ACNW0019

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TO: Bill Hinze

FROM: Darrell Leap

SUBJ: Reflections upon the December 14, 1993 meeting of ACNW, Las Vegas.

Pursuant to your request, I have outlined below my perceptions and conclusions concerning the presentations and the philosophy behind them, as I heard them at the Decmber 14 meeting. I will discuss them in the order in which they were presented.

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**(1) 8:15 - 8:30: Ernie Hardin, (UA) -- Overview of Apache Leap Research Program.**

First of all, I was duly impressed by Mr. Hardin. He obviously is a very bright and capable scientist, and has not yet received his Ph.D.. The results he presented were most interesting and certainly well-researched. However, I believe that the work in the Apache Leap site is directly applicable to Yucca Mountain in only two cases.

**In the first case,** the hydraulic theory developed at the Apache Leap site regarding the iteraction of fracture flow and matrix flow and the hydraulic equilibrium between them is applicable to Yucca Mountain. Essentially the same ideas prevail at Yucca Mountain -- the investigators have found that hydraulic equilibrium does not necessarily always exist between fractures and matrix, thus nullifying the possibility that flow is uniformly distributed in the entire system.

**In the second case,** mineralogically, the Apache Leap and Yucca Mountain sites seem to have some similarities, and chemical activity should thus be similar. In is unclear why the nitrate levels are so high at Apache Leap (20 ppm); Hardin did not know either. I wonder if it could be due to the redisdue from nitrate explosives used in mining.

They put a lot of emphasis on Carbon-14 dating of water, but have not yet looked at tritium, which they should do. I would have considered tritium before $^{14}C$, because it can yield more realistic recent travel times. Dale Moeller asked a good question about breathing of gasses in and out of the mountain for which Hardin had no good answer.



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Aside from these two cases, the similarity between the two sites essentially ends. Yucca Mountain is a highly stratified system with many faults and fractures and Apache Leap is not. Thus the hydrology of Yucca Mountain is much more complex, as is the hydrogeochemistry.

Because of the conceptually complex interactions of water flow systems, gas flow systems, ion exchange, potential effects of climate change, and possibly other undiscovered factors, I believe that Apache Leap can offer little additional insight to the problems of Yucca Mountain characterization. Therefore, I believe that the NRC should consider emphasizing less Apache Leap research and more Yucca Mountain investigations.

**(2) 9:15 - 9:30: Joe DLugosz, (DOE) -- Opening Remarks and Introductions.**

There was little in the way of technical material in this talk. However, the outline of concerns was good. I was pleased that the DOE is considering investigating the potential effects of future climate changes. Another important point is that DOE admits that it may have underestimated the effects of ventilation throughout the site. An especially important point he brought out was the need to study the interrelationships between precipitation, infiltration, percolation and recharge.

The milestone chart seems to adequately take into account the immediate unsaturated-zone issues to be studied, but I do not know how realistic the goals really are.

**(3) 9:30 -9:45: April Gill, (DOE) -- Regulatory Issues Being Addresed by DOE/YMPO Unsaturated Zone Studies.**

This was a good presentation of both favorable and unfavorable conditions prevailing at the Yucca Mountain site, as presently set forth in EPA regulations and the CFR.

It is apparent the DOE believes that the unsaturated zone will prevent flow to the accessible environment for well more than 1,000 years, under present climatic conditions.

There were several items shown that supported the site's desirability in view of CFR requirements; i.e., low moisture flux, free drainage of host rock, low-permeability rock above the host rock, and precipitation which is small percentage of potential evapotranspiration.

The most pressing potentially adverse conditions, in my opinion; are (1) potential for climatic changes to adversely affect ground water flow, including water table rise to the repository level; (2) perched water; and (3) gaseous movement of radionuclides. These conclusions are predicated upon the following information:

(1) Climatological models predict a possible 10-fold increase in precipitation in the Southwest in the next 100 - 150 years if the atmospheric carbon dioxide levels continue to increase, and if nothing is done to abate its discharge into the atmosphere. There is a great deal of controversy about this prediction and some do not believe that precipitation will increase by this amount.

Regardless of the real scenario, which no one has a grasp of yet, there is a real possibility that preipitation will increase, and perhaps by a substantial amount

If indeed this increase does come about, there will undoubdtedly be a change in the flow dynamics between fractures and matrix. Local zones of saturation will likely appear and fracture flow will likely be increased over that of matrix flow as the matrix becomes saturated near the fractures.

