see pocket for reviews

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RS-NMS-85-009
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U.S. Nuclear Regulatory Commission
Division of Waste Management
Geotechnical Branch
MS-623-SS
Washington, DC 20555

Attention: **Mr. Jeff Pohle, Project Officer**
**Technical Assistance in Hydrogeology - Project B (RS-NMS-85-009)**

Re: **Reviews of Williams and Associates' Papers on Uncertainty and Groundwater Travel Time**

Dear Mr. Pohle:

This cover letter transmits to the NRC staff the review comments of Nuclear Waste Consultants and its site teams on the papers "'Uncertainty' in Uncertainty Analysis of Groundwater Travel Times" and "Procedures for Predicting Groundwater Travel Time", both prepared by Williams and Associates.

The papers have been reviewed by Drs. Daniel Stephens and Jim Yeh of Daniel B. Stephens and Associates; Dr. David McWhorter and Mssrs. Lyle Davis and Tom Sniff of Water, Waste and Land; Mssrs. Michael Galloway and Fred Marinelli of Terra Therma; and Mssrs. Brown and Logsdon of NWC. Because of the emphasis that Dr. Williams placed on the geologic perspective of his paper on "'Uncertainty' in Uncertainty Analysis of Groundwater Travel Time", I also asked that the paper be reviewed by two geologists from our technical pool, Dr. James I. Drever of the University of Wyoming and Dr. Jonathan F. Callender of the New Mexico Museum of Natural History. Each of the reviews was performed independently and the differences are quite apparent. Some of the reviews are very detailed, and others are quite general. I have attached all reviews to this letter, which will serve as a cover letter and summary of the major comments of the reviewers. I am transmitting this letter and all attached reviews to Dr. Williams simultaneously with our transmittal to you.

## "'UNCERTAINTY' IN UNCERTAINTY ANALYSIS OF GROUNDWATER TRAVEL TIME"

The paper represents an early step in a much-needed, detailed assessment of the nature of uncertainty in not only groundwater travel time analyses, but all analyses associated with assessments of the performance of HLW repositories. Williams and Associates (W&A) are to be congratulated on taking the difficult first step, and NWC hopes that this first step and its evaluation can be the foundation of a steadily developing understanding of the nature of uncertainty, and perhaps more importantly, in developing methods by which the uncertainties can be identified, quantified, and, where necessary, reduced.



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U.S NUCLEAR REGULATORY COMMISSION
DIVISION OF WASTE MANAGEMENT

# REVIEWS OF WILLIAMS AND ASSOCIATES' PAPERS ON UNCERTAINTY AND GROUNDWATER TRAVEL TIME

TECHNICAL ASSISTANCE IN HYDROGEOLOGY
PROJECT B - ANALYSIS
RS-NMS-85-009

OCTOBER 3, 1986

The NRC has recognized for many years that the decisions on licensing of a geologic repository can and will be made under uncertainty:

> Proof of the future performance of the engineered barrier system and the geologic setting over time periods of many thousands of years is not to be had in the ordinary sense of the word. For such long-term objectives and criteria, what is required is reasonable assurance, making allowances for the time period, hazards, and uncertainties involved, that the outcome will be in conformance with these objectives and criteria.
> (10 CFR 60.101(a)(2))

The task confronting the NRC Staff is not how to eliminate uncertainty - a frankly impossible and unnecessary task - but rather how to deal with uncertainty in a manner that will permit the Staff and the Licensing Board to reach a finding of reasonable assurance that the public health and safety and the environment will be protected.

The W&A paper attributes sources of uncertainty in groundwater travel time to uncertainties in conceptual model, spatial variability of hydrogeologic properties, errors in data collection (including errors related to scale, sampling variation and measurement), and errors in computing. The bulk of the paper deals with approaches to dealing with spatial variability and the related concerns of "stochasticity", parameter variation, and randomness in flow path versus randomness in "hydrogeologic input coefficients".

NWC considers that the major categories cited by W&A do summarize the well-known major sources of uncertainty in hydrologic evaluations. However, the NWC reviewers find that the W&A paper so severely misinterprets the use of stochastic process mathematics to treat uncertainty in spatial distribution of hydrogeologic properties that the resulting conclusions are fundamentally in error. Equally importantly, the paper does not provide any particular help to the NRC in evaluating the nature and extent of uncertainty in the evaluations that are required.

The NWC reviewers cannot subscribe to the positions taken by W&A with respect to evaluating spatial variability. No one in quantitative hydrology has ever proposed that the properties are stochastic in the sense of varying in time. Quite obviously, at the scale of inquiry with which the NRC is concerned, the hydraulic properties are quantifiable characteristics of the porous medium. The problem is that this well-known situation is not helpful to the NRC, because the quantifiable characteristics are known at only a few places. Yet, in order to treat flow and transport from the repository to the accessible environment, the hydrologist must have a complete "model" of the distribution of the hydrogeologic properties of concern. In order to deal with the unknown values of the hydrogeologic properties in space between the points of measurement, all hydrologists must use some method of inference. Statistics is - and has been for many generations - the standard technical tool in science and engineering for dealing with uncertainty that derives from lack of "complete" knowledge. The formalism of stochastic-process mathematics can be used to treat a statistical model to characterize the parameters of interest. The quantity of concern is modeled by the mathematics in such a manner that the interrelationships between parameters impose consistency requirements on the field of input values, and thus limit the frequency and range of output values in subsequent performance modeling.

The regionalized variable approach is a method of modeling "persistence" in space. As is pointed out in the attached reviews, the method of conditional simulation produces a number of possible scenarios of what may be present at unsampled locations. Very importantly, both kriging and conditional simulation make full use of the data that have been measured: they preserve the known values at the locations of measurement.

The W&A discussion of randomness in path versus randomness in aquifer parameters is misdirected. The aquifer parameters are measured at selected spatial locations; the values of the aquifer properties at other locations are unknown. One can use statistical procedures to model the "degrees of belief" concerning what values may be present at the unsampled locations. Then, using conditional simulations, one identifies alternative versions of the aquifer that are internally consistent with the measured data, including the correlation structure. Using an analytical or more commonly a numerical model with each of the conditional simulations as input produces a range of possible behavior that may exist for the aquifer, based on the incomplete knowledge of the aquifer properties. The uncertainty in performance lies not in path randomness (nor in a truly random distribution of the hydrogeologic properties, which no one seriously considers to exist), but rather in the incompleteness of the data. For real hydrogeologic materials it is not helpful to say that "In a steady-state system, all subsequent particles entering at precisely the same $y_o$ will follow the same pathway." (W&A, p. 8). This statement would be true if and only if the system were completely known at a microscopic scale and also if one neglected diffusion, all mechanical interactions between the fluid particle and the solids along the flow path, and all energetic effects down to and including the level that induces Brownian motion. The existence of dispersion at all scales of potential significance to the NRC makes it clear that the W&A assertion is irrelevant and does not provide any support for their general case.

For all classes of uncertainty cited by W&A, the origin of the uncertainty lies in our lack of knowledge of the "true" conditions (due either to lack of measurements, errors in measured data or computed values, or lack of ability to analyze the system at the actual degree of complexity that exists in the field). In order for the NRC to evaluate the uncertainty inherent in DOE's evaluations of flow and transport, it seems likely that the only fruitful avenue is to develop a framework that allows the analyst to segment sources of uncertainty into manageable categories. Such a scheme is outlined in Section 3.2 of the attached review on the "uncertainty" paper from Nuclear Waste Consultants. Each category's contribution to the total uncertainty in the performance measure could then be evaluated, and the principal sources of the uncertainty could be identified. The uncertainty in the performance measure that derives from the uncertainty in the spatial distribution of hydrogeologic properties is one of these categories that can be analyzed quantitatively. NWC and all of its subcontractors consider that the use of statistical models of hydrologic systems exercised through stochastic-process mathematics is an acceptable approach to evaluating the impacts of that source of uncertainty.

Clearly, the NRC needs a technical position on uncertainty that will allow the Staff to perform the needed evaluations and provide useful guidance to the DOE on data collection and analysis. Such a technical position must be developed using a quality-assured and technically defensible approach, and we recommend that the Staff devlop such a position as a high priority activity for FY 87.

PROCEDURES FOR PREDICTING GROUNDWATER TRAVEL TIME

The paper on "Procedures for Predicting Groundwater Travel Time" "...is directed at the establishment of testing procedures and criteria for the two basic methodologies that are being employed for estimating groundwater travel time." (p. 3). The two methods are identified in the W&A paper as the "purely deterministic approach" and the "stochastic appproach". The paper goes on to discuss general procedures for field tests to determine hydraulic parameters, emphasizing the selection of the appropriate scale of testing. W&A suggests that large numbers of small-scale tests (a few meters or less) over the whole flow domain are needed for stochastic analyses, whereas a few, large-scale tests (hundreds of meters to kilometers) along the previously identified "fastest path of likely radionuclide travel" are recommended for deterministic models.

NWC reviewers have commented at length in the reviews of the previous paper on the W&A presentation of the nature and purpose of stochastic analyses, much of which was incorporated verbatim into the second paper. The generalized procedures for applying stochastic methods to a determination of groundwater travel time, described in Section 6 of this W&A paper, appear to be accurately presented. The NWC reviewers (particularly Stephens, Yeh and Brown/Logsdon) have a large number of specific comments and criticisms on the W&A interpretations of data requirements (and therefore testing requirements) for stochastic analyses. As with the W&A presentation of the nature and purpose of stochastic analyses in the "Uncertainty" paper, the NWC reviewers find the W&A treatment in this paper of details (e.g., scale and number of tests; demonstration of hydraulic continuity; determination of flow direction) so severely flawed that they consider that important conclusions drawn by the authors are incorrect.

In addition, NWC and its subcontractors disagree fundamentally and in detail with the W&A proposals for deterministic assessments based on their presentation of the capabilities of large-scale testing. We consider that the proposed testing strategy is confused and, in portions, incorrect, particularly as it could be applied to heterogeneous media. For example, W&A propose to use pre-emplacement heads to define a flow path, along which large-scale testing would then be conducted. This notion is so over-simplified as to risk producing an incorrect answer: physical flow does not necessarily occur directly down-gradient except for the idealized case of irrotational flow of a liquid of constant density and viscosity in homogenous, isotropic, isothermal materials that exhibit temporally stable geometries. The "fastest path of likely radionuclide transport" cannot be shown to meet these criteria: indeed, for any of the sites currently in consideration in this country (or anywhere else that NWC staff and contractors know of), the geologic repository system is expected to include flow of variable density and viscosity fluids through heterogeneous, anisotropic and nonisothermal materials. Even if the NRC Staff chose to ignore the clear implication of their own rule that the flow-path is to be defined for post-emplacement conditions and also permitted an isothermal analysis in lieu of addressing ambient temperature distributions, the heterogeneous and anisotropic nature of the rocks through which the fluids will flow needs to be addressed.

The computation of groundwater travel time requires (by definition) a knowledge of the pathway along which the water is flowing and the use of values (however derived) for hydraulic conductivity, effective porosity, and hydraulic gradient. Without performing a natural-gradient tracer test (which certainly at BWIP and probably at any other potential site is not practicable), NWC and its contractors consider that there is no practical and defensible way of predicting the flow path other than modeling the relevant portions of the flow domain. If this were to be done "deterministically", the hydraulic parameters would have to be identified "deterministically" throughout the flow domain. At this point, one re-enters the discussion previously presented on the use of statistical models and stochastic-process mathematics to support one's inferences about the spatial distribution of hydraulic parameters.

The NWC reviewers consider that the W&A paper does not sustain the unstated, but strongly implicit, argument that stochastic methods are inappropriate for the evaluation of the pre-emplacement groundwater travel time. Furthermore, the report does not address the feasibility of performing the proposed large-scale testing/deterministic analysis, and is, in significant portions, technically incorrect or incomplete concerning important aspects of practical and theoretical hydrology.

NWC considers that the W&A authors have a strong preference for the deterministic approach and limited experience of the actual use of stochastic approaches in geohydrology. Therefore, NWC recommends that an alternative view of the subjects be prepared by NWC, to allow different points of view to be incorporated into the NRC Staff's technical evaluations. You and I have discussed the preparation of an NWC position papaer at some length over the last several months, and we will now proceed to prepare such a document. Furthermore, NWC recommends that W&A revise the paper to address the general and specific comments in the attached reviews, and then that the revised W&A paper and the NWC paper (after NRC and W&A review) be used along with the current draft Generic Technical Position on Groundwater Travel Time to formulate a revised GTP.

Finally, NWC recommends that key members of the WMGT technical staff and of DWM management read the attached reviews in some detail. While we understand that this will involve a considerable investment of their time and energy, we consider it very important that key NRC staff and management understand that there are detailed, technical differences between W&A and NWC on these matters and not conclude on the basis of this cover letter alone that this is only another example of the difference in "philosophies" between the two groups.

