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Risk Assessment Methodology Development for Waste Isolation in Geologic Media

Technical Review of NUREG/CR-1636, Volume 4;
NUREG/CR-2324 and NUREG/CR-2343

Prepared by C. A. Stevens, R. R. Fullwood, B. Amirijafari,
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CONTRACTOR'S NOTICE

ABSTRACT

A review of three documents prepared for the USNRC by Sandia National Laboratories (SNL) is presented. These are NUREG/CR-1634, Volume 4 concerned with the effects of variable hydrology on waste migration; NUREG/CR-2324, a user's manual for SWIFT; and NUREG/2343, a user's manual for DNET. This review completes Task 4 of the detailed technical review of the SNL program for Risk Assessment Methodology Development for Waste Isolation in Geologic Media.

In general, these reports exhibit high technical quality that characterizes the SNL work. They are tersely written with little condescension to the non-expert reader for understanding the physical situation being modeled. Indeed, the emphasis is on the mathematical procedures rather than the repository physics, leaving the adequacy of the results presented in many computer plots, pretty much to the interpretation of the reader. Other general comments have been presented previously, such as the data conservatisms, need for data that cannot be measured without disturbing the geometry, and the overall plan for use of the many codes developed in the program.

EXECUTIVE SUMMARY

The review procedure used for these three documents is similar to that used in the previous three volumes of this series, namely the review criteria are those specified by the contract and presented in Section 1. These criteria are not applied directly to the review of each report but rather implicitly. The reason for so doing is that no single set of criteria will fit all types of reports and, rather than indicating inapplicability, it was elected to conduct the review in such a manner that applicable criteria are used to exhibit the features or deficiencies of the reports as the case may be. Many of the previous commendations and criticisms of other reviews in this series are applicable to this set as well. The technical quality of the work is high and the reports are generally well written, but there is little pedagogical intent in the presentation and much of the interpretation of results is left to the reader. There is little reference to work done outside of SNL and there is no master plan for the work such as when certain codes should be selected for certain purposes. There is little indication that the codes are user-friendly and it would seem appropriate for an outside organization to use the codes for a repository model so as to judge their applicability to licensing.

The first report, NUREG/CR-1636 Volume 4, "Risk Methodology for Geologic Disposal of Radioactive Waste: Effect of Variable Hydrologic Patterns on the Environmental Transport Model", develops the theory for the Environmental Transport Model (ETM) for the case of stochastic periodic hydrology such as may result from annual rain patterns. The results are that the effect is negligible; however, this result is not completely convincing because of the assumption of annual periodicity. There may be long-term weather cycles that indeed produce a significant effect. It would have seemed more appropriate to explore the problem with a definite periodic function to find the ranges of periodicity for which the environmental time constants exhibit effects. Then, if these are historically improbable, the effect may be assumed to be negligible.

The second report, NUREG/CR-2324, "... User's Manual for the Sandia Waste-Isolation Flow and Transport Model (SWIFT) Release 4.81", is as the title says a user's manual for SWIFT. This is a well written user's manual that should assist in operating the code. We feel that its usage would be facilitated by application to a specific but perhaps simple site and by showing the reader how the information is assembled and input in the code. We have the impression that it is a very exact code for up to 3 dimensional modeling, but that it is long-running for a given problem and not very user-friendly, contrary to that which may be desirable for licensing purposes. This impression should be dispelled, if incorrect.

The last report, NUREG/CR-2343, "... the DNET Computer Code User's Manual", provides the theoretical and model development of the DNET code with instructions for usage. Many results comparisons are made with SWIFT to verify accuracy. DNET is a two-dimensional code similar to NWFT in that they both use a network flow model but, whereas NWFT uses static hydraulic properties, those in DNET are dynamic for the purposes of studying feedback mechanisms such as: fluid flow, salt dissolution, thermal expansion, fracture formation, and closure and salt creep. The equations are not solved simultaneously, but sequentially. The first third of the report is taken up with discussing the theory and models used in the code and the sequential computations. The remainder is taken up with the user's manuals, comparison with SWIFT calculations and a sample problem. It appears that DNET is not a stand alone code but must use a SWIFT calculation of the flow field. DNET does not treat geometric changes as might be expected from the dissolution of salt which would form cavities possibly followed by roof collapse.

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1.0 INTRODUCTION

1.1 Background

The results of Task 4, an independent multidisciplinary review and critique which Science Applications, Inc. (SAI) has performed for the Nuclear Regulatory Commission (NRC) are reported herein. The reviewed material consists of technical reports resulting from a project entitled, "Risk Assessment Methodology Development for Waste Isolation in Geologic Media", which has been prepared by Sandia Laboratories for the NRC.

The reports reviewed under Task 1 were:

1. NUREG/CR-0458 (SAND78-0029), Risk Methodology for Geologic Disposal of Redioactive Waste: Interim Report, by J.E. Campbell, et al., October 1978.

2. NUREG/CR-0394 (SAND78-0912), Risk Methodology for Geologic Disposal of Radioactive Waste: Sensitivity Analysis Techniques, by R. L. Iman, J. C. Helton, and J. E. Campbell, October 1978.

3. NUREG/CR-0424 (SAND78-1267), Risk Methodology for Geologic Disposal of Radioactive Waste: The Sandia Waste Isolation Flow and Transport (SWIFT) Model, by R. T. Dillon, R. B. Lantz, and S. B. Pahwa, October 1978, and associated computer code and user's manual describing SWIFT.

The reports reviewed under Task 2 were:

1. NUREG/CR-1262 (SAND80-0157), Risk Methodology for Geologic Disposal of Radioactive Waste: A Distribution Free Approach to Inducing Rank Correlation Among Input Variables for Simulation Studies, by Ronald L. Iman and W. J. Conover, March 1980.

2. NUREG/CR-1376 (SAND-0717), The Distributed Volocity Method of Solving the Convective Dispersion Equation, by James E. Campbell, Dennis E. Longsine, and Mark Reeves, July 1980a.

3. NUREG/CR-1377 (SAND80-0644), Risk Methodology for Geologic Disposal of Radioactive Waste: Transport Model Sensitivity Analysis, by James E. Campbell, Ronald L. Iman, and Mark Reeves, June 1980b.

4. NUREG/CR-1397 (SAND80-0020), Risk Methodology for Geologic Disposal of Radioactive Waste: Small Sample Sensitivity Analysis Techniques for Computer Models, With An Application to Risk Assessment, by Ronald L. Iman, W. J. Conover, and James E. Campbell, March 1980.

5. NUREG/CR-1608 (CGS/NR85F060), Scenario Development and Evaluation Related to the Risk Assessment of High Level Radioactive Waste Repositories, by F. W. Schwartz and F. A. Donath, June 1980.

The reports reviewed under Task 3 were:

1. NUREG/CR-1636, Volume 1 (SAND78-1711), Risk Methodology for Geologic Disposal of Radioactive Waste: Model Description and User Manual for Pathways Model, by Jon C. Helton and Peter C. Kaestner, March 1981,

2. NUREG/CR-1636, Volume 2 (SAND79-1393), Risk Methodology for Geologic Disposal of Radioactive Waste: Sensitivity Analysis of the Environmental Transport Model, by Jon C. Helton and Ronald L. Iman, December 1980,

3. NUREG/CR-1636, Volume 3 (SAND79-1908), Risk Methodology for Geologic Disposal of Radioactive Waste: Asymptotic Properties of the Environmental Transport Model, by Jon C. Helton, Jack B. Brown, and Ronald L. Iman.

The results of Tasks 1, 2, and 3 were presented in NUREG/CR-1672, Volumes 1, 2, and 3, respectively. The reports reviewed here are:

1. NUREG/CR-1636, Volume 4 (SAND79-1909), Risk Methodology, for Geologic Disposal of Radioactive Waste: Effects of Variable Hydrologic Patterns on the Environmental Transport Model, by Jack B. Brown and Jon C. Helton, December 1981.

2. NUREG/CR-2324 (SAND 81-2516), User's Manual for the Sandia Waste-Isolation Flow and Transport Model (SWIFT) Relase 4.81, by Mark Reeves and Robert M. Cranwell, November 1981.

3. NUREG/CR-2343 (SAND81-1663), Risk Methodology for Geologic Disposal of Radioactive WAste: The DNET Computer Code User's Manuals, by Robert M. Cranwell, et al., January 1982.

At the beginning of Task 1, SAI assembled a panel of experts whose collective knowledge spanned all of the technical areas covered in the reports reviewed during that task and the areas which were expected to be covered in later tasks of this project. In-depth reviews were performed by a sub-panel selected on the basis of technical expertise, availability and absence of conflict of interest. Similar sub-panels were selected for Tasks 2, 3, and 4. The sub-panel members selected for Task 4 were: B. Amirijafari, S. Basin, J. Cohen, R. Fullwood, and C. Stevens. The reviewers' resumes are in Appendix A.

As before, the management and coordination for the review effort was performed by a technical coordinator and a management coordinator. The technical coordinator, Dr. R. Fullwood, had the responsibility for the technical content of this final report. The management coordinator, Dr. C. Stevens, had the responsibility for the technical editing of the final report and for the overall project management.

In order to assure that the review be independent, several restrictions were imposed on the sub-panel. Basically, these were:

1. No contact was to be made between the SAI reviewers and the Sandia personnel engaged in the study,

2. Essentially, no guidance was to be given by the NRC on how the review was to be conducted, other than that supplied in writing in the Contract Work Statement,

3. The SAI reviewers were not to be involved in other programs for the Department of Energy or otherwise be involved in projects which would lead to an actual or perceived conflict of interest.

All of these restrictions were complied with during the review period. Although there was no restriction on the use of outside consultants to assist in the review, none were used for the reports reviewed during Phase 4.

All members of the sub-panel were requested to review all three documents in their entirety; that is, they were encouraged to review material beyond their area of expertise as well as the material in their areas of specialty. This was in accordance with NRC preference. In practice, each reviewer tended to put the major effort into those areas which matched his expertise.

The depth of review was necessarily restricted, both by time and financial limitations. Thus, for example, the use of computer codes to provide independent checks of results or to identify limitations of the methodology was precluded. Review was restricted to the three documents; that is, the reviewers were not asked to examine backup or reference material.

The reviewers were requested to send written comments covering the material they had reviewed each month to the management coordinator. The management coordinator, in turn, distributed the material to all sub-panel members. Thus, each sub-panel member was periodically updated on the progress and opinions of the others. In addition, the sub-panel members were encouraged to communicate with each other in person, by mail and by telephone, as appropriate.

After three months, a final report was compiled using the material generated by the individual reviewers.

1.2 Review Procedure

The procedure for the reviews carried out in Task 4 was similar to that used previously. The method of review was devised to meet certain objectives, while abiding by a set of restrictions. The most important objective was to have expertise in every technical aspect of the work reviewed. A secondary objective was to have at least two comments, and preferably more, on each part of the work. The goal was not to be critical for its own sake, but to make suggestions for improvement when fault was found, and to do this in a thorough manner. The following questions were prepared by the NRC to assist in the review process:

1. Are the models realistic?

    a. Are the assumptions valid?

    b. What would be the impact on the analysis results of any incorrect assumptions?

    c. How should any identified weaknesses in the models by improved?

2. Is the methodology valid?

3. Are the data valid?

    a. What uncertainty in the data would render the model results unrealistic?

    b. Was each datum uncertainty and its contribution to the uncertainty in the results assessed appropriately?

4. Is the time period examined or used in calculations appropriate?

5. Do the event sequences chosen for calculation cover a reasonably complete range?

    a. Were any important potential risk contributors omitted? If so, identify.

    b. Were the quantitative or qualitative criteria for choice of sequences valid?

6. Was an effort made to identify (i.e., rank according to importance to risk) key parameters, processes and events?

    a. If so, was the effort adequate?

    b. Evaluate the methods used to achieve the ranking.

7. Were the uncertainties in the results considered?

   a. Were these uncertainties propagated and quantified?

   b. Were acceptable numerical methods used?

   c. Were the contributing uncertainties correctly assessed?

8. Which of the models and which parts of the methodology could be used to resolve discrete questions (e.g., for a licensing review) or would they only be useful as supporting information to discrete questions?

   a. What types of questions could be resolved by use of a given model or the methodology?

9. Conclusions.

10. Recommendations.

    Any recommendation shall be accompanied by an estimate of the contribution to error in the results of a specific suggestion for improving the analysis.

    Not all of the questions listed above are applicable to each report; however, they were intended to provide a general guide during the review process.

1.3 Global Considerations

While these very specific review criteria are appropriate, it is also appropriate to consider the work in a broad perspective. By implication, the program anticipates a risk-based licensing procedure. Presumably the license applicant would be free to site and design what he considers to be the safest repository for the investment. The level of safety achieved would be judged by the applicant's risk assessment. To evaluate this submittal, the NRC would need to perform an independent risk assessment of the same quality or superior quality to that performed by the licensee. The development of this risk assessment methodology for the NRC is the purpose of the SNL work. It would seem that the risk assessment methodology should meet certain criteria:

1. It should be capable of analyzing the engineered barriers for which an applicant could conceivably want to take credit. Presumably this would include: age of waste, waste matrix, waste canister, buffer, sealing and local hydraulic considerations, e.g., bypass channel to divert flow from the volume containing the wastes.

2. It should be capable of analyzing a site any place in the U.S.and particularly a site chosen because of climatology, demographics and hydrology to provide barriers to radionuclide dispersal.

3. The work should clearly define the assumptions and measures used therein. For example, is it appropriate to only consider individual dose? If so, repository siting in presently populous areas, such as large cities, is as safe as desert areas - this does not seem reasonable?

4. The work should be capable of analyzing the probability that ground water will enter and disrupt the repository. Analyses to date have considered this as occurring as a disrupting event whereas it could also be the result of partial degradations that eventually combine to cause a release.

5. The parts of the analysis should have accuracy commensurate with the accuracy of the overall risk analysis. This consideration may allow the use of simpler nuclide migration models.