Potential evapotranspiration will likely be reduced with increasing air humidity; and recharge to the unsaturated zone, and ultimately the saturated zone, will be greatly increased. Storativity of the presently unsaturated materials is likely to be reduced as the degree of saturation increases. The water table is likely to rise over many years--just how many years it will take to rise is unknown.

(2) Perched water may already exist within the repository block. According to Montazer's and Wilson's conceptual model, perched water may exist at the contact between a fairly impermeable layer and a fault, assuming the fault has a low permeability due to fault gouge or juxtaposition of low permeability beds. If this is true at present, and I'm not sure that I heard that anyone has proved or disproved this point, then an increase of precipitation of 10-fold will certainly exacerbate this phenomenon. The potential presence of perched water is troublesome for repository construction and safe operation.

(3) Gaseous movement (if it exists) is a problem that should be investigated. I am happy that the USGS (i.e., Rosseau, et al.) is planning an investigation of this potential problem. It would seem that this is a possible transport mode that would be less likely to occur in conditions of greater saturation, where pores has less gas in them. At this time, it seems that no one can actually quantify this process. Tracer gas analysis would certainly help in this investigation--chlorinated fluorocarbons, tritium, carbon-14, etc.

The remaining concerns of the DOE for UZ hydrology, including Potential for changes in hydrologic conditions that would affect radionuclide migration --10 CFR 60.122 (c)(5); Groundwater conditions in the host rock that could increase the solubility or chemical reactivity of the EBS -- 10 CFR 60.122 (c)(7); Geochemical processes that would reduce sorption -- 10 CFR (c)(8); Groundwater conditions that are not reducing (i.e., oxidizing) -- 10 CFR 60.122 (c)(9); and Rock or groundwater conditions that would require complex engineering measures (10 CFR 60.122 (c( (20); would be most likely caused by changes in recharge rates which in turn would be the direct consequences of climatic changes.

As a final note on this presentation, it seems to me that from the organizational charts presented, the UZ program is vastly overmanaged. This is not an observation that I can quantify, but I know from experience how much of my time and effort was spent in "jumping through the hoops" and QA/QC efforts. I still do not know how DOE can expect to properly oversee all the activities in this program, or how the scientists and engineers involved can spend enough time in the investigations.

**(4) 9:45 - 10:15 M. Chornack (USGS) -- Overview of DOE/YMPO Studies of the Unsaturated Zone.**

This was an excellent presentation. The USGS field and laboratory scientists know what it is all about when it comes to hydrogeology of this area. It was quite a contrast from the DOE "supermanagement" presentations. Mike impressed me as a very capable scientist who has his act together. Unfortunately, he like so many other USGS personnel in the trenches is hampered by the glacial pace of the USGS upper-management decision-making process. There has always been the friction between USGS and DOE which provides the funds.

The main problem has always been threefold: In the first place, the USGS chafes under the restrictions DOE places on it for data release and dissemination, as well as QA/QC requirements. Traditionally, the USGS did things its own way and relied on its own internal peer review process for QA/QC, but in this program it has to take a second seat to DOE.

I asked Mike the question, "If there is ever a question of disagreement between the USGS and DOE about the interpretation of data or results, who's argument prevails (DOE or USGS)?" He deferred the question to Larry Hayes who answered that in the end, he (Hayes) would have the last say and could release or withold data or results at his discretion. He also mentioned that USGS could address independently the concerns of the state of Nevada. This statement seems to be contradictory to a later statement that the State of Nevada could not get data as easily as a project participant.

I don't know if this position has ever been put to the test, but it could cause some ill will between the two agencies if it ever came to a head. I know for a fact that data taken and analyzed by USGS for the Yucca Mountain project has to be approved by DOE for release in papers or public reports.

The second problem has to do with the very slow process of getting USGS analyses, results and interpretations of data to the DOE and cooperating laboratories and cooperating contractors. Usually, the other cooperators can produce results weeks or months before the USGS investigators can, simply due to the rigid USGS way of operating. This was brought out in questions to Mike and Larry Hayes. Although Hayes admits this is a problem and that something is being done about it, it still exists, and it is not likely to be totally resolved soon, although a data center has been set up in Las Vegas, and that interpretative papers are being sent out as analysis papers. The major problems or bottlenecks are in Reston and Denver, not in NTS USGS offices.

