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To Lynne Deering

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DARRELL L LEAP, Ph.D.

Registered Professional Hydrogeologist (AIH)
Certified Professional Geologist (IN)

5932 Lookout Drive, West Lafayette, IN
(317) 494-3699 office: (317) 496-1210 fax: (317) 567-2578 home

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Dr. William J. Hinze
US Nuclear Regulatory Commission
ACNW
c/o The Residence Hotel
Washington, DC
FAX: (301) 718-0679

Dear Bill:

The following are my comments and impressions about the field trip and the meeting in Las Vegas on the subject of ground-water travel time, on Oct. 21, 1994:

Stan Davis's Talk

(1) He is right that we cannot "date" ground water per se. This is because the tracer is mixed with water over time an space and diluted such that the amount of tracer (tritium or whatever) in the ground water is not necessarily all the tracer that was in the recharge water. However, it can be used to advantage and with proper consideration of hydraulics, can be used to estimate dates of travel. Nevertheless, the presence of significant tritium in the ground water at depth beneath Yucca Mountain does indicate the recharge of fairly recent water--a most troublesome point.

(2) He also pointed out the potential usefulness of other tracers that could and should be tried, namely Si-32, Ar-39, Kr-81 and Cl-36.

The problems with Cl-36 are predominantly those of determining its origin. Although Stan did not mention it, Cl-36 is often produced by nuclear power plants and carried downwind. It can be produced geologically, for example, by fission of uranium, and the rates of its production must be determined first. This is not an easy task. At Purdue, we have had problems with using Cl-36 because of it high background level, and lack of understanding of where it is coming from.

The best way to use tracers, as Stan pointed out, is to used more than one as checks on the others.

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Although some tracers can be produced at depth, I cannot foresee that happening with tritium, unless the water comes from an area where nuclear tests were conducted, but that is not likely the case at Yucca Mountain.

Although I think that the tritium at depth beneath Yucca Mountain is real and significant, the problem at the present time is ,"Along what path did it enter"?. Travel time can be estimated more accurately if the pathways are known. The tritium indicates that recent water has gotten into the system, but the question is from where. This last question is the most critical of all, because it crucial to determining along what routes water can get into the system, but also how it can get out.

Austin Long's Talk

(1) Austin reiterated much of what Stan said, so I won't repeat it here. Austin did make several pertinent comments about the pitfalls in trying to use C-14, and the mitigating factors to be considered in doing so. One of the most significant is the changing flux of cosmic particles entering the earth's atmosphere due to changing sunspot activities and consequent changing magnetic field intensity.

He also mentioned that atmospheric nuclear testing increases the C-14 levels in the atmosphere. If recharge containing this extra C-14 were to get into the Yucca Mountain system, which it may have already done, it could give a false date, which would likely be younger than it should be.

Bacterial action (probably not a big factor at Yucca Mountain) can also affect the ratio of C-14 to other carbon. We also know this to be true for sulfur isotopes. Generally, bacteria concentrate heavier isotopes, which under the right conditions can produce an apparent carbon-14 age younger than reality.

The important point to remember is that using C-14, as in the case of H-3, can be done to advantage if one can elucidate the flow paths, and can then find relative changes in the concentration levels at various places. The relative changes in concentration can be used as a kind of normalizing feature which will help to negate the effects of non-natural C-14 or H-3. This assumes of course, that tracer spiked water found in different parts of the mountain all came from the same source. Again, this latter point cannot be ascertained unless one knows, or has a pretty good idea about the flow pathways of entry.

Austin did make a good point about the variability of H-3 over time and space on the surface, and pointed out that its variability was even more notable than that of C-14. But it should be pointed out that tritium is much more conservative as a tracer underground than is C-14, because of the interchange of tracer carbon with carbonate minerals.

The USGS has long known, from its continuing study of tritium in precipitation, that more H-3 will rain out or snow out in the Sierras than will be found in water to the east. Heavier isotopes will fall out first. This will also give an apparent date younger than normal if this water gets into the ground water system.

