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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

by

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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 a two-man Minority Report, the latter report being authored by Charles B. Archambeau and Neville 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 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 deemed 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 Spengler, 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.

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Report

A. 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 $\delta^{13}C$ versus $\delta^{18}O$ 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 mammouth 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 (>= 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 (>= .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 occassionally 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 gullying 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 movment 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.

B. Surficial 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 (3Kmql).

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> 3Kmql -- 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 gm/sq cm)/(450 thousand years) = 0.24 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).

3) The Ca^{++} content of rainfall in the area is 4 mg/liter or higher (Junge and Werby, 1958).
4) These numbers yield 0.20 gm/sq cm/thousand years.
5) In 450,000 years (490 - 40), that's 90 gm/sq cm.
6) All analyses agree that we are presently in a time of extreme aridity as regards the last 500,000 years. Thus, rainfall has certainly had a higher average value over the last 500 Ka than is being recorded today.
7) Thus, the rate of soil carbonate accumulation (0.24 gm/sq cm/thousand years) has been so slow that even rainfall could in principle have supplied all of it (.20 gm/sq cm/thousand years x (something > 1)).
8) As regards rates of dust accumulation. The Minority Report considers present rates of accumulation as the proper normative value to use when discussing Pleistocene accumulation rates, concluding from the rate they assume to apply that accumulation of thick calcretes is impossible by such a mechanism. However
    a> There is strong evidence for global increase in windiness during full glacial and pluvial climates, the evidence consisting of dust in marine sediments, dust in cores of Antarctic ice, and loess on the continents. Therefore, modern rates of dust accumulation are certainly well below the average for the last 500,000 - 1,000,000 years.
    b> As elaborated elsewhere in this report, development of some massive pedogenic calcite deposits in SW Nevada is unequivocally linked to availability of carbonate-laden dust. Development of thick pedogenic calcite deposits on non-calcareous rocks downwind from sources of calcareous dust, as well as failure to develop such pedogenic deposits on rocks of any type when no down-wind source is available, demonstrate unequivocally the role of wind-blown calcareous dust in the formation of pedogenic calcite deposits.
    c> At an abandoned mine site (Carrera) south of Beatty on the east side of US 95, there is an exposed concrete foundation about a foot high. The volume within this foundation wall is now about half full of wind-blown dust. This deposit was sampled near its mid-line in order to get an average thickness. A volume 10 x 10 x 6.25 cm^3 weighted 800.2 grams and was about 9% carbonate or .12 gr/cm^2 per cm of thickness. This dust has been accumulating for approximately 50 years. In 450,000 years (age span of Trench 14 calcretes), .12 x 450,000/50 =1080 gm of carbonate per cm^2 of surface might have accumulated, i. e., ten times the rate of accumulation of pedogenic carbonate at Trench 14 (110 $gm/cm^2/450$ Ka, see above). One may well argue with the details of this calculation, but it seems abundantly clear that dust certainly could have supplied all of the carbonate to Yucca Mountain.
9) Thus, when the contributions of dust are added to the rain-supplied soil carbonate budget, it is obvious that these processes can have supplied the requisite amount of soil carbonate at Trench 14.

c) Conclusion -- There is no validity to the argument of the Minority Report.

d) The argument can be made that the extant aquifer waters could not develop the Trench 14 carbonates. Carbonate concentrations in these waters are so low that no carbonate veins could form from their rising and deposits would probaby not form if these aquifer fluids did reach the surface (see later discussion of chemistry of the Yucca Mountain Tertiary and Paleozoic aquifers). In fact, it could be argued that the existence of carbonate veins within the vadose zone requires downward moving carbonate-laden waters and associated evaporation in the unsaturated vadose zone, the exact opposite of the unsupported pronouncement of the

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 handlens 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.

C. "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 $\delta^{13}C$ 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.

D. 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).

E. Significance 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.

F. 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.

G. 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 clinoptololite, 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 clinoptololite, mordenite, opal and cristobolite. 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 varaible 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 10^5 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 -- $\delta^{13}C$ 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 $\delta^{13}C$ values of -3 or thereabouts.
3) increasing levels of wetness and cooler, but always with evaporation exceeding precipitation -- dominantly C4 biomass and $\delta^{13}C$ 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° 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 $P(CO_2)$ values.

4) The high $P(CO_2)$ 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.

H. Data from boreholes on and near Yucca Mountain - Part 2

1. Sr ratio ($\delta^{87}Sr/^{86}Sr$) 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	Ratios: Deposit or Aquifer	Ratios: Wall-rock
Surficial - "pedogenic"	.71233±0.00028 [75]	----
Vadose zone (d <=400 m)	.71215±0.00034 [12]	.716
Vadose zone (48/84 m ab. WT)	.71098 [4]	.713
[Tertiary aquifer]	.7107 ±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 δ13C 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 $\delta^{13}C$ 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 $\delta^{13}C$ 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 $\delta^{13}C$ 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> $\delta^{13}C$ values in the present Tertiary aquifer are -6 to -9 %., values in great disagreement with the $\delta^{13}C$ 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 minumum 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 metosomatic 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> $\delta^{18}O$ values in both the Tertiary aquifer and Paleozoic aquifers are -13 to -14 %., and that

2> $\delta^{18}O$ 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 ($\delta^{18}O$ of +23 %. @ 50° C, +28 %. @ 25°, +30 %. @ 15° and +32 %. @ 5°, thus giving the values of Figure 1B. Quade and Cerling use $\delta^{18}O$ values re PDB while Whelan and Stuckless use values re SMOW. The approximate conversion formula is

$$\delta^{18}O_{SMOW} = \delta^{18}O_{PDB} + 31)$$

4> All of these waters would give carbonate precipitates with the same $\delta^{18}O$ 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 $\delta^{18}O$ values comparable to those only 500 meters below the WT.

2) Might warm fluids have risen, cooling as they rose, thus giving the observed $\delta^{18}O$ 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 $\delta^{13}C$ values. Waters of the present Tertiary aquifer would if cooled to 15° C give $\delta^{13}O$ 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 $\delta^{13}C$ 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 x 10^5 years) daughter product of ^{238}U (half-life = 4.5 x 10^9 years. In a system that has been closed for an adequate length of time (few hundred thousand years), the $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ and $^{230}Th/^{234}U$ nuclear activities of all samples and plot them on a figure with the two activity values as coordinates.
7) Compare the implied initial $^{234}U/^{238}U$ value with that of natural waters from various sources.

b) Data:

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

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

The Paleozoic aquifer as sampled has a $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ vs. $^{230}Th/^{234}U$ figure follow an evolution curve with an initial $^{234}U/^{238}U$ activity of 1.4 - 1.5, i. e., within the range of natural soil waters and definitely not approaching the $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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^{-}, $P(CO_2)$, 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 transitting 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^-, $P(CO_2)$, 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 $P(CO_2)$ 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 $P(CO_2)$ 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

$$a(H_2CO_3) = K(CO_2) \times P(CO_2).$$

where "a" refers to chemical activity.

"Thus for every $P(CO_2)$ there is a corresponding $a(H_2CO_3)$ and for every $a(H_2CO_3)$ there is a corresponding $P(CO_2)$. In the literature, it is quite common to report $a(H_2CO_3)$ as the corresponding $P(CO_2)$ 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 $a(H_2CO_3)$ 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 $\log(P(CO_2)$, 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

$$K^* = K_1 x K_{cal} x K_{CO}2/(K_2 x \tau(Ca^{++}) x \tau^2(HCO_3^-)),$$

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 ± .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

$$m_{Ca^{++}}(2m_{Ca^{++}} + M)2 = P(CO_2) x K^*$$

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 $P(CO_2)$, 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 $P(CO_2)$ as solid squares, and observed M versus $P(CO_2)$ as solid circles, the latter values expressing the M values consistent with Ca^{++} saturation in the presence of solid $CaCO_3$ at the calculated $P(CO_2)$ (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 overlie 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 $P(CO_2)$ 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 $P(CO_2)$ values as related to actual pressures forcing retention of H_2CO_3 in solution, it might be expected that $P(CO_2)$ (or $a(H_2CO_3)$) 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, $P(CO_2)$ 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 $P(CO_2)$ falls to that appropriate to water in contact with the atmosphere (the dashed vertical line at $10^{-3.5}$ $P(CO_2)$, 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, in 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 $P(CO_2)$ 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 $P(CO_2)$ but at 15° C, the aquifer would be strongly undersaturated. If it degassed to a $P(CO_2)$ 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 $P(CO_2)$ 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 $P(CO_2)$ when in contact with the atmosphere, the evidence is that a calculated $P(CO_2)$ 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 Meadowsand Devil's Hole.

I. 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 Devils'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 marshall 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).

J. 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 Quasternary 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 ignumbrites, 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 SiO2 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 $P(CO_2)$, 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, gullying 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 their 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 discuused 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 unex-posed 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 misinterpreta-tion. 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 veins .....at 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 indicated ....Here 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 CO_2 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 redddish 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 is 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 concominant 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 SiO_2, 117 ppm of Na^+, 55 ppm of K^+, 72 ppm of Mg^{++}, 351 ppm of Ca^{++}, <.01 ppm of Fe, 412 ppm of HCO_3^-, 744 ppm of SO_4^{--}, 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 $^{234}U/^{238}U$ 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 ($^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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 $^{234}U/^{238}U$ 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/cm^3, the rate of accumulation of carbonate at this site would be (30 x 2.5 / 250) or 0.30 gm/cm^2/1000 years, 75 gm/cm^2/250 Ka, or 135 gm/cm^2/450 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. Lieing 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.

K. 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 $^{87}Sr/^{86}Sr$ 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 $^{87}Sr/^{86}Sr$ 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	$^{87}Sr/^{86}Sr$
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 $^{87}Sr/^{86}Sr$ values in the soluble fractions of playa clay samples has yielded the following 5 values:

Site	$^{87}Sr/^{86}Sr$
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 $^{87}Sr/^{86}Sr$ 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 $^{87}Sr/^{86}Sr$ values than the associated limestone, thus attesting to introduction of high isotopic ratio Sr by atmospheric processes, consider the following data (paired $^{87}Sr/^{86}Sr$ values from limestone and attached calcrete):

Formation	$^{87}Sr/^{86}Sr$	
	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.

