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

RE: Draft Generic Technical Position Papers

Dear Jeff:

We have enclosed a copy of our reviews of the "Draft Generic Technical Position: "Interpretation and Identification of the Extent of the Disturbed Zone in the High-Level Waste Rule (10 CFR 60)" and the "Draft Generic Technical Position on Groundwater Travel Time (GWTT)." Our reviews do not follow the WMGT Document Review format because we believe that a letter format is more appropriate for the review of internal documents. We have rushed these reviews more than we would have liked. More time would have enabled us to do a better job of strengthening these NRC documents. They do need some revision. In addition our review may appear to be a bit disorganized due to the rush.

If you have any questions concerning our reviews, please call.

Sincerely,

Roy Williams/sl

Roy E. Williams

REW:sl

enclosure

WM-RES
WM Record File D1020 W+A

WM Project 10, 11, 16
Docket No.
PDR ✓
LPDR ✓ (B, N, S)

Distribution:
J Pohle

(Return to WM, 623-SS) SAC

2650

Review of

DRAFT GENERIC TECHNICAL POSITION:
INTERPRETATION AND IDENTIFICATION OF THE EXTENT OF THE DISTURBED ZONE IN THE HIGH-LEVEL WASTE RULE (10 CFR 60)

We have reviewed the hydrogeologic implications of the document under review. We comment specifically upon those aspects of the report under review that deal with hydrogeology.

The last paragraph of page 2, first sentence, states that "The volume of the geologic setting which contributes to isolation of HLW from the accessible environment clearly has its outer boundary at the accessible environment." We believe that use of the phrase "volume of the geologic setting" is awkward. The term geologic setting usually is used to describe the physical characteristics of the geology with respect to the site. The geologic setting usually is considered synonymous with geologic framework. Perhaps you could use geologic column.

The second sentence in the first paragraph on page 4 is inconsistent with the definition provided on page 1 of the report.

Page 4, paragraph 3, of the document under review states that, "Since ground-water flow essentially does not occur within pure salt ..." We believe that this sentence should be modified because justification is not provided in the technical position paper to warrant this blanket statement. The mechanism controlling ground water flow in salt is still being discussed and has not been resolved at this time. Therefore it seems inappropriate at this time to state that ground water flow does not occur in salt or pure salt.

The first sentence of the first paragraph, page 8, states that "Taking the envelope of the above distributions for a reasonable range of anticipated field conditions, the no-stress-change contour could be somewhat conservatively estimated in many cases to be about 5 diameters or 5 times the opening height from the edge of the opening." We question whether this sentence should state 'taking the mean envelope of the above distributions ...' The sentence should be better stated whether our suggestion is valid or not.

The document under review states on page 8, paragraph 3, "its depth, prevailing stresses, and other geologic anomalies." We do not understand what "other geologic anomalies" means in this context. Salt is not an anomaly in the geologic environment. It is reasonably common. If the word "other" was deleted the sentence would be clearer to the reader.

The third sentence of paragraph 3, page 8, states that "Excavations in salt undergo elastic-plastic deformation and quickly go into the creep phase which is an important feature of salt behavior." The sentence isn't clear. The salt creeps; the excavation does not creep.

The report also states on page 8 that, "Traditional concepts of groundwater flow through porous media and jointed rocks are not applicable to pure salt." Again, we must reiterate our belief that this statement is inappropriate at this time. These issues have not yet been resolved by the scientific community.

Page 15 of the report under review states that, "in establishing the origin surface from which to calculate ..." We do not understand what is meant by "origin surface." Perhaps the term surface could be deleted from the sentence, unless it is needed for a reason that we have missed.

Appendix A discusses the relationship among rock properties, ground water flow and travel time calculations. We believe that the equation and definition of terms should be modified so that "$\bar{v}$" becomes v or q. We believe that this change should be made because the standard designation for specific discharge is with the term v or q. The term "$\bar{v}$" ordinarily designates average linear velocity.

We believe that the last paragraph of Appendix A should be deleted. This paragraph does not add to the discussion but rather detracts from the technical accuracy of the remainder of the discussion. In the geologic media under consideration in the high level waste program intrinsic permeability and effective porosity are not "correlated" nor are they correlatable. A rock with high effective porosity may have a low or high hydraulic conductivity depending on fracture size and density. In addition the change in technical terminology (intrinsic permeability to hydraulic conductivity) adds confusion for the uninitiated. The reference to empirical relationships (Kozeny-Carman or Fair-Hatch equations) is insufficient. The discussion, if left in the position paper, should note that these relationships were developed for ideal media such as glass beads, not for rocks. Problems with these models occur when these relationships are transferred to geologic materials in the field. These relationships in no way have been demonstrated to be appropriate for fractured porous media such as basalt (BWIP), dolomite (SALT) or welded tuff (NTS).

