons may have been caused by hydrothermal solutions that produced the post-Timber Mountain alkali earth zeolitization and carbonatization of Yucca Mountain. This inference is supported by the results of mineral assays of Trench #14 breccias (Weiss et al., 1990) showing significant enrichment in base and noble metals relative to the stratigraphically equivalent background.

The third set of paleogeothermal data missing from OFR 92-516 are homogenization temperatures of fluid inclusions in vein calcites from drill cores. In drillhole USW G-3, homogenization temperatures exceeding 100°C were obtained for calcites from depths of 31 and 131 meters (Bish, 1989). Although Bish suggested that these inclusions probably formed during initial deposition or cooling of the tuffs, this seems unlikely in view of young uranium-series ages obtained for nearby calcite specimens in USW G-3. Ages reported by Szabo and Kyser (1985) for specimens obtained at depths of 19, 40, 97 and 101 meters are 227 ± 20 , 26 ± 2 , >400 , and 30 ± 4 ka, respectively.

The fourth set of paleogeothermal data missing from OFR 92-516 are the results of chemical analyses of intersitial fluids from the Yucca Mountain vadose zone. Samples of water residing in pore space have been separated from volcanic rock cores from two shallow dry-drilled boreholes in the unsaturated zone (the rock above the water table where the repository would be situated) (Smith, 1991). Chemical analysis of water shows that it is mineral water, which welled up from the carbonates and other Precambrian rocks underlying the volcanics (Szymanski, 1992). Mineral enrichment of this water, relative to water residing in fractures in volcanic rocks below the water table, is illustrated in Figure 11 of Somerville et al., (1992). This water resembles water residing in carbonate fractures, as indicated by the ten-fold enrichment of calcium and magnesium. The enrichment in trace elements, including the rare earth elements and base and noble metals, indicates a hydrothermal source. In addition to the overall enrichment in rare earth elements (REE), there is an unusual enrichment of heavy REE relative to light REE (Smith, 1991). In contrast, the host volcanics have the usual

relative enrichment of light REE (Scott and Castellanos, 1984). Enrichment of heavy REE is observed for hydrothermal solutions that are concentrated and rich in carbon dioxide. The heavy REE enrichment mechanism is believed to involve carbonate anion complexing. This observation supports the interpretation of the role of CO_2 in forming the mosaic breccias. Infiltrating rainwater cannot explain either the enrichment of trace elements or the enrichments of REE found in the interstitial fluids above the water table. The only reasonable explanation is that the interstitial fluids are remnants from the previous invasion(s) of the repository horizon by waters of hydrothermal origin.

Finally, the only paleogeothermal results considered in OFR 92-516 are rejected by the author. The results are oxygen isotope data for calcites, reported by Whelan and Stuckless (1992), which they interpret in terms of a minimum paleogeothermal gradient of $34^\circ\text{C}/\text{km}$. This is approximately 50% higher than the present-day gradients of 18 - $24^\circ\text{C}/\text{km}$ measured by Sass et al. (1988) in the drillholes yielding the calcites analyzed for oxygen isotopes. The author of OFR-92-516, to his credit, perceives the difficulty in reconciling the high paleogradient obtained from the calcites with the belief that they crystallized from infiltrating rainwater. However, it is difficult to accept his solution to the dilemma, namely to reject the paleogradient results:

Thus, the overall suggested interpretation given by Whelan and Stuckless must almost certainly be wrong. (p. 37)

One rationale given for rejecting the paleogradient results consists of (1.) removing from consideration calcites more than 200 meters below the water table by assuming that they are 10.5 million years old and therefore unrelated to the recent carbonatization of Yucca Mountain and (2.) stating that there are then too few remaining data to resolve

the paleogradient at shallower depths. A second rationale given for discounting the significance of the 34°C/km paleogradient is its presumed transience:

The persistence of such a high gradient seems doubtful as the normal gradient in the Basin and Range area is only 20°C/km. (p. 37)

The problem with this statement is that it does not distinguish correctly between normal and aberrant temperature gradients in the Basin and Range. The paleogradient of 34°C/km is normal for the Basin and Range (DOE, 1988), while the present-day gradient of about 20°C/km at Yucca Mountain is abnormally low, constituting a pronounced anomaly of conductive heat flow (Sass et al., 1988). To rephrase the OFR 92-516 statement:

The persistence of such a low gradient seems doubtful as the normal gradient in the Basin and Range is 30-40°C/km. (p. 37)

It is precisely the transience of the thermal regime, and the hydrologic and stress regimes, that prompts concern about the suitability of the Yucca Mountain site to safely accommodate a high level nuclear waste repository, Szymanski (1989) and Archambeau and Price (1991).

STRONTIUM ISOTOPIC RATIOS

The first of the two issues that OFR 92-516 lists as remaining problems is the strontium isotopic ratio of calcites in Trench #14. This problem is listed as "source of dust" (p. 63), with the premise being that the calcites crystallized from rainwater that had dissolved calcereous dust, presumably derived from the local Paleozoic carbonates. The difficulty is to explain the crystallization of calcites with a strontium isotopic ratio ($^{87}\text{Sr}/^{86}\text{Sr}$) of 0.7125 from rainwater that has dissolved Paleozoic carbonates with a strontium isotopic ratio of 0.709. The problem as posed by OFR 92-516 is to discover an "atmospheric"

"process" that solves this isotopic discord. OFR 92-516 is correct in identifying Precambrian material as the missing ingredient. The problem is the availability of the "Precambrian dust thrown in for good measure" (p. 64).

This problem vanishes if, instead of appealing to the atmosphere for the source of strontium, one appeals directly to the source in the Precambrian basement underlying Yucca Mountain. This, of course, involves accepting a hypogene origin of the calcites.

GRADIENT OF HYDRAULIC POTENTIAL

The second issue that OFR 92-516 lists as a remaining problem is the potentiometric gradient in northern Yucca Mountain, where the water table rises some 300 meters in elevation between drillholes USW G-1 and G-2. The author frankly admits his inability to contribute to the resolution of the problem. It seems unlikely that the problem can be resolved in the framework of the author's "tectonically dead" model of Yucca Mountain. The prospects of resolution are better if one admits inhomogeneous tectonic strain accumulation (Szymanski, 1989).

CONCLUSION

While this review has not covered all the points made in OFR 92-516, it has addressed several of the topics on which the author asserts his strongest convictions, reinforced by repetition. As we have seen, these convictions are at variance with the relevant data compiled by DOE (1988) and other project documents. Concerns as to site suitability, as expressed in both the Minority Report and the report to the President of NAS, are in

no way allayed by OFR 92-516. On the contrary, the revisionism of OFR 92-516 relative to project data causes yet graver concern, as does the National Research Council's use of OFR 92-516 in its attempt to dismiss the Minority Report and to defend its own report, *Ground Water at Yucca Mountain: How High Can It Rise?*

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