L. 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 support-ed); 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 charac-teristics of max developed $CaCO_3$	Discrete stratiform;mounded, or draped strata; commonly display-ing vegetative molds or vugs	Continuous layers underlain by bedrock or a plugged horizon	Continuous laminar layers that have formed plates
Change in con-centration 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 distin-guishing petro-graphic & mine-ralogic charac-ters	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

Factor	Non-Pedogenic	Pedogenic	Observed in Trench 14
$CaCO_3$ crystallinity & percent	Coarse sparry calcite crystals, microsparite, and sparite; crystals > 99.5 % pure	Microcrystalline (micrite), crystallic b-fabric; commonly clay, $MgCO_3$, and opaline SiO_2 present; << 99.5% pure	Microcrystalline, crystallic 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
$\delta^{13}C$ vs $\delta^{18}O$ in $CaCO_3$ (see note below)	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 $CaCO_3$
(Note: $\delta^{13}C$ is vegetation dependent and $\delta^{18}O$ is dependent on mineralization of $CaCO_3$ and fluid source)			
δD vs $\delta^{18}O$ in CaCO3	Shift in $\delta^{18}O$ concentrations away from the concentrations of meteoric water	No shift in $\delta^{18}O$ 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 $^{87}Sr/^{86}Sr$ ratios within range of independently obtained samples from ground water, spring water, spring deposits, limestone or volcanic tuffs, or both	Expected $^{87}Sr/^{86}Sr$ ratios within the range of independently obtained samples from soils developed on stable alluvial surfaces or from eolian samples	$^{87}Sr/^{86}Sr$ concentrations in the slope-wash alluvium and veins are similar to independently obtained soil and eolian samples
(Note: geochemical analogue of Ca^{++}; indicates the isotopic composition of the rocks with which the water that precipitated the $CaCO_3$ was in contact)			

Factor	Non-Pedogenic	Pedogenic	Observed in Trench 14
Useries 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 precipi 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

-------- 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 RNGE
J-12	954	225	347	729	607	668	27	122
J-13	1011	282	1063	729	-52	338	31.5	781
UE-25B#1	1200	470	1220	730	-20	355	36	750
UE-25B#1	1200	470	1220	730	-20	355	36	750
UE-25B#1	1200	470	1220	730	-20	355	37.2	12
UE-25C#1	1131	400	914	731	217	474	41.5	514
UE-25C#2	1132	401	913	731	219	475	40.5	512
UE-25C#3	1132	402	913	730	219	474	40.8	511
UE-25P#1	1114	381	1805	733	-691	-283	56	200
UE-29A#2	1215	29	422	1186	793	915	25.1	107
UE-A9A#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	MAXRWT	WT (200-400)	TOTAL WT	GRADIENT °C/km.
ALL	1	1	28	19.0
9 of WT 1	25	0.17	10.5	18.6
(Figure 3)	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	25	0.17	9.5	12.8
(Figure 4)	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	25	0.17	6.5	3.8
(Figure 5)	50	0.09	5.7	5.9
	200	0.02	5.2	11.9
	1000	0.005	5.04	17.7

MAXRWT = 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* mmol/l^3/atm
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
USW-G4	INT	1270	541/915	729	35.6	0.324	0.75	2.278	7.7	0.00313	1.767	3.66	1.639
USW-H1	572-687	1302	572/1829	730	33	0.133	0.729	1.942	7.6	0.00328	1.672	2.88	1.562
USW-H1	687-1829				34.7								
USW-H3	822-1220	1483	../1220	(730)	26.5	0.02	0.716	4.491	9.2	0.00015	4.147	5.79	1.828
USW-H4	INT	1249	519/1220	730	34.8	0.424	0.766	2.835	7.4	0.00739	2.211	4.71	1.735
USW-H5	INT	1478	704/1220	774	35.6	0.049	0.799	2.073	7.9	0.00206	1.949	2.99	1.574
USW-H6	INT	1302	526/1220	776	37.8	0.088	0.799	3.458	8.2	0.00148	2.671	4.67	1.731
USW-H6	753-835				41.6								
USW-H6	606-646				37.2								
USW-VH1	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	1297-1805	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	SiO2	HCO3	PH	Mg	Na	K	Cl	SO4	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

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

Figures

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

Figure 1A

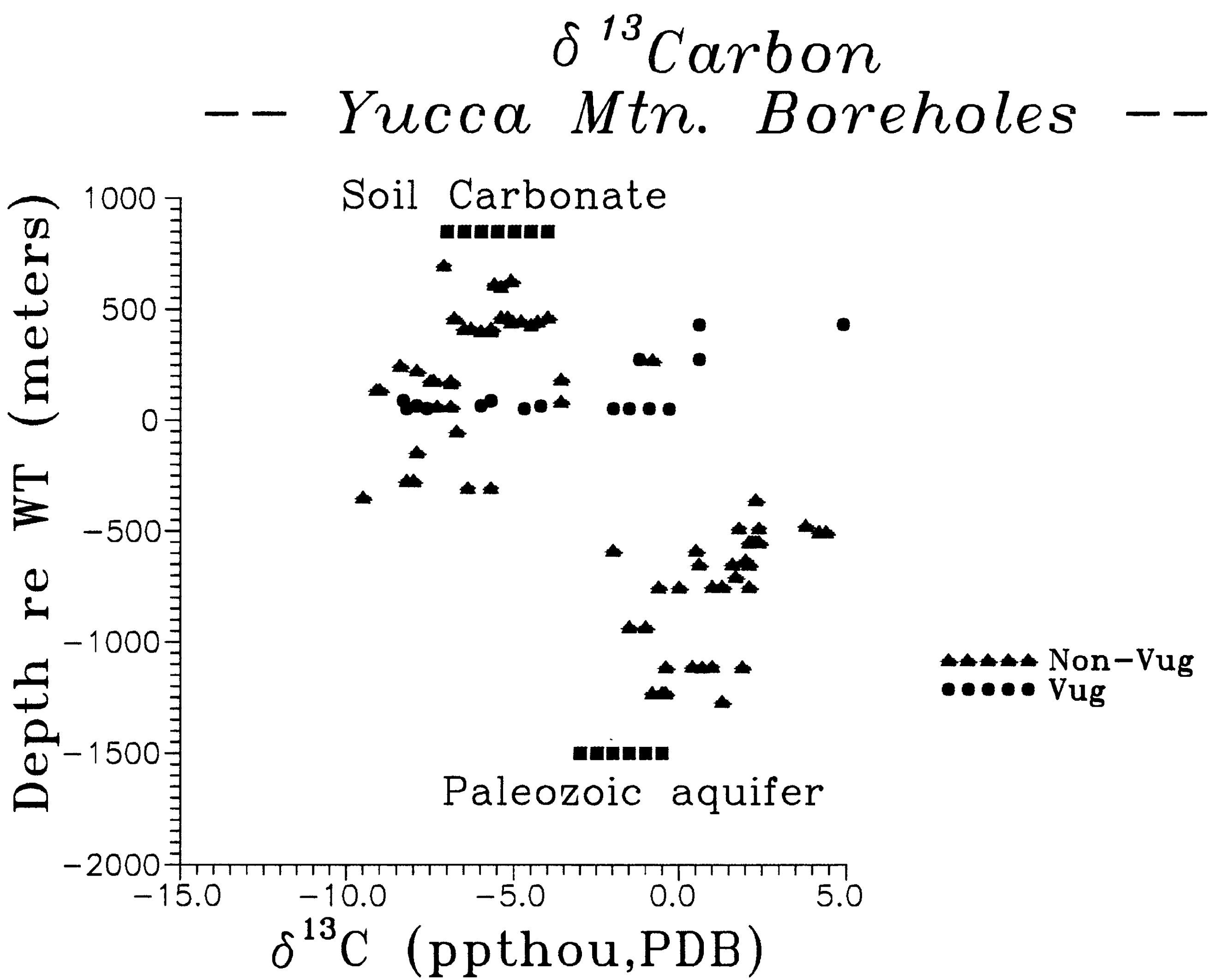

δ 13Carbon
-- Yucca Mtn. Boreholes --
Soil Carbonate
Paleozoic aquifer
Depth re WT (meters)
1000
500
0
−500
−1000
−1500
−2000
−15.0
−10.0
−5.0
0.0
5.0
δ13C (ppthou,PDB)
Non−Vug
Vug

Figure 1B

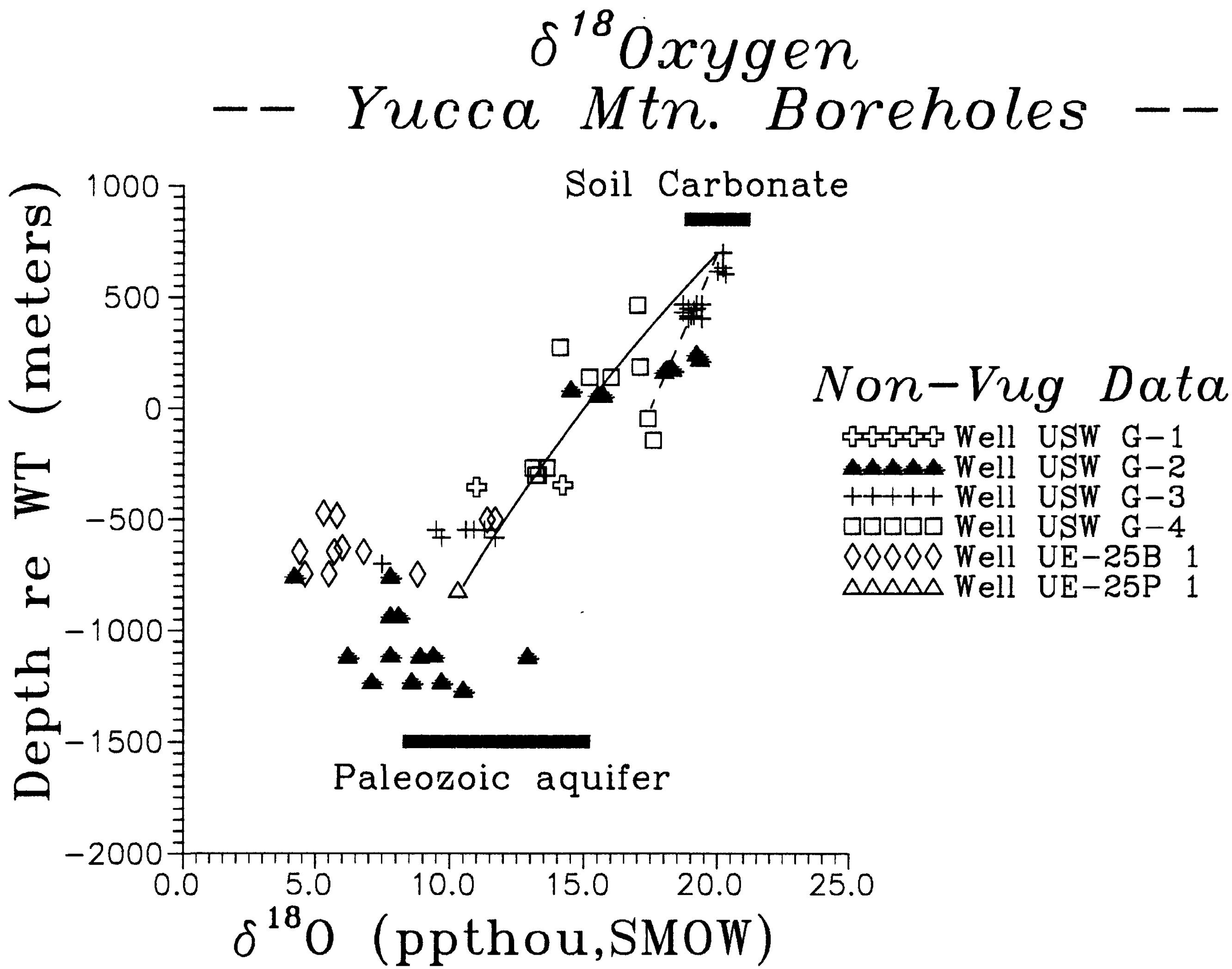

δ18 Oxygen
-- Yucca Mtn. Boreholes --
Soil Carbonate
Paleozoic aquifer
Depth re WT (meters)
1000
500
0
-500
-1000
-1500
-2000
0.0
5.0
10.0
15.0
20.0
25.0
δ18O (ppthou,SMOW)
Non-Vug Data
Well USW G-1
Well USW G-2
Well USW G-3
Well USW G-4
Well UE-25B 1
Well UE-25P 1