Review of

DRAFT GENERIC TECHNICAL POSITION ON GROUNDWATER TRAVEL TIME (GWTT)

We concur that a stochastic approach to estimating ground water travel time (GWTT) is a viable alternative to estimating travel time by deterministic methods alone. But the subject document should be discussed at length before the decision is made to publish it in it's present form in order to pursue this objective. It will be strengthened considerably if certain cloudy issues can be cleared up. We have several major general comments on the paper. These general comments are presented first. They are followed by additional editorial type comments about the text.

The overall discussion treats GWTT as a random variable, yet the paper never states explicitly that this is so. On p. 10 (paragraph 1), this random variable is defined, albeit somewhat inadvertently, as <u>the time required for any tracer particle to travel from the disturbed zone to the accessible environment</u>. A working definition of the random variable should be stated early in the text, along with a listing of the known factors (other random variables and interpreted hydrogeologic conditions expressed in the form of conceptual models) that influence the probability distribution of the random variable GWTT. It is important to recognize that GWTT is a random variable because of the associated statistical constraints on a random variable and because of the nature of the data base required for a distribution function to be defined for the random variable.

In a theoretical sense the true distribution of GWTT includes all effects of all controls on the tracer particles traveling to the accessible environment along all physically viable pathways emanating from the site. The traveling particles may be subjected to numerous rationally conceivable variations in geologic and hydrogeologic conditions (boundaries, discontinuous aquifers, thinning and thickening of units, anisotropies, etc.). However, because NRC objectives state that GWTT must be determined for the "fastest path of likely radionuclide travel" (p. 3, paragraph 2), the concept of a distribution that represents the <u>true</u> distribution of GWTT is only of limited interest. Consequently, the random variable, GWTT, has to be viewed in a manner that includes certain assumptions and restrictions that influence the sampling techniques that are implemented to obtain field values for it. These assumptions should be stated clearly and justified. These assumptions pertain primarily to the data base from which the variables that comprise the GWTT distribution are derived. The defensibility of the data base will in large part determine whether or not the

"fastest path of likely radionuclide travel" has been identified and used to calculate GWTT. But the identification process probably can be based on a stochastic procedure even though the acquisition of data cannot be. Somehow the testing procedure itself, not the stochastic procedure, must be conducted so as to guarantee that the fastest path is included in the distribution for GWTT. The stochastic analysis of the data will not be able to achieve this objective. The paper should make this point clear so that the GWTT distribution is not based on guesses or hypothesized distributions.

As the above discussion points out GWTT is a random variable that must be succinctly defined and adequately described so as to avoid confusion in its interpretation, measurement, and application.

In the case of deterministic approaches the assumption that flow paths are known or have been measured ("fastest path of likely radionuclide travel"--implies deterministic) raises two critical problems. The paper leans away from this approach because of these problems. These problems are:

a) Will it ever be possible to develop a data base that describes confidently and characterizes subsurface hydrogeology so as to define all specific flow paths, including the fastest flow path? It seems reasonable that even with considerable state of the art field data one can only do his best to define confidently the fastest flow path in a deterministic sense. In spite of such efforts he may have measured some other path. Interpretations and judgments that yield a probabilistic model of subsurface conditions on the basis of a sound data base probably add safety to the procedure. But the data base must at least bracket the fastest path so that the GWTT distribution is not simply a best guess.

b) Is there a justifiable place for deterministic conditions in a supposed probabilistic (stochastic) assessment? The GWTT approach presented in the subject document leans toward a hybrid analysis that consists of deterministic and stochastic components. Such hybrid methods can be applied effectively, provided that the probability of occurrence of a defined (deterministic) flow path can be assigned. Consequently, the overall probability that GWTT will be less than a given time t (say 1,000 years) is given by:

$$P(GWTT<t) = \sum_{i=1}^{N} P(GWTT_i<t)\cdot P(path_i)$$

where N is the total number of viable flow paths as determined by the field test data for the various hydrogeologic conceptual models. From a practical standpoint, N should be limited to a reasonably small value that brackets the fastest possible flow paths. Data derived from data collection procedures designed to measure slow GWTT's are only of limited interest as explained previously. When viewed in this context the importance of the design of the data collection procedure to the hybrid model becomes quite clear. The paper should emphasize this point.