If you have any questions concerning this letter or the attached individual reviews, please contact me immediately.

Respectfully submitted,
NUCLEAR WASTE CONSULTANTS, INC.

Mark J. Logsdon

Mark J. Logsdon, Project Manager

Att: Team reviews of Williams and Associates' papers

cc: US NRC - Director, NMSS (ATTN: PSB)
DWM (ATTN: Division Director) - 2
Mary Little, Contract Administrator
WMGT (ATTN: Branch Chief)
R. Codell, WMGT

Dr. Roy Williams, Williams and Associates

L. Davis, WWL
M. Galloway, TTI
J. Minier, DBS

10/3/86 reviews of Williams and Associates Papers on Uncertainty and Groundwater Travel Time

## 1.0 INTRODUCTORY INFORMATION

FILE NO:

DOCUMENT: "Uncertainty in Uncertainty Analysis of Groundwater Travel Time Analysis, by Williams and Associates, Draft dated August 11, 1986.

REVIEWERS: Adrian Brown and Mark Logsdon, Nuclear Waste Consultants

DATE REVIEW COMPLETED: September 26, 1986

DATE APPROVED: MJL 10/3/86

## 2.0 SUMMARY OF DOCUMENT AND REVIEW CONCLUSIONS

### 2.1 SUMMARY OF DOCUMENT

The paper prepared by Williams and Associates represents a beginning step in a needed detailed assessment of the nature of uncertainty in analyses associated with prediction of high level nuclear waste disposal system performance. This paper focuses on the relatively narrow, but fairly difficult topic of uncertainty in groundwater travel time.

The purpose of the paper is stated to be "...to elucidate (the) sources of uncertainty and to consider their interactions" (p. 1). The paper is "...intended for use as a baseline for interpreting analyses of uncertainty in groundwater travel time predictions performed by hydrogeologists studying possible high level waste repository sites" (p. 1). The paper provides a definition of uncertainty, a partial description of the nature and sources of uncertainty, and a critical and in general negative assessment of the uncertainties introduced into GWTT analyses by the use of stochastic procedures.

## 2.2 SUMMARY OF REVIEW CONCLUSIONS

In the opinion of the reviewers this paper leaves a number of issues either unaddressed, or unsatisfactorily addressed:

1. it does not appear to be particularly helpful in evaluating the nature or extent of uncertainty in the subject analyses.

2. it does not present a clear statement of what uncertainty is, or how it might be identified and where necessary reduced.

3. it does not appear to be comprehensive with respect to the genesis of uncertainties.

This review first looks at four specific issues which are raised in the evaluation, as it is believed that these are worthy of discussion by a reviewer, and secondly produces an outline of at least a conceptual statement about the nature of uncertainty as the reviewers see it, in the hope that this may assist in moving towards development of an NRC draft generic position on uncertainty.

## 2.3 SUMMARY OF RECOMMENDATIONS

It is recommended in Section 5 that the paper be revised, and used as a starting point to develop a NRC Generic Technical Position on the handling of uncertainty in the high level nuclear waste program. As a minimum, the revision should include evaluation of the literature on reliability and the statistical basis of the handling of uncertainty in other human safety situations, including nuclear reactors.

In addition, it is recommended that an alternate view of the subject be prepared by another group, to allow different points of view to be injected into the final position.

## 3.0 SIGNIFICANCE TO NRC WASTE MANAGEMENT PROGRAM

The regulatory rule under which any High Level Nuclear Waste Disposal Repository must be licensed (10 CFR 60) recognizes that uncertainty is inherent in all analyses performed by an applicant. It states that

> "Proof of the future performance of engineered barrier systems and the geologic setting over time periods of many hundreds or many thousands of years is not to be had in the ordinary sense of the word. For such long term objectives and criteria, what is required is reasonable assurance, making allowance for the time period, hazards, and uncertainties involved, that the outcome will be inconformance with those objectives and criteria."

Accordingly, the methods of accomodating uncertainty in the evaluations needed for licensing are of critical importance to the NRC.

The GWTT to the accessible environment is required to be "... at least 1000 years or such other travel time as may be approved by the Commission." (10 CFR 60.113 (a)(2)). In addition, the rule identifies a favorable siting criterion which is that the site exhibit "Pre waste-emplacement groundwater travel time along the fastest path of likely radionuclide travel from the disturbed zone to the accessible environment that substantially exceeds 1,000 years." (10 CFR 60.122 (b)(2)(iv)). Clearly for the NRC to be able to evaluate compliance with the requirement, and to evaluate whether the favorable siting condition exists, it is necessary to have defensible approaches to handling uncertainty in computation of GWTT, which is the stated purpose of the review paper.

## 4.0 DETAILED REVIEW COMMENTS (PROBLEMS, DEFICIENCIES AND LIMITATIONS)

### 4.1 ISSUES RAISED BY WILLIAMS AND ASSOCIATES

In no particular order of importance, the following four issues appear to have been raised by Williams and Associates, and warrant some discussion in review.

#### 4.1.1 Use of the term "parameter"

The Williams paper makes a strong case that the word "parameter" is incorrectly used by the groundwater community. They claim that a more appropriate word would be "coefficient". In support of this Williams and Associates present the Webster's Dictionary definition of "parameter", and it is stated that the use by groundwater hydrologists of the term "parameter" does not fit comfortably with this definition. The reviewers do not entirely agree with this assessment, as my reading of the definition as presented does include the usage of the term of "parameter" by groundwater hydrologists.

In order to further illuminate this the reviwers looked up the word "parameter" in the Oxford English Dictionary, reasoning that as the generators and originators of the language the English might have something to say on the subject. The definition is: "Quantity constant in case considered, but varying in different cases". This would appear to be exactly the usage of the technical community.

In addition, the use of the suggested replacement word "coefficient" does not appear to be appropriate. The Oxford definition of "coefficient" is "joint agent or factor; (Algebra) number placed before and multiplying another quantity known or unknown; (Physics) multiplier that measures some property (c. of friction, expansion, etc.)". Accordingly, the reviewers find it difficult to support its substitution for the commonly accepted term "parameter".

#### 4.1.2 Stochasticity of Spatial Hydrologic Variables

A significant portion of the total paper is taken up with the question as to whether hydrologic variables defined in space are indeed stochastic. The Williams and Associates paper concludes that they are not. While a definition of stochasticity is not provided in the paper, the reviewers agree that such parameters as hydraulic conductivity and porosity are not stochastic, at least in the Williams and Associates sense that they do not vary with time.

However, this is hardly relevant to the discussion on uncertainty. Stochastic methods have been used in groundwater hydrology to describe the variation of these parameters as a way of recognizing the lack of knowledge of investigators about the values of these parameters at points where they are not measured. Thus stochastic approaches are used as a way of accommodating and describing the uncertainty in these parameters, rather than there being any suggestion that these parameters are indeed stochastic (i.e. genuinely random in time) at any given point. This matter will be addressed again in the third section of this review as part of a general discussion of uncertainty.

The issue therefore is whether significant uncertainty is introduced into evaluations of groundwater travel time by the description of this uncertainty using stochastic methods. While the reviewers have not performed a detailed evaluation of the literature, nor of the direct question, they are not aware that any author argues that the use of stochastic approaches to recognize uncertainty in parameters in this way is inappropriate, or itself introduces significant uncertainty into analyses properly performed.

### 4.1.3 Parameter Variation as a Way of Identifying Uncertainty

Review of this matter is made more difficult for the reviewers because of the lack of clarity in Section 2.2.1 of the paper. However, the reviewers agree with Gutjahr that there is a difference between sensitivity analyses and uncertainty analyses, though this difference is (in the opinion of the reviewers) more in the nature of the uses of the results rather than necessarily in differences in analytical approach.

In the experience of the reviewers the main difficulty in utilizing partial-differential-based sensitivity analysis in identifying uncertainty stems from the use of these approaches when variations in parameters are very large. The analytical methods used in sensitivity analysis in general assume relatively small variations in parameters around a "best case" estimate. In the case of many matters associated with the high level nuclear waste program the variations about the so called "best case" are enormous, and can in no way be considered to be small perturbations. As a result most linear theories of perturbation fail in these circumstances. Accordingly it is considered that in concept the use of sensitivity analysis to assess uncertainty is acceptable, but in practice the methods generally used in such analyses are inappropriate when applied to uncertainty analyses under very large uncertainty. In fact large parameter variations have obliged analysts in the high level nuclear waste hydrology area to use stochastically-based parameter selection techniques with multiple realizations as a way of estimating the actual variation in the predicted quantities of interest.

#### 4.1.4 The Surrogate Argument

It is stated in the subject paper that "models currently in use... substitute randomness in the hydrogeologic input coefficients (parameters) for randomness in the flow path..." (page 8) and again on page 18 it is stated that modelers use "... coefficient (parameter) randomness as a surrogate for randomness in the water particle's pathway...". As noted above, in the opinion of the reviewers, modelers are in fact using stochasticity as a way of accommodating uncertainty. They do not appear to the reviewers to ever be implying that the parameter at any given specific location is in fact stochastic in the sense that it can vary at the time. The variation is over space and the variation is caused not by real variation in the parameter at any given point, but by lack of knowledge of the observer. Therefore the reviewers disagree with the premise and find it difficult as a result to agree that this is a major cause for uncertainty.

### 4.2 PHILOSOPHICAL OBSERVATIONS ABOUT UNCERTAINTY

In addition to the above specific comments, it is appropriate that the concerns expressed about completeness and about the sources of uncertainty be a little further illuminated here, so that it may be possible to move forward from this initial offering towards an accepted NRC generic technical position.

#### 4.2.1 Uncertainty in Repository Licensing

The licensing of facilities for the disposal of high level nuclear waste has always been considered to be an activity that requires prediction of future events under conditions of great uncertainty. As a result, the disposal system performance requirement (40 CFR 191) is couched in terms of probabilities of meeting certain release and other performance standards, and the licensing regulation (10 CFR 60) recognizes, through the multi-barrier concept, the uncertainty of any single containment feature. Further, it has been recognized that information about the repository area is difficult, expensive, and disruptive to obtain, and hence is expected to be relatively sparse at license time. Accordingly, it is clear that the task of considering a license application for a geologic repository for high level nuclear waste disposal is to make a supportable licensing finding under conditions of considerable uncertainty. This is unprecedented in nuclear facilities licensing.

4.2.2 Sources of Uncertainty in Groundwater Travel Time Evaluation

It would appear to the reviewers that ultimately the cause of uncertainty is lack of knowledge. One comes to this conclusion by considering that if one had absolute knowledge of all conditions, processes, and parameters one could, given absolute capability to analyse the available information, exactly define all future states of the system of interest. Put another way: if you know everything about a system, by definition you have no uncertainty about it. The direct conclusion therefore is that uncertainty stems from lack of knowledge.

A way to identify the areas from which uncertainty occurs in predictions of groundwater travel time is to examine what one would need to know to be able to perfectly analyse groundwater travel time (to eliminate completely any uncertainty in an evaluation of groundwater travel time). It appears that one would need to know at least the following in order to exactly identify groundwater travel time:

1. The physical processes of groundwater flow. These processes include such things as laminar flow, Darcy flow, etc.

2. The physical framework in which the flow processes are occurring. This is part of what is generally considered to be the conceptual model for the flow system of interest. Obviously perfect knowledge would be needed for all relevant flow paths in order to be able to calculate the flow along these paths.

3. Parameters which govern the flow processes. These parameters would include such things as the hydraulic conductivity, porosity and a variety of other parametric information.

4. Stimuli driving the system. Obviously if the groundwater is to move then there must be some energetic reason for it to do so.

5. Boundary conditions of the system. In order to understand the way in which the system works it is necessary to understand the boundary conditions, and how they change. Only in the circumstances of a completely open system is this requirement not necessary.

6. The initial conditions of the system. In order to make a prediction of future conditions one must start at some place in time. At the point at which one starts it is necessary to know the conditions of all of the stimuli, the boundary conditions, and of all of the variables.

In addition to having a perfect knowledge of at least the above matters, it would also be necessary to have a perfect method of performing the analyses which would produce the predictions. This implies that one has some algorithmic method of applying the parameters, the framework, the boundary conditions, the stimuli, and the initial conditions to the physical processes which are of relevance, and coming out with the resulting groundwater travel time.

In reality of course none of the above things are known to perfection. The physical processes which we use (particularly Darcy's law) are indeed approximations of far more complicated processes that occur on the macroscopic and microscopic level in groundwater flow systems. The physical framework in which groundwater flow systems occur are geologic systems of almost infinite complexity and variation at the microscopic level, but which do show significant uniformities when viewed at a macroscopic level. Most conceptual model approaches to groundwater hydrology try to identify those similarities and take advantage of them in simplifying analysis. Hydrogeologic parameters of rock groundwater systems show very large variations at the microscopic, and sometimes even in the macroscopic level and as a result are usually approximated by some statistical distribution in order for any reasonable computational evaluation of travel time. The stimuli, the boundary conditions, and the initial conditions are all also of great variability when viewed on a point by point basis, and generally require some simplification for manageable analysis. In each of these simplifications, uncertainty is introduced through either a lack of knowledge, or a lack of ability to analyse the system at the actual degree of complexity that exists in the field.

