

NUCLEAR WASTE CONSULTANTS INC.

155 South Madison Street, Suite 306
Denver, Colorado 80209-3014
(303) 399-9657 FAX (303) 399-9701

September 30, 1988

009/Tsk5/NWC.013
RS-NMS-85-009
Communication No. 287

U.S. Nuclear Regulatory Commission
Division of High-Level Waste Management
Technical Review Branch
OWFN - 4H3
Washington, DC 20555

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

Re: Task 5 - A Review of the Scale Problem and Applications of Stochastic
Methods to Determine Groundwater Travel Time and Path, by T.-C. Jim Yeh
and Daniel B. Stephens

Dear Mr. Pohle:

Attached please find the final version of the Task 5 report, "A Review of the Scale Problem and Applications of Stochastic Methods to Determine Groundwater Travel Time and Path", by Drs. T.-C. Jim Yeh and Daniel B. Stephens (Daniel B. Stephens and Associates). The report, prepared under the QA program of DBS, has been reviewed by M. Logsdon of Nuclear Waste Consultants and received an external technical review from Dr. Alan Gutjahr of New Mexico Institute of Mining and Technology. For your information, I am enclosing a copy of Dr. Gutjahr's review comments.

The report addresses the following items:

- o review of uncertainties in groundwater travel time and path that may be attributed to hydrogeologic parameters;
- o critical reviews of relevant scientific publications concerning uncertainties in travel time and path;
- o discussion of the significance of the finding for the NRC's waste management program;
- o explanations and illustrations of stochastic concepts and methods of stochastic analyses;
- o presentation of a glossary of terms relevant to stochastic analyses.

8810110184 880930
PDR WMRES EECNWC
D-1021 PDC

1/2
D/D21
NH14
Wm-11

The final report has been reorganized and supplemented in the manner described in NWC Communication No. 270, in which we transmitted the first draft of the report. NWC considers that the revisions have made the entire work a much better focused and more easily used report. In particular, the front material sets the framework for the analysis that follows, new illustrations make the concepts much more accessible, and the detailed mathematical derivations of the earlier version's Section 2 have been moved to an appendix. The bibliography contains almost 400 citations to the literature on geostatistical and stochastic methods in groundwater hydrology.

Most readers at the NRC, after looking at the abstract and the front material, will probably turn to Section 3, Groundwater Travel Times and Paths. Note that there is material pertaining to groundwater travel time and paths throughout the text, not only in this section. After reviewing the text, NWC considers that you will find that Drs. Yeh and Stephens have reached essentially the same conclusions that have been reached by the CNWRA Program Architecture working group on GWTT: there are ambiguities in the wording of the current version of the performance objective (10 CFR 60.113 (a) (2)) that need further consideration. Yeh and Stephens propose a technical alternative to the current form of the rule, based on the travel time of a certain concentration of a hypothetical tracer released under pre-emplacment conditions. This technical alternative is not one that has been considered so far by the CNWRA group, but it is conceptually similar to aspects of the analysis originally prepared by Dr. Richard Codell of the NRC (Draft Generic Position on Groundwater Travel Time, June 30, 1986). Yeh and Stephens present the arguments for why the transport analysis is a technically sound surrogate for the performance of the natural system. It should be noted that the analysis could still be relatively simple (at least compared to analyses for the overall system performance), since the analysis would be performed under isothermal (or nearly so) conditions and could be limited to an ideally conservative tracer to eliminate geochemical complications. This analysis would allow consideration of such physical characteristics of the flow system as the applicant could document are a functional part of the natural barrier, which is after all, what is to be shown. For example, if the applicant could demonstrate that dead-end pore spaces in a dual-porosity system were effective at isolating radionuclides under pre-emplacment conditions, then this would seem a legitimate part of the credit that they might wish to claim. Their burden would be to demonstrate that the model they invoke and the data they use are reasonable for the site. This is no different as a matter of proof than would be the burden to demonstrate a representative value for effective porosity in a one-dimensional seepage-velocity analysis. Reasonable technical people could disagree with the Yeh/Stephens analysis, and there may well be policy reasons for which this approach would be found unsatisfactory. But NWC considers that Yeh and Stephens have presented their position clearly and that it is one which should receive consideration.

