MIFFLIN AND ASSOCIATES, INC.
HCR 38 Box 126
Las Vegas, Nevada 89124
702-645-2969 702-395-2981 Fax
Email maihydrogeol@msn.com

MEMORANDUM

To: Richard R. Parizek, Chair, Panel on the Natural System, U.S.N.W.T.R.B.
From: M.D. Mifflin, Mifflin and Associates, Inc.

Re: Written comments on the March 9 and 10, 2004 meeting, Panel on the Natural System, Crown Plaza Hotel, Las Vegas, Nevada

Please accept these additional written comments, as time allotted did not allow verbal transmittal at the March 10, 2004 session. These address saturated zone, vadose, and climate change related presentations on March 9 and 10, 2004 in Las Vegas, Nevada

Climate Change Comment

There are Yucca Mountain site characteristics related to forecasted climate changes leading to repeat periods of markedly increased fluxes within the vadose zone and saturated zone. These climate change induced hydrologic states eliminate long-term waste isolation postulated for the site because of the arid climate. No amount of engineering design (nor plausible performance) would likely prove effective due to the magnitudes of climatic variations, their cyclic nature, and forecasted durations. The original site selection criteria (DOE and NRC) treated climate change very seriously by making it a disqualifying criterion. The DOE site selection program scientists failed to acknowledge forecasted climate changes would significantly change the hydrology of the candidate site, a position that put the site in contention for the nation's HLW repository. DOE appears to accept 400,000-year isolation related cycles of varied climatic states based on varied insolation to the higher latitudes of the northern hemisphere. A "monsoonal" (2.7x) and "transitional" (3.9x) effective moisture (relative to current climate effective moisture) are being forecasted and evaluated for the 10,000-year performance period. This is a fundamental program shift in terms of site evaluation, even though the adopted effective moisture multipliers are open to question.

Effective moisture" is the name of the game, as it is a measure of the changing magnitudes of hydrologic processes that accompanied past variations in climates of the region. A TSPA issue is how quantitatively different "pluvial" state "effective moisture" might be during the performance of 10,000 years. Accurately forecasting this allows for the estimated hydrologic fluxes of current climate to be adjusted to the pluvial state effective moisture fluxes, for example, the 2.7 times or 3.9 times current climatic fluxes.

I'm not going to argue about conservative values for climate change "effective moisture" other than to discuss a multiplier value of an order of magnitude increase that was established in Mifflin and Wheat (1979) for the Great Basin region for the last two pluvial climates. Subsequent studies have better dated the pluvial shorelines and groundwater-discharge deposits used in that study, and better constrained climatic parameters of the pluvial cycles that produced almost equal maximum lake-stage shorelines. The earlier highest lake stage occurred between 40,000 and 20,000 years ago, and the "Younger Dryas" highest lake stage is better dated at about 13,500 years ago. These shoreline deposits are very similar in maximum stage (elevation) when both can be separated and recognized, and therefore document equal hydrologic results (apparent effective moisture), *even though the climates were apparently different*. Biologic and other proxy lines of evidence indicate colder and relatively modest increases in precipitation characterize the climates of the earlier pluvial cycle, and cool temperatures and considerably greater precipitation characterized the last pluvial climates. For the purpose of forecasting future effective moisture (hydrologic response) they were close to equal in producing pluvial lakes which expanded to the same surface areas.

