Rec'd 3/21/01

CENTER FOR NUCLEAR WASTE REGULATORY ANALYSES

TRIP REPORT

SUBJECT: Fifth Quarterly Meeting of the UNLV-DOE Fluid Inclusion Studies Group

DATE/PLACE: February 8, 2001, at UNLV—Las Vegas, Nevada

AUTHORS: Mary Beth Gray, Consultant for the CNWRA, Professor, Department of Geology, Bucknell University, and John Stamatakos

DISTRIBUTION:

CNWRA

W. Patrick
CNWRA Directors
CNWRA Element Managers
D. Ferrill
J. Winterle
D. Pickett
L. Browning
B. Hill

NRC-NMSS

J. Linehan
W. Reamer
B. Leslie
D. DeMarco
E. Whitt
B. Meehan
J. Greeves
J. Holonich
S. Wastler
T. Essig
P. Justus
J. Bradbury
D. Brooks
N. Coleman

SwRI Contracts

T. Nagy (Contracts)

CENTER FOR NUCLEAR WASTE REGULATORY ANALYSES

TRIP REPORT

SUBJECT: Fifth Quarterly Meeting of the UNLV-DOE Fluid Inclusion Studies Group

DATE/PLACE: February 8, 2001, at UNLV—Las Vegas, Nevada

AUTHORS: Mary Beth Gray, Consultant for the CNWRA, Professor, Department of Geology, Bucknell University, and John Stamatakos

PERSONS PRESENT:

Jean Cline (UNLV)
Nick Wilson (UNLV)
Robert Bodnar (Virginia Tech, consultant for the UNLV Study)
Yuri Amelin (Royal Ontario Museum, sub-contractor to UNLV)
Drew Coleman (DOE)
William Boyle (DOE)
Bill Arnold (Sandia)
Brian Marshall (USGS)
Leonid Neymark (USGS)
Jim Paces (USGS)
Zell Peterman (USGS)
Ed Roedder (Harvard University, consultant to the USGS)
William Scott (USGS)
John Stuckless (USGS)
Joe Whelan (USGS)
Yuri Dublyansky (Nevada)
Jerry Szymanski (Nevada)
Susan Zimmerman (Nevada)
Reina Downing (Nye County)
Don Shettal (Nye County)
E. Von Teisenhausen (Clark County)
Merna Adams (Nevada Attorney General's Office)
Tony Clark (Solictor General of the Nevada Attorney General's Office)
Leon Reiter (NWTRB)
Don Runnells (NTWRB)
Chad Glen (NRC)
Mary Beth Gray (Bucknell University, consultant for the CNWRA)
John Stamatakos (CNWRA)

BACKGROUND AND PURPOSE OF TRIP:

In 1999 the DOE awarded a 1.4 million dollar grant to Jean Cline at the University of Nevada, Las Vegas (UNLV) to conduct an independent evaluation of secondary mineralization at Yucca Mountain (YM). The goal of the study was to evaluate remaining uncertainties in the thermal history of the mountain (including contentious questions about possible hydrothermal upwelling most recently raised by scientists working for the State of Nevada). The UNLV study was designed to determine the ages, and temperatures of secondary mineralization at YM through a study of mineral fluid inclusions (mineralization fluids trapped within microscopic inclusions during crystallization). The UNLV study focused on assemblages of two-phase fluid inclusions (gas and liquid). Two-phase inclusions are deemed one of the more reliable indicators of the temperatures and pressure conditions during growth of the secondary minerals. These paleotemperatures and paleopressures are determined from a measurement of the homogenization temperature (the laboratory heating temperature at which the vapor bubbles disappear from the two-phase fluid inclusion). Because the tuffs at YM were never deeply buried, the homogenization temperatures are considered to represent the temperatures at which fluids were trapped in microscopic voids during crystal growth.

In order to gain a consensus on the study's data collection, analytical methods, and results, the study included active participation of experts representing the State of Nevada, UNLV, and the USGS. To integrate and report on results of the study, Dr. Cline established a meeting protocol. Periodically, the core group of active project participants met for several days to discuss recent findings followed by a general information seminar to explain the results to all interested parties (e.g., NRC, DOE, and the NWTRB). Jean Cline originally established quarterly meetings, but in reality only about two meetings per year were held.

One of several occurrences of secondary minerals containing fluid inclusions is within fault zones exposed in the ESF and Cross Drift. Because CNWRA work is focused on the evaluation of faults at Yucca Mountain, our results bear directly on interpretations of the UNLV results. John Stamatakos and Mary Beth Gray attended this meeting to learn, first hand, of the study's results and to keep abreast of all technical developments important to the resolution of this technically-charged issue.

SUMMARY OF PERTINENT POINTS:

This meeting was the fifth and final quarterly meeting of the UNLV Fluid Inclusion Studies Group. Two days of meetings with a core group of scientists (Bodnar, Dublyansky, Roedder, Whelan, Cline, and Wilson) on February 6 and 7 were followed by a daylong meeting attended by the 28 scientists listed above. No report was given on the content of the "small group" meetings on February 6–7 at the "large group" meeting. Jean Cline called the meeting to order at 9 a.m. and said that no meeting agenda would be distributed. She introduced Bill Arnold, a hydrologist at Sandia, and invited him to give a presentation to the group.

Fluid inclusion entrapment in the vadose zone

Bill Arnold presented a theoretical model for mineralization of secondary calcite in the vadose zone (unsaturated conditions) in which two-phase fluid inclusions could develop at present-day ambient temperatures (22–25 °C). He suggested that as calcite crystals grow at 22–25 °C, depressions in the crystal face could localize fluid inclusions. Just before the fluid inclusion was sealed by mineral growth a sufficiently narrow "pore throat" (fluid inclusion aperture) could develop within each inclusion. Capillary forces within the pore throat would lower the pressure of the liquid within the inclusion. Eventual closure of the pore throat

would then trap the liquid with a relatively low fluid pressure, and in some cases a vapor bubble could eventually form. Because pore throat radii are expected to vary, the resulting two-phase fluid inclusion assemblage would have highly variable liquid to vapor ratios. Nevertheless, if these assemblages were subsequently measured by conventional fluid inclusion procedures, the resulting assemblages could be misinterpreted to indicate elevated temperatures rather than initially lower fluid pressures at the time of entrapment. Bill Arnold noted that, if correct, his model would call into question the results of the UNLV geothemometry study.

The model was not well received by the fluid inclusion experts. Yuri Dublyansky noted that leaking of inclusions and/or trapping of heterogeneous fluids could also produce these types of assemblages.

Jean Cline responded that the model may be valid in a few special cases, but it could not apply to her data set. Jean Cline emphasized that she selected only those two-phase fluid inclusion assemblages that had "consistent" liquid/vapor ratios. She noted that less than one percent of all two-phase fluid inclusion assemblages found in Yucca Mountain rocks had such "consistent" ratios. Within such a suitable assemblage, a significant percentage of inclusions would be deemed unsuitable for measuring. Mary Beth Gray asked what percentage of all two-phase fluid inclusions within a suitable assemblage would typically be deemed unsuitable for measurement. Jean Cline said she didn't know. In practice, she measured only two-phase fluid inclusions that had a liquid/vapor ratio that met her expectations for trapping conditions (i.e. only those inclusions that had very small vapor bubbles). On this basis, Jean Cline felt that Bill Arnold's model would not impact the validity of her data. Stamatakos and Gray inquired whether Jean Cline used an objective, quantitative standard to differentiate between "variable" and "consistent" two-phase fluid inclusion assemblages and to decide which individual inclusions were suitable for microthermometry. Her response indicated that she had relied on qualitative screening from microscope observations. Jean Cline pointed out that Bill Arnold's model does not explain: (1) the presence of abundant all liquid inclusions; (2) the consistent microthermometry results from the three different groups of scientists (UNLV, State of Nevada, and USGS); and (3) the systematic and predictable variations in fluid inclusion temperatures within zoned secondary minerals.

