Impressions of the Yucca Mountain Project and in Particular the Isotopic Investigations as Presented at the ACNW Working Group Meeting on Groundwater Dating Methods October 20-21, 1994

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Comments are in random order.

June Martin is proposing a mixing model to explain young ages found relatively deep[1]. The argument is that a small proportion of modern water (on the order of 1% to 5%) mixed with old water can make water sampled appear relatively young. For this argument to be relevant we must presuppose a flux averaged approach to calculation groundwater travel time. Seems that everyone across the project is doing this. The Sandia PA folks do much the same thing when they present a PDF of travel times derived by calculating a travel time for each particle tracked from a single realization. As explained by Paul Davis, it appears appropriate to flux average travel time to meet the DOE requirement because the DOE regulation links the travel time calculation to demonstrating overall system performance (by integrated discharge or dose). However, the NRC travel time requirement apparently need not be linked directly to system performance. It is viewed as a separate independent line of evidence in evaluating site suitability, and as such it is not clear as to whether flux averaging is appropriate for calculating a groundwater travel time. However, it seems to me that a flux-based GWTT requirement is redundant to the integrated discharge requirement because small fluxes favor compliance and large fluxes make compliance more difficult. If the GWTT requirement is to be viewed as one of several completely independent means to help establish site suitability, then flux averaging is not appropriate. If it is not viewed that way then perhaps flux averaging may be appropriate, but then the GWTT requirement ought to be scrapped because it is redundant to the integrated discharge requirement.

As I understand it, June is proposing this mixing model to explain discrepancies between her chlorine-36 results and Al Yang's carbon-14 results. Al Yang has calculated groundwater ages that are younger than June's. The mixing model calculations show that relatively old water mixed with as little as 1% modern water (presumable supplied from fractures) will give younger apparent ages for both chlorine-36 and carbon-14, but the apparent ages for the carbon-14 will be

[1]It should be noted that a mechanism for how this mixing might occur has not yet been proposed.

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much younger. In other words, the real ages of relatively old matrix water may be much closer to one another than the apparent ages would lead us to believe. However, I'm not too sure that there are any discrepancies that need explaining. Let me propose another equally plausible conceptual model. It was clear from the presentations in the workshop that the system is very heterogeneous and that it is very likely that water flows by way of multiple (unknown and possibly unknowable) pathways. Therefore, it is quite reasonable to presume that June and Al have sampled water migrating through different pathways. All other things being equal such as the fraction of modern water (which we have no way of knowing), Al's migration path just happens to be faster than June's. But let's not get too mired in the academic, both paths are pretty darn fast.

The usefulness of June's work:

1. Use bomb pulse chlorine-36 in tandem with Al flint's soil physics measurements to develop an upper boundary condition (water flux, infiltration) for water flow models of the site. We have seen the bomb pulse and moisture content data that support the hypothesis of deep infiltration being inversely proportional to the depth of alluvial cover. Thick alluvium stores water allowing ET to remove it before it may percolate into the underlying fractured tuff. Conversely, the presence of thin alluvium allows water to percolate into the underlying fracture network. Although the evidence seems pretty compelling on a conceptual level, we have not yet seen how this work has been used operationally to provide the upper flux boundary condition to Bo's flow model or any PA flow model. What is the status of this work? Is it complete, or is additional work required? If so, what work? For what purpose? Are there any criteria established to decide when the work will be complete?

2. Deep chlorine-36 work provides and independent line of evidence to test the conceptual models used in unsaturated zone water flow modeling at the site. Al Yang's work with carbon-14 and tritium provide the same benefit. It is not at all clear however, that this work is being used with any particular rigor in testing conceptual models. Can any of the models currently in vogue produce young water found at the depths found by June and Al Yang? If not, don't we have some problem with the models. If so, why haven't these results been presented? Indeed it appears that the recently proposed mixing model is an attempt to square the isotopic results with the single existing conceptual model currently under evaluation. Ideally, one might envision several competing conceptual models under evaluation with the isotopic work being used as one independent line of evidence to evaluate the appropriateness of each proposed model.