Assumptions regarding statistical stationarity* for the input parameters (also random variables) used to estimate the GWTT distribution should be stated. Some discussion is needed about how stationarity will affect the GWTT distributions and their validity, as well as how the stationarity assumptions will be assessed and verified using the field data base. Both spatial and temporal stationarity should be considered if spatial variations and short term temporal variations in parameter values are to be incorporated into the analysis as is stated on pages 6 and 7 of the technical position paper. Discussion of stationarity is important because each of the parameter's- permeability, gradient and effective porosity (the measured parameters necessary to calculate travel time) will have its own distribution for a given site. These distributions will be determined by the hydrogeologic conditions at the site and to a larger extent by the design of the testing procedures applied to the site. First order stationarity requires that the <u>expected</u> value for each of these parameters is everywhere the same even though the true value may vary over some range. Second order stationarity further requires that the expected squared difference between samples separated by some distance is independent of the location from which that pair was taken. Quite frankly we are not certain about the implications of stationarity with respect to the distributions that comprise the distribution for GWTT. We have not had time to work that out. But it is clear that the issue should be addressed in the paper. Given more time we might be able to contribute to this effort.

The assertion that "conservative" assumptions will be used in the definition of flow paths and travel time calculations (as stated several times in the document) is contrary to the fundamental notion of a probabilistic (stochastic) analysis of an existing hydrogeologic environment (as opposed to a data base). If the GWTT distribution produced by the available data on permeability, effective porosity and gradient is to be treated as an unbiased, valid representation of the true GWTT distribution then input parameters must be obtained in an unbiased, essentially random fashion. If valid random variable data are scarce, thereby making the uncertainties high, then subjective judgments will be

required to place reasonable ranges on parameters (with associated high variances). The rationale for these two somewhat different approaches should be explained in the paper. If the sample distribution is biased toward the high side of the prototype distribution (as hopefully it will be) then this should be explained in the paper. We have alluded to this problem above in a slightly different context. The paper, as is, leaves the reader somewhat confused about how the distribution of a random variable is determined. It is important to make clear to the reader when the distribution of the data is important and when the real distribution is important. The paper tacitly implies that the distribution derived from the data base actually represents in an unbiased way the real GWTT distribution. But if "conservative assumptions" are used justifiably, then this cannot be true.

The use of the term "deterministic models" on pages 22 and 23 is somewhat confusing. In reality the model discussed on pages 22 and 23 is a deterministic model with data input stochasticly. Whether or not such a model is conservative is determined by the characteristics of the domain of the numerical or analytical function and by the values of the input parameters. We suggest retitling this section "Combinations of Deterministic and Stochastic Models" or "Hybrid Models".

Our last general comment is intended to attempt to strengthen the paper with respect to the relationship between the GWTT distribution and the existing data base. This comment is not completely independent of previous comments; rather it supplements them.

Once the paper has been placed in the context of distributions of random variables wherein stationarity issues are resolved (component random variables are addressed in the context of expected values of field measurements), then it seems reasonable to discuss the real basis for each component random variable and its distribution. These distributions will either be guesses or they will be the product of expert judgment about valid field data. Hopefully the latter will prevail. The paper, as is, does not make this point sufficiently strongly. Unfortunately the acquisition of field data required to begin the process of creating a GWTT distribution (or any other stochastic function) requires a conceptual model. This conceptual model has to be deterministic because all known procedures for measuring permeability, gradient and effective porosity in the field require a deterministic conceptual model. This fact simply must be accepted at the outset of the paper. Otherwise the reader is misled into thinking that a deterministic conceptual model of the site is not necessary. This fact is indeed a fact because the design of a drill and test program requires that the hydrogeologist choose among many alternative designs and tests

when he selects the test or tests to apply at a given site. For example, a pumping test cannot measure permeability in a heterogeneous hydrogeologic environment and a homogeneous but multilayered hydrogeologic environment (each layer is homogeneous) at the same time. Two different tests with two different well field designs for these two different deterministic conceptual models are required. One conceptual model or the other must be selected in advance of the first test. If information is gained during the test that suggests that the wrong conceptual model was selected, then the design of the drill and test program must be reevaluated and probably the test repeated after new holes are drilled. This problem occurred at the BWIP site during the first tracer test. The head at the injection well was almost an order of magnitude greater than the head (opposite direction) at the recovery well. This phenomenon indicates hydrogeologic heterogeneity instead of homogeneity. A homogeneous deterministic conceptual model was assumed in the test design. Head (gradient) data measurements require similar assumptions prior to design of piezometers.