### 4.2.3 NRC Needs for Uncertainty Evaluation

In order for the NRC to understand the uncertainty inherent in the Department of Energy's evaluations of groundwater travel time it would appear necessary to develop a framework of the style suggested above which would allow a segmentation of the uncertainty into manageable categories. Each category's contribution to uncertainty could then be evaluated, and the principle sources of uncertainty identified in this way. It should be noted that uncertainty is not necessarily a bad thing. Providing that the range of uncertainty falls outside of the decision points which the NRC is obliged to judge the performance of repository systems, then the magnitude of the uncertainty is not of particular interest to a regulator. As a result, the elimination of uncertainty for its own sake is not an activity which is legitimately within the purview of the regulator. The only place where uncertainty becomes important to the regulator is where the uncertainty inherent in the analysis presented by the license applicant includes both sides of the regulatory decision point. Accordingly, the NRC needs to be able to identify the range of uncertainty in those analyses presented to it as a way of deciding whether or not the analysis falls in that uncomfortable range where the decision might be changed if the estimate of uncertainty is incorrect.

## 5.0 RECOMMENDATIONS

Clearly the NRC needs a technical position on uncertainty that allows the above evaluations to be made using a quality assured and defensible approach. Accordingly it is considered important for the NRC to receive scientifically supportable, generally acceptable input on the subject of the resolution of uncertainties in its evaluations, including groundwater travel time.

Therefore, it is suggested that this paper be revised, and used as a starting point to develop a NRC Generic Technical Position on computation of GWTT. In order for this paper to be useful as input to this GTP, it is recommended that the comments on the paper be considered in the revision, particularly with respect to the correction of those statements that are incorrect, the support of those statements that are presently unsupported, and the performance of, and inclusion in the text of, a comprehensive literature review of the subject area. This review should as a minimum include review of the literature on reliability and the statistical basis of the handling of uncertainty in other human safety situations, including nuclear reactors.

As it appears to the reviewers that there are other approaches to the subject of accomodation of uncertainty in geohydrologic analyses for support of HLW facility licensing, it is further recommended that an alternate view of the subject be prepared by another group, to allow different points of view to be injected into the final position.

## 1.0 INTRODUCTORY INFORMATION

FILE NO:

DOCUMENT: PROCEDURES FOR PREDICTING GROUNDWATER TRAVEL TIME, by Williams and Associates, Draft dated August 11, 1986.

REVIEWERS: Adrian Brown and Mark Logsdon, Nuclear Waste Consultants

DATE REVIEW COMPLETED: October 1, 1986

DATE APPROVED: mjz 10/3/86

## 2.0 SUMMARY OF DOCUMENT AND REVIEW CONCLUSIONS

### 2.1 SUMMARY OF DOCUMENT

The paper prepared by Williams and Associates "...is directed at the establishment of testing procedures and criteria for the two basic methodologies that are being employed for estimating groundwater travel time." (p. 3). The "two basic methods" are identified in the paper as the "purely deterministic approach" and the "stochastic approach". In addition, the report purpose includes a discussion of the "...testing techniques...for obtaining the necessary hydrogeologic data required for model verification and incorporation into the two prevalent methods of estimating groundwater travel time." (p. 3).

The document provides a discussion of the impacts of scale and conceptual model on the accuracy of the results of stochastic models, and appears to conclude that stochastic analytical approaches require small scale testing of parameters over the entire flow domain, while suggesting that deterministic analytical approaches require (or allow) large scale testing approaches along only the identified fastest path of likely radionuclide travel. The report concludes with an overview of stochastic approaches to computing groundwater travel time (GWTT).

## 2.2 SUMMARY OF REVIEW CONCLUSIONS

The report is considered by the reviewers to fail in its purpose. The terms "stochastic" and "deterministic" are not directly defined, and appear to change in meaning during the paper.

With respect to "stochastic" methods of computation of GWTT, it is the opinion of the reviewers that the paper does not adequately demonstrate the unstated but implicit argument that stochastic methods are inappropriate for the evaluation of GWTT. Further, the conclusion that stochastic methods require large numbers of small scale tests (which are likely at an inappropriate scale) is not found to be supported.

With respect to "deterministic" methods of computation of GWTT, it is not clear in the paper what these techniques involve. It is suggested in the paper that the approach requires the following steps:

1. Identification of the fastest path of likely radionuclide travel using detailed three dimensional head gradient information from the site.

2. Direct detailed measurement of required geohydrologic parameters (hydraulic conductivity and effective porosity) along only that path.

3. Computation of the exact GWTT using these values.

The report declines to discuss the feasibility of this approach (p. 26), which appears to be contrary to the stated purpose of the paper.

Accordingly, it is the finding of the reviewers that the paper does not make a significant contribution to the state of the art of evaluation of GWTT, and many of the assertions and opinions in the paper are not (and possibly cannot) be supported. Detailed criticism of the paper is included in Section 4 below for support of this finding.

## 2.3 SUMMARY OF RECOMMENDATIONS

It is recommended in Section 5 that the paper be revised, and used as a starting point to develop a NRC Generic Technical Position on computation of GWTT. In addition, it is recommended that an alternate view of the subject be prepared by another group, to allow different points of view to be injected into the final position.

## 3.0 SIGNIFICANCE TO NRC WASTE MANAGEMENT PROGRAM

The GWTT to the accessible environment is required to be "... at least 1000 years or such other travel time as may be approved by the Commission." (10 CFR 60.113 (a)(2)). In addition, the rule identifies a favorable siting criterion which is that the site exhibit "Pre waste-emplacement groundwater travel time along the fastest path of likely radionuclide travel from the disturbed zone to the accessible environment that substantially exceeds 1,000 years." (10 CFR 60.122 (b)(2)(iv)).

Clearly for the NRC to be able to evaluate compliance with the requirement, and to evaluate whether the favorable siting condition exists, it is necessary to have defensible approaches to computation of GWTT, which is the stated purpose of the review paper.

## 4.0 DETAILED REVIEW COMMENTS (PROBLEMS, DEFICIENCIES AND LIMITATIONS)

The following detailed comments are presented for the purpose of completeness. They are referenced by page/paragraph.

3/1 Acquisition of data sets does not in general "require the use of analytical or numerical models.", as stated in the paper. The interpretation of data sets obtained from testing may require such models, but even this may not be necessary, for example in the case of direct testing of heads, or direct measurement of time of transit of a tracer through a groundwater system. Analytical or numerical models are needed to interpret field data for prediction of conditions which differ from those in the field test; it has been found frequently useful to back analyze the test data to develop parameters of the system so that the test may be extrapolated to these different conditions of time, geometry, stimulus, boundary condition, or other factors.

3/3 It is questioned that the two approaches to predicting groundwater travel time are indeed fundamentally different. The difference is in the way that uncertainty in parameters is handled. In the deterministic analysis, the "best", or sometimes the "most conservative" value of each parameter is selected, and a single analysis is performed. This becomes more and more difficult as the number of parameters increase, as it becomes less and less obvious which values are reasonable or conservative without trying a few analyses. In the stochastic approach, statistically reasonable sets of parameters are selected and a deterministic analysis is performed for each. The result is described in terms of the distribution of the solutions produced by the analyses. The analyst then gets to choose the value from this distribution that is to become the "answer", if an answer must be obtained in this single form. It would seem to the reviewers that the stochastic approach is simply multiple repetitions of the deterministic approach, and is therefore fundamentally the same.

4/2 While it is in general agreed that "...hydrologic coefficients do not represent stochastic processes in a spatial hydrogeologic sense at our scale of interest...", it is noted that what is relevant is that the stochastic approach is utilized so as to represent the variability of hydrogeologic parameters for the purposes of analysis.

5/1 "The movement of elk in the Bitterroot Mountains ... is a legitimate stochastic process". This statement is true only to the extent that the observer lacks knowledge. If everything associated with the movement of the elk were known (which in theory it could be, for example by a

God-like entity), then by definition the future movements would be determinable, and thus not "legitimately" stochastic.

5/2 The paper to this point has concentrated on demonstrating that stochastic approaches are inappropriate in this context. If the authors of the paper believe their case, why do they devote a significant portion of the remainder of the paper to the data needs for an approach that is considered inappropriate?

6/2 The differences in the data needs for the two "different" approaches are not clear to the reviewers. In fact, it seems reasonable that the data needs should be independent of the method of analysis, if one is trying to evaluate some physical phenomenon to a required level of accuracy. In this case, using the description of the approaches presented in the comment labeled 3/3 above, it is reasonably clear that in both cases (if properly done) the same level of knowledge of the parameters and the conceptual model is required. The only real difference is that in the "deterministic" approach the choice of the "right" value of the parameter is made early in the process, reducing computational load, while in the "stochastic" approach the choice is made of the result, reducing the pressure on the analyst to estimate the effect of each parameter, but increasing his analytical burden.

6/3 The reviewers object to the use of the term "performance assessment scale" for a scale between basinwide and (presumably) local. The scale that is appropriate to an analysis is the scale of the problem at hand; in this case the problem is to compute the time it takes water to move from one point to another in a pre-emplacement groundwater flow system.

8/2 The statement "Observation wells ... may be installed in the confining units above and/or below the pumping unit in order to measure the vertical permeability of the confining layer." suggests to the reviewers that the author of the paper is under the incorrect impression that:

1. response in the confining layer is a "measurement" of vertical permeability (rather than a response that may be useable to indicate the magnitude of that parameter); and

2. vertical hydraulic conductivity cannot be estimated using the response of an observation well in the same horizon as is being pumped in the test.

Interestingly this latter view is reversed in the second paragraph of page 9, where the Neuman/Witherspoon method seems to be disavowed as a method of regional vertical hydraulic conductivity measurement. As it happens, the reviewers agree with the statements on page 9 with respect to <u>large scale</u> vertical hydraulic conductivity evaluation in layered media.

8/2 The statement "Significantly more data and hydrogeologic information are obtained from (large scale) tests due to he the larger volume of rock characterized." is not necessarily true. More pieces of data are in general gathered from a large number of small tests, rather than a single large one. Often the end result of a large scale test is a single set of bulked parameters, which sometimes troubles statisticians, as the single values do no constitute a population which they are used to utilizing. In fact BWIP have already stated that they need many large scale tests so that they may interpret the results statistically; the reviewers do not necessarily agree with their view.

8/2 The paper states that "Analytical or numerical techniques are available for testing anisotropic rocks, bounded aquifers, partially penetrating conditions, leaky aquifers and aquifers whose thickness vary in space.". This statement appears to the reviewers to suggest that one should limit tests to those activities where there exist accepted solutions to specific geometric conditions. The reviewers would define a geohydrologic test as any activity where the response to a hydrologic stimulus on a system is observed. Whether it can be analysed at all for parameters is hardly relevant. A prototype test of an aquifer (for example to demonstrate its yield) is an example of a perfectly acceptable test that requires no analysis (if it produces the water it is successful); there are many other examples. Fortunately for the geohydrological technical community, the great power of available analysis techniques render almost any carefully observed and recorded test analyzable for the parameters that determine system behavior.

9/2 The description of an appropriate method of evaluating large scale vertical hydraulic conductivity provided in this section is in agreement with the practical and theoretical experience of the reviewers. However it is considered that whether the method is "state of the art" or not is irrelevant. In the opinion of the reviewers it is more important that methods used in the program are where possible "state of the practice": effective for the purpose at hand; and generally acceptable to, and in common use by, the geohydrologic community.

10/1 "Effective thickness" (the product of effective porosity and aquifer thickness, or the total integrated thickness of voidspace for a specific thickness of aquifer) is not "synonymous with effective porosity" as stated in the paper.

10-11 The discussion of the measurement of porosity in the paper is considered to be inadequate. The discussion omits any reference to published works in the area of porosity measurement, and is not in any way comprehensive. The measurement of porosity is in the opinion of the reviewers a key problem area for the high level waste disposal program, and a thorough, well researched evaluation of the field as it applies to deep geologic media is required. The statement in the paper does not provide this.

11/3 The statement "Mechanical problems usually override hydrogeological problems in long term tests" is not in accord with the considerable long term testing experience of the reviewers. In fact the reviewers have found the reverse is true, if the identification of "non-standard" behavior of the groundwater system in the long term can be considered as a "problem".