6. The analysis should not require parameters that are not measurable. If such parameters are required, then acceptable methods for obtaining or inferring them should be suggested. It should also be recognized that these parameters are applicable during time periods in the distant future.

7. The methodology should be traceable and its virtues and limitations should be known to people other than the developers.

8. It should be sufficiently self-contained that it is not subject to human error that may arise in interpreting an output in preparation for the next calculation.

As this relates to the SNL work, reports to date have not addressed Item 1 except for the age of waste which in the calculations has been conservatively taken to be rather young. The nuclide dispersion aspects of the work seems capable of addressing Item 2, but not the probability aspects. The work has given little consideration to siting for mitigation purposes. The assumptions and measures (Item 3) are generally not clearly stated. Use of the term risk in the work was not defined. The probability aspects of the work (Item 4) have not kept pace with the nuclide migration aspects of the calculation. Considerable effort has gone into sensitivity analysis, but generally this has not been extended to error analysis (Item 5). The general impression is that nuclide migration is modeled to an accurancy greater than the other aspects of risk, e.g., the probabilities. The models for ground water and surface water migration of radionuclides require the measurement of parameters which are inaccessible (Item 6). The methodology is only traceable with considerable difficulty. It is not possible to state "this is the probabilistic method" or "this is the ground water radionuclide method". In the latter case there are many radionuclide methods and little guidance is provided as to which ones should be used. The methodology is not self-contained but is distributed through various reports and papers. It would seem unlikely that two organizations assessing the risk of the same repository using the methodology would arrive at the same or similar results. This statement is based on the selection of codes available within the methodology, the parameter range and the latitude available in setting up problems and using the output of one calculation in later calculations.

In defense of the SNL program, their work scope is not as encompassing as the above criteria which are presented as a global program perspective without regard to work assignments. The technical quality is very good in the areas that have been addressed; the criticism is that the full methodology for assessing the risk of a geologic waste repository has not been presented.

The next section presents very briefly the reports reviewed to date, a review of the three reports reviewed in this volume as they may relate to each other and a more detailed separate review of each report.

2.0 REVIEW AND CRITIQUE

2.1 Overview of the Set of Documents

Figure 2-1 presents the taxonomy of the risk methodology reports that have been reviewed to date. The report by Campbell, et al, (1978) provides a general outline of the project. It contains a discussion of the disruptive event initiators and describes a deterministic-probabilistic methodology for estimating the likelihood of the repository disruption. It also describes a method of predicting nuclide transport rates (SWIFT) and presents a compartment model for describing the pathways to man, the Environmental Transport Model (ETM). The report concludes with a discussion of the health effect on man due to radiation. This key report is emphasized because of its relationship to the program. It appears that the program has departed from the program as outlined in this report as is illustrated by the number of reports in the various subject areas. It should be emphasized that the stated purpose of the SNL program is one of methodology development. None of the reports actually calculates the risk of the waste repository, real or hypothetical.

It is appropriate to present the documents reviewed to date. These are published as various volumes of NUREG/CR-1672.*

Volume 1 presented reviews of:

1-1 Campbell, et al. (1978) which provided the overview of the work.

1-2 Iman, et al. (1978) which introduced Latin Hypercube Sampling (LHS) for sensitivity analysis.

1-3 Dillon, et al. (1978) describing the Sandia Waste Isolation Flow and Transport (SWIFT) code

Volume 2 presented reviews of:

2-1 Iman and Conover (1980) discussing a distribution-free rank correlation method for treating sensitivity analyses having input-variable interactions.

2-2 Campbell, et al. (1980a) discussed the Distributed Velocity Method (DVM) for calculating nuclide transport.

2-3 Campbell, et al. (1980b) is concerned with sensitivity studies for 3 accident scenarios using the Network Flow and Transport model (NWFT).

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* Refer to page 1-1 et. seq. for full references.

2-4 Iman, et al., (1980) addresses a generalization of LHS with scenario applications.

2-5 Schwartz and Donath (1980) presents the Deterministic-Probabilistic CONTAMINANT Transport code (DPCT).

Volume 2 presented reviews of:

3-1 Helton and Kaestner (1981) reported on the Environmental Transport Model (ETM) and the transport to man model.

3-2 Helton and Iman (1980) presents the results of a sensitivity analysis of ETM.

3-3 Helton, et al. (1981) discusses "asymptotic" properties of ETM.

This report presents:

4-1 Brown and Helton (1981) discusses the effects of stochastic-periodic hydraulics on ETM.

4-2 Reeves and Cranwell (1981) provides a user's manual for SWIFT.

4-3 Cranwell, et al. (1982) presents the Dynamic Network (DNET) model and its user's manual. DNET is similar to NWFT except the hydraulic properties are treated dynamically by sequential calculations.

Referring to Figure 2-1, the first column concerns the probability that nuclides will be released and columns 3-6 are aspects of the calculation of the effects on man if these nuclides are released. Column 2 is for sensitivity analyses which may apply to all of the other columns. Some of the reports are repeated if they contain significant information which is appropriate for several columns. Since sensitivity analysis is not part of the risk dyad it is perhaps not appropriate that it be included in the constituents of a risk assessment, however, it constitutes a major part of the work. Sensitivity analyses are very important, but because none of the data are precise, they should be extended to determine the degree of confidence that may be placed on the final results and conclusion. In this sense, a sensitivity analysis is a step along the way to uncertainty estimation but this last step is not taken. Sensitivity analysis may also be important in its own right for directing a research program relating to the accuracy requirements for data but even here the basic requirement is for error reduction.

The figure shows that of the documents reviewed to date, probability of disruption and health effects have not progressed beyond Campbell, et al. (1978) while there have been 6 additional reports on groundwater transport of radionuclides. (The various programs developed by SNL are: SWIFT, DVM, NWFT, DPCT and DNET. These are in addition to codes written by other

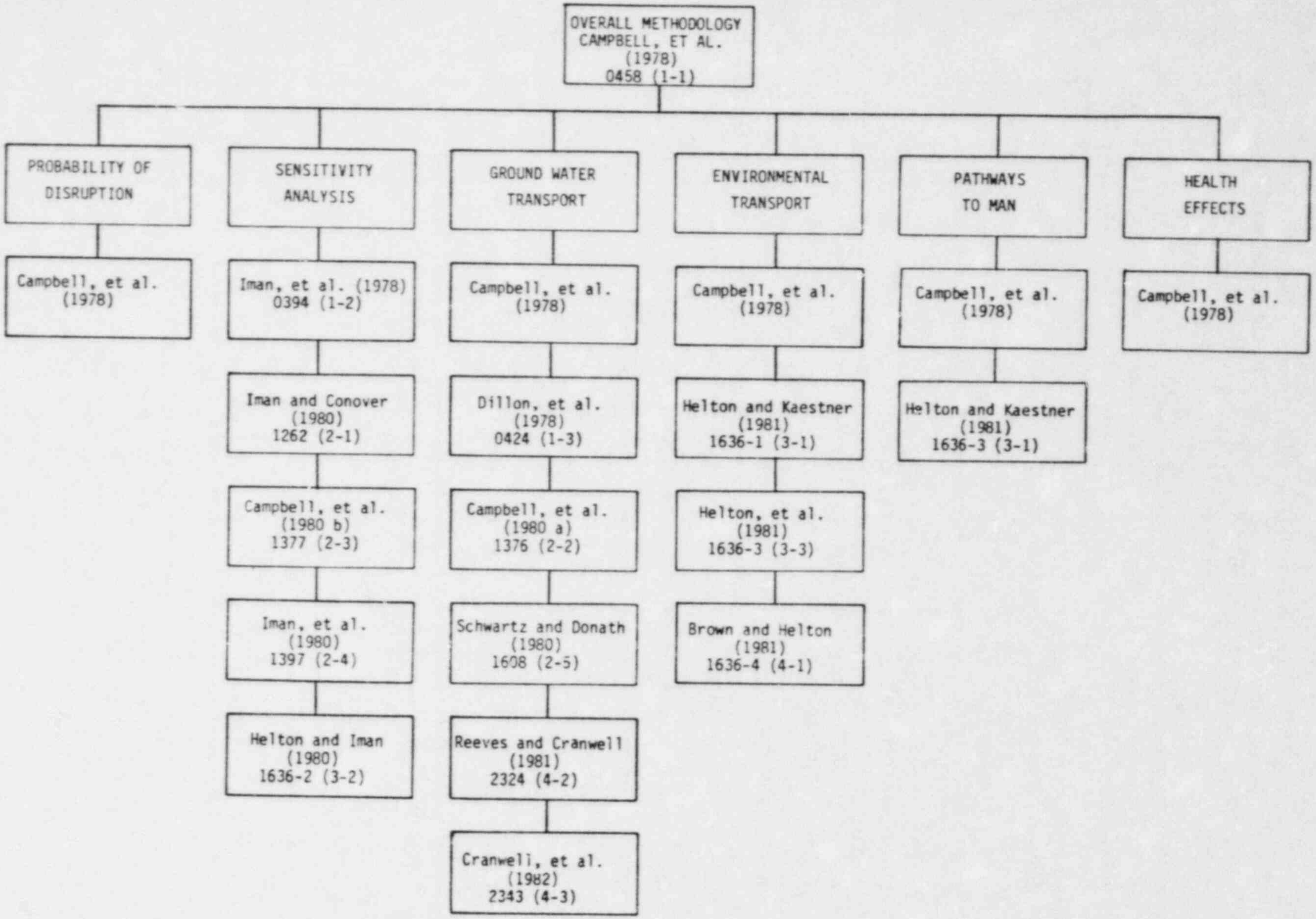

Figure 2-1. Taxonomy of the Risk Methodology that has been Reviewed (4-digit number is last part of NUREG/CR-xxxx, first number in parenthesis is review phase number, second number is document number).

organizations for similar purposes.) Five reports concern sensitivity analyses, four including Campbell, et al. (1978) concern environmental transport and two, including Campbell, et al. (1978) address pathways to man.

It appears that the program emphasis has shifted with time. Unfortunately a modified program description has not been published. Of particular concern is the apparent absence of progress in probabilistic development to keep pace with the work in the consequence analysis area. It would seem reasonable, early in the program, to push through a "first-cut" calculation of repository risk (probabilities and consequences), go back and estimate the uncertainty arising from input data and methodology and determine at what confidence limit the repository becomes a health hazard. Having done this, go back and allocate resources as needed to reduce the uncertainty.

Turning to the three documents of concern in this volume, document 1 is quite distinct and separate. It is concerned with the effect of stochastic periodic hydrological changes and their effect on the environmental transport model. The conclusions are that the effect is expected to be negligible. It would seem that this result could have been obtained as a scoping calculation using a deterministic periodic function coupled with an exploration of the time constant involved in the problem. This report fails to convince the reader because only annual periodicity was considered while it is well known that there are long-term weather changes well within the time periods of interest. The stochastic periodic treatment has mathematical eloquence but hardly seems justified until a problem is shown to exist.

The other two reports are concerned with deep burial nuclide transport. The first of these is a user's manual for SWIFT and as such is quite well written. SWIFT gives the impression of being "Protean" in that it is not very user friendly but if used right, it is capable of solving all problems at the expense of computer time. As a user's manual, report 2 succeeds very well but it does not provide any insight as to when it is necessary to use SWIFT and when some other code such as DNET would provide sufficient accuracy. An example showing how SWIFT is set up for a typical calculation would help to remove the impression that SWIFT is primarily for the use of specialists. If it is difficult to use, it would not seem to fulfill its licensing purpose.

The last report is entitled "The DNET Computer Code User's Manual". As a user's manual it does not succeed as well as report 2 does for the SWIFT code. But report 3 is more than a user's manual in that it develops the theory and models for DNET. DNET is a network flow model similar to NWFT (NUREG/CR-1370 and the critique NUREG/CR-1672). DNET differs from NWFT in its dynamic treatment of flow parameters to allow the study of feedback mechanisms and much of the report is taken up in discussions of how this is accomplished and the models used in this accomplishment. The physical processes that are modeled are: fluid flow, salt dissolution, thermal expansion and fracture formation and closure. As presented DNET does not seem to be capable of handling the geometry changes that are expected to take place as salt is dissolved and removed from the repository area. Sample problems are provided although no results are tested against observations.

The following sections present detailed reviews of the individual reports.

2.2 Review of NUREG/CR 1636, Volume 4: Effects of Variable Hydrologic Patterns on the Environmental Transport Model

Overview

The authors have done a credible job of developing some understanding of the effects of periodic and/or stochastic variations in hydrologic phenomena on the predictions made by the environmental transport model (ETM). If such variations do not affect ETM predictions significantly then the mean annual flow rate may be used when deriving estimates of the ETM parameters.

The environmental transport model, consists of a system of first order differential equations of the form

$$\frac{dX}{dt} = AX + R \qquad (1)$$

Each element of the vector X(t) denotes the concentration of a particular radionuclide at time t, in a given subzone or compartment. The model includes four subzones: the ground water subzone, soil subzone, surface water subzone and the sediment subzone. The matrix A, contains the transfer coefficients between each of the subzones.

The elements of the vector R represents the rates of which radionuclides are entering the subzones. In the present analysis, R is assumed to be constant. Since the elements of the A matrix are derived from the flow rates of water and solid materials between the subzones the effect of periodic and/or stochastic variations in these flow rate is of concern. The authors study these effects by considering three variations of the above model:

$$\frac{dY}{dt} = p(t)AY + R \qquad (2)$$

$$\frac{dY}{dt} = (p(t)F + D)Y + R \qquad (3)$$

and

$$\frac{dZ(t,w)}{dt} = S(t,w)p(t)AZ(t,w) + R \qquad (4)$$

On the basis of their analysis, the authors have concluded that it is reasonable to continue to use average annual flow rates when estimating the values of the elements of the A matrix. This decision was based upon the following observations:

(1) the periodic or stochastic variations result in changes in radionuclide concentrations which swing above and below those concentrations obtained when constant values (average annual flow rates) are used,

(2) the highest concentrations occur for only a part of a year, and

(3) much greater uncertainty in surface concentrations of radionuclides will result from: (i) variations in discharge rates to the surface environment and (ii) changes in the surface environment itself.