Figure 2

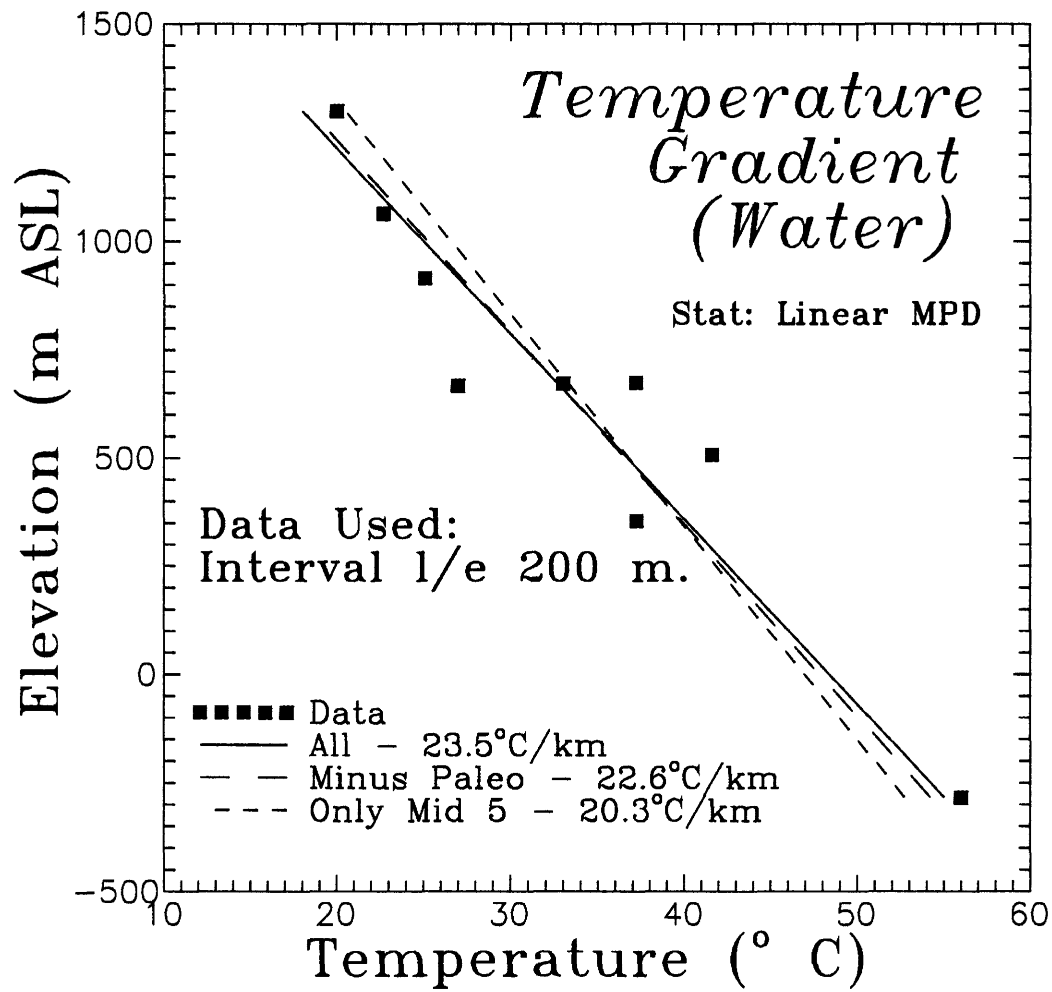

Temperature
Gradient
(Water)
Stat: Linear MPD
Data Used:
Interval l/e 200 m.
Data
All - 23.5°C/km
Minus Paleo - 22.6°C/km
Only Mid 5 - 20.3°C/km
1500
1000
500
0
-500
10
20
30
40
50
60
Elevation (m ASL)
Temperature (° C)

Figure 3

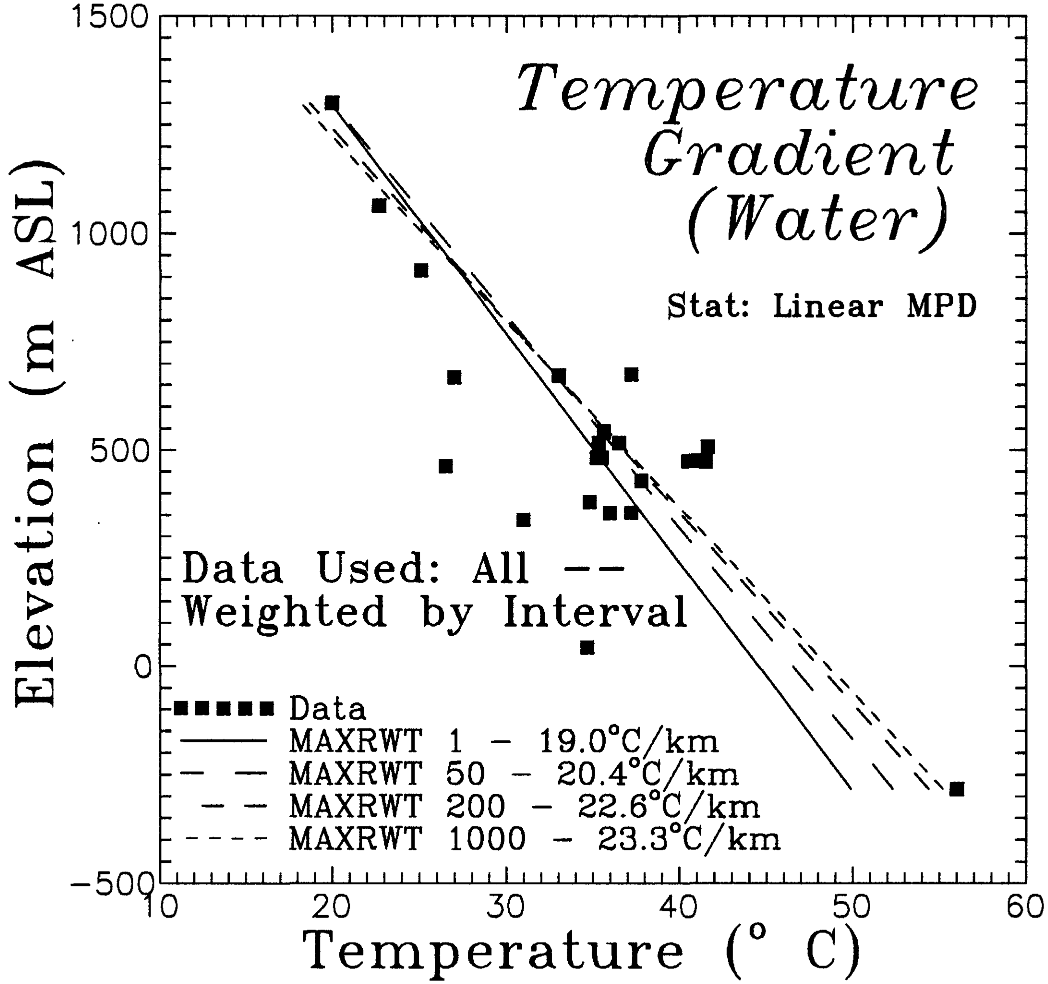

Temperature
Gradient
(Water)
Stat: Linear MPD
Data Used: All --
Weighted by Interval
Data
MAXRWT 1 - 19.0°C/km
MAXRWT 50 - 20.4°C/km
MAXRWT 200 - 22.6°C/km
MAXRWT 1000 - 23.3°C/km
Elevation (m ASL)
Temperature (° C)
1500
1000
500
0
-500
10
20
30
40
50
60