Mifflin and Wheat (1979), like many prior investigators, were attempting to demonstrate the pluvial climates that produced markedly different paleohydrologic conditions than current arid and semi-arid climate hydrology throughout the Great Basin. Internal drainage combined with "pluvial" climates markedly increased "effective moisture" and all the associated hydrologically related features. The most important are well preserved pluvial lake shoreline deposits and landforms, and local areas termed "paleodischarge deposits" or "paleospring deposits" related to shifted locations and markedly expanded groundwater discharge areas. Most are located in southern basins where both hydrographically closed basins and pluvial lakes are generally absent. The groundwater-discharge deposits are generally very distinctive fine-grained deposits, and for many years they were mistakenly interpreted as "lake" deposits as well. The "paleodischarge deposits" in the Amargosa Desert were mapped as "lake" deposits in the early years of the Yucca Mountain project by the USGS surface mapping team. These are now recognized by the USGS as paleodischarge deposits and have been dated in some areas, with results demonstrating at least three pluvial cycles recorded at the same locality. However, perhaps the site performance significance of these deposits has not fully penetrated the DOE program---these are the prime candidate areas for the mobile longer-lived radionuclides (transported as solutes) to concentrate at and near land surface after engineered barrier failures during pluvial state climates. Most would be transported during the future climatic periods of greater effective moisture.

I suggest the Mifflin and Wheat (1979) investigation to understand "effective moisture" and the regional evidence for the order of magnitude of hydrologic differences between current climates and two well-documented pluvial state climates. The key quantitative relationship is that pluvial climate hydrologic fluxes were about ten times or more the modern climatic hydrologic fluxes throughout the region studied. Considering that the pluvial climate fluxes were quantified at the bolsons (lowlands within the basins) after catchment (tributary) area evaporation and evapotranspiration losses had already occurred indicates a reasonable, but not totally conservative, quantitative index of pluvial effective moisture was developed directly from measured lake areas and tributary basin areas. Of considerable significance is that the "Younger Dryas" or last pluvial cycle was not triggered directly by insolation minima in the northern hemisphere. This suggests to me that the 10x current climate effective moisture constitutes a documented "transitional" climate maxima for this region.

The climate change hydrologic impact criterion should have disqualified the site during site selection. DOE ranked Yucca Mountain the best of the three finalist sites and assured Congress that everything known indicated the site could be licensed. However, disposal of HLW at the Yucca Mountain site assures long-lived mobile radionuclides reconcentrated at or very near land surface in the pluvial state groundwater discharge areas in the Amargosa Desert and also dispersed over greater areas through eolian and surface-water transport (to as far as Death Valley pluvial lake extents). The site was unacceptable between1982 and 1987 on the basis of climate change site selection criterion and the concurrent knowledge of climate change hydrology in the region. Better databases add detail to the climate-change problems, but they are still intractable as far as HLW disposal in the vadose zone. The NRC "credit" life for that generation of waste canisters (early 1980's) was internally considered to be 350 years (this may not have been official). Twenty years later its unclear just how prolonged engineered barrier performance can be assured, particularly as tunnel moisture supplies would vary markedly over time and the thermal loading design would promote highly aggressive geochemical environments.

Colloid Transport

A presenter's comment illustrates how regulatory focus often deflects focus in the repository program from more fundamental questions. 239 Pu is a colloid that will likely be sorbed and not reach the accessible environment. This was noted, and therefore this helps the release and dose calculations. Hurrah! But what about waste-emplacement tunnel inventories of 239 Pu that, indeed, partition and move by colloidal transport to local, and potentially very selective sites of reconcentrations? This, to my understanding is an unresolved problem. One study published in the mid-1990's specifically addressed the question of autocatalytic criticality in the proposed Yucca Mountain repository. The expert opinion (a host of coauthor experts) was that the engineered barriers at the Yucca site would likely last until the 239 Pu inventory decayed away! This is something like 150,000 or more years. This was the only offered solution to the hazard.