Transmittal of this report completes the deliverable for Task 5 under the August 14, 1987 direction of the NRC Project Officer. If you have any questions about this letter or about the report by Drs. Yeh and Stephens, please contact me immediately.

Respectfully submitted,
NUCLEAR WASTE CONSULTANTS, INC.



Mark J. Logsdon, Project Manager

Att: A Review of the Scale Problem and Applications of Stochastic Methods to Determine Groundwater Travel Time and Path, by T.-C. Jim Yeh and Daniel B. Stephens

cc: US NRC - Director, NMSS (ATTN PSB)
HLWM (ATTN Division Director)
Edna Knox, Contract Administrator (Documentation only)
HLTR (ATTN Branch Chief)
R. Codell, HLOB
D. Chery, HLTR

CNRA - Dr. John Russell

L. Davis, WWL

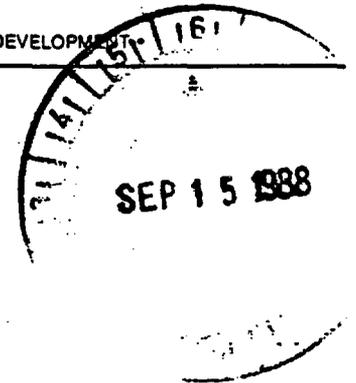
bc: J. Minier, DBS



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

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

September 12, 1988



Mr. Mark Logsdon
Nuclear Waste Consultants
155 South Madison #302
Denver, CO 80209

Dear Mark:

Enclosed please find a copy of Al Gutjahr's review of our NRC - stochastic report. Although his review is complementary, we want to take the time to polish the report by incorporating his suggestions. I discussed the extent of work with Jim Yeh, and we hope to have a final draft in our office by Tuesday September 20. You should have a bound copy for your review and distribution by about September 23.

Kind regards.

Yours truly,

Daniel B. Stephens & Associates, Inc.

Daniel B. Stephens, Ph.D.
President

DBS/vo
Enclosure

File: Logsdon.912
Disk: 85-130

September 6, 1988

Eelen, N.M.

Daniel B. Stephens & Associates, Inc.
Socorro, N.M. 87801

Dear Dan,

Attached is my report on Jim Yeh's opus! I have indicated several corrections and suggestions that I believe will improve the document. I hope you and Jim find my comments useful-- please call me if you have any questions about them. I would like very much to receive a copy of the final copy of your document when it is finished. My compliments to Jim and you-- I think it does a very good job in addressing the travel time issues and in discussing the stochastic approach.

Sincerely,

Allan
Allan Gutjahr

REVIEW OF
THE SCALE PROBLEM IN GROUNDWATER TRAVEL TIME AND PATH

By T.-C. Jim Yeh

The document was reviewed for clarity, for accuracy with special reference to the probability and stochastic content, and for appropriateness to the audience. Overall, I thought this was a well-written, well-thought out approach. The examples used to illustrate the points were especially well chosen and the sections on "Basic Concepts" and "Groundwater Travel Times and Paths" were very well done.

I do have some specific comments below which I believe would strengthen the document and clarify some issues. In addition, I have made some suggestions (in red) on the manuscript which I think might help the flow of the narrative in several places.

Comments on suggestions made in the text:

p. 1.3. I would say the heterogeneity observed is related to the scale problem rather than a direct consequence - there is no cause/effect relationship here. Also I would move that sentence to one end of the paragraph.

CHAPTER 2: Specific comments are attached. I thought this was a very good chapter that helped set the stage for the rest of the document.

p. 2.2. Distinguish between approach and continuum assumption.

p. 2.7. The weighting function $f(\eta)$ used within the REV concept is a rather special one: namely one where

$$f(\eta) = \begin{cases} 1/V, & \eta \text{ is volume } V \\ 0 & , \text{ otherwise.} \end{cases}$$

I think it would help to specify that clearly in that manner. It also might be interesting to discuss the REV in the more general framework where $f(\eta)$ could be more complicated - eg. a function like

$$f(\eta) = c_1 e^{-\eta^2 c_2}, \quad c_1, c_2 \text{ constants. (Though that may}$$

cause more confusion than clarity for the intended audience.)

p. 2.15. Reword the second sentence: "If the head at the interface is not measured...."