USGS upper management should be told in no uncertain terms to get its act together and start cooperating better, and start expediting in a more efficient manner its data processing and release, especially if it wants continued funding from DOE.

I can remember when the Water Resources Division of the USGS in Denver was approached by AEC and then ERDA to get involved in the early repository investigations at NTS, the hydrology group in Denver, working on the test site, was told by USGS regional management to stay out of it. Later, pressure in Washington changed their tune.

The third problem is persistent lack of adequate funding. This has been a problem since the USGS first got involved, and I have never been able to figure out why this is the case. The national laboratories always seemed to have adequate fund to do a first-class job and to have the best equipment. USGS always seemed to be a poor country cousin. Larry Hayes also lamented that this is presently the case; he could drill more holes by laying off scientists.

I would think the DOE could lay off several layers of managers and put more funding into scientific research.

As Chornack described the planned studies of the UZ hydrology of the site, he was careful to cover everything possible (short of climatic change studies) that could be studied in the way of water movement. The USGS field investigation program (as described in his handout and presentation) appears to be well thought out and staffed by capable scientists and engineers, although they could use more.

I was particularly pleased that he also discussed a horizontal borehole across Solitario Canyon Fault, even though a drift is also planned across the fault. The borehole can be instrumented for changes in properties and will have less disturbing influence on the fault than a drift. I am happy to see the USGS concentrating its effort on real field-acquisition of data, rather than concentrating all their effortson modeling.

**(5) 10:30 - 11:30: A. Flint (USGS) -- DOE/YMPO Characterization of Unsaturated Zone Infiltration.**

Alan Flint was most impressive. He obviously knows whereof he speaks about unsaturated flow. The outline of planned activities for studying infiltration was very good, and are what is needed. I was especially impressed with information already gathered, particularly the information about the high storage capacity of the alluvium. Also, the fact that the alluvium and the carbonate layer beneath its surface can serve to hold water near the surface is an important piece of information regarding infiltration.

It is particulary noteworthy that if fractures exist beneath alluvium, water may not even reach the fractures. The fact that their modeled predictions of water content and the measured values were very close lends credence to their theories. I am happy to know that they plan to study 40-Mile Wash, because the underlying fault could be a major pathway of escape of water from Yuccal Mountain.

Another important point that Flint mentioned is that faults have low permeabilities due to calcite cementation in the faults. It would be most worthwhile to know if this condition persists in all faults, and if it can predicted. He also stated that fault permeability is not independent of water retention curve, i.e., moisture content also affects permeability.

He was careful to point out that steady-state models do not yield field-measured values of flux, but episodal events of wet times versus exfiltration must be taken in account to obtain proper results that match field-measured values.
One troubling statement he made was that performance assesment models are not taking into account real data and characterization of the site; the three-dimensional makeup of the site and known properties of the site are not being considered. This is a point that must be worked out with the DOE, USGS, and the investigators involved.

Another important statement Flint made was that the hydrogeological properties of the site are **deterministic, not stochastic.** He also suggested that this may be why USGS models work-they use real, deterministic data. This is a very enlightening statement. I have always been leery of stochastic gurus who believe that geological properties are random and that geological processes are essentially ignored. I am glad that Flint brought out this point.

The deterministic properties of the site are easy to believe when one considers the depositional history of the site; beds of tuff were laid down from fallout from the Timber Mountain Caldera so that the coarser materals were laid down nearest the source and the finer particles farther away from the source. Thus, aside from subsequent faults and fractures, we have deterministic properties within the individual beds.

The planned activities as outlined by Flint are very good.

When Paul Davis asked Flint when will he know if he has enough data, Flint said "I don't know". This is a problem for everyone--how much data will be required? Perhaps this should be a major point to discuss with the DOE. I discuss this later at the end of this report.

**(6) 11:30 - 12:00: E. Kwicklis. (USGS) --DOE/YMPO Site Scale Unsaturated Zone Modeling.**

I am not sure why Kwicklis wants to model the site scale using stochastic approaches after Flint mentioned that the properties are deterministic. His presentation brought out some important and needed investigations that are being conducted in the subsurface where the USGS and most others agree investigations should be concentrated.