These experts adopted the DOE "dry" site uniformly distributed fluxes at the time of the study (early 1990's) and also believed in their engineered barrier materials. They also assumed the hydrothermal phase would be brief and relatively unimportant to performance and pluvial states would provide uniform ceiling drips. Not in tunnels I know! There is very little evidence to support these assumptions, as they were assumptions based on a daisy chain of assumptions. I have trouble not envisioning capable reconcentrated configurations of 239 Pu for autocatalytic criticality events 1) within the canister remains, 2) within waste emplacement tunnels (configurations that could be large multiple canister inventories), 3) below the repository in local fault and fracture zones, and 4) at or near the uppermost saturation where vertical fracture or fault permeability becomes much reduced within the Calico Hills, and lateral flow in the rock matrix traps the colloids. The most indepth evaluation of the autocatalytic criticality questions was not based on conservative hydrogeologic and hydrogeochemical perspectives. I urge that the Board give this aspect systematic, indepth attention because there is little evidence to anticipate prolonged waste-package performance (but contrary evidence). If this is the "solution" to 239 Pu autocatalytic criticality hazards presented by the proposed inventory, it's unacceptable at the Yucca Mountain site with the proposed thermal loading design and climate change realities.

If the Board addresses the 239 Pu hazard in depth, I believe the Board should also focus on 235 U hazards, far more controversial. Repeated (cyclic) order of magnitude greater pluvial fluxes through the repository over the inventory life of 235 U would mobilize and transport a significant part of the uranium inventory as it decays to safe enrichment levels over several million years. This mobile (somewhat soluble in oxidizing groundwater) thermally fissile radionuclide would tend to be concentrated at the pluvial climate discharge areas as carbonates and oxides. The 235 U enrichments (based on enrichment levels in spent fuel and defense waste) are known to be capable of autocatalytic criticality events in porous geologic media. The same experts concluded the highly limited fluxes (combined with limited solubility, subsequent dispersion, and long flow paths) would prevent the necessary configurations for autocatalytic criticality from developing. Pluvial climate fluxes are likely to be an order of magnitude (or more) than used in "dry" site calculations, tunnel inflows could be highly localized rather than uniformly distributed drips, and the shallow downgradient flow paths will, without any doubt, converge at one or more of the pluvial hydrologic state discharge areas (highly localized) during the wetter climates which dominate the forecasted future. A high percentage of discharge flux is transpired or evaporated, which assures that the majority of mobilized uranium transported in solution would become concentrated (and reconcentrated?). These paleodischarge environments were, and will again be, constituted by springs and outflow channels, wet meadows, spring ponds, marshes, and shallow water tables of phreatophyte flats. These environments of deposition are well documented based on sedimentalogical and faunal evidence, as well as excellent modern analogues provided by groundwater discharge areas in Central and Northeastern Nevada (see Mifflin and Quade, 1988, and references therein). They incorporate environments that are ideal to concentrate, and reconcentrate, uranium enriched at levels capable of autocatalytic criticality in porous geologic media, enrichments currently projected for disposal. The same group of experts pointed out a potential solution to the autocatalytic criticality hazard posed by the enriched uranium---adding depleted uranium to bring net enrichment down to the level that does not allow capable autocatalytic criticality configurations in porous geologic media---but this is not in any design document---and it is a major design departure.

Criticality is a very sensitive as well as complex topic in the HLW program, and a credible autocatalytic criticality hazard related to the thermally fissile 235U in HLW is controversial. There is recognized an overmoderated accumulation process that might allow positive feedback (high energy release) criticality events in porous geologic media, but only under very demanding conditions that are deemed "highly unlikely". Nevertheless, this general topic needs more study, because I also note there is a very common (and apparently unrecognized) geologic process related to the overmoderated scenario that seems capable of leading to and possibly triggering supercritical configurations in unconsolidated in porous media. Overmoderated configurations (pore water allowing configurations to form in porous media that are concentrated enough to be supercritical with less moderating pore water) is the theoretical general process that might allow configurations leading to explosive events. However, a very common geologic process in unconsolidated porous media seems not to have been recognized when considering how such overmoderated configurations might form and be triggered. Such includes typical media where the uranium would concentrate if disposed at the Yucca Mountain site. The potentially capable concentrating and triggering process is deposition of the sufficiently enriched uranium in near surface unconsolidated sediments that subsequently undergo consolidation. High pore-water content, typical of dominantly fine-grained sediments when initially deposited, overmoderates the uranium as it is deposited, but later, when the deposits are buried and begin to consolidate by pore-water losses, two processes are occurring which could lead to perfectly moderated supercritical configurations: the pore water is being reduced per unit volume, while deposited uranium concentration is increasing per unit volume (due to the vertical shortening). These two processes that work together are slow acting, but also could be uniform throughout the configuration, so the question comes down to whether or not such configurations would trigger the positive feedback reactions, or still need some instantaneous forcing trigger, such as a seismic induced load (which tends to compress and dilate the granular fraction of the porous media). The fact that such a common geologic process may have the potential to lead to perfectly moderated, potentially supercritical configurations has not been recognized suggests that more attention is warranted.