It appeared (but I'm not entirely certain) that all chlorine-36, carbon-14, and tritium samples were water samples taken from the matrix. Is this true? If so, what is the mechanism for relatively young water to imbibe into the matrix? Is it technically feasible to sample from fracture zones? Is anyone planning to take samples from fracture zones? Are there ways to discern whether samples are being taken from the matrix or from fracture zones?

It would be helpful to see where samples have been taken with respect to the lithology and the fracture zones. Summary maps and cross sections (or perhaps something 3-D) might help everyone make more sense of the data.

Statements were made by several presenters to the effect that water flow through the unsaturated zone at the site is likely to be intermittent with periods of active flow separated by periods when flow is minimal. In a letter dated December 7, 1994 from Larry R. Hayes (USGS) to Carol L. Hanlon (DOE), it is stated that Dr. Yang believes that "flow in the unsaturated zone could slow dramatically or cease in a fracture that closes with depth or is stratigraphically terminated, or by means of reaching a perched water body." He goes on to suggest that upon reaching some impediment the water would remain essentially ponded but that this ponded water "would still seep into adjacent rock matrix and continue to move by interstitial flow."

We need to consider mass balance when evaluating these claims[2]. The introduction of additional water into the system, however intermittent, must lead to either a change in storage or water leaving to accommodate the new water coming in. Change in storage implies the system is not in steady state. We can envision either a relatively dry matrix sucking up water only now becoming available or we can envision a system in which the perched zones are becoming larger that they have been in the past, expanding to accommodate some new influx of water. But climate history suggests we are not going from dry to wet but from wet to dry. We are definitely in an interglacial period and a particularly dry part of the interglacial at that. Also, where is all this matrix suction coming from? There is abundant evidence that at depth the matrix is pretty well saturated. Under these conditions, I'm not comfortable proposing a mechanism for sucking water from fractures into the matrix. However, I am comfortable with the notion of fractures supplying water to perched zones where the storage is under positive pressures (not capillary suction).

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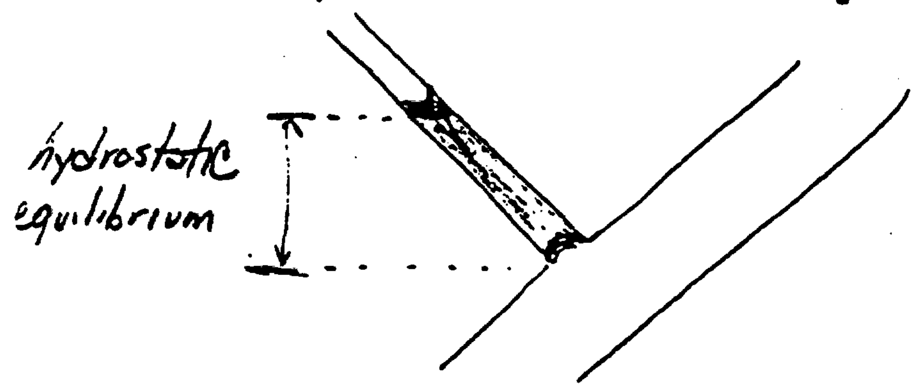
[2]this idea is shamelessly stolen from Darrell Leap, but it is such a good one I would like to support it.

So now our other alternative is to consider a steady system not undergoing any change in storage. Here we envision a system in which the water mass in a ponded or perched zone remains relatively constant. Either we can presume a system in which water influx is balanced by seepage into adjacent rock matrix and slow movement by interstitial flow much as Al Yang has proposed. Alternatively, we could presume a system with some overflow mechanism that the controls the size of the perched zone. Perhaps some stratagraphic control could form a "lip to the bathtub," or the perched zone could be connected to an underlying fracture that flows once sufficient pressure in the perched zone is reached. Given all these possibilities, I'm not sure why only slow movement by interstitial flow is being considered, especially since no geologic argument has been offered as to why fracture flow would predominate above these perched zones while only matrix flow occurs below. In summary, I don't think anyone has a problem with the idea of intermittent flow. However, as of yet I don't see any basis for presuming that even though water has reached significant depths in a short time, the only mechanism for movement beyond these depths is slow flow through the matrix. Yet apparently that is the only mechanism being considered. And any proposed conceptual model needs to be consistent with the geologic data available.