I have no criticism of this work. The information acquired so far seems good and enlightening. It is especially noteworthy that results so far indicate that

(a) van Genuchten equation may be questionable for Yucca Mountain tuffs--predicted and masured hydraulic conductivities don't jive;
(b) there is imperfect correlation between several hydrogeological properties;
(c) tritium analyses hint at infiltration within the last 21 years.

This research should be strongly supported. It is crucial to understanding the flow inside the mountain.

**(7) 12:00 -12:30: J. Rousseau, (USGS) -- DOE/YMPO Surface-Based Data Collection Studies on Unsaturated Zone Percolation.**

Important areas to stress are elucidating the water in fractures in the Prow Pass unit--this is troublesome. Also, the fracture density in the Topoah Spring member is much greater than formerly anticipated. The problem of percolation addressed in this talk is very important and should be further researched, but it will require more holes than they previously have drilled. I assume additional holes will be drilled for this purpose.

The question as to whether upward flow from the saturated Prow Pass member is actually a real phenomenon has to be answered. This could really throw a monkey wrench into hydrologic interpretations.

Rosseau's experimental apparatus which he demostrated on the field trip is most impressive. I believe he is on the right track, and I have no criticism of his approach.

**(8) A. Yang, (USGS) -- DOE/YMPO Hydrochemical Characterization of the Unsaturated Zone.**

This was another presentation of very good science. The use of more thtn one isotope to elucidate the possible movement of water is a good move.

I learned something important that we should try in our own tritium lab in the Earth and Atmospheric Sciences Department--extracting of pore water by high pressure squeezing rather than by toluene extraction as we now do it.

Yang's research opens up more questions which must be resolved before the site can be declared satisfactory for waste emplacement:

(A) Does gaseous diffusion, as suggested by rapid transport of $CO_2$ play an important role today, especially in the Topopah Spring member; and is it potentially as important as gaseous convection as a transport mode?

(B) Why do tritium and Cl-36 show up in UZ-4,UZ-5, and UZ-16; and why does high tritium occur in the Calico Hills water when the chemistry indicates the presence of paleowater? Could the tritium be due to gas convection? This is very troublesome.

Data on Carbon-14 which had not yet been received back from the lab at the time of this meeting may help to shed some light on these questions.

I asked Yang the question, "If the tritium can't get in vertically (as Yang maintains), where is it coming from?" He had no explanation.

I asked another question, "If vertical percolation is ruled out, how can we account for the presence of deep old water (above the present water table); did it get there via different flow paths during wetter pluvial times?" Yang said no. He said that today heavy rainstorms run off into canyons.

Alan Flint then suggested that the deep old water may be related to flow into the site from Pahute Mesa.

I am not sure that I totally agree with the above answers, but then, I am not as close to the problem as they are. It is interesting to note that water samples taken several years ago at a depth of 13,000 feet below Pahute Mesa showed significant dissolved oxygen, indicating rapid recharge from the surface through vertical fractures. In the early 1970's Clebsch et al. found high tritium contents in water seeping into a tunnel beneath either Pahute or Ranier Mesa, indicated a depth of percolation of at least a hundred feet in seven years, as I recall--I can find these references if you want them. So, I do not believe that vertical percolation can be totally ruled out.

My subsequent question is this: Does possibly old water in the Calico Hills unit indicate that the water table was once in the unit and recharge from Pahute Mesa came in through a saturated zone, or are there other flow paths above the water table that have in past recharged the Calico Hills from Pahute Mesa, and perhaps from elsewhere?

On the other hand, although Flint and Yang discount modern vertical recharge from the surface to the Calico Hills and other units through faults and fractures (due to carbonate cementation), it does not necessarily stand to reason that the same conditions existed during the pluvial time when there was much more precipitation and thus, probably more $CO_2$ getting into the faults and fractures which would tend to keep the pathways more open than today.

This might seem like a moot point today, but if precipitation does increase significantly, then we can expect that more $CO_2$ will get into the system and perhaps dissolve out some of the carbonate. Again, as I suggested in the round-table discussion, if increased precipitation continues for several hundred or a few thousand years, then we might see soil zones develop on the surface which would produce $CO_2$ from decaying vegetation, which in turn might get into the system.

This is scientific conjecture at the present time, but it should be considered along with climatic-change investigations.