Bob Bodnar pointed out that the effect of low fluid pressures due to capillary action at trapping had the same effect as trapping at elevated temperature. If entrapment occurred in the vadose zone at elevated temperatures, both effects (capillary action and elevated temperatures) could have the expected result of yielding homogenization temperatures that would constitute a maximum trapping temperature. The UNLV results would therefore simply represent maximum paleotemperatures.

Yuri Dublyansky rejected Bill Arnold's model because of the premise that the minerals were formed in the vadose zone. Dublyansky restated his earlier position that the mineral chemistry data require mineralization under saturated-zone conditions.

The USGS scientist working on this project remained split on how to view Bill Arnold's model. Joe Whelan said that he saw merit in both Jean Cline's points and Bill Arnold's model, but that the consistency of the temperature measurements from the two-phase fluid inclusions suggested to him that the fluid inclusion homogenization temperatures ultimately must represent paleotemperature. In contrast, Zell Peterman agreed with Arnold's model and added that the fluid inclusion data were also suspect because the dispersion of the data was much greater than the precision of the analytical method. Jim Paces also concluded that the fluid inclusion miocrothermomtery data is not a valid measurement of any paleotemperatures in the rocks during the last 10 Ma.

None of these criticisms swayed Jean Cline from reiterating her conclusion that the measured fluid inclusions are representative of mineralization temperatures within YM.

Assessment of Meeting Stated Research Goals

Jean Cline revisited the four questions initially posed in the project proposal:

1. Do populations of fluid inclusions exist that indicate the recent influx of thermal waters into the repository site? Over the course of the project, Jean re-phased this question to read, "Have fluid inclusions recorded the passage of fluids with elevated temperatures at Yucca Mountain?".

2. If these inclusions are present, what minimum fluid temperatures do they indicate? Over the course of this project, Jean Cline re-phrased this question to read, "If yes, what was the temperature of these fluids?

3. If present, when were these inclusions trapped, i.e., when did this influx(es) occur? Over the course of this project, Jean re-phrased this question to read, "If yes, what was the timing of these fluids?

4. If an influx occurred, how widespread, within the repository site, was this influx?

UNLV collected 155 samples from throughout the ESF/ECRB. The USGS scientists received about 100 thick sections and Nevada geologists received 40 thick sections from these samples. Jean Cline apologized for the uneven distribution of thick sections. Two-phase fluid inclusion assemblages were found in secondary minerals in all areas of the ESF/ECRB. Two-phase fluid inclusion assemblages with "consistent" liquid-vapor ratios were found in 78 samples. Although UNLV found two-phase fluid inclusion assemblages in all minerals regardless of relative age, no two-phase fluid inclusion assemblages with "consistent" liquid-vapor ratios were found in the youngest (outermost) calcite in lithophysal cavities. This calcite is typically enriched in magnesium and lacks two-phase fluid inclusions (at least those two-phase fluid inclusions which have consistent liquid/vapor ratios). It has clear, sparry, euhedral calcite crystals and $\delta^{13}C_{PDB}$ values less than -2‰. This magnesium-rich sparry calcite is found in 65 percent of all samples.

Jean Cline pointed out that all two-phase fluid inclusion assemblages with "consistent" liquid-vapor ratios are only as young as the basal portion of intermediate-age calcite, which is constrained to ages older than about 4 Ma. Joe Whelan said that his data do not preclude two-phase fluid inclusion assemblages with "consistent" liquid-vapor ratios younger than 4 Ma., but both Joe Whelan and Jean Cline ruled out two-phase fluid inclusions (at least those suitable for microthermometry) younger than 2 Ma (the apparent age of the initial deposition of magnesium-rich calcite). Age constraints were provided by 48 U-series and 48 U-Pb dates from inter-laminated opal deposits that are in recognizable stratigraphic positions relative to the calcite bearing two-phase fluid inclusion assemblages with "consistent" liquid-vapor ratios. Some questions ensued about the reliability of the age dates. Yuri Amelin (a sub-contractor to the UNLV project) stated that the U-series dates may be off by 5–10 percent but that the errors were systematic such that the dates should be considered minimum ages. Jerry Szymanski rejected U-series data because he does not agree with the assumptions inherent in the method.

Homogenization temperatures average between 40–60 °C for the main drift and the ECRB. The intensely fractured zone has average homogenization temperatures of 40–50 °C and the north and south ramps have

average homogenization temperatures of 60–80 °C. UNLV interprets the homogenization temperatures to represent maximum trapping temperatures. No explanations were offered to describe the observed distribution of fluid inclusion temperatures.

No fluid inclusions were found with homogenization temperatures below 35 °C. The 35 °C temperature was cited as a cut-off temperature, in the sense that fluids trapped at or below 35 °C would be sufficiently metastable to effectively inhibit formation of vapor phase bubbles. Yuri Dublyansky noted that the UNLV data do not preclude recent (within the last 2 million years) flow of fluids heated to less than or equal to 34 °C. A discussion ensued regarding the definition of "elevated temperature" in this geologic context. John Stuckless mentioned that present day hot springs in southern Nevada have water temperatures less than or equal to 41 °C.

Jean Cline noted a positive correlation between homogenization temperatures and relative age in several lithophysal cavity crusts. Slightly higher fluid inclusion temperatures appear to be in relatively older mineralization horizons. An opposite relationship appears within the YM volcanic stratigraphy, with the highest fluid inclusion temperatures located on the North Ramp in the Tiva Canyon Tuff. Jim Paces asked why the Tiva samples should have higher average homogenization temperatures than the Topopah or other older tuffs. Nevada geologists attribute this observation to fault localization of fluids with elevated temperatures. Joe Whelan concluded that the relatively higher temperatures in the Tiva were from fumerolic activity. Joe alluded to (but did not provide) evidence of fumerolic activity in the north ramp. Jean Cline chose not to respond to the question.

A freezing stage was used to determine the melting temperature of ice in the fluid inclusions as a means to constrain the chemistry of the liquid phase. Yuri Dublyansky reported 0–21,000 ppm NaCl equivalent salinity. Jean Cline declined to report NaCl equivalent salinities (despite proposing to do so) because she said such reported salinities could be misleading. She noted that the depressed melting temperatures are artifacts of both salinity and dissolved gas. Jean Cline reported 152 measurements of freezing point depressions from 30–40 samples ranging between -0.4 to -0.9 °C. Bob Bodnar suggested that these apparent melting temperatures would correspond to about 7,000–15,000 ppm NaCl equivalent salinity. For reference, Bodnar said that Mississippi Valley Type deposits have salinities of approximately 300,000 ppm NaCl equivalent salinity. Jerry Szymanski mentioned that infiltrating water in Rainier Mesa has 50–100 ppm dissolved solids.