Has any isotopic sampling occurred beneath the perched zones? Do all the perched zones occur along the basal vitrophyre, or do they occur elsewhere as well?

While we're on the subject of considering various conceptual models of unsaturated flow through Yucca mountain, Bob Glass is preparing a paper that proposes intermittent flow and storage in a system of fractures only, requiring neither mixing nor storage in the matrix. Here is a brief (and undoubtedly butchered) version of his argument. Fracture flow begins in the near subsurface under the typically stated mechanisms (i.e. the June and Al Flint stuff). There are two reasons for lack of fracture/matrix interaction: (1) fracture coatings inhibit imbibition into the matrix, and (2) with the matrix virtually saturated, there is no capillary gradient to drive imbibition. So now where does the water go? With no mechanism for matrix storage, water in the fractures must zip all the way through the mountain pretty darn fast unless there is some mechanism for storage and intermittent flow within the fractures themselves. Bob proposes a mechanism at the intersections of fractures that will do two things -- focus the flow and store water. I'll leave it to Bob to explain the focusing of flow since it is not germane to our argument (maybe Bob will disagree), and concentrate on the mechanism for storage. Just like the conditions required for the onset of fracture flow, some hydrostatic pressure (proportional to the fracture width) must be achieved before flow is initiated. Now for the purpose of illustration, let's consider the case in which a small flowing fracture intersects some larger fracture. The bottom of the small fracture will continue to fill with water until it has built up enough head for water to enter the larger fracture. Likewise flow will cease once the head drops below the hydrostatic requirement for flow. So the bottom of the small fracture will always store some water. Intermittently, as new water is supplied from above, it will release water to the fracture below. As water flow from above ceases, flow to the larger fracture will also cease as the head drops below that required for flow. There will always be water stored in the smaller fracture at the point at which it intersects the larger fracture.



By this argument, sampling old water in the matrix is no guarantee that rapid movement of significant quantities of water is not occurring.

Using this conceptual model that postulates little interaction between the fractures and the matrix, let's return to June's mixing argument. If all the significant water flow is occurring through the fractures, then sampling water that has some fraction derived from the matrix gives apparent ages that are much older than the real age of the water derived from fracture flow. And since fracture flow is where all the action is for this model, we are interested in the ages of the water in the fractures and not ages of the water held in the matrix. Of course, this argument is just the inverse of June's mixing argument given above. In this model, we want to see how far through the mountain the modern water has penetrated. We would not view the modern water as some form of "contamination", giving apparent ages younger than the "real" ages. In June's mixing argument the water flux supplied by fractures is assumed to be negligible, hence the enthusiasm for a flux-based GWTT. However, there is no evidence to suggest that the flux through the fractures may not be appreciable. Indeed, for this proposed conceptual model fracture flow flux is dominant. So if we believe this model, then we should attempt to sample from fractures and perched zones fed by fractures and not from the matrix. And if we divorce GWTT from flux, then short residence times inferred from young water ages become a strong indication that the site may not be suitable.

Does anyone else think that it is incredibly serendipitous that both June and Al Yang were able to locate young water at depth unless there is quite some quantity of young water to be found down there (even if we consider June's mixing arguments)?

Very belated responses to Dr. Hinze's questions about the isotopic methods paraphrased as follows:
1. Will isotopic methods be able to provide credible info about GWTT within any reasonable time frame?
2. If not, is there an alternative?

By themselves, it is only possible for the isotopic methods to provide proof of short residence times. Long residence times prove nothing except that you failed to sample from the fast paths. While as Al Yang pointed out, residence times are not the same as groundwater travel times, short residence times can serve as a strong indicator against the suitability of the site. I would remain very skeptical about site suitability until such time as it could be compellingly shown that any water having a young age had travelled a path nowhere near to passing through the location of the proposed repository or that a compelling case could be made for why the water must travel very slowly from the point sampled to the accessible environment. Needless to say, given the geologic complexities as well as the complexities associated with our understanding of unsaturated water flow through fractured media, this task will be difficult indeed.