p. 2.16. I would emphasize that the value of K is estimated at the bottom of the page.

p. 2.17. (center of page) Note if the heads are kept constant over the cross-section, then the average of the values is that constant. It seems you really want to emphasize that the head value is an average over the cross-section and not that it is the same at each point in the cross-section. I think the two sentences should be combined. Could you just say: "The average heads over the cross-section at the ends of the columns are kept constant."

p. 2.19. The use of the term "center of mass" in the discussion of tracer-arrival times can lead to confusion. I understand that the reference is to the center of mass of the probability distribution - the center of mass of the particles, however, is really a point in 3-space. I think it would be sufficient to just say "the average arrival time" and omit the words center of mass which may confuse more than clarify (also see Figure 2.6).

p. 2.29. Note that Δ in the middle of the page is now a two-dimensional operator. Also the equation

$$\Delta \times (T\Delta H) = \zeta \frac{2H}{2t}$$

doesn't follow directly from averaging the 3-d equations. Namely, the average of the products is not the product of the averages. (Later reference is made to that fact on page 3.7 and so additional "error" is introduced in that derivation or once again uses an "equivalent" T. I think that should be discussed here.)

p. 2.31. Reference is made to ensemble averaged before that concept is explored or without reference to its later discussion.

p. 2.39. Drop the phrase: "because of the ergodicity assumption." It isn't really due to that assumption - it's essentially just the fact that single realization results can vary from mean results.

p. 2.40. The statement on stationarity is more confusing than clarifying - I suggest it be omitted here since stationarity is discussed later. Also $E(V^2)$ is not the variance unless $E(V)=0$: the fact that $E(V)=0$ is not introduced until later. Either introduce it earlier (see p. 2.42 where you state $E(V)=0$ and also why) or introduce the general definition of

$$\text{var}(V) = E[V-E(V)]^2$$

(For the purposes of this discussion, I'd suggest introducing $E(V)=0$).

p. 2.42. Equation 2.6.7: upper limit should depend on $\lambda+t$ for the discussion that follows - also you may want to use a different variable of integration (say ξ). As it stands, the change of D_m with time is not shown.

p. 2.43. Note the exponential form (2.6.8) is assumed; it is not implied by the discussion.

Figure 2.12c. not shown as bell-shaped, as referred to in the text on p. 2.45.

p. 2.53. Line 3 - head gradient should be .01 not 80.01.

CHAPTER 3: Again, I have very few comments. Initially there was strong conviction that the travel time criteria, as defined by NRC regulations was not viable. However, this resolve seemed to weaken as the chapter progressed. I thought the initial points made regarding travel times and the refocusing on exceedence of mass flux in a specified area were well taken and you should "stick-by-your-guns" on that issue. There is not much specific that I can point out here but just a feeling of weakening of that position as I read through this chapter. Perhaps a reiteration of the points at the end might be useful. Again the discussion of uncertainties is good and needed. However, it may overshadow the travel-time argument and so a repetition at the end of the chapter could be useful.

p. 3.15. At the bottom of the page - I don't think that even in the uncorrelated or statistically isotropic case that the observed drawdown represents the average drawdown. I think you could reword those sentences and drop that reference.

CHAPTER 4: I have several suggestions to make here - essentially they involve tightening up the probabilistic discussion a bit while still retaining the informal analogies. Specifically, some definitions and terms need to be more precise as discussed below.

p. 4.6. I would say the hydraulic properties of the aquifer are viewed as random variables.

p. 4.7. (8th line down) - I would add something like: Namely, it is not completely predictable: only probabilities can be associated with the possible values and hence the values are considered as realization of a random variable.

(2nd paragraph) - I would say the properties are taken to be log-normal or normal.

p. 4.8. (bottom of page) - Note that in any particular case, however, we only want the specific values for that case. We may still model this as a stochastic process (representing our lack of knowledge, for example) though we also may wish to condition the results with data.

p. 4.10. The discussion on stochastic processes starting on line 7 is not very clear. First, I would start a new paragraph with the current line (line 7 etc.): "For example.....one possible outcome of a stochastic process." In a stochastic process the value of the quantity, say conductivity, $k(x)$, is a random

variable for each location x. Namely, if conductivity is observed at locations x_1, x_2, \dots, x_n then $k(x_1)$ is a random variable, $k(x_2)$ another random variable, and so on out to $k(x_n)$. Each has a probability distribution and furthermore the probability distributions may be interrelated. The chance of finding a particular sequence.....locations. (See bottom of p. 4.10) now add sentence from 4.10, "This implies.....(Figure 4.3)".