The authors point out the important fact that the above observations are not proved in the usual mathematical sense of proof; rather, the collection of analytical and simulation results presented lend credence to their conclusion.

Critique

In summarizing the general comments submitted by the reviewers we find:

(1) The authors have done a credible job in investigating the effects of periodic and/or stochastic variations in water and sediment flow rates on predictions of radionuclide concentrations.

(2) The report was, in general, well written with the exception of the following criticisms:

(i) A more complete summary of the approach and the conclusions should be presented within the Introduction. This should include a complete statement of the motivation behind the authors' concern for the effects of periodic and/or stochastic variation of hydrologic phenomena. For this purpose, the material contained in Chapter 6 should be included in the Introduction and, perhaps, restated in Chapter 6. This will provide the reader with a better understanding of the problem and the general approach at the outset.

(ii) More descriptive chapter titles should be employed.

(iii) Smoother and more complete transitions should be provided between chapters. Once again, this will help the reader better understand the purpose of each chapter and where it fits into the overall structure of the report.

(iv) Motivation behind the selection of the models which the authors are using should be given. In short, the authors should say why they selected these particular models. Why not use other models or other approaches such as straight forward Monte Carlo simulation of the flow processes?

(v) Perhaps a more effective way of presenting the results should be devised. The use of tables rather than graphs has been suggested by one reviewer. Some combination of both tabulated data and plots with much more discussion regarding the key points to be observed would possibly be the best alternative.

(vi) The captions and the annotations of the various figures and plots are in need of improvement.

(3) The summary and conclusions, presented in Chapter 6, should include some discussion of unresolved issues and suggestions regarding what should be done relative to these issues. In other words, if the authors feel that they have presented sufficiently strong evidence that the question of periodic and/or stochastic variations in hydrologic phenomena, as it pertains to geologic disposal of radioactive waste, has been laid to rest once and for all, then they should say so. If not, they should state what remains to be done relative to this subject.

2.3 Review and Critique of NUREG/CR-2324, "User's Manual for the Sandia Waste Isolation Flow and Transport Model (SWIFT) Release 4.81"

General Critique

A previous Sandia report (NUREG/CR-0424, "Risk Methodology for Geologic Disposal of Radioactive Waste: The Sandia Waste Isolation Flow and Transport (SWIFT) Modoel," Dillon et al., 1978) presented the development of the SWIFT model. A technical review of this previous report was conducted in NUREG/CR-1672, Vol. 1, ("Risk Assessment Methodology Development for Waste Isolation in Geologic Media," Stevens, et al., 1980). The current report under review is a user's manual for the SWIFT model which also covers the background and mathematical development of the model. Since the model itself and its application were considered in the earlier review, many of the review questions have already been addressed and will not be duplicated here.

It was the consensus of the reviewers that this report is a well-written and useful guide to the application of the SWIFT model. Chapter 6 describing Data Input was particularly well-done. It was noted, however, that an important missing element is the presentation of sample problems with discussion of appropriate inputs and elaboration of resultant outputs. Although such information is available elsewhere (e.g., NUREG/CR-1968, "SWIFT Self-Teaching Curriculum"), the user's guide to the SWIFT Model should include such examples. Microfiche copies of the program listing and sample problems were included with the report, but these appeared to be inserted as an afterthought since they are not listed in the table of contents, and are not discussed in any detail in the text.

The SWIFT model calculates the groundwater transport of radionuclides from the repository to the near surface vicinity. A section describing the functions and uses of the model relative to other available models would be a useful inclusion in the user's manual. This could be shown in a flow chart, with an explanatory paragraph or two. For example, it might be useful to identify the differences between the SWIFT model and the DNET code. The two codes overlap, and a discussion of the interrelationship between SWIFT and other models would be useful.

Recommendation of appropriate uses and assumptions would also be useful in the manual. For example, considering the ratio of repository dimension to the depth below the surface, one might expect that heat transfer should be treated as a three-dimensional, or least a two-dimensional, problem. The user's manual is an appropriate document for discussion of such alternative uses.

Submodels relating densities and viscosities to pressure are presented in this report. However, these submodels are different from the submodels performing the same functions in other Sandia models (e.g., DNET). While the submodels may be satisfactory, we are confused by the lack of consistency within the Sandia modeling program.

As it is, the user's guide presents a model which does not appear to be "user friendly" due to its detail and versatility. To a great extent, this problem might be solved by a more detailed discussion of uses and a display of sample applications as discussed previously.

Review of Report Chapters

The first chapter of this report offers introductory comments involving primarily the model description, application and historical aspects of the SWIFT Model. The statement of purpose of the report is found at the end of the introduction. It points out that detailed instructions on application of the model, information normally included in a user's manual, is reserved for a companion document by Finley and Reeves [1981].* This report has not

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*Finley, N.C. and Reeves, M., 1981, "SWIFT Self-Teaching Curriculum," NUREG/CR-1968, SAND81-0410.

been included in the review of the Sandia methodology. We would recommend that the two documents be combined or issued together as companions.

The second chapter of the report presents the mathematical models and solution techniques utilized in the SWIFT code. These aspects of the model have been previously considered as part of the review of NUREG/CR-0424.

The third chapter presents discussions of submodels for density, viscosity, wells, waste-leach, salt dissolution, heat loss to overburden/underburden, and a radiation boundary option. This section would be improved if the report included a model-submodel overview prior to discussion of the mathematical basis. This would assist the user in selecting appropriate submodels for use in typical problems without having to first read through all the choices.

Chapter 4 offers application notes to assist in problem set up and selection of user options. The discussions of numerical criteria for dispersion, overshoot, and secular equilibrium are well-written and useful. Additional discussion of spatial effects of nuclide transport on secular equilibrium would constitute an improvement.

Chapter 5 presents a description of the SWIFT program, including program control structure flowcharts and a summary of subroutines used in the model. Sections 5.2 and 5.3, which refer the reader to the companion document (Finley and Reeves, 1981) and microfiche copies of sample problems and program listings, should have been presented in an earlier (e.g., introduction) chapter to allow the reader to refer to them sooner.

Chapter 6, the longest section of the report, gives a very detailed guide to data inputs for the code. The chapter includes discussion of the data input forms, the use of the forms to obtain maps for restart records, and the auxiliary data files. In addition, three appendices are included to give definitions of errors and program variables, and an index of variables. This section of the report offers much useful information in a format that provides for practical and easy understanding and reference. Chapter 6 provides a noticeable improvement in style and layout over Chapter 4 of NUREG/CR-0424.

Summary Comments

The basic review questions regarding model, methodology, and data validity refer primarily to the previous report presenting the SWIFT code (NUREG/CR-0424). The present report, the SWIFT user's manual, is a well-written and useful guide to the SWIFT model.

Major criticisms of the User's Manual are:

a) lack of a discussion of model overview

b) lack of a detailed discussion of uses and applications

c) the necessity of having to refer to a separate document for sample problems

2.4 Review and Critique of NUREG/CR-2343; Risk Methodology for Geologic Disposal of Radioactive Waste: The DNET Computer Code User's Manual

The report is a user's manuals for the Dynamic Network (DNET) code, a network flow model for use in simulating the process of salt dissolution in bedded salt formations. The model contains simulations for a variety of complex processes. Included are the capabilities for simulating processes such as salt creep, subsidence, and thermomechanical effects.

The DNET model was developed to investigage processes near the depository such as salt dissolution and salt creep that could affect the release of redioactive waste to circulating ground water. The DNET model also provides a systematic means for investigating the effects of feedback mechanisms such as thermal expansion, subsidence, fracture formation and fracture closure. For a depository in bedded salt, these mechanisms can act to accelerate or decelerate the salt dissolution process and thus increase or decrease the potential for release of radioactive waste.

In Chapter 2, the physical structure and content of DNET are described in some detail including a cross section of the reference site which motivates the form of the flow network used in DNET. Chapter 3 provides a brief description of the individual subroutines in DNET and a description of the various program and error messages. Chapter 4 contains a description of user input data. Five sample problems are presented in Chapter 5.

An objective of the DNET code seems to be the modeling of a large number of different physical phenomena with relatively short computer running times. With this in mind, the physical processes have been modeled with considerable simplicity. Unfortunately, the authors do not present a systematic derivation of most of the submodels used; that is, one which clearly states what the underlying approximations and simplifications are. It would be useful to specify regimes for which the simple models are adequate. This could be done by comparison with more exact models, or with appropriate experimental results.

Several submodels are discussed: salt dissolution, solution channel formation, dissolution along boreholes, salt creep, the thermal treatment, and fracture formation and closure. The development of each submodel is satisfactory, but could be improved. Simplifying assumptions are noted, but little or no attempt is made to justify these. For example, in Section 2.5, which discusses solution channel formation, it is assumed that solution channels are formed, and then it is assumed that these are uniform and rectangular channels. No justification for either assumption is attempted. Confidence in the use of the DNET model would be enhanced if, instead of only stating the assumptions, the reasoning and justifications for making them were included. The discussion in Section 2.6, on dissolution along boreholes, is better in this respect. The assumptions made are justified by citing experimental evidence, with appropriate references, for modeling the manner in which the dissolution process proceeds. Section 2.8, on the thermal treatment, presents an impressive comparison between one-dimensional results obtained with DNET and two-dimensional results obtained with SWIFT. This builds confidence, and similar comparisons are needed for the other submodels.

The manner in which the various submodels are combined to form the DNET code requires more discussion than is presented in the report. The discussion of the overall structure of DNET is summarized in one figure, Figure 3.1. While the figure is useful, it is hardly sufficient for the unfamiliar reader.

The work represents a significant attempt to model a very complicated physical situation in a simplified, and therefore, tractable and efficient manner.

APPENDIX A

RESUMES OF SUB-PANEL REVIEWERS

BAHRAM AMIRIJAFARI

University of California, Berkeley:	B.S., Chemical Engineering (1964)
University of Oklahoma, Norman:	M.S., Chemical and Petroleum Engineering (1967)
University of Oklahoma, Norman:	Ph.D., Petroleum Engineering (1969)

WORK SUMMARY

Dr. Amirijafari is presently Director of Petroleum Engineering at SAI. He is responsible for projects related to Enhanced Oil Recovery (EOR), Unconventional Gas Recovery (UGR) from various sources such as Devonian Shales, Geopressured/Geothermal reservoirs, Methane from Coal Beds, etc., and Tar Sands. He is involved in directing the development or modification of existing computer models in fields of Hydrology or Reservoir engineering to fit the various related projects. He has been a member of a review committee for NRC since 1980 reviewing reports prepared for NRC by Sandia National Laboratory. He has served as a developer of mathematical and computer models for Gulf Research; managed an engineering consulting firm in Iran; directed the chemical and petrochemical engineering department at Tehran Polytechnic; as an associate professor, teaching Gas Processing and Design and Economics for chemical engineers.

PROFESSIONAL EXPERIENCE

Science Applications, Inc.

Dr. Amirijafari joined SAI Golden, Colorado in January 1979. He is presently the Director of Petroleum Engineering.

Wilmeg, a joint company formed by Williams Brothers Engineers and IMEG.

Member, Board of Directors, in charge of supervision and management of second Iranian Gas Trunkline designed to carry about 2.7 billion scf/day from Kangan in southern Iran to Astara in northern Iran (Russian border). A 1400 kilometer line of 56" and 48" gas transmission line. Also continued to teach natural gas processing and treatment plus chemical engineering design and economics courses at the University in Iran (3/78 - 12/78).

Iranian Management and Engineering Group (IMEG)

Managing Director and Executive Vice President. For IMEG Dr. Amirijafari was managing their consulting engineering dealing with design, engineering, management and supervision of oil, gas, and product pipelines, pressure buildup stations, treatment and processing and supplementary facilities. IMEG employs about 200 people (12/76 - 12/78).

Iranian Management and Engineering

Manager of Engineering. Managing a total of 100 engineers, draftsmen and technical personnel - involved in carrying out the design and detail engineering of projects outlined in project section (6/76 - 12/76).

Manager of Process and Computer Division. Dealing with establishing process and treatment philosophies for various oil and gas processing projects. Also initiating computerization of various process and hydraulic calculations. Also involved with bid preparation and evaluation of bids received for various projects described in project section (6/75 - 6/76).

Process Engineer. Dr. Amirijafari dealt with process and hydraulic calculations for various projects and was responsible for preparation of Process and Instrumentation Diagrams, computerizing process and hydraulic calculations (12/74 - 6/75).

Gulf Research and Development Co., Harmarville, PA

Research Engineer. Responsibilities were development of computer models for thermal secondary recovery processes such as steam injection and air injection and matching results with the computer models (3/69 - 7/71).

Food Machinery & Chemicals Corp. (FMC), Newark, California

Process Technician. He carried out tests and physical model studies to improve product quality (1/64 - 1/65).

Academic Experience

Tehran Polytechnic University (9/72 - 12/78)

Chairman of Chemical & Petrochemical Engineering Department (7/71-12/74). Dr. Amirijafari has taught chemical engineering, natural gas processing and treatment, chemical engineering design and economic courses. The University has an enrollment of 200-300 students.

Oklahoma University

Special Instructor and Research Assistant (1965-1969). Research at the University was on solubility of hydrocarbon gases in water at pressures above 2000 psi.

Stanley L. Basin

San Jose State University: B.A., Mathematics (1962)
University of Santa Clara: M.S., Applied Mathematics (1972)
University of Santa Clara: M.S., Economics (1976)

Mr. Basin is an applied mathematician with considerable experience in the application of statistical methods to the analysis of operational data, probability theory, and reliability modeling. Over the past three years, Mr. Basin has served as a consultant to the Electric Power Research Institute (EPRI). This work included the analysis of data pertaining to nuclear power plant availability and component reliability. He is currently providing EPRI's Fossil Fuel and Advanced Systems Division with technical support in the area of statistical design of experiments and reliability modeling. Mr. Basin has also participated in two projects for the Bay Area Rapid Transit District (BART) involving the collection and analysis of data relevant to operational safety. He has also contributed to a study of the feasibility and benefits of underground nuclear power plants. This work was sponsored by the California State Energy Commission.