Figure 4

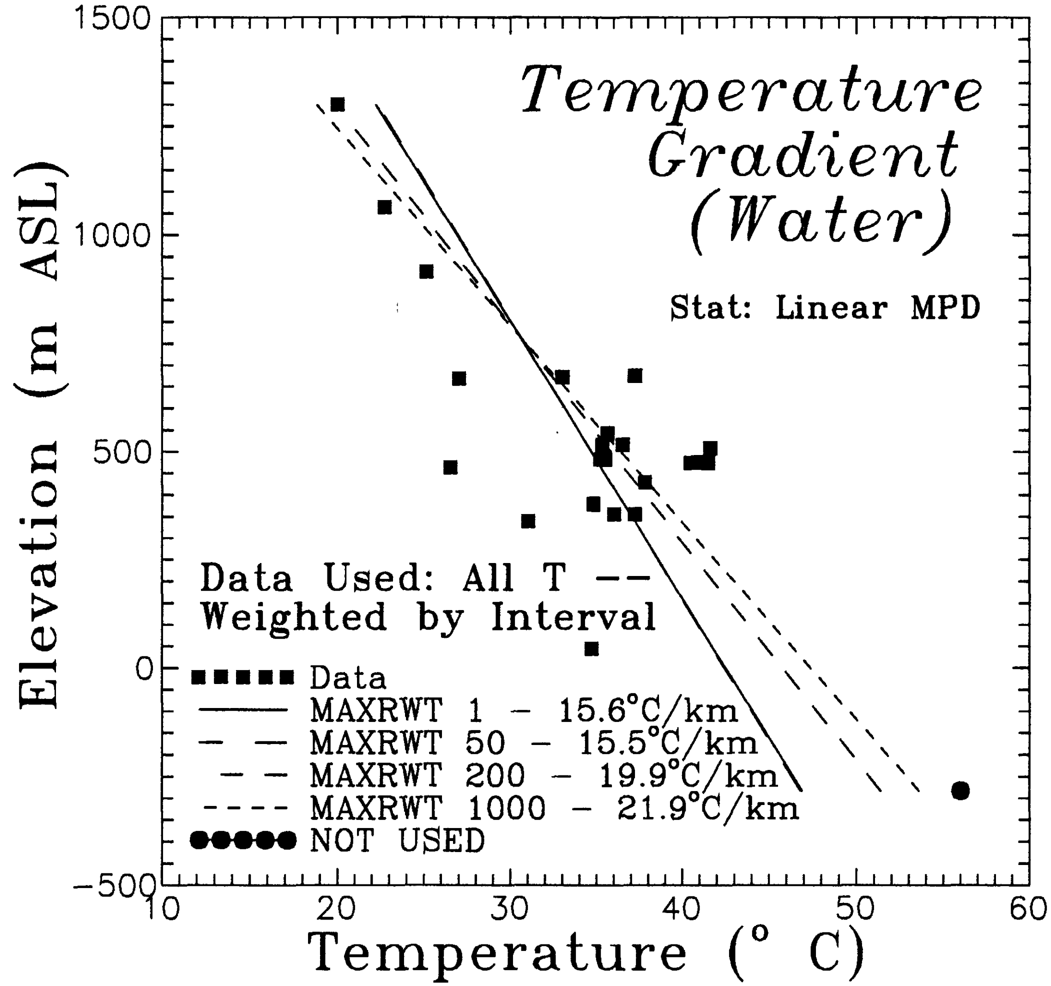
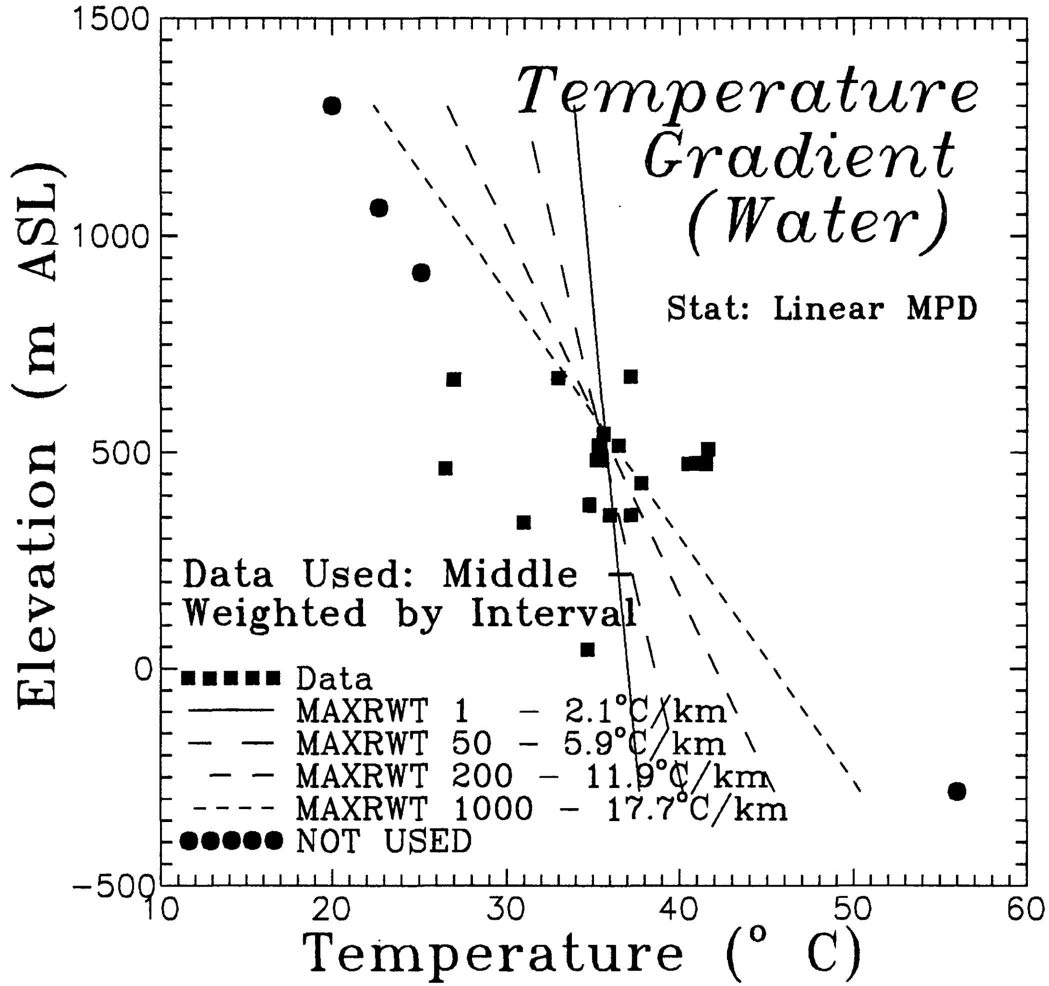
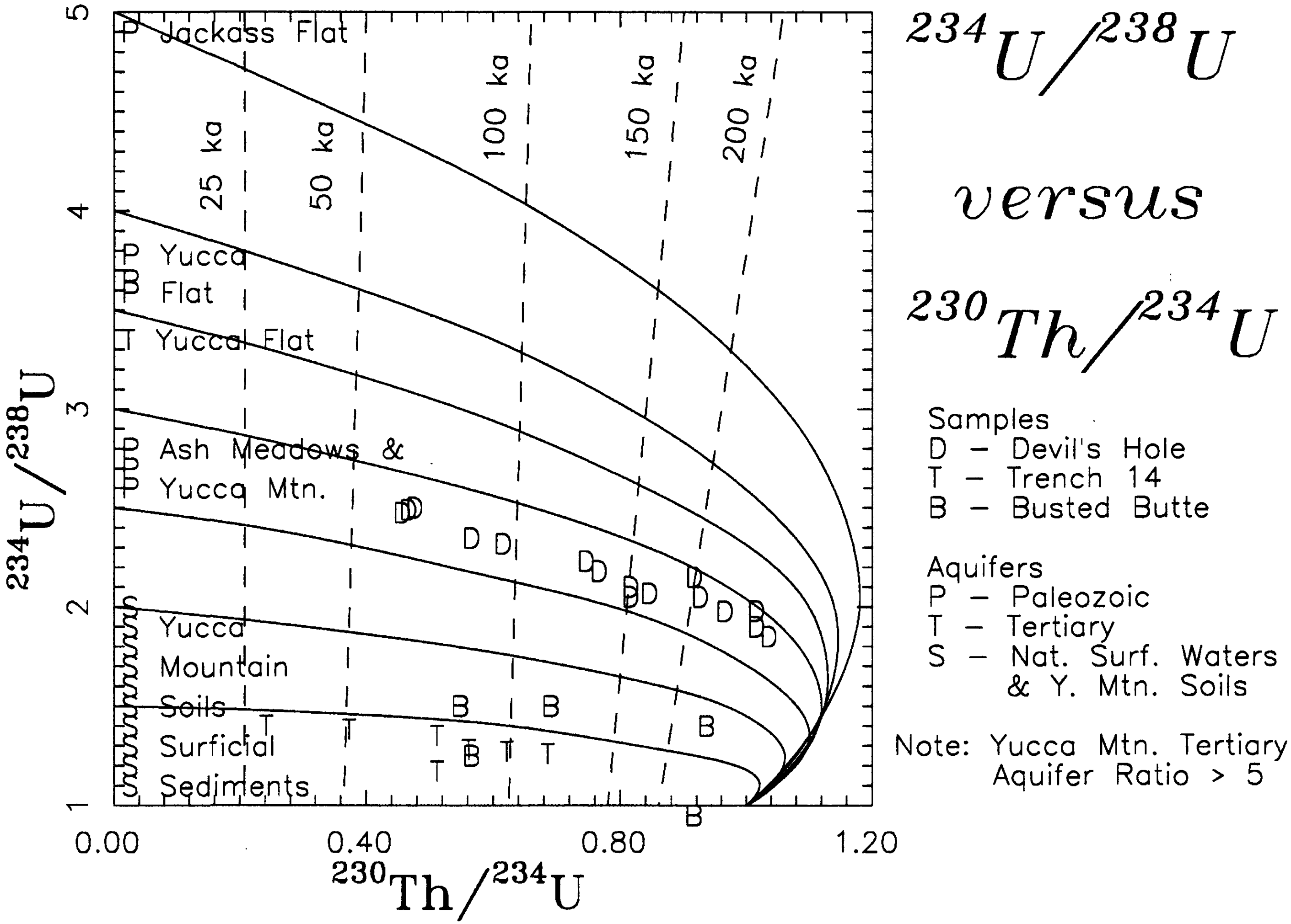

Temperature Gradient (Water)
Stat: Linear MPD
Data Used: All T --
Weighted by Interval
Data
MAXRWT 1 - 15.6°C/km
MAXRWT 50 - 15.5°C/km
MAXRWT 200 - 19.9°C/km
MAXRWT 1000 - 21.9°C/km
NOT USED
Elevation (m ASL)
Temperature (° C)
1500
1000
500
0
-500
10
20
30
40
50
60

Figure 5



Temperature Gradient (Water)
Stat: Linear MPD
Data Used: Middle
Weighted by Interval
Data
MAXRWT 1 – 2.1°C/km
MAXRWT 50 – 5.9°C/km
MAXRWT 200 – 11.9°C/km
MAXRWT 1000 – 17.7°C/km
NOT USED
Elevation (m ASL)
1500
1000
500
0
−500
10
20
30
40
50
60
Temperature (° C)

Figure 6



$^{234}U/^{238}U$ versus $^{230}Th/^{234}U$
Samples
D – Devil's Hole
T – Trench 14
B – Busted Butte
Aquifers
P – Paleozoic
T – Tertiary
S – Nat. Surf. Waters & Y. Mtn. Soils
Note: Yucca Mtn. Tertiary Aquifer Ratio > 5
Jackass Flat
Yucca Flat
Yucca Flat
Ash Meadows & Yucca Mtn.
Yucca Mountain Soils
Surficial Sediments
25 ka
50 ka
100 ka
150 ka
200 ka
$^{234}U/^{238}U$
$^{230}Th/^{234}U$
0.00
0.40
0.80
1.20
1
2
3
4
5

Figure 7

Cation Composition of Aquifers

(Expressed in mmol/l Per Cent)

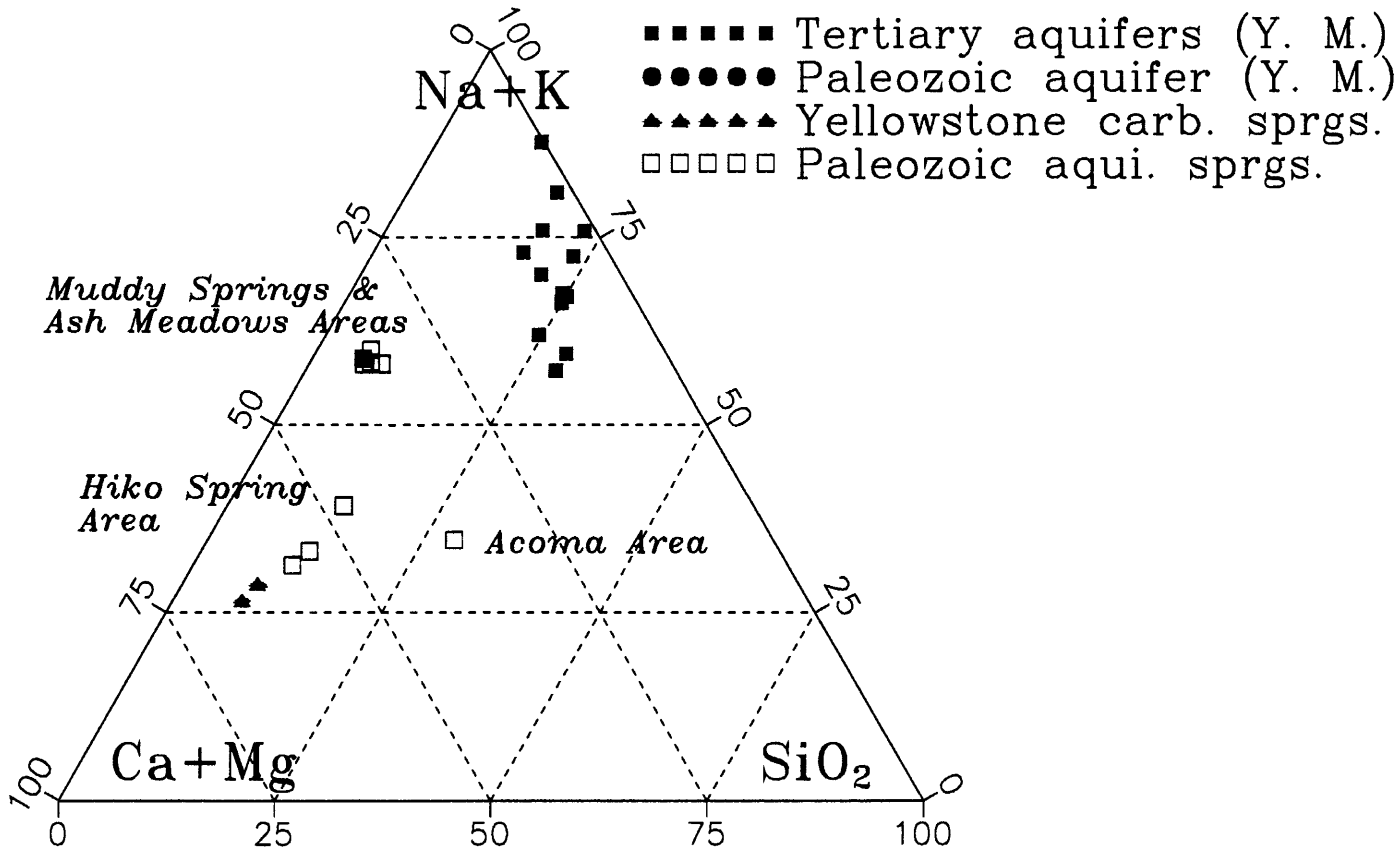


Figure 8

Anion Composition of Aquifers

(Expressed in mmol/l Per Cent)

