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Hydrogeology • Mineral Resources Waste Management • Geological Engineering • Mine Hydrology

September 24, 1987
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Communication No. 149

Mr. Jeff Pohle
Division of Waste Management
Mail Stop 623-SS
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

RE: BWIP

Dear Jeff:

This letter outlines our comments on the document entitled "Numerical Modeling of Ground-Water Flow Systems in the Vicinity of the Reference Repository Location, Hanford Site, Washington." The authors of the document under review are Paul Davis, Walt Beyeler, Mark Logsdon, Neil Coleman, and Ken Brinster. Your letter refers to this document as a final draft; however, the draft is not dated. You requested in your letter that we conduct a technical peer review "with emphasis on hydrogeology, alternative conceptual models of flow, hydrochemistry, and geostatistical methods." We comment herein on these topics in the order in which they appear in the report under review. A brief list of typographical and assorted errors that we identified while reviewing the document is appended to this letter. We did not review the document for the explicit purpose of conducting this type of detailed review. We are supplying this information in order to facilitate the correction of the final draft.

The section on regional geologic structures, which begins on page 17, does not discuss the Smyrna Anticline and the Smyrna Anticline aeromagnetic anomaly referred to by Reidel (1984). We believe that a discussion or at least recognition of this feature should be included in the report because the anticline and associated aeromagnetic anomaly align with that portion of the Columbia River which lies along the eastern side of the Hanford site to the east of the Reference Repository Location. The hydrologic and hydrogeologic significance of this coincident location is not known at this time. We believe they should be included because the disposition of the anticline may be relevant to conceptual models regarding flow in the vicinity of the Columbia River on the east side of the Hanford Reservation.

We agree with the report's statements regarding the purposes of the conceptual models required for predicting groundwater travel time. The report states that one model is required for predicting the travel time while another model is required for designing and analyzing the tests needed to obtain values for the coefficients for predicting travel time. The difference in the models is required because of the difference in the scales of the two requirements (models). We also concur with the report's statement regarding the nature of the measured zones and their correspondence to the defined hydrostratigraphic units at the BWIP site. The report states that researchers by necessity have relied on measurements taken from test intervals that are of different thicknesses than those of the defined hydrostratigraphic units. This problem has been noted in the report under review but it commonly is not acknowledged in many such modeling efforts.

The report discusses the assumption that porous media flow is applicable at the BWIP site for the units of interest. We concur with the report in that it states that "It is impossible to conclude that the assumption of the porous media behavior for the flow tops and interiors is correct" (p. 32). We agree that the groundwater flow systems at the site may be assumed to be equivalent to porous media flow with respect to large scale hydraulic behavior. We do not believe that the flow system will behave as a porous media equivalent for considerations of transport of contaminants. Fracture flow will dominate the transport of contaminants.

The report refers to the fact that historically the domain of interest for hydrogeologic property tests is at a much different and smaller scale than the scale of the elements used in the flow model. Scale is a common problem which we have discussed at length in the past.

Figures 3.5, 3.6, and 3.7 in the report are potentiometric surface maps of the Saddle Mountains Basalts, the Wanapum Basalts and the Grand Ronde Basalts, respectively. The potentiometric surfaces were prepared for this report using data obtained during the spring of 1984. We believe these potentiometric maps should be qualified; the qualifications should state that the implied continuity of long flow paths that is depicted on these figures is not necessarily definitive. These long continuous flow paths in reality may be disconnected; they may not reflect the effect of geologic structures on groundwater flow. We do not believe that the report adequately qualifies these three figures. The report under review qualifies more appropriately the figure for the Grande Ronde Basalts; it states that very few data points exist and that conclusions about flow directions are highly speculative.

The report refers to a "flat horizontal and vertical gradient" on page 55. We believe that it would be more valid to use the word "low" instead of "flat" to describe the nature of the hydraulic gradients in both the horizontal and vertical directions. This is a minor point but we believe it may be confusing with respect to describing the vertical hydraulic gradient.

The summary of hydrochemical characteristics of the basalt formations on and adjacent to the Hanford site is very appropriate and worthwhile. We are not aware that anyone else has compiled the various views of different researches on the hydrochemistry of the Hanford site. We do take exception to the statement in the report under review on page 68 that the conclusions of Williams and Associates are in conflict with the conclusions derived by Lehman (1983). Ms. Lehman used a different data base which extended her area of study beyond the bounds of the Hanford site. Our studies have dealt only with data obtained by Rockwell Hanford Operations on the Hanford site and specifically west of the Columbia River. We believe that it is misleading to state that our conclusions are in conflict with Ms. Lehman's conclusions because of the differences in data bases. We believe it would have been difficult for Ms. Lehman to interpret the on-site data (as we did) and obtain the three types of groundwater which she defined in her study. We have specific comments regarding the Lehman study but we do not believe this review constitutes the proper forum for the inclusion of those comments.