Mr. Basin provided the statistical support required in a number of long-range planning studies sponsored by the National Cancer Institute, including a patient referral study performed in cooperation with the Michigan Cancer Foundation.

Prior to joining SAI, Mr. Basin was employed by Science Management Corporation as a Senior Operations Research Analyst. At that time, Mr. Basin was engaged in a variety of studies in industrial systems, including material flow processes, inventory control, and logistics.

Mr. Basin is the author or coauthor of 45 technical publications. He is on the faculty of the University of Santa Clara Graduate Schools of Engineering and Business Administration, and is a member of the Mathematical Association of America, Operations Research Society of America, Sigma Xi, and Beta Gamma Sigma.

Jerry J. Cohen

University of Michigan: B.S., Chemistry (1950)
University of Michigan: M.P.H., Environmental Science (1955)

Mr. Cohen joined SAI in 1981 as manager of the Energy and Environmental Evaluation Division. He has a strong background in Environmental Science, Risk Assessment, Health Physics and Industrial Hygiene. He received a certificate in the comprehensive practice of Industrial Hygiene in 1962. Mr. Cohen serves as an advisor to the International Atomic Energy Agency (IAEA) on shallow land burial of radioactive waste, and is a member of the World Health Organization (WHO) working group on Health Implications of Radioactive Waste Management.

Prior to joining SAI, Mr. Cohen was the Program Director of the Waste Management Hazard Assessment project at Lawrence Livermore National Laboratory (LLNL). He was responsible for the management of outside contractors as well as Laboratory personnel and resources in research evaluating the risk of various radioactive and hazardous waste activities. Mr. Cohen's work in this capacity included the development of waste management hazard indices, and surveys of waste management perspectives and the geotoxicity of the natural environment. He directed efforts toward the developmennt of environental monitoring methodology and procedures, and general strategy assessment for radioactive waste disposal.

Previous work at LLNL included the direction of technical support efforts in waste management, the development of waste form performance criteria and classification systems, and general risk assessment. Mr. Cohen was a research environmental scientist for the Plowshare program and, in addition, worked in hazards control and industrial hygiene.

Mr. Cohen worked at the IAEA International Institute of Applied Systems Analysis (IIASA) joint project on risk assessment during the period 1974-1976. The IAEA/IIASA project was located in Vienna, Austria. At IIASA, Mr. Cohen conducted research in cost-risk-benefit analysis of energy development programs and remote siting of nuclear power reactors.

Mr. Cohen has published extensively in the areas of Risk Assessment, Radioactive Waste Management, Industrial Hygiene, and Health Physics. His professional society memberships include the American Nuclear Society, the Health Physics Society, the American Board of Industrial Hygiene, the American Industrial Hygiene Association, and the American Conference of Governmental Industrial Hygienists.

Ralph R. Fullwood

Texas Technological University: B.S., Physics (1952)
Harvard University: A.M., Physics (1954)
Rensselaer Polytechnic Institute: Ph.D., Nuclear Engineering (1965)
Professional Engineer, State of California (1976)

Dr. Fullwood is a nuclear engineer specializing in energy production and safety. At SAI he has conducted light water reactor safety studies for the Atomic Energy Commission/Nuclear Regulatory Commission, the Electric Power Research Institute, the National Science Foundation, General Electric, Oak Ridge National Lab, Battelle NW Labs, EBASCO, and Bechtel. He has also participated in fast reactor safety analysis projects for the Clinch River Breeder Reactor, General Electric, and the Environmental Protection Agency. Dr. Fullwood's work on analysis of nuclear fuel cycle has included risk analyses for fuel reprocessing, transportation, fabrication, and the pre-closure and postclosure phases of repository performance. This has addressed the U-Pu fuel cycle and a partitioning-transmutation fuel cycle. Other SAI projects in which he has been engaged include application of fault trees to NUREG-0700 control room review; nuclear safeguards work involving the application of fault tree methods to safeguards in nuclear power plants; reliability modeling, e.g., the General Electric electromagnetic pump; isotopic power studies; and human factor analyses. In addition, he contributed to on-line data acquisition studies of brake test data for the Bay Area Rapid Transit (BART) and a reliability data acquisition study for EPRI. He designed and constructed a prototype monitor for perimeter radiation levels and contributed to the design of a prototype portable radon monitor.

From 1966 to 1972, at the Los Alamos Scientific Laboratory, Dr. Fullwood directed the design study of a high-intensity proton storage ring using charge exchange injection. He designed modular instruments for data acquisition in field and laboratory tests on material behavior and specified a large scintillation tank used for capture and fission neutron experiments, neutron-gamma discrimination detectors, and etch-fission fragment detectors.

As associate professor at Rensselaer Polytechnic Institute, Dr. Fullwood taught courses in nuclear physics and radiation detection and instrumentation and supervised the reactor laboratory and the LINAC facility.

Dr. Fullwood has authored or coauthored over ninety publications and numerous reports. He is a member of the American Nuclear Society, American Physical Society, International Solar Energy Society, and Institute of Electrical and Electronic Engineers.

Charles A. Stevens

New York University: B.S., Engineering Science (1957)
University of Michigan: M.S., Nuclear Engineering (1958)
University of Michigan: Ph.D., Nuclear Engineering (1962)
Professional Engineer, State of California

Dr. Stevens is a nuclear engineer with strong interests in nuclear reactor performance and related fields.

Since 1974 Dr. Stevens has participated in nuclear reactor safety analysis. He has been involved in accident consequence predictions for light water reactors, and he has managed several programs dealing with LMFBR safety. He has also participated in nuclear fuel cycle studies. He has lead a program on an assessment of the safety of the nuclear waste cycle and he has been involved with a reassessment of the SL-1 incident.

Dr. Stevens was a founder of SAI in 1969 and initially worked on time-dependent radiation transport analysis using Monte Carlo and other techniques, as well as nuclear source term estimations from nuclear devices. He has been program manager for several nuclear weapons effects research programs, including electromagnetic pulse phenomenology and the prediction of ground motion following a nuclear explosion. As part of this work, he developed a three-dimensional, time-dependent coupled neutron-gamma transport code, using the Monte Carlo method. He has published research work in the area of controlled thermo-nuclear reactors.

In addition to his work as an engineer and technical manager, he has experience in dealing with complex financial, legal, and organizational issues. As a member of the SAI Retirement Plan Committee, he has responsibility for the administration and investment of employee retirement funds. He is the Chairman of the Acquisition Committee, which seeks, evaluates, and negotiates with potential mergers and acquisitions candidates. He has served as Corporate Secretary and Assistant to the President. Currently, he is manager of the experimental engineering department and an SAI Corporate Vice-President.

From 1964 to 1969 Dr. Stevens worked at General Atomic where he was engaged in reactor physics research, nuclear model calculations, and nuclear design calculations for the HTGR and GCFR programs. He developed the GAROL and GAR computer codes to analyze resonance capture for thermal and fast reactors. He analyzed fast subcritical assemblies for comparison with experiments and he was principal investigator for preliminary design of the accelerator booster fast pulsed reactor project.

In the early 1960s, Dr. Stevens worked on the NERVA nuclear rocket project at Westinghouse, concentrating on reactor design methods development. He has worked on heat transfer calculations in support of a sodium-deuterium reactor concept at the United Nuclear Corporation, and he has carried out reactivity worth calculations for the ZPR fast critical assemblies while at Argonne National Laboratory.

Dr. Stevens is a member of the American Nuclear Society and has authored some thirty technical publications.

APPENDIX B

Individual reviewers' comments on NUREG/CR-1636 Volume 4, (SAND79-1909), Risk Methodology for Geologic Disposal of Radioactive Waste: Effects of Variable Hydrologic Patterns on the Environmental Transport Model, by Jack B. Brown and Jon C. Helton.

REVIEWER: BAHRAM AMIRIJAFARI

a. General Comments

1. As a report, it is well-written. However, it discusses the theoretical aspects of the subject matter more than is necessary to achieve the objectives of the report.

2. This report is based on NUREG/CR-1636, Volume 3. Hence, any comments regarding that report which appear in Appendix D of NUREG/CR-1672 (SAI-288-82-PA) also apply here.

3. Considerable amount of work could have been saved if tables instead of graphs were used. Further discussion of this point is given below (see specific comments).

4. A much simpler approach could have been taken to accomplish the authors' objectives. The Environmental Transport Model is too complex to begin with; as a result simplifications could begin there.

5. The authors have done a very thorough job in discussing the theories, proving theorems, etc. If derivations and proofs, etc. were put in an Appendix, it would be much simpler to read and understand the report.

b. Specific Comments

1. Page 10. Use of a Greek letter which has not been used earlier in this report instead of "j" in the equation following (2.3) is recommended to avoid any possible confusion.

2. Page 18. In Figures 3-5 to 3-8 both EZ and EZ+SDZ are represented by dotted lines. It is not clear which dotted line is EZ and which is EZ+SDZ. The same comment applied to Figures 3-9 to 3-12.

3. Pages 18 & 19. In the discussions of the computational results, it is recommended that individual figure numbers be given as they are discussed.

4. Page 20. Except for the figures which show the behavior of X and SX, the asymptotic value of X, for the different subzones in the river or the lake zones, it may have been better to present the results in tabular form, rather than graphical form. Many pages and much drafting effort could have been saved. Instead of 16 graphs (32 pages), only 4 tables would be necessary. For example:

   Table 3-1 Input to the Groundwater Subzone

   Table 3-2 Input to the Soil Subzone

   Table 3-3 Input to the Surface Water Subzone

   Table 3-4 Input to the Sediment Subzone

   Tables 3-1 to 3-4 would each contain the following information:

	River Zone								Lake Zone							
	Year 100				Year 1000				Year 100				Year 1000			
	X	Y	EZ	EZ+SDZ	X	Y	EZ	EZ+SDZ	X	Y	EZ	EZ+SDZ	X	Y	EZ	EZ+SDZ
GW																
SOIL																
SW																
SED																

   These tables would provide a very quick and efficient means of comparison.

   Figures 3-7 (100 yr.) and 3-11 (1000 yr.) for the Surface Water Component of the Surface Water Subzone look quite different from the other graphs for the River Zone. However, Figures 3-19 (100 yr.) and 3-23 (1000 yr.) for the SW component of the SW Subzone for the Lake Zone do not look similar to their counterparts in the River Zone. The explanation on page 20 is not sufficient to describe the difference.

5. Page 72. The Equation $y(t) = \int_0^t$ ... is the same as Equation (4.4). It is not derived from (4-4). I do not see how the authors got y(t) = x (t) exp. (1-max p)a. Integrating the right hand side of the inequality gives x(t) exp. (1-max p)a. Why does the inequality change into an equality?

6. Page 73. The same comment as on Page 72 applies here. Why did the inequality change into an equality? It is important to note that s, t, and p are independent variables. Equation

(4.8) shows that both comments are correct; i.e., $y(t) \geq x(t) \exp[(1-\max p)a]$ and $y(t) \leq x(t) \exp[(1-\min p)a]$.

Referring to Equation (2.5) as the definition of Q is not exactly correct. Usually the variable to be defined is on the left hand side of an equality and is singled out, i.e., it stands alone. Equation (2.5) is a definition of p.

7. Page 74. Showing that $y(t_1) \leq x(t_1)$ and $y(t_2) \geq x(t_2)$ and then concluding that there must exist a number t between t_1 and t_2 where $x(t) = y(t)$ is only true if x and y are both continuous functions of t. This has not been made clear in the report.

REVIEWER STAN BASIN

a. General Comments

1. The authors have done a credible job of developing some understanding of the effects of periodic and/or stochastic variations in hydrologic phenomena on the predictions made by the environmental transport model. The basic question which the authors address is whether it is necessary to account for periodic and/or stochastic variation in water and sediment flow rates when predicting radionuclide concentrations over long periods of time. In the course of their analysis the authors have provided a rather nice summary of the compartment model used to represent radionuclide movement.

   The approach taken by the authors was to study the behavior of systems of differential equations that included terms which account for periodic variations of the transport matrix A in the original system of differential equations, namely:

$$\frac{dX}{dt} = AX + R,$$

   Three alternative models were considered:

$$\frac{dY}{dt} = p(t)AY + R \qquad (1)$$

$$\frac{dY}{dt} = (p(t)F + D)Y + R \qquad (2)$$

   and

$$\frac{dZ(t,w)}{dt} = S(t,w)p(t)AZ(t,w) + R \qquad (3)$$

Motivation behind the selection of the alternative models was not made clear. The authors did mention that such representations of A are common in biological and ecological modeling. Three references were given. A general description of the maxtrix A and the asymptotic values of the solution vectors X and Y are given in Chapter 2.

No errors have been found in the mathematical development. However, a number of improvements can be made which will add to the clarity of the presentation, namely:

1. The material currently presented in Chapter 6, Pages 94 - 95, should be moved to the front of the report. This would provide the reader with a better idea of what the objectives of this report are and what to look for throughout the mathematical development.

2. The chapter headings should be made more descriptive. For example, the title of Chapter 2, which currently reads, "General Description of A, X, and Y", might be changed to "General Properties of Completely Open Systems".

3. Clarity could be improved greatly by providing smoother transition between chapters. In particular, there is quite an abrupt transition between Chapters 2 and 3. It would have been nice to have some discussion relative to where and how the authors planned to use the results of Chapter 2 in the remainder of the report, if at all.

b. Specific Comments

1. Page 1, Paragraph 1. The authors talk about variable hydrologic patterns. Perhaps the terms time varying patterns or temporal patterns and stochastic variations would be more descriptive.