11/4 The "low hydraulic gradients" at the BWIP RRL are close enough to zero that the direction of pre-emplacement groundwater flow from the RRL is now, and likely will remain, indeterminate. There is a serious question as to how long the NRC (or any other agency) should insist on BWIP waiting to obtain "the" pre-emplacement hydraulic gradient. Accordingly the statement made in the paper: "The direction of groundwater flow and hydraulic gradients should be measured before construction of the repository begins." is a class of statement which is unquestionably true, but is not of much use to an investigator without some guidance being given about the degree of precision needed for the purpose at hand. This has become painfully evident in the BWIP case, where the time needed for the (transient induced) change in gradient to become small with respect to the absolute magnitude of the gradient is very great, as a result of the very small (and possibly zero) absolute gradient.

12/1 The statement "Conditions of low hydraulic conductivity require long periods of recovery from induced transients" is theoretically incorrect. First, the magnitude of the impact of a transient (created for example by pumping a well) is a function (among other things) of the hydraulic conductivity of the medium in which the transient occurs. Thus in a low hydraulic conductivity material the transient will spread a small distance, and will approach a new equilibrium condition (absent other perturbations) in a time that bears a definable relation to the time over which the transient occurred. The same is true of a perturbation in a high conductivity medium. Second, given a perturbed situation, the time that it will take to revert to equilibrium (absent other perturbations) is a function of the hydraulic diffusivity or coefficient of consolidation of the system in which the perturbation occurs. This parameter is a function of the hydraulic conductivity and the storage characteristics of the material(s), and has the units of length squared/time. Thus it is incorrect to suggest that the time to approach equilibrium is simply a function of hydraulic conductivity.

12/2 The paper states that "Solute transport over 5 kms cannot be demonstrated on a 1000 year criteria (sic)". This is an unsupported statement. While it is true if the current hydraulic gradients (approaching $10^{-4}$) are maintained, it might well be possible to test much of the length of a selected pathway by increasing the gradient by injection and/or withdrawal. For example if the head were raised 250 meters at one end of the system and lowered 250 meters at the other, then the average gradient over 5 kilometers would be 0.1, and a tracer test that would take 1000 years at "natural" gradients would then be able to be done in 1 year.

13/1 The paper states that "The identification of the most likely flow path requires that the hydrogeologic system be understood to the extent that the hydraulic gradients can be defined in both the vertical and horizontal directions." This is an oversimplification. First, physical flow does not necessarily occur directly down-gradient, except for irrotational flow of a liquid of constant viscosity and density in isotropic, isothermal, materials exhibiting temporally stable geometries. As it is expected that the materials in which disposal of high level nuclear waste is presently contemplated will be anisotropic (perhaps highly so), will exhibit thermal and chemical gradients in the pre-emplacement period, and may react with the groundwaters themselves, even precise knowledge of the gradients alone is not adequate to define the flow path. Second, the use of the word "defined" indicates the fundamental problem with the so-called "deterministic" approach. As the experience at BWIP clearly demonstrates, it is impossible to "define" any geohydrologic quantity: at 1000 meters depth and in a highly confined rock aquifer environment even the precise absolute measurement of head has been found to be impossible. Thus one is immediately faced with the classical determinist's problem: which value do we choose when we do not know and will never know the "real" value. In the BWIP GWTT case, it is even difficult to pick a "conservative" value, and this difficulty is becoming more acute, rather than less, as time goes on, and the readings of head approach the same value. This kind of dilemma is what caused hydrologists (and other disciplines, for example weather forcasters) to use the tools of statistics, including stochastic approaches.

13/1 The deterministic (or any other) approach to <u>computation</u> of groundwater travel time requires (by definition) a knowledge of the pathway along which the water is flowing, and also requires the use of values (however arrived at) of hydraulic conductivity, effective porosity, and hydraulic gradient at all points along the length of that pathway. Without performing a tracer test under natural conditions, which at BWIP at least would appear to require the full 1000 years or more to complete, there appears to the reviewers to be no practical way of identifying the pathway other than modeling of the relevant portion of the flow domain. Thus the hydraulic parameters would need to be known throughout this domain in order to "deterministically" identify the pathway.

14/1 The paper states that "The treatment of the problem purely deterministically at the scale of 5 km <u>by definition</u> requires large scale tests." (emphasis added). While it is possible that large scale tests as defined in the paper are indeed needed for GWTT computation, it does not appear to the reviewers to be a definitional requirement.

14/2 The paper states that "(The stochastic) approach defines the distribution of vertical gradients and horizontal gradients at the site but not necessarily in any particular direction." This statement is incorrect: there is nothing in the general stochastic approach that requires this. Clifton, in his evaluation of GWTT for BWIP, chose to regard the direction of flow as indeterminate, because the data did not support any particular direction as being preferred at the RRL. In so doing he was using the information available to him. In the BWIP context, it indeed does appear difficult to rule out any flow direction from the RRL on head gradient grounds, due to the inherent uncertainty in the measurement of heads in this situation. He allowed his choice of parameters to define the gradients via modeling, and computed the GWTT along the path that resulted from that choice. This appears to the reviewers to be an internally consistent approach (the path selection is consistent with the parametric selection), and reflects the real uncertainties inherent in the knowledge of the actual situation.

15/2 The paper states that "...the statistical requirements of the (stochastic) approach guide the investigator toward the acquisition of large numbers of data points." While this is unquestionably what has happened in the BWIP case, it does not appear to the reviewers to be an inevitable consequence of the approach. Consider the situation where a large scale test has determined the average hydraulic conductivity of a flow top in the area from the repository location to the accessible environment to good accuracy (say +/- 20%). This value can still be used in a stochastic style analysis: most analysts would use a single value for the hydraulic conductivity at all points in the layer, and would probably vary this value stochastically within the measured accuracy range using some assumed probability distribution for the estimate of the mean hydraulic conductivity. As Clifton did in his work, where there is little variability in a parameter it is possible (desireable?) to leave it as a constant in the analysis, and concentrate on the items that have great uncertainty.

15/2 The statement "Stochastic theory in combination with applied hydrogeologic experience suggests that hydrogeologic coefficients measured analytically at different scales should not be combined as inputs to a stochastic model." This statement appears to fly in the face of normal scientific practice. It is certainly true that uncritical mixing of results of different tests can lead to erroneous results; however if results at different scales are different, identification of the reasons for such differences often provides considerable insight into the system. If the tests are properly performed, then the results are indicative of real behavior of the system, and should be used if appropriate in the predictive activities on which the licensing process is expected to depend. Due to the generally low density of information that is expected at license time, it seems inconceivable that any of the field information that is obtained will be ignored in the licensing procedures. Interestingly, the average hydraulic conductivities computed from the only large scale test available in the program (at BWIP) accords rather closely with the geometric mean of widely varying spot tests in the same horizon, suggesting that both sets of results could be used in a stochastic analysis without creating an internal conflict.

17/1 The paper states that "Faults, fracture zones, ... discontinuities, anticlines, synclines, facies changes and hydrostratigraphic unit pinchouts (facies changes) must be located for the determination of groundwater travel times because of the need for identifying the fastest most probable flow path." (emphasis added). If an advisor to the NRC makes such an absolute public statement, it should be supported with evidence that proves that the activities demanded indeed must be undertaken for licensing purposes. No such support is given for this statement.

19/1 The paper states that groundwater travel time evaluations will require specific testing (last sentence of paragraph). While peripheral to this paper, it is worthy of note that this implies that by requiring evaluation of groundwater travel time the NRC may be adding data needs to the Licensee's program. If it is ultimately agreed that the groundwater travel time measure is not related to human health and safety, then this implies that the GWTT requirement will have the effect of diverting resources away from the evaluations that are within the NRC's statutory mandate.

19/2 The discussion on the difference between the stochastic and deterministic method based on the lack of need and need (respectively) to demonstrate hydraulic continuity of flow paths appears to the reviewers to be spurious. In both cases a calculation of flow along a flow path is required. If it is known from test results or other factors that there can be no flow through a given area, then this fact can be included in either style of analysis. There is no requirement in a stochastic approach that all parameters be selected from a random pool: as is pointed out well in Section 6 of the paper, different zones of materials may be used (in this or any other analytical approach) to reflect different degrees of confidence in parameters, and different material types. Inclusion of some impermeable zones is entirely consistent with either approach. This concern reflects an apparent confusion on the part of the author(s) of the paper between stochastic representation of parametric uncertainty, and selection of a conceptual model for use in analysis.

20/1 The report states that "The purely deterministic approach assumes that equivalent porous media flow is appropriate for the medium of interest." This is incorrect as a generalization: it is true that this assumption is often made in any style of analysis, but it is not required by the selection of deterministic or stochastic approaches.

20/2 The paper states that "Smaller scale openings are dead ended and not reflected in the test results". In most rock masses openings of any size are not dead ended: thus this absolute statement is incorrect.

21/1 The report incorrectly states "data obtained from observation wells completed in the pumped hydrostratigraphic unit can be analyzed to yield a value of vertical hydraulic diffusivity...". What the analysis produces is the vertical leakance of the confining layer(s). With a knowledge of the layer geometry it is possible to compute a bulk effective vertical hydraulic conductivity. In order to obtain a vertical diffusivity, it would be necessary to measure a storage coefficient in the aquitard, which is not possible using only observation wells in the pumped layer.

21/2 Similarly, the completion of wells in the confining units allows a measure of hydraulic diffusivity of the confining units, not of the hydraulic conductivity as stated in the paper. This is usually computed by assuming (or independently measuring) a value of specific storage for the material.

21/3 The paper states that "Fault zones must be tested and incorporated along the fastest path in the purely deterministic method of predicting travel times." (emphasis added). Again, this imperative is not self evident, and if followed as a directive from the NRC to the licensee, would require a very extensive requirement for identifying and testing all fault zones. This might be actually unnecessary, for example if the licensee decided to take no credit for such zones in the GWTT estimate.

22/1 The paper implies that it is necessary for head testing to continue until "full recovery from perturbations to the system" is achieved (emphasis added). Again, this absolute requirement is impractical, and actually theoretically unachievable. As direction to a license applicant, it is necessary to indicate the extent to which it is required to approach the theoretical ideal. Even better, it is more useful to indicate how the NRC will evaluate whether an appropriate level has been reached.

22/1 The latter portion of this paragraph repeats the misapprehension of the authors about the relationship between the time to reach equilibrium and the hydraulic conductivity, which was discussed above.

23/1 The report states that "...effective porosity must be measured by the use of insitu tracer tests." (emphasis added). There are other techniques, including direct measurement from cores and geophysical techniques. It may be reasonable to say that insitu tracer tests are the best approach, but is not reasonable in this context to mandate the use of only one technique.

23/1 The report states that "Low hydraulic conductivities also make difficult the injection of the tracer and the removal of tracer from the observation well." While this is true, at least low hydraulic conductivities allow the gradient to be dramatically increased, which in turn dramatically reduces the time needed for the test. This may not be feasible in a high hydraulic conductivity material.

23/1 It appears self defeating to advance as a reason for the unfeasibility of truly large scale tracer tests that there is an "unknown distribution of heterogeneities at the site...". Surely the identification of these heterogeneities, or at least their effects, is critical to the evaluation of GWTT.

23/2 The report states that the need to demonstrate hydraulic continuity is only present for deterministic methods. This is untrue. If there is no hydraulic continuity, then there is no pathway. No matter how the analysis is performed, it is spurious if it passes water down non-existent pathways.

23/2 It is not clear to the reviewers, based on extensive large scale pump test experience, that hydraulic continuity is demonstrated by the test. The senior reviewer has had experience of a test where the heads on either side of a narrow lenticular clay layer varied almost identically, leaving the impression that there was continuity where it transpired that none existed. In the reviewers' opinion, this paragraph leaves an incorrect impression about the ease of demonstrating continuity.

24/2 The report states that "A large number of tests is required in order to obtain a defensible input distribution of the pertinent hydrogeologic coefficient". This is not true. There are a variety of methods of estimating the variability or uncertainty associated with a measurement or parameter, not all of them requiring large numbers of tests. For example, back analysis of a set of test results (the inverse technique) can be performed to find those ranges of parameters for which reasonable agreement between the observed and predicted response in the test can be achieved. These ranges can be used to define the remaining uncertainty in the parameters. Accordingly, the reviewers strongly disagree with the statement in the report that "Stochastic methods require a large number of values to establish a distribution for input to the model which is not practical to obtain with large scale tests; consequently small scale tests are necessary even though they may not reflect hydraulic properties at the scale of interest."

25/2 The paper states that "A stochastic method does not require that flow direction be determined by field data." This is untrue. The authors of the paper appear to consider that the stochastic modeling performed by Clifton for the BWIP GWTT defines what a stochastic modeling effort is and is not. Clifton <u>chose</u> to hold the gradient constant as he considered that the impact of the variability in the gradient was vanishingly small when compared with the much greater variability in the hydraulic conductivities that he was using. While this appears to the reviewers to be a reasonable decision on Clifton's part, it most certainly is not a "requirement" of all stochastic models.

25/3 The discussion of effective porosity essentially mimics the critique of hydraulic conductivity. Accordingly, the above comments may also be considered to be applicable to effective porosity.