It is apparent that with all the uncertainties and conflicting isotopic data taken thus far, and the presence of water in the system, that this project should also be accelerated and supported. Answers to these questions must be found before performance assessment studies can truly gain any credence.

**(9) 2:00 - 2:30: M. Chornack, (USGS) -- Exploratory Studies Facility Interface - Construction Phase Activities - Main Test Level Activities.**

I have no comment on these plans other than it seems to me that they have been carefully thought out, and are in place to take maximum advantage of the opportunity of the tunnel. They all seem reasonable to me.

**(10) 2:30 - 3:00: B. Bodvarsson, (LBL) -- Three - Dimensional Model of Unsaturated Zone Flow.**

This is a most ambitious project. I don't know if it is oversold or if it will accomplish everything that it is purported to do. Like so many models, it will be only as good as the data put into it. I have no problem with the technical expertise behind the modeling effort or the plans for the modeling effort itself, or the theory behind it.

The model takes into account about everything one can think of in the way of processes going on at Yucca Mountain -- gas flow, geothermal gradient, water flow, the ESF, etc.

However, I do seriously question the statements that it is to be used to (a) "Predict conditions at new boreholes"; and (b) "Guide in the site-characterization process". I do believe that it can help to "Integrate the available data", and in "Sensitivity studies".

My rationale for making these statements is as follows:

(a) I do not understand how the model can predict the conditions at individual boreholes in light of the new and unexpected anomalies that are constantly being found (e.g., tritium, unexpected water, gas circulation and diffusion, etc). It might help to predict such things as water table elevations in the saturated zone, but you don't need a model to do that. In addition, I do not think, in light of the data presented, that all the flow pathways have been truly delineated thus far. This is crucial to making a model work in a believable fashion. Bodvarsson did state that the character of fault flow may have to be changed in the model as more data are acquired about this subject.

(b) I simply do not know how the modeling effort can "guide the site-characterization process", unless it can be used for sensitivity studies to show how sensitive the model is to additional data: it probably can be used to advantage in that sense, and for that reason, should be used. But, the quality of the data going into the model is most crucial to making sense out of such studies.

I remember a meeting I attended ten years ago in which a consultant gave a talk in which he said that modeling could "reduce the number of observation wells needed in a project." Such a philosphy did not make sense then and still does not make sense. Remember, GIGO = Garbage In, Garbage Out!

From what I have read and gathered from my modeling colleagues, integration of available data of a particular site, and predicting response of the well-characterized site (assuming parameters don't change over time) to particular stimuli are much more realistic goals of modeling than extrapolation of the same parameters to the unkown. This point is especially pertinent to Yucca Mountain where climatic changes may cause as yet unknown changes in hydrologic parameters and flow pathways.

Recent papers by Bredehoeft and Konikow call into serious question the efficacy of predictive modeling unless the flow system is understood in the most minute detail. In light of the experience of modelers over the last 20 years, and in light of the continuing discovery of unexpected parameter values at Yucca Mountain, I would seriously question the validity of extrapolated modeling results in the unsaturated zone at Yucca Mountain.

Finally, the most critical question pertaining to the modeling effort is this: Will the DOE believe the modeling results in lieu of enough real data and use them to prove its point? With all the PR the DOE is assembling to make its case, I fear that the model could become a "source of truth" in and of itself!

**(11) 3:00 - 3:30 C. Newwberry, (YMPO) -- Integration of Unsaturated Zone Data Collection, Modeling Studies, and Performance Assessment.**

My major comments about this talk are; (A) the speaker did not know how to answer many of the questions put to her -- I got the feeling that she was being used as the "sacrificial lamb" by her superiors; (B) the whole infrastructure and logical flow of information and decision making, as oulined in her overheads was horribly complex and overmanaged.

The whole effort of the DOE in this aspect is to rely on models and model interaction at various scales to direct the entire site characterization process. This is not only too restrictive in light of the frequent new and unexpected discoveries that change the conceptual model of the area, but it is far too restrictive for the scientists that are obtaining "real data". I think it is obvious from all the real data gathered, and from the contiuing changes in the conceptual model of the site, that the flow chart of "Performance Assesment Model Integration", as Newberry showed it, is nothing short of an exercise in overmanagement.