Many scientists questioned how Jean Cline would interpret the fluid inclusion data in terms of the thermal history of YM. Jean Cline repeatedly said that the interpretation or "story" should be left up to the geologists. Her project was only to provide answers to the four questions. Yet, in spite of general claims that the project would not attempt to interpret the data in terms of a possible source for the elevated temperature, Jean Cline concluded at the meeting and in her press release that the UNLV data and observations do not favor the upwelling fluid model. She cited three observations to support her conclusion. (1) She felt that if upwelling occurred, there should be more extensive mineralization throughout YM. (2) She noted the lack of wall rock alteration. (3) She also surmised that if upwelling had occurred, then the homogenization temperatures should be significantly higher. Jerry Szymanski noted that he observed wall rock alteration in numerous fractures and faults.

Both Leon Reiter and John Stamatakos pressed Jean Cline to comment on any evidence for a maximum age of fluid inclusions. The premise of the questions was that older fluid inclusion temperatures could be used to evaluate the recently proposed "slow cooling caldera" model by Brian Marshall of the USGS at the

November 2000 Geological Society of America meeting in Reno, Nevada. Dr. Cline could not provide a clear answer because her study "remained focused" on minimum ages. Joe Whelan indicated that he thought there were fluid inclusions in some of his samples that were older than 9 Ma, but he did not have any information on their homogenization temperatures.

The meeting ended with a press conference during which Jean Cline distributed and read a press release (attachment 1). It was attended by the meeting participants and representatives of the Las Vegas Sun (attachment 2), the Review Journal (attachment 3), and Channel 8 news. Following the meeting, Jerry Szymanski distributed to the group his thoughts on the UNLV study findings (attachment 4). Jean Cline's response and Jerry Szymanski's follow-up remarks are fond in attachments 5 and 6.

IMPRESSIONS/CONCLUSIONS:

The UNLV study appears to have provided answers to all four revised questions.

1. Populations of fluid inclusions document influx of thermal waters into the Yucca Mountain repository site.

2. The fluid inclusions indicate fluids with temperatures between 45 and 60 °C.

3. The data provide no evidence for an influx of fluids with elevated temperatures in the last 2 million years and possibly since 4–5 Ma.

4. Fluid inclusions with elevated temperatures (45–60 °C) were found throughout the repository site.

The UNLV deliverable will take the form a publication(s) in the March issue of Geochemica Cosmochemica Acta. Preliminary copies of the manuscript(s) were not made available.

Based on the continued disagreements between scientists, we suggest that the original goal, one of consensus on the approach and analytical methods of the study in an effort to generate a widely agreed upon interpretation, was not achieved. We believe that uneven distribution of samples and preconceived notions of the outcome biased many of the project scientists toward pre-established models of the behavior of the unsaturated zone. These biases contributed to the resulting disparate interpretations of the validity and significance of the fluid inclusion data.

At present, even the USGS scientists who worked on this project appear to be divided on how to interpret the fluid inclusion data. Several USGS scientists want to disregard the fluid inclusion data. They believe that mircothermometry is not valid for fluid inclusions trapped in the vadose zone. Other USGS scientists appear to accept the microthermometry data as a valid indicator of temperature and are willing to incorporate the temperatures into a general thermal history model. All USGS scientists appear to favor an interpretation that involves meteoric sources for fluid and unsaturated conditions of mineralization.

The State of Nevada scientists accept the fluid inclusion data as valid but claim that these data do not capture the most recent thermal history of the mountain. They therefore question the conclusion that the thermal source (whatever it is) ceased to effect YM sometime between 2 and 4 million years ago. They maintain a hypothesis that recent (less than 2 Ma) warm fluids invaded the mountain from below the repository horizon

and saturated the repository host horizon. They cite "high" salinities, elevated homogenization temperatures, and suggestions of the presence of hydrocarbons in fluid inclusions as support for their hypothesis.

None of the core group of scientists, including Jean Cline, chose to address CNWRA results suggesting polygenetic mineralization of secondary calcite crystals in the fault zones or the evidence of elevated deformation temperatures as indicated by deformation twins in fault-zone calcite.

Given the remaining uncertainties, we suggest that NRC reserve judgment on the findings until they are evaluated, within the context of repository performance, by the DOE. As per existing USFIC and ENFE Issue Resolution agreements, the DOE must provide a coherent and technically defendable interpretation of the thermal history of YM and the possible implications to performance.

PROBLEMS ENCOUNTERED:

None.

PENDING ACTIONS:

Official meeting minutes are yet to be distributed. Copies will be forwarded to NRC when the minutes are sent to the meeting attendees by Jean Cline.

RECOMMENDATIONS:

Stamatakos and Gray propose to review the UNLV paper when it becomes available and keep the NRC appraised of any other developments as they occur. We will also provide technical support to review of the DOE analyses of these findings as they become available, per existing NRC and DOE Issue resolution agreements. A paper summarizing the fault zone studies will be submitted to the NRC by Stamatakos and Gray by the end of FY01.

REFERENCES:

None.

ATTACHMENTS:

Attachment 1: Jean Cline's February 8, 2001, press release.

Attachment 2: The Las Vegas Sun news article pertaining to the February 8, 2001, meeting and press conference.

Attachment 3: The Review Journal news article pertaining to the February 8, 2001, meeting and press conference.

Attachment 4: Jerry Szymanski, "Summary Statement on the Final Outcome of the UNLV Project," February 20, 2001.

Attachment 5: Jean Cline's e-mail response to Szymanski essay.

Attachment 6: Jerry Szymanski's e-mail response to Jean Cline.

SIGNATURES:

for M.B GRAY

Mary Beth Gray
Consultant, CNWRA

MARCH 20, 2001
Date

John Stamatakos
Senior Research Scientist

MARCH 20 2001
Date

CONCURRENCE:

H. Lawrence McKague, Manager
Geology and Geophysics

MARCH 20, 2001
Date

Budhi Sagar
Technical Director

3/20/2001
Date

rae

Attachments

ATTACHMENT 1

2/8/01

PRESS RELEASE

Yucca Mountain Fluid Inclusion Thermochronology Project

We are here to report the final results of a two-year investigation of the fluid history at Yucca Mountain. This project was designed to address the following questions. First, were fluids with elevated temperatures ever present within the Yucca Mountain site? If such fluids were recognized, three additional questions would be addressed: 1) what were the *temperatures* of these fluids, 2) what was their *spatial distribution* across the repository site, and 3) *when* did these fluids move through the site?

We have answered all of these questions and the data are not ambiguous. Fluids with temperatures typically ranging from about 45 to 60 degrees C were present throughout the site in the geological past. The data provide no evidence for the presence of these fluids with elevated temperatures at the Yucca Mountain site during the past 2 million years. Some samples constrain the presence of fluids with elevated temperatures to more than 4 or 5 million years. Data obtained by the USGS and limited geochemical data reported by State of Nevada funded researchers are consistent with our results. Our results do not allow us to determine if the fluids that precipitated the secondary minerals originated as surface water or upwelling fluids. However, our observations are not consistent with a hydrothermal origin for secondary minerals at Yucca Mountain.

This study addressed the issue of the presence and timing of fluids with elevated temperatures at Yucca Mountain. We did not address other safety issues regarding the site that remain to be resolved. These issues include, but are not limited to, questions relating to potential volcanic activity, the lifetime of the nuclear waste packages, and the transport of radionuclides to the accessible environment.