In the case where isotopic analyses do not rule out the site, they can not yield any definitive answers by themselves. Their use would be to serve as an independent line of evidence from which can be used to help evaluate the appropriateness of the flow models used estimate GWTT. Not using the isotopic methods only deprives us of a chance of independently evaluating the estimates derived from the flow models. Given where the project is now, I can't imagine seeing anything credible within the time frame Dr. Hinze is looking (1997) -- with or without the isotopic work.

In perusing the ACNW hydrology work plan for FY94-95, I noticed a working group on groundwater modeling to meet on april 18th, 1995. The key questions to be addressed are critical to understanding the Yucca Mountain program. Perhaps we did things backwards. Really, we should have had these key questions answered before we looked at the isotopic work. Unfortunately, we had little in the way of context with which to evaluate the isotope work. Perhaps that was apparent from the meeting and that realization became the catalyst for this upcoming meeting on modeling.

One positive outcome of the meeting was that it seemed to help foster some interaction among the key investigators. It says something about the lack of integration of the project that a meeting with the ACNW is helping to improve interaction within the project. It was clear that any cooperative work already taking place was initiated by the PIs themselves and not by anyone of authority in the project. In the absence of any overall technical leadership from the project, the PIs have taken it upon themselves to begin to build some semblance of an overall strategy for demonstrating site suitability. In addition to inertia, two factors work against them: (1) its not in their job description (they have no authority, and time spent on collaboration comes at the expense of meeting their own project milestones), and (2) they don't have a good understanding or appreciation of the regulations (ideally everyone in the project should understand the "big picture" objectives of the project, but unfortunately very few do). Although it is heartening to see more collaboration occurring among some of the PIs, the degree of integration is not nearly sufficient.

The overall problem is one of regulatory compliance but often it is treated as a purely scientific problem. This is not to say that there are not many scientific issues to be addressed in evaluating compliance, but the overall problem is one of decision making under uncertainty, best handled by application of decision theory of some form or another. In choosing to treat solution of the problem solely as a scientific exercise, the project can (and has) become sidetracked in completely understanding the processes, or completely understanding the system, or in realistically representing the system, or in finding the truth. Sometimes these are required, but they are not uniformly required unless they are needed to help make the decision about site suitability. So how do we decide what is needed? Something like, "we need to better understand about water flow instabilities because it may make a difference in performance of the site" doesn't wash because its too vague and conceivably there are a bazillion things like this that could impact performance. Do we study them all before we can make a determination about site performance? I think Yucca Mountain is funding alot of basic science that may or may not ultimately be of use to evaluating performance. While it's not ACNW's concern whether or not Yucca Mountain wastes money, ACNW should be concerned that in applying a scientific approach, the project's focus may be diverted from regulatory compliance. Especially in attempting to meet an accelerated schedule, failure to prioritize the work to address compliance means that you will likely be given an incomplete demonstration of site suitability. I don't understand why characterization work will continue beyond time that site suitability is to be demonstrated. Seemingly, the project is saying to the NRC, "we can throw together something to satisfy the regulatory requirements for demonstrating site suitability, but we'll be continuing on with our investigations for quite some time to satisfy ourselves." So apparently a science-based approach cannot give a credible answer about site suitability within any reasonable time frame. Or else why would they still be working?

By the way, something more concrete like the following is sufficient to justify scientific investigation, because there is an explicit link from the proposed investigation to evaluating performance:

> "It was thought that flow instabilities might effect site performance, so PA calculations were conducted for two conceptual models (one with and one without instabilities) to evaluate the sensitivity of performance to instabilities. Results show significant differences in performance and that incorporation of instabilities may lead to noncompliance. We need to study whether instabilities actually occur and to study their possible effect on performance.

Science is interested in finding truth, but we're only interested in finding enough truth to confidently make a decision.

The ACNW needs to be supplied a road map that shows how the Yucca Mountain project will get from where it is today to regulatory compliance. You need to be given some sense of how much additional work will need to be performed to understand the system sufficiently for a confident regulatory decision to be made. (Let alone how this work will fit within the project schedule.) There should be some prioritization for the planned work. Which activities will be absolutely essential? And which activities are supplemental work that may or may not be needed? Every piece of work should be mapped to show its context in the overall scheme for demonstrating compliance. Ask today about a road map, and you will be referred to the SCP. Its ten years old and less of a plan than a laundry list. With all the data presented that are beginning to show the complexity of water flow through the unsaturated zone, its becoming clear that the data requirements for demonstrating compliance will be quite substantial. That's why, more than ever, a concrete plan is needed. The planning exercise is essential to answering the question, "Is this even doable?" Also the plan will probably need to incorporate how the possible effects of climate change will be incorporated.