We suggest that the paper explain that we understand that field data are obtained after a deterministic conceptual model is assumed to exist. We should explain also that we understand that conceptual models necessary for testing must be altered as testing progresses. We understand this in part because we have sweated through this evolutionary process at BWIP. Once we make this position clear then the role of the stochastic model can be portrayed in a more accurate and valid manner. That role is to use the data resulting from the above described process and convert it to a usable stochastic tool. That tool reflects the stochastic character of the prototype only to the extent that the deterministic data base reflects it. If we follow this procedure then the use of words such as "conservative assumptions", as discussed above, begin to become consistent with statistical theory. In short the stochastic process involves the data base that in fact is the product of deterministic conceptual models and professional judgment. The paper will become much clearer if this sequence of events is placed in proper perspective in the paper.

The following editorial type comments may be helpful also.

The last sentence (page 5) of the first full paragraph states that the 1,000 year GWTT objective assures that ground water conditions are favorable, since the repository will be controlled by regional hydrologic processes (which are characterized by long travel times), outside of any local, relatively fast-moving ground water systems. We suggest that the phrase "since the repository will be controlled by hydrologic processes" be changed to "since the repository will be influenced by regional

hydrogeologic processes." In addition, the word "systems" should be deleted from the phrase "relatively fast-moving ground water systems".

The first sentence of the second paragraph on page 11 states that "At the upper and lower limits of the distribution there will be ground water travel times which, although possible, are not likely to represent significant quantities of radionuclides, and are therefore inappropriate measures of GWTT along the pathway." We believe that this sentence is misleading because each pathway will have a separate GWTT derived from an appropriate measurement (by definition of CDF). The sentence should state that the GWTT's along some pathways are not of interest because they will be much slower than the fastest pathway (as discussed above). The same explanation about the limited need for such data would be appropriate.

Special consideration number one on page 13 states that in a medium unaffected by boundaries, the direction of unsaturated flow is predominantly vertical, while saturated flow is primarily horizontal (confining features such as aquicludes, faults and dikes complicate this general picture). This statement is incorrect because the direction of ground water flow in the saturated zone (horizontal or vertical) is dependent primarily upon location with respect to recharge and discharge areas. Vertical flow generally occurs beneath recharge areas or discharge areas. Horizontal flow occurs between recharge areas and discharge areas, with a few exceptions. Special consideration number two indicates that unsaturated flow tends to be more transient than saturated flow. We believe that the phrase "more transient" should be explained further. The extent to which flow in the unsaturated zone is transient is almost exclusively a function of the temporal distribution of recharge. In arid regions unsaturated flow may approximate steady state or even be zero.

The last paragraph on page 14, first sentence, states that repositories in fractured porous media exhibit transport behavior characterized by a wide range of ground water travel times. This sentence should be reworded because repositories do not exhibit transport behavior. In addition we are not certain that transport behavior in fractured rock has been shown to be characterized by a wide range of travel times. Transport in fractured rocks is hardly understood at all because so few data exist on factors such as distribution coefficient.

The second sentence on page 15 states that effective matrix diffusion probably is more significant in media with high matrix permeability and porosity such as fractured tuff than in relatively tight rocks such as basalt and granite (Blencoe and Grisak, 1984). This statement is unclear. Basalt, granite or

tuff can be permeable or impermeable, depending on fracturing. In addition, fractured (usually welded) tuff has low matrix permeability and not high matrix permeability as stated in the document. The effects of matrix diffusion generally would be expected to be most significant where contaminant movement due to advection is very slow.

We suggest that on page 18 of the document that "or inferred" be deleted from line 8. We believe the sentence should imply that calculation of travel time should be based on real data rather than stating that 'inferred data' can be used. We do not understand what is meant in the next line by "unquantified uncertainty" and "conservatisms". As explained above conservatisms must be viewed in the context of a data base, not in the context of a guess. We are not able to conceptualize "unquantified uncertainty." Uncertainty itself is difficult enough to deal with.