(new paragraph see p. 4.13)

In order to determine..... must be known. The joint distribution is completely defined only if the probabilities associated with all possible sequence of $K(x_i)$ values along the transect are known. (omit "However....up to equation 4.1.1) (omit 1st sentence, next paragraph - Generally speaking.... and add the second and third sentence to the part ending "stationarity and ergodicity." to the same paragraph.

Now start a new paragraph:

"Stationarity....constant in space. (Reword the sentence on ergodicity - eg. combine with next sentence, say something like: Ergodicity means that by observing the spatial variation of a single realization...

p. 4.14. don't couple the second order stationarity to ergodicity - this makes it sound as if second-order stationarity was a consequence, rather than an assumption. I would instead suggest that line 12, etc., be replaced by: "Because it is difficult to obtain all of the joint distributional information required and because in many cases important properties are assessed by moments like the mean, variance or other lower order moments, an assumption of weak or second order stationarity is often involved." The first moment.....

$$\mu = E(K)$$

The autocovariance or covariance function is then defined as

$$C(\xi) = \text{cov}(K(x+\xi), K(x)) E[(K(x+\xi) - \mu)(K(x) - \mu)]$$

The second-order stationarity assumptions are that (i) $\mu = E(K)$ is constant and (ii) $\text{cov}(K(x+\xi), K(x))$ only depends on ξ , where $\xi \dots$

The example of the sine series is a good one though it should be noted this is a strictly deterministic function.

p. 4.20. (top) - you might want to add a reference to MacMillan & Gutjahr (198) where an attempt is made to relate correlation lengths to geological fractures.

p. 4.23. I would add a statement on non-stationarity in covariance (which can at times be more restrictual than changes in the mean).

lines 12 and 13 add: "uncorrelated or statistically independent from one point to another. Also in the next sentence, indicate that the covariance presumably depends only on separation distance.

p. 4.29. I would slightly reword the sentences on random functions and regionalized variables to first introduce random functions (indicating equivalence to stochastic processes) and regionalized variables (as realizations of stochastic processes).

p. 4.30. Geostatistics really consists of more than two parts (eg. I would include conditional simulations, etc.). I understand what you are trying to get at but it would be better to reword that paragraph a bit. For example, you might say: Two important parts of geostatistics are (1) identification of the spatial structure of the variable (variogram estimation, trend estimation, etc.) and (2) interpolation or estimating the value of a spatially distributed variable from neighboring values taking into account the spatial structure of the variable (Davis, 1973)

In equation 4.2.1, take $M(\xi) = M$ (since above you state this is constant) - here M is not a function of ξ .

p. 4.31. I would drop the semi-variance or semi-variogram terms and you introduce the variogram - it just is an added term which can cause more confusion than clarity.

p. 4.40. Note that the model and objections behind polynomial or surface fitting differ from those in kriging. For that reason a comparison between them may not be appropriate. In trend surfaces you essentially want to fit the mean value while in kriging you are trying to reconstruct the actual surface - in that sense kriging includes a kind of conditioning.

p. 4.41. You should point out that a bridged surface (eg. using kriging to get a transmissivity field) is smoother than reality - that is a reason for doing conditional simulations which could be mentioned here.

p. 4.43. The point regarding Monte Carlo techniques (that is probably the most powerful method) is debatable. Monte Carlo methods require knowledge of the input distribution (eg. the distribution of the field generated). Most (if not all) techniques only produce normal (Ganssion) or simple transformed Ganssions (log-Ganssion) as input. In that sense, as well as the sense that they work with very specific domains, it is actually quite restrictive.