The report points out accurately that "precision" and "sample variability" must be considered when reviewing the hydrochemistry at the BWIP site. Too often these aspects of technical review are overlooked.

The report states (p. 74) that many conceptual models are equally plausible. We concur with this statement and will point out in subsequent comments that additional conceptual models may exist that would be interesting to investigate using the model which has been developed by Sandia National Laboratories.

The report discusses the continuity of hydrostratigraphic units and specifically the validity of the claim by Rockwell Hanford Operations that a barrier exists to the west of the RRL which currently is referred to as the "Cold Creek Barrier." The discussion in the report under review, which begins on page 76, questions the interpretation of data by Rockwell Hanford Operations regarding the existence of this barrier boundary. We believe this discussion is appropriate and accurate.

The report discusses the validity of the assumption that porous

media theory can be utilized to characterize fracture flow (p. 78-79). We believe this discussion is appropriate, as is the discussion of homogeneity versus heterogeneity and isotropy versus anisotropy (p. 79-80). These topics must be considered even though they cannot be addressed completely at this time due to the limited data base. We concur also with the report's statement that the representativeness of the measured hydraulic coefficients is in question because of the nature of the testing which has been conducted to date. The question of representativeness arises because of the small volume of rock which has been tested by DOE due to the small scale single well test techniques that have been used.

The report summarizes the study conducted by Arnett (1980). The report states in the last paragraph of page 93 that "flow was produced to the north and then upward to the Columbia River." The location of the origin for the northern direction is not clear; however, this comment a minor one.

The section of the report under review which evaluates numerical models is excellent. This evaluation begins on page 102. It is highly appropriate that an NRC contractor discusses the drawbacks and advantages of each of the modeling studies which has been conducted for the BWIP site. The report states (p. 108) that "boundary conditions are the root of the differences in model-predicted flow paths." We agree with the statement that the models will be of little use in predicting groundwater flow and transport until a sufficient number of tests have been performed over a much larger scale than those which have been conducted to date. In addition, the hydrogeologic aspects of the various geologic structures in the area must be tested directly. The report concludes that a clearer definition of boundary conditions such as recharge must be obtained. We concur with these conclusions and hope that they are acknowledged by future modelers.

Conflicts are apparent between the definitions of sensitivity analysis, error analysis, and uncertainty analysis used in the paper and the definitions which appear in the literature. The majority of this discussion occurs between pages 93 and 110. We provide the following definitions common in the literature related to these topics in order to clarify our views. A sensitivity analysis is a mathematical and modeling construct associated with the variation witnessed in model outputs in response to variation in model inputs, model form, or model coefficients. Error analysis has two historical meanings. The first is the concept of propagation of errors in outputs from errors in inputs. The second is the use of Monte Carlo simulation to perform a type of sensitivity analysis. Uncertainty analysis is aimed at analyzing a number of sources of

variabilities in model output. These phrases do not appear to be used consistently in the reports reviewed by Sandia National Laboratories based on the definitions we present above.

It is significant to note that the regional model was calibrated by a nonstandard process. Calibration began with the adjustment of hydraulic coefficients to obtain head values which were within the range of measured head values. The report states that calibration continued by adding complexities to the conceptual model in order to try and achieve a better match of modeled heads to kriged head values. As stated above, this procedure is not customary but we believe it is appropriate considering the small data base.

The report (p. 120) states that vertical leakances were calculated from the vertical hydraulic conductivity and the thicknesses which were assumed appropriate for the modeled units. It is not clear where the vertical hydraulic conductivities were obtained. This point could be clarified if the report stated on this page the source of the values of vertical hydraulic conductivity. The report does state that hydraulic conductivities were obtained from Bonano and others (1986) but we believe this reference pertains to horizontal hydraulic conductivities only.

The large gap in contoured kriged heads for the Grande Ronde Basalts on figure 7.9 should be explained. The gap may be caused by the lack of data in this area (Hanford site). But we believe the report should discuss the absence of contours in area of the gap.

The conceptual models tested by the first modeling effort discussed in this report are limited as noted in the report under review. Other conceptual models exist that could be tested using the model developed by Sandia National Laboratory. Additional conceptual models were tested using the revised model produced by Sandia National Laboratory. Test results are discussed later in the report under review.

The report states (p. 132) that the assumed homogeneity of the units requires that the mechanism for generating variations in gradient that could correspond to the areal extent of the Hanford Formation constitute the connection of those layers below the Hanford Formation to the layer that represents the Hanford Formation. We believe that it might be possible to achieve the same objective by using more boundaries with less recharge along with higher hydraulic conductivities in a horizontal direction. We are not stating that we believe the report should be changed but we believe that this additional scenario should be evaluated. We are not certain that our statement is completely correct

because we do not have access to the model to conduct sensitivity analyses that are needed to support our statement.