In the final analysis, all the management and performance schemes must depend on the real field and laboratory data acquired, and the proper hydrogeologic theory to put it all together. Overmanagement of such research by DOE only slows the process. In addition, such complicated management schemes become ends in themselves, rather than means to an end.

I still do not believe, in spite of what I heard, that such management can produce the kind of communication between all parties that is needed in a project of this scope and magnitude, and I am speaking largely from my own experience in a time when the management infrastructure and QA/QC demands were considerably less complex than they are today. In short, this talk was so much DOE management "arm waving", in my opinion.

**(12) 3:45 - 4:00 Joe DLugosz, (DOE) -- Review of DOE/YMPO Response to ACNW Concerns. -- deferred to Round Table Discussion.**

**(13) 4:00 - 4:30 Linda Lehman, (Linda Lehman and Associates) -- Alternative Conceptual Models of Unsaturated Flow at Yucca Mountain**

Linda Lehman represents the State of Nevada. Her presentation, although it did bring out an interesting concept, namely "focused recharge", did not add much to the overall level of knowledge at the meeting.

Her main thesis was that alternative conceptual models might be realistic. It seems to me that the work by the USGS is capable of determining, in much more scientific manner than her approach, if alternative conceptual models are realistic are not.

It also appears that she does not have the scientific or technical backup that even comes close to that of the USGS. From what I heard from the USGS personnel, they are also looking for alternative interpretations of the hydrogeology of the site.

I don't think that I agree with her statement that "Choice of an alternative conceptual model has a large impact on certain parameters." I believe that the information presented at the meeting indicates that the parameters drive the conceptual models, and that conceptual models in turn are modified according to new data about hydrogeological parameters (e.g., Montazer's and Wilson's conceptual model is being modified in light of new data coming in).

She admitted that data is not yet sufficient to fully elucidate "focused recharge" areas, and that more work needs to be done on the west side of the mountain. She also thinks that work so far is biased toward matrix flow. From what I have been reading, it seems to me that the USGS is coming around to a closer look at fracture flow.

Her insistence in "fair treatment" of alternative conceptual models, or "analysis of bias " are moot points in light of field data. Ths is why much more emphasis must be put on subsurface investigations in order to reduce the number of possible "alternative conceptual models" that could be derived with inadequate data, in spite of all the models in the world: shades of Chamberlin's "Multiple Working Hypotheses"!

However, in this case, we want only **ONE conceptual model-- the Right One!** There is too much at stake to choose the wrong one.

It might be conceivable to work Linda in with some of the investigations to eliminate any feeling of "bias" on the part of USGS or DOE to her ideas, or to the concerns of the State of Nevada.

**(14) 4:30 - 5:00 Marty Mifflin, (Mifflin and Associates) -- Fracture and Matrix Flow in the Unsaturated Zone at Yucca Mountain.**

Marty's main contribution was the description of scenarios that might come about as the result of changes in the climate withnin the next 10,000 years. This is a noteworthy contribution. It seems to me tht far too little is being done to account for possible problems resulting from increased recharge.

If he is right that recharge in the future could be 10 times that currently, then we can expect the results he outlined -- water table rise, perched water in the Paintbrush tuff and Calicao Hills tuff, and increase in the perennial and ephemeral flow through fractures, vapor phase transport due to steam generated by the waste packages, and possible repository flooding.

These ideas should not be ignored because climatologists are predicting significant increase in precipitation in the area. Hydrogeological studies of both the saturated and the unsaturated zones, and performance assessment studies must take into account the possibility of increased precipitation and recharge.

**(15) 5:00 - 5:30 D. Kreamer, (UNLV) -- ESSE Peer Review Comments on DOE's Hydrology Program.**

I did not get much out of this talk. I do recall him mentioning that permeability of fractures might change with time due to fracture coatings. This, I believe, is a real possibility with increasing recharge and increasing carbon dioxide flux as I mentioned above. But how one predicts this is beyond me.

**(16) 5:30 - 5:45 Wrap-Up/Round Table.**

The most pertinent points in this discussion were

(A) Much more subsurface date is needed, but no one knows yet how much is needed. Perhaps this problem can be solved by sensitivity analyses by computer models, as mentioned earlier.