26/1 The statement that "Hydraulic continuity does not have to be demonstrated with the stochastic approach..." is fundamentally incorrect. The statements made in the rest of the paragraph that suggest that small scale testing is required for the stochastic approach are also considered to be incorrect for the reasons noted above.

26/2 The paper spends most of the first 26 pages with a detailed and generally unfavorable critique of the stochastic approach to estimating GWTT, yet declines to provide a similar description and evaluation of the strengths and weaknesses of the deterministic method. As the purpose of the paper is stated to be "...the establishment of testing procedures and criteria for the two basic methodologies that are being employed for estimating groundwater travel time" (p. 3, para 2), this seems to defeat the purpose of the paper.

27/2 The paper states that in the case of direct (deterministic) evaluation of the mean and variance of the output variable, "...no assumptions are mandated about the shapes of the distributions for the input random variables." This is believed to be incorrect. Without a knowledge of the shapes of the distributions, it is not possible to reliably determine the shape of the output distribution, and the computation of the resulting variance is almost certainly spurious. This can be demonstrated by performing the same computation by using the same mean and variance for the parameters, but assuming first a normal, and second a log-normal distribution. The resulting distribution is generally dramatically different.

29/3 The paper states that in stochastic approaches, "...there is no capability for modeling multiple hydrogeologic units and the random flow paths that cross their boundaries." This is incorrect, as segmentation of the flow path into a number of portions is one way that this is done.

33/2 The paper states that "Whether or not the distribution of simulated values includes the real travel time can never be determined because of the spatial randomness of hydrogeologic data that serve as model inputs and because of the uncertainty about the validity of the conceptual model." The reviewers are of the opinion that the strength of stochastically based analyses is that they attempt to explicitly describe the uncertainties in the parameters and the flow paths, and to preserve this uncertainty throughout the analysis to the result. Correspondingly the inability of the deterministic methods (as the reviewers understand the use of the term in the majority of the paper) is seen as the great weakness of this method, which historically caused investigators to move to a more stochastically based approach.

33/2 The statement "At any rate, the generated cumulative frequency distribution curve does not represent all of the uncertainty inherent in groundwater travel time prediction." is in the reviewers' opinion not a useful observation. The question is whether the curve reasonably represents the uncertainty. While it may not in specific applications, it at least makes an attempt, which is not true of "deterministic" approaches as the term is used in this paper.

## 5.0 RECOMMENDATIONS

It is suggested that this paper be revised, and used as a starting point to develop a NRC Generic Technical Position on computation of GWTT. In order for this paper to be useful as input to this GTP, it is recommended that the comments on the paper be considered in the revision, particularly with respect to the correction of those statements that are incorrect, the support of those statements that are presently unsupported, and the performance of, and inclusion in the text of, a comprehensive literature review of the subject area.

As it appears to the reviewers that the authors of the reviewed paper have a strong preference for a deterministic approach, and very limited experience of the use of stochastic approach in geohydrology, it is further recommended that an alternate view of the subject be prepared by another group, to allow different points of view to be injected into the final position.

Water, Waste & Land, Inc.
CONSULTING ENGINEERS & SCIENTISTS

OCT 1 1986

September 28, 1986

Mr. Mark Logsdon
Nuclear Waste Consultants
8341 So. Sangre de Cristo Rd., Suite 6
Littleton, CO 80127

Dear Mark:

This letter is written in response to your request that we evaluate the following draft reports written by Williams and Associates (W&A):

<u>"Uncertainty" in Uncertainty Analysis of Groundwater Travel Times</u>

<u>Procedures for Predicting Groundwater Travel Time</u>

The members of the Water, Waste and Land, Inc., (WWL) staff who have reviewed the documents include Dr. David McWhorter, Mr. Tom Sniff, and myself. Because of the limited time for review and the draft status of the reports, we are limiting our comments to a general nature and have not provided any comments relating to specific areas of concern.

The primary response of all team members with regard to the two papers was that neither offered any new or innovative concepts or methods with regard to estimating travel time or in evaluating the uncertainty inherent in those travel time estimates. In general, the types of uncertainty enumerated in the first paper are generally acknowledged to exist by most hydrologists. In many instances, each element which adds to the uncertainty in a prediction or estimate may not be specified (such as the uncertainty due to errors in the conceptual model) in a paper or report. Nonetheless, most hydrogeologists recognize the inherent uncertainty in any prediction regarding groundwater. With respect to the second paper, the authors do not identify any new methods for predicting groundwater travel time. In fact, while some stochastic methods of estimating travel time are described, little is provided in the way of description of the deterministic models which are available.

The second general response of the WWL team was that it was difficult to determine the true objectives of the two reports. It was also difficult to ascertain what the conclusions of the authors were, therefore. This is particularly true of the paper regarding prediction of groundwater travel time. The cover letter for this paper seems to indicate that the authors have a higher regard for the deterministic type model than the stochastic approach.

Creekside Two Building, 2629 Redwing Road, Suite 200, Fort Collins, Colorado 80526 (303) 226-3535

Since conclusions are not reached as part of the papers themselves, it is not possible to insure that this is indeed the case. Nonetheless, it seems premature at the present time to disregard either as unacceptable. In the final analysis, it is likely that both will be important tools that hydrogeologists will have to use to convince themselves and others that the travel time criteria can be met. The utility of travel time as a valid evaluation criterion is also debatable in our opinion.

Finally, the WWL staff believes that it is difficult for us to assess much of the material presented since we tend to approach such problems from the deterministic standpoint. Therefore, without a formal detailed review of the documents in question along with review of the references cited, we are reluctant to criticize specific aspects of the two papers. Because we see little material which is significantly new to the hydrogeologic community, we therefore see the two papers essentially as opinions. If, on the other hand, these papers are to serve as the basis for important decisions which may be made with regard to the high level waste management program, we believe it is imperative that the papers be thoroughly reviewed and debated by the hydrogeologic community. To this end, we believe it is important to include the input and evaluation of stochastic modelers as well as deterministic modelers.

Hopefully, the above discussion meets your needs with regard to evaluation of these draft reports. While we defer to your evaluation as to the critical nature of these two papers with regard to uncertainty and stochastic hydrology, our basic opinion is that the papers are so general in nature that they provide no new insight into problems that must be addressed. If so desired, the WWL staff is prepared to develop a more thorough review of the papers including detailed reviews of the various references with which we are currently unfamiliar. We also plan to provide you with input regarding the NWC position paper which you spoke of in your project update memorandum of August 25, 1986. I wish to apologize for being late in returning these evaluations to you. Unfortunately, my vacation and the office move caused some things to 'slip through the cracks.' Hopefully, this has not inconvenienced you too much.

Sincerely,

WATER, WASTE AND LAND, INC.

Lyle A. Davis

Lyle A. Davis
Project Manager

LAD:kh

**TERRA THERMA, INC.**

**WATER CONSULTANTS AND ENGINEERS**

8341 S. Sangre de Cristo Rd., Suite 14, Littleton, CO 80127 (303) 973-7492

September 12, 1986

Mark Logsdon
Nuclear Waste Consultants
8341 S. Sangre de Cristo Rd
Littleton, Colorado 80127

**RE: Informal Review of Two Williams and Associates' Documents**

Dear Mark:

Please find attached copies of informal reviews of "Procedures for Predicting Groundwater Travel Time" and "Uncertainty in Uncertainty Analysis of Groundwater Travel Times" by Williams and Associates. As requested, we have kept the reviews informal, and therefore, to the point regarding our views. Although both Fred Marinelli and I have read both papers, we each concentrated on one specific paper, incorporating the comments of the other.

Our basic reaction to both papers is that, while being thought provoking at times, the papers do not offer any significant breakthroughs in the area of groundwater travel times. In fact, quite the opposite seems to be true. The philosophical approach of the Williams team would hinder successful resolution of the problems facing DOE and NRC in the area of groundwater travel time. Taken to its extreme, resolution seems impossible.

We have begun to think about what type of response may be appropriate and have started a list of ideas. We will talk with you in the next few days regarding possible approaches for a position paper.

If we can provide any additional information, please contact us.

Sincerely,
TERRA THERMA, INC.

Michael Galloway

Michael Galloway
Project Manager

TECHNICAL MEMORANDOM

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To: BWIP Team (TTI) | Date: September 11, 1986

From: Michael Galloway | Project: NWC (BWIP)

Re: Review of "Procedures for Predicting Groundwater Travel Time" by Williams and Associates

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## INTRODUCTION

Although the title indicates this paper presents procedures for predicting groundwater travel time, the primary contention of the Williams and Associates document is that the use of stochastic modeling will lead to incorrect results in calculating groundwater travel time at any HLW site. Although some limitations of deterministic methods are presented, the paper seems to emphasize why the stochastic method is inappropriate. In reaching this conclusion, several observations, logical arguments, and opinions are expressed. This review will discuss these points with respect to the soundness of the logical arguments and validity of the conclusions.

Although not specifically stated, the philosophical basis for much of Williams and Associates approach to the HLW program is apparent. The underlying theme of the paper is that there is one answer to every question and it is necessary to determine that one answer precisely, reducing uncertainty to an absolute minimum, which in some cases may be beyond the "state of the art". Even with "state of the art" testing techniques, it seems unlikely that any of the HLW sites could be characterized or satisfy the regulations, using this philospshy as a guiding basis.

## SPECIFIC COMMENTS

Page 3; Lines 1-13

The point is made in the introductory section regarding the model that is selected "determines the scale of the test (both in time and space)." The logical arguments supporting this statement are provided later in the paper. Therefore, this point will be discussed later in the review.

Pages 4 and 5

As a basis for much of the criticsm for stochastic methods, Williams and Associates contend that the natural distribution of hydrogeologic "coefficients" is not stochastic. In other other words, there can be a systematic distribution of "coefficients" in nature as a function of natural processes, rather than being randomly distributed. They conclude, therefore, that hydrogeology values cannot be predicted using stochastic methods.

From the geologic point of view of this reviewer, processes such as sedimentation, lithofication, igneous flow structures, etc., can result in systematic distribution of such hydrologic values such as hydraulic conductivity and effective porosity, rather than being purely random. However, stochastic methods are often used in groundwater hydrology where it is not possible or practical to define the systematic distributions of the natural system. Despite this shortcoming, calibration of stochastic models to real hydraulic responses of the real system (large scale stress testing) can provide what may be the only means of defining the hydrologic system within given limits.

An example from another discipline (physics) is the theory of light. In order to explain all the observed behavior of light, two distinct conceptual models are used, wave theory and particle theory. No one conceptual model at the present can successfully explain all behavior of light, so two models are used, which at times are conflicting. The need to use two models does not create a paralysis in the study and utilization of light, but rather provides a basis for further study and refinement of the theory.

Page 6 - 13

Much of the discussion of "scale" is a rather basic and general description of various testing techniques and to what extent (scale) the tests provide hydraulic information about the medium, in both time and space.

The difficulty this reviewer has with the scale discussion is the use of "scale" to define data sets which are only applicable to specific methods of analysis. Other than stated opinion, there does not seem to be any justification for limiting the use of different scales of data (testing) in either deterministic or stochastic models. Examples or scenarios can be given where both large and small scale tests can be used in either method of analysis.

If either data scale can be used in either method of analysis, then the conclusion that the method of analysis determines the scale of the testing is not valid.

Page 12; Lines 2-6

Williams and Associates note that drilling and testing activities have created perturbations on the system. The implication of this statement and the remaining paragraph suggest that ground water travel time cannot be calculated until all transient effects have recovered. This "requirement" does not seem practical for the BWIP site, considering the precision that is possible with respect to the very low gradients.

Page 13

Several statements in this section imply that only one answer is possible for a number of hydrologic questions. Practically, the constraints this philosophy places on site characterization would be terminal.

Page 13; Line 21

Although unjustified, this statement seems to preclude using bounding variables to solve given problems, which again implies there is only one answer to the flow path problem.

Page 15; Lines 20-25

Justification is not provided for the statement that data of different scales cannot be combined as inputs to a stochastic model. Also, evidence is not presented to support a related statement "Data sets derived at different scales reflect completely different characteristics for the same rocks." Even if this latter statement were always true, comparisons of data derived from different scales could yield correlations which might be useful in evaluating a given system. However, one could think of situations where this statement would not be true.

Page 19; Lines 2-5

This statement is another example of the underlying philosophy of this document. The question has not been asked as to the significance of hydraulic gradients (at BWIP), to ground water travel time, given the low vertical and horizontal gradients at BWIP. Rather than asking that question, the unsupported statement is made that "the hydraulic gradients must be measured to such an extent that the fastest path [singular] can be ascertained with reasonable certainty." Again, this approach would probably unnecessarily preclude characterization of the BWIP site.