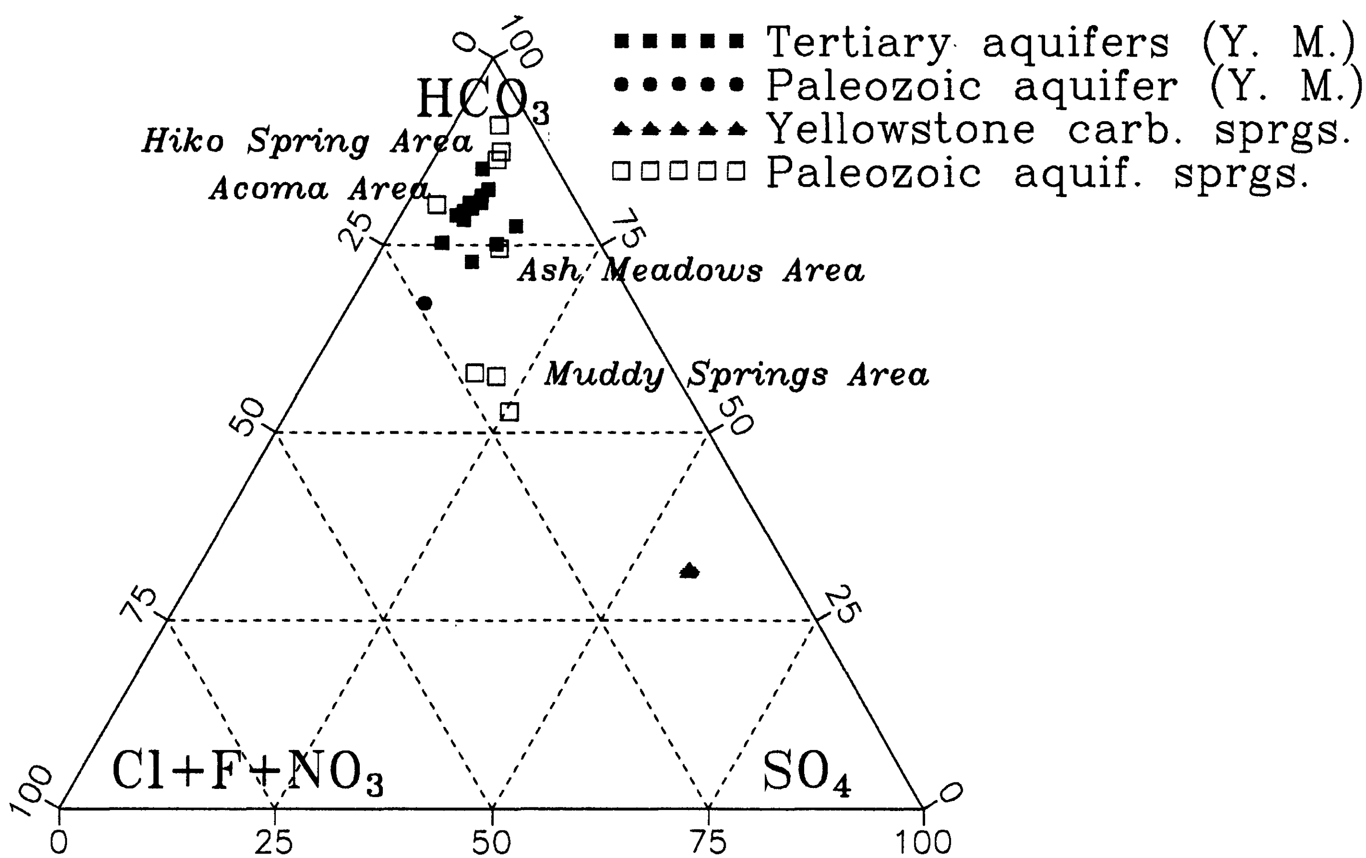
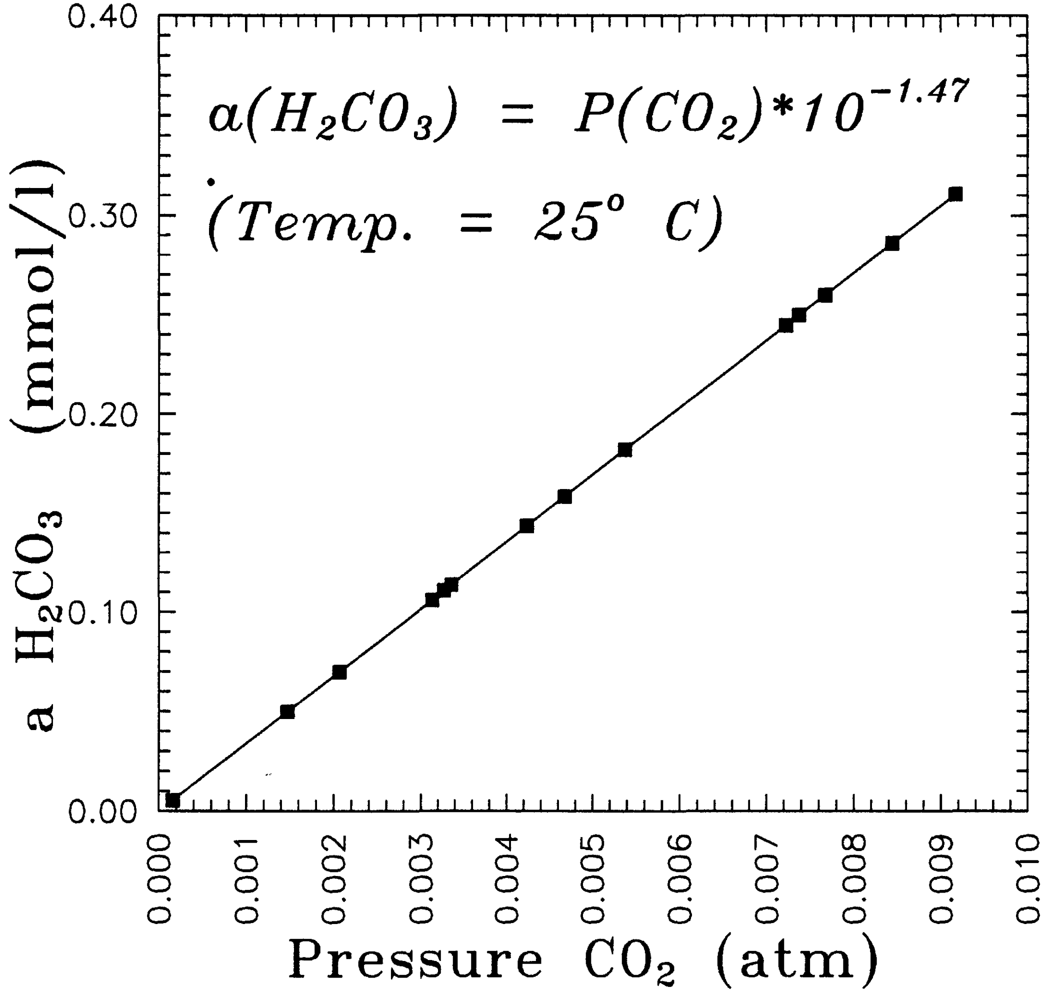
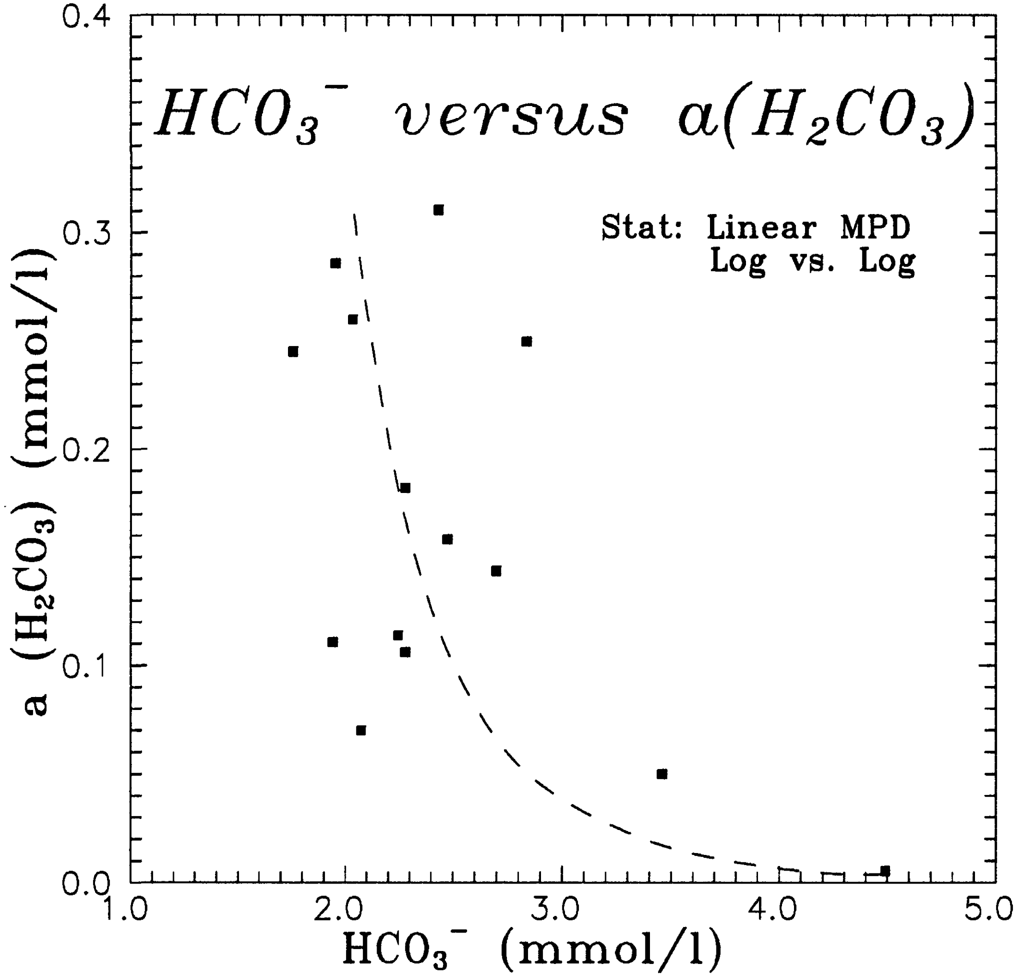