6

p. 4.49. Comparison of Smith and Freeze's work should really be made with Mizell et al. which is also two-dimensional.

p. 4.53. Omit the phrase on average deviation - it causes more confusion than clarity.

p. 4.58. The statement, on line 3, that the travel time should be the same as that in an effective homogeneous medium is not necessarily true - in fact I think it is generally false. I suggest that sentence be omitted.

p. 4.74. Note that this is just one way to do conditional simulations - it should really be referred to as a method of conditional simulations. I would also point out conditional simulation paths are (1) smoother than unconditioned fields but (2) more variable than kriged fields which essentially represent the conditional expected values.

p. 4.84. Spectral analysis is not a variant of perturbation analysis. Instead I would say: "Spectral analysis is an analytical approach generally used along with perturbation analysis". (eg. - one could use Green's functions with perturbation analysis and may even use spectral analysis in cases where perturbation is not used).

p. 4.86. The paragraph starting on line 2 is not quite accurate as it stands. (Especially the sentence about a collecting of infinite elements being equivalent to an ensemble: Each realization would essentially be made of a collection of elements.) - I would either completely redo that paragraph or simply remove it.

Also on page 4.86, when stating the spectral representation theorem, include the connection between the dz's and the spectra - that is the important point of its use. Namely move up the discussion or at least the equation from page 4.89 - equations 4.3.10 - 4.3.12.

p. 4.89. Note $J = \frac{dh}{dx}$ is assumed constant.

p.4.91. Again the reference to finite elements (as stated) is not quite right - I would omit it or reword that statement (lines 10-12).

(I seem to be missing pp 4.92 - 4.96)

p.4.99. Check the equations: You want 4.3.26 to be

$$\frac{\partial (nc)}{\partial t} = 0 ?$$

also on 4.27, right hand side, one subscript on the x's should be a j.

2

p.100. Equation 4.3.30 should have $E(q_i^1 c^1)$ or $q_i^1 c^1$ rather than q_i^1, c^1 .

p. 4.127. Reword the lines on uncertainty of uncertainty estimates at the bottom of the page - as stated they are too vague.

p.4.129. Close to the bottom of the page, I don't know what the sentence regarding variance and ergodicity: I don't see how ergodicity can cause large variances around the mean profile. It seems to me it just says there is a lot of uncertainty - I don't see what role ergodicity plays.

In Appendix (A) several terms could be combined, also I have indicated various corrections on the manuscript itself. The only real difficult one is ergodicity: Here is my attempt at a definition.

Ergodicity: A property of a stochastic process which says the ensemble moments equal the corresponding averages calculated from a single realization. These averages in turn can be approximated from a finite part of the realization.

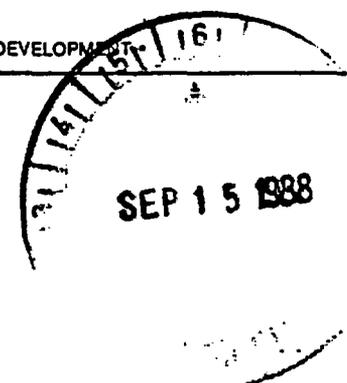
These comments should not be taken to detract or diminish the value of the document. It has a well chosen focus and good point of view. It seems to me to be very appropriate for the intended audience and meets the objectives. I hope my comments will be useful and help improve an already excellent product.



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

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

September 12, 1988



Mr. Mark Logsdon
Nuclear Waste Consultants
155 South Madison #302
Denver, CO 80209

Dear Mark:

Enclosed please find a copy of Al Gutjahr's review of our NRC - stochastic report. Although his review is complementary, we want to take the time to polish the report by incorporating his suggestions. I discussed the extent of work with Jim Yeh, and we hope to have a final draft in our office by Tuesday September 20. You should have a bound copy for your review and distribution by about September 23.

Kind regards.

Yours truly,

Daniel B. Stephens & Associates, Inc.

Daniel B. Stephens, Ph.D.
President

DBS/vo
Enclosure

File: Logsdon.912
Disk: 85-130

September 6, 1988

Belen, N.M.

Daniel B. Stephens & Associates, Inc.
Socorro, N.M. 87801

Dear Dan,

Attached is my report on Jim Yeh's opus! I have indicated several corrections and suggestions that I believe will improve the document. I hope you and Jim find my comments useful-- please call me if you have any questions about them. I would like very much to receive a copy of the final copy of your document when it is finished. My compliments to Jim and you-- I think it does a very good job in addressing the travel time issues and in discussing the stochastic approach.