Good luck in attempting to get this kind of a plan. It will likely be a "blood from a turnip" experience.

While I'm an avid proponent of PA driving site characterization, I have some trepidation about Bo's model being used to drive site characterization. First of all, it is not a PA model. Its purpose is not to assess compliance but to better understand the system. And secondly, there has been but a single (equivalent porous media) conceptual model enacted thus far that will not handle fracture flow very well. So, were Bo's model to be used to direct data collection, all the data collection would be based on an implicit assumption of the validity of Bo's (single) underlying conceptual model. Data collection would proceed under the assumption that there is no uncertainty about the conceptual model, and really all we need to do is to collect a bunch of input parameters.

When sending out the final agenda for the working group meeting on October 5th, Lynn Deering also enclosed a status report that identified some key issues to be covered during the meeting. In looking over this list again, I wanted to see how many of the issues raised were actually addressed. The working group meeting did a good job of identifying the primary methods for dating groundwater, discussed their appropriateness for use at Yucca Mountain, and noted the limitations and difficulties associated with each method. And of course, the results obtained to date were presented. However many key issues remain unaddressed:

- I'm still not sure of DOE's response to the new information on fracture flow derived from the isotopic dating. Perhaps they are still sorting it out. Likewise, I didn't get a good sense of the emphasis DOE places on these results and how much emphasis they will now place on understanding the role that fracture flow may play in assessing compliance. I'm not sure that they are yet willing to entertain new conceptual models that allow fracture flow to dominate the system. On the other hand, info I've gotten subsequent to the meeting suggests that DOE supports PA calculations based on dual permeability models which will accommodate significant fracture flow. And for quite some time, they've commissioned work using the WEEPS model to give some sort of bounding type analysis for predominantly fracture flow systems.
- I'm not at all sure how DOE has modified its site characterization activities in response to the isotopic results. We got a detailed accounting of the results to date, but virtually no sense of what is planned for the future.
- We got some intermediate modeling results from Bo (which wasn't GWTT per se), but we didn't get any of Sandia's GWTT modeling results. There was no assessment of the consistency of these results with the isotopic data.
- The implications of the isotopic results on GWTT were not addressed.

- It was stressed that comparison of results between methods is needed to logically constrain the multiple interpretations possible from the application of any one method. At the meeting, however, there was no attempt to compare results[3]. It is difficult to compare the results because the samples for the various methods were not taken at the same location. The PIs have recognized the need to sample concurrently in the future. Subsequent to the meeting, June developed her mixing theory in an attempt to explain discrepancies between her results and Al Yang's results.

John Stuckless' results of a water table about 100 meters higher than present under wetter climatic conditions as recently as 15 ky ago, may have important implications for the containment requirements because of a possibly shortened pathway to the accessible environment. However, it is my understanding that a higher water table under climate change is not a consideration for the GWTT requirements because they are applicable only for undisturbed conditions.

The dramatic water rises in several boreholes penetrating perched zones presented by Al Yang serves as an illustration of the intermittent nature of flow down to the perched zones. On the face of it, I'd presume this is modern water supplied by some recent weather event. Water levels should be monitored in these perched regions in an attempt to correlate water table rise and fall to the weather. Has this water been sampled to see if it is indeed modern? If it's not modern, I'll be interested to see the conceptual model developed to explain intermittent releases of large quantities of old water.

There seems to be a bit of a siege mentality across much of the Yucca Mountain project. This mentality hinders the free and open exchange of ideas that scientists in particular rely upon to perform their work. I definitely get the feeling that questions about the project from outsiders are not appreciated.

[3]except for a thinly veiled attempt by Al Flint to discredit Al Yang's results presumably because they are inconsistent with the party line conceptual model. By the way, the quality of the argument Al Flint used was very poor. Al Yang was very gracious to put up with such a stunt.