We observe that Sandia National Laboratory used model revision as a tool to calibrate model generated heads to kriged heads within the Pasco Basin and the Hanford site. This calibration procedure changes the conceptual model used in the formulation of the numerical model. Appendix C describes a situation where a conceptual model used for kriging head values does not match necessarily the conceptual model used for modeling. The conceptual model used for kriging heads does not appear to be altered during calibration. The kriged head distributions are given preeminence over the conceptual model used in the numerical model; this procedure constitutes a potentially fatal flaw. Decisions concerning conflicts between conceptual models should be based upon professional judgement.

An additional conceptual model appears in the discussion of the revised model. A possible discharge area is suggested that is associated with a fault that underlies the Columbia River. Unfortunately, it is not clear to us whether this feature was incorporated in the subsequent numerical modeling. The figures which outline recharge, constant head boundaries, and no flow boundaries do not indicate whether such a conceptual model was incorporated into the numerical modeling. We believe this scenario to be viable and it should be modeled. We believe also that a no flow boundary should be instituted in the vicinity of the Columbia River to test the conceptual model that would place the RRL site area in isolation from inflow through the basalt layers to the east, west, and southwest. This particular conceptual model was not incorporated in Sandia National Laboratory's modeling effort.

The model results are discussed beginning on page 140. The results are evaluated by comparing the modeled head distributions with the kriged surface. Unfortunately, the kriged head surfaces are rather idealized and do not seem to reflect the existence of potential barriers to flow of groundwater in the basalt sequences. We do not understand completely how the modeling effort and the review of the model output incorporate the existence of the potential barriers to flow. It appears that the model output is assumed to be more correct if it more closely matches the kriged heads. This assumption should be discussed in greater detail.

The report under review states that the local scale model was not converging at the time work was stopped on this modeling effort. It is unfortunate that this work stopped because it would have been most enlightening to see whether the model converged. We believe that the remedial actions suggested in the report

(smaller time step and larger storage coefficient) would achieve convergence.

The decision process used to address uncertainty is discussed to a limited extent in the report under review. The greatest manifestation of the treatment of uncertainty is evident in the discussion of the calibration of the model. Professional judgement is the method selected, by necessity, to reduce uncertainty by selecting viable conceptual models for model calibration. The calibration process is described well but all the possible alternatives are not explored.

We are impressed by the effort that has gone into producing this modeling report. The report exhibits a great deal of effort in reviewing the existing literature on the hydrogeology of the area as well as on previous modeling studies. Some of our comments may require some minor editorial changes; please call if you have any questions regarding our comments.

Sincerely,

Gerry

Gerry V. Winter

GVW:sl

REFERENCES

Arnett, R.C., 1980, Far-field Modeling: Simulation of the Natural Groundwater System in the Pasco Basin. Basalt Waste Isolation Project Annual Report - 1980, RHO-BWI-80-100.

Bonano and others, 1986, The reference is not cited at the end of the report hence we cannot provide the reference for our letter.

Lehman, L., 1983, Hanford Reservation: Analysis of Chemical Data Released By DOE on February 15, 1983. Unpublished Report, dated March 27, 1983.

Reidel, S.P., October 1984, The Saddle Mountains: The Evolution of An Anticline in the Yakima Fold Belt. American Journal of Science, vol. 284, p. 942-978.

TYPOGRAPHICAL AND OTHER ERRORS

1. Page 32, line 9. A period is missing at the end of the sentence.

2. Page 49, lines 9-11. The sentence infers that the Mabton interbed is one of the units that makes up the Saddle Mountains Basalts. Technically, the Mabton interbed is part of the Ellensburg Formation which is designated as a separate formation from the Saddle Mountains Basalts Formation.

3. Page 49, line 12. The "with" should be "within."

4. Page 54, line 18. The period is missing at the end of the sentence.

5. Page 55, line 23. The word "addition" should be "additional."

6. Page 68, line 12. The work "methodology" should be "methods."

7. Page 81, line 4. The word "spacial" should be "spatial."

8. Page 81, line 14. The work "in" should be "is."

9. Page 93, line 19. The word "Use" should not be capitalized.

10. Page 100, line 19. The small "e" after 5 should be deleted.

11. Page 136, line 11. The "were" should be "was."

12. Page 137, line 1. The "e" is missing off the end of feature.

13. Page 137, line 2. The verb "turn" should be "turns."

14. Page 151. The reference to Bonano and others, 1986, is missing from the reference list.

15. Page ?, Table A.4.4. The table is missing.

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