TECHNICAL MEMORANDUM

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To: Mike Galloway (TTI) Date: September 11, 1986

From: Fred Marinelli (TTI) F.M. Project: NWC (BWIP)

Re: Review of "Uncertainty in Uncertainty Analysis of Groundwater Travel Times"

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## INTRODUCTION

This memorandum presents my informal review of the document entitled, "Uncertainty in Uncertainty Analysis of Groundwater Travel Times" by Williams and Associates, Inc. Because I am not a statistician, it is difficult for me to comment on certain theoretical aspects of stochastic analysis presented in the review document. I have focused instead on procedural issues associated with the HLW program, with emphasis on BWIP.

## INITIAL REACTION

The paper presents a detailed description of sources of uncertainty associated with stochastic modeling of ground water travel time. In deed, many of the concepts apply to hydrologic modeling in general and represent informal (qualitative) considerations used by hydrogeologists in routine investigations. I did find many aspects of the document to be thought provoking.

In their cover letter, Williams and Associates indicates that the technical content of their report is controversial and that the reaction of many individuals is likely to be negative. I personally do not find their paper to be as controversial or unique as the cover letter suggests. The text points out obvious and not so obvious sources of uncertainty in stochastic modeling that, as a hydrogeologist, I find reasonable. Perhaps, these concepts have not been considered by some of the more mathematical stochastic modelers to which Williams and Associates refers. I think a major limitation of the paper is that little attempt is made weigh the relative significance of one source of uncertainty over another. Also, practical methods for reducing the stated sources of uncertainty are not described.

## GENERAL COMMENTS

### Philosophical Approach

This document illustrates a basic difference between the philosophical approaches adopted by Williams and Associates and Terra Therma. The approach taken in the paper seems to be that there exists an absolute answer with regard to ground water travel time at BWIP and the goal of performance modeling is to determine that answer as precisely as possible. If significant uncertainties exist in the mechanics of a model, the end results can not be considered reliable (or useful). Thus, in order to have a reliable result, uncertainty of all factors must be reduced to a minimum level. This approach would continually ask the question, "what can be done to increase the knowledge of all model aspects in order to obtain the most accurate answer possible?". This is basically a "knowledge" oriented approach.

Terra Therma's approach to performance modeling is "goal" oriented. It is recognized that uncertainty is inherent in any mathematical calculation which attempts to simulate a physical process. However, relatively high levels of uncertainty can exist in many aspects of a performance model, provided that the question which the model addresses is either not sensitive to the uncertainties or can be answered using conservative scenarios. Terra Therma's approach continually asks the question, "is it important to know a particular aspect of the model in order to answer the question being posed?" The approach used by Terra Therma would not place a high priority on uncertainties associated with factors A and B, if it can be shown that the overall uncertainty of the end result is dominated by parameter C. Futhermore, the uncertainty associated with C would not be considered important if conservative values (e.g., upper/lower bounds) can appropriately be used to answer the question at hand.

### Physical Characteristics of a Model

A model of a physical process is used as a basis for making predictions. To this end, a model does not necessarily have to incorporate all the physical characteristics which actually exist in the system. An example of this in chemistry is the periodic table of elements. It is generally agreed that the model used to construct periodic table (atoms, electrons, rings, etc) bears little resemblance to the actual physical characteristics of matter. However, in spite of these inaccuracies, the periodic table is a very reliable predictor of certain aspects of chemical behavior. Likewise, a hydrologic model used for determining ground water travel time should not necessarily be judged by the degree to which the real aspects of the system are incorporated. The model should be judged instead on the accuracy with which it can be used to answer the questions being posed.

The review document places a high priority on the degree to which a model incorporates real aspects of the system (hydrogeology, parameter distributions, etc.). "Acceptability" of a model tends to be enhanced when it accurately incorporates known aspects of the real system. However, the "reliability" of a model rests primarily on its ability to make predictions. In many cases, a model may contain gross oversimplifications of the real system without jeopardizing its predictive capabilities. In my opinion, the review document overstates the need for a model to contain a precise representation of the real world.

Confidence of a model is enhanced when its prediction (based on a prescribed scenario) conforms closely with an observed event or realization. In this regard, the review document points out a very real problem with ground water travel time stochastic models. Because it is not feasible to conduct repository scale tracer tests at BWIP, there exists no formal basis for comparing modeling results with observed events. However, it might be possible to compare certain aspects of the model with observations. For example, the ability of the model to predict flux and potentials could be tested by comparison with large scale hydraulic stress tests. It might also be possible to make a comparison between simulated travel times and the movement of environmental tracers.

## SPECIFIC COMMENTS

### Page 3; Second Equation

The vector Yt is contained in both sides of the equation. It would seem that the left hand side should contain Yt and the right hand side should contain derivatives of Yt. [Since I have only a marginal knowledge of stochastic theory, I may be incorrect in this statement.]

### Page 5; Lines 8-9

Since repository scale tracer tests are not technically feasible, it is agreed that direct observations of ground water travel time are probably not possible at BWIP. However, it is possible to obtain field data which can be compared to particular aspects of the model, For example, the ability of the model to predict ground water flux and potential could be tested by comparing simulated results with observations made during LHS testing. In addition, movement of environmental tracers through the Columbia River Basalt may provide indirect field measurements of ground water velocity from which travel time can be inferred.

Page 5; Lines 21-22

A probabilistic distribution of head can be considered to exist throughout the media. However, this distribution is "conditioned" in such a way that the uncertainties in head are not generally as significant as other factors. For example, head potentials in saturated media are, by definition, continuous (i.e., do not contain step changes). Also, in regional systems without hydrologic boundaries, potential surfaces are generally smooth without abrupt changes in gradient.

Page 7; Lines 1-4

The text states that a property which is not produced by a stochastic process can not be a random variable. However, it seems possible that a variable can in fact contain stochastic "properties", even though it was produced by a systematic process. After all, the purpose of stochastic analysis is to form a basis for prediction in the absence of complete knowledge. If it is demonstrated that stochastic analysis provides a reliable basis for predicting the spatial distribution of a variable, it would seem that the method is valid regardless of the process by which the variable was produced. For example, in some studies, kriging has been very successful in predicting the spacial (conditioned) distribution of hydraulic head, even though hydraulic head is not produced by a random stochastic process.

Page 7; Lines 19-22

The text states that because hydrologic properties are fixed in space, they will have <u>no</u> probability associated with them under steady-state conditions. However, stochastic analysis is based on our "knowledge" of the properties of the system, rather than the exact values which exist in the system (but are unknown). In this regard, our "knowledge" of the system at a point can in fact be assigned a probability.

Page 8; Lines 12-13

The text states that in a steady-state system, all particles entering at the same point will follow the same pathway. This statement conflicts with dispersion theory which states that particles entering at a point will follow a distribution of (conditioned) random paths. This is demonstrated by mechanical dispersion of solutes originating from a point source.

Page 9; Lines 8-11

Refer to comment for Page 5; Lines 8-9.

Page 10; Lines 18-19

The text states that sensitivity of a particular parameter is dependent on the sensitivity of other parameters. However, a partial derivative is defined as the rate change of a variable, holding all other independent variables constant. Thus, the sensitivity of a parameter is dependent on the "fixed" values of the other parameters, rather than on their sensitivities.

Page 13; Lines 8-12

The text indicates that point measurements do not represent hydraulic properties on the scale of one to five kilometers. This might be interpreted to suggest that field measurements at this scale are required for performance modeling. However, for the purpose of stochastic modeling, assigned coefficient values need only be considered at a scale which is comparable to the dimensions of the model elements, not the size of the flow region under consideration. This requirement is much less restrictive than the implication made in the text.

Page 15; 12-18

In situations where uncertainty cannot be defined, the only recourse is to assume conservative (i.e., upper/lower bound) values, realizing the they do not yield "the" answer, but a "safe" answer.

Page 15; Line 15

It may not be totally accurate to state that coefficient uncertainty is used as a surrogate for pathway uncertainty. This is because the stochastic models being used at BWIP allow for (deterministic) changes in pathway as a result of parameter distributions.

Page 17; Lines 5-8

Refer to previous comment.

Page 17; Lines 14-16

Neglecting randomness is hydraulic head is probably not a bad assumption. This is because potentials, by definition, are continuous and in regional systems, head distributions are generally smooth.

Page 17; Lines 17-20

Stochastic modeling at BWIP does not have to assume that data is collected at a scale of 5 to 10 kilometers. It is rather assumed that the scale of measurements are comparable to the dimensions of model elements.

Page 19; Line 1

Would it be possible to incorporate the randomness in hydraulic coefficients <u>and</u> travel paths through the use of "particle pusher" model?

DANIEL B. STEPHENS & ASSOCIATES, INC.
CONSULTANTS IN GROUND-WATER HYDROLOGY

• GROUND-WATER CONTAMINATION • UNSATURATED ZONE INVESTIGATIONS • WATER SUPPLY DEVELOPMENT •

SEP 17 1986

September 15, 1986

Mr. Mark Logsdon
Nuclear Waste Consultants, Inc.
8341 S. Sngre de Cristo Raod
Littleton, CO 80127

Dear Mark:

Enclosed please find two reviews of each of the two articles by Williams and Associates which you requested on August 25. Let us know if you have any questions.

Kind regards.

Yours truly,

Daniel B Stephens cmc

Daniel B. Stephens, Ph.D.
President

DBS/cmc

Enclosures

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Document Reviewed: "Uncertainty" in uncertainty analysis of Ground-Water Travel Times, Williams and Associates, Inc.

Reviewed by: D. B. Stephens

Date Reviewed: September 10, 1986

Summary of Article: The article is a draft of a manuscript which is intended to present a useful explanation of sources of uncertainty which would serve as a reference document for interpreting analyses of uncertainty in ground-water travel time. The article defines uncertainty as it appears in a dictionary. It also attributes sources of uncertainty in ground-water travel time to errors in the conceptual model, spatial variability in hydrogeologic properties, errors in data collection (including errors related to scale, sampling variation, and measurement), and errors in computing.

Summary of Review: In this reviewer's opinion the article correctly identifies the principal sources of error and uncertainty. However, the article in its present form may not be particularly useful as a reference. To be a baseline or reference document, the article should stick to facts and definitions, and it should also report the conclusions of related research. There appears to be too much speculation on the part of the author of the article on topics related to scale of measurement, the random nature of fluid particle pathways, and the "stochasticity" of hydrogeologic properties, for example. The author cites very little of the scientific literature describing field studies of spatial variability, and he appears to lack an understanding of some important aspects of the stochastic approach, such as conditional simulation. This reviewer believes that the article incorrectly portrays the stochastic approach as one in which all the hydrogeologic parameters are assumed to be completely unknown and random. This leads the author to conclude that the true ground-water travel path will not be contained within the cumulative frequency distribution predicted by a stochastic approach.

The cover letter to Mr. Pohle contains misinformation in places and it gives the impression that this report bridges some gap between the complex mathematics of stochastic processes and geology. I found no equation of stochastic processes and no documentation which convinced me that the author had in fact "laid out for the first time the true relationship between the geologic environment and the treatment of that environment stochastically", and as advertised in the cover letter. Likewise the article does not seem to cover the advertised objective to address "the true meaning of the correlation structure relative to the realities of the spatial distribution of existing geologic environments". It also seems unfair to suggest in the cover letter that anyone who disagrees with the author's criticism of the stochastic approach, especially someone who has published in peer-reviewed, first-rate scientific publications, must be more like a mathematician than a geologist; and therefore their dissenting opinion should be categorically dismissed in favor of the author's viewpoint.

Detailed Comments:

1. Cover-letter page 1, para 2. I would like to call attention to the attached article published in the Soil Science Society of American Journal by Byers and Stephens (1983) which attempted to demonstrate that correlation length, a parameter used in stochastic analysis, was related to bed thickness, a geologic characteristic. I also believe the author overstates the importance of his paper as a contribution to developments in stochastic methods in hydrogeology. However, the article does correctly point to the need for additional work in relating the correlation structure to geologic fabric and in work needed to relate the scale of hydrogeologic testing to the scale of modeling in heterogeneous systems.

2. Text, page 1 line 10. Specify what confusion is reflected in DOE's FEA's and which documents are confusing.

3. Page 5, Section 2.2. It is not clear why the author refers to the "questionable" or "possible" stochasticity of hydrogeologic properties. The properties are in fact not stochastic, they are quantifiable characteristics of the porous medium. Usually the characteristics are measured at only a few places. Due to our lack of complete knowledge of the porous medium, the unknown hydrogeologic properties between measurement points may be inferred using stochastic methods. To apply the stochastic method, one needs to know or assume from the existing data base information regarding mean, variance, and correlation structure. The use of a stochastic approach does not preclude the retention of the measured parameters in realizations of data.

Such a stochastic approach is called conditional simulation. There does not have to be uncertainty or randomness at points of measurement when the stochastic method is applied. There is randomness however in the value of parameters one may "guess" in between points of measurement. In predicting values, the stochastic approach does take account of correlation structure; that is, the predicted parameter is likely to be large within a short distance of locations where the measured value of the parameter is large. At a distance greater than the correlation length the predicted parameter is not likely to be correlated with the measured parameter. This is considerably more realistic than assigning values on a purely random basis. In fact it is quite consistent with sedimentary geologic processes that lead to gradual facies changes, for example.