Jean S. Cline
Associate Professor
UNLV

2/09/01 LAS VEGAS SUN

ATTACHMENT 2

Study: Hot water hasn't invaded site in eons

By Mary Manning
LAS VEGAS SUN

Experts have concluded after a two-year study that hot water has not invaded Yucca Mountain in 2 million years, answering at least one scientific question about the safety of using the site as a high-level nuclear waste repository.

If scientists had proven hot water had flooded into the mountain within a million years, the plan to store the nation's radioactive waste in Nevada could have been in jeopardy.

Although other studies are pending that could disqualify Yucca Mountain as a site for the repository, Thursday's announcement puts to rest one question that has been lingering for more than 15 years and removes one impediment to the project's approval, scientists said.

The findings disappointed state officials who have been fighting the project, said Robert Loux, director of the Nevada Agency for Nuclear Projects.

"Obviously, we are disappointed that ages of minerals were not discovered that would disqualify the site immediately," Loux said. "The question remains about what was happening inside the mountain 5 (million) or 6 million years ago."

See Yucca, 5A

Yucca

from page 1A

Lead scientist Jean Cline of UNLV said on Thursday that minerals in roughly half of the 155 rock samples collected throughout the mountain, 90 miles northwest of Las Vegas, indicated the presence of fluids ranging from 113 to 141 degrees Fahrenheit more than 2 million years ago, well below the boiling point of 212 degrees Fahrenheit.

Those temperatures do not indicate that geothermal water left calcite in cracks and crevices inside Yucca Mountain, she said.

"We have answered all of the questions, and the data are not ambiguous," Cline said.

The UNLV study did not answer the question of whether the mountain is safe from volcanic eruptions that could crush buried nuclear waste containers, releasing radiation into the air and ground water, she said.

The study also does not answer how long the layered metal containers will last in the mountain or how fast radiation could escape.

Other studies are looking at those questions. The Energy Department and scientists working for Nevada and the Nuclear Regulatory Commission, which must license a repository, have raised doubts surrounding the mountain's volcanic and seismic activity as well rapid water flow through its rocks.

Loux said the hot water study added important information to what was going on inside the mountain after it formed 12 million years ago and neither the DOE nor licensing regulators can ignore it.

The UNLV team's conclusion contradicts a theory by a Russian scientist, hired by Nevada, who believes hot, deep water flooded the mountain and left mineral deposits, making the Yucca site unsafe for nuclear waste disposal.

Yuri Dublyansky compared his theory of gushing hot water to geysers at Yellowstone National Park. Yellowstone is geologically young at 700,000 years and still active.

"Yucca Mountain is not Yellowstone," Cline said. "Yuri goes far beyond the bounds of the study and the topic."

The team's findings at Yucca Mountain are significant, she said, because they agree with earlier evidence collected by the U.S. Geological Survey indicating that hot water has not flooded the repository site.

Instead, the evidence from the latest study, which will be published in late March, points to minerals built in layers after rainwater had dried up at the bottom of cavities throughout the mountain.

USGS geochemist Joe Whelan, a team member, said that after nearby volcanos erupted more than 12 million years ago, it took 6 million years for the ash forming Yucca Mountain to cool down, allowing rainwater entering Yucca's earthquake faults to deposit calcite containing bubbles of water and gas known as fluid inclusions.

If hot water from deep in the mountain had flooded the repository, scientists would find calcite deposits on the ceilings and sides of those cavities, Whelan said.

But Dublyansky of the Russian Academy of Sciences Siberian Branch said he believes water rising from deep under the mountain deposited minerals indicating the site is unsafe to keep 77,000 tons of highly radioactive waste safe for at least 10,000 years.

Water temperatures cannot be explained from surface fluids trickling into the mountain, Dublyansky said, basing his theory on former DOE scientist Jerry Szymanski, who worked at the mountain 20 years ago. Szymanski posed the theory of water rising periodically into the mountain, making it unsafe to store radioactive waste.

The DOE spent $1.4 million to allow the UNLV team to answer the question of whether water hotter than the mountain's rock had ever been present in Yucca Mountain.

Geochemical expert Robert Bodnar of Virginia Polytechnical Institute, who acted as the team's consultant, said there was no evidence indicating that deep water such as that forming the geysers at Yellowstone ever reached Yucca Mountain.

Mary Manning *covers environmental issues for the Sun. She can be reached at (702) 259-4065 or by e-mail at manning@lasvegassun.com.*

ATTACHMENT 3

LV Review Journal 2/09/01

Expert disagrees with team over Yucca Mountain

Whether dump site would be exposed to hot water debated

By KEITH ROGERS
REVIEW-JOURNAL

While a team of geologists agreed Thursday that hot water existed in Yucca Mountain more than 4 million years ago, one member was still at odds over what data from a $1.4 million study mean and their significance in deciding whether to bury nuclear waste there.

Yuri Dublyansky of the

▶ HOUSE TO START ON NUCLEAR WASTE BILL PAGE 8B

Russian Academy of Sciences Siberian Branch, who represented Nevada on the team, said he is not as convinced as federal scientists are that the temperatures indicated by tiny bubbles inside minerals in the mountain stem from its cooling some 6 million years after nearby subsurface volcanic activity reheated its interior.

He said he thinks the evidence indicates thermal groundwater shot upward in the mountain, and if that happens again after nuclear waste is put there, the proposed repository could be flooded and potentially deadly radioactive materials carried off.

"My personal opinion is you can take the data ... and come to the conclusion this cannot have resulted without having (thermal) water flowing inside," Dublyansky said at a briefing late Thursday to announce the team's results.

> **"You can take the data ... and come to the conclusion this cannot have resulted without having (thermal) water flowing inside."**
>
> YURI DUBLYANSKY
> SCIENTIST REPRESENTING NEVADA

He sided with his colleague, state geological consultant Jerry Szymanski, who contends the site is flawed and is being pursued by the Department of Energy to relieve the federal government of billions of dollars in liability to the nuclear power industry. The pair argue the mountain clearly is not a safe place to store nuclear waste.

The volcanic-rock ridge, 100 miles northwest of Las Vegas, is the only site being studied to entomb the nation's high-level nuclear waste, mostly metal rods containing spent fuel pellets from commercial power reactors.

The ridge, scientists agree, was formed by hot ash that showered down from volcanic eruptions nearly 13 million years ago. Then, roughly 11

▶ SEE WATER PAGE 16B

▶ WATER: *Other team members see no proof of hot fluids within Yucca Mountain*

CONTINUED FROM PAGE 1B

million years ago, the mountain was reheated by below-ground volcanic activity from nearby Timber Mountain.

From that time, based on fluids trapped in Yucca Mountain's calcite minerals, the majority of the team members — especially geochemist Joseph Whelan of the U.S. Geological Survey's Denver office — think the minerals took 6 million years to cool to the point that temperatures were between 113 degrees and 140 degrees Fahrenheit.

Out of 155 mineral samples that were analyzed, about half, 78, contained "fluid inclusion" records indicating elevated temperatures.

"The data provide no evidence for the presence of these fluids with elevated temperatures at the Yucca Mountain site during the past 2 million years," according to a statement from Jean Cline, the University of Nevada, Las Vegas associate professor who led the two-year study.

Cline and Whelan think the elevated temperatures do not stem from hot groundwater moving inside the mountain.