Ninety-nine percent of focus has been on 10,000-year containment issues. Now that release and transport are under serious discussion, fate and hazards of radionuclides based on site and design characteristics should be possible to seriously address. Very few of us over the past two decades have stepped back from the regulatory objectives to ask the question: what are the fates of the radionuclide inventories and is there safe permanent disposal of HLW waste at the site?

Saturated Zone Modeling Analyses

The saturated zone models yield the key inputs that determine 1) downgradient flow paths and fluxes, which in turn determine travel times and pathways for radionuclides transported as solutes and colloids, which in turn impact dose calculations due to dispersion, sorption, precipitation---all related to the pathway media. Such modeling warrants not only good, comprehensive field databases throughout the potential flow domains, but also the use of codes that reasonably characterize the conceptual model constrained by the site specific databases. We may be a long way from the ideal based on two saturated zone model presentations. I have reservations because I'm familiar with some of the databases available in the local repository area and the general nature of regional databases. Based on the model presentations, we seem to know less about the repository area and much more about regional hydrology than databases tell us. Some questions posed by the Board members suggest I'm not alone.

Briefly, the USGS regional model is necessarily predicated on a host of assumptions poorly constrained by regionally distributed databases. Many are of the nature that helps determine flux, patterns of flow, etc in any given subregional area---including the subregion of Yucca Mountain. The earlier subregional/regional modeling efforts by Waddel and Zarnecki are likely as useful (at least just as valid). However, the presentation that got my attention is the CNWRA modeling analyses, and comparisons, because they demonstrate how both code selection and conceptual models influence results (and may not meet the full spectrum databases). First and foremost, the conceptional models adopted don't necessarily fit the databases, as "steps" in east-west fluid potential data may (based on hydrogeochemistry of N-S oriented zones of distinctive water types) indicate important barriers to E-W flow. Further, the northern N-S fluid potential step or discontinuity may or may not be caused by a linear change in lithology-induced transmissivity, but it's not as critical an issue (other than to suggest the site hydrology remains uncertain). The apparent disconnect between fluid potential data and hydrogeochemical data suggests continuing degrees of uncertainty with respect to site characterization of the saturated zone, *where there exists the technology and strategies to be very confident of saturated zone hydrology*. Perhaps not enough was done to resolve, with confidence, the meaning of available fluid potential data. I might point out; the water chemistry tells considerable about where it has come from, and with the available sampling density, where it is going. Fluid potential point measurements tell us where the water *has the potential energy to go to* and *might have come from*, but it does not confirm either. These databases combined, ideally in 3-D in such terrain, will tell a confident story.

If the varying flow model results (Viewgraph: Effects of Hydrogeologic Interpretation on Modeled Flow Paths) are considered in the context of the presentation demonstrating hydrogeochemistry groups, we should see E-W, or SE patterns in the water chemistry groups. But we don't see much indication, except perhaps in the NE quadrant. I believe the water chemistry in such terrain because it confirms where water came from and where it is going. The modeling codes are responding to fluid potential and assumptions about the porous media between fluid potential data points---flow down the apparent hydraulic gradients. But if we leak radionuclides into the saturated zone, the existing groups of distinctive water chemistry seem to tell us where the pathways will be ---basically southward in compartmentalized flow zones. Everyone has used a conceptual model and flow code that relies on fluid potential gradients, but the apparent direction favored by all the modeling results may be spurious if there is little or no hydraulic continuity in this direction in the repository area —and the chemistry of the water seems to be telling us this rather important message.