4. Page 8. para 5. Kriging is a technique which also utilizes the correlation structure of the system obtained from known points of measure using variogram analysis. Kriging in itself does not provide descriptive statistics. The Kriging approach leads to smooth interpolations between measured values, and it preserves the known values at the location of measurement.

5. Page 8, para 2 to page 10 and Page 18 last para. When the hydraulic properties are not known everywhere, obviously there is uncertainty in their values at locations between points of measurement. If we treat this uncertainty as a stochastic process, we can make many different predictions of ground-water

travel-time using the many realizations of possible data sets. Each data set may include all the measured parameters. In this sense, there is as much uncertainty in a data set used in a stochastic analysis as one might use in a single deterministic analysis to predict travel time. If the parameters are considered as random variables between points of measurement, then the flow path will be different for each realization. The assemblage of possible travel times compiled as a cumulative frequency diagram will have a variance which depends in part upon the correlation structure. The variance is a measure of uncertainty. With a deterministic approach, one would only have a single prediction of ground-water travel time. With the deterministic approach there would be no uncertainty in the prediction due to uncertainty in hydrogeologic parameters because the parameters in between points of measurement would be assigned as constant values within blocks of a numerical model based on geologic intuition or interpolation. Alternatively one could average the known measurements and assign a constant value to the entire domain. (If this approach is used regulators must be convinced that such a deterministic approach is extremely conservative). Given a sparse set of field data, there is no way to be certain that the true travel path is predicted by a single deterministic calculation; on the other hand, using a stochastic approach, it appears more likely to this reviewer that the true travel path will be included within the cumulative frequency distribution. In summary, only when the site is perfectly characterized will there be no uncertainty in travel path and travel time; at that time a deterministic analysis is all that is needed. Otherwise, if one treats the imperfectly known system as containing random variables between locations where the parameters have actually been measured in the field, then the flow paths will be random, but we expect these to vary statistically about the true mean.

6. Page 13, section 2.3. Stochastic methods have shown that uncertainty is attributed in part to correlation length of the hydrogeologic setting. The most important issue here seems to be related to determining whether the expected variability in this parameter is likely to be crucial to the prediction of ground-water travel time; and if so, then how should it be measured in the field. That is, what type of tests should be used, what areas or volume of porous medium should be represented by a single test in order to characterize this parameter, how many tests are needed for characterization, and is it practical to include as a site characterization activity. An investigation into this problem using stochastic analyses and numerical experiments prior to site characterization may provide useful and practical solutions.

7. Page 15, 3rd line from bottom. The author seems to have neglected uncertainty in heads along the boundary. Boundary conditions generate the drawing forces which are often critical to predicting ground-water travel path.

Document Reviewed: "Uncertainty" in Uncertainty analysis of groundwater travel time. by Williams and Associates, Inc.

Reviewer: Jim Yeh

Date of Review: September 5, 1986

GENERAL COMMENT;

It is this reviewer's opinion that the document being reviewed does not adequately address all possible sources of uncertainty in prediction of groundwater travel times. This document contains useless philosophical arguments on the stochastic representation of hydrogeologic properties. This document assesses the ability of stochastic analyses of uncertainty in prediction with no scientific basis but philosophical arguments which is irrelevant to the NRC rules and regulations regarding to groundwater travel time.

This document fails to point out the importance of the uncertainty in boundary conditions which are required for groundwater flow and transport models (the uncertainty in the types of boundaries, prescribed head or flux conditions, the uncertainty in the values assigned to the boundaries, and the uncertainty in the locations of the boundaries.) It also fails to mention the relative importance of each source of uncertainty in predictions which is relevant to the site characterization program.

DETAILED COMMENTS:

Page 6.

The philosophical debate on the coefficient and parameter is useless and irrelevant to the NRC. Does coefficient or parameter solve the uncertainty problem in groundwater travel time? Page 7.

The debate over the validity of stochastic representation of spatial variability of hydrologic properties is philosophical. The rationale for stochastic approach and the procedures are well summarized in recent hydrogeology literature (such as Freeze, 1975; Bakr et al., 1978, Gutjahr et al, 1978, Smith and Freeze, 1980, Simth and Schwartz, 1980, 1981a and b; Yeh et al., 1985 a, b,and c....etc.)

We agree that under steady state conditions the values of hydrogeologic properties are fixed at all points in space. In other words, the values are deterministic. However, The divergence in conceptualization of spatial variability occurs when we want to describe the spatial distribution of the property. For example, consider the variability of hydraulic conductivity values along a long core sample. Deterministic hydrologists may describe the spatial distribution of the hydraulic conductivity values by specifying the values at every point along the core (deterministic description). On the other hand, stochastic hydrologists may characterize the spatial distribution by using statistical parameters such as mean, autocovariance function, and probability distribution density function (stochastic description.) Obviously, any prediction of groundwater travel time along this core using the known values of hydrologic properties at every point (deterministic description) and physically correct mathematic models should provide an "exact" prediction. There is no need to use a stochastic description of the spatial distribution of the hydrologic property values. Thus, there is no need to use statistics and mathematical models to predict variability of groundwater travel time.

Unfortunately, to delineate the spatial distribution of hydrologic properties in an area with a radius 5 to 10 km, we encounter a problem; that is, can we take samples at one-inch intervals (or the scale of our measurement) in all directions at the site? If we can and the extensive sampling will not alter the nature of the geologic formation, then there is no need to use a stochastic description. For any practical purpose, we know such a detailed sampling program is impossible. In reality we might only be able to take samples at 100 locations, for example. We, then, may ask ourselves if these samples can characterize the fastest pathways in the entire geologic formation of our interests. What is the possibility that the fastest pathways exist at the unsampled locations? To answer these questions, one must resort to stochastic representation; that is, to "guess" all possible values of hydrologic properties at the unsampled locations. Certainly, the "guessing" dictates a stochastic representation of the spatial distribution of hydrologic properties. However, the stochastic description guesses the values at unsampled locations but utilizes the statistics (mean, autocovariance functions, and probability distribution) characterizing the spatial variability of the properties. At this moment, it should be clear that the spatial distribution of the property is a deterministic one, but it is conceptualized as a stochastic process due to our incomplete knowledge of the spatial distribution.

Page 9.

The argument on the substitution of random coefficients for random pathways is inappropriate.

Whether the pathway of a water particle is deterministic or stochastic is another philosophical argument which is irrelevant to the NRC. (We will not elaborate on this point.) To predict groundwater travel paths, groundwater hydrologists have to rely on Darcy's Law and continuity equation. Darcy's Law requires the hydraulic conductivity value and gradient for determining flux. With the knowledge of the porosity and flux, one can calculate the groundwater velocity which determines the groundwater paths (see Freeze and Cherry, 1972, or any groundwater textbook.) Thus, the pathway of a water particle is determined by the hydraulic conductivity and porosity, and gradient. If Darcy's law is correct, certainly the randomness of the pathway should be described by the randomness of the hydrologic properties and appropriate boundary conditions.

Page 10.

The variance of Yt should be $\sigma_a^2 + \sigma_b^2 t^2 + \sigma_\xi^2$ + covariance, instead of $\sigma_a^2 + \sigma_b^2 + \sigma_\xi^2$ + covariance.

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Document Reviewed: "Procedures for Predicting Ground-Water Travel-Time" by Williams and Associates, Inc., August 1986

Reviewed by: Daniel B. Stephens

Date Reviewed: September 12, 1986

Summary of Article:

The article presents two general approaches to predict ground-water travel-time: deterministic and stochastic. It also discusses general procedures to test for hydraulic parameters. Emphasis is placed on selecting the appropriate scale of testing. Large numbers of small-scale (a few meters or less) tests are suggested to be appropriate for stochastic analysis, whereas a few large scale (hundreds of meters to kilometers) tests are recommended for deterministic models.

In the deterministic approach, the article indicates that the flow path must be known from hydraulic gradient determinations before testing occurs. Large-scale testing for hydraulic conductivity and effective porosity is recommended along this path. According to the stochastic approach described, the flow path does not need to be known a priori. Numerous small scale tests provide statistical data needed for the stochastic analysis. Many realizations of possible hydraulic properties are used in a flow model to predict travel time and the uncertainty in the prediction is expressed as a cumulative frequency distribution.

Summary of Review:

The general procedures described section 6 in the article for stochastic analysis of ground-water travel time appear to be correct, although parts of the discussion about the nature of stochastic methods described in previous sections appears to be incorrect; these problems were addressed in my review of the companion article on uncertainty. However, I disagree with the proposed deterministic approach. The concept of scale of testing, and its relationship to deterministic and stochastic modeling, is confusing and probably incorrect as presented for heterogeneous media.

In lieu of specific comments, I will elaborate on the two problems with the deterministic approach and scale, because these topics are brought-up in several places within the article.

In a deterministic model, the parameters are prescribed throughout the system. There is no uncertainty assigned to the parameter, so there is no uncertainty in the predicted ground-water travel-time. The porous medium may be modeled as either homogeneous or heterogeneous. In the former case, hydraulic head data may be sufficient to map the expected flow path. In the homogeneous case, only one test location for hydraulic parameters is required to characterize the entire domain. On the other hand, if the domain is heterogeneous, we have two choices. Either to determine some effective average for the hydraulic properties and treat the domain as an equivalent homogeneous system or to model the system as heterogeneous. For the heterogeneous system with a sparse data base, local sub-areas of the domain around the locations of measurement may be treated as homogeneous, or the nature of heterogeneity between points of measurement may be inferred by interpolation, for example. The heterogeneous case is more difficult, inasmuch as it requires numerical modeling; however, it will produce a flow path which is more accurate. As more data are collected in the domain, the accuracy of the prediction in ground-water travel-time may be expected to improve, because the flow path is better defined. In a heterogeneous system with fixed head boundaries, the flow path is very sensitive to contrasts in hydraulic conductivity, as evidenced in elementary hydrogeology text books which describe the "law of tangents". The hydraulic gradient may be a very poor predictor of the direction of tracer movement (e.g. Frind et al, 1985), especially in heterogeneous systems. Therefore, this reviewer cannot support the recommendation to determine the flow path within the accessible environment prior to and independent of field tests for hydraulic conductivity. To the contrary, the flow path is strongly dependent upon the hydraulic conductivity distribution. The use of an effective hydraulic conductivity (some average over the range of conductivities) and the observed hydraulic gradient, as proposed in the article to determine ground-water travel-time, will not be conservative. In general, the radionuclides will follow the most permeable path to the accessible environment, and the hydraulic conductivity along most of this path will exceed that of the effective value of the hydraulic conductivity. Therefore, the deterministic approach described in the article is not recommended as a conservative alternative to an uncertainty analysis, as described in the NRC Draft Generic Technical Position on Ground Water Travel-Time.

El-Kadi and Brutsaert (1985) investigated the applicability of an effective hydraulic conductivity parameter to characterize transient flow in a heterogeneous aquifer, and they concluded that the appropriate effective conductivity is a function of the variability in the hydraulic conductivity and its correlation length. We infer from this study that determining an effective hydraulic conductivity may not be as simple as conducting a few aquifer pumping tests - the nature of spatial variability must also be known in order to reduce uncertainties in predicting ground-water travel-time.

The second major point of disagreement I have with the article pertains to the discussions of scale of testing. It is suggested that large scale aquifer pumping tests should be utilized to obtain the effective hydraulic conductivity for the deterministic models of flow in heterogeneous media. This reviewer is unaware of a procedure for uniquely interpreting aquifer pumping test results in heterogeneous media; all available solutions pertain to homogeneous systems. Assuming that the prescribed analytical procedure for analyzing the pumping test data are those developed by Theis, Hantush and Neuman, for example, at lease one observation well will be placed in the pumped aquifer. The article suggests that if there is drawdown due to pumping in the observation well, then there is hydraulic "connectivity" over this portion of the aquifer. Furthermore, the article suggests that the hydraulic properties derived from this test would be representative of an effective average at least over the area within the radius from the pumped well to the observation well (page 21, Middle paragraph).

It is this reviewer's opinion that the value of the hydraulic properties obtained from pumping tests in heterogeneous systems is strongly affected by the structure of the heterogeneity and by the properties in the immediate vicinity of the pumped aquifer. The introduction of lithologic heterogeneity into a conceptual model of the system which may also contain fault barriers, leakage, and water table conditions, will make interpretation of many aquifer pumping tests exceedingly difficult, and the parameters derived will be virtually impossible to defend. To obtain aquifer parameters in a heterogeneous system it may be more appropriate to conduct many small scale tests or short-term aquifer pumping tests using observation wells as close to the pumping well as possible, in order to minimize the effects of heterogeneity, in contrast to the recommendation in the article. Alternatively, a sufficient number of observation wells could be emplaced at varying distances from the pumped well to attempt to evaluate heterogeneity using an inverse procedure with a numerical flow model; however, such an approach is not commonly used in practise and may not produce unique results without prescribing some known values of the hydraulic parameters (from small scale tests) at a few locations. A pumping test which produces hydraulic parameters at many different locations would be extremely useful in evaluating the spatial heterogeneity structure, and the results may in fact lead to the selection of appropriate effective hydraulic properties. It is not clear from the article how heterogeneity will be evaluated by large scale multiple well tests.