2. Page 2 (Top). This reviewer is, once again, (see comments in NUREG/CR-1672 (SAI-288-82-PA) pages B-8 and D-8) bothered by the fact that the method of subscripting the variables, namely X, where $L = 4N(I-1) + 4(J-1) + K$, does not yield a unique correspondence between radionuclides (J), subzones (K) and zones (I). In particular, consider the situation in which the integers N_1, N_2, I_1, I_2, J_1 and J_2 satisfy the equation

$$N_1 (I_1 - 1) + J_1 = N_2 (I_2 - 1) + J_2,$$

for example, $N_1 = 3$, $I_1 = 6$, $J_1 = 5$, $N_2 = 5$, $I_2 = 3$, $J_2 = 10$. If we let $L_1 = 4N_1 (I_1 - 1) + K_1$ and $L_2 = 4n_2 (I_2 - 1) + 4 (J_2 - 1) + K_2$ then $L_1 = L_2 = 77$ for $N_1 = 3$, $I_1 = 6$, $J_1 = 5$, $K_1 = 1$ and $N_2 = 5$, $I_2 = 3$, $J_2 = 10$, and $K_2 = 1$.

3. Pages 2 & 3. The terms zone and subzone are clear, However, when the authors talk about compartments and use the terms compartment and subzone interchangeably, a certain degree of confusion results. This is particularly true in the caption to Figure 1-2 on Page 3 where the authors make the following statments,

   "Each subzone has N compartments associated with it. Each radionuclide has 4M compartments associated with it."

   I believe this should read as follows:

   Each zone has 4N campartments associated with it, 4 subzones per radionuclide combined with N radionuclides; also each radionuclide has 4M compartments associated with it, 4 subzones per zone combined with M zones.

4. Page 9. Use of the notation SX and SY to denote the asymptotic solutions to 1.1 and 1.2 is confusing. S may be interpreted as a matrix operator. Why not use a subscript such as X_a or Y_a? The notation X_∞ and Y_∞ would be another, less confusing, alternative. This notation appears throughout the report, specifically on Pages 10, 13, 14, 17, 81, 82, 85, 86, 87, 88, and 93.

5. Page 10. The concept of convergence is first mentioned on the top of Page 10. The definition, provided in the form of a footnote, appears on Page 12. It would be helpful if the footnote was moved to Page 10.

6. Page 19, Paragraph 3. In the third sentence of Paragraph 3, the authors refer to the fact that Y_3 oscillates between $1/(\max p)SX_3$ and $1/(\min p)SX_3$. It would have been helpful if they had referred specifically to Figure 3-7 on Page 33 and Figure 3-19 on Page 57. In general, whenever referring to figures or tables that appear much later in the report, it would be helpful if the authors would provide the page numbers where the figures or tables may be found.

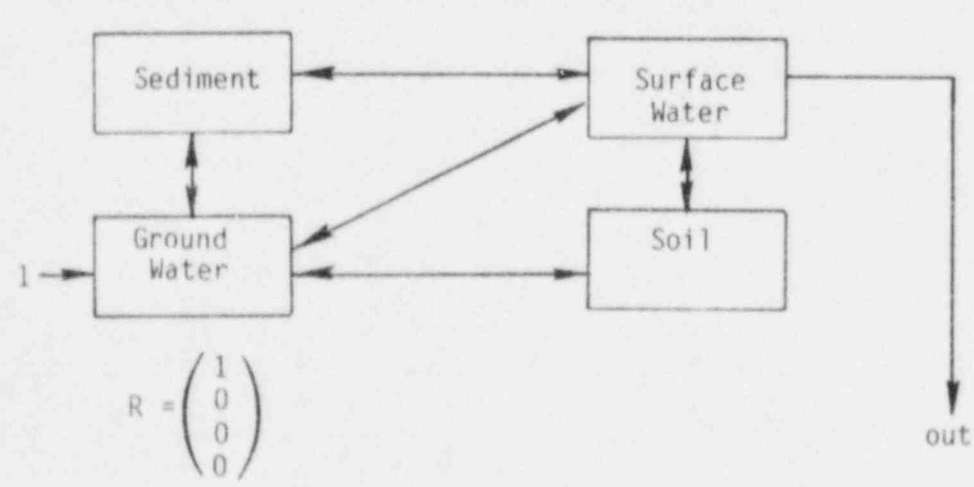
7. Page 19, Last Paragraph. The authors state that comparisons of X and Z are now considered. I believe that Z should be EZ, the expected value of Z. This also applies to the statement in the first line on Page 20. It is not exactly clear how Chebyshev's inequality is being used when comparing X with Z. As a matter of fact most of the material on Page 20 is not exactly clear. In particular, it is difficult to see where the multipliers 1.2, 1.8 and 12 that appear in Lines 8, 5 and 3 from the bottom of Page 20 come from. Perhaps the source of confusion, at least as far as I am concerned, comes from the fact that Chebyshev's inequality provides a two-sided bound on $Z_k(t)$, specifically,

$$Pr[|Z_k(t) - EZ_k(t)| \geq 2SDZ_k(t)] \leq 0.25.$$

The authors are somehow using this, to derive a statement concerning the deviation of X(t) from EZ. The exact manner in which they are using Chebyshev's inequality is unclear.

8. Pages 21 - 68. The captions associated with each of the figures are very confusing at first glance. It takes some work on the part of the reader to figure out what is going on. First of all, the reader must recall that the authors are describing a very simplified situation, namely the case involving one zone and one radionuclide. This is mentioned on the top of Page 4. Second the reader must recall the statement on the bottom of Page 17 which mentions the fact that each case involves a unit rate of input (one atom/year) in each subzone. All of this should be made very explicit. One way to do this is to include the following vectors in the captions of the figures: R = [1, 0, 0, 0], R^T = [0, 1, 0, 0], etc.

An even more explicit graphical description could be given at the beginning of each sequence of figures such as:



The latter suggestion may, in fact, be an "over all".

9. Pages 21 - 68. The following comments pertain to the rather large collection of figures, 96 figures to be exact, that appear on Pages 21 through 68.

   (a) Although some of the general features of the functions $X(t)$, $Y(t)$, $EZ(t)$ + $SDZ(t)$ are discussed on Pages 19 and 20, this reviewer was left with many more questions than answers relative to the behavior of these functions. For example, the figures which describe the behavior of the various functions over a one year period at 100 years and 1000 years suggest an oscillatory or wave-like variation. This is verified analytically, at least in the case of the scalar differential Equations 4.1 and 4.2 on Pages 73 and 74. It may have been more informative if, instead of a one year period at 100 years and 1000 years, the authors had presented a graphical display spanning two or five or perhaps ten years. Each of the figures which describe the behavior of $X(t)$ over a one year period at 100 years or 1000 years represents an exploded view of a one year segment of one of the previously displayed graphs which shows the asymptotic behavior of $X(t)$. Perhaps it would have been more informative if the authors had placed these figures on one page or on facing pages in order to facilitate visual comparison. For example, the soil component plot in Figures 3-1, 3-5 and 3-9 should be placed on the same page for visual comparison. Several other combinations of figures should be placed on the same page for visual comparison. For example, the reader may wish to compare the GW component plots with different R vectors.

   (b) It is the opinion of this reviewer that the use of fewer figures, coupled with a more thorough discussion of the physical significance or explanation for the behavior, would be more effective than the presentation of 96 figures all at once. The full collection could be presented in an appendix for those readers who may wish to compare different combinations of figures.

10. Page 71. The first equality in Equation 4.6 is not exactly obvious; a little explanation would certainly have assisted this reader.

11. Page 93. "$Q(t,\omega) = SX_3/S(t,\ )p(t)$" should be $Q(t,\omega) = SX_3/S(t,\omega)p(t)$

REVIEWER: JERRY COHEN

a. General Comment

1. Although the project's purpose is discussed in the final chapter (Summary and Conclusions), a succinct statement of study objectives should be included in the introduction. Appropriate points to discuss when addressing the study objective might be:

   o Why do we need to know about the effect of hydrologic patterns?

   o What physical basis is there that suggest that normal variations in hydrologic patterns might affect environmental transport for a deep burial situation? If such physical processes have not been identified in advance and factored into the model itself (through appropriate compartmentalization and definition of intercompartmental transfer parameters) then it appears very unlikely that valuable new insights will be revealed.

   o Inclusion of periodicity or stochastic variability would appear to reflect a level of detail in the modeling of environmental transport not matched by other aspects of the modeling effort. For example, in the last paragraph on page 6, several assumptions are discussed.

3. In general, the report is well written and understandable from the standpoint of work that was carried out.

b. Specific Comments

1. Page 1, Paragraph 3. The following statement was made, "In the following, R is assumed to be constant". Although the reason becomes clear several pages later, it would be useful to offer an explanation at the outset.

2. Page 2 (Top). The equation and discussion is somewhat unclear and would benefit from rewriting and elaboration. An expanded description of each parameter in the formula would help in avoiding confusion. This would also lead to a better understanding of the point of the discussion: i.e., the numbering scheme used for compartment identification.

3. Page 4. The use of the symbol "L" to represent the unit "liter" is unusual.

Included in the discussion of a 4 x 4 matrix whose elements are the terms a_{ij} is the statement that "For i = 5, the term a_{ij} defines a movement ..." This is confusing.

4. Page 5. Equations 1.2 and 1.3 are introduced with no immediate identification of how they will be used. Although this becomes clear in the subsequent pages, an immediate comment would be useful.

5. Page 6. The terminology A(t,•) is confusing for the introductory chapter and should be deferred for detailed explanation in a subsequent section.

6. Page 7. The discussion describes simplifying assumptions that eliminate the consideration of radioactive decay chain phenomena. This results in ignoring potential decay chain partitioning, a process that may or may not be important. However, since the handling of differential behavior of decay chain members is an important capability of the Environmental Transport Model (e.g., see Figure 1.2), it seems inappropriate to apply such simplifying assumptions when minute hydrologic parameterization is included.

7. Page 17. The values v_i are assumed to be lognormally distributed with $E(v_i) = 1.0$, $Var\ (v_i) = 0.25$, and a month-to-month serial correlation of 0.5. No basis for these selections is given, except that they were made after an examination of the literature. The reader might wonder if these selections correspond with historical data.

8. Page 77. It is stated that "The indicated patterns for direct radionuclide input to the surface water subzone are probably the most meaningful (i.e., revealing) of those presented", yet no discussion of why this is so is offered. In fact, the report would greatly benefit from an expanded discussion of the meaningfulness and implications of all the study results.

9. Page 92, Third Paragraph. Identifies an anomalous situation where the study results contradict the expectation that "the mean of the stochastic model exceeds the deterministic model evaluated at the mean rates". This point needs reconciliation.

10. Page 92, Last Paragraph. Indicates that there should be no concern about the practice of using "X" as the model. What basis or criteria were used in arriving at that conclusion?

11. Page 94. The purpose of the study to "provide a feeling as to how a real system might behave" is stated. Yet the "feeling" they arrived at is not discussed. This information might be interesting and insightful.

12. The study represented by this report considers the effect on the Environmental Transport Model of increasingly sophisticated and detailed representations of hydrologic input, ranging from annual average values, to periodically variable annual rates, to a periodic/ stochastic representation. Missing from the report, however, is a serious discussion of what basis there might be for expecting this increasingly complex representation to provide useful additional insights.

REVIEWER RALPH FULLWOOD

a. General Comments

As we have noted in the past, the report is well written; however, I suggest the use of a better format. Placing the summary up front is generally considered to be helpful in deciding if the document should be studied extensively. In this regard, the Table of Contents headings are not helpful in providing guidance to the document contents.

An additional comment is that the report is quite abstract and the authors do not relate conclusions to the physical reasons for these conclusions. The effect of annual periodicity may be small compared with long-term cycles and acyclic effects.

Page 1. (General) It would assist the reader to be given an overview of the document at the beginning of the introduction. The reader knows from the abstract that the document is concerned with fluctuating (periodic or semiperiodic) flow rates in the Environmental Transport Model. The reader does not have any idea as to how these fluctuations will be mathematically treated. Furthermore, it is only reasonable for the reader to wonder why these fluctuations would not be smoothed by the fairly long time constants of the Environmental Transport Model. In addition, since our knowledge of the mean values of the flow rates in the future is faulty, how can we expect to know the time dependent variations any better?

b. Specific Comments

1. Page 2, Paragraph 1. The need for and the uniqueness of L has never been well explained.

2. Page 4, Paragraph 1. The authors could assist the reader by explaining the physical meaning of the a_{ij}'s. The ratio RS_{ij}/MS_j is the per unit mass rate of flow from j to i so (S_j)

RS_{ij}/MS_j is the rate of sorbtion of radionuclide per unit mass in subzone j. Similarly RW_{ij}/VW_j is the water rate of flow per unit volume from j to i so $(1-S_j)\ RW_{ij}/VW_j$ must be the rate of desorbtion of the radionuclide in subzone j. Presumably a_{ij} is a measure of the net accumulation of the radionuclide j due to mass and water movement between subzones j and i.

3. Page 4, Paragraph 1. What is the physical meaning of S = 1 and S = 0? If $VWj \to 0$, or if $KD \to \infty$, or if $MS_j \to \infty$, then $S_i \to 1$. Apparently $S_j = 1$ means that the radionuclide does not move.

4. Page 4, Paragraph 1. If the third equation is true does that mean that $i \neq j$ for the first equation on this page?

5. Page 4, Paragraph 1. We were told that A is a 4 x 4 matrix, yet toward the bottom of the page, the authors talk about i = 5. This does not seem consistent. The authors seem to be saying that the a_{jj} elements represent movement from subzone j out of the zone entirely. Perhaps this would have been better than discussing i = 5.

6. Page 5, Paragraph 2. This contains the statement of purposes that should be presented earlier because of its importance. It is:

   "This study examines the rates at which radionuclide buildup occurs when this variability in A is taken into account. The desire is to determine if such variability has sufficient effects on predictions by the Environmental Transport Model to require some method for its incorporation into model predictions. In investigating these processes, the intent is to perform bounding calculations to develop a feeling for the extent of their effects rather than to develop detailed models which incorporate these effects into the Environmental Transport Model".