The last sentence on page 20 states that the tracer particles in a heterogeneous medium will diverge from their neighbors, following the paths of least resistance which are not necessarily the shortest paths. We believe that this statement is not necessarily correct. Heterogeneous porous media may or may not behave this way depending on the type of heterogeneity.

The document does not offer the reader any guidance on the collection of valid data that are necessary to generate valid cumulative distribution functions for travel times. We have alluded to this problem slightly in our last general comment. Indeed page 24 alludes to "estimates of parameters" rather than to measurements. The last bullet on the page even suggests that best estimates can be based on "field <u>estimates</u>." We prefer to deal with defensible field measurements rather than estimates. This point is related to our previous comments about identification of fastest path. The heading on page 24 (A.e) should be changed to "Derivation of Parameter Values from Field Data Bases." We should never insinuate that we might condone guesses. Instead we should add guidance on what constitutes a valid data base from which a stochastic model can be derived.

The document contains a few misconceptions. For example, page 25 alludes to a probable relationship between porosity and permeability. No usable relationship has been demonstrated to exist for rocks of interest to us. The equation that contains both terms also contains a term which reflects the degree of connectedness of the pore spaces. This term cannot be defined for field situations; consequently the two parameters cannot be related except in very simple laboratory situations that are not analogous to our problem.

We suggest that several editing changes be made on page 27 of the paper. Line 2 of the first full paragraph would be clearer if the word 'interpreted' were inserted between the words "may be" and "incorrectly." We suggest further that the phrase "converted to heads at lower levels" be deleted from the sentence. We suggest in line 5 of the same paragraph that 'transmissivities' be inserted in front of "hydraulic conductivities" with the phrase "hydraulic conductivities" in parentheses. We suggest that in line 7 that "drawdown cone" be replaced with 'cone of depression'.

We suggest that in line 2 of the first paragraph (page 28) that "determination" be replaced with 'quantification'; we suggest that "hydrologic" be replaced with 'hydrogeologic'. These terms state more accurately the apparent intent of this sentence.

Paragraph 1 on page 28 states that lateral boundaries to ground water flow are commonly ignored in the determination of hydraulic properties from transient well tests. This statement is incorrect. Properly conducted multiple well tests accommodate boundaries by a technically proven procedure which facilitates the delineation of defensible hydraulic property values. This technical procedure is termed image well theory. The Jacob straight line procedure is one method that is used to interpret such boundaries. The document should be gleaned for misconceptions such as these and they should be removed. We do not have sufficient time in our review to be comprehensive in our identification of them.

We suggest that in the second paragraph on page 28 that the sentence beginning in line 4 be deleted. We are not sure what is meant by the statement that "required analytical assumptions are not readily quantifiable". We do not understand why the "analytical assumptions" should be "quantifiable." Perhaps "assumption" is being used in a manner that we do not understand; or perhaps the word "analytical" is used in a manner that we do not understand.

We suggest that the first line of the third paragraph on page 28 be modified to read: The spatial and temporal distribution of hydrogeologic field data are always less dense than desirable. This change incorporates the idea of distribution of data and it specifies that hydrogeologic data, not hydrologic data, are sparse. Hydrologic data generally are reasonably abundant. Hydrogeologic data are too expensive to be abundant.

Lines 11 through 14 on page 29 of the document should be deleted. This paper should not constitute a recommendation by the NRC staff on the selection of methods for interpolation of data by the DOE. Each method has its drawbacks in certain situations. Generally speaking, the greater the vertical and areal

heterogeneity of the geologic column the less successful will be the method, regardless of choice. In the initial stages subjective judgment is unavoidable. In any case the NRC staff should not tell the DOE how to interpolate sparse data.

We suggest that the word "node" be replaced with 'element' in the second line of the last paragraph. Nodes do not have a size.

We suggest that line 2 of the second paragraph on page 30 be modified to read 'characterization of the site'. We believe this phraseology is more appropriate because one does not characterize data. We suggest that the last line on this page be modified to read: 'Sites that have been tested with valid drill and test programs, based on defensible conceptual models, will facilitate the development of a more defensible GWTT distribution function.'

* See: Journel, A.G. and C.J. Huijbregts, 1978, Mining Geostatistics, Academic Press, N.Y., N.Y. and Henley, S., 1981, Nonparametric Statistics, Applied Science Publishers, Halsted Press, London, for discussion of stationarity.