Figure 9



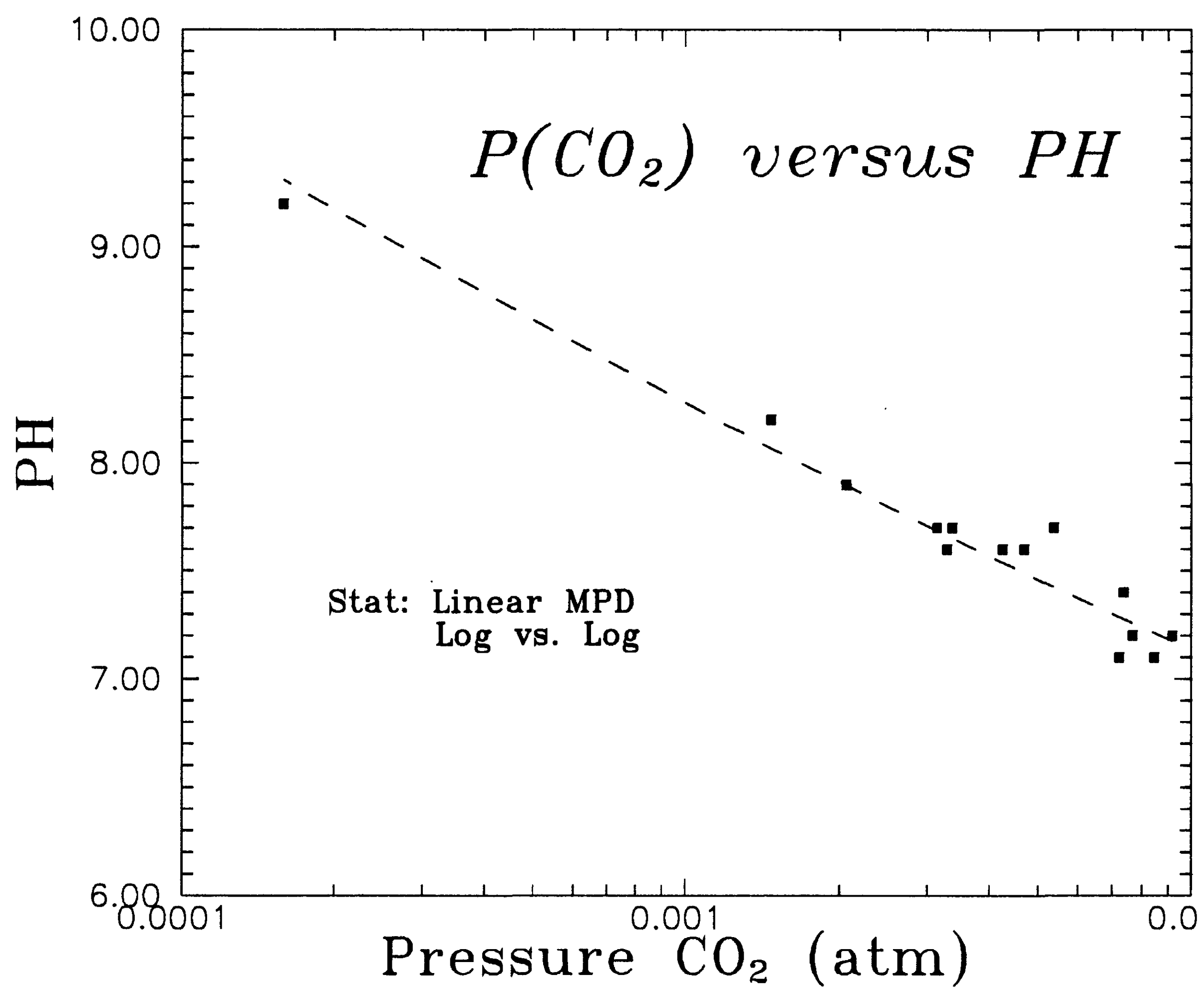
$\alpha(H_2CO_3) = P(CO_2)*10^{-1.47}$
(Temp. = 25° C)
a H_2CO_3 (mmol/l)
0.40
0.30
0.20
0.10
0.00
0.000
0.001
0.002
0.003
0.004
0.005
0.006
0.007
0.008
0.009
0.010
Pressure CO_2 (atm)

Figure 10



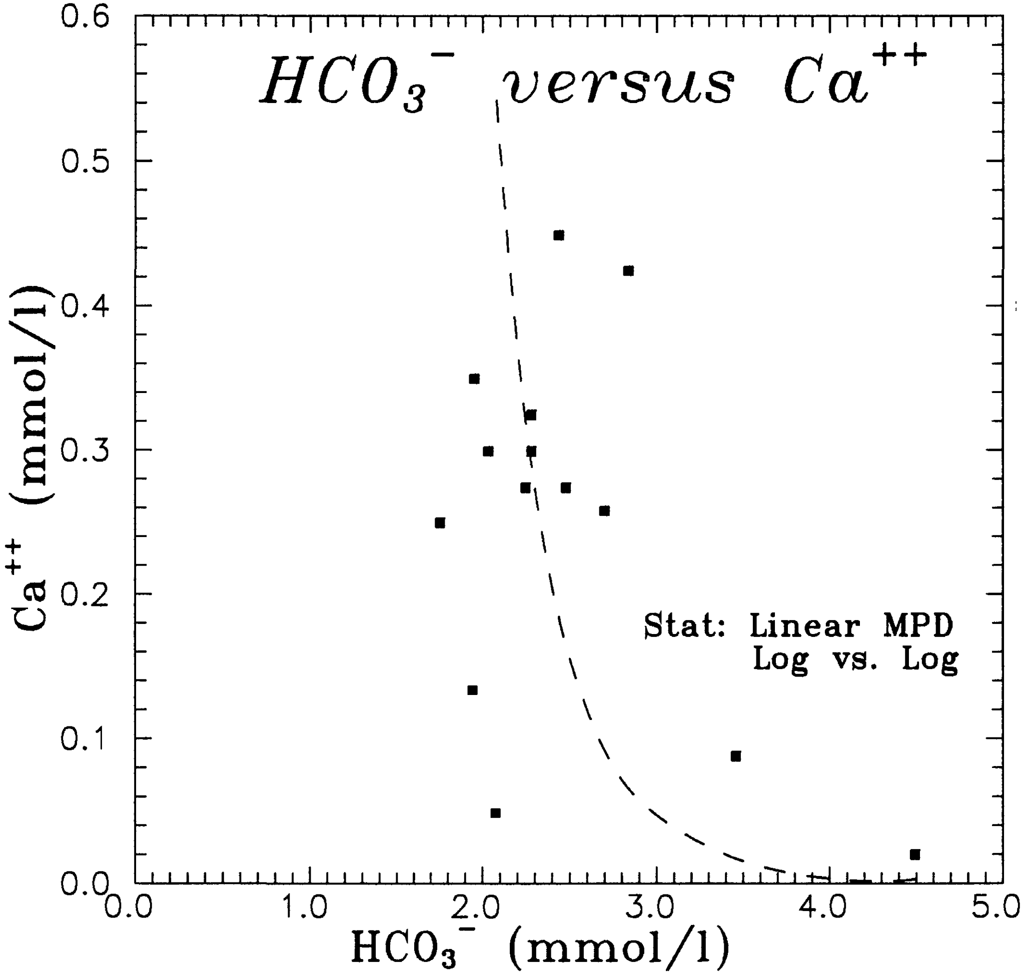
HCO_3^- versus $\alpha(H_2CO_3)$
Stat: Linear MPD
Log vs. Log
0.4
0.3
0.2
0.1
0.0
1.0
2.0
3.0
4.0
5.0
a (H_2CO_3) (mmol/l)
HCO_3^- (mmol/l)

Figure 11



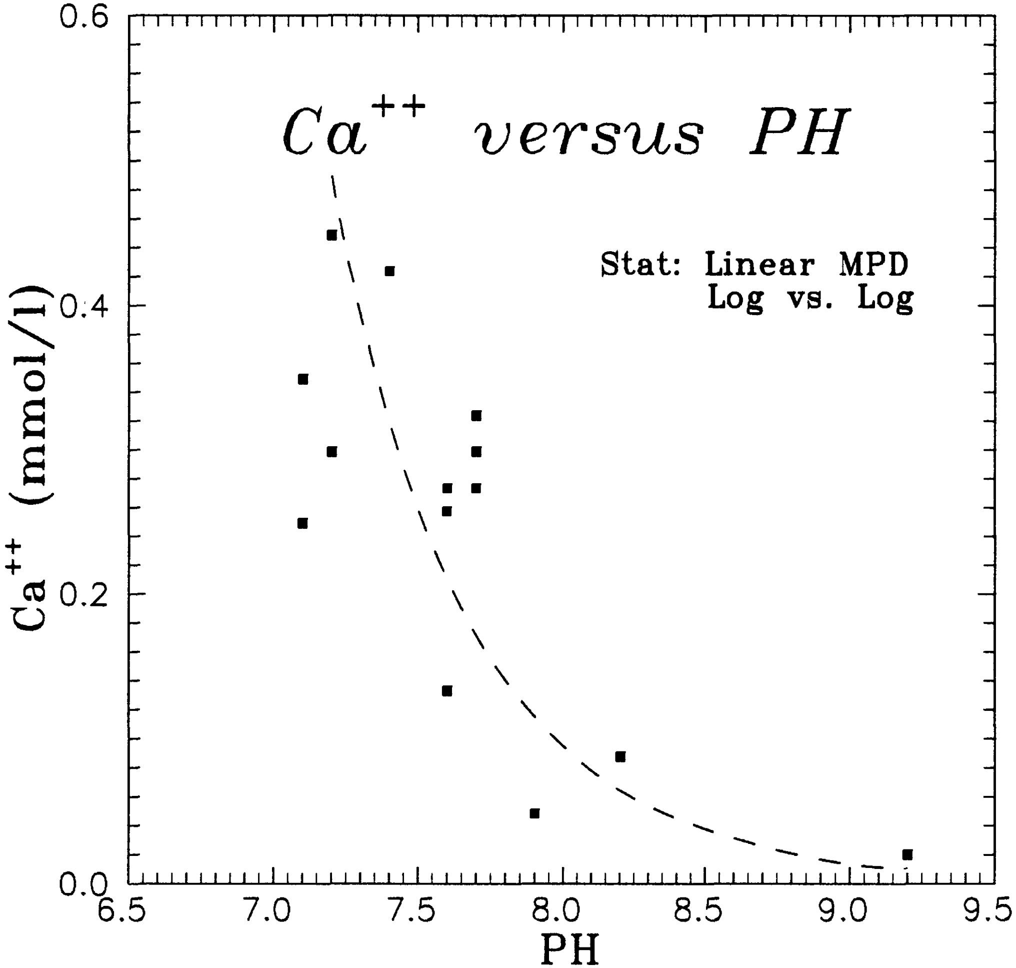
P(CO2) versus PH
Stat: Linear MPD
Log vs. Log
PH
10.00
9.00
8.00
7.00
6.00
0.0001
0.001
0.01
Pressure CO2 (atm)