7. Page 5, Paragraph 2. Y is not defined. Presumably it represents the number of atoms of a particular radionuclide present at time t under conditions of stochastically periodic varying flow rates. If this is so, the authors should state it because equation 1.2 begins the new work in this report and is very important.

8. Page 5, Paragraph 3. The authors should point out that they are assuming an annual periodicity for the flow rates even though there may be periodicities having larger periods, such

as sun spots. There also seems to be a periodicity associated with the ice ages. These longer periodicities may cause major effects that are not smoothed by the system time constants.

9. Page 6, Paragraph 1. Presumably the negative diagonal element corresponds to decay transfering mass out of the zone and the positive off-diagonal elements correspond to flows into the zone via the daughters.

10. Page 8. the authors could select a more descriptive title for the chapter than, "General Description of A, X and Y". A candidate title might be General Description of the Theory of Periodic Environmental Transport.

11. Page 8, Paragraph 1. The authors should indicate that the summation is on the index i. In fact, it appears to be

$$d_j \geq \sum_{i=1}^{j-i} a_{ij}$$

(since the upper triangle is zero).

12. Page 8, Paragraph 1. Is there an importance associated with a compartment system being "open", "closed" or "completely closed"? The emphasis given these definitions implies such. If so, the physical importance should be explained. Are systems normally encountered in nature completely open? Furthermore, whether a system is closed or completely open would seem to depend on the time scale of consideration. All systems are closed if the time is short enough; all systems are open in infinite time.

13. Page 10. General comment. I find the mathematics very impressive, but why it is necessary to go to this extent to investigate the effect of annual periodicity on the Environmental Transport Model is not clear.

It was stated on Page 9, "the general mathematical nature of the solutions to (1.1), (1.2), and (1.3) is now discussed". The information that follows obscures the nature of these solutions that is found.

For example the nature of the solutions to (1.1) are examined on Page 10 and it is found that if $V_0 = X(0) \geq 0$, then $e^{At} V_0 > 0$. What is surprising about this? How can $e^{AT} V_0$ be negative? I do not see how the nature of (1.1) has been explored.

For equation (1.2) and (1.3), the authors assert a unique solution is given by (2.4) using ϕ as the homogeneous solution. This is used to show that there is a unique periodic asymptotic solution to which all solutions of (1.2) converge. In the end, I fail to understand the significance of the uniqueness of the solution or the asymptotic character. One would not expect nature to have multiple solutions. Similarly, since all radioactive material must decay, both X and Y must go to zero as time increases without bound. Since we are only dealing with first order linear differential equations, isn't there a simpler way to solve them?

14. Page 14, Paragraph 2. On the basis of the information given, how are the matrices A formed?

15. Page 15, Paragraph 1. p(t) is not so much the hydrological pattern as the periodicity of the hydrology. After all, it only has one variable.

16. Page 16, Paragraph 1. This technique for investigating the statistical properties of the stochastic flow process is very good. I expected a Monte Carlo simulation, but this analytical method is better. The authors should give their reasons for assuming a log-normal model.

17. Page 16. It would be instructive if the authors would provide a sample calculation to show numerically how they set up a problem as well as exploring the results of the calculation.

18. Page 21. et. seq. These graphs are very difficult to interpret. First the definition of X is not clearly stated. Referring back to page 1, the authors state that $X_i(t)$ denotes the number of atoms of a particular radionuclide which is present in the i-th subzone (groundwater - 1, soil - 2, surface water -3, and sediment -4). We also know from previous reading that this is based on 1 atom/year input (Page 17). If this be the case, what is the meaning of a GW scale going to 4000? Is this the number of atoms present in this subzone? The captions for Figures 3-1 through 3-4 are for groundwater, soil, surface water and sediment, but these are also the cyclic order of the ordinates plotted on each set of figures. This deserves some explanation.

19. Page 29. Do the authors have any explanation as to why EZ, which is the expected value from Monte Carlo simulations, deviates markedly from the X solution (the solution using average data). This departure is primarily in the lower figure on this page. Is this deviation a statistical effect as would be implied if EZ-SDZ were also plotted; i.e., this would place both X and Y within the uncertainties.

20. Page 30. What causes the hump at year 100.4? Is this due to the increased wetness assumed in p_i on Page 15? Does this occur every year?

21. Page 69. The data in this table should not be taken as representative of a typical site. The data were somewhat contrived as was discussed on Page 15.

22. Page 70. For heuristic purposes, it might be simpler to assume a periodic dependence for p, such as a sinusoid, and solve 4.2 directly.

23. Page 74. I find this work on the bounds of y quite interesting. One would think, on physical grounds, that the bounds would also depend on the zone time-constants compared with the periodicity.

24. Page 74. If the graphs of functions presented in Section 3 had been extended for several cycles, they presumably would illustrate the periodicity of Y and its equality with X at t = 1.

25. Page 80, Paragraph 1. Apparently "identify matrix" should be "identity matrix".

26. Page 94. The Summary and Conclusions could be expanded to include the major accomplishments of the report such as the bounds on Y and the physical reasons for the results obtained which may be explained by time-constants controlling the approach to equilibrium (they are in the A matrix - I think).

    I think the conclusions could be misleading because at a time of flood, the flooding water may cover a very wide area, thereby changing deposition patterns to compartments not normally accessible and possibly causing surface deposition followed by dryout and atmospheric dispersion. Such major system perturbations make conclusions that are obtained from mean values invalid.

REVIEWER: CHARLES STEVENS

a. General Comments

1. The report is well-written and provides an impressive demonstration of mathematical expertise.

2. Assuming that the source R is constant simplified the mathematics, but there seems to be no physical basis for it.

3. It is hard to imagine why one would expect seasonal variations to have a significant impact on a repository over a long period of time. The effect of seasonal variations could be examined more easily by assuming a periodic variation while, at first, ignoring the stochastic aspect. In order to include stochastic effects at the start, and still limit the mathematical complexity, the authors assume that radioactive decay rates vary like the hydrologic patterns. The introduction of such nonphysical assumptions is undesirable but probably does not affect the authors' conclusions that seasonal variations are unimportant.

APPENDIX C

Individual reviewer's comments on NUREG/CR-2324 (SAND81-2516), User's Manual for the Sandia Waste-Isolation Flow and Transport Model (SWIFT) Release 4.81, by Mark Reeves and Robert M. Granwell.

REVIEWER: BAHRAM AMIRIJAFARI

a) General Comments

1. It is very difficult to write a report which describes a complicated computer model that the authors are very familiar with themselves and wish the reader or the user to understand. I believe the authors of this report have done a good job of writing a User's Manual for a computer model such as SWIFT. Specifically, Section 6.1 is well done in introducing the Input Data. Appendices A and B are also very helpful.

2. The report does not contain a sample of typical output based on a corresponding typical input values. Including typical input values, this can be very helpful to a user.

3. The equations in Chapter 2 are the same as those described in earlier reports by Sandia. My comments on the earlier reports still applies.

4. I still do not like the choice of notation. Specifically when q designates both heat and fluid flow, and k is used for permeability, equilibrium absorption distribution constant, retardation factor, and hydraulic conductivity.

5. This User's Manual is very concise and useful; however, it is difficult to believe that someone totally unfamiliar with the SWIFT model would be able to utilize it only using the information in this manual. It is understandable that a more comprehensive manual would be quite lengthy.

b) Specific Comments

1. Page iii. The report does not include the output in the Abstract or the Table of Contents. In documenting any computer model the description of the output is an integral part of the documentation.

2. Page v. In the Introduction Chapter (Chapter 1), it would seem more logical to have 1.4 "Purpose of this Document" come first.

3. Page vii. 4-1. Definition given in the content is not the same as the one given on page 38. "Grid-Block Centers, r and Grid-Block Boundaries, r" should be "Grid-Block Centers, r_i, and Grid-Block Boundaries, r_i".

4. Page 3. The formulation here is the same or very similar to those developed by Dillon, et al., NUREG/ CR-0424. I had several comments then which are included in SAI's report NUREG/CR-1672, September 1980 and will not repeat them here.

5. Page 4. Equation (2-3). q_c = should be q_C. Equation (2-4) $\underline{\underline{E}}_c$ should be $\underline{\underline{E}}_{ci}$.

6. Page 9. Last sentence "All terms ...." A designation, like a star, etc. is missing. This sentence is related to Equation (3-1).

7. Page 11. Figure 3.1 "pur" should be "pure". In the notation ρ = 100 Bar, ρ = 1 Bar. ρ should be replaced by a P.

8. Page 21. Equation (3-14) An operation symbol is missing between r_w and 1 (the first two terms) on the right hand side of the equation. Last sentence "is here" should be "here is".

9. Page 25. In Equation (3-23) q must have s superscript "it"; otherwise $q/\sum_k q_k^{it-1}$ will be equal to 1 according to Equation (3-19).

10. Page 29. Last paragraph, 5th line, subscript "1" is dropped from "k".

11. Page 30. Equation (3-27). $T(x,y,z, = L,t) = T_o(L)$ is incorrect. It should be either $T(x,y,z = L,t) = T_t(L)$ or $T(x,y,z = L,t=o) = T_o(L)$.

12. Page 31. Last paragraph: q_{L1} should be q_1 and q_{L2} should be q2.

13. Page 32. Equation (3-35) applies only when a black body is surrounded by another black body, not just "A body" as stated in the first line of the first paragraph.

14. Page 33. 2nd paragraph, 10th line. Reference date 1966 does not correspond to the date given in the reference list, (1964).

15. Page 35. Table 4-1 is not well described.

16. Page 44. Figure 5-1 shows that if the answer to condition 1 is "yes", the answer to condition 14 must also be "yes" before a plot is produced for a previous run. Is this intentional or is it possible that the flow chart needs to be corrected?

17. Page 45. It seems that a condition could be added to determine whether it is necessary to run the heat loss (HLOSS) calculations or not? As flexible as the SWIFT model is, one can think of situations where heat transfer calculations may not be necessary.

18. Page 52. It is suggested that Table 5-1 be given before Figure 5-1 through 5-4. This will make it easier to understand the structure of the MAIN, ITER, ITERS, and ITERC.

19. Page 53. Are definitions of PRINT 1 and PRINT 2 switched or is it a typographical error, or simply not in order?

20. Page 56. READ M-2 (7I5). The format provides for reading 7 parameters; however, 8 parameters are listed and defined.

21. Page 63. The description of the categories of input data (Section 6.1, p. 55) is great. It is suggested that the definition of variables and their values on the M cards, R0, R1, I, etc. be separated. For instance, READ M-3 is followed by READ R0-1, READ R0-2, and then comes READ M-4 and READ M-5 followed by the "I" cards. I do not understand why the M and R0 cards are mixed.

22. Page 74. Why are R1-24 and R1-25 skipped? Is it possible they are left out?

23. Page 83. READ (R1A-1) (715). It should be (7I5).

24. Page 95. Card numbers R2-10.5 and R2-11.5. Where these afterthoughts? It is a different way of numbering data cards.

25. Page 109. References are not numbered. In the text they are referred to by the author's name and the year, e.g., [Carslaw and Jaeger 1959]. This is the 3rd or 4th referencing method I have seen in the reports. It would be helpful if a uniform method were adopted.

26. I suggest a summary table giving the number of input data cards in each category as follows:

Type of Input Card	M	R0	R1	I	R1A	R2	P
Number of Cards	7	2	23	4	9	20	4

REVIEWER: J. COHEN

1. This report is well written guide to the use of the SWIFT code. An important missing element to the manual, however, is sample problems with input and generated output to demonstrate the use of the code. It is noted that such information is

presented elsewhere in NUREG/CR-1968, SAND81-0410, "SWIFT Self-Teaching Curriculum". Consideration should be given to combining these documents.

2. The nomenclature of Chapter 2 is somewhat confusing. To some extent, this results from the complex nature of the mathematical models being described. However, there are some specific terms which are not defined.

   (a) In Equation 2-4, k_{ij} is not defined and is easily confused with the variable k_i in the same equation.

   (b) In Equation 2-5, the subscript C is not explained.

   (c) In Equation 2-13, ρ_I and ρ_N are not defined.

   (d) In Equation 3-3, k_s is not defined.

REVIEWER: RALPH FULLWOOD

a. General Comments

Of the three reports being reviewed in this phase, this one is the best organized. It is difficult to write a user's manual, but this one is written to aid the reader. It begins with a review of the mathematical model, then goes to the submodels and has a section on application notes, then discusses the program structure and input data forms. There is a section on notation and appendices summarizing useful information.

b. Specific Comments

1. Page 3. The equations look very much like the equations of hydrodynamics. One wonders why there is no reference to some of this work. The CONTEMPT code which is decended from PISCES is a hydrodynamic code that has been used for waste repository analysis.

2. Page 15. It seems strange that case III only crosses the experimental data once, although 2 data points were provided.

3. Comment. So far it seems that SWIFT does everything that DNET does and does it better. Why was DNET written and how does one decide when to use one or the other?

4. Page 16, Paragraph 3. The leach rate is a function of the leaching surface area as well as several other variables. This does not seem consistent with the constant leach rate. Is "solubilizing" the same as dissolving?

5. Page 17. Equations 2-4 or 3-7 do not appear to allow for isotopes that are transported and then decay, since they do not show a geometry dependence. The change in chemical character could strongly affect the transport of the waste. This point seems to be brought out by the remark that the decay calculation may be performed externally using ORIGEN.

6. Page 21. Equation 3-14 is strange. Is it r_w . 1 or is a bracket left out? The latter explanation would seem to be correct from dimensional analysis of the equation.

7. Page 25, Paragraph 2. How does the user know whether the situation is rate limited or pressure limited or makes a transition between those in the course of the modeling?

8. Pages 30-32. Considering the ratio of the repository dimension to the depth below the surface, one would think that the heat transfer would be a 3-dimensional problem, or at least 2-dimensional.