"I don't think Yucca Mountain was a hydrothermal system," Cline said after the briefing. She said that "something had to cause those elevated temperatures, but our study didn't address that."

She said earlier that "there is no compelling evidence that says, 'Yes it's that story,' or 'No, it's not that story.' But it's not consistent with hydrothermal origin."

At a November conference of geologists in Reno, a Nuclear Regulatory Commission scientist, Bret Leslie, spelled out the issue in a paper.

"If flooding of the repository by warm groundwater has at least one chance in 10,000 of occurring over 10,000 years, then the performance assessment" would need to estimate radiation doses to people near the repository, Leslie wrote.

Szymanski, a former Energy Department geologist who fostered the upwelling thermal-water theory more than two decades ago, thinks the evidence means the chances of a sudden, hot-water event recurring are great enough to disqualify the site for nuclear waste disposal.

"My understanding of what professor Cline stated is that she does not take a position with regard to the upwelling water hypothesis," Szymanski said after the briefing at UNLV. "She had clarified her previous conclusion that the passage of water with elevated temperature happened prior to 1.9 million years ago. She now says she does not know if this is true or not.

"With these statements it is my personal opinion Yucca Mountain cannot be licensed as a permanent repository," Szymanski said.

He said he thinks the Department of Energy has squandered $8 billion studying a flawed site, and the federal government faces a $100 billion liability for failing to provide storage of spent fuel outside of reactor sites.

Despite the U.S. Geological Survey scientists' insistence the minerals were formed by rainwater that percolated from Yucca Mountain's surface, Szymanski and Dublyansky hold to their claim that minerals were formed by hot water from within and that the mountain did not take 6 million years to cool.

"That mountain was cold 100,000 years after eruptions," Szymanski told the team.

ATTACHMENT 4

Summary Statement on the Final Outcome of the UNLV Project

By Jerzy S. Szymanski, February19, 2001

Independent consultant to the Office of the Attorney General of the State of Nevada

The UNLV Project has yielded a set of reliable scientific data concerned with a mineral assemblage that occurs in the unsaturated zone at Yucca Mountain. The data allow us to address two questions of critical importance for establishing the suitability of Yucca Mountain as a permanent repository for commercial spent nuclear fuel and high-level radioactive waste from military defense activities.

The first question deals with the origin of the minerals in the assemblage. Were they precipitated from rainwater or, alternatively, were they precipitated from hot or warm water that ascended from the deep interior of Yucca Mountain? The answer to this question is critical to determining whether or not Yucca Mountain is licensable pursuant to regulations set forth by the Nuclear Regulatory Commission. The second question deals with the ages of the assemblage. Specifically, can this assemblage be dated with accuracy and precision sufficient to identify the most recent deposition of the minerals? Answering this second question will allow us to better quantify the magnitude of the danger to which the humanity would be exposed should the proposed repository be sanctioned.

A compelling first conclusion from the UNLV data set is that the minerals as a whole could not possibly have been formed from rainwater. As reported by Dr. Cline, the principal UNLV researcher, in roughly half of the 155 samples analyzed from the mineral assemblage, the temperatures of the parent fluids ranged from 35 to about 70 °C. Elevated water temperatures were identified in samples that presumably could have been deposited as late as 1.9 - 4.0 Ma ago. Unless it is accepted that the mountain took up to several Ma to cool from the last known hydrothermal episode 10 - 11 Ma ago, an impossibility from any scientific perspective but one nevertheless supported by the USGS, the only possible origin for the mineral assemblages is the precipitation from ascending hot water.

Although the scientific facts are sufficient to establish the hot-water origin for these minerals, the UNLV researchers neither refute nor support such origin when questioned directly for the record. The unequivocal definition of this origin, however, is clearly a starting point for all considerations of Yucca Mountain as a site where the radioactive waste permanent disposal and isolation from the biosphere could be achieved with the required degree of safety. The neutrality in this regard demonstrates that the UNLV had become in fact a partisan political fief of the USGS, an agency which has been engaged in promoting the rainwater sham for decades, whilst backing up the claims for intellectual

honesty by empty rhetoric. How else can one rationalize the UNLV researcher's public statements, which although intentionally ambiguous nevertheless have the effect of telling the public that they have confidently established the geologic antiquity of the hot-water events? The UNLV researchers, however, have admitted during the course of the February 08 meeting that, in fact, only a different statement is supported by their data, namely that these data do not allow any conclusions to be drawn in this regard.

In addition to carefully avoiding the core issue, which is the origin of the minerals in the assemblage, the UNLV researchers also refuse to provide an interpretation in regard to a potential meaning of many facts that were established during the investigation. Examples of such neglected facts are uncommonly numerous. The most notable are: the stable isotope signatures, the spatial distribution of the measured homogenization temperatures, the measured salinity, the presence of accessory minerals (apatite, strontianite, heulandite, barite, fluorite, quartz, gypsum), the morphology of calcite, the origin of Mg-enriched calcite, and the presence of large all-gas inclusions. Scientific interpretations of this evidential data would have provided additional support for the ascending water origin of the mineral assemblage. As Poincaré has remarked: "*Science is built up with facts, as a house is with stones. But a collection of facts is no more science than a heap of stones is a house.*"

It is obviously necessary that the missing interpretations should be constructed by allowing only true statements to be derived from statements previously known to be true and, for these interpretations to be accepted by the scientific community, they must be free of contradictions. But how is it possible to establish that an interpretation that has not yet been constructed is free of contradictions, and further that it has been derived only from statements previously known to be true? The avoidance of interpretations prevents a detection of the contradictions and thus creates a situation wherein neither correctness nor falseness of a thesis can be established. Politicians know the trick "*say nothing and nobody can hold you wrong*".

The implications of taking a position in regard to the origin of the mineral-forming water are profound. An unequivocal statement by the UNLV researchers that the minerals formed from ascending hot water would send a message to the Congress that, despite an investment of several billions of dollars, Yucca Mountain site must be abandoned. Such a statement would put the Congress on notice that it will have to address the inevitable task of providing new direction for a solution to the serious national problem of accumulating spent fuel at the Nation's reactor sites. It would further advise the Congress that it not only must cut the losses, in terms of time and resources lost in the failed Yucca Mountain Project, but it must also contain the skyrocketing liability it has incurred in contracts with

the nuclear utilities. These utilities have contributed to the Nuclear Waste Fund in reliance on the Federal Government's obligation to provide for out-of- reactor storage and permanent disposal of their spent fuel. Such a statement would require the kind of courage that the UNLV researchers have been loath to demonstrate to date.

Short of the message being received and understood by the Congress, the course of the future developments is sufficiently pre-determined to be largely predictable. First and foremost, the Secretary will approve the Site Recommendation Report. The President will then recommend Yucca Mountain for development as a permanent underground repository. Next, the State of Nevada will prepare the Notice of Disapproval under the leadership of Robert Loux, which the Governor and the Legislature will file with the Congress. This document would advise the Congress that the State of Nevada objects to the recommendation and would also specify issues that supposedly justify this action. A statement of reasons in support of the Notice of Disapproval will be largely based on the fact that the transportation is risky, that the EIS has not been yet finalized, that the socioeconomic impact is excessive, that the site contains a fairly recent volcano as well as a number of earthquake-faults, etc. Of course, the Congress will be fully apprised by the DOE of the consequences of these shortcomings, that remedial measures may be employed, and further that the magnitude of the problem requires a political decision which assumes a degree of risk. Therefore, it is reasonable to expect that the Congress will override the Notice of Disapproval. The next logical step would be legislation that allows for the construction of an "interim" storage facility at the Area 25 of the Nevada Test Site. It is unlikely that the President will veto such legislation, and thus the "interim" storage facility, unwelcome to some and potentially ruinous to the others, will become a reality.