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Frind, E.O., G.B. Matanga, and J.A. Cherry, 1985, The Dual Formulation of Flow for Contaminant Transport Transport Modeling 2. The Borden Aquifer, Water Resources Res., 21(2) 170-182.

El-Kadi, A. and W. Brutsaert, 1985, Applicability of Effective Parameters for Unsteady Flow in Nonuniform Aquifers, Water Resources Res. 21(2):183-198.

Document Reviewed: Procedures for Predicting Groundwater Travel Time by Williams and Associates, Inc.

Reviewer: Jim Yeh

Date of Review: September 5, 1986

## GENERAL COMMENT

This document discusses some deterministic and stochastic procedures for predicting groundwater travel time. However, most discussions of the procedures are not written clearly. No previous work is cited in the document. Discussion of hydrogeologic testing methodology is ambiguous and does not provide any specific recommendations. The advantages and disadvantages of various stochastic methods and their specific data needs are not mentioned. The discussion on the influence of the deterministic approach and the stochastic approach on testing is too general to be useful for establishing any guidelines for any site characterization program. Additionally, the document portrays the stochastic approach incorrectly.

## DETAILED COMMENTS

Page 4.

There is no need to debate if hydrologic properties should be called coefficients or parameters. The terminology is irrelevant to the NRC regulatory concern.

Page 5.

(1) The discussion on the stochastic representation of hydrogeologic properties is incorrect. We agree that at steady

state the hydrogeologic properties are fixed in time but vary from point to point; they will have no probability associated with them. However, the document fails to recognize the fact that it is not of practical interest to sample every part of the aquifer to completely characterize the spatial distribution of hydrogeologic properties. Because of this limitation, we have to "guess" or predict the hydrogeologic property values at unsampled locations in order to make predictions. Hence, the lack of data dictates a stochastic representation of the hydrogeologic properties. Stochastic representation does not predict the "future" values of hydrogeologic properties but those at unsampled locations with some estimated confidence levels.

(2) "We have pointed out in our paper on uncertainty that regionalized variables are being treated as random variables produced by stochastic processes in lieu of random flow pathways" is an incorrect statement.

Whether the pathway of a radionuclide is a random process or deterministic process depends upon our ability to describe it. If we know exactly the pathway a particle travels in space and time, then, the pathway is a deterministic process because there is no probability associated with it. In general, we do not have such information. Thus, to predict all possible paths that a radionuclide may travel, one must consider the path as a random process. In groundwater hydrology, it is well-known that the velocity (with direction and magnitude) of a water particle can be closely determined by employing Darcy's Law and the continuity equation. These equations require the knowiedge of the hydraulic conductivity and porosity values and associated boundary conditions. Therefore, the "random" pathway of the particle can be determined by knowledge of the "random" hydrogeologic properties with appropriate equations derived from a physical basis. Random paths determined by this approach certainly are not fictitious as mentioned in the document.

Page 8.

"Significantly more data and hydrogeologic information are obtained from this type test (i.e. multiple well testings) due to the large volume of rock characterized" is an incorrect statement. Such a large scale test does not necessarily provide us with more data and hydrogeologic information. Because the classical and common analysis of an aquifer test assumes that the aquifer is homogeneous and isotropic (anisotropy may be included in the analysis), the coefficients or parameters obtained from a large scale aquifer test, thus, represent nothing but average values of these parameters over a large area. Therefore, groundwater models based on these average values predict only the average or mean travel time for groundwater particles travel within such a large region. Certainly, this type of test does not provide any information pertaining to the "fastest path" (not the mean path) of likely radionuclide travel as stated in the NRC rules and regulations under CFR 60.113.

A large-scale trace test may be helpful for delineating the fastest path of likely radionuclide travel. However, this type of test is not practical as mentioned in the document (line 14, page 11, and lines 14 and 17 on page 12).

Page 14.

(1)"The treatment of the problem purely deterministically at the scale of 5 km by definition requires large scale tests." is another incorrect statement. First, what kind of large scale tests is the document referring to? Large scale tracer tests are impractical as mentioned in the document. Second, the large scale tests (aquifer tests) do not provide information pertaining to the fastest path as discussed previously.

(2) "This (stochastic) approach defines the distribution of vertical gradients and horizontal gradients at the site but not necessarily in any particular direction." is an incorrect statement. This may apply to the PTRACK model but not others. The PTRACK model does not conserve mass and is an incorrect model. In fact, most stochastic approaches define the distribution of hydrogeologic properties at a site, instead of vertical and horizontal gradients (see Smith and Schwartz, 1980, 1981 a and b).

Page 25.

The discussion on stochastic method again applies to the PTRACK model only and should not apply to other stochastic approaches such as Smith and Schwartz (1980, 1981a and b) and Clifton et al., 1985).

Page 28.

The section under the title "6.2 Linear Combination... Models" is not clearly written.

Page 30.

The definitions of stationarity and statistical homogeneity are incorrect (see Breiman, 1969; Jenkins and Watts, 1968; Lumley and Panofsky, 1964.)

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REC'D
SEP 1 8 1986

14 September 1986

Mark Logsdon
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Re: Review of Williams and Associates paper

Dear Mark:

In response to your letter of 26 August, I have reviewed this paper with the following in mind: is it quantitative, is it original, is it publishable, is it useful? The answers to all these questions is an unqualified NO.

<u>General comments</u>:
1. This paper is a philosophical exercise, not a quantitative treatment. The "mathematics" are simplistic and are used simply as a construct for a rather general discussion of what is wrong with the quantitative hydrogeologic treatments used by others.

2. Although sematics seems to be a critical issue in this paper, the authors don't have a complete grasp of English, particularly the word "vary" or "variation". These are both <u>time</u> terms, and do not properly refer to the description of the <u>range</u> of physical parameters that occur in geologic space. Over and over again, the authors refer to variations in parameters (or their "coefficients"); these parameters do not <u>vary</u> in space, they [may] vary in time. Rather, the values of the parameters <u>range</u> from place to place. See my notes on the manuscript for specifics.

3. The paper is a rehash of classical geologic criticism of hydrologic models couched in "quantitative" language. For example, removing all the verbiage, one of the "bottom lines" is that you can't measure hydrologic conductivity without many tests at many scales, a common geologic complaint. The authors do not discuss whether a reasonable range of test values allow one to prescribe reasonable limits to hydrologic parameters.

4. There is no geology here except a basic [and classical] geologic assumption: geology is so complex that the range of physical values for important parameters [which are not random values] is too great to model accurately. I don't happen to believe that we are that limited in modeling the real world.

5. The criticisms on pages 17-18 are oversimplified, typical of classical geology, and undocumented. They certainly need to be considered in any study of the ground-water system, but we all know that, and have for years. Nothing new has been added by this paper, including their only major insight--the recognition of random hydrogeologic coefficients in space and their use in flow-path analysis may not be equivalent to the results of stochastic flow modeling.

Specific comments:

1. See my comments in manuscript.

2. Paper does NOT identify in mathematical format all sources of uncertainty that are operative.

3. Paper does NOT lay out the true relationship between the geologic environment and the treatment of that environment stochastically. It hardly addresses this question other than philosophically, and no useful recommendations, solutions, approaches, and so forth are given.

4. The paper does NOT treat the spatial variability of hydrogeologic properties whose values are fixed at each point in space. In fact, the authors are confused about the relationship between fixed values in space which can range in the medium, and values which can vary with time.

Mark, this paper needs signficant work to become a paper with enough merit for publication or discussion. Please call me if you have further questions. I have attached an invoice.

Very truly yours,

Jon (J. F. Callender)

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September 5, 1986

SEP 12 1986

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Dear Mark:

"Uncertainty" in uncertainty analysis..." by Williams and Associates

Before commenting on the manuscript, let me make it clear that I am not a statistician. I consulted colleagues who work with stochastic modeling, and our overall conclusion was that this would be a stimulating paper for provoking discussion in the literature, but it is theoretically questionable and does not really lead anywhere in terms of policy issues. Some specific points:

1. Probability as "degrees of belief" concerning the unknown:

Consider a quantity $Q(x,y,z,t)$ that has values in space and time. Suppose measurements are made at arguments $(x_1,y_1,z_1,t_1)$, $(x_2,y_2,z_2,t_2)$, ....., $(x_n,y_n,z_n,t_n)$, that is

$$
\begin{aligned}
Q_1 &= Q(x_1,y_1,z_1,t_1) \\
Q_2 &= Q(x_2,y_2,z_2,t_2) \\
&\vdots \\
Q_n &= Q(x_n,y_n,z_n,t_n)
\end{aligned}
$$

are given values. Because games of chance typically pertain to uncertainty concerning future time, most people feel comfortable using probability to model the likely events that may happen in the future. However, they feel uncomfortable using statistics to predict values at other locations in space at the same time, because the quantities at the other locations are definite, co-existing "certain" values.

The significant point is that in a space-time continuum, there is no difference between translation in time and in space. One can only have degrees of belief concerning the value at any space-time location where there is no measurement. Statistics summarizes or "models" these "degrees of belief". The formalism of stochastic process mathematics can be used to manipulate the "degrees of belief" once they are expressed in a statistical model. If the quantity Q is modeled by differential equations in space and time, the interrelations impose consistency requirements on the field of values.

The "regionalized variable" approach is really just a way to model "persistence" in space (and in time if this is included). On can use the variogram to guide the "degrees of belief" for time-space locations not in the data set. Kriging is just one procedure for producing possible values at the unknown locations which are "reasonable" in a carefully defined way. Conditional simulation is probably more honest in that it produces a number of different possible "scenarios" of what may be present at other unsampled locations. The criticism of regionalized variables as pertaining to one realization of a random field is not appropriate, since in a space-time continuum what actually happens is always just one realization of the ensemble of what might have happened. Time and space are interchangeable in this context. An unknown measurement in space is the same as an unknown measurement in time.

In summary, random field mathematics just provides a framework for inserting one's "degrees of belief" into a selected model, and manipulating that input in a consistent way to the implied consequences.

2. Randomness in path versus randomness in aquifer parameters:

The aquifer properties are measured at selected spatial locations. The values of the aquifer parameters at other locations are unknown. Hence one can use statistical procedures to model the "degrees of belief" concerning what values may be present at the unsampled locations. Each conditional simulation of aquifer characteristics produces an alternate version of the aquifer which is internally consistent with the correlation structures and with the measured data. Running a numerical model with each of the simulations as input gives a range of possible behavior that may be present relative to the incomplete knowledge of the aquifer. The uncertainty lies not in the path randomness but in in the incompleteness of the data. Obviously the uncertainty can be reduced by a more detailed sampling of the aquifer characteristics, at least in so far as it is related to the random field variation.

3. "Determinism" of flow pathway:

I do not accept the statements in the second paragraph of p. 8: "In a steady-state system, all subsequent particles entering at precisely the same

$y_0$ will follow exactly the same pathway". This is almost equivalent to saying there is no such thing as dispersion. Given any mixing by thermal motion, the "particle" may follow any of several pathways through the medium. This is the basis of dispersion. It has nothing to do with stochastic vs. non-stochastic modeling.

4. <u>General comments</u>

The whole tone of the article is polemical and negative: "You guys don't know what you are doing and you're all wrong". I did not see any quantitative evaluation of the significance of these "errors", and I did not see any presentation of a better way of formulating the problem. The difference between a parameter and a coefficient was belabored to death. The final punch line "...this procedure need not be expected to contain the true groundwater travel times at all." is particularly argumentative. Any hydrologic model must extrapolate from a limited data set--say porosities and permeabilities at a limited number of locations--to the properties of the "aquifer" as a whole. Even if "correct" statistical formulations are used, there can be no guarantee that the properties of the aquifer in the unsampled areas are within the range used in the model. There is thus no guarantee that the true groundwater travel time would be within the range predicted by the model. Considering that the paper is written "...from the point of view of geologists who are experienced in the spatial variability of hydrogeologic properties...", I would have expected to see more emphasis on the spatial heterogeneity of geologic media, and the fact that the heterogeneity is non-random and hard to characterize statistically. I see this as a much greater problem than those discussed in the paper.

In summary, regardless of the correctness or incorrectness of the detailed statistical arguments, I don't see where this paper leads. Must all existing stochastic modeling be thrown out because of the theoretical flaws? If not, how much uncertainty does the "misformulation" introduce?

I hope all of this is of some use to you.

Best regards,

Tim

James I. Drever