9. General Comment. SWIFT is a very impressive code, but because of its detail and versatility, it does not appear to be "user friendly". Perhaps this would be dispelled by a typical problem setup. The waste itself is fairly standard and the primary thing a user would be inputting is information about the geology, the location of the repository, details of the waste, canisters, overpack, location of the surface water, etc. An interactive preprocessor could be used to assist its use by other than the specialist who wrote it. This seems to be needed for it to be useful for licensing unless SWIFT is to be primarily a code tester for specialist use.

REVIEWER: CHARLES STEVENS

a. General Comment

Both reports NUREG/CR-2324 and NUREG/CR-2343 present submodels which relate densities and viscosities to pressure, temperature, and salt concentration. However, the submodels appear to be satisfactory, it seems an unnecessary complication to have different ones in the two reports. It adds an element of confusion in making comparisons between SWIFT and DNET.

b. Specific Comments

1. Page 21. There appear to be some brackets missing in Equation (3-14).

2. <u>Page 24</u>. This reader is confused by the comment, "For convenience, indices i and j, which locate the well itself, will be suppressed throughout the remainder of this note." What note is being referred to? Also, up to this point indices i and j have been used to denote components of the waste, not the location of a well.

APPENDIX D

Individual Reviewer's comments on NUREG/CR-2343, Risk Methodology for Geologic Disposal of Radioactive Waste: The DNET Computer Code User's Manual.

REVIEWER: BAHRAM AMIRIJAFARI

a. General Comments

1. The report is well written, especially chapter two which deals with the structure and content of DNET. However, it has missed its major goal as being a User's Manual for the DNET code. It is doubtful that anyone could use the DNET code by only having access to this User's Manuals. For one thing, the codes are not given in the text or the appendices.

2. The authors treat the various physical phenomena simulated by the DNET in a very simplified fashion. This is a big plus especially if their results agree within an acceptable accuracy with other codes which use a very sophisticated formulation. They have shown this to be true when they compare their results of temperature distribution with that from SWIFT. They also need to show this for the way they treat Dissolution Along Boreholes, Salt Creep (Solution Channel Closure and Borehole Closure), Fracture Formation and Closure, and Thermal Expansion.

3. The report is inconsistent in the precision used to treat the different areas. In some areas, the authors treat the subject matter much more precisely than in other areas. For example, in their calculations, in the same formula, they give the value of some variables to five or six significant figures and others to only two or three significant figures.

4. Chapter three describes the program, its subroutines and the variables. This chapter needs improvement. It needs more details in the description of the subroutines and its variables. The overall structure should be described in much more detail than just Figure 3.1. The writers are intimately familiar with the code; therefore, have skipped over details that can puzzle an unfamiliar reader. A better organization of this chapter can clarify the subject matter. It should start with the big picture and then go into the various pieces which make up the program.

5. Chapter four describes the input data. These data are provided in form of variables and their formats. It is suggested that the description of the input data be categorized. The actual physical data which are required should

be separated from print options or data required by the internal system of the model. For the input data such as physical properties, etc. the minimum, maximum or average values should be given. This will provide the reader with some mental picture of what the physical system being simulated is all about.

6. Chapter five describes four sample problems. The input data and output date are described in some detail; however, the physical system being simulated is not very clear. There should be some discussion of the results. The sample problems and their solutions should be compared to provide more insight to the system which is being simulated and the structure of the computer model.

7. It is suggested that chapters be separated. The way it is set up the report sections give the feeling that it is one long string with parts running into each other.

b. Specific Comments

1. Page 10. In accordance to Figures 2.6 and 2.7, Leg 18 cannot lead from the lower sandstone to River L. According to Figure 2.6 Leg 18 can only lead from lower sandstone to the middle sandstone.

2. Page 11. In Equations (1)-(18), the Pressures P_1 to P_{18} must have the units pounds force per ft^2. The terms $(D_j - D_i)$ must have a conversion factor changing pound mass to pound force; i.e., it must be multiplied by $\frac{g}{gc}$ where g has units ft/sec^2 and g_c has units $(lb\ mass\text{-}ft)/lb_f - sec^2)$.

3. Page 13. I agree with the authors that other second order terms are negligible. However, I am not sure that $\frac{\partial 2\rho}{\partial c \partial T}$ can also be neglected. This needs to be shown.

4. Page 14. It is important to see the reference "Muller, Finley and Pearson (1981) to find out how they came out with the correlation in Equation (39). I am sure that experimental results are available for density of brines at different concentrations and temperatures. It would be quite useful for the authors to check Equation (39) against experimental data.

5. Page 17. Viscosity, line 2 "composition" would be a much better word than "salt content".

Second Paragraph. The reference sited here is shown in the reference section as, "to be published." Without this reference, it is difficult to know what is the basis for Equation (40).

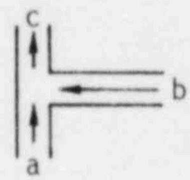
6. Page 18. In the calculation of $C(Na^+)$, why is it that the molecular weight of NaCl is given to 3 significant figures after the decimal (58.443) whereas the conversion factors such as gram to pounds mass is rounded off to 454 (instead of 453.59) and ft^3 to cm^3 is 28,300 (instead of 28,317)? What are units of 3?

7. Page 19. Middle of the page: B should have the units °K. Equation (44) is checked for fresh water only. I am sure plenty of data exists for viscosity of water containing different concentrations of salt. It would be much more interesting to compare results of Equation (44) with water solutions containing salt, especially where this will be much closer to the reality. Some experimental viscosity data for salt solutions were found from the literature. A comparison of these data with those calculated from Equation (44) are shown in Table 1. The comparisons were made at 10° and 20°C for which data was available. Higher temperatures will be of more interest.

8. Page 21. The experimental point is not shown at 120°C. The title of Figure 2.8 should say, "For Fresh Water".

9. Page 23. In solving Equation (47) to arrive at Equation (48), it has been assumed that both K_s and f_s remain constant with respect to distance x. Equation (50) also makes the same assumption. This is not a valid assumption. However, it is realized that the mathematics will become quite cumbersome, if such an assumption was not made. The authors should try to determine what the effect of the above assumption is on results of Equations (48) and (50).

10. Page 25. Figure 2.9A represents junctions such as 6, 8, 10, 12, and 13. Junction such as 4, 5, 7, 9, and 11 would be more correctly represented by the following figure than Figure 2.9B. However, the principle of Equation (52) is correct.



11. Page 27. I do not agree with the physics of the solution channel formation as represented by the authors. The channels are assumed to be rectangular ducts free of porous media. This does not agree with the equation on page 12 ($Vi = q_i/A_i\phi_i$) which indicates that the porosity should increase as the salt goes into solution. Equation (55) also gives a porous media impression.

12. Page 32. It is not quite clear where the model will use Poiseuille's Law based on molecular diffusion and where they will use Darcy's Equation.

Table 1

COMPARISON OF EXPERIMENTAL DATA AND THOSE CALCULATED FROM EQUATION (44) FOR VISCOSITY OF BRINES

WC (Weight Fraction)	20°C			10°C		
	$\mu_{formula}$	μ_{table}*	error	$\mu_{formula}$	μ_{table}*	error
2.9	1.067	1.020	5%	1.3364	1.329	1%
4.9	1.113	1.049	6%	1.395	1.368	2%
10.2	1.242	1.137	9%	1.556	1.496	4%
20.1	1.510	1.491	1%	1.892	2.015	-6%
26.3	1.696	1.933	-12%	2.125	2.508	-15%

*These data are from Stakelbeck, H. and Plank, R., Z. ges. Kalte-Ind. 36, 105, 133 (1929).

13. Page 33. For waste depositories, assuming that the temperature (θ) is constant, is inappropriate. A change in temperature will affect the material properties A, Q, and n and thus these are not constant. The assumption that A, Q, n, and θ are constant makes the analysis very oversimplified.

14. Page 34. Assumption is made but not specified that the analysis in Figure 2.14 is "plane strain" (i.e., no straining occurs in the direction perpendicular to this figure).

15. Page 36. Was the creep model used in the SANCHO code the same as in the DNET code?

16. Page 38. Poisson's ratio for salt is typically large ($\approx$0.4). The lateral Poisson stresses are, therefore, unlikely to be large and not negligible as assumed here.

The vertical overburden stress (σ_1) is not likely to be reduced by lateral channel pressure.

List of variables should include: L = 1 (ft) in this plane strain analysis.

17. Page 39. The linear relationship shown in Figures 2.17 and 2.18 result because a "steady-state" creep model is employed (Equation (64)). The linear creep is appropriate because the beginning time of this calculation was 75 years (Figure 2.16) and the deviatoric stress change rate is constant at later times.

18. Page 41. This approach is acceptable, as a first approximation. The elapsed time (0_e) should be considered variable if large amounts of dissolution occur and the deviatoric stress vs. time is not as uniform as in Figure 2.16.

19. Page 44. This again assumes no Poisson stresses which may be relatively large. σ_z may, therefore, be larger than the fluid pressure in the borehole.

20. Page 48. The thermal treatment, where the problem is reduced to a one dimensional heat conduction equation in a semi-infinite plane is an oversimplification. This may be true at the beginning. However, as fluid flow is established in the salt zone, the thermal treatment must consider both conduction and convection modes of heat transfer. It is possible to neglect radiation only if temperatures are not high. Since the heat flux in radiation is proportional to $(T_1^4-T_2^4)$, this difference can be substantial if, for example, T_1 = 1001°R and T_2 = 1000°R. The temperature difference ΔT = 1°R, gives $(T_1^4-T_2^4)$ as approximately $4x10^6$.

21. Page 50. I do not see much reason for the details given on pages 49-51 regarding the solution of Equation (73). This type of detail can be found in references such as Carslaw and Jaeger, "Conduction of Heat in Solids".

22. Page 52, Second Paragraph. If the DNET thermal treatment agrees so well with SWIFT, as shown in Figures 2.23 and 2.24, then the SWIFT model should be simplified a great deal. It is evident that SWIFT's comprehensive treatment of the heat transfer does not change the results of the temperature distribution calculation. However, I would be surprised if convection does not play an important role.

23. Page 55. The treatment of fracture formation and closure is simplified. It is possible that a simple treatment such as the one given here is sufficient. One cannot be sure until the results of DNET are compared with other fracture formation and closure models which use a more sophisticated formulation whether or not the simpler treatment is satisfactory.

24. Page 57. Section 2.10 treats the thermal expansion of the salt and shale assuming that both the salt and shale are uniform and homogeneous throughout, the temperature varies in the vertical direction, and there is no fluid flow, dissolution, etc. in the salt section; hence, it is not homogeneous.

25. Page 63. In subroutine DGESL is the last parameter zero or the letter O?

26. Page 97. It is not shown how the maximum time step size is controlled (Initial Time Step). If something goes wrong with this system, the program will continue using TMMIN, hence a long CPU time. There should be a built-in system where the program kicks out if it uses time steps equal to TMMIN some N times.

27. Page 100. The porosity value for the Middle/Lower Shale (0.3) is too high.

28. Page 112. In the output Table 5.4, the time unit is missing. I believe it should be years. The unit for time is also missing in Tables 5.7, 5.9, 5.11, and 5.13. Neither the flow rate or the velocity in Legs 3 or 6 seem to change with time.

REVIEWER: STAN BASIN

The manner in which the material in the above referenced document has been organized is quite good. There are, however, a few questions and comments regarding the authors' methodology:

1. Question Regarding the Order of the Computational Sequence: The authors clearly state (bottom of Page 4) that the sequential application of the various submodels in DNET implies that the system remains static over the interval (t, t + Δt). Will the order in which the calculations are carried out have a significant effect on the results? To what extent has this question been investigated?

2. Question Regarding the Initial Conditions: In the case of the first two steps, brine concentration is specified as part of the input data (Page 4, 5th line from the top). How is the value determined? Presumably, the effect of this initial value will diminish in time. How long might this take?

3. Dimensionality, one-, two-, three-dimensional models: It is clear that the DNET model represents a two-dimensional geometry by a network of one-dimensional "legs". How difficult would it be to extend the DNET model to include 3-D networks? Would such an extension be worthwhile? Is a 2-D network model adequate? Some discussion regarding these questions would definitely help the reader in his evaluation of the model.

4. Annotation of Figures 2.3 and 2.6: Two horizontal scales and a vertical scale are included in these figures. The exact meaning of these scales is not clear. The only scale that is clear, at least to me, is the lowest horizontal distance scale whose origin is on the right-hand side. Also, the unit in which the hydraulic head is measured is not included in Figure 2.4.

5. Listing of Assumptions: It would be helpful if all the key assumptions behind the DNET model were listed and briefly discussed. One such assumption appears on the top of Page 9 (DNET requires constant pressure boundary conditions at the aquifer inlets and the point of discharge). Another key assumption appears on the bottom of Page 4 (the system remains static over the interval $(t, t + \Delta t)$).

6. Structure of the Network Model: The particular choice of the network flow model is not exactly clear. Why not use a lattice or grid network or some variation of the network actually used? Apparently, the network under consideration is the simplest network that exhibits some of the salient features of the actual flow system. The reader is left guessing that this must be the case.

7. Series Expansion of $\rho(C, T, P)$: The expansion of $\rho(C, T, P)$ in the form of a Taylor series in three variables brings to mind several questions. Formally, there is no problem with such a series representation; however, from a mathematical or computation point of view one should be concerned about the effect of dropping terms. How rapidly does the given series converge? What about the region of convergence? Perhaps some consideration should be given to special cases, e.g. the value of $\rho(C, T, P,)$ when $C = 0$, $T, = 4°C$. Simple examples may provide the reader with some degree of confidence in the use of such a representation. Also, the evaluation of the individual terms in the series was not exactly clear.

