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Dear Dr. Williams:

I reviewed with great interest your communication #123 (and several of the references cited therein), which proposed the development of a methodology for evaluating the effective porosity of basalts at the BWIP site. I have also had several of my NRC colleagues (Joel Hunt, Dan Goode, Atef Elzeftawy, Neil Coleman, and Mike Weber) review the proposal and offer me their opinions on the proposed effort. The topic is an interesting one and potentially useful to the program. However, I have questions which cause me to be skeptical about the degree that the work will provide the intended corroborative tool for effective porosity estimates derived from tracer tests. Prior to pursuing the proposed work, please consider the following comments:

- 1) The effort seems directed more towards an application of a previously developed methodology (Bredehoeft (1967); Narasimhan et al. (1984)) than the development of a new methodology. As such, it may be more sensible for us to suggest that BWIP investigate the tool than for us to perform the analysis.
- 2) It is not clear that the analysis can be applied to a multiple-aquifer system as is present at BWIP. What is the relationship between either the measured barometric efficiency, or the measured specific storage, and the effective porosity of individual units?
- 3) It is not clear that the analysis will in fact yield effective porosity, as opposed to apparent porosity or total porosity. In a fractured medium in particular, the apparent (connected) porosity may be much larger than the effective (connected and flow-contributing) porosity. The formulation of the methodology, described in your letter and in the reference documents, refers only to "porosity", which is evidently (at best) the apparent porosity, not effective porosity. Part of the compression of the aquifer fluid may be due to compressed non-effective pores. What is the relationship between effective porosity and a) tidal efficiency; b) specific storage; and c) barometric efficiency? If in fact the method will only yield apparent porosity estimates, do you consider that such estimates will be valuable to the program?

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- 4) Narasimhan et al. (1984) question the ability of the method to yield porosity without independent measurements of storativity from dynamic hydrologic tests. They claim that the barometric and earth tidal responses will yield identical information, which is apparently just the ratio of specific storage to porosity. Thus, they say, there are not enough independent equations to solve for porosity.
- 5) The effect of well bore storage, for wells open to the atmosphere, both on measured barometric efficiency and on evaluation of the response to earth tides should be discussed by you in more detail.
- 6) The methodology is based on an assumption of a hydraulically perfect well which instantaneously equilibrates to any fluid pressure change in the aquifer. Narasimhan et al. (1984) state that "this assumption will be of practical meaning only when the aquifer has high transmissivities and the specific storage of the well, S_{sw} , defined as the volume of water released from the well per unit change in pressure head, is small." You also note in your letter that Bredehoeft (1967) stated that the method might not apply to basalt media. Therefore, the applicability of the method under the conditions at the BWIP site ("low"-permeability basalt) may be questionable.
- 7) It is not clear that the specific storage determined by earth tide analysis will be more reliable than the specific storage determined from the numerous pulse and slug tests performed at the site to date, particularly for individual units (see comment 2 above). These specific storage estimates will be augmented by the results of the future multi-well pump tests. The specific storage estimates could be utilized to determine porosity without dependence on the earth-tide/barometric efficiency methodology.
- 8) You note that Bredehoeft (1967) stated that this methodology would yield large scale values of porosity. What length scale, both vertically and horizontally, is meant by "large scale" in this case?
- 9) While you state that the tracer testing methodology is not well established in terms of field application, the BWIP efforts in this area have not been altogether unsuccessful. I do agree that a corroborative methodology is desirable, however.
- 10) I agree that the year-long near-continuous baseline monitoring data collected at BWIP offers an excellent opportunity for an investigator to attempt to apply the Bredehoeft (1967) methodology.

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If, after considering the comments above, you feel that the concerns expressed in comments 1 through 8 can be resolved, and that the proposed work should be pursued, please address each of these points in a letter for my consideration, at which time I would direct you whether or not to pursue this work. Also please include your estimate of the cost and level of effort required for this work. (Such a letter need not be prepared if you decide not to make a case for pursuing the work.) In either case, I find the topic very interesting and potentially useful to the program.

The action taken by this letter is considered to be within the scope of the current contract NRC-02-82-044. No changes to cost or delivery of contracted services and products are authorized. Please notify me immediately if you believe that this letter would result in changes to cost or delivery of contracted products.

Sincerely,

Original Signed BY

Matthew Gordon, Project Officer
Geotechnical Branch
Division of Waste Management

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REFERENCES

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Narasimhan, T., B. Karehiro, and P. Witherspoon, "Interpretation of Earth Tide
Response of Three Deep Confined Aquifers," J. Geophys. Research, 89(B3), 1984.

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