The message is important because of the modeling objectives. If the majority of pathways from the repository to the accessible environment are mostly south to the accessible environment near US 95, it would also mean flows restricted to fractured rocks and probably highly transmissive pathways associated with N-S faulting, which in turn change the travel times, sorption, dispersions, retardation, even fluxes. The important point is that all the current modeling (NRC and DOE) may not correctly characterize the site or provide appropriate input data to forecast pathways and releases. Confident field databases, constrained conceptual models, and then appropriate codes for the conceptual model, will inspire confidence in results. Frankly, at the scales of data presentation, it appears a small portion of the repository might leak out to the SE, but the majority would leak out to the South.

Vadose Zone

These saturated zone databases and modeling efforts send warning signals about vadose zone modeling at this site. We have about 150 years of experience in studies of saturated zone flow systems, but extremely limited experience in thick, complex, vadose zones. The applied science is available to understand and document flow in saturated porous medias, including dual porosity (fractured rock) terrains (even if it hasn't been fully employed at the site). This program has had difficulty with the saturated zone, where the supporting applied science is relatively mature. What about the vadose zone where the program was forced to develop the applied science beginning in 1982? From my direct observations during the first 17 years of technical oversight of the vadose zone program, much of that period was spent casting around with all sorts of lab and modeling studies, and more limited trial and error data collection strategies that accomplished minimal results in terms of databases that will confidently allow forecast modeling of heat and transport *at repository scale*. Experience in saturated fractured rock terrains demonstrates that the most reliable, and useful databases are those that capture the characteristics or behavior at the required scale of analyses because of the strong influence on the hydrology imparted by some fractured networks, and some faults.

Repository scale is the problem, because *nearly all databases* are at highly localized scales, except surface and subsurface geologic mapping. Therefore, even if the conceptual models of processes have been improved to recognize the relative importance of matrix and fracture flow interactions, and even if flux estimates are distributed appropriately and accurately for varied hydrologic states, there are no databases that characterize at the larger or repository scale, fractures and faults which control the transport processes, or could, to a large degree. "Synthetic" modeling also cannot be "calibrated" with site scale values for heat, vapor phase or liquid phase fluxes. The roles of faults and fracture networks with and without well developed pneumatic and hydraulic continuity will determine transport, but the developed model realizations cannot be bounded by "expert " opinion of an untested environment. The local saturation zone data and general experience in fractured and faulted terrains illustrate the basic problem. This is an environment where only "what if" modeling can be accomplished, regardless of the efforts that have been made by a very accomplished model development team. It's an arm wave.

There are one or two studies that might have reduced uncertainty for vadose zone modeling during the proposed thermal cycle. DOE (and NWTRB) didn't support the opportunity, however, when the ESF was first rushed into construction before a database of delayed pneumatic responses to atmosphere pressure changes was established. The opportunity to do so was apparent in two newly established vadose zone monitoring wells below the PTn. The technical oversight group (Nevada, Nye Co., Inyo Co.) concept was to determine the repository scale degrees of pneumatic confinement below the PTn in the host rock horizon throughout the repository area. This type of database seemed to be extremely useful, but ESF construction was continued, penetrated the PTn, and eliminated the apparent important degree of confinement before widespread monitoring or the best atmospheric (frontal system) forcing occurred. Another study urged was thermal imaging of the entire repository area---which would have allowed for locating the surficial areas where warm soil gas naturally discharged. Both surface and airborne scoping studies were conducted by the State of Nevada, and numerous thermal anomalies were noted during atmospheric pressure drops associated with passing frontal systems. DOE opposed this even though a panel of independent experts favored the study. The State demonstrated it was a process that was real, and technologically feasible to document, but didn't have the funds to do it in a comprehensive manner.