Sincerely,

Allan
Allan Gutjahr

REVIEW OF

THE SCALE PROBLEM IN GROUNDWATER TRAVEL TIME AND PATH

By T.-C. Jim Yeh

The document was reviewed for clarity, for accuracy with special reference to the probability and stochastic content, and for appropriateness to the audience. Overall, I thought this was a well-written, well-thought out approach. The examples used to illustrate the points were especially well chosen and the sections on "Basic Concepts" and "Groundwater Travel Times and Paths" were very well done.

I do have some specific comments below which I believe would strengthen the document and clarify some issues. In addition, I have made some suggestions (in red) on the manuscript which I think might help the flow of the narrative in several places.

Comments on suggestions made in the text:

p. 1.3. I would say the heterogeneity observed is related to the scale problem rather than a direct consequence - there is no cause/effect relationship here. Also I would move that sentence to one end of the paragraph.

CHAPTER 2: Specific comments are attached. I thought this was a very good chapter that helped set the stage for the rest of the document.

p. 2.2. Distinguish between approach and continuum assumption.

p. 2.7. The weighting function $f(\eta)$ used within the REV concept is a rather special one: namely one where

$$f(\eta) = \begin{cases} 1/V, & \eta \text{ is volume } V \\ 0 & , \text{ otherwise.} \end{cases}$$

I think it would help to specify that clearly in that manner. It also might be interesting to discuss the REV in the more general framework where $f(\eta)$ could be more complicated - eg. a function like

$$f(\eta) = c_1 e^{-\eta^2 c_2}, \quad c_1, c_2 \text{ constants. (Though that may}$$

cause more confusion than clarity for the intended audience.)

p. 2.15. Reword the second sentence: "If the head at the interface is not measured...."

p. 2.16. I would emphasize that the value of K is estimated at the bottom of the page.

p. 2.17. (center of page) Note if the heads are kept constant over the cross-section, then the average of the values is that constant. It seems you really want to emphasize that the head value is an average over the cross-section and not that it is the same at each point in the cross-section. I think the two sentences should be combined. Could you just say: "The average heads over the cross-section at the ends of the columns are kept constant."

p. 2.19. The use of the term "center of mass" in the discussion of tracer-arrival times can lead to confusion. I understand that the reference is to the center of mass of the probability distribution - the center of mass of the particles, however, is really a point in 3-space. I think it would be sufficient to just say "the average arrival time" and omit the words center of mass which may confuse more than clarify (also see Figure 2.6).

p. 2.29. Note that Δ in the middle of the page is now a two-dimensional operator. Also the equation

$$\Delta \times (T\Delta H) = \zeta \frac{2H}{2t}$$

doesn't follow directly from averaging the 3-d equations. Namely, the average of the products is not the product of the averages. (Later reference is made to that fact on page 3.7 and so additional "error" is introduced in that derivation or once again uses an "equivalent" T. I think that should be discussed here.)

p. 2.31. Reference is made to ensemble averaged before that concept is explored or without reference to its later discussion.

p. 2.39. Drop the phrase: "because of the ergodicity assumption." It isn't really due to that assumption - it's essentially just the fact that single realization results can vary from mean results.

p. 2.40. The statement on stationarity is more confusing than clarifying - I suggest it be omitted here since stationarity is discussed later. Also $E(V^2)$ is not the variance unless $E(V)=0$: the fact that $E(V)=0$ is not introduced until later. Either introduce it earlier (see p. 2.42 where you state $E(V)=0$ and also why) or introduce the general definition of

$$\text{var}(V) = E[V-E(V)]^2$$

(For the purposes of this discussion, I'd suggest introducing $E(V)=0$).

p. 2.42. Equation 2.6.7: upper limit should depend on $\lambda+t$ for the discussion that follows - also you may want to use a different variable of integration (say ξ). As it stands, the change of D_m with time is not shown.

p. 2.43. Note the exponential form (2.6.8) is assumed; it is not implied by the discussion.

Figure 2.12c. not shown as bell-shaped, as referred to in the text on p. 2.45.

p. 2.53. Line 3 - head gradient should be .01 not 80.01.