However, those of us who are familiar with the facts, which were established by the UNLV Project, already know that Yucca Mountain cannot possibly be licensed as a permanent repository. Of course, this expectation stems from an assumption that the licensing action will be based solely on scientific evidence and reasoning, and that this process will reject the "logic" and the "facts" similar to those that appear in the Mary Manning's newspaper accounts. [See in this regard the November 08, 2000 and February 09, 2001 issues of the Las Vegas Sun.] Further, some of us already know that Yucca Mountain is a very dangerous place to dispose of spent nuclear fuel and high-level radioactive waste, although only a few are willing to go on record as to their views. A nuclear catastrophe that could dwarf the Chernobyl event is not only possible here, but also it is highly probable during a period when most of the disposed spent fuel will still remain fissile and lethal.

By any standard of judgement, the UNLV data show that the controversial minerals are less than 10-11 Ma old, that the minerals were precipitated intermittently during the past 9 Ma, and further that they are of a single origin. In spite of the limited number of age determinations so far completed by the UNLV researchers, as well as the questionable reliability of these determinations, the results already are very revealing. They demonstrate that, in accordance with the 10 CFR Part 63, Yucca Mountain cannot be licensed by the Nuclear Regulatory Commission as an underground repository. Regardless of the actual minimum age of the minerals, it follows from the UNLV findings, taken at a face value, that the resulting minimum annual probability of flooding of the repository is at least two orders of magnitude greater than that permitted by the licensing standard. The DOE cannot demonstrate with reasonable assurances that the expected radiation doses resulting from a repository-flooding scenario will not exceed the radiation limits established by the EPA. Clearly this would be a mission impossible. However, responding to the UNLV findings and to questions raised by the NRC staff regarding these findings, the DOE has promised additional investigations to be completed in the year 2002. In the meantime, the DOE can be expected to promote an "interim" storage facility as a reasonable solution to the pressing problem, and clearly that this would be in the national interest.

In view of the unlicensability of Yucca Mountain, only two outcomes are possible. Either the spent nuclear fuel will be transported twice (i.e., in and out of the Nevada Test Site) or this fuel will remain in *de facto* permanent storage above the ground where it would be exposed to many perils. By any standard of judgement, such outcome of the Yucca Mountain saga can hardly be regarded as responsible and ethically correct.

The preceding predictions are predicated on the assumption that the UNLV researchers lack the courage to reach and disclose the conclusions that are clearly demanded by the facts they have established. Such an abdication of scientific and civil responsibilities is misleading to the public and to public officials, who have responsibility for nuclear waste disposal decisions, and would poorly serve the scientific community, upon which the public relies for guidance.

It is abundantly clear to me, from the February 08 final large-group meeting of the UNLV Termochronology Project committee, that this project has been a very important step towards exposing the rainwater sham. This is because the Project has, in fact, produced a reliable database upon which conclusions may be based. A logical and focused interpretation of the established facts may readily be constructed from this database by only allowing true statements to be derived from statements previously known to be true, and then by demonstrating that the result is uniquely free of any contradictions. The most

important among the previously known true statements is the fact that magmatic bodies in the Earth's crust, such as the Timber Mountain granite batholith, are known with certainty to hav solidified and cooled fairly rapidly via the combined conductive and advective transfer of heat into the atmosphere. It is impossible therefore to justify and accept a notion that this process involved only the conductive transfer, and that it could have lasted for a minimum of about seven Ma. The other true statement is that the studied minerals are of a single origin, which means that they must have been formed either from rainwater that descended through the hot mountain or, alternatively, from hot water that ascended into the cool mountain. Once both of these statements are accepted, then the next logical step is to evaluate truthfulness of the first premise, which is the *a priori* assumption that the UNLV $^{207}Pb/U$ and $^{230}Th/U$ ages reflect true ages of deposition for the secondary minerals studied.

The reliability evaluation must be made by keeping in mind the contrasting geochemistry of fluids that would be associated with the competing mineral-precipitation models, as Dr. C. B. Archambeau previously has suggested. In this regard, the upwelling model calls for the partial involvement of solutions, which would have been accumulating for a very long time span (say 10 000 years) in the pre-Cambrian basement at a depth of say 10 - 15 kilometers. The accumulated solutions would then be mobilized and brought up to the Earth's surface by a seismic pumping mechanism. These initially very hot (say 300 - 400 °C), stagnant, saturated with juvenile CO_2, and reducing solutions could reasonably be presumed to have been in a state approaching the secular isotope equilibrium. In the case of uranium-235, this means that the activity of all the intermediate daughter isotopes is close to that of the ^{235}U parent. With the occurrence of a seismic criticality along the Paintbrush and/or Solitario Canyon fault, however, they would be transported to the surface with a high velocity, probably in the range from 1 to 10 m/sec. As the seismically induced flow would become more established, however, a Bernoulli effect would become operational, which would draw fluids that are shallower into the ruptured fault. Such fluids reside in the Paleozoic aquifer, and would have a higher redox potential. A more sustained inflow of these fracture-based fluids would follow, and this inflow would be a result of invigoration of the pre-existing Rayleigh-Bernard instability, which have occupied hydraulic conductivity channel surrounding the ruptured fault.

Thus, by contrast to the descending rainwater hypothesis, the competing hypothesis requires that the mineral-forming solutions be in the form of a time varying mixture of two end-members. One of these end-members would consist of the reducing solutions from the dilated hypocentral region of an earthquake and the other would involve shallower, more oxidizing and mainly fracture-based fluids from the Paleozoic aquifer. Needlessly to say, these two end-members would have quite different $^{234}U/^{238}U$ and

^{235}U/(daughters of ^{235}U) ratios. Specifically, both of these ratios would be high for the fracture-based aquifer fluids, but small for solutions that were accumulating inside the nucleating hypocentral region. The time varying intermixing of the two end-members therefore would lead to systematic changes in uranium isotope chemistry of the mineral-forming solutions.

The assurances that the maximum possible age uncertainty is constant and equal to about ± 50 Ka, which were given by Dr. Amelin during the February 08 meeting, are most certainly valid, but only in cases that involve supergene and therefore oxidizing solutions. However, they do not have any relevance whatsoever to the tectonically mobilized fluids. This is because these fluids happen to be a result of time varying intermixing of fluids that were first mobilized by seismic pumping with those that were then mobilized by Bernoulli and Raleigh-Bernard processes.

Because the ascending water model calls for the time varying intermixing of oxidizing and reducing aqueous solutions, which is in contrast to the rainwater model, this model does not allow the $^{207}Pb/^{204}Pb$ common lead correction to have a constant value. In addition, this model does not require the corrected initial ^{207}Pb abundance to be a sole result of the ^{235}U radioactive decay. This is because the assumed absence of the intermediate daughters (such as ^{231}Th, ^{231}Pa, ^{227}Ac, ^{227}Th, etc), as well as the assumed insolubility of ^{207}Pb and ^{211}Pb, are not valid in case of the seismically mobilized solutions.