8. Notation: On Page 19 the value of $\mu(T_0, 0)$ is given as 1.002Cp (7th line from the top and 6th line from the bottom). At first glance the abbreviation for centipoise, the unit of viscosity, is somewhat confusing. The reason for this is that

C is used to denote brine concentration. The product of brine concentration and brine density, namely $C\rho$, appears in Equations 42 and 44. Hence, the appearance of Cp is that of a product of concentration and some other variable or a typographic error in which p has been substituted for ρ.

The symbol W_F is used to denote "total leg width" on Page 28, "total width of flow area" on Page 38 and "average fracture opening" on Page 57. Are these essentially the same quantities?

9. Solution Channel Model: The solution channel model, as described in Equation 53, Page 26, seems a little too simplistic. In particular, the assumption of a uniform rectangular channel, appears to be rather naive. Surely, there must be some empirical evidence of actual channel geometries and rates of growth. (c.f. comment on Page 30 regarding experiments involving the geometry of boreholes.) The fact that the authors are talking about average channel width, average channel depth and average cross-sectional area suggests that the authors are treating these variables as random variables. This is not made clear. If channel width and depth are considered random variables then the average value of their product is not equal to the product of their individual average values unless they are statistically independent, an assumption this reviewer finds hard to believe.

The basis for Equations 55 and 56 is not obvious, at least to this reviewer, especially the term involving the porosity of salt.

The terms "total leg width" and "secondary porosity" which appear on Page 28 are not exactly clear. Inclusion of the definitions would be helpful. The same comment pertains to the definition of specific surface, the surface exposed to the fluid per unit volume of solid. How is the volume of solids determined?

An expression is given on Page 33 for the hydraulic conductivity, K, of each leg. It is not clear where this quantity is used. Hydraulic conductivity did not appear, for example, in any of the Flow Equations (1)-(29) on Pages 10 through 12.

The term "deviatoric stress" which first appears on Page 35 is not well known, at least to this reviewer. It would help if the definition was included or references to texts where a reasonably clear definition and explanation may be found.

On Pages 36 - 38, it's not exactly clear whether the computer code SANCHO was used to derive deviatoric stress curves as an input to DNET or whether the SANCHO calculations where used to check the DNET calculations.

In DNET, the initial deviatoric stress, $\Delta\sigma(0)$, is given by $\sigma_1 - \sigma_3$. It is not clear how $\Delta\sigma(t)$ is calculated for t>0. On Page 43, the authors state that time dependent stress curves for the bore hole closure models were generated by SANCHO. This suggests that the SANCHO code must be used in conjunction with DNET. However, the plots of creep versus time (Figures 2.17, 2.18 and 2.20) suggests that SANCHO and DNET calculations were performed independently. This point is in need of some clarification.

REVIEWER: JERRY COHEN

1. Page 1, Second Paragraph. Offers a good description of the purpose and need for the DNET model:

   "The DNET model was developed to investigate processes near the depository such as salt dissolution and salt creep that could affect the release of radioactive waste to circulating groundwater. The DNET model also provides a systematic means for investigating the effects of feedback mechanisms such as thermal expansion, subsidence, fracture formation and fracture closure. These mechanisms can act to accelerate or decelerate the salt potential for release of radioactive waste."

2. The terms "repository" and "depository" are used on page one, and "depository" is subsequently used throughout the text. Although the dictionary definitions of the two terms are nearly synonymous, the term "repository" has found widespread usage in connection with waste disposal and would appear to be the preferred term.

3. Page 2. The specific physical processes included in DNET are listed. However, there is no discussion of why these particular mechanisms were chosen and why any others were not. For example, other physical processes which might affect the final result could include: inhomogeniety of the media (variable properties, rock inclusions, etc.), faulting and jointing, brine pocket migration, absorption phenomena, diaperism, and other physical disruptions. Although these processes might have only a minor affect, they at least should be discussed and some rationale presented for selection of those processes which are considered.

4. The sequential application of the DNET submodels represents a simplifying assumption. The possible impacts of this simplification for typical Δt time intervals should be addressed.

5. Page 5. The recommended geometry for the DNET network is presented. The text seems to imply that the model assumes a very specific situation (i.e., upper shale, middle sandstone,

middle shale, salt bed, lower shale, and lower sandstone layers). We wonder how adaptable the program is to alternative flow systems.

6. What is the dimension of the horizontal scale ranging from 1-69 on Figure 2.3? It is not labeled.

7. In Figure 2.5, the symbols "+" and ">" should be explained in a key.

8. Page 9. It is stated that "these boundary conditions are valid if the aquifer inlet and discharge points are sufficiently far removed from the simulated disruption near the repository". The phrase "sufficiently far removed" should be quantified. How can one be sure that disruptions near Junction 1 would have a small effect on boundary pressure?

9. Page 13. It is stated that higher order terms are negligible. This should be further explained.

10. Page 17. It is not clear how one arrives at Equation (39). The fourth and fifth terms of this equation should be derived and presented earlier in the text.

11. Page 18. The temperature coefficients should be referenced.

12. Page 19. The introduction of the unit symbol Cp (centipose) is confusing, especially when the term C_p has been used in the immediately preceding equation (48).

13. Pages 20 and 21. Viscosity data are given without reference.

14. Page 27. Solution channels are described has having uniform cross sectional areas. One might expect a phenomenon of gradually tapering cross section along the channel length as salt dissolution increases the brine concentration.

15. Section 4. Presents the requirements for user input data. This section is well written and is a useful guide to input and use of the DNET code.

16. Section 5. Provides sample applications of the DNET code. This section is quite useful in providing insight into the capabilities, purpose, and potential applications of the DNET code.

REVIEWER: RALPH FULLWOOD

a. General Comments

This is a well written report. It is logically organized and the Table of Contents provides a detailed breakdown of the contents. The abstract is quite brief but there is no summary and conclusions. The authors could assist the reader by providing some critique of their own work. Has it no limitations? The Introduction (page 1) starts off by saying one of the "most likely means for radioactive waste to migrate from the depository to the ground surface environment is by dissolution and transport in ground water". What other means are of comparable likelihood?

The text goes on, "For a depository in bedded salt, transport in ground water would, for most breachment scenarios, have to be preceded by dissolution of all or portions of the salt layers surrounding the depository". This leaves the reader with the mental picture of water excavating the salt beginning at the point of entry and working toward the location of the waste. One would expect roof collapse as the salt is removed but such geometry changes do not seem to be included in DNET. Furthermore, the scenario seems to depend on whether or not salt is a permeable medium which I believe is a point of controversy.

b. Specific Comments

1. Page 2, Paragraph 1

   The objectives stated in this paragraph are far-reaching and have not been included in the other risk assessments that I am aware of. The authors do not make it clear whether or not DNET works in 3-dimensional geometry. Presumably SWIFT determines the flow field in 3 dimensions but the mathematics in DNET seems to be in one dimension. This should be clarified.

2. Page 5, Paragraph 1. The way this is stated, it seems that SWIFT is only used once to get the flow field. Is it not necessary to keep recalculating the flow field as salt is dissolved?

3. Page 8, Figure 2.7. Paths 2, 15, 17 and 5 are located at interfaces between middle sandstone to middle shale, middle shale to salt, salt to lower shale and lower shale to lower sandstone, yet the flow vector in Figure 2.5 shows no such interface activity. Why is this done?

4. Page 9, Paragraph 1. "The flow network in Figure 2.7, by representing the full (or nearly so) reference site flow system, assures that the disruptions near the depository generally have small effect on the boundary pressures." Why is this true? It may be true mathematically or approximately,

but is it also true physically? One would think that disruptions would remove flow impedances and could affect boundary pressure changes.

5. Page 10. "q" is not defined except to say that it is the fluid discharge. What are the units of q, P, k, A, etc.?

6. Page 10. Comment: I wonder how much of, and how well the necessary input data can be obtained before site closure. As an example P is the pressure up away from the depository in the middle sandstone-middle shale interface. A probe shoved in there to measure the pressure would be disruptive of the flow patterns.

7. Page 13, Paragraph 3. This equation is just a Taylor's expansion and does not contain physical information until the partial derivatives are evaluated so how can we say: "Except for the term containing $\frac{\partial^2 \rho}{\partial T^2}$, the second and higher order terms of Equation 30 are negligible. Thus,

$$\left.\frac{\partial \rho}{\partial C}\right|_{C=0} = \rho_1 - \rho_0 ." \qquad (31)$$

8. Page 14, Paragraph 1. Why are we justified to take the reference pressure as 1 atmosphere? The water head should be about 2500 ft. from Figure 2.6.

9. Page 16. The demonstration that water density does not change much with pressure hardly seems necessary. This is generally believed and can be seen in standard references such as the Chemical Rubber Handbook.

10. Page 17, Equation 40. Why is A_i called the temperature dependent coefficient when it is a multiplier on the concentration?

11. Page 18, Paragraph 1. How are the A_i's evaluated?

12. Page 22. Presumable $C = W/W_{sat}$.

13. Page 22, Paragraph 2. I wonder how one would measure the brine concentration C_0 entering a leg.

14. Page 22, Paragraph 2. The authors have changed variables thus,

$$\frac{d}{dt} = v \cdot \nabla + \frac{\partial}{\partial t} = v \frac{d}{dx} + \frac{\partial}{\partial t}$$

They ignore the $\frac{\partial c}{\partial t}$ term. Equation (47) for the stagnant case says C = 1, but does not allow for the change in brine concentration with time.

15. Page 26, Paragraph 2. Is there a reason to believe that channels predominately form at interfaces?

16. Page 27, Paragraph 2. The assumption of uniform salt removal must be very approximate, because Equation (45) says the rate of removal depends on saturation which changes with distance.

17. Page 28, Paragraph 3. The authors could assist the non-specialist reader by defining the term "secondary porosity".

18. Equation 57. Examination shows secondary porosity to be:

$$\frac{\text{\# of channels} \cdot \text{channel volume}}{\text{effective channel volume}}$$

What is the difference between a leg and a channel?

19. Page 29, Paragraph 2. I do not understand the following: "One final change in the flow system properties must be accounted for following removal of salt by dissolution, namely, lowering of the horizontal leg which represents the solution channels."

20. Page 30, Paragraph 2. Why doesn't this "flaring" occur with horizontal channels?

21. Page 38, Paragraph 2. The test of DNET and SANCHO does not seem decisive since results from SANCHO were input to DNET (page 36, line 6). Because salt creep occurs fairly fast, are there data to test DNET against? What were the overburden and fluid pressures used in the comparisons? It does not seem possible to exert an overburden pressure unless the overburden is flexing downward. This suggests overburden pinchoff of the aquifer or at least a disruption of the flow paths. This does not appear to be treated.

22. Page 49. H is not defined; it is the Heaviside step function occuring at t = 0. In Equation (74), H(t) is the superposition of many Heaviside functions to obtain the superposed solution.

Comment: This heat transfer calculation does not account for heat removal by the ground water flow. Is this effect negligible?

23. Page 52. The 60 kw/acre heat production assumption is high from present commercial wastes, some of which has aged 20 years. This is 66 MWt for the whole repository.

24. Page 54. It is practically impossible to back fill the repository to the density of the original salt. One would think salt expansion would first close these voids before uplifting the shale.

25. Page 55, Paragraph 1. "... the loci of maximum tensile stresses are near the repository margins ...". I suspect this conclusion is more of an artifact of the model than real. Figure 2.26 shows the linear displacement assumption which has a slope discontinuity at the margins.

26. Page 56. Equation 78 seems to ignore any elasticity of the shale. Folded formations would seem to show it has quite a bit of elasticity.

27. Comment: The report readability would be enhanced by beginning major sections on a different page from the preceding. For example, see Page 58 and Page 99.

28. Page 99. The description of the Base Case is very sparse. Is the Base Case the DNET problem with dissolutioning, heat effects, etc.? What are the results?

29. Page 100, Table 5.1. Hydraulic properties have been commented on previously as being highly conservative.

30. Page 119. The results of Sample Problem 2 are buried beginning on this page.

31. Page 137. This page contains the result from Problem 3A. They are interesting, but hard to find.

32. Page 146. This page contains the results for Problem 4.

REVIEWER: CHARLES STEVENS

1. Page 12. This section is confusing. The authors state, "Equations 19 through 29 are solved simultaneously to determine the unknown pressures P_4 through P_{14}." But the eleven equations referred to have the eighteen variables q_1 through q_{18}, which does not enable a solution for the q_i, and the quantities P_i don't appear at all.

2. Page 31. Why does the porosity ϕ enter an equation which relates the area of a circle to its radius?

3. Page 48 - 49. The solution of the heat conduction problem is poorly presented. The problem is posed in terms of an initial condition, $T = f(z)$ when $t = 0$. Also, Equation (72), namely

$$\frac{\partial T}{\partial t} = \alpha \frac{\partial^2 T}{\partial z^2},$$

does not include a heat source term. The solution presented in Equation (73), on the other hand, is presented in terms of an instantaneous source H, and does not involve the boundary condition function f(z). Such inconsistencies unnecessarily confuse the reader.

4. Page 58. Equation (81) presents the evaluation of an integral by approximating the integrand by a histogram whose height within each interval is the mid-point value. Standard trapezoidal integration would be more accurate, while requiring no additional effort.

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16. ABSTRACT (200 words or less)

This project is an ongoing independent technical review of products prepared for the NRC by Sandia National Laboratories on the Risk Assessment Methodology Development for Waste Isolation in Geologic Media program. This report contains a review of three documents. They are NUREG/CR-1634, Vol. 4, concerned with the effects of variable hydrology on waste migration; NUREG/CR-2324, a user's manual for SWIFT; and NUREG/CR-2343, a user's manual for DNET.

In general, these reports exhibit high technical quality that characterizes the SNL work. They are tersely written with little condescension to the non-expert reader for understanding the physical situation being modeled. Indeed, the emphasis is on the mathematical procedures rather than the repository physics, leaving the adequacy of the results presented in many computer plots, pretty much to the interpretation of the reader. Other general comments have been presented previously, such as the data conservatisms, need for data that cannot be measured without disturbing the geometry, and the overall plan for use of the many codes developed in the program.

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