Figure 12



HCO_3^- versus Ca^{++}
Stat: Linear MPD
Log vs. Log
0.0
0.1
0.2
0.3
0.4
0.5
0.6
Ca^{++} (mmol/l)
0.0
1.0
2.0
3.0
4.0
5.0
HCO_3^- (mmol/l)

Figure 13



Ca++ versus PH
Stat: Linear MPD
Log vs. Log
0.6
0.4
0.2
0.0
Ca++ (mmol/l)
6.5
7.0
7.5
8.0
8.5
9.0
9.5
PH

Figure 14

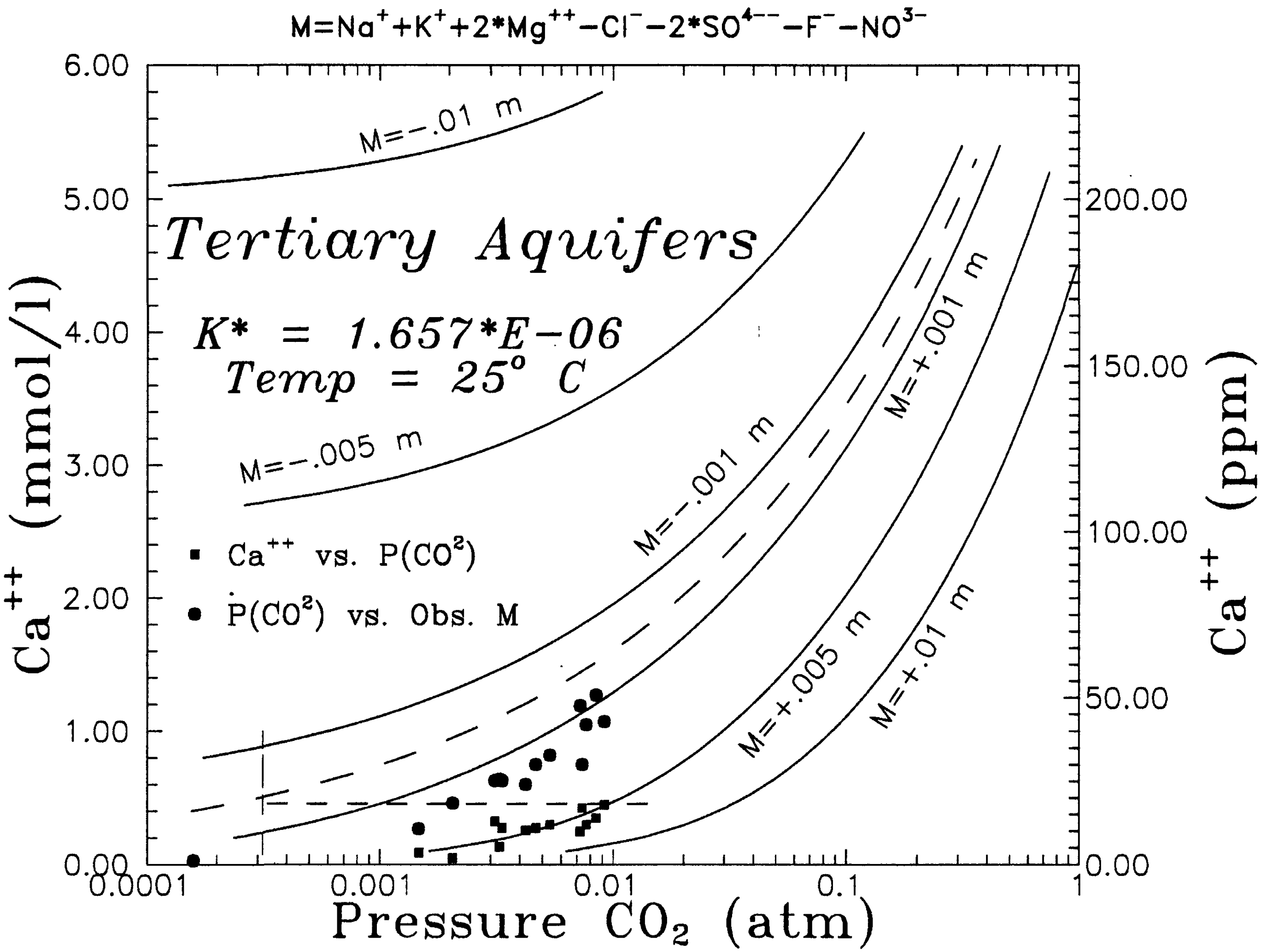
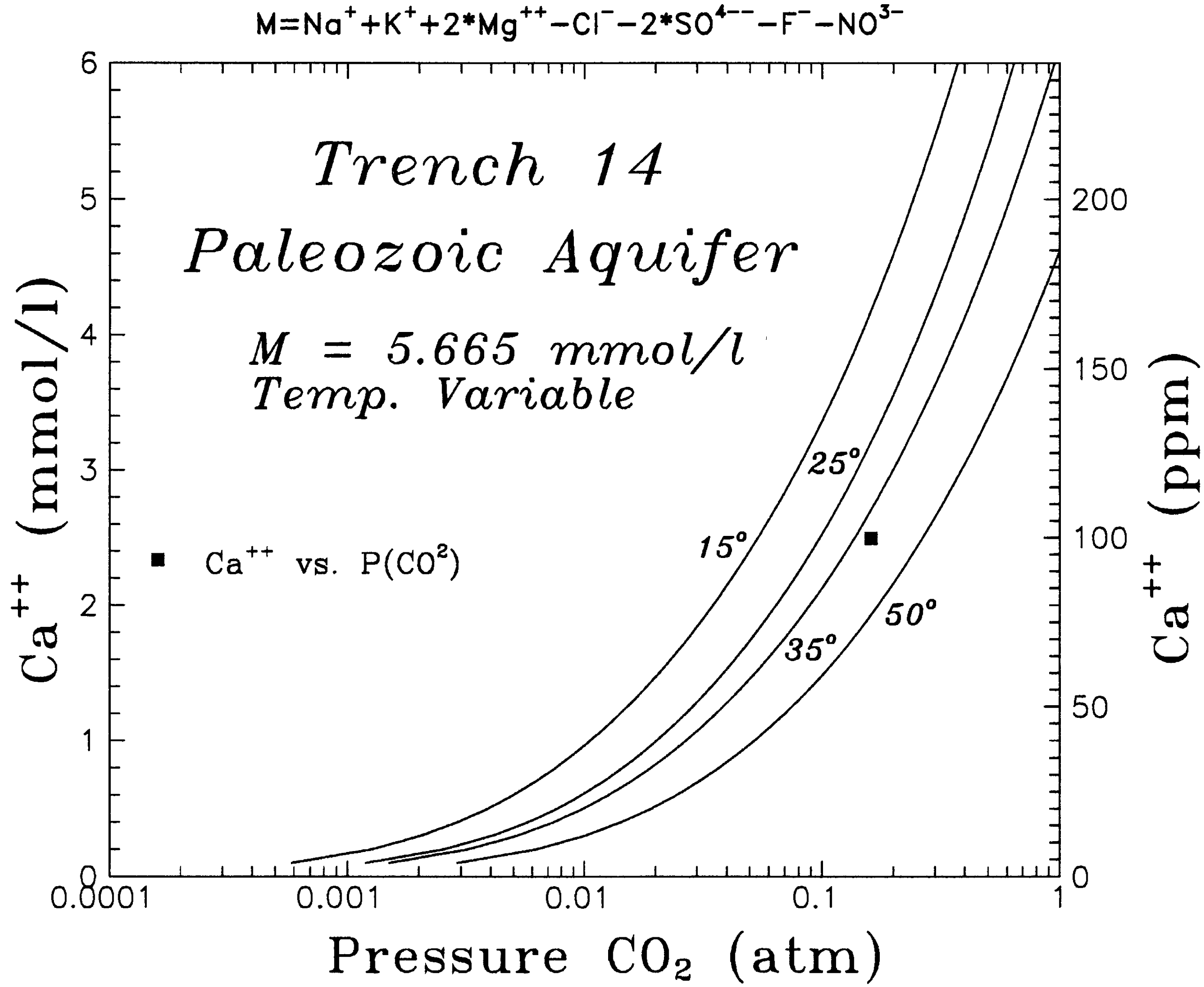

M=Na+ +K+ +2*Mg++ −Cl− −2*SO4−− −F− −NO3−
Tertiary Aquifers
K* = 1.657*E−06
Temp = 25° C
M=−.01 m
M=−.005 m
M=−.001 m
M=+.001 m
M=+.005 m
M=+.01 m
Ca++ vs. P(CO2)
P(CO2) vs. Obs. M
Ca++ (mmol/l)
Ca++ (ppm)
Pressure CO2 (atm)
6.00
5.00
4.00
3.00
2.00
1.00
0.00
200.00
150.00
100.00
50.00
0.00
0.0001
0.001
0.01
0.1
1

Figure 15



M=Na+ +K+ +2*Mg++ −Cl− −2*SO4−− −F− −NO3−
Trench 14
Paleozoic Aquifer
M = 5.665 mmol/l
Temp. Variable
Ca++ vs. P(CO2)
15°
25°
35°
50°
Ca++ (mmol/l)
Ca++ (ppm)
Pressure CO2 (atm)
0
1
2
3
4
5
6
0.0001
0.001
0.01
0.1
1
50
100
150
200