As the redox potential (or the Gibbs free energy) increases with the decreasing intensity of seismic pumping, from a minimum at the initial stage of an upwelling event to a maximum at the end of this event, the ^{235}U daughter's solubility decreases, which reduces their abundance in the composite solutions. At the same time, the increasing intensity of inflow of the Mg-enriched and fracture-based aquifer fluids, which were previously affected by the α - recoil mechanism, has the effect of steadily increasing the $^{234}U/^{238}U$ ratio in the composite solutions. These aquifer fluids likely carry the dissolved uranium in the form of uranyl ion, which has +6 valence. In such an oxidation state and under the presence of abundant carbonate complexing ions, uranium solubility is quite high. The increasing input of the Mg-enriched fluids therefore might have the effect of actually increasing the abundance of uranium isotopes in the composite solution, but most certainly not significantly reducing this abundance. This together with the decreasing abundance of the ^{235}U daughters has the combined effect of steadily increasing ^{235}U/(daughters of ^{235}U) ratio in the composite solution, in addition to increasing the $^{234}U/^{238}U$ ratio.

Under the changing conditions, the coagulating gelatinous opal would scavenge less and less of the ^{235}U daughters and increasingly more of the ^{234}U daughter, whilst the scavenging of uranium isotopes would remain relatively less affected by the mixing. Once solidified, the opal sequence would have an abundance of ^{207}Pb and ^{230}Th that increases with the increasing micro-stratigraphic depth. If these were converted into the $^{230}Th/U$ and $^{207}Pb/U$ radiometric ages, a process which necessarily would have to involve assuming that the $^{234}U/^{238}U$ = const. and the $^{207}Pb = f(t,^{235}U_{in})$, then a mineral that has been precipitated over a short time span (x) would appear as having been precipitated over a much longer time span ($x + \Delta x$). The Δx would be relatively small for the youngest apparent age, but much larger for the oldest apparent age, although it would always have to be smaller than 9 Ma.

The evidence that the redox potential indeed has been increasing during a hiatus-free growth of some of the coatings studied is provided by the UNLV stable isotope profiles, as well as by the profiles that were obtained by Dr. Dublyansky. This evidence is in the form of del ^{13}C values, which steadily decrease with the decreasing micro-stratigraphic depth, from + 8.0 to about - 8.0 per mil wrt. PDB. As I have explained earlier, this conspicuous change expresses an increase of the fugacity of O_2 (i.e., the Gibbs free energy, dG = const. × dlnf), from $\log fO_2 = -37$, or less, to some higher value. The UNLV researchers of course did not attempt to interpret their stable isotope profiles, and therefore they have missed this subtle yet crucially important evidence.

Both the increasing Eh and the resulting increasing $^{234}U/^{238}U$ and ^{235}U/(daughters of ^{235}U) ratios in the mineral-forming solutions are features that might reasonably be inferred to be only associated with the upwelling scenario. In the case of descending rainwater, these features would be of course absent. Thus, the $^{230}Th/U$ and $^{207}Pb/U$ ages would be approximately reflecting true deposition ages for the two-phase inclusion bearing minerals, but only if these minerals would have been formed from descending rainwater. However, no such claim could be made in case of the competing origin.

The USGS attempt to salvage the rainwater hypothesis by "demonstrating" that the controversial minerals were precipitated "*at slow and relatively constant rates*", a conclusion which is based on the $^{207}Pb/U$ and $^{230}Th/U$ ages, amounts therefore to a deduction process that involves circular logic. Any such process is of course devoid of scientific validity. This is because the conclusion (i.e., the rainwater origin) is a disguised form of the main premise from which this conclusion has been derived. Of course, the premise here is that the $^{207}Pb/U$ and $^{230}Th/U$ ages are valid because the host minerals were formed in a vadose and therefore oxidizing and chemically stable environment. Based on my nearly 20 year, more or less active involvement in confronting the rainwater sham, I

am entitled to state that similarly circular logic has always been the trademark of the USGS. Surprisingly none of the reviewers and editors of their "scientific" papers has recognized it as such. Erroneous logic remains just that, whether published in some prestigious journals or not.

The record shows that Dr. Dublyansky has raised the age reliability question at the onset of the UNLV research, however, this crucial questions has been ignored in the subsequent agendas, and has not been adequately addressed so far. This has the effect that the rainwater hypothesis and the upwelling water hypothesis are both seemingly inconsistent with observations. Because one of the competing hypothesis must be true, then it follows that the main premise, which is the correctness of the $^{207}Pb/^{235}U$ ages, must be false. The alternative would be that no model fits the observed data.

I will attempt to explain why the UNLV $^{207}Pb/U$ age determinations cause both of the tested hypotheses to appear to be inconsistent with observations. If these ages were taken at a face value, then it follows that the temperatures that exceed the present-day ambient temperature, by as much as a few tens of °C, persisted at a depth of only 50 - 200 meters for a period as long as several Ma. This is because the homogenization temperatures were derived from minerals that give the strong appearance of having been formed continuously, under a very gradually relaxing thermal regime, and in an environment characterized by a very gradual increase and decrease of the del ^{18}O and del ^{13}C values, respectively.

Within the context of the rainwater hypothesis, the combined homogenization and $^{207}Pb/U$ data would have to be explained by assuming that these data indicate a prolonged cooling of a hypothetical magmatic body in the crust. And further that this body would be transferring its heat content into the surrounding rock and atmosphere via conduction alone. The measured homogenization temperatures imply that the resulting heat flow would have an average intensity of at least 400 mWm^{-2}, and the $^{207}Pb/U$ ages are telling us that this intensity has endured for a period of several Ma. Although the proposal that a magmatic body has cooled by conductive transfer of the heat is a geologic impossibility, let's assume nevertheless that such a process is feasible. A 1760 km^3 body of basalt magma emplaced at a depth of 10 km with T = 1200 °C, which is about twice as hot as a body of granite magma, would produce an increase in the conductive heat flow having a maximum intensity of about 52 mWm^{-2} at the surface. This increase would be directly above the body's center, and would occur about 0.8 Ma after the emplacement. This maximum intensity, however, would decline quite rapidly attaining 20.0 and about 5.0 mWm^{-2}, at 2.0 and 4.0 Ma after the emplacement, respectively. At a depth of 200 meters, the resulting maximum increase in temperature would range between only 5.6 and 0.5 °C,

at 0.8 and 4.0 Ma after the emplacement, respectively. Thus, even under the impossible assumption, the rainwater hypothesis is incapable of accounting for the measured UNLV homogenization temperatures. Of course, in reality, convection of groundwater when heated by magma would greatly reduce both of these time estimayes.

Within the context of the competing hypothesis, however, the combined UNLV data would have to be taken as indicating the continuous presence of hot water over a time span as long as several Ma. The combined conductive and advective heat flow could attain values that are measured in terms of a few hundreds of mWm^{-2}, but this would only be possible in association with very short-lived (say 1000 - 10 000 years) bursts of hydrothermal activity. Thus, it is impossible to explain the duration of heat flow having average intensity that is measured in terms of a few hundreds of mWm^{-2}, which is implied by the UNLV $^{207}Pb/U$ age determinations. This is because the implied cumulative output of heat, through an area of which Yucca Mountain is only a small part, would be huge enough to be in direct violation of the first law of thermodynamics. This law instructs us that the change of energy of a thermodynamic system is equal to the heat transferred minus the work done. In order to avoid violation of the first law, however, one would have to assume that the intense heat flow was present only intermittently, and that in the meantime heat flow with much lower intensity was operational. If such were the case, however, how would it be possible to explain the evident continuous growth of the minerals that have yielded the elevated temperatures? It is this continuous growth in combination with the measured homogenization temperatures that leaves us with little choice by to conclude that the UNLV age determinations must be erroneous.