(B) Data is catalogued and put in a repository in Las Vegas. Supposedly it is available to all investigators through formal requests. Linda Lehman stated that she had been trying for years to get data, but was unsuccessful. The USGS has to have the data approved by the USGS before it can be given to the state and other non-project people--this is contrary to the earlier statement by Larry Hayes.

This sounds like a PR problem. Perhaps, some better way can be worked out to get data to the state and its contractors, and thus, reduce the feeling that the state is being left out of the loop. This can only cause ill will as the situation now exists.

(C) One of the more troubling problems that came up during the round-table discussion was the fact that no one knew when they would have enough data. It would seem that sensitivity analyses by computer could help to bring this issue to a head--use a model to determine how much results change with new data. If the change is within preset tolerance limits, then enough data will have been taken. I believe that this is DOE's position.

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**Final Observations and Generalizations**

(1) When a high-level waste repository at NTS was first being discussed, in the middle to late 70's, an attempt was made to investigate the deep unsaturated alluvium in Yucca Flat, on the eastern side of NTS, were underground nuclear weapons tests were being conducted. The Defense Nuclear Agency simply would not hear of it because they wanted to keep Yucca Flat for future weapons testing purposes.

Interest then shifted to the saturated zone in Jackass Flats, and an argillite was investigated for a possible host rock. It turned out to be too highly metamorphosed, too brittle, and too fractured for a repository.

Finally, the only place left on NTS was Yucca Mountain. Earlier in a meeting in which the proper hydrogeological setting was discussed, Pat Domenico, who was hired by the USGS as a consultant, suggested that it would be best to put the material in the unsaturated zone, rather than the saturated zone, in order to reduce the problems of boiling and transport by water.

Consequently, the unsaturated zone in Yucca Mountain was chosen for investigation, and at that time was in competition with the Basalt Waste Isolation program in eastern Washington, and the bedded salt in western Texas.

Thus, the Yucca Mountain site was chosen as a "third-round draft choice" so to speak, and on the basis of political decisions at that -- it was on goverment land.

It is becoming apparent that Yucca Mountain is as complex if not more so, than the other sites in Washington and Texas which have been removed from consideration. It is unfortunate that at the outset, more emphasis on finding the right geological conditions, regardless of the location, was not put into the project.

I still believe that thick unsaturated alluvium in some remote valley on NTS or environs would be best for a repositiory because

(A) Alluvium is dry and porous, and thus little changes can be expected in host media due to canister heat.

(B) Alluvium, as Alan Flint points out is very sorptive, has a high storativity, and greatly restricts the flow of water through it in the arid climate of southern Nevada.

(C) The water table 1,500 - 2,000 feet deep is low enough that it is unlikely to reach the repository level in time of increased precipitation

(D) Alluvium has a fairly high cation exchange capacity.

(E) The air-filled pores of alluvium makes it a natural insulator against thermal loading.

(F) Alluvium is much more homogeneous with far less discontinuities than the fractured tuffs at Yucca Mountain. As a result, the hydrogeology would be more easily understood, and it would be much more easily modeled.

(G) If a repository could be constructed in alluvium, and a heavy concrete pad built over it with diversionary channels to keep the water away from vertical percolation, then if no water were to get to the waste, it would stay put forever.

(2) Early in the program, when Don Veath was head of operations in Las Vegas, he had a sign on his office wall that stated that the goal of the DOE was to build a repository at Yucca Mountain. DOE is hell-bent to do just that, and seems to be sparing no expense to convince the public that their very complex and unwieldy management structure and computer models will assure the world of the safety and wisdom of doing just that. I believe, from my observations, that DOE wants to believe its models.

From the evidence presented at this meeting, and from what I have been reading about the unsaturated zone at Yucca Mountain, I am convinced that Yucca Mountain is just as complex as the other sites, if not more so, and that the hydrogeology may never fully understood, especially if the climate changes.

Therefore, I have serious reservations about the wisdom of using Yucca Mountain--basically because we still do not fully understand the mechanics of flow and the pathways of flow now and in the future within the site, and certainly cannot make any believable model predictions until we do. I seriously wonder if we ever will.

(3) Basically, the science being done by the USGS in the unsaturated zone is very good.

(4) Performance assessment studies at this time may be premature until more is known about UZ hydrogeology.

(5) Models can be used to advantage in sensitivity studies to indicate if enough data has been obtained, providing the hydrogeological parameters are well-enough known.