Neil Plummer's Talk

Neil pointed out two very important points that the previous two speakers did not. That is, (1) the importance of knowing the initial value of C-14 in an aquifer before one can use it for dating. This is difficult at if not impossible at Yucca Mountain because of the lack of information about what the C-14 levels were after recharge, whenever that was. An additional mitigating factor is the C-14 levels in the atmosphere, and in the soil zone, during the pluvial period of the last ice age (14,000 yrs bp). Obviously, a soil zone had to have been formed with the added water. Plant and microbial activity must have greatly added to the carbon load in the recharge water.

(2) What are the reactions involving C-14 along the flow pathways? Again, this might not be possible to fully understand because of the difficulty of observing fragile and ephemeral reactions with CO_2 at depth. When one takes a sample, the partial pressure of CO_2 is very important to understanding the reactions taking place (Groundwater, Chap. 7; Freeze and Cherry, 1979). The mere act of drilling into a deep aquifer or fracture may cause CO_2 to escape, thus precipitating $CaCO_3$ out of solution before a water sample can be brought to the surface.

The new USGS model, NETPATH, is purported to include all carbon forms and reactions in the system. If so, then it will be a useful tool in perhaps described and quantifying the various carbon reactions that Could or Might take place as water is recharged to the ground water system in Yucca Mountain. Hoever, no model is any good at all unless one has proper data to put into it.

My major concern here is that it may be impossible to adequately determine the initial concentrations of carbon, especially of C-14 in the system in order to adequately model the chemical reactions. It might be possible, from observations or modern carbon concentrations and assumed reactions, to back out, in an inverse-modeling scheme, the initial concentrations. However, one would have to be somewhat suspect about this number, considering all the aforementioned perturbations to the carbon cycle.

Finally, it is not clear to me that enough background water chemistry data is available or even obtainable to adequately calibrate the model runs.

Neil pointed out the possibility of using chlorinated fluorocarbons(CFC's)as environmental tracers. This is an area that should be stressed. These anthropogenic compounds, good as tracers for the last 40 years, have been used to advantage in several environmental studies (I can get references for you). The ease of analysis (15 minutes) and low cost of analysis, and the sensitivity (1 in 10^{15}, almost as good as tritium which is 1 in 10^{18}), make these substances ideal as tracer under the right conditions. The major advantages to using these as environmental tracers is that they are, as far as we know, unnatural. Thus we know when they started to appear in the atmosphere, and we also know their present concentrations, and have pretty good estimates about their concentration history with time.

They are conservative, and don't seem to react with minerals very much, although there might be some activity in the presence of organics (probably not a big problem at Yucca Mountain).

Don Thorstenson's Talk

I heard Don Thorstenson of the USGS describe results of using CFC's in the unsaturated zone. Evidently, they feel that it can set an upper limit on travel time an order of magnitude shorter than that of C-14. This is obviously because of the half life of C-14 (5000+ yrs).

At least in this case, it has been used to good advantage with C-14 in the unsaturated zone. From the results in the UZ zone, it would seem apparent that CFC's can be used to advantage in saturated ground water environments.

John Stukless's Talk

John mentioned something of great importance and that was that in the beginning of the Yucca Mountain study, holes were not drilled primarily for isotopic studies, and therefore, may not yield the right kind of information necessary. There are questions about contamination from pipe dope, greases, cement and salt upon C-14, Sr and Cl respectively. It appears in some cases that this may be true, but keeping the hole clean during drilling is not a easy task.

He also brought out a point discussed above--namely the mixing of C-14 with dead carbon from calcite in the soil and in the formations which may make the apparent ages too old. The excess dead carbon would give the impression that most of the C-14 has decayed, and this may not be true at all.

Perhaps the most crucial of all his findings were that Sr, C-14, U-series and other methods has pretty well shown that water has discharged 100 m higher in the past than at present. This is of greatest concern in the event of a climatic change which could again raise the water table. This is a point that must not be overlooked.

In addition, another pertinent point he made was that today's travel time may not be the same in the case of a climatic change. This point may be overlooked, but it should be carefully considered.