CHAPTER 3: Again, I have very few comments. Initially there was strong conviction that the travel time criteria, as defined by NRC regulations was not viable. However, this resolve seemed to weaken as the chapter progressed. I thought the initial points made regarding travel times and the refocusing on exceedence of mass flux in a specified area were well taken and you should "stick-by-your-guns" on that issue. There is not much specific that I can point out here but just a feeling of weakening of that position as I read through this chapter. Perhaps a reiteration of the points at the end might be useful. Again the discussion of uncertainties is good and needed. However, it may overshadow the travel-time argument and so a repetition at the end of the chapter could be useful.

p. 3.15. At the bottom of the page - I don't think that even in the uncorrelated or statistically isotropic case that the observed drawdown represents the average drawdown. I think you could reword those sentences and drop that reference.

CHAPTER 4: I have several suggestions to make here - essentially they involve tightening up the probabilistic discussion a bit while still retaining the informal analogies. Specifically, some definitions and terms need to be more precise as discussed below.

p. 4.6. I would say the hydraulic properties of the aquifer are viewed as random variables.

p. 4.7. (8th line down) - I would add something like: Namely, it is not completely predictable: only probabilities can be associated with the possible values and hence the values are considered as realization of a random variable.

(2nd paragraph) - I would say the properties are taken to be log-normal or normal.

p. 4.8. (bottom of page) - Note that in any particular case, however, we only want the specific values for that case. We may still model this as a stochastic process (representing our lack of knowledge, for example) though we also may wish to condition the results with data.

p. 4.10. The discussion on stochastic processes starting on line 7 is not very clear. First, I would start a new paragraph with the current line (line 7 etc.): "For example.....one possible outcome of a stochastic process." In a stochastic process the value of the quantity, say conductivity, $k(x)$, is a random

variable for each location x. Namely, if conductivity is observed at locations x_1, x_2, \dots, x_n then $k(x_1)$ is a random variable, $k(x_2)$ another random variable, and so on out to $k(x_n)$. Each has a probability distribution and furthermore the probability distributions may be interrelated. The chance of finding a particular sequence.....locations. (See bottom of p. 4.10) now add sentence from 4.10, "This implies.....(Figure 4.3)".

(new paragraph see p. 4.13)

In order to determine..... must be known. The joint distribution is completely defined only if the probabilities associated with all possible sequence of $K(x_i)$ values along the transect are known. (omit "However...up to equation 4.1.1) (omit 1st sentence, next paragraph - Generally speaking.... and add the second and third sentence to the part ending "stationarity and ergodicity." to the same paragraph.

Now start a new paragraph:

"Stationarity....constant in space. (Reword the sentence on ergodicity - eg. combine with next sentence, say something like: Ergodicity means that by observing the spatial variation of a single realization...

p. 4.14. don't couple the second order stationarity to ergodicity - this makes it sound as if second-order stationarity was a consequence, rather than an assumption. I would instead suggest that line 12, etc., be replaced by: "Because it is difficult to obtain all of the joint distributional information required and because in many cases important properties are assessed by moments like the mean, variance or other lower order moments, an assumption of weak or second order stationarity is often involved." The first moment.....

$$\mu = E(K)$$

The autocovariance or covariance function is then defined as

$$C(\xi) = cov(K(x+\xi), K(x)) E[(K(x+\xi) - \mu)(K(x) - \mu)]$$

The second-order stationarity assumptions are that (i) $\mu = E(K)$ is constant and (ii) $cov(K(x+\xi), K(x))$ only depends on ξ , where $\xi \dots$

The example of the sine series is a good one though it should be noted this is a strictly deterministic function.

p. 4.20. (top) - you might want to add a reference to MacMillan & Gutjahr (198) where an attempt is made to relate correlation lengths to geological fractures.