Thus, rather than delivering a decisive blow to the upwelling water issue, as appears to be the DOE objective from the start of the UNLV Thermochronology Project, this Project has in fact produced the database that may be used in exposing the bogus rainwater hypothesis. This project therefore may rightly be regarded as a beginning of the scientific end for the Yucca Mountain Project. The political end of this project, however, is a different matter all together. It will, necessarily, be informed and guided by the scientific input, whether true or falsely contrived.

In closing, I will take the liberty of reminding the UNLV and USGS researchers of their responsibilities as scientists. Society is necessarily reliant upon the honesty and skills of scientists for accurate and balanced analyses of matters that concern public well being. Scientists are not infallible and society will accept genuine scientific error. But society does not forgive the knowing portrayal of a falsehood as scientific truth. With the passage of time, exposure of a falsehood is an inevitable consequence of the scientific process. Public intolerance becomes most extreme if it learns that it has become the victim of

unjustifiable reassurance by intentional misrepresentation, or distortion, of scientific findings, for which it has paid handsomely. In the context of the licensing of nuclear facilities, this will constitute perjury. Here the rule is tell it as it is (that is, "the truth and nothing but the truth") otherwise society's intolerance is expressed as prosecution under the law.

Jean Cline, 2/21/01 7:45 PM -0500, Re: Szymanski reply ATTACHMENT 5 1

Date: Wed, 21 Feb 2001 16:45:09 -0800
To: pattieb@nevada.edu, bubbles@vt.edu, william_boyle@ymp.gov, jwb@nrc.gov,
jcline@nevada.edu, drew_coleman@ymp.gov, wld@nrc.gov, rdowning@nrff.com,
kyoto_yuri@hotmail.com, mbgray@bucknell.edu, bwl@nrc.gov, cjg1@nrc.gov,
psj@nrc.gov, marilyn_kavchak@ymp.gov, sslevy@lanl.gov, bloux@govmail.state.nv.us, lundberg@nevada.edu, bdmarsha@usgs.gov, Alan_Mitchell@notes.ymp.gov, lneymark@usgs.gov, jbpaces@usgs.gov, peterman@usgs.gov, mtpeters@lanl.gov, max_powell@notes.ymp.gov, jprice@nbmg.unr.edu, reiter@nwtrb.gov, roedder@shore.net, rotertj@nevada.edu, drunnells@shepmill.com, wbscott@usgs.gov, smiecins@nevada.edu, jstam@swri.org, stellanick@aol.com, stetzenb@nevada.edu, stuckles@usgs.gov, szymchtr@intermind.net, evt@co.clark.nv.us, jfwhelan@usgs.gov, nwilson@nevada.edu, szeee@govmail.state.nv.us
From: Jean Cline <jcline@nevada.edu>
Subject: Re: Szymanski reply

Mr. Szymanski:

Are you threatening me with prosecution if I do not support your theory?

Jean Cline

>Dear All,
>Enclosed please find the attached "Summary Statement on the Final Outcome
>of
>the UNLV Project". I hope that this summary be useful in preparing the
>forthcoming scientific publications by the UNLV and USGS researchers.
>Regards, Jurek

Jean S. Cline
Associate Professor, UNLV
Department of Geoscience
4505 Maryland Parkway, Box 454010
Las Vegas, NV 89154-4010

702/895-1091 (phone)
702/895-4064 (fax)
jcline@nevada.edu

Szymanski, 2/22/01 12:51 AM -0500, Re: Szymanski reply ATTACHMENT 6

Date: Wed, 21 Feb 2001 21:51:47 -0800
Subject: Re: Szymanski reply
From: "Szymanski" <szymchtr@intermind.net>
To: Jean Cline <jcline@nevada.edu>, pattieb@nevada.edu, bubbles@vt.edu, william_boyle@ymp.gov, jwb@nrc.gov, jcline@nevada.edu, drew_coleman@ymp.gov, wld@nrc.gov, rdowning@nrff.com, kyoto_yuri@hotmail.com, mbgray@bucknell.edu, bwl@nrc.gov, cjg1@nrc.gov, psj@nrc.gov, marilyn_kavchak@ymp.gov, sslevy@lanl.gov, bloux@govmail.state.nv.us, lundberg@nevada.edu, bdmarsha@usgs.gov, alan_mitchell@notes.ymp.gov, lneymark@usgs.gov, jbpaces@usgs.gov, peterman@usgs.gov, mtpeters@lanl.gov, max_powell@notes.ymp.gov, jprice@nbmg.unr.edu, reiter@nwtrb.gov, roedder@shore.net, rotertj@nevada.edu, drunnells@shepmill.com, wbscott@usgs.gov, smiecins@nevada.edu, jstam@swri.org, stellanick@aol.com, stetzenb@nevada.edu, stuckles@usgs.gov, evt@co.clark.nv.us, jfwhelan@usgs.gov, nwilson@nevada.edu, szeee@govmail.state.nv.us
X-Priority: 3

Dear Dr. Cline,
Of course NOT. All I can and should do is to try to remind about your responsibilities as a scientist and a citizen.
Regards, Jurek

>From: Jean Cline <jcline@nevada.edu>
>To: pattieb@nevada.edu, bubbles@vt.edu, william_boyle@ymp.gov, jwb@nrc.gov, jcline@nevada.edu, drew_coleman@ymp.gov, wld@nrc.gov, rdowning@nrff.com, kyoto_yuri@hotmail.com, mbgray@bucknell.edu, bwl@nrc.gov, cjg1@nrc.gov, psj@nrc.gov, marilyn_kavchak@ymp.gov, sslevy@lanl.gov, bloux@govmail.state.nv.us, lundberg@nevada.edu, bdmarsha@usgs.gov, Alan_Mitchell@notes.ymp.gov, lneymark@usgs.gov, jbpaces@usgs.gov, peterman@usgs.gov, mtpeters@lanl.gov, max_powell@notes.ymp.gov, jprice@nbmg.unr.edu, reiter@nwtrb.gov, roedder@shore.net, rotertj@nevada.edu, drunnells@shepmill.com, wbscott@usgs.gov, smiecins@nevada.edu, jstam@swri.org, stellanick@aol.com, stetzenb@nevada.edu, stuckles@usgs.gov, szymchtr@intermind.net, evt@co.clark.nv.us, jfwhelan@usgs.gov, nwilson@nevada.edu, szeee@govmail.state.nv.us
>Subject: Re: Szymanski reply
>Date: Wed, Feb 21, 2001, 4:45 PM
>

>Mr. Szymanski:
>
>Are you threatening me with prosecution if I do not support your theory?
>
>Jean Cline
>
>>Dear All,
>>Enclosed please find the attached "Summary Statement on the Final Outcome
>>of
>>the UNLV Project". I hope that this summary be useful in preparing the
>>forthcoming scientific publications by the UNLV and USGS researchers.
>>Regards, Jurek