Thus, the site was never characterized at the repository scales for vapor phase (water and heat) transport for evaluating the repository scale responses to the proposed thermal cycle, which in turn is fundamental for determining waste emplacement tunnel environments throughout the thermal cycle, and therefore a key input for engineered barrier performance. Pneumatic properties were instead determined at borehole injection scales, useful, but not confidant databases in the highly fractured and faulted terrain for identifying bounding conditions at the repository scale of the thermal load. A good idea of where steam (and heat) will be advected to (and where condensation will occur) is fundamental at the repository scale. Two key boundaries could have been studied at the repository scale---at the land surface and the PTn.

In the very thick vadose zone, no zone, a few zones, or many zones of well developed hydraulic or pneumatic continuity extending through the PTn, and/or to the saturated zone, near or within the repository area, have the potential to totally change performance characteristics during the thermal loading cycle and pluvial climate fluxes. Both vertical and lateral hydraulic and pneumatic continuity must be known for the host rock in contact with tunnels and *this knowledge does not exist at the scale of performance* of the repository. I can not get very excited about the DOE and NRC favored TSPA regulatory test because its not just a matter of high degrees of uncertainly, there is not the minimal knowledge at the appropriate scale to establish meaningful analyses based on what is proposed.

I recall Pat Dominico's (deceased former NWTRB member) somewhat reactionary comment during a Board hearing addressing the above-boiling thermal loading design, something to the effect "----you want steam to come out the top of the mountain?" The problem is, at the present state of knowledge, Pat's conceptual model might be right, or might be wrong. If wrong, all steam generated (and back of the envelope calculations of combinations of matrix and accumulating recharge suggest a very large volume of equivalent water over the life of a 2,000 year boiling envelope) would necessarily condense to liquid phase within the mountain---forming extensive perched zones, some above the repository. If right, less water overall would likely find its way to waste emplacement tunnels.

The Board should focus on the site-specific databases that would be required for total systems performance assessment modeling *at repository scale*. The waste package performances can only be projected if the hydrothermal conditions of the tunnels---throughout the repository and thermal cycle---are anticipated and if the performance of the metal is known for those environmental conditions. The evolving dynamics depend much on advected heat, liquid and vapor phases, and water supply throughout the thermal cycle. My impression is that the site may be too complex to predict performance for the most critical system---the engineered barriers—even if characterization had concentrated on site scale database development. Currently there seems to be no reliable starting point to constrain where vapor phase moisture will become liquid phase---saturated (perched) zones, for example. Nobody seems to realize that perched water will go somewhere during the slow heat up and cool down---even to waste emplacement tunnels.

Site Complexity, Characterizations, TSPA Modeling

There is paucity of appropriately distributed hydrogeologic databases of the nature, and quality, needed that would confidently characterize flow patterns, media properties, and boundary conditions resulting from the highly fractured and faulted terrain from the repository block area to the accessible environment. Considering the duration and the cost of the site characterization program, there can be only two explanations: either the site is too complex for confident characterization, or it is too complex for a DOE managed program to characterize. At least for the saturated zone hydrology, we know the latter is true. Roughly 80% of the downgradient flow domain remains unknown from the perspective of confident transport modeling, and uncertainty continues in the repository block area as to flow patterns and media properties. The vadose zone characterizing problems have been discussed. What this means in TSPA modeling is that adopted conceptual models, necessary for arriving at transport analyses, remain highly unconstrained, and media properties and boundary conditions are largely assumed.

In the presentations dealing with the site and adjacent areas, the figures often indicated (at the scale of presentation) relatively dense and seemingly adequate database control. It's worth a careful look at the nature and density of these databases from the perspective of analytical requirements and the nature of the terrain involved. The site and the majority of the downgradient flowfield domains remain poorly, to totally, uncharacterized for TSPA modeling requirements.

References

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