Al Yang's Talk

What Al revealed, an update on last December's presentation, was scary. He has documented water entering not only unsaturated holes UZ-N2, and UZN-46 during spring snow melt (which can be understood), but also, seepage at a depth of 1,300 feet. In addition, perched water at a depth of over 1,000 feet in holes north of the central block was reported. The most bothersome discoveries were water in UZ-14 at 2,206 ft which rose for 100 days; and tritium at 100 TU and C-14 at 90% modern, in UZ-16 at 1400 ft in the Calico Hills section.

I also wonder if the perched water could be due to matrix recharge from the surface, as mentioned later by Alan Flint. If so, could the surface in that area be sealed with some kind of pavement? It may seem far-fetched, but it should be considered.

Both C-14 and H-3 were high, which is probably a dead ringer for rapid infiltration of recent water--FROM WHERE? There is obviously a pathway or pathways which no one knows, but only suspects as to its extent. This must be dealt with before the Yucca Mountain site is approved (if ever). If this hole has high tritium and C-14 with depth, and others such as UZ-4 has a C-14 age of ±1,000 years as well as a water content of 30% at depth, then obviously, water is getting down there from somewhere, and it is very likely that additional holes will show similar findings.

I believe that Al and his colleagues may be right in their statement that fracture and lateral flows in the bedded and Calico Hills units dominate the hydrologic flow system in Yucca Mountain.

Finally, if water is getting in, then it stands to reason that it is getting out, SOMEWHERE! The question is WHERE? That is the 64-dollar question, and nothing that I have found in the extensive readings about the area and recent research there, or in this recent meeting has really addressed this question. This question should be the paramount question asked, even exceeding in importance the question of which tracer is the best, or how fast can the tunnel be drilled.

Yang and others seem to think that residence time is the most important issue, even superseding travel time in significance, because both their "Piston Flow" model and their "Well-Mixed" model (for tritium) seems to hint at a mean residence time of roughly 32 to 36 years. It should be noted that these calculations were for only one hole, UZ-16, and there is a very great possibility that the results could be different for other holes, especially if there are different flow pathways of different conductivity intercepted by the hole. The results from just one hole are probably not significant alone, although they are probably good for the data available, but data from several holes would have to be analyzed to determine the statistical significance of their conclusion.

The Piston Flow Model (from Yang's handout)

$$C_{out}(t) = C_{in}(t_0 - T)e^{-kT}$$

The Well-Mixed Model (from Yang's handout)

$$dC(t)/dt = Q_{in}C_{in}(t)/V - Q_{out}C(t)/V - kC(t)$$

C_{out} = output concentration
C_{in} = input concentration
T = residence time
k = tritium decay constant ($.693/t_{1/2}$)
$t_{1/2}$ = half life.
Q_{in} =recharge flow rate
Q_{out} = discharge flow rate.

I certainly do not have any problem with their calculations, but I do question whether or not residence time is more important than travel time. This will be especially critical in times of greatly increased precipitation and increased recharge.

The mere fact that the water is in residence (at least in the hole studied) for 30-odd years says that there is an escape pathway from which it leaves the area. The major question then is WHERE and HOW MANY pathways are there, and HOW CONDUCTIVE are they? This is a question I fear is not being adequately addressed, for even if there was a long residence time for water, it has to be leaving the site to maintain an equilibrium between recharge and discharge.

An important issue for risk assessment and effects on the environment is the actual travel time during exiting of the site, and in what direction or directions is it moving. This information seems to be lacking and no amount of sophisticated modeling will answer these questions without some real subsurface starting data and model-calibration data about real pathways.

I must confess that I do not at this point have a suggestion as how to go about finding this information other than intensive and expensive drilling. Perhaps as an exploratory geophysicist, you may have some ideas.

June Fabryka-Martin's Talk

Her conclusion about work she and Alan Flint are doing concerning the infiltration rates determined with Cl-36 are interesting. They maintain that the ≤ 7 mm/yr rate in the rooting zone, and the 0.01 to 0.8 mm/yr rate below the rooting zone are due primarily to the transpiration effects of plants which draw water down from the surface.

I would like to suggest that this might also be due to the fact that in the root zone, the soil is often more permeable than below. This could also be a factor.