5

p. 4.23. I would add a statement on non-stationarity in covariance (which can at times be more restrictual than changes in the mean).

lines 12 and 13 add: "uncorrelated or statistically independent from one point to another. Also in the next sentence, indicate that the covariance presumably depends only on separation distance.

p. 4.29. I would slightly reword the sentences on random functions and regionalized variables to first introduce random functions (indicating equivalence to stochastic processes) and regionalized variables (as realizations of stochastic processes).

p. 4.30. Geostatistics really consists of more than two parts (eg. I would include conditional simulations, etc.). I understand what you are trying to get at but it would be better to reword that paragraph a bit. For example, you might say: Two important parts of geostatistics are (1) identification of the spatial structure of the variable (variogram estimation, trend estimation, etc.) and (2) interpolation or estimating the value of a spatially distributed variable from neighboring values taking into account the spatial structure of the variable (Davis, 1973)

In equation 4.2.1, take $M(\xi) = M$ (since above you state this is constant) - here M is not a function of ξ .

p. 4.31. I would drop the semi-variance or semi-variogram terms and you introduce the variogram - it just is an added term which can cause more confusion than clarity.

p. 4.40. Note that the model and objections behind polynomial or surface fitting differ from those in kriging. For that reason a comparison between them may not be appropriate. In trend surfaces you essentially want to fit the mean value while in kriging you are trying to reconstruct the actual surface - in that sense kriging includes a kind of conditioning.

p. 4.41. You should point out that a bridged surface (eg. using kriging to get a transmissivity field) is smoother than reality - that is a reason for doing conditional simulations which could be mentioned here.

p. 4.43. The point regarding Monte Carlo techniques (that is probably the most powerful method) is debatable. Monte Carlo methods require knowledge of the input distribution (eg. the distribution of the field generated). Most (if not all) techniques only produce normal (Ganssion) or simple transformed Ganssions (log-Ganssion) as input. In that sense, as well as the sense that they work with very specific domains, it is actually quite restrictive.

6

p. 4.49. Comparison of Smith and Freeze's work should really be made with Mizell et al. which is also two-dimensional.

p. 4.53. Omit the phrase on average deviation - it causes more confusion than clarity.

p. 4.58. The statement, on line 3, that the travel time should be the same as that in an effective homogeneous medium is not necessarily true - in fact I think it is generally false. I suggest that sentence be omitted.

p. 4.74. Note that this is just one way to do conditional simulations - it should really be referred to as a method of conditional simulations. I would also point out conditional simulation paths are (1) smoother than unconditioned fields but (2) more variable than kriged fields which essentially represent the conditional expected values.

p. 4.84. Spectral analysis is not a variant of perturbation analysis. Instead I would say: "Spectral analysis is an analytical approach generally used along with perturbation analysis". (eg. - one could use Green's functions with perturbation analysis and may even use spectral analysis in cases where perturbation is not used).

p. 4.86. The paragraph starting on line 2 is not quite accurate as it stands. (Especially the sentence about a collecting of infinite elements being equivalent to an ensemble: Each realization would essentially be made of a collection of elements.) - I would either completely redo that paragraph or simply remove it.

Also on page 4.86, when stating the spectral representation theorem, include the connection between the dz's and the spectra - that is the important point of its use. Namely move up the discussion or at least the equation from page 4.89 - equations 4.3.10 - 4.3.12.

p. 4.89. Note $J = \frac{dh}{dx}$ is assumed constant.

p.4.91. Again the reference to finite elements (as stated) is not quite right - I would omit it or reword that statement (lines 10-12).

(I seem to be missing pp 4.92 - 4.96)

p.4.99. Check the equations: You want 4.3.26 to be

$$\frac{\partial}{\partial t}(\underline{nc}) = 0 ?$$

also on 4.27, right hand side, one subscript on the x's should be a j.

7

p.100. Equation 4.3.30 should have $E(q_i^1 c^1)$ or $q_i^1 c^1$ rather than q_i^1, c^1 .

p. 4.127. Reword the lines on uncertainty of uncertainty estimates at the bottom of the page - as stated they are too vague.

p.4.129. Close to the bottom of the page, I don't know what the sentence regarding variance and ergodicity: I don't see how ergodicity can cause large variances around the mean profile. It seems to me it just says there is a lot of uncertainty - I don't see what role ergodicity plays.

In Appendix (A) several terms could be combined, also I have indicated various corrections on the manuscript itself. The only real difficult one is ergodicity: Here is my attempt at a definition.

Ergodicity: A property of a stochastic process which says the ensemble moments equal the corresponding averages calculated from a single realization. These averages in turn can be approximated from a finite part of the realization.

These comments should not be taken to detract or diminish the value of the document. It has a well chosen focus and good point of view. It seems to me to be very appropriate for the intended audience and meets the objectives. I hope my comments will be useful and help improve an already excellent product.