Figure 16

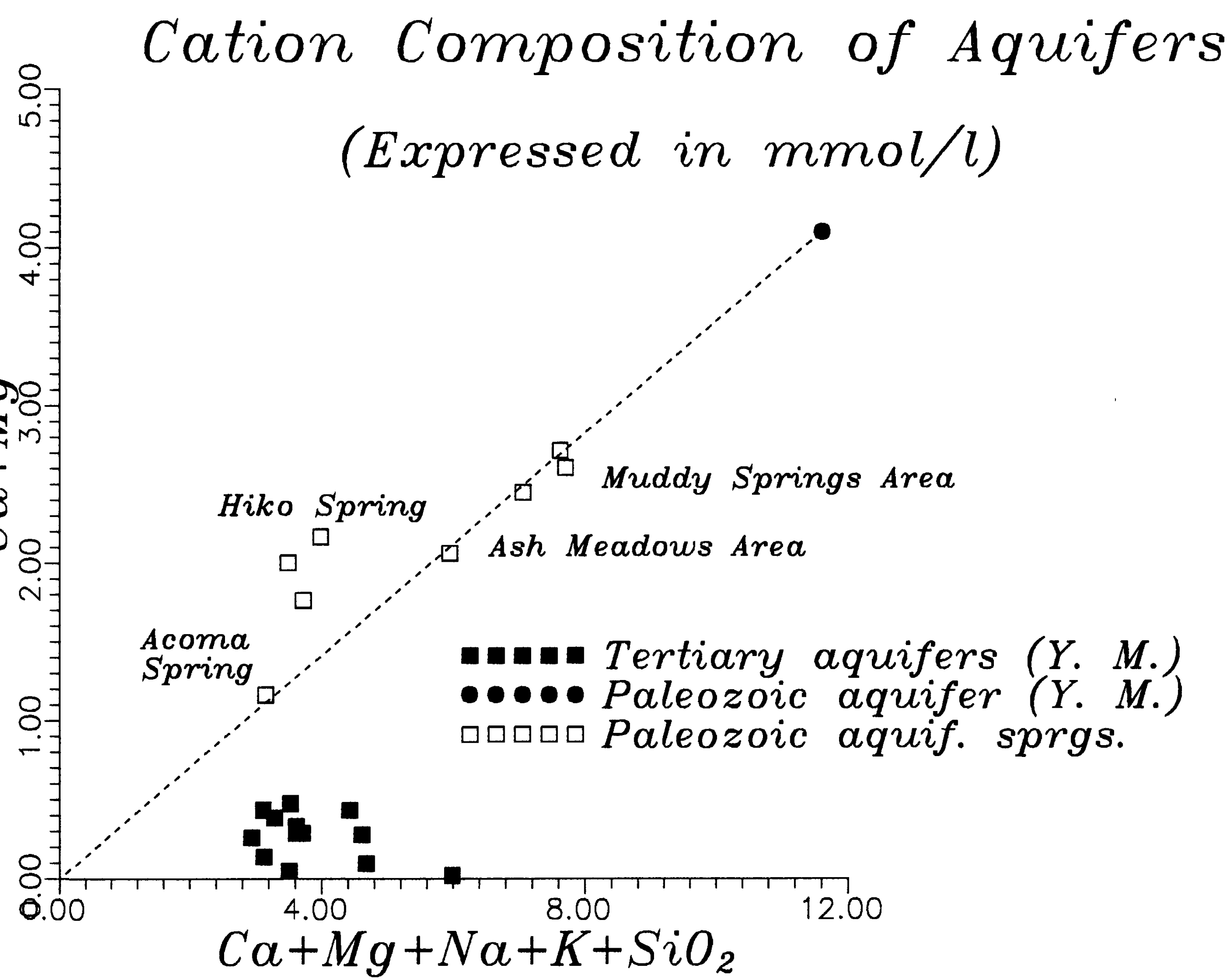
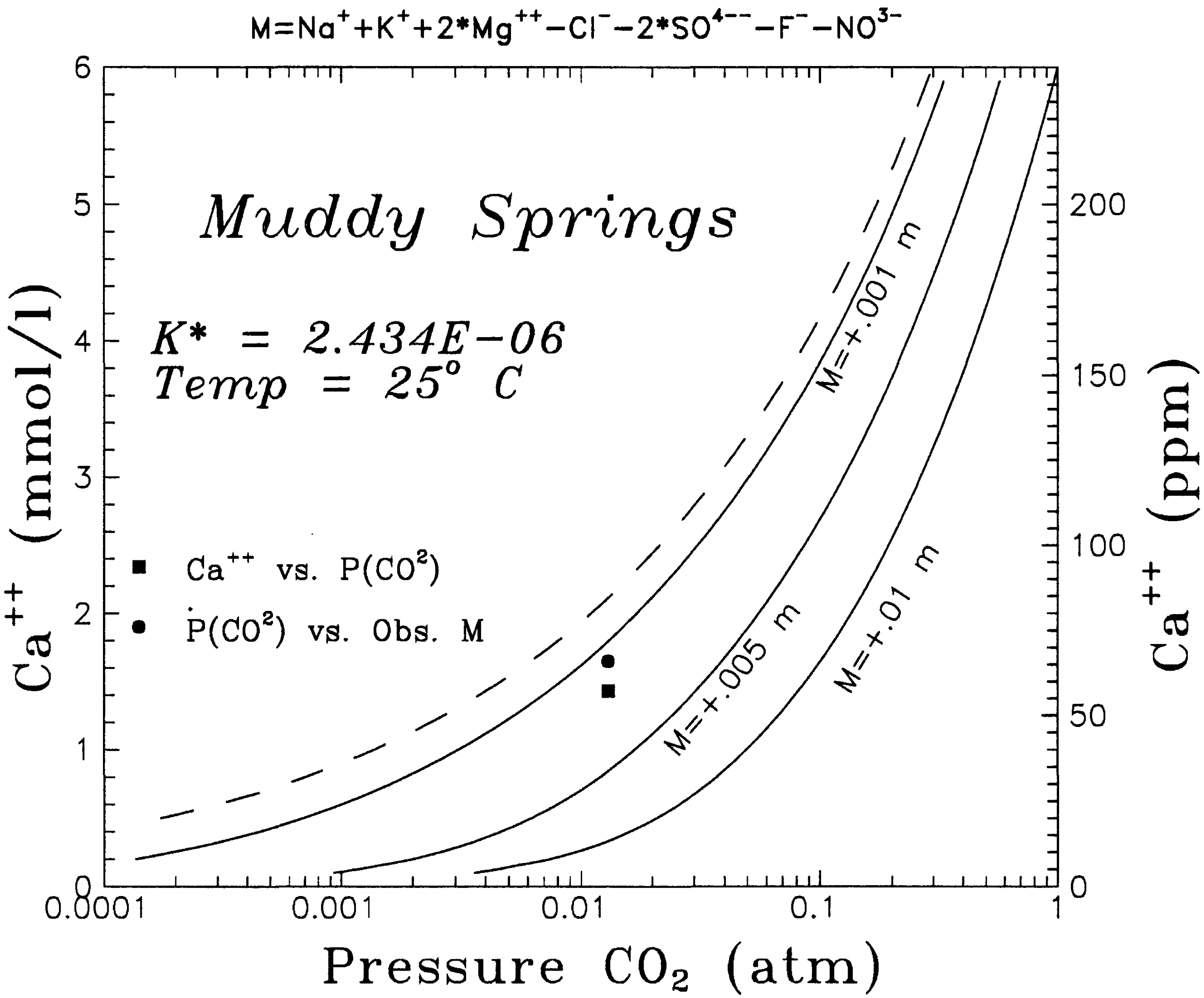
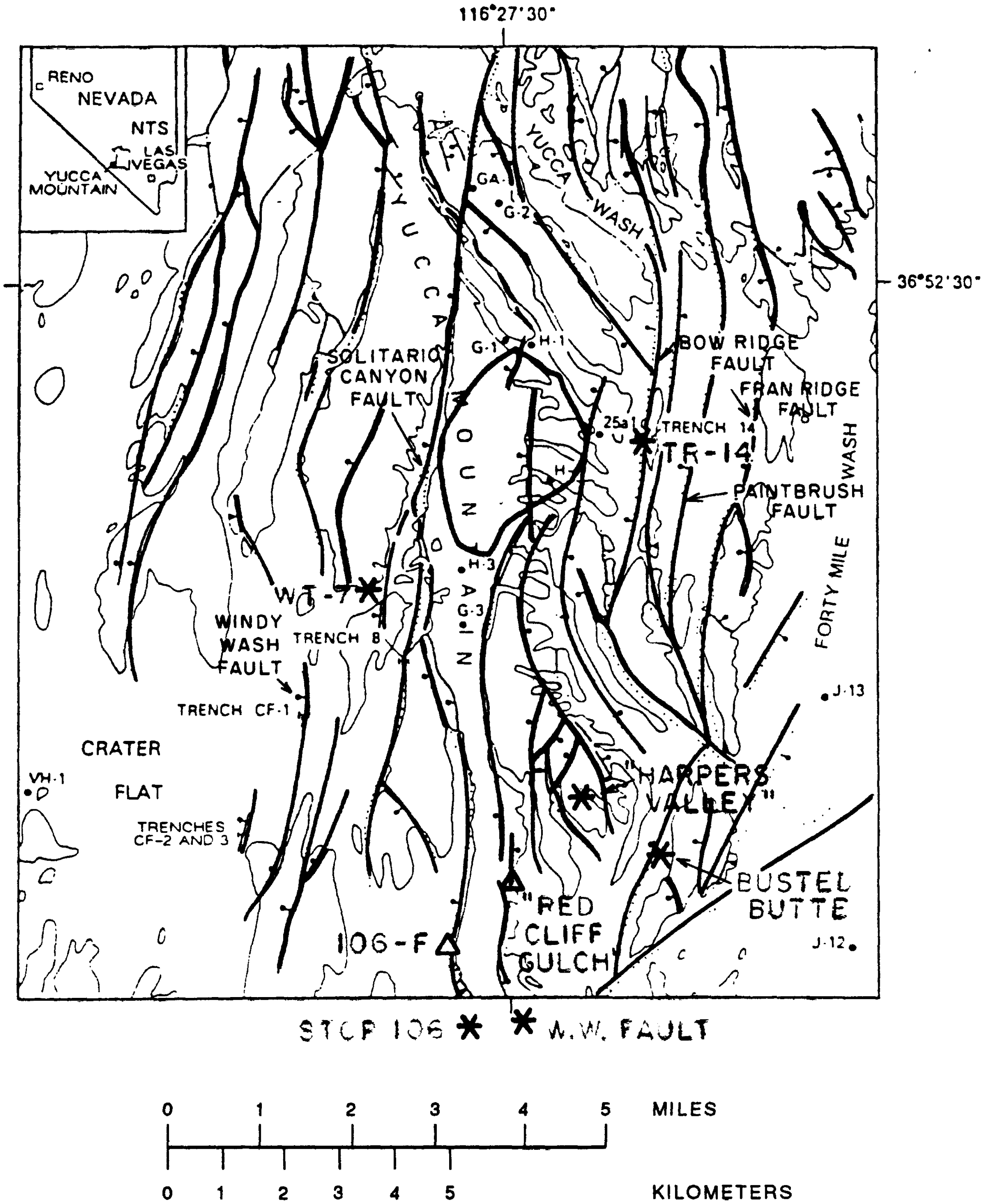

Cation Composition of Aquifers
(Expressed in mmol/l)
Ca+Mg
0.00
1.00
2.00
3.00
4.00
5.00
0.00
4.00
8.00
12.00
Ca+Mg+Na+K+SiO2
Acoma Spring
Hiko Spring
Ash Meadows Area
Muddy Springs Area
Tertiary aquifers (Y. M.)
Paleozoic aquifer (Y. M.)
Paleozoic aquif. sprgs.

Figure 17



M=Na⁺+K⁺+2*Mg⁺⁺−Cl⁻−2*SO⁴⁻⁻−F⁻−NO³⁻
Muddy Springs
K* = 2.434E−06
Temp = 25° C
Ca⁺⁺ vs. P(CO²)
P(CO²) vs. Obs. M
M=+.001 m
M=+.005 m
M=+.01 m
Ca⁺⁺ (mmol/l)
Ca⁺⁺ (ppm)
Pressure CO₂ (atm)
0
1
2
3
4
5
6
0.0001
0.001
0.01
0.1
1
50
100
150
200



EXPLANATION

- G-2 DRILL HOLE
- TRENCH
- NORMAL FAULT--BAR AND BALL ON DOWNTHROWN SIDE
- PERIMETER DRIFT BOUNDRY

Figure 18

References

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