Their evidence for fast transport paths includes the discovery that fluxes determined by chloride mass-balance methods exceed maximum possible matrix flow for overlying welded tuffs.

My immediate question is about the possibility of fractures which shunt the water faster than matrix flow. The next question I pose is this; Would it be more feasible to use fracture models for the worst-possible case scenarios of transport and escape, than trying to work with double-porosity models which include both fractures and matrix flow. Either is difficult enough in its own right, but combining the two makes it even more difficult. However, in some cases double porosity models may be necessary due to interchange of water between matrix and fractures.

Finally, the residence time estimates of anywhere from 100 to 700 ka are uncorrected, and the real ages are not certain.

One important piece of work that should be done is to determine the source of inconsistencies between the dates gotten by Cl-36, C-14, and H-3. This is a basic kind of research which should be undertaken in order to improve the accuracy and confidence in the various methods.

I agree with Al Flint that these three isotopes should be analyzed from the same spots and the same samples.

Bo Bodvarsson's Talk

I did not get much more out of this talk than I got at the meeting last December. It seems that in the interval, he has run more models and tweaked the parameters a bit more. I will have to say that the project is ambitious, and I am pleased at his admittance of the following uncertainties:

A. Hydrological characterization of major faults:

Although the intrinsic permeabilities of the Tiva Canyon, Topopah Spring, and Calico Hills members are 1, 5, and 10 microdarcies respectively, they will have trouble adjusting to the presence of faults and fractures that may now be known or that may be discovered in the future.

I asked him if his model could take into consideration the "Wild Card" appearances of faults and fractures yet to be discovered. His answer was "yes". It is not clear to me that this is entirely possible.

B. Relative permeabilities of fractures:

This is one of the most difficult of problems. I do not believe that one can do a decent job of determining permeabilities of fractures unless it is done *in-situ*. This requires testing of boreholes, and constructing them especially for this task.

The equipment exists, so I understand, for drilling very clean holes, and air permeability tests are being run as we saw on the field trip. But it will take a lot of data of this kind to be meaningful for a model.

Another problem I see is the uncertainty about climate-induced changes in fracture permeability that could take place at some later time. I do not believe that they have a good feeling about this question at this time.

C. Relative permeabilities of rock matrix:

This question can be answered much more easily than the previous one. Laboratory testing, like that being done by Al Flint, can help to solve this problem, because many such samples can be tested in the lab. A lot more work should be done in this area until it is felt that the matrix permeability throughout the mountain is sufficiently known. This will take time.

D. Continuities of layers and sublayers:

This is the biggest problem. How well connected and how permeable are the disconnected parts. This goes back to what I discussed above about the crucial nature of knowing more than is known now about the location, number and hydraulic parameters of potential escape pathways before the modeling can be truly effective.

My final comment on this topic is that the model results will be no better than the quality of the data put into it. Although the model seems to be flexible enough to account for the ESF and major faults, and to predict possible sites for new wells, the actual values of parameters such as hydraulic conductivity and ground-water flow pathways, now and under future climatic conditions, are most necessary.

Zell Peterman's Talk

Again, as in a previous talk earlier, the fact that some of the wells were not drilled or later treated in the cleanest matter possible, seems to reduce the accuracy of the data reported in Zell's talk. Foreign water, heavy metals and cement in the well UZ-14 caused obvious difficulties. The talk gave a good description of how Sr in Calcite (as a substitution) can give a picture of the strontium concentration when the calcite was precipitated. It also discussed how both Uranium and Strontium can be used as tracers in ground water. More work should be done with these isotopes, and I was glad to see that both background Sr and U values at the Test Site and vicinity had been obtained over a period of time.

Even though the Sr values in tertiary water are about 35% lower than in calcite and UZ-14 water, and U-234/U-238 vary by seven times, it is unclear to me at this point just how efficient theses isotopes would be as tracers when compared to tritium and other substances. Uranium-234 is preferentially dissolved in oxidizing water, and it seems to me that this is a problem not unlike the non-conservatism of certain other tracers where the actual amount in solution varies along the flow paths.

I believe more work is needed to answer more basic questions before much reliability can be place on these methods alone. Perhaps they can be used in conjunction with other methods. In fact, if at all possible, several reliable methods should be used on the same samples in order to check consistency of results, and to calibrate the various methods with each other.

Roundtable Discussion

(1) Alan Flint gave an ad-hoc presentation about recharge and his statement "that it takes a large precipitation event to push water in the fractures is probably true, at least as far as is known. I believe he knows what he is talking about. He also said that at the north end of the block, the infiltration rate is high due to matrix flow alone, regardless of the fractures, but the volume is very small.

It seems to me that this is still a pressing question. And an additional question, is how much gets into the mountain. Even if little gets in due to surface recharge, there is the big question of subsurface flow into the mountain from other places surrounding the mountain.

Again, the above conclusions may not be true for results of climatic change.

(2) The question about error bars surrounding data points that was raised by Marty was answered well, I think. Ralston Barnhard of Sandia, Al Yang and Zel Peterman of the USGS all stated that their data that was presented were the result of several analyses.

(3) The question of integration of work, data and results among all participants was raised again as in the December meeting. Larry Hayes defended the USGS efforts at integration, as he did then. I still wonder how much pressure the USGS is getting from DOE which provides the funding. I heard rather obliquely that DOE was pressuring USGS and looking down on their publication in journals, ostensibly implying that such activities were a waste of time and only serve to "pad" the USGS investigator's publication records. Another comment I heard was that the contractors are now calling the shots, and that they have little interest in the more scientific approaches.

I do not know how true these statements are, but I am sure there is some basis for them. It seems to me that the USGS is doing the best scientific work of all the investigators, but it is also somewhat hampered by DOE bureaucracy.

An example is the tracer facility using the C holes that we visited on the field trip. Here, the investigators are tooled up, but having to wait for more than a year to get started because a pump was not wired up correctly. This sounds like pure nonsense to me. I can appreciate the frustration of USGS investigators, and others, in having to dance to DOE's tune--I have been there.

Finally, your question, "Is the travel-time investigation doable"?, was not really answered. If it is not "do-able", then no valid statement can be made about the safety of the repository, and I am sure that it will not fly unless that question can be answered.

The Field Trip

(1) The visit to the two faults was quite interesting, and it seems apparent that little water if any is getting into the block via these faults. The big question is whether or not the faults will ever move and what damage that could cause. I fear that I am not qualified to discuss this question, and will have to leave it up to the tectonics and seismic people, as well as the risk assessors.

(2) Just to reiterate, I was very disappointed to see the USGS tracer experiments held on ice because of the wiring of a pump. This seems like a waste of money. I do not know why this is being done, perhaps DOE thinks other work is more important. Traditionally, the DOE has treated the USGS people like errand boys, and given most of the "goodies" to the national labs.

(3) The only other criticism I have is the exploratory tunnel. Perhaps it will reveal fractures and other water-laden pathways into the repository area, but can these features and their chemistry be adequately determined with the mechanical setup they are using? I am not sure about this. This one activity of boring the tunnel seems like an extraordinarily expensive exercise where there is so much more scientific data that should be obtained.

(4) I am worried about the outcome and upshot of the PPA. Will DOE now carry the ball alone on this project. The question must be asked of DOE if they intend to ignore the advice and needs of the other investigators, and the public and go ahead with the project at full speed. I can see real trouble brewing for DOE from complaints from the scientific community, and

especially from the State of Nevada, if indeed, DOE does take the "damn the torpedoes" attitude. Will they get support of Congress to go ahead and develop the repository? This needs to be hashed out.

(5) Finally, my own opinion, for what it is worth, is that Yucca Mountain is not a good place to put high-level radioactive waste. I pointed out my argument in my letter to you after the December meeting. It may turn out that the extra effort and expense involved in determining the travel time and pathways of escape may be too expensive to continue considering the site altogether, especially considering the fact that over $6-billion has already been spent.

If you have any questions, or want additional comments, please call me at work (317) 494-3699, or at home at (317) 567-2578.

Sincerely,

Darrell

Darrell I. Leap

cc: Lynne Deering, ACNW